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¿Los seguros sociales y atención médica mejoran la salud? Evidencia del Seguro Popular en México

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Roberto González Téllez

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Fecha

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Firma

Un viaje de mil millas comienza con el primer paso.
- Lao-Tsé

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Resumen

Los programas de Cobertura de Salud Universal son un tema cada vez más discutido mundialmente. En México hubo un debate por la cancelación del programa Seguro Popular, el programa mexicano de Salud Universal, en 2020. Los motivos alegados para su cancelación fueron, entre otros, que no existe evidencia clara sobre los beneficios de dicho programa en salud, además de que era un programa costoso. En esta tesis estudio, inicialmente, el efecto que tuvo el Seguro Popular en la tasa de mortalidad por cada 1000 personas sin distinguir por causa de muerte. Posteriormente analizo el efecto que tuvo el Seguro Popular en las tasas de mortalidad por cada 1000 personas de tres de los rubros en los que más se gastó: cáncer de mama, antiretrovirales contra VIH y atención a recién nacidos. Por último, estudio el efecto de Seguro Popular en las tasas de mortalidad por cada 1000 personas debido a las dos causas de muerte más comunes en México en el período 2000–2019: diabetes e hipertensión. Aprovechando la adopción gradual del Seguro Popular entre municipios y con una estrategia de Diferencia en Diferencias identifico los efectos causales del Seguro Popular y encuentro que si bien el Seguro Popular disminuyó la mortalidad sin distinguir por causa de muerte, no existe evidencia clara de que el Seguro Popular haya tenido efectos positivos en la mortalidad por alguna causa en particular, a excepción de la mortalidad por hipertensión. Haciendo un análisis de heterogeneidad del efecto del Seguro Popular entre municipios pobres y ricos encuentro que las reducciones en mortalidad agregada y a causa de hipertensión se deben a reducciones en la mortalidad de personas en municipios ricos.

Abstract

Worldwide, Universal Healthcare Coverage has been discussed increasingly. In Mexico there was a debate around the shut down of Seguro Popular, the mexican Universal Healthcare Programme, in 2020. The arguments for its cancellation were, among others, that there is no clear evidence about the benefits of Seguro Popular on health outcomes and that it was a costly programme. In this thesis I study, initially, the effects Seguro Popular had on the mortality rate per 1000 people without distinguishing by death cause. I then analyze the effect Seguro Popular had on the mortality rates per 1000 people of three of the programme's major expenditure targets: breast cancer, AIDS antiretrovirals and newborn care. Finally, I study Seguro Popular's effect on the mortality rates of the two most common death causes in Mexico during the period 2000–2019: diabetes and high blood-pressure. Leveraging the staggered rollout of Seguro Popular across municipalities and with a Difference-in-Differences strategy I identify the causal effects of Seguro Popular and find that even though Seguro Popular decreased all-cause mortality, there does not exist clear evidence of Seguro Popular having positive effects on mortality for any particular death cause, with the exception of the one related to high blood-pressure. Doing a heterogeneity analysis of the effects of Seguro Popular between poor and rich municipalities I find that the reductions in all cause- and high blood-pressure mortality rates are driven by reductions in the mortality rates within rich municipalities.

Contents

Introduction

Disclaimer: This work is based on "Did Seguro Popular reduce formal jobs?" (Seira et al. (2023)). That article focuses on the costs of Seguro Popular, in particular, the *Distorsion towards informality hypothesis.* My role in that paper was to provide research assistance (i.e. cleaning data, producing figures, tables and performing econometric analyses.)

Since the publication of the Constitution of the World Health Organization (WHO) from 1948, there has been an advocacy for "[...] the highest attainable standard of health as a fundamental right of every human being." (WHO (2021)). The world is rapidly evolving and so are diseases and viruses. This calls for action, particularly in developing countries, where healthcare services are not only expensive, but also inaccessible for a non-trivial share of the population. However, it is still not clear whether government-funded public insurance and medical attention indeed work. Nor is it clear how to best implement such a public insurance scheme and the way in which medical attention should be provided.

"Over the past 20 years, various countries in Latin America [...] have implemented health insurance reforms to improve the coverage of marginalized groups" (Sosa-Rubí, Galárraga, and López-Ridaura (2009)). In Mexico, aiming to provide the poor and the uninsured with access to qualified health care services at an affordable price, as well as aiming to reduce catastrophic healthcare expenditure, Seguro Popular was created in 2000 (CONEVAL (2012) and King et al. (2009).) Seguro Popular was a public health insurance and medical attention scheme funded mostly by the government —both federal- and state-level— and particularly targeted to minorities at risk and informal workers.

Since its creation, Seguro Popular received a lot of attention. On the one hand, some researchers and policymakers were against a programme such as Seguro Popular, arguing that although there may be benefits from it, it simultaneously creates distortions in the labour market (e.g. Levy (2008) and UNDP (2021).) On the other hand, another group of researchers and policymakers were in favour of it because, if implemented correctly, it would grant access to health care services to the most vulnerable groups within the mexican population and it would reduce catastrophic healthcare out-of-pocket expenditure, thus being a step closer towards closing one of the many inequality gaps in the country. While there have been studies supporting both of these points of view, the evidence around both costs and benefits of the programme is still scarce[1], mixed and limited in the evaluation methods used.

