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Who benefits from free health insurance: evidence from Mexico

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Abstract

We present a comprehensive evaluation of the health impacts of the introduction and expansion of a large non-contributory health insurance program in Mexico, the Seguro Popular (SP). SP provided access to health services without co-pays to individuals with no Social Security protection. We identify the program impacts using its rollout across municipalities between 2002 and 2010. In general, we do not detect significant effects on mortality (overall or at any age); the only exception is a reduction in infant mortality (IM) in poor municipalities for which intention-to-treat estimates show a 10% decline due to SP. This decline is attributable to reductions in deaths associated with conditions originating in the perinatal period, congenital malformations, diarrhea and respiratory infections. In these poor municipalities, SP increased obstetric-related hospital admissions by 7%, and hospital admissions among infants by 6%. There were no impacts on mortality or use of hospitals in rich municipalities. The decline in IM rate caused by SP closed nearly all the IMR gap between poor and rich municipalities.

JEL Codes: H10, I12, I13, J13, O18.
Keywords: Health Insurance, Infant Mortality, Health Care Utilization, Mexico.

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1. Introduction

In recent years, many countries have moved towards universal health coverage with various degrees of success (Boerma et al., 2014; Reich and Evans, 2016; WHO, 2015). In particular, many developing nations in Latin America and elsewhere (Atun et al., 2015) have increased the funding for public health insurance programs like the Mexican *Seguro Popular* (hereafter, SP), which we study in this paper. Economists from 44 countries have recently signed a call on global policy makers to prioritize a pro-poor pathway to universal health coverage as an essential pillar of development (Summers, 2015). The relevance of this type of policies is unprecedented especially for those countries, like Mexico, which are undergoing a rapid epidemiological transition, with the burden of disease shifting from infectious towards metabolic conditions, such as obesity and diabetes. SP, with its comprehensive package of both preventive and curative interventions providing a "continuum of care", constitutes an important attempt to meet the complex health needs emerging in such epidemiological landscapes.[1] Are these policies an effective mean to improve the health of the population? If so, why and for whom? In this paper we address these questions in the context of the recent Mexican experience.

The *Seguro Popular* is an ambitious non-contributory health insurance program for unprotected individuals in Mexico. Given that the eligibility requirement for SP is the lack of access to employment-based health insurance, half of the country's population was to be enrolled. The Ministry of Health (or *Secretaria de la Salud*, SSA) introduced the program as a pilot in 2002 with the aim of transforming the existing health services into a national health insurance system. Individuals affiliated to SP are guaranteed access to a comprehensive package of health services without co-payments, within a dedicated network of hospitals and health centers, which are run by the Ministry of Health. In exchange, affiliated individuals are required to pay a subsidized premium; in practice, nearly all affiliates are exempted from it.

Our identification strategy exploits the staggered rollout of *Seguro Popular* across all municipalities in Mexico, between 2002 and 2010. Our paper provides a comprehensive study of the health impacts of SP using a large set of data sources, ranging from the registry of families affiliated with the program, administrative data on deaths, the universe of admissions to public

[1]This is in contrast with other health insurance schemes recently introduced in countries at a similar stage of the epidemiological transition, such as the Indian RSBY (Rashtriya Swasthya Bima Yojna), which is restricted to hospital services (secondary and/or tertiary care), i.e. it excludes primary care.

hospitals, the registries of human and physical resources of all public medical units in Mexico, and health surveys. Combining mortality and hospitalization data sets allows us to pin down the mechanisms behind the health impacts found; the microdata on all households affiliated to SP, which we use for the first time, allows us to relate the characteristics of individuals enrolled at different phases of the expansion of the program with the impacts found. Additionally, all the data sets we use cover several years, since before the introduction of SP (in 2002), up to until the program had reached full coverage (2012).

We study the program impacts on mortality across all age groups, finding mostly insignificant effects, with the exception of a reduction in mortality among infants residing in poor municipalities. Reduction of child mortality (which includes infant mortality) is one of the eight Millennium Development Goals, and, since its introduction, SP offered generous coverage for conditions prevalent among poor children below the age of five, as well as prenatal care and hospital deliveries of births. We present estimates for all Mexican municipalities, and also study heterogeneity of impacts by the poverty status before the introduction of the program, as more deprived areas may have larger gains from the reform, as shown in other settings. A municipality is administratively classified as poor by the Mexican authorities if the 2000 marginalization index is high or very high, as opposed to very low, low or medium; about half of the municipalities in the country are poor. Hence, our sample-split is based on an administrative criterion defined prior to the introduction of SP.[2]

While we are unable to detect robust impacts of SP on mortality in the full sample at all ages that we study (1-4, 5-19, 20-59, 60+),[3] we find a significant reduction in infant mortality in infant mortality (hereafter IMR) by 10% in poor municipalities, which corresponds to 1.55 deaths per

[2]Poor municipalities are defined as priority in the launch of some social programs (for example, the Progresa-Oportunidades; see CONAPO (2001); Hernandez, Orozco and Vazquez (2005)). The marginalization index is used in the planning process and in the allocation of budgetary resources of federal and state governments to public policies aimed at improving the living conditions of the most disadvantaged population. The marginalization index is the first principal component extracted using principal component analysis on the information collected in the 2000 CENSUS in four areas: lack of access to education, inadequate housing, insufficient income and residence in small localities. Within the four broader areas, nine indicators are used to construct the index for a given geographic area: (1) percentage of population living in homes without piped water, (2) percentage of population in dwellings without sewage or sanitation for exclusive use of the house, (3) percentage of population living in housing with earthen floor, (4) percentage of population living in homes without electricity, (5) percentage of population in housing with some level of overcrowding, (6) percentage of employed population with income of up to two minimum wages, (7) percentage of the population aged 15 or over who are illiterate; (8) percentage of population aged 15 years or more without full primary education, (9) percentage of population living in localities with less than 5,000 inhabitants (ie, rural localities).

[3]There is a 3% reduction in mortality among adults aged 20-59 in poor municipalities, that is not robust to controlling for multiply hypotheses testing.

1000 livebirths. We do not detect effects on overall IMR or on rich municipalities. The reduction in IMR in poor municipalities is detected three years after the implementation of SP and closes 98% of the baseline gap in infant mortality between poor and rich municipalities. SP reduced infant deaths associated to three types of medical conditions. First, SP reduced deaths due to preventable and communicable conditions, mainly intestinal and respiratory infections; the conditions causing 59% of the deaths in this group were immediately covered by SP when introduced in 2002. Second, SP reduced deaths associated to perinatal conditions, such as respiratory disorders and sepsis; finally, it reduced deaths due to congenital malformations. All these conditions would have led to the death of the newborn, without immediate care by skilled medical personnel, and, by 2006, the conditions causing about 50% of deaths in these two last groups had the treatment covered by the SP.

We then examine potential mechanisms through which SP reduced infant mortality in poor municipalities, by investigating the role played by demand and supply of health services. We show that, upon the introduction of SP, there is an immediate 10% increase in deliveries in SSA hospitals; this effect becomes stronger with longer exposure to the program, reaching 14% three or more years after its implementation in a municipality. We show that these are births, which would have otherwise occurred outside the health system, and not additional births due to an increase in fertility. We also find an immediate increase in other obstetric-related admissions, and a 7% increase in hospital admissions for infants, mainly due to diarrhea and respiratory infections. On the supply side, we find a 1.5-2% increase in the health personnel per capita in poor municipalities, which is in line with SP's aim to increase the availability of care among the needy; however, we do not detect significant impacts on the ratio of consultations per doctor or nurse.

We provide different pieces of evidence that the main identification assumption is likely to hold. First, we show that municipalities that launched SP in different years were not in differential mortality trends prior to its introduction. Second, we use a flexible time-to-event specification, which has the double benefit of allowing to understand the dynamics of program impacts and to test whether there was a significant differential change in mortality prior to the launch of SP. Finally, our estimates are robust to a battery of alternative specifications, namely, to including state-year trends, state-year fixed effects, municipality trends in pre-program characteristics and municipality linear trends. We note that the impacts on IMR, in the basic specification, are robust to adjusted inference for multiple hypotheses testing. Our main results are obtained using weighted least squares; we do not detect any program effects when using unweighted OLS.

Our paper provides several contributions.[4] First, we contribute to the literature on the effects of health insurance expansions for low SES individuals (as are the uninsured in developing countries), so our findings are also relevant for the undergoing (or attempted) reforms in developed countries like US.[5] In the case of Mexico, no previous paper has comprehensively examined the impact of SP (across all municipalities and over all the years of implementation) on health outcomes, utilization of medical services and supply of health care, using the rich array of data we exploit here; the evidence to date is mixed and there is still incomplete understanding about the timing of impacts and possible mechanisms through which the program operates. Second, we add to the literature on the role of early life interventions as a means to promote health across the lifecourse (see e.g. Conti and Heckman (2013) and Currie and Rossin-Slater (2015)).

The paper proceeds as follows. Section 2 reviews the pertinent literature, and section 3 presents the institutional background and the main features of the program. Section 4 describes the data used, section 5 details the empirical strategy and the results are presented in section 6. Section 7 concludes.

2. Related Literature

While economic theory provides unambiguous predictions about the effects free or subsidized health insurance has on the demand for medical care (see Kondo and Shigeoka, 2013), whether it has any effects on health is still a fundamental and debated question, especially in less developed countries, where the evidence is scarcer. Here we review the evidence for these countries and discuss how SP relates with other reforms (see Appendix C for a detailed review).

Several countries in Latin America have undertaken health reforms since 1980s. Chile introduced a dual system in 1981, which requires workers and retirees to affiliate with either the National Health Fund (FONASA), or with private health insurance institutions (ISAPRES). The public system (FONASA) is a universal health plan that resembles SP and it suffers from long

[4]Table B.1 provides details of previous literature on Seguro Popular, and we discuss it further in section 3.

[5]Contrary to the Mexican experience, in the United States universal health coverage has not been reached yet, despite the remarkable progress obtained with the Affordable Care Act (ACA): affordable care insurance is still out of reach for many, in particular poor individuals, minorities and unemployed (Gostin, DeBartolo and Hougendobler, 2015) – all categories which have been covered by Seguro Popular.

waiting times, poor quality and shortage of specialists (Savedoff and Smith, 2011). Despite these issues, the program increased access and coverage, and reduced hospital case-fatality rate for some diseases, such as hypertension, diabetes and depression (Bitrán, Escobar and Gassibe, 2010). In 1988, Brazil created the Unified Health System (*Sistema Único de Saúde*) that serves more than 80% of the population (Paim and Macinko, 2011). The anchor of the Brazilian system is the Family Health Program (*Programa Saúde de Família*), adopted in 1994 to promote and provide primary care services through the use of professional health care teams that intervene directly at the family level. The program has been consistently associated with a reduction in infant mortality (Macinko, Guanais and Souza, 2005; Aquino, Oliveira and Barreto, 2009; Bhalotra, Rocha and Soares, 2019), in maternal mortality and with an increase in prenatal care (Bhalotra, Rocha and Soares, 2019).

More recently, Colombia introduced the Subsidized Regime (*Regimen Subsidiado*) in 1993 which fully subsidized the poor to purchase insurance from private, government-approved, insurers.[6] As the SP, the Subsidized Regime provided a package of health services for pregnant women, including prenatal care, delivery care, cesarean delivery, special care for women with high-risk pregnancies, a package of medicines, vitamins, and nutritional supplements. The program successfully protected families from financial risk, increased the use of preventive services, improved health (Miller, Pinto and Vera-Hernández, 2013), and reduced the incidence of low birth weight (Camacho and Conover, 2013). Lastly, Peru introduced the Comprehensive Health Insurance (*Seguro Integral de Salud*) in 2001, which is similar to Seguro Popular in the type of coverage offered without co-payments or fees. The Comprehensive Health Insurance decreased out-of-pocket health expenditures, increased visits to doctors, prescription of medicines and diagnostic testing, but there were no impacts on the use of preventive care, with the exception of women in fertile age (Bernal, Carpio and Klein, 2017).