My work adds to the literature on the benefits of the Seguro Popular programme by using the most complete data sources (most similar to the data used in Bosch and Campos-Vazquez (2014) and Ginja and Conti (2015)) and state-of-the-art econometric methods (see de Chaisemartin and d'Haultfoeuille (2022) and Wooldridge (2021)) that yield more credible results under credible assumptions.

Previous research, e.g. King et al. (2009), Barros (2009), Rivera-Hernández, Rahman, and Galárraga (2019), and Miranda (2012), has studied effects of Seguro Popular on catastrophic health expenditures and a well documented result is that Seguro Popular achieved its goal reducing catastrophic health expenditure for programme enrolles, but the evidence regarding the programme's effect on health outcomes is still not clear. Studies examining the effect of Seguro Popular on mortality, e.g. Ginja and Conti (2015), have failed to find significant effects. In light of recent papers (Goodman-Bacon (2021), de Chaisemartin and d'Haultfoeuille (2022), Wooldridge (2021), and Roth et al. (2022)) studying the mechanics of so-called Two-Way Fixed Effects regressions (TWFE) —which is the *go to* model for evaluating the effect of a program such

[1] Literature on the benefits side is not that scarce, however, most studies lack nationally representative data or credible causal identification strategies.

as SP—, one wonders whether the null results found in previous research are due to biased estimates. Motivated by this, I first study whether there exist reductions in the mortality rate per 1000 people because of the implementation of Seguro Popular. After this, I study whether the reductions in the aggregate mortality rate are driven by reductions in the mortality rate of either the three main targets of Seguro Popular expenditure (Breast cancer, Newborn care and AIDS) or by reductions in the mortality rate of the two most common death causes in Mexico (High Blood-pressure and Diabetes.)

Seguro Popular causes a decrease in the aggregate mortality rate per 1000 people of about 0.052 deaths per 1000. This accounts for about 5.1 % of the mean mortality rate between 2000 and 2011. While Seguro Popular decreases the aggregate mortality rate, an interesting result is finding that Seguro Popular caused *increases* in the breast cancer mortality rate as well as on the mortality rate for newborns, although the estimates are somewhat imprecise. The increases account for about 55.6 % and 9.5 %, respectively, over the corresponding mortality rate in 2000, which serves as a benchmark. These estimates and the nature of the Seguro Popular programme raise the question of whether there existed underreporting of breast cancer prior to Seguro Popular. On the other hand, the high blood-pressure mortality rate did in fact decrease because of the implementation of Seguro Popular. On average, Seguro Popular caused a decrease of 0.036 deaths per 1000 because of high blood-pressure, which accounts for 23.9 % of the high blood-pressure mortality rate in 2000. As for AIDS and diabetes there is no evidence suggesting the implementation of Seguro Popular caused changes in the mortality rate for any of these two diseases. However, in the case of diabetes it is worth taking into account that it is closely related to blood-pressure conditions, so Seguro Popular effects on diabetes might be reflecting in the high blood-pressure mortality rate reduction.

Ex-ante one could believe that Seguro Popular would be most effective in municipalities with reduced access to medical services and larger shares of uninsured people prior to the implementation of Seguro Popular. These kind of municipalities are likely more marginalized and poor than those with more insured inhabitants and better provision of health services. To assess whether it is in fact the case that Seguro Popular works best in poorer (more marginalized) municipalities I employ a heterogeneity analysis labeling municipalities as either poor or rich[2] and I estimate the Seguro Popular treatment effects on each of the municipality subgroups. Contrary to my beliefs, the Seguro Popular effects estimated on all municipalities are completely driven by effects in rich rather than poor municipalities. It is also worth noting that the null results estimated on AIDS and diabetes are a consequence of null results in both types of municipalities rather than treatment effects having opposite signs across municipality type and hence cancelling out on the aggregate.

Even considering that Seguro Popular did decrease the mortality rate on the mexican population, the scope of this thesis is limited with respect to welfare analyses in that it only focuses on mortality outcomes. When studying the causal effects programmes such as Seguro Popular have on the population of interest one can also study many other aspects that provide useful insights for policymakers. For starters, the effect on catastrophic health expenditure is of remarkable interest since this helps reduce a burden for the poorest households. Additionally, one could study whether Seguro Popular was more effective in preventing diseases or in treating them, and why do both effects differ if they do. Did the programme benefit, e.g., indigenous communities? Did programme implementation cause changes in the demand for medical education? Some of these questions have been addressed (Knox (2018), King et al. (2009), Rivera-Hernández, Rahman, and Galárraga (2019), Serván-Mori et al. (2015), Barros (2009), González-Pier, Gómez-Dantes, and García-Junco (2006), and Marie Knaul et al. (2012)), but the access to better and full-program length data should allow for better assessments of what the programme worked for and not.

The rest of the thesis is organized as follows. Chapter 1 provides context on the insurance schemes before and after the introduction of Seguro Popular. Chapter 2 summarises previous studies on the benefits of Seguro Popular. Chapter 3 details the empirical strategy used in this study and the necessary identification assumptions for estimating the causal parameters of interest. Chapter 4 describes the data sources and its preparation for the analysis.