Outside Latin America, Thailand introduced the ”30 Baht” in 2001, which increased funding available to hospitals to care for the poor and reduced the co-pays to 30 Baht. This reform increased health care use and reduced postneonatal mortality (Gruber, Hendren and Townsend, 2014) and out-of-pocket medical expenditure (Limwattananon et al., 2015). Turkey launched in 2005 the

[6]Among health insurance schemes targeting the poor, subsidies to the purchase of health insurance in Ghana promote enrollment and health care service utilization up to three years after randomization into the subsidy, although impacts health are short lived (see Asuming and Sim, 2017).

Family Medicine Program that assigns a GP to each citizen, and primary care services are offered free of charge in health centers; Cesur et al. (2017) find that it reduced mortality among infants, children 1-4 years old and elderly.[7] Finally, evidence from Nicaragua and Vietnam shows that health insurance schemes targeting the poor and informal workers tend to suffer from low-take (see Thornton et al., 2010; Wagstaff et al., 2016).

While the recent evidence we reviewed above has significantly expanded our knowledge on health insurance in less developed countries, we are able to further contribute to it. Relative to the papers above, we are able to explore richer data on mortality (which includes information on causes of death) and also on hospitalizations, in addition to study the determinants of local implementation of the program.

We now turn to the evidence on Mexico. To date, a large part of the SP literature has focused on the labor market impacts, studying the potential distortion of workers' incentives to switch from formal work arrangements, which provided health insurance coverage before SP, to informal jobs. The evidence on this issue is mixed: some studies do not find any impact (Gallardo-García, 2006; Barros, 2008), while others find small increases in the share of informal workers among the less educated and those with children (Aterido and Pages, 2011; Azuara and Marinescu, 2013; Bosch and Campos-Vazquez, 2014; del Valle, 2014; Conti, Ginja and Narita, 2018). The differences in the impacts do not seem driven by the identification strategy employed, but rather by the period studied - with smaller effects found in studies that have examined the earlier period.[8]

The literature on the health impacts of SP is more recent, but vast, and we summarize it in Table B.1 in Appendix B. For each paper listed in the table we include the data set used, the period of analysis covered, the identification strategy adopted, and the findings. Here we summarize the main findings of the various studies. King et al. (2009); Barros (2008); Galárraga et al. (2010) and Grogger et al. (2015) focus on out-of-pocket expenditures, and unanimously show that SP has been

[7]In 1988, the Chinese government increased cost sharing for patients through a combination of deductible, coinsurance, and individual medical savings accounts rather than reducing the price of services. Huang and Gan (2017) finds that this reform decreased outpatient medical care use and expenditures, without decreasing inpatient care utilization or expenditures; there was little impact on patient's health. Patients from low-income and middle-income households or with less severe medical conditions were the most sensitive to changes in prices.

[8]These studies use subsamples of the more than 2400 municipalities in the country (for example, Azuara and Marinescu (2013) and Conti, Ginja and Narita (2018) rely on the Mexican labor force survey and use, respectively, 350 and 600 municipalities, whereas Bosch and Campos-Vazquez (2014) use data for 1395 municipalities).

effective in substantially reducing them. There is evidence of mixed impacts of SP on health care use and health. Sosa-Rubí, Galárraga and Harris (2009) and Bernal and Grogger (2013*a*,*b*) find an increased use of obstetric services among those affiliated to SP, while King et al. (2009) and Barros (2008) find no effect on the population at large. Knox (2015) finds an increase in the use of health services provided by SP among the poorest urban population. Barros (2008), Knox (2015) and King et al. (2009) are unable to detect any health impact of SP, using experimental or survey data. Finally, SP reduced infant mortality as of 2010 (Pfutze, 2014) and the likelihood of miscarriages among women pregnant between 2004 and 2008 (Pfutze, 2015).

In summary, the evidence available to date has provided a fragmented and partial picture of the health impacts of SP. Of the 19 papers listed in Table B.1 no study has used data from before the introduction of the program in 2002 and up to after its full rollout (ie, after 2010), which would allow understanding the dynamics of treatment effects; no paper has used variation from all municipalities in country, which is needed to study the characteristics of the municipalities launching the program in different years; finally – and somewhat surprisingly – no paper so far has used the administrative records on mortality. The current literature provides a partial picture of the possible health impacts of SP due to a number of issues. First, part of the evidence draws on the experimental data of King et al. (2009), which is based on 100 health clusters in 7 (of the 32) states (Spenkuch, 2012; Bernal and Grogger, 2013*a*,*b*; Grogger et al., 2015). Besides the limited geographic coverage, the experiment includes a baseline survey collected in 2005 and a 10-months follow-up, which is too short to learn about the program maturity. Second, except from Pfutze (2014) and Pfutze (2015), none of the other papers is able to study medium- or long-run effects of the SP. Third, most papers rely on survey data, which cover just a few hundreds of the municipalities in the country (Gakidou et al., 2006; Scott, 2006; Gallardo-García, 2006; Sosa-Rubí, Galárraga and Harris, 2009; Harris and Sosa-Rubí, 2009; Hernández-Torres et al., 2008; Barros, 2008; Knox, 2015; Pfutze, 2015; Turrini et al., 2015). Our work overcomes all of the limitations of previous studies.

3. Background

The Health Care System before *Seguro Popular* Before SP, health care in Mexico was characterized by a two-tiered system. About half of the population was covered through a contributory system (still in place today) guaranteed by the Social Security Institutions: the Mexican Social Se- curity Institute (*Instituto Mexicano del Seguro Social*, IMSS), covering the private sector workers; the Institute for Social Security and Services for State Workers (*Instituto de Seguridad y Servicios Sociales de los Trabajadores del Estado*, ISSSTE), covering the civil servants; and Mexican Petroleums (*Petroleos Mexicanos*, PEMEX), covering the employees in the oil industries. Health coverage was provided by these institutions in public hospitals; however, individuals could also pay for care in private hospitals, or buy private health insurance. In 2000, IMSS covered 40%, and ISSSTE 7% of the population, respectively (Frenk et al., 2006).

Health care was also available to the poor through two programs. The first one was the Coverage Expansion Program (*Programa de Ampliacion de Copertura*, PAC), which started in 1996 and consisted of health brigades visiting the more rural and marginalized areas of the country. The other program was the Program for Education, Health and Nutrition (*Programa de Educacion, Salud y Alimentación*, Progresa), that was launched in 1997 in rural areas as the main anti-poverty program in Mexico, and renamed *Oportunidades* and expanded to urban areas in 2002.

The uninsured population not covered by PAC or *Progresa* could seek health care either in public health units run by the Ministry of Health (*Secretaria de Salud*, SSA) or in private ones. In both cases, payment was at the point of use and patients had to buy their own medications. Hence, in 2000, approximately 50% of health expenditures was classified as "out-of-pocket expenses" (Frenk et al., 2009), and 50% of the Mexican population - about 50 million individuals - had no guaranteed health insurance coverage.

The Implementation of *Seguro Popular* SP was launched as a pilot program in 2002 in 26 municipalities (in 5 states: Campeche, Tabasco, Jalisco, Aguascalientes, Colima) under the name Health for All (*Salud para Todos*). During 2002, 15 additional states[9] implemented the program, by agreeing with the federal government to provide the health services covered by SP. By the end of the pilot phase, on December 31*st* 2003, six additional states[10] had joined. The System of Social Protection in Health (*Sistema de Protección Social en Salud*, SPSS) was officially introduced on January 1st 2004 to extend health coverage and financial protection to the eligible population. The

[9]Baja California, Chiapas, Coahuila, Guanajuato, Guerrero, Hidalgo, Mexico, Morelos, Oaxaca, Quintana Roo, San Luis Potosi, Sinaloa, Sonora, Tamaulipas and Zacatecas.
[10]Baja California Sur, Michoacán, Puebla, Tlaxcala, Veracruz and Yucatán.

expansion prioritized states with: (1) low social security coverage; (2) large number of uninsured in the first six deciles of income; (3) ability to provide the services covered by the program; (4) potential demand for enrollment; (5) explicit request of the state; and (6) existence of sufficient budget for the program. In 2004, three more states introduced the program (Nayarit, Nuevo Leon and Querétaro). The last three states (Chihuahua, Distrito Federal and Durango) joined SP in 2005.

Eligibility and Enrolment Individuals who are not beneficiaries of social security institutions are eligible to enroll in SP. Enrollment in the program is voluntary, and is granted upon compliance with simple requirements (proof of residence in the Mexican territory; lack of health insurance, ascertained with self-declaration; and possession of the individual ID). The basic unit of protection is the household. Within ten years since the piloting of SP, by April 2012, 98% of the Mexican population was covered by some health insurance (Knaul et al., 2012). The main reasons for affiliation in SP were access to free medicines and to primary care at reduced costs (Nigenda, 2009).

Funding Before 2004, the public health expenditure on the insured was twice that on the uninsured, but the gap was substantially closed after 2004 (see Figure A.1 in Appendix A). Hence, the program seems to have been successful in accomplishing one of its goals, that of redistributing resources from the insured to the uninsured. As a non-contributory health insurance system SP is funded by revenues from general taxes, on the basis of a tripartite structure similar to that adopted by the two major social insurance agencies in Mexico, IMSS and ISSSTE. More precisely, it is funded by contributions from the federal government, the states, and the families.[11]

Coverage of Health Services Once a family is enrolled in SP, it is assigned a health center (which, in turn, is associated to a general hospital) and a family doctor for primary care, and it has access to a package of health services. The number of interventions covered increased yearly, from 78 in 2002 to 284 in 2012, as listed in a 'Catalogue of Health Services' that is revised annually (Knaul et al., 2006).

[11]The family contribution was based on the position of the average household income in the national income distribution. In 2010, 96.1% of the enrolled families were exempted from paying the family contribution, on the basis of their low socioeconomic status; in practice, very few households ever contributed at all (Bonilla-Chacin and Aguilera, 2013).

A wide range of services are included, from prevention, family planning, prenatal, obstetric and perinatal care, to ambulatory, emergency and hospital care, including surgery. The bulk of the services covered since 2002 are preventive age-specific interventions. For children under five years old, SP covers vaccinations, comprehensive physical check-ups (including measurement of height and weight, and nutritional advice for parents), and diagnosis and treatment (e.g. up to seven days of medicines) of acute intestinal and respiratory infections. The package of services for this age group underwent a further expansion in 2006 with the introduction of Health Insurance for a New Generation (*Seguro Medico para una Nueva Generación*, SMNG), which covers conditions specific to the perinatal period.

Prenatal care is also covered; it is delivered in health centers and it includes five medical check- ups during a normal pregnancy (during the first 12 weeks and at weeks 22-24, 27-29, 33-37, 38-40). In addition to the provision of acid folic, a set of laboratory tests should be performed during the medical check-ups: blood and urine tests, VDRL test (screening test for syphilis), blood type and HIV test for women at potential risk. Diagnoses associated with high risk pregnancies, such as obesity, eclampsia, diabetes, placenta previa, and growth retardation are referred to specialist care (CNPSS, 2002, 2004). Covered services include also normal delivery, puerperium and perinatal care of the newborn, metabolic screening of the newborn to detect phenylketonuria and congenital hypothyroidism, and treatment of congenital hypothyroidism.