[2] A municipality is labeled as poor if its marginalization index in 2000 was "Very High" or "High" according to CONAPO; rich municipalities have a marginalization index of "Very Low", "Low" or "Medium" (CONAPO (2000).)

Chapter 5 presents the main results of the estimation on the aggregate mortality rate as well as on the mortality rates specifically for breast cancer, newborns, AIDS, high blood-pressure and diabetes. Chapter 6 is an extension to the main analysis and discusses Seguro Popular effects on the birth rate as well as on abortions that complement the findings for the newborn mortality rate. There is a discussion on results and a conclusion to finalize.

Chapter 1

Context of Seguro Popular implementation

1.1 Health care coverage in Mexico before Seguro Popular

The Mexican health care system has always been characterized by the segregation between coverage for *formal private* workers and coverage for *formal public* workers.

Instituto Mexicano del Seguro Social (IMSS) is the provider of health insurance for formal private workers —as well as their families— and it is the largest provider of health insurance in Mexico[1]. IMSS has its own network of health care facilities through which it provides its services. As for formal public workers (i.e. those formally employed by the federal government), Instituto de Seguridad y Servicios Sociales de los Trabajadores del Estado (ISSSTE) is in charge of providing health care services to them. ISSSTE is not as large as IMSS in terms of insured population.

Additionally, public employees from the armed forces, the government-owned oil company PEMEX or the states' administration receive

[1] IMSS affiliation also includes various benefits such as children daycare and compensation for work-related injuries.

health care services through a provider with which each employer signs an arrangement.

Notably, informality is a common working condition in Mexico. This implies that those who are hired under an informal working scheme are left uninsured if they do not personally acquire health insurance through a private provider. Roughly 50 % of the mexican workers did not receive social security prior to Seguro Popular (Marie Knaul et al. (2012).) "The insured population received health care from well financed, vertically-integrated, federal institutions, whereas the uninsured relied on underfunded, state decentralised institutions [. . .]. Every public institution is responsible for financing and service delivery only for its particular population. At the same time, many families relied on the poorly regulated and costly private sector" (Marie Knaul et al. (2012).)

Moreover, health services were paid for by uninsured[2] people via out-of-pocket expenses and services were provided through severely underfunded public assistance facilities, as documented in Marie Knaul et al. (2012).

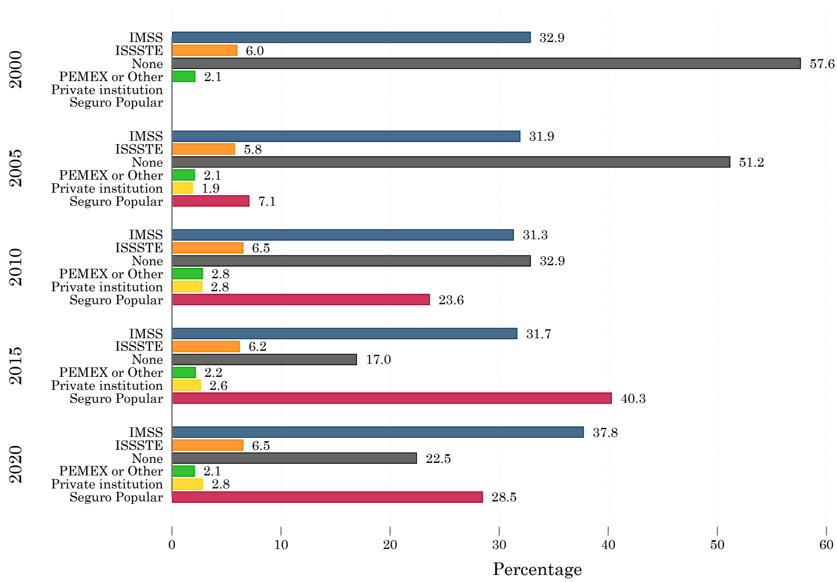
In 2005, right after Seguro Popular was created, the OECD wrote that public health-care spending by Mexico was low at 2.8 per cent of GDP in 2002, and that the supply of inputs was very limited "leading to significant implicit rationing throughout the system" with the consequence that "poorer households are less well covered by social insurance than richer households and a larger share of the poor also face catastrophic and poverty-creating health-care expenditures." (OECD (2005)) Furthermore, most resources were allocated unequally across the country (Marie Knaul et al. (2012),) and IMSS spending was twice as large as that devoted to the uninsured.

Figure 1.1 shows the evolution over time of insurance provider shares for the mexican population using census data. Two things in particular are worth noting. First, reductions in the share of people who answer "None" to whether they are affiliated to some institution are similar in magnitude to increases in

[2] As of 2020, less than 10 % of the Mexican population has private health insurance, and about 3 % use private sector hospitals. https://www.inegi.org.mx/temas/derechohabiencia/ and https://www.forbes.com.mx/solo-1-de-cada-10-mexicanos-tiene-seguro-de-gastos-medicos/. These numbers were likely lower in 2000.

Seguro Popular shares, which can be taken as suggestive evidence of the targeted population —i.e. the uninsured— effectively enrolling into the programme. Second, the Seguro Popular programme experienced large increases in take-up over time, suggesting there is enough variation in the exposure to the program so as to be able to estimate its impact[3].