For adults 20-59 years of age, the coverage included vaccinations, and regular check-ups every three years after the age of 40. Among those over 60, it included medical checks-up with blood tests for cholesterol and lipids detection every three years, annual checks for hypertension, and regular cervical cystology and mammography every other year up to age 69.[12]

Finally, SP includes package of high-cost, specialized interventions financed through the *Fondo de Protección contra Gastos Catastróficos* (Fund for Protection against Catastrophic Expenditures, FPGC). The FPGC covers a package of services that were selected using cost, effectiveness and social acceptability criteria. This fund finances nearly 20 interventions, including neonatal intensive care and the management of paediatric cancers, cervical cancer, breast cancer and HIV/AIDS.

[12]Before the introduction of SP, tabulations from the 2000 Health Survey (ENSA), show that nearly all new mothers had access to some type of prenatal care. Nevertheless, in 2000 also only around half of the children 0-4 had a health care visit by a doctor or a nurse in case of diarrhea or respiratory infections, and less than 10% of women aged 40-59 reported taking a mammography in the last 12 months in the poor municipalities.

The services are delivered in the hospitals and clinics run by the Ministry of Health, which has a completely separate network from that of the contributory systems.

Supply of Health Care One of the main objectives of the health reform was to increase investment in health care infrastructure and to achieve a more equitable distribution of health care resources. In addition, medical facilities could only enter in the SP network upon receiving accreditation, which was granted only if the required resources to provide the covered interventions were in place (Frenk et al., 2009). Coherently with this objective, the proportion of the Ministry of Health budget devoted to investment in health infrastructure increased from 3.8% in 2000 to 9.1% in 2010, with the construction of 2,284 outpatient clinics and 262 (community, general and specialized) hospitals between 2001 and 2006 (Table B.2 in Appendix B);[13] as a consequence, the number of municipalities covered by each hospital declined from an average of 7 in 2000 to an average of 5 in 2010.[14] As a result, the gap between individuals covered and not by Social Security was reduced in terms of the availability of general and specialist doctors, nurses and beds (Knaul et al. (2012) and Table B.3 in Appendix B show bigger increases in medical personnel in SSA than non-SSA units). Further redistribution was achieved by prioritizing the resources in poor municipalities: Table B.4 shows that the number of hospitals and beds in poor grew more than in rich municipalities. Nevertheless, this increase in resources was not enough to close the gap in the sup- ply of services between the poor and rich municipalities in Mexico. Table B.3 and Table B.4 show that poor municipalities still have less medical personnel per 1,000 inhabitants and less hospital beds than rich municipalities in 2010.

4. Data

[13] In the public sector as a whole, 1,054 outpatient clinics and 124 general hospitals were built in the same period (Frenk et al., 2009).

[14] Source: own calculations based on the Health Ministry discharges data. Table B.2 in the Appendix shows that there was an increase in the total number of medical units under the SSA by about 21%, from 11,824 in 2001 to 14,374 in 2010. The increase in the number of units varied by type, with an increase by about 20% in the number of outpatient units, and by about 60% in the number of inpatient units. This latter increase was mainly driven by the community hospitals (*hospitales integrales/comunitarios*).

We combine rich administrative and survey data to provide complementary evidence on the health impacts of SP and the mechanisms through which they occurred. Table B.5 in Appendix B includes the description all data sets used and their sources.

Administrative Data We use seven administrative data sources. First, for this project, we were granted access to the registry of *all* families with a valid enrolment in Seguro Popular by December 31st of each year, since 2002 until 2010, which is called the *Padrón*. This is the key source used by the Federal Government and the States to decide the amount of funds to allocate to the program. In addition to the exact affiliation date, the *Padrón* contains information on the demographic and socioeconomic characteristics of the enrolled families, on their address of residence, and on the identifiers of the health center and of the general hospital assigned at the time of enrolment. The exact date of affiliation of each family is used to construct the treatment indicator: the date of implementation of the program at the level of the municipality. For the years 2002 and 2003 (when the program ran as a pilot), only information on the date of enrolment and on the state of residence was recorded. Since each family has a unique identifier, we identified the exact date of implementation of SP in a given municipality by backtracking the relevant information from the subsequent years. We then confirmed the accuracy of the implementation date obtained with this procedure by cross-checking it against the official list of municipalities which adopted SP in the pilot period.

Second, to analyze the impact on mortality we use the death certificates for the whole country between 1998 and 2012. The data contains information on the date, place and cause of death (ICD10 classification), its registration date, and on the date of birth, gender, type of health insurance and residence of the deceased. We use this data to construct municipality-year counts of deaths by age group (infants less than 1, children 1-4, children and adolescents 5-19, adults 20-59 and 60-89).

Third, we use administrative data on births between 1998 and 2012. This data includes information on the exact date of birth, gender, status of the baby at birth (ie, born alive or not), municipality of birth and municipality of residence of the mother, whether the birth took place in hospital or not (but no information about the type of insurance coverage or the entity managing the hospital), and age of the mother. This data is used to construct annual counts of live births per municipality-year to study the impact of SP on fertility and to compute the infant mortality rate (ie, the number of deaths before age 1 per 1,000 live births). For individuals older than 1, we construct the age adjusted mortality rate by age group (that is, 1-4, 5-19, 20-59 and 60-89) dividing the deaths counts by the population in each age group in that municipality in a given year per 1,000. After computing the age-specific mortality rate for each municipality, $ASMR_{ta}$, in year t for age group a (infants, 1-4, 5-19, 20-59, 60-89), we obtain the age-adjusted mortality rate in year t as a weighted sum of age-specific mortality rates, $AMR_{ta} = \sum_{a=1}^{5} s_a \, ASMR_{ta}$, where s_a is the population share of age group a in 2000 (ie, 1-4, 5-19, 20-59, 60-89; for infants we use the number of live births).

Fourth, we use two data sources on hospital discharges. The first is the administrative data with the information from discharges from any public hospital in Mexico, which is available for the years 2004-2012. This data includes limited information: gender and age of the patient (banded in categories), main medical condition at admission, state in which the medical unit is located and the entity managing it (i.e., SSA hospitals, IMSS, ISSSTE, IMSS-Oportunidades or PEMEX). The second is the administrative data containing all discharges from the SSA–Health Ministry hospitals, which is available for the years 2000-2012. This data includes more detailed information: the identifier of the medical unit, demographic characteristics of the patient (age, gender, state and municipality of residence), the dates of admission and discharge, the main conditions diagnosed, and the medical procedures carried out during the hospitalization. We use this data to examine the impact of SP on hospital admissions (total and by cause), mode of entry (ie, via E.R. or planned) and length of stay. We focus on admissions to general or integrated hospitals, speciality hospitals and clinics, excluding psychiatric hospitals and federal health institutes.[15] In Mexico, SSA hospitals are present in 544 of the 2,454 municipalities.

Fifth, we use two data sources on the supply of health care. The first is the administrative data containing information on the human resources for all public inpatient and outpatient units providing health services for the years 1996-2011. This data is obtained from the State and Municipal System Databases (*Sistema Estatal y Municipal de Bases de Datos*, SIMBAD), and it has information at municipality level on the medical personnel (doctors and nurses) and the number of outpatient visits for each public provider of health services (i.e., IMSS, ISSSTE, PEMEX, IMSS-Oportunidades, SSA and others such as military or local providers), including both health centers and hospitals. The second data source is the administrative data that includes for each outpatient and inpatient unit administered by the Health Ministry information on the physical (e.g., number of

[15]These are medical units specialized for the treatment of cancer or cardiovascular diseases, pediatric care or geriatric care. They are mostly located in the Distrito Federal, but serve the whole country.

beds, MRI equipment) and human resources (number of doctors by speciality, nurses and other health technicians) for the period 2001-2010.

Health Survey Lastly, we use the National Health and Nutrition Survey (*Encuesta Nacional de Salud y Nutrición*, ENSA/ENSANUT). These are repeated cross-sections fielded in 2000, late 2005/early 2006, and late 2011/early 2012, i.e. before, in the middle and at the end of the SP rollout.[16] The data includes both self-reported and objective health measures, and age-specific modules. Several variables are not consistently collected across the three waves, which limits the use of this data to study the impact of SP. Nevertheless, we use this data to measure simultaneously the place of birth (ie, at hospital or not) and also the entity managing the hospital of delivery.

5. Empirical Strategy

Our identification strategy exploits the quasi-exogenous variation in the timing of implementation of SP at the level of the municipality. Given its scale and the constraints imposed by financial resources and availability of infrastructure, the SP was gradually introduced across the Mexican states, and across municipalities within each state. While the state-level rollout was regulated by law, the municipality-level expansion was unregulated (see section 3). As specified in section 4, we use information from the *Padrón* on the date in which each household enrolled in SP to construct the treatment variable. In the absence of a formal definition, we consider that SP is introduced in a municipality when the number of families affiliated to the program is at least 10. We adopt this number for a variety of reasons. First, we prefer an absolute to a percentage measure as we want to capture the fact that the residents of a municipality can effectively use the services provided by SP as a result of the authority's decision, and not the fact that a certain proportion of the population has been covered (which is determined by individual choice). We show below that our results are robust to the choice of threshold, and that they are unchanged if we use a definition based on 5, 15 or 20 families. Second, we do not use smaller figures such as one or two households due to possible

[16] This survey includes 45,711, 47,152 and 50,528 households living in 321, 582 and 712 municipalities for the years of 2000, 2006 and 2012, respectively. In our analysis, we restrict the sample to municipalities observed at least twice in data (that is, 432 municipalities out of the 990 ever surveyed).

measurement error.[17] Thus, we use a definition which has become relatively common in the SP-related literature, see e.g. Bosch and Campos-Vazquez (2014) and del Valle (2014).

5.1. Event-Study Specification

Figure A.2 in the Appendix displays the year of implementation of SP in each municipality in Mexico, between 2002 and 2010 (Panel A of Table B.6 includes the number of municipalities implementing SP per year). This graph (together with its zoomed state-level version reported in Figure A.3-Figure A.5) shows that there is considerable variation across municipalities in the timing of implementation of SP; in Figure A.6 we include the total number of municipalities offering SP in each month. Hence, we exploit the staggered timing of implementation of SP by comparing changes in outcomes for municipalities that introduced it in different years between 2002 and 2010, i.e. earlier vs. later entrants, within an event-study framework. Therefore, we start by presenting evidence from two empirical tests to support the key identification assumption of our strategy, that the timing of SP establishment is uncorrelated with other determinants of changes in mortality.

First, we study whether socio-demographic characteristics of municipalities predict the timing of a municipality implementation of SP. Table 1 presents estimates for η in the following equation:

$$Year_{ms} = \eta \mathbf{X}_{ms,t0} + \pi_s + \chi_{ms} \tag{1}$$

where $Year_{ms}$ is the year of implementation of SP in municipality m of state s, $\mathbf{X}_{ms,t0}$ is a vector of pre-SP municipality-level socio-demographic and political characteristics and health care resources and π_s are state fixed effects. We use 2000 as our baseline year for the socio-demographic characteristics, with the exception of the resources allocated to medical units run by the SSA-

[17]For example, a municipality in the state of Aguascalientes (Asientos) has one family enrolled in September 2002, and, after this, four families were recorded in January 2004.

Ministry of Health, for which information is only available since 2001.[18] By December 2010, 2,443 municipalities in Mexico had implemented the program. Throughout the paper, we use a sample of 2,382 municipalities which existed in 2000 and implemented SP by 2010, for which there is non-missing data on baseline characteristics and that had more than one infant death throughout the period studied (1998-2012).