Figure 1.1. Insurance affiliation 2000-2020



Note: Constructed with data from the 2000, 2010 and 2020 Censo de Población y Vivienda, the 2005 Conteo de Población y Vivienda and the 2015 Encuesta Intercensal, all conducted by INEGI. Each bar represents the percentage of people who claimed being insured by each institution. In particular the question they answered was *Are you affiliated or do you have right to use the medical services provided by* `institution name`*?* Where `institution name` is one of: IMSS, ISSSTE, PEMEX, Private institution, Seguro Popular, Other or None. For the year 2000 the answer option "Private institution" was not available.

[3] Figure A-4 in Appendix section I. also shows the number of municipalities implementing Seguro Popular across quarters.

1.2 Seguro Popular

The Sistema de Protección Social en Salud, Seguro Popular (SP, herafter), was a universal healthcare coverage programme created in 2002 (as a pilot). The main target of SP were uninsured people and, in particular, minorities at risk: women, children, the indigenous and the elderly. Any person not insured by IMSS or ISSSTE was a potential beneficiary of the program. Enrollment into SP was voluntary, the funding scheme was devised so as to be able to provide a more equitable allocation of resources and yearly budgets were assigned in proportion to the number of enrollees each state managed to insure. This last point provided incentives for governments to actively improve their health care facilities, making enrollment to SP more attractive.

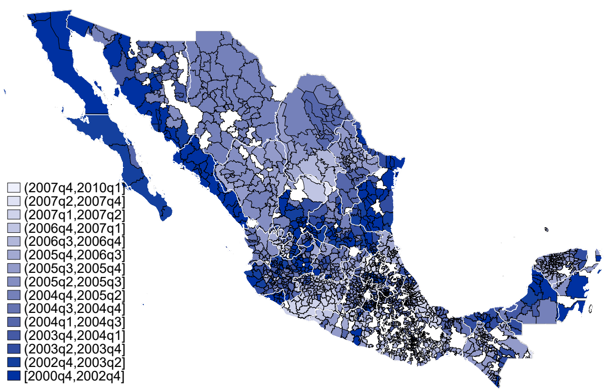
As for the funding scheme, a tri-partite arrangement was made. A means-tested premium was supposed to be charged to enrollees over the fourth decile of the income distribution, the federal government would contribute to SP as much as it did to IMSS and states would allocate tax-revenue resources to the necessities of SP. In practice, very few beneficiaries did in fact pay the premium.

Funds acquired by the states were supposed to be invested into the improvement of current healthcare facilities, the quality of services provided there and into the increase in supply. By doing this, affiliation was supposed to be promoted and states would increase their capability of satisfying the population's health needs. Importantly, municipalities with less resources would be privileged in the allocation of funds in order to close the gaps between them and municipalities with more coverage capacity.

SP was implemented in a staggered fashion over all 2,427 mexican municipalities from 2002 to 2011 (the program ran until 2018 formally, but ended in practice in 2020.) In 2000, the pilot phase of the program began in 128 municipalities. From 2002–2004 the implementation tasks were administrative matters rather than actual provision of health care services. In 2004 *de facto* provision began as a pilot program with the inclusion of municipalities located in 7 more states. The expansion of SP thus followed a gradual expansion across states and, within states, across municipalities based on health services needs, capacity of service provision and local budget constraints. As of 2011, twenty nine out of thirty two states reported having achieved universal coverage, while the three remaining reported having achieved, at least, 83 per cent (González-Pier, Gómez-Dantes, and García-Junco (2006) and González-Pier et al. (2006).) The gradual expansion across municipalities over time is the key element of the identification strategy for evaluating the impact of SP.

Figure 1.2 shows the gradual expansion of SP across municipalities over time.

Figure 1.2. Geographical coverage of SP by municipality

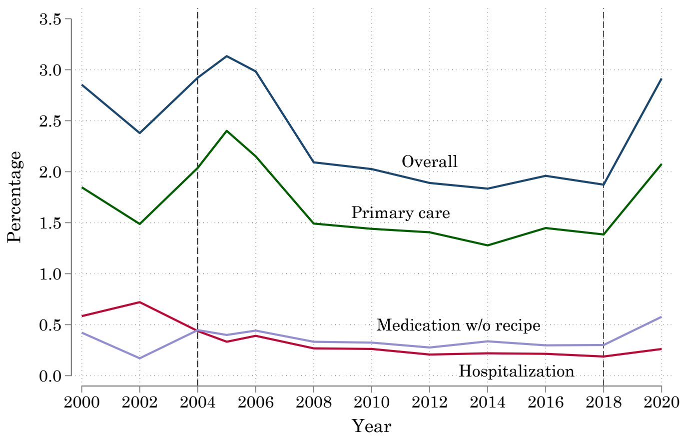


Note: This figure shows the geographic expansion of SP over time. Darker areas represent early-implementing municipalities and lighter colored areas represent later-implementing municipalities, so as to depict exposure intensity to SP. Figure self-made with data from the "Padrón de Beneficiarios," the administrative registry of Seguro Popular enrollees.