Column (1) of Table 1 presents the mean for each variable; in column (2) we include estimates for a version of equation 1 without state fixed effects. It shows that, across states, earlier implementation of SP took place in more populous (see also Azuara and Marinescu, 2013; Bosch and Campos-Vazquez, 2014) and richer municipalities, with a smaller share of eligible individuals (see also Azuara and Marinescu, 2013), of children 0-4, with more hospitals, health centers and doctors per eligible (see also Azuara and Marinescu, 2013), and where there is alignment between the party of the mayor and that of the governor of the state. When we study the determinants of the time of entry within states in column (3),[19] the coefficient on the share of children 0-4 is no longer significant; all the other estimated coefficients keep the same sign as in column (2), their magnitude is reduced, but they are still significant. It is not surprising that larger municipalities are early entrants: they have more resources, and thus could fulfill the necessary requirements to have certified medical units to provide SP services. These are also municipalities equipped with medical units and physicians to staff them. To account for these potential threats to internal validity, we control for municipality fixed effects to account for pre-existing differences in levels across areas. We also provide additional robustness checks to our analyses by including controls for municipality linear trends in baseline characteristics of municipalities (see Acemoglu, Autor and Lyle (2004)), in particular, we include trends for the following characteristics: socioeconomic indicators measured in 2000 (quadratic of the index of marginalization, log of total population, and share of population of ages 0-4); labor market indicators measured in 2000 (share of uninsured individuals, share of individuals employed in the primary, secondary and tertiary sectors); health care indicators measured in 2001 (number of hospitals, health centers, and doctors in hospitals, all per uninsured).

[18]The list of the variables and their sources is provided in Table B.5 in the Appendix. We present in Table 1 health supply indicators measured by eligible since the information used on the number of hospitals, health centers and doctors is for medical units administrated by the SSA-Health Ministry, which is the dedicated network for the uninsured and where SP health services are offered.

[19]Unobserved time-invariant state-level characteristics explain about 50% of the variation in the timing of entry of a municipality.

Second, we examine whether the timing of the introduction of SP was correlated with levels or trends in pre-program mortality rates. This could be the case if, for instance, the program was implemented earlier in locations with higher mortality rates or with declining trends. Table 2 presents estimates of mortality rates in 2002 and changes in MR from 1998 to 2002 against the year of SP introduction, controlling for state fixed effects. This table shows no evidence of such correlations.[20]

Therefore, we estimate the following dynamic differences-in-differences model:

$$\gamma_{mst} = \sum_{k=-K}^{-2} \beta_k^B SP_{mst} \mathbf{1}[t - T_{sm} = k] + \sum_{k=0}^{L} \beta_k^A SP_{mst} \mathbf{1}[t - T_{sm} = k] + \mu_{ms} + \pi_t + \varepsilon_{mst} \quad (2)$$

where SP_{mst} is an indicator variable equal to 1 if the municipality of residence m in state s offers SP in year t. T_{sm} is the year of implementation of the program. The exact values of k depend on the number of years available in the data, before (K) and after (L) the implementation of SP. For sake of precision, in our most flexible specification we assume constant effects for five or more years before introduction of SP (so $K = 5$) and six or more years of exposure ($L = 6$). For most of the analysis we use registry data on deaths and hospital discharges aggregated at the level of the municipality of residence m (in state s) in year t, which refers to the time of the death and of the admission to the medical unit, respectively. In all our models we include fixed effects for the municipality of residence, μ_{ms}, to account for time-invariant municipality-level unobserved heterogeneity. Year fixed effects π_t account for yearly shocks which are common to all municipalities which may affect the outcome y_{mst}. Finally, ε_{mst} are idiosyncratic shocks.

The standard errors are clustered at the municipality level to account for autocorrelation in the outcomes (Bertrand, Duflo and Mullainathan, 2004). In our estimation we measure outcomes (mostly, mortality and hospitalizations) for five age groups: infants (ie, before 1 year of age), at ages 1-4, 5-19, adults (ages 20-59) and elderly (age 60+). These age groups reflect the age- specific medical interventions covered by the SP (see section 3). As the unit of observation is at municipality-year level, we present weighted estimates to account for heteroskedasticity by

[20]Notice that the municipality fixed effects absorb permanent unobserved heterogeneity related to pre-existing differences in the level of elderly mortality.

population size, using as weight the population in each age group in the municipality in 2000 (see Almond, Hoynes and Schanzenbach, 2011; Bailey and Goodman-Bacon, 2015); in Appendix B we also present unweighted estimates.

The impact of being exposed to SP is captured by the coefficients β_k, where k is the difference between the year of observation t and the year of implementation T_{sm}. Thus, the estimated coefficients β_k^B and β_k^A describe the evolution of the outcome in (eventually) treated municipalities before SP, and the divergence in outcomes t years after its introduction, respectively, relative to the year prior to the implementation (since $t = -1$ is omitted).[21]

This event-study framework has two main advantages. First, it allows for an immediate test of the existence of differential pre-program trends in the outcome. That is, rather than assuming that $\beta_k^B = 0$ for $k < 0$, this more flexible model allows to visualize whether the key identifying assumption that there are no group-specific trends that are correlated with the treatment is met or not. Second, it allows for dynamics in the treatment effects, which might arise for several reasons. For example, individuals may not be immediately aware of the availability of SP in their municipality of residence, which might occur either because they are not exposed to the relevant sources of information, or because people tend to become affiliated at the time they use medical services; and/or medical units may take time to adjust their technology of provision of care to the potential new demand.

For most of the results presented in the paper, we summarize the magnitudes and test the joint statistical significance of the event-study estimates in a differences-in-differences specification, where the individual event-year indicators of equation (2) are replaced with indicators for groups of event-year of three categories. In particular, the specification used to present our results in the tables below is the following one:

$$\begin{aligned}\gamma_{mst} = {} & \beta_1 SP_{mst}\mathbf{1}[t - T_{sm} \leq -2] + \beta_2 SP_{mst}\mathbf{1}[0 \leq t - T_{sm} \leq 2] \\ & + \beta_3 SP_{mst} 1[t - T_{sm} \geq 3] + \mu_{ms} + \pi_t + \varepsilon_{mst}\end{aligned} \quad (3)$$

[21]We use $t = -1$ as the control year as Hoynes and Schanzenbach (2009) and Bailey and Goodman-Bacon (2015) who use strategies similar to ours and in a similar context (introduction of Food Stamps and Community Health Centers across counties in the US, respectively). Additionally, throughout the year of implementation of SP ($t = 0$), some municipalities may reach the 10-families threshold in either January or in December, meaning that for those municipalities who launched the program early in the year $t = 0$ may effectively include some of the program immediate impacts. Panel B of Table B.6 in Appendix presents the number of municipalities introducing the program in the first, second, third or fourth quarter of the year. Interestingly, the third quarter is when most municipalities launch SP; as a note, the federal budget is approved in November.

Here β_1 subsumes the impact up to 2 years before the introduction of SP, β_2 captures the short run impact (up to 2 years after the introduction of SP), and β_3 captures the impact of exposure after 3 years or more. We interpret the coefficients as intention-to-treat effects (ITT), since our regression model estimates the reduced form impacts of implementing SP, and our estimated coefficients average the SP effects over all individuals in the municipality, although not all are affected by the health reform. Hence, our estimates are a lower bound of the program impacts. In 2000, the mean share of eligible per municipality was .75 (Table 1; the standard deviation – not included in table – is .18). Figure 1 shows the enrolment rate in SP among eligible across municipalities from the year of the implementation of the program (t = 0) onward. The dots are the mean enrolment rate among eligible, whereas the crosses are the 25th and 75th percentiles. In the year of introduction of SP, on average nearly 40% of the eligible enroll in the program, with considerable variation across municipalities (the 25th and 50th percentiles are 10% and just over 50%, respectively). This figure is similar across poor and rich municipalities (Figure A.7 in Appendix B).

When presenting our results, we include estimates for all municipalities, and allow also for heterogeneity of impacts by the poverty status before the introduction of the program. This is because we expect that more deprived areas may have larger gains from the reform. This analysis by deprivation level is based on the criteria of local targeting of resources to fund social programs in place before the introduction of SP. Poor municipalities, defined as those with high and very high deprivation levels (as opposed to medium, low or very low), are more likely to have high pre-SP IMR (as measured by an indicator for whether the mean IMR between 2000 and 2002 is above the IMR of the median municipality, ie, 13 deaths per 1,000 births) and they have fewer pre-SP resources in the health sector (ie, in 2001 they are more likely not to have an hospital and have fewer doctors in SSA-Health Ministry medical units than rich municipalities).

5.2. Robustness Analyses

We run a battery of robustness checks for threats to the validity of our research design. We summarize here the nine alternative specifications we use and defer to section 6 the discussion of the results. First, we exclude from our baseline specification those municipalities that launched the program during the pilot period, that is, 2002 and 2003. Second, rather than clustering the standard errors by municipality, we do it by state-year to account for within state-year correlation in the allotment of funds across municipalities. Third, as mentioned in previous subsection, we control for municipality linear trends in baseline characteristics of municipalities. Fourth, we control by an indicator of alignment between the party ruling in the municipality and in the state in a given year. Fifth, we use the fact that we are able to measure mortality and hospitalizations before the introduction of SP to include municipality-level pre-reform linear trends in these variables and account for omitted trends in outcomes that might be correlated with the introduction of SP.[22] Sixth, we control for linear municipality specific trends. Seventh, we control for state cubic trends. Eighth, we include state-year fixed effects. Finally, we control for the number of years since the implementation of *Oportunidades* in the municipality, since the program underwent the urban expansion in the same years in which SP was rolled out.

As we study effects on a relatively large number of outcomes, for each table presented we adjust inference for multiple hypotheses testing. To do so, we use the free step-down resampling method to account for family–wise error rate and report the adjusted p-values as in Westfall and Young (1993) and Kling, Liebman and Katz (2007).[23]

Lastly, we deal with an additional concern, that of selective migration of uninsured individuals residing in municipalities not yet providing SP to municipalities already offering it. We investigate this possibility using data from the extended questionnaire of the 2010 CENSUS, which surveys 2.9 millions households. We use the sample of households with working age heads (ie, 21 to 65 years old), and we regress an indicator for whether they moved between 2005 and 2010 on an indicator for whether the municipality of residence in 2010 started offering the program between 2002 and 2004. We control for characteristics of the household (quadratic for the age of the head, gender of the head, presence of children less than 18, an indicator for whether the head is married or living in partnership, the level of education) and fixed effects for the municipality of residence in

[22]We estimate municipality-specific trends using data before the implementation of SP, and we obtain a slope estimate λ_{ms} for each municipality. We then extrapolate the pre-expansion time trends to the post-reform period as follows (see also Bhuller et al. (2013)):

$$y_{mst} = \beta_1 SP_{mst}\mathbf{1}[t - T_{sm} \leq -2] + \beta_2 SP_{mst}\mathbf{1}[0 \leq t - T_{sm} \leq 2] + \beta_3 SP_{mst}\mathbf{1}[t - T_{sm} \geq 3] + \widehat{\delta\lambda_{ms}} t + \mu_{ms} + \pi_t + \varepsilon_{mst}.$$

[23]We use 1000 bootstrap replications to obtain the adjusted p-values, the exception are the two tables that control for municipality specific trends, to which 500 bootstrap replications are used (Table B.11 and Table B.19).