SP covered a package of 91 services and medications during the pilot stage of the program. The services and medicines covered were listed in the official Cátalogo Universal de Servicios de Salud (CAUSES) which also increased gradually —with an update every 2 years, approximately— until it covered 275 interventions in 2011. Alongside SP, a fund for protection against catastrophic spending was created. The fund reimbursed resources to people who required and paid for specialty care on a per-case basis.

With the expansion of SP's services and medicine coverage, public spending on health services not provided by IMSS increased from 0.8 to 1.2 per cent of GDP, while IMSS expenditure was reduced from 1.7 to 1.5 per cent of GDP in the period 2003–2008 (Bosch and Campos-Vazquez (2014).) Most of the SP expenses were on so-called catastrophic health expenditure but a non-negligible amount was also used in preventive health care interventions. Miranda (2012), King et al. (2009), Barros (2009), and Rivera-Hernández, Rahman, and Galárraga (2019) argue that household savings attributable to SP on out-of-pocket expenses can be substantial. King et al. (2009), for example, shows that SP caused out-of-pocket expenses to decrease by almost 55 % relative to the control group mean catastrophic expenditure in their study. Figure 1.3 shows a clear decline in out-of-pocket expenses as proportion of current income during the years in which SP was active. Most of the decline is driven by reductions in primary care expenditures.

Figure 1.3. Health expenditure as percentage of current income



Note: This figure uses Mexico´s income and expenditure survey ENIGH —conducted by INEGI — from 2000 to 2020 to plot the mean health expenditure as proportion of a household's current income. To construct each variable I take the quarterly reported expenditure on each of the expenditure categories and make them annual quantities multiplying them by 4, as suggested by INEGI. I then standardize expenditure to 2018 Mexican pesos to make quantities comparable over time. Lastly, I divide each category's expenditure by the annualized current income to compute health expenditures as proportions of current income. For computing the mean I use frequency weights provided by INEGI in each survey. ENIGH data is collected every two years; in 2005 an additional survey was run in response to the demand for data by policymakers and researchers. Vertical dashed lines denote beginning and ending of SP programme.

Chapter 2

Literature review

As mentioned in the introduction, there are two strands of literature concerning the effects of Seguro Popular. One focuses on the costs associated to it, namely, the so-called *Distortion towards informality hypothesis* (Levy (2008).) This hypothesis states that providing health insurance and medical attention to workers conditional on them being informal generates incentives for people to leave formal jobs in favour of being employed in an informal job. The other strand of the literature focuses on evaluating the impact Seguro Popular had on catastrophic out-of-pocket expenses and various health-related outcomes, the benefits side of the policy. Given that this thesis deals with health-related outcomes I review only studies on the benefit side of the policy, but I refer those interested in the cost-side literature to Seira et al. (2023).

2.1 Using survey data

Knox (2018) studies whether Seguro Popular affected demand for preventive care among vulnerable populations. She finds that women and children are in fact more likely to enroll into Seguro Popular. This is not seen for indigenous people. Using an IV identification strategy and data from 58 urban municipalities from 2004–2007 she estimates that adult demand for physical exams increased. On the other hand, waiting times at health care facilities increased as well, which might deter people from attending health care facilities in the long run.

Rivera-Hernández, Rahman, and Galárraga (2019) uses three rounds of the ENSANUT (2000, 2006, 2012) and an IV identification strategy and finds people with diabetes and high blood-pressure had increased access to medical care. Importantly, there is no effect on *treatment* of these diseases for the elderly. Consistent with other studies, they find a decrease in catastrophic health-expenditure. Their analyses focus on low-income elderly.

Serván-Mori et al. (2015) uses the cross-sectional ENSANUT from 2012 and matching methods to evaluate the impacts of Seguro Popular on ante-natal care. They find increased probability of receiving timely ante-natal care for low-income women but no effect of SP either on timely ante-natal care nor on the probability of completing 4 ante-natal care visits.

Parker, Saenz, and Wong (2018) uses the longitudinal Mexican Health and Aging Study to estimate the probability of receiving preventive/treatment care on adults aged 50 and over. Using difference-in-differences matching methods they find no effects on treatment probability of diabetes and high blood-pressure, but they do find that SP affiliates are more likely to receive diagnostic tests and preventive care.

Sosa-Rubí, Galárraga, and López-Ridaura (2009) uses matching methods and the 2006 ENSANUT & finds that poor people had increased access to glucose control tests and insulin injections.

Studies in this section mainly use survey data in the measurement of their outcomes so one cannot rule out the existence of measurement error. Another thing to worry about is the weakness of the identification strategies adopted throughout the studies given the data they use.

2.2 Using administrative data

The most comprehensive observational study I found on the benefits of Seguro Popular is Ginja and Conti (2015). With rich administrative data such as public hospital discharges, health-related infrastructure and the mortality registry they find that Seguro Popular had no effect on overall mortality rates, nor for any particular age-group (1-4, 5-19, 20-59, 60-89). The only exception is infant mortality in poor municipalities, where they define infants as all live births plus children under 1 year of age. They produce all estimates with weighted least squares regression.

2.3 Randomized evaluations

To the best of my knowledge, the only experimental study evaluating Seguro Popular is King et al. (2009). In collaboration with the Mexican Ministry of health, they implement an experimental design within the staggered rollout of the programme to induce random variation in programme exposure. They pair-match 74 "health-clusters" within seven states and randomly assign treatment[1] to one of the clusters of the pair. They collect a survey in August-September 2005 and a follow-up survey in July-August 2006 (10 months after) and they find treatment assignment is more effective in poorer areas than in areas with higher average asset ownership. They find that 23 % less households experience catastrophic out-of-pocket spending in treatment clusters compared to control ones. SP also reduced catastrophic expenditure by 55 %, with most of the effect coming from reductions for poorer households. Notably, they find that SP does not cause changes in the use of medical services nor on preventive interventions such as mammograms, cervical- or pap-tests. Even though their study overcomes the main problems of observational studies, they do acknowledge that their null results might be due to the limited duration of their study and that their results might not be generalized to the whole mexican population given the communities in which their experiment was conducted.

[1] Their treatment is "a campaign to persuade every family to enrol in Seguro Popular, and procedures initiated by states [...] to implement the programme effectively" (King et al. (2009)).

Chapter 3

Empirical strategy

As with every research question involving the measurement of causal treatment effects, the problem lies in finding ways to estimate the counterfactual (i.e. what would have happened to treated units had those units not been treated.) Under certain identification assumptions —discussed later on— I am able to leverage the staggered implementation of SP in different municipalities to estimate such a counterfactual, which in turn allows me to estimate an average treatment effect on treated (ATT) municipalities, by comparing municipalities where SP is implemented to those where it is not yet implemented.

3.1 Difference-in-Differences

The canonical difference-in-differences method (DiD) compares changes in pre-treatment vs post-treatment outcomes for the treated groups against changes in pre- vs post-treatment outcomes for non-treated groups.

Moreover, DiD can be extended to a context with geographical and time differentiated rollout of a program. In the context of SP, one would compare treated municipalities to not-yet-treated municipalities over time. In order for this method to properly identify a causal effect one must be able to argue, among other things, that early implementing municipalities would have followed the same outcome trends as late implementing municipalities had SP not been implemented. This is the so-called parallel trends assumption as per Angrist and Pischke (2009).

Until very recently, researchers used Two-Way Fixed Effects (TWFE) regression models in order to estimate the ATT. However, it has been shown that these regression models only identify the causal treatment effect of interest under treatment effect homogeneity across panel units and time periods, in addition to the standard parallel trends and no anticipation assumptions (Goodman-Bacon (2021), de Chaisemartin and d'Haultfoeuille (2022), Wooldridge (2021), and Roth et al. (2022).)

Given that in my setting there is no reason to believe that SP effects will be constant both across municipalities and over time it is likely that estimates based on Two-Way Fixed Effects regressions will yield biased estimates. Because of this, I estimate my main results using the estimator proposed in de Chaisemartin and d'Haultfœuille (2020). In its most basic version their estimator does not allow for the inclusion of covariates, but they propose an alternative version of it that can handle covariates. Although the main results are presented with the basic version of the estimator, I also include as a robustness check estimations conducted with the estimator that includes covariates.

3.1.1 Causal parameter of interest

As in any difference-in-differences research design, I will be able to identify the Average Treatment Effect on the Treated provided that my identification assumptions hold. Consider a potential outcomes framework as introduced in Rubin (1974) and, following the notation used in de Chaisemartin and d'Haultfœuille (2020), let $D_{igt} = \{0, 1\}$ denote treatment status of municipality $i \in \{1, 2, \ldots, I\}$, treatment-group $g \in \{1, 2, \ldots, G\}$ and time period $t \in \{1, 2, \ldots, T\}$ and $Y_{igt}(D_{igt})$ denote the potential outcome of municipality i, treatment-group g and time period t as a function of the treatment D. Then let

$$\Delta_{gt} = \frac{1}{N_{gt}} \sum_{i=1}^{N_{gt}} [Y_{igt}(1) - Y_{igt}(0)] \qquad (\text{ATT}_{gt})$$

denote the Average Treatment Effect in the cell for treatment-implementation group g at time period t. Note that

$$\delta^{SP} = \mathbb{E}\left[\sum_{gt:D_{gt}=1} \frac{N_{gt}}{N_1}\Delta_{gt}\right] \tag{ATT}$$

is the Average Treatment Effect on Treated municipalities —my causal parameter of interest—, where $N_1 = \sum_{igt} D_{igt}$ is the number of treated units. The estimates for this parameter are shown in tables, whereas estimates for ATT_{gt}'s are shown in figures for ease of presentation.

3.1.2 Estimator and identification assumptions

For properly defining the estimator, let

$$N_{d,d',t} = \sum_{g:D_{gt}=d, D_{g,t-1}=d'} N_{gt} \quad \forall t \in \{2,3,\ldots,T\} \quad \forall (d,d') \in \{0,1\}^2$$

denote the number of observations with treatment d' at period $t-1$ and treatment d at period t. We can now define

$$DID_{+,t} = \sum_{g:D_{gt}=1, D_{g,t-1}=0} \frac{N_{gt}}{N_{1,0,t}}(Y_{gt} - Y_{gt-1}) - \sum_{g:D_{gt}=D_{g,t-1}=0} \frac{N_{gt}}{N_{0,0,t}}(Y_{gt} - Y_{gt-1})$$

$$DID_{-,t} = \sum_{g:D_{gt}=D_{g,t-1}=1} \frac{N_{gt}}{N_{1,1,t}}(Y_{gt} - Y_{gt-1}) - \sum_{g:D_{gt}=0, D_{g,t-1}=1} \frac{N_{gt}}{N_{0,1,t}}(Y_{gt} - Y_{gt-1})$$

Then the estimator for δ^{SP} is:

$$DID = \sum_{t=2}^{T}\left(\frac{N_{1,0,t}}{N_{switchers}} DID_{+,t} + \frac{N_{0,1,t}}{N_{switchers}} DID_{-,t}\right)$$

where $N_{switchers} = \sum_{gt:t\geq 2, D_{gt}\neq D_{gt-1}} N_{gt}$ However, municipalities implementing SP did not ever stop implementing it during my period of study.