2005. We do not find evidence of cross-municipality migration induced by SP (results available upon request).[24]

6. Results

6.1.Impacts on Mortality

We start by presenting estimates of the impacts of SP on mortality in Table 3, where we report estimates of equation (3) for all municipalities (Panel A) and separately by the level of poverty of the municipality (in Panels B and C for poor and rich municipalities, respectively). In column (1) we include the estimates for the age-adjusted mortality rate, and columns (2) to (6) include the estimates for age-specific mortality rates for the five groups studied (ie, IMR, ages 1-4, 5-19, 20-59, 60+). The estimates for the whole sample do not show any impact of SP for any age group studied. However, estimates for poor municipalities show a reduction in 0.119 deaths per 1,000 individuals three or more years after the implementation of SP. Column (2) shows that this is driven by a reduction in 1.549 deaths per 1,000 live births, which given a baseline mortality rate of 15.36 deaths per 1,000 live births, corresponds to a 10% decline; and by a reduction of 0.12 deaths per 1,000 in adult mortality, corresponding to a 3.6% reduction in mortality for individuals 20-59 years old (column 5). The statistical significance of the impacts on overall age-adjusted mortality and IMR in poor municipalities survives multiple hypotheses testing adjustment.[25] There are no detectable impacts for any other age groups in poor municipalities, or for rich municipalities.[26]

The full event study estimates from equation (2) are plotted in Figure 2, in Panels A, B and C for the all, poor and rich municipalities, respectively. In the bottom of each graph we include the *p-values* for the null hypotheses of no effect before and after the introduction of SP in a municipality. Panel B shows that, for poor municipalities, there is no significant evidence of a differential trend in mortality

[24]Based on panel data collected for families in a subsample of Mexican municipalities, Mahé (2019) finds that families in municipalities that experienced a significant change in program coverage compared to municipalities that did not are more likely to have migrated between 2005 and 2009. Her treatment definition has not been used elsewhere in the SP-related literature.

[25]The increase in elderly mortality in Table 3 is not statistically significant when adjusting inference for multiple hypotheses testing (p-value 0.138; column 6).

[26]The impact on IMR detected three or more years after the implementation of SP in poor municipalities is statistically different from the corresponding estimate for rich municipalities (Table B.7 in the Appendix).

in treated locations before the introduction of SP, with the coefficients for the pre- program years being all statistically insignificant. Instead, after the introduction of SP, the infant mortality rate fell sharply in poor municipalities, with statistically significant impacts detectable after two years. On the other hand, we detect no significant impact of SP on infant mortality for all or rich municipalities (Panels A and C). In Figure A.8 in Appendix we present the corresponding full event study estimates for the impact of SP on mortality at other ages; namely, the impacts on mortality at ages 1-4 (Panel A), 5-19 (Panel B), 20-59 (Panel C) and among the elderly (60+; Panel D). The estimates in the graphs do not show any significant impact of SP on mortality for any age group.

Our main results are obtained by weighted least squares, where the weight is the population per age group in 2000 in the municipality (see section 5). Nevertheless, we have also included unweighted estimates in Appendix Table B.8. For these unweighted estimates we cannot detect program impacts (except for a reduction in mortality of children 1 to 4 years old in poor municipalities). However, municipalities that implemented earlier SP are more populous (see Table 1), thus, the longer term impacts are identified from more populous municipalities. In the unweighted estimation large and small municipalities are weighted equally (in 2000, the standard deviation of population was 120,000 inhabitants).

Given that SP seems to have successfully reduced IMR in poor municipalities, in the rest of the paper we mainly focus on understanding the mechanisms behind such finding.

As eligibility itself can be affected by the introduction of the program,[27] we do not restrict our estimation sample to eligible individuals. Nevertheless, we examine whether the reduction in IMR in poor municipalities is driven by the sample of infants eligible to SP, i.e. in those families without access to Social Security. The results are presented in Table B.9 in the Appendix, that include estimates of model (3) by eligibility status for the three samples of municipalities. The results in column (4) show that the decrease in infant mortality in poor municipalities is indeed concentrated among the eligibles, and that in these municipalities SP does not have impacts among the non-eligibles (column 3) – however, the estimates are very imprecise for this group.[28] Additionally, the reduction

[27]The program has been associated with a small increase in informality rates, that is, an increase the share of families eligible to SP (see section 2).

[28]An alternative interpretation of this finding is the absence of spillover effects on the non-eligibles. This is not unexpected: given that the two systems (SP and IMSS/ISSSTE) delivered care in two completely separate networks of hospitals and health centers, there was virtually no scope for contamination. Additionally, we study a sample of children who do not attend school yet, so also this channel of potential contagion can be ruled out.

in infant mortality among the eligibles is detected immediately and amounts to 1.253 and 1.680 fewer infant deaths per 1,000 live births soon after the introduction of the program and three years after its implementation, respectively. This corresponds to a reduction of 8-11%, from a baseline of 15.2 deaths per 1,000 infants among eligibles. Throughout the paper we refer to the ITT estimates, i.e., to the average effect of SP among all individuals in the municipality; however, since the program achieved universal coverage in 2012, the effect on the eligibles is indeed the implied average treatment effect on the treated (ATT) for infant mortality.

Sensitivity Analysis We now investigate the robustness of our findings to different specifications of equation (3). We start by subjecting the estimates on IMR in poor municipalities to a battery of specification checks, as this is the largest impact detected in Table 3. These results are displayed in Table B.10. Column (1) reports our baseline estimates; columns (2) to (10) are the alternative specifications described in subsection 5.2. The fact that our estimates are virtually unchanged across the various columns of Table B.10 provides evidence that the decline in infant mortality in poor municipalities was driven by SP and not by local shocks or underlying trends.[29]

In Table B.11 we present estimates for equation (3) using our most demanding specification, which controls by a municipality-specific linear trend. Panels A and C do not show any effect of SP on mortality for any age group on all or rich municipalities. For IMR in poor municipalities (Panel B – col. 2), the adjusted p-value for $\widehat{\beta_3}$ is 0.106.

In Table B.12 in the Appendix we show that the impacts on infant and adult mortality in poor municipalities are not driven by the definition of introduction of SP, which relies on at least 10 families enrolled in the program in the municipality. The impacts are similar if three alternative thresholds to assign SP to a municipality are used: 5, 15 and 20 families enrolled in the program.

We also conduct a randomization inference test for the significant estimates on IMR, in the spirit of a placebo test. To do so, we randomly assign the year of implementation for each municipality using 1,000 permutations (Duflo, Glennerster and Kremer, 2006). In accordance with MacKinnon and Webb (2019), we present randomization inference results based on t-statistics, as this is superior to inference based on coefficients. Figure A.9 in the Appendix plots the distribution of

[29]Our sample is a balanced panel of municipalities, where the number of deaths is set to 0 in years in which no deaths are recorded. The results are similar if we restrict the sample to municipalities that always have non-zero deaths in the 15 years under analysis: β_3 is -1.468 (standard error 0.540).

placebo treatment effects; the actual t-statistics for β_3 in equation (3) is the vertical line, while dashed lines are the 5th and 95th percentiles of the distribution of placebo treatment effects. The distribution of placebo treatment looks smooth and the solid line allows to reject the null of no effect.

Lastly, it is possible that infant deaths are measured with error in the administrative records; in particular, that they are under-reported. Two situations are possible. First, if under-reporting is systematically correlated with permanent local conditions which also affect mortality, then this is accounted for by the municipality fixed effects. Second, a more serious concern would arise if the introduction of SP affected the quality of reporting; more precisely, if it led to an improvement in the recording of deaths since health services become more accessible. Reassuringly, we find no evidence that the introduction of SP in a municipality affects the proportion of missing information about the place of reported death.[30] In any case, if the reporting of infant deaths improves with SP, then our findings underestimate the impacts of the program.

6.2. Mechanisms: Understanding the Reduction in Infant Deaths

After having established that the introduction and expansion of SP led to a significant decline in infant mortality, we investigate possible mechanisms through which this reduction might have occurred.

Mortality due to Specific Conditions In Table 4 we re-estimate specification (3) separately by cause of death to pin down which ones are driving the reduction in infant mortality in poor municipalities. In columns 2-5 of the table we present four types of conditions, which account for 90% of all infant deaths; the remaining 10% of infant deaths are scattered across different categories which we aggregate in column 6, due to lack of power to study them separately. Column (2) of Table 4 shows that SP led to a significant reduction of 0.380 deaths due to intestinal and malnutrition-related conditions (ICD10 codes A and E, respectively) and respiratory infections (ICD10 codes J, predominantly influenza and pneumonia), which represented 26% of all infant deaths in 2000. This

[30] To do so, we re-estimate equation (3) using as dependent variable the share of missing information about the place of reported death of the infant and we obtain the following estimates for β_1, β_2 and β_3, respectively (standard errors in parenthesis): -0.004 (0.005), 0.011 (0.006), 0.007 (0.010). The p-value for the null hypothesis $H_0 : \beta_2 = \beta_3 = 0$ is 0.131.

corresponds to a reduction of nearly 10% in IMR due to these conditions.[31] Importantly, most of the conditions causing these deaths have been covered by SP since its introduction. The *Catalogos de Beneficios Medicos (CABEME)* (2002-2003) includes, among others, "diagnosis and treatment of acute respiratory infections", "diagnosis and treatment of acute diarrhea", and "monitoring of nutrition, growth and well-baby visits". Indeed, Knaul et al. (2012) report that, between 2000 and 2006, coverage and effective coverage of SP have increased for a variety of conditions, including *treatment of diarrhoea and acute respiratory infections in children*, concentrated in the poorest states and income deciles. This is precisely what is shown in the bottom three rows of the table, where we include the share of deaths in 2002, 2006 and 2010 which are attributable to conditions covered by SP, within group of medical conditions.

Column (3) of Table 4 shows that SP is associated with a 12.4% reduction in infant deaths due to conditions originating in the perinatal period (ie, a reduction in 0.854 deaths/1,000 live births), that represented 45% of the infant deaths in 2000. The most prevalent of these conditions in poor municipalities in 2000 are the following five, which account for two thirds of the related deaths: birth asphyxia (ICD10 P21), which is most commonly due to a drop in maternal blood pressure or some other substantial interference with blood flow to the infant's brain during delivery; respiratory distress of newborn (ICD10 P22), that is, any signs of breathing difficulties in the neonate; congenital pneumonia (ICD10 P23); neonatal aspiration syndromes (ICD10 P24), which occur when fluids, typically meconium, is present in the lungs of the baby during or before delivery; and, finally, bacterial sepsis of newborn (ICD10 P36), which refers to the presence of a bacterial blood stream infection in the newborn (such as meningitis, pneumonia, pyelonephritis, or gastroenteritis).[32] Early identification and treatment of neonates at risk of infection or with symptoms of infection reduces both morbidity and mortality (Gallacher, Hart and Kotecha, 2016). Neonatal aspiration syndromes are difficult to prevent before birth, thus identification of risk factors and assisted delivery are associated with decreases mortality due to these conditions (Usta and Sibai, 1995).

Finally, the decrease in IMR can also be attributed to the reduction in deaths due to congenital malformations (ie, medical conditions associated with ICD10 codes Q). SP is associated with a

[31] In Table 4, we pool together ICD10 codes A and E since they are strictly related, however, given that only the main cause of death/admission is reported in the Mexican data, malnutrition is less likely to be cited (see e.g. Rice and Black (2000)). We also bundle together ICD10 codes A, E and J, due to the link between gastrointestinal and respiratory diseases (see Budden et al. (2016)).

[32] The symptoms of congenital pneumonia are similar to those of sepsis, and these include signs of respiratory distress accompanied by temperature instability.

reduction of 0.387 deaths/1000 live births, which represents a 17.5% reduction in deaths due to these conditions (column (4) – Table 4). Among these, malformations of the circulatory system (coded ICD10 Q20—Q28) are the most prevalent, accounting for nearly 40% of deaths due to congenital malformations.

While conditions associated with respiratory and intestinal infections have been covered since the introduction of the program (see the bottom three rows of the table), perinatal conditions and congenital malformations were not covered initially, but have been part of the FPGC since 2004. Additionally, hospital deliveries were covered, which could have contributed to a further reduction in mortality due to these conditions via timely diagnosis, as we show below. The estimates for β_3 in columns (2)-(4) of Table 4 are statistically significant after accounting for multiple hypotheses testing.

Reassuringly, column (5) shows no impact of SP on deaths due to external causes (e.g., accidents), which at this age group occur due to conditions not covered by SP (see the panel in the bottom of the table).

Use of Hospitals by Infants and Pregnant Women As seen above, the introduction of SP was associated with a decrease in infant mortality due to three types of conditions: intestinal and respiratory infections, conditions originating in the perinatal period and congenital malformations. We now turn to the impacts on access to medical care associated with SP. Dafny and Gruber (2005) notice that greater access to care may increase hospitalizations, however improved efficiency of care for newly eligible children might also reduce them. Using data from the universe of SSA hospital discharges, Table 5 shows that the introduction of SP led to an immediate 7% increase in hospital admissions for infants in poor municipalities, from a pre-program mean of 15 admissions/municipality in 2000 (column 1). As in Dafny and Gruber (2005), the access outweighs the efficiency effect as consequence of the introduction of SP. Complementary evidence from the universe of discharges from any public hospital in Mexico presented in Panel A of Figure 3 shows that the increase in hospital admissions for infants is only detectable in the Ministry of Health units, whereas there is a slight decrease in admissions in hospitals run by all other public providers (non-SSA).[33] Table B.13 in the Appendix shows that this effect of SP is robust to the same nine alternative specifications to which we subjected the estimates for IMR.

Columns 2-5 of Table 5 show that the increase in hospital admissions before age 1 is driven by admissions due to intestinal and malnutrition-related conditions and respiratory infections (column 2). There are no impacts on admissions due to external causes (column 5) - consistently with the evidence we find for infant mortality – but also no impacts on admissions due to perinatal conditions and congenital malformations (columns 3 and 4, respectively). We turn to these two types of conditions in the following paragraph in more detail. Columns 6 and 7 of Table 5 show that the introduction of SP led to no detectable change in the length of stay, but it significantly increased the share of admissions through E.R.

As mentioned above, part of the decrease in IMR is due to perinatal conditions and congenital malformations, although we are unable to detect a corresponding increase in hospital admissions due to such conditions. These conditions can be either triggered or detected during delivery, and morbidity and mortality can be reduced with immediate treatment. Since SP covers hospital births, in columns 8-12 of Table 5 we examine its impacts on all obstetric-related admissions (coded ICD10 O) to SSA hospitals among women 15-44 years old. We consider four types of obstetric admissions: births (IDC10 O80-84) are included in column (9), conditions related to the fetus and amniotic cavity and possible delivery problems (ICD10 O30-48) are in column (10), complications of labor and delivery (ICD10 O60-75) are in column (11), whereas all other obstetric-related ad- missions are included in column (12). The impact on overall obstetric admissions is immediate and it strengthens with exposure to the program (see also Sosa-Rubí, Galárraga and Harris (2009)). In particular, obstetric-related admissions increase by 6.8% in the first two years of operation and by 11.5% after two years (column 8). Among these, the impact is stronger for deliveries and it varies from 10 to 14.2% (column 9), whereas it is slightly weaker in magnitude, but still significant, for all other types of obstetric admissions (columns 10-12).

Using data from deliveries that occurred in all public hospitals in Mexico, in Panel B of Figure 3 we show that while deliveries in SSA units increased between 2004 and 2012, they remained nearly stable in non-SSA units. Furthermore, using the Mexican health survey ENSANUT (2000, 2006 and

[33]This alternative data source only contains information from 2004 onward, hence it does not allow us to control for pre-SP trends and it only contains information at state level, so we cannot report two separate figures for rich and poor municipalities. Additionally, in Table B.18 and Table B.19 we show the impacts on admissions to hospitals managed by SSA for all age groups studied. Also, Figure A.10 in Appendix presents the corresponding admissions to all public hospitals for children ages 1-4, adults 15-64 and among elderly (65+).

2012), in Table B.16 we provide suggestive evidence that in poor municipalities the increase in deliveries in SSA hospitals is due to births which would have occurred at home in the absence of the SP (Bernal and Grogger, 2013*a*,*b*). This table has three columns for three mutually exclusive places of delivery: birth at SSA hospital (col. 1), birth at an hospital managed by other public or private provider (col. 2) or at other place (col. 3; typically home). Information in the data is only available for infants and, due to sample size, we cannot separately estimate the model for poor and rich municipalities, instead we interact the treatment variable with the indicator for the type of municipality. Finally, we do not detect any impact of the program on the number of births, corroborating the fact that the increase in hospital deliveries is due to a shift and not to an overall increase in fertility (see Table B.17).

In sum, access to skilled delivery and emergency obstetric and neonatal care provided under SP are likely to be the reason behind the decrease in deaths due to congenital malformations and perinatal conditions.[34]

6.3. Supply of Health Care and Timing of Effects

So far, we have mainly studied changes in the use of hospital services. But what happened to the supply of health care services? The launch of SP was accompanied by a strengthening of the health infrastructure (section 3). To study whether the introduction of SP in a municipality affected the availability of services, we use data which includes the information on medical personnel and the number of outpatient visits in all medical units (hospitals and health centers) run by SSA-Ministry of Health, which offer the SP services. To undertake this analysis, we use data at municipality- level since 1998, rather than data at the unit level, as such information is only available from 2001 onward (see section 4); however, in either dataset there is no information on outpatient visits disaggregated by age. Table B.20 in the Appendix includes the estimated impact on the (log) of medical personnel per capital and visits per doctor/nurse. The table includes two panels: Panel A presents estimates for model (3) and Panel B has estimates for a specification which includes also a municipality-specific linear trend. Columns 1-3 show an increase in the personnel in SSA units in poor, but not in rich, municipalities; such increase in poor municipalities is robust to the inclusion of

[34] We note that no impact in hospital admissions among infants or for obstetric related conditions is detected for the samples of all (Table B.14) or rich municipalities (Table B.15).

a municipality linear trend (Panel B). Columns 4-6 show no impact on the number of visits per doctor. The combined estimates presented in Panels A and B of Table B.20 show that the increase in the health personnel did not translate into a reduction in the number of outpatient consultations of those delivering outpatient services in SSA in poor municipalities.[35]

To understand why we detect immediate impacts of the program on the use of hospital services, we resort to the *Padrón* and examine the association between several household characteristics and the year of enrolment in SP. The results, reported in Table B.21 in the Appendix, show that the households who enroll earlier in the program within a municipality are more likely to be among the poorest (i.e., in the 1st decile of the national income distribution), headed by a female, with a head having less than primary education, with a disabled member, a larger family, with children 0-4 years old, and enrolled in *Oportunidades*.[36] In other words, earlier entrants are in a condition of disadvantage with greater potential benefits from access to health care.

Interestingly, part of the reduction in IMR is driven by intestinal and malnutrition-related conditions and respiratory infections (subsection 6.2). The treatment of these medical conditions had been already covered by the health component of *Oportunidades* in municipalities offering the program. Progresa/*Oportunidades* beneficiaries receive free of charge the Guaranteed Basic Health Package *(Paquete Basico Garantizado de Salud*). The package available to *Oportunidades* beneficiaries offers free of charge a set of age-specific interventions, including periodic check-ups and vaccinations for children. Deliveries and perinatal care are also included in the package, as well as five prenatal checks for pregnant women and identification of risky pregnancies. Information about preventive health behaviors is provided through community workshops, and emergency services are secured by the Ministry of Health, IMSS-Oportunidades (the dedicated network of medical units for families enrolled in the program) and other state institutions; lastly, beneficiary families protected by Social Security have also access to second- and third-level care in the units administered by IMSS, while those unprotected have only limited access to second-level care.[37] However, the Progresa/*Oportunidades* guidelines are mute about the treatment of high cost conditions such as

[35]Knox (2015) also finds an increase in the use of outpatient services.

[36]Of the total of 17.6 million families observed in the data, about 816,000 are assigned to IMSS-Oportunidades centers when they enroll in SP (less than 5% of the families), among the 3.7 million families that entered SP through the *Oportunidades* program (about 22% of the total).

[37] For the legislation of *Oportunidades* see http://www.normateca.sedesol.gob.mx/es/NORMATECA/Historicas (accessed July 3, 2020).

certain conditions specific to the perinatal period and congenital malformations. Hence, we investigate a possible interaction in the coverage offered by the two programs in Table B.22 in Appendix; the table includes estimates of model (3), interacting the three treatment indicator variables with indicators for whether the municipality had a high or low coverage of *Oportunidades* in 2001 (just prior to the launch of SP; a municipality is defined as having ”high coverage” if at least 40% of the families are enrolled in the program, which is the coverage of the median poor municipality). The estimates in the table are imprecise, but the impacts of SP in poor municipalities are still significant when we allow them to vary by the level of coverage of *Oportunidades* in the municipality. The table suggests that the impacts are driven by municipalities with higher coverage of *Oportunidades*, that are also the most deprived in the country. In such municipalities, SP has gains over and above those of *Oportunidades* (see Barham, 2011).

7. Conclusion

In this paper, we have contributed to the ongoing debate on universal health coverage by estimating health impacts and mechanisms of the Mexican health insurance program *Seguro Popular*. Building on the previous literature, we have used a unique combination of administrative and survey data and exploited the temporal and spatial variation arising from the introduction of SP in all the municipalities in Mexico. While we have investigated impacts on infants (<1), children (1-4), children and adolescents (5-19), adults (20-59) and elderly (60+), we have detected robust effects of SP only for infants in poor municipalities.

Our intent-to-treat estimates show that the introduction of SP led to a significant reduction in infant mortality by 10% in poor municipalities. This amounts to avoid the deaths of approximately 976 babies before age 1 per year. The impact of SP is detected since three years after the introduction of the program in a municipality and is robust to a variety of alternative specifications. Part of the reduction in infant mortality is driven by preventable conditions, namely respiratory and intestinal infections, which can be cured with timely access to medicines, and which have been covered by the program since 2002. Another part of the reduction in infant mortality can also be attributed to conditions specific to the perinatal period and congenital malformations, which de- crease the probability of survival in case of unassisted births or deliveries by unskilled personnel and, thus, can be diagnosed and treated in case of a hospital delivery.

We have also examined potential mechanisms which might have driven these impacts in poor municipalities, investigating the role played by demand and supply of health services. We have shown that the introduction of SP led to an increase in hospital admissions among infants, for whom we find a reduction in deaths. We have also shown that SP led to an increase in hospital births, which would have otherwise occurred outside the medical system, and in other obstetric-related admissions. In rich municipalities we do not detect any effects of SP on mortality (in any age group) or on the use of hospital care. Additionally, we provide evidence that the program was rolled out gradually starting in municipalities which had adequate pre-existing supply.

Our findings suggest caution in focusing uniquely on the provision and delivery of health care for the promotion of the health of population. While it is certainly a necessary input, health insurance alone is not sufficient at improving population's health, and successful health policies need to consider the wider social determinants. Additionally, while reaching full coverage in only nine years of operation has been a major achievement, the implementation of SP still faces significant challenges (Nigenda et al., 2015). Nonetheless, our results suggest that universal health coverage, by providing access to hospital deliveries and treatment of risky pregnancies, and also to preventive care with cheap timely treatment, can significantly contribute to reduce the gap in mortality for poor infants in less developed countries. For the Mexican case, SP closed nearly all the gap in infant mortality between poor and rich municipalities.