Hence, the estimator for δ^{SP} reduces to:

$$DID = \sum_{t=2}^{T} \frac{N_{1,0,t}}{N_{switchers}} DID_{+,t}$$

Under the following identification assumptions, the DID estimator is an unbiased and consistent estimator of the ATT parameter of interest (de Chaisemartin and d'Haultfœuille (2020).)

Assumption 1 - Balanced Panel No group appears or disappears over time.

$$\forall (g,t) \in \{1,2,\ldots,G\} \times \{1,2,\ldots,T\}, N_{gt} > 0$$

Assumption 2 - Sharp Design Units' treatments do not vary within each (g,t) cell.

$$\forall (g,t) \in \{1,2,\ldots,G\} \times \{1,2,\ldots,T\} \textit{ and } i \in \{1,2,\ldots,N_{gt}\}, D_{igt} = D_{gt}$$

Assumption 3 - Strong Exogeneity Shocks affecting group g's untreated potential outcome, $Y_{gt}(0)$, are mean independent of group g's treatment sequence.

$$\mathbb{E}[Y_{gt}(0) - Y_{gt-1}(0) \mid D_{g1}, D_{g2}, \ldots, D_{gT}] = \mathbb{E}[Y_{gt}(0) - Y_{gt-1}(0)]$$
$$\forall (g,t) \in \{1,2,\ldots,G\} \times \{1,2,\ldots,T\}$$

Assumption 4 - Parallel Trends The expectation of the outcome without treatment follows the same evolution over time in every group.

$$\textit{For } t \geq 2 \quad \forall g \neq g^{'}, \mathbb{E}[Y_{gt}(0) - Y_{gt-1}(0)] = \mathbb{E}[Y_{g^{'}t}(0) - Y_{g^{'}t-1}(0)]$$

Assumption 5 - Existence of "Stable" Groups Between each pair of consecutive time periods, if there is a group of municipalities that implements SP, then there exists at least one group of municipalities that does not implement SP at both time periods.

$$\textit{If there is at least one } g \in \{1, 2, \ldots, G\} \textit{ such that } D_{gt-1} = 0, D_{gt} = 1,$$
$$\textit{then there exists at least one } g^{'} \neq g, g^{'} \in \{1, 2, \ldots, G\} \textit{ such that } D_{g^{'}t-1} = D_{g^{'}t} = 0$$

Assumption 6 - Mean Independence between a Group's Outcome and Other Groups' Treatments Conditional on its own treatments, a group's outcomes are mean independent of other groups' treatments.

$$\forall g \in \{1, 2, \ldots, G\} \textit{ let } \mathbf{D}_g = (D_{g1}, D_{g2}, \ldots, D_{gT}), \textit{ then}$$
$$\forall g, t, \mathbb{E}[Y_{gt}(0) \mid \mathbf{D}] = \mathbb{E}[Y_{gt}(0) \mid \mathbf{D}_g] \textit{ and } \mathbb{E}[Y_{gt}(1) \mid \mathbf{D}] = \mathbb{E}[Y_{gt}(1) \mid \mathbf{D}_g]$$

The Balanced Panel, Sharp Design and Existence of "Stable" Groups assumptions are satisfied naturally given the data I have access to (see next section.) It is impossible to test whether the Parallel Trends assumption holds, however, in the main results' figures I present evidence in favour of it by showing that one cannot reject that outcome evolution was similar between early- and later-implementer groups prior to SP implementation, although estimates are imprecise given the nature of the outcome (mortality rates.) As for the Strong Exogeneity assumption, I have no way to test whether it holds or not. Nonetheless, the untreated potential mortality rate's evolution is likely independent of treatment sequence for any particular group g since the outcome is defined at the municipality level and, for example, if less healthy people decided to move towards early-implementing municipalities there would have to exist a sufficiently large change in the composition of people residing in the municipalities composing group g in order for those changes in composition to reflect into my outcomes of interest. Also, the estimation is done 2 years before/after SP implementation, a time window probably not as large as needed for big mobility phenomena to occur due to the implementation of SP. Seira et al. (2023) shows, using IMSS administrative data, that (formal) workers do not seem to migrate across municipalities due to SP implementation. Lastly, Assumption 6 is also likely to hold since, for the mortality rates I am interested in, one could argue that SP being implemented earlier/later at other municipalities does not affect other group's outcomes. This is likely since, apart from AIDS, the causes of death in which I am interested are not contagious diseases.

3.2 Outcomes of interest

I am interested in studying the mortality rates per 1000 people, defined as follows, where m denotes municipality and t denotes quarter:

$$AllCauseMR_{mt} = 1000\left(\frac{\#Deaths_{mt}}{Population_{mt}}\right)$$
$$BreastCancerMR_{mt} = 1000\left(\frac{\#BreastCancerDeaths_{mt}}{\#Women_{mt}}\right)$$
$$AIDSMR_{mt} = 1000\left(\frac{\#AIDSDeaths_{mt}}{Population_{mt}}\right)$$
$$NewbornMR_{mt} = 1000\left(\frac{\#NewbornDeaths_{mt}}{\#PopulationUnder1_{mt}}\right)$$
$$DiabetesMR_{mt} = 1000\left(\frac{\#DiabetesDeaths_{mt}}{Population_{mt}}\right)$$
$$HighBloodPressureMR_{mt} = 1000\left(\frac{\#HighBloodPressureDeaths_{mt}}{Population_{mt}}\right)$$

In Section 6 the outcomes of interest are:

$$AbortionMR_{mt} = 1000\left(\frac{\#AbortionDeaths_{mt}}{\#WomenOver15_{mt}}\right)$$
$$BirthRate_{mt} = 1000\left(\frac{\#LiveBirths_{mt}}{Population_{mt}}\right)$$

Chapter 4

Data sources

4.1 Data from Seguro Popular

I use the Beneficiarios de Protección Social en Salud de Seguro Popular (Padrón, henceforth,) the beneficiaries registry, for the period 2000–2009. In this dataset one can find the number of enrollees in the SP program per municipality-quarter-year[1]. I complement these data with the government-published Padrón at the open government data repository. One drawback is that these data are at the municipality-year level, but the benefit of using it comes at obtaining data from all years in which SP was active from 2004 to 2019, as well as obtaining data from *all* mexican municipalities. The Bosch & Campos-Vázquez and government-published datasets are consistent with one another. Using these data I construct the SP implementation indicator, where I define SP to be implemented in a municipality whenever there are at least 10 individuals affiliated to SP (analogous to Bosch and Campos-Vazquez (2014) and Ginja and Conti (2015).) Results are robust to defining the implementation date with enrollees $= \{1, 10, 100\}$. This variable allows us to compare treated vs not-yet-treated municipalities, which in turn allows us to estimate the ATT and ATT_{gt} parameters of interest.

[1] The dataset was originally published by Bosch and Campos-Vazquez (2014) at OpenIPCSR

4.2 Data from INEGI

I use data from Mexico's Instituto Nacional de Estadística y Geografía (INEGI) to create the outcome variables and control for possible confounders of the relationship between SP implementation and the outcomes of interest in the robustness checks.

4.2.1 Population censuses

I use the population census from 2000, 2010 and 2020 as well as the intercensus data from 2005 and 2015 to obtain population levels per municipality-year, population's sex share and share of insured population. Since analysis is done at the quarterly level, I use these data to linearly interpolate population at each quarter of the 5 year gaps between each census, using a constant growth rate. In particular for the breast cancer mortality rate outcome I also linearly interpolate women population levels at each of the missing quarters during the 5 year gaps.

4.2.2 Registry of deceases

I use the yearly registry of diseases from 2000 to 2019 to obtain counts of deceases at the municipality-year-quarter level. A registry contains —among other sociodemographic data— the year, month, place and cause of death of the deceased. For the cases where the place of death is not registered I impute the municipality of residence at the month of death as the place of death, however, this is the case only for less than 10 % of my sample. I also use these data to obtain the deaths because of an abortion at the municipality-year-quarter level.

4.2.3 Births data

I use the yearly registry of births from 2000 to 2020 to obtain counts of live-surviving births at the municipality-year-quarter level. The birth registry data contains information on the number of live births as well as the number of surviving babies per birth-giving event. There are birth registries without a specified date of birth. I drop those observations from the sample but this happens for less than 0.04 % of the sample total.

4.2.4 Employment survey

I use the Encuesta Nacional de Ocupación y Empleo, which is collected every two years, to obtain mean educational attainment at the municipality level as well as industry shares and share of informal workers in the municipality at each quarter. For missing quarters the data is linearly interpolated, as done in the census data, using a constant growth rate.

4.3 Other data

4.3.1 Luminosity measured via satellite

A valid concern is that wealthier municipalities (i.e. municipalities whose residents are wealthier, on average) might be larger and have easier access to healthcare, which might make them healthier on average. To alleviate this concern about possible omitted variable bias I use luminosity data as a proxy for economic activity, which is in turn a proxy for municipality income. Luminosity[2] is defined as the night-time light data systematically sensed via satellites. In the economics literature, luminosity data has been used as a proxy for economic activity (e.g. Michalopoulos and Papaioannou (2013).) Luminosity data is gathered at the yearly level for every mexican municipality in my period of study.

4.4 Sample

For the estimation I keep only a balanced panel of municipalities, that is, municipalities that exist in every quarter between 2000 and 2011.

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[2]Luminosity rasters are from **Li2020** Harmonized Global Nighttime Light Dataset 1992-2018; I use data from the repository at `https://github.com/emagar/luminosity`

Chapter 5

Main results

Authors of some studies