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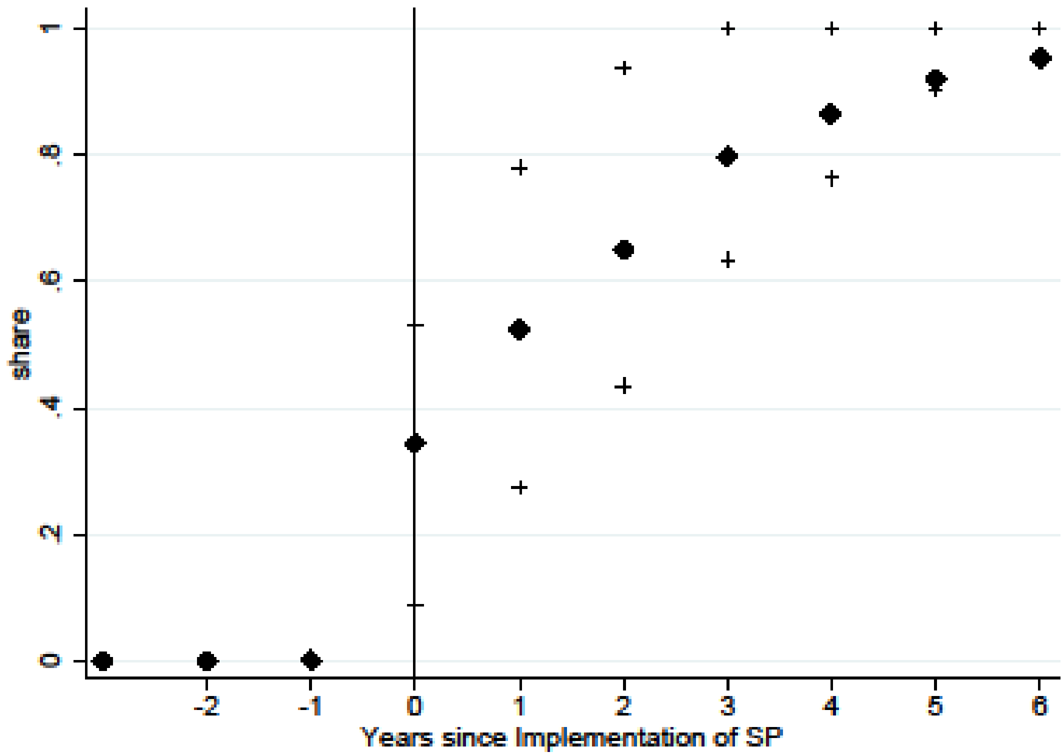
Figures and Tables

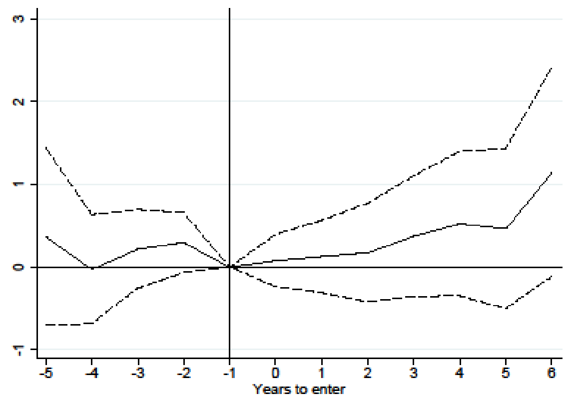
Figure 1: Average Share of Families Eligible Enrolled in SP.



Note: The figure includes the mean of the share of families eligible to SP enrolled in the program (black dots) in each year around the introduction of SP in a municipality (year 0). The plus signs are the percentiles 25 and 75 of this share. Source: Own calculations from the Padron (the administrative data of all households affiliated to SP).

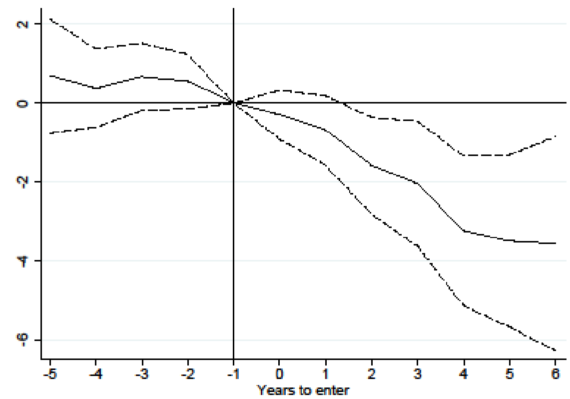
Figure 2: Impact of SP on Infant Mortality, by Poverty of the Municipality.

Panel A: All Municipalities



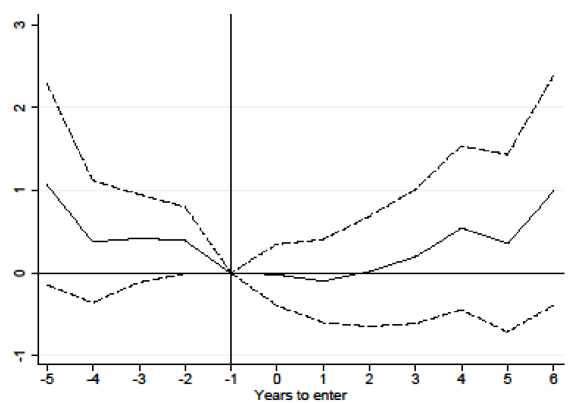
p-value $H_0 : \beta_{-5} = ...\beta_{-2} = 0$: 0.061
p-value $H_0 : \beta_0 = ...\beta_6 = 0$: 0.155

Panel B: Poor Municipalities



p-value $H_0 : \beta_{-5} = ...\beta_{-2} = 0$: 0.504
p-value $H_0 : \beta_0 = ...\beta_6 = 0$: 0.000

Panel C: Rich Municipalities



p-value $H_0 : \beta_{-5} = ...\beta_{-2} = 0$: 0.093
p-value $H_0 : \beta_0 = ...\beta_6 = 0$: 0.118

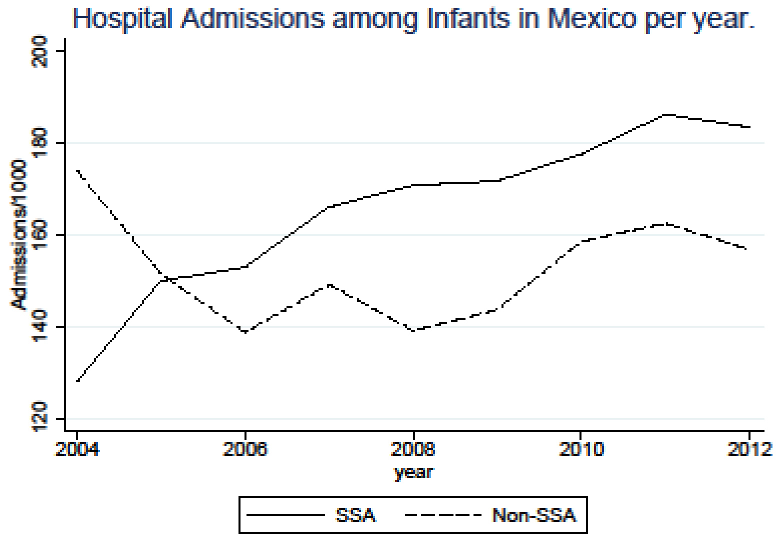
Note: The figures plot weighted least square estimates of XX from specification (2):

$$\gamma_{mst} = \sum_{k=-K}^{-2} \beta_k^B SP_{mst} \mathbf{1}[t - T_{sm} = k] + \sum_{k=0}^{L} \beta_k^A SP_{mst} \mathbf{1}[t - T_{sm} = k] + \mu_{ms} + \pi_t + \varepsilon_{mst}$$

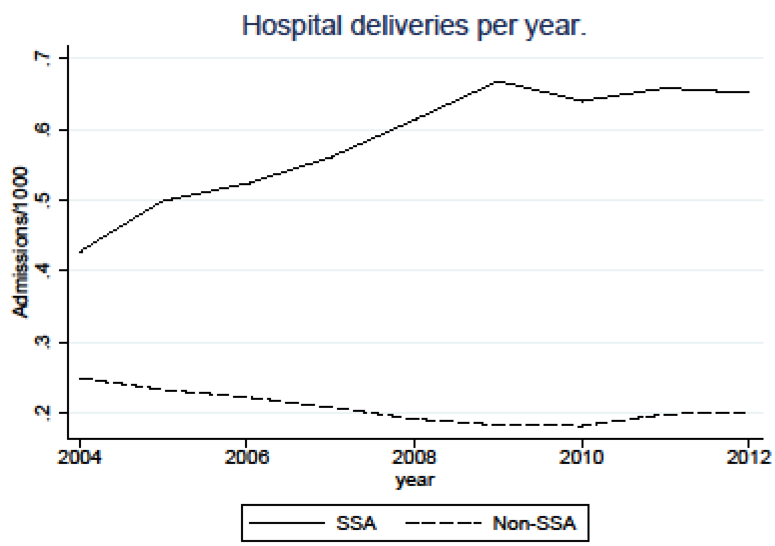
where SP_{mst} is an indicator variable equal to 1 if the municipality of residence m in state s offers SP in year t. T_{sm} is the year of implementation of the program. The exact values of k depend on the number of years available in the data, before (K) and after (L) the implementation of SP. We assume constant effects for five or more years before introduction of SP (so K = 5) and six or more years of exposure (L = 6). The dependent variable is the infant mortality rate. The dashed lines are 95% confidence intervals. In the figure the p-values for the null hypotheses tests are: (1) XXX . Data source: Mortality Registry 1998-2012.

Figure 3: Hospital Admissions due to Births and among Infants in SSA and non-SSA Hospitals.

Panel A: Infants (children < 1 year old).



Panel B: All Deliveries (ICD10 O80, O81, O82, O83 and O84).



Note: Panel A includes all admissions among infants. Panel B shows the number of hospital admissions in all public hospitals in Mexico between 2004 and 2012 for deliveries. "SSA" includes all hospital admission in SSA (Ministry of Health) units. "Non-SSA" includes all hospital admissions in hospitals not run by SSA (IMSS, *IMSS-Oportunidades*, ISSSTE, PEMEX and the military). Note that, even if *IMSS-Oportunidades* provides medical services to *Oportunidades* people covered by SP, in this figure we bundle them into the "Non-SSA" category since they are not included in the hospital discharges data, to make the two categories comparable.

Table 1: The Determinants of the Timing of the Municipality Rollout of SP (Levels).

	(1)	(2)	(3)
Socio-demographic and Political Indicators (2000)			
Log population	-2.103	-0.3923***	-0.3285***
		[0.0229]	[0.0246]
Marginalization Index	0.004	0.4634***	0.2177***
		[0.0297]	[0.0374]
% eligible population	75.18	0.0178***	0.0037*
		[0.0018]	[0.0020]
% of population 0-4 years of age	0.113	4.4473***	-2.3058
		[1.5158]	[1.4370]
Alignment b/w party in power in municipality and state in t=0	0.245	-1.3876***	-0.7735***
		[0.0726]	[0.0875]
Supply of Health Care (2001)			
No. Hospitals (per 100,000 eligible)	0.575	-0.0618***	-0.0345**
		[0.0172]	[0.0160]
No. Health Centers (per 100,000 eligible)	37.450	-0.0039***	-0.0013*
		[0.0007]	[0.0007]
No. Doctors in Hospitals (per 100,000 eligible)	20.340	-0.0025***	-0.0016***
		[0.0005]	[0.0003]
Observations		2,382	2,382
State Fixed Effects		No	Yes

Note: Column (1) presents the mean for each variable. Each cell in column (2) presents the estimated coefficient from a linear regression of the year of entry of SP in a municipality on a pre-program characteristic. Column (3) controls for state fixed effects. Robust standard errors in parentheses. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table 2: Relationship between SP introduction and Mortality Rates

	(1)	(2)	(3)	(4)	(5)	(6)
Age Group	All	0	1 to 4	5 to 19	20 to 59	60+
Panel A: 2002 MR						
Year of Introduction	-0.003	-0.179	0.012	0.002	-0.019	-0.426***
	(0.026)	(0.225)	(0.012)	(0.004)	(0.016)	(0.145)
Adjusted *p-values*	[0.994]	[0.964]	[0.917]	[0.986]	[0.890]	[0.099]
Panel B: 1998-2002 change in MR						
Year of Introduction	-0.012	0.053	-0.009	-0.009*	-0.017	-0.000
	(0.012)	(0.126)	(0.014)	(0.005)	(0.012)	(0.105)
Adjusted *p-values*	[0.924]	[0.986]	[0.977]	[0.533]	[0.762]	[1.000]
Observations	2,382	2,382	2,382	2,382	2,382	2,382

Note: Each cell in Panel A presents the estimated coefficient from a linear regression of the year of entry of SP in a municipality on the 2002 Age Adjusted Mortality Rate for all ages (column 1) and for specific ages (columns 2 to 6), controlling for state fixed effects. In Panel B, instead of using the 2002 Mortality Rate, we use the change between mortality rates in 1998 and 2002. The p-values in brackets are adjusted for multiple hypotheses testing. Robust standard errors in parentheses. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table 3: Impact of SP on Mortality at Different Ages.

Age Group	(1) All	(2) 0	(3) 1 to 4	(4) 5 to 19	(5) 20 to 59	(6) 60+
Panel A: Whole Sample						
Up to 2 years before SP (β_1)	-0.008	0.339**	0.392	-0.001	-0.000	-0.267
	(0.020)	(0.171)	(0.848)	(0.007)	(0.021)	(0.167)
Adjusted *p-values*	[1.000]	[0.230]	[1.000]	[1.000]	[1.000]	[0.579]
0 to 2 years after SP (β_2)	0.003	0.004	-0.386	-0.005	-0.012	0.156
	(0.015)	(0.145)	(0.447)	(0.006)	(0.015)	(0.135)
Adjusted *p-values*	[1.000]	[1.000]	[0.990]	[0.993]	[0.994]	[0.933]
3 or more years after SP (β_3)	0.001	0.214	-0.697	-0.002	0.001	-0.116
	(0.027)	(0.247)	(0.476)	(0.011)	(0.033)	(0.207)
Adjusted *p-values*	[1.000]	[0.990]	[0.679]	[1.000]	[0.126]	[1.000]
p-value $H0: \beta2 = \beta3 = 0$	0.943	0.419	0.335	0.551	0.443	0.013
Mean in 2000	4.500	14.62	2.915	0.480	2.992	30.20
S.D.	1.677	16.60	6.904	0.532	1.664	10.03
Observations	35,730					
Nb. Municipalities.	2382					
Panel B: Poor Municipalities						
Up to 2 years before SP (β_1)	0.039	0.239	0.255	-0.003	0.032	0.228
	(0.029)	(0.288)	(0.243)	(0.012)	(0.034)	(0.229)
Adjusted *p-values*	[0.828]	[0.993]	[0.969]	[1.000]	[0.978]	[0.976]
0 to 2 years after SP (β_2)	0.011	-0.381	0.063	-0.009	-0.008	0.410*
	(0.026)	(0.286)	(0.141)	(0.012)	(0.035)	(0.216)
Adjusted *p-values*	[1.000]	[0.828]	[1.000]	[0.998]	[1.000]	[0.297]
3 or more years after SP (β_3)	-0.119***	-1.549***	-0.494	-0.000	-0.120**	-0.096
	(0.042)	(0.499)	(0.326)	(0.018)	(0.055)	(0.316)
Adjusted *p-values*	[0.012]	[0.003]	[0.654]	[1.000]	[0.126]	[1.000]
p-value $H0: \beta2 = \beta3 = 0$	0.000	0.001	0.095	0.562	0.004	0.006
Mean in 2000	4.519	15.36	1.756	0.502	3.297	28.38
S.D.	1.933	19.23	2.966	0.606	1.986	11.39
Observations	18,900					
Nb. Municipalities	1260					
Panel C: Rich Municipalities						
Up to 2 years before SP (β_1)	0.001	0.483**	0.683	0.000	-0.008	-0.121
	(0.022)	(0.194)	(1.065)	(0.009)	(0.024)	(0.178)
Adjusted *p-values*	[1.000]	[0.039]	[0.999]	[1.000]	[1.000]	[0.999]
0 to 2 years after SP (β_2)	-0.007	-0.016	-0.552	-0.004	-0.010	-0.020
	(0.018)	(0.173)	(0.571)	(0.007)	(0.017)	(0.158)
Adjusted *p-values*	[1.000]	[1.000]	[0.976]	[1.000]	[0.999]	[1.000]
3 or more years after SP (β_3)	0.012	0.432	-0.759	-0.002	0.022	-0.197
	(0.032)	(0.292)	(0.589)	(0.013)	(0.039)	(0.243)
Adjusted *p-values*	[1.000]	[.690]	[0.836]	[1.000]	[1.000]	[0.994]
p-value $H0: \beta2 = \beta3 = 0$	0.426	0.050	0.435	0.769	0.200	0.421
Mean in 2000	4.478	13.78	4.217	0.455	2.650	32.24
S.D.	1.333	12.99	9.389	0.432	1.109	7.762
Observations	16,830					
Nb Municipalities	1122					

Note: This table displays weighted least squares estimates of specification (3). The dependent variable y_{mst} is the mortality rate in municipality m of state s in year t. Each column presents results for separate weighted regressions, where the weights are given by the population by age group in municipality m in state s in 2000 in columns 1, 3-6 (and by births in 2000 in column 2). Controls include fixed effects for year (π_t) and municipality of residence (μ_{ms}). The p-values in brackets are adjusted for multiple hypotheses testing. Standard errors (in parentheses) are clustered at the level of the municipality. *** Significant at 1%, ** Significant at 5%, * Significant at 10%. In Data source: Mortality Registry 1998-2012.

Table 4: Impact of SP on Infant Mortality, By Condition (Sample of Poor Municipalities).

	(1) All	(2) Bacterial/Intestin. Malnutrition Respiratory (ICD10 A E J)	(3) Perinatal (ICD10 P)	(4) Congenital (ICD 10 Q)	(5) External Causes (ICD10 V, W, X)	(6) All other conditions not in, 2-5
Up to 2 years before SP (β_1)	0.239	0.212	0.173	-0.023	-0.005	-0.118
	(0.288)	(0.137)	(0.185)	(0.094)	(0.045)	(0.072)
Adjusted *p-values*	[0.781]	[0.264]	[0.766]	[0.986]	[0.986]	[0.217]
0 to 2 years after SP (β_2)	-0.381	-0.133	-0.239	-0.071	0.082*	-0.019
	(0.286)	(0.114)	(0.202)	(0.099)	(0.046)	(0.074)
Adjusted *p-values*	[0.448]	[0.619]	[0.619]	[.817]	[0.147]	[0.986]
3 or more years after SP (β_3)	-1.548***	-0.380**	-0.854***	-0.387**	0.061	0.011
	(0.499)	(0.186)	(0.320)	(0.168)	(0.068)	(0.112)
Adjusted *p-values*	[0.000]	[0.057]	[0.004]	[0.018]	[0.772]	[0.986]
p-value $H_0 : \beta_2 = \beta_3 = 0$	0.001	0.111	0.004	0.033	0.149	0.867
Mean in 2000	15.36	3.979	6.907	2.214	0.774	1.491
S.D.	19.23	7.471	12.97	4.765	4.174	5.182
Observations	18,900	18,900	18,900	18,900	18,900	18,900
Nb. Municipalities	1260	1260	1260	1260	1260	1260
% of all Deaths	100 %	26 %	45 %	14 %	5 %	10 %
Covered by SP by 2002?	17 %	59 %	1 %	0 %	0 %	3 %
Covered by SP by 2006?	50 %	75 %	46 %	50 %	0 %	44 %
Covered by SP by 2010?	71 %	73 %	80 %	76 %	0 %	61 %

Note: This table displays weighted least squares estimates of our baseline specification (3) on the deaths data, aggregated at municipality-year level. The dependent variable is the infant mortality rate. Each column presents results for separate weighted regressions, where the weights are given by the births in municipality *m* in state *s* in 2000. The p-values in brackets are adjusted for multiple hypotheses testing. Standard errors (in parentheses) are clustered at the level of the municipality. *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Data source: Mortality Registry 1998-2012.

Table 5: Impact of SP on Hospital Admissions among Infants and Pregnant Women, by Condition (Sample of Poor Municipalities).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
			Before age 1			Before age 1		Obstetric (Women 15-44)				
	All	Bacterial/Int. Malnutrition Respiratory (ICD10 A, E J)	Perinatal (ICD10 P)	Congenital (ICD10 Q)	External Causes (ICD10 V, W, X)	Days	Share ER	All ICD 10 O	Deliveries O80-84	Care related to fetus & delivery O30-48	Complications of labor O85-92	obstetric conditions not in 9-12
before SP (β_1)	0.004	-0.012	0.002	-0.002	0.024	0.005	-0.001	-0.012	-0.026	-0.010	-0.025	0.022
	(0.024)	(0.034)	(0.025)	(0.029)	(0.034)	(0.151)	(0.009)	(0.028)	(0.033)	(0.028)	(0.030)	(0.025)
Adjusted *p-values*	[1.000]	[.999]	[1.000]	[1.000]	[0.979]	[0.996]	[1.000]	[0.998]	[0.966]	[0.999]	[0.963]	[0.945]
0 to 2 years after SP (β_2)	0.070***	0.061*	0.034	0.042	0.036	0.061	0.023**	0.068***	0.100***	0.028	0.056*	0.046*
	(0.026)	(0.036)	(0.027)	(0.031)	(0.037)	(0.142)	(0.010)	(0.024)	(0.028)	(0.027)	(0.030)	(0.025)
Adjusted *p-values*	[0.005]	[0.278]	[0.716]	[0.584]	[0.908]	[0.998]	[0.044]	[0.003]	[0.000]	[0.879]	[0.143]	[0.145]
3 or more years after SP (β_3)	0.074*	0.101*	0.030	0.072	0.028	0.038	0.045***	0.115***	0.142***	0.117***	0.142***	0.108***
	(0.038)	(0.054)	(0.041)	(0.048)	(0.041)	(0.209)	(0.016)	(0.042)	(0.050)	(0.043)	(0.054)	(0.039)
Adjusted *p-values*	[0.128]	[0.143]	[0.979]	[0.453]	[0.974]	[0.999]	[0.003]	[0.004]	[0.003]	[0.005]	[0.006]	[0.003]
p-value H_0: $\beta_2 = \beta_3 = 0$	0.029	0.160	0.451	0.323	0.628	0.877	0.018	0.010	0.001	0.012	0.033	0.018
Mean in 2000	15.30	4.654	8.211	1.017	0.255		0.725	76.37	45.98	11.97	6.252	12.17
S.D.	36.26	13.09	20.09	2.140	0.766	5.646	0.267	196.2	137.7	27.97	13.40	30.41
Observations	16,380	16,380	16,380	16,380	16,380	14,153	14,164	16,380	16,380	16,380	16,380	16,380
Nb Munic.	1,260	1,260	1,260	1,260	1,260	1,255	1,255	1,260	1,260	1,260	1,260	1,260

Note: This table displays weighted least squares estimates of our baseline specification (3) on the discharges data, aggregated at municipality-year level. The dependent variable is the log number of discharges in columns (1)-(5), (8)-(12), the number of days in column (6), and the share of admissions through E.R. in column (7). Each column presents results for separate weighted regressions, where the weights are given by the births in municipality *m* in state *s* in 2000. The p-values in brackets are adjusted for multiple hypotheses testing. Standard errors (in parentheses) are clustered at the level of the municipality. *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Data source: Registry of Admissions to SSA Hospitals 2000-2012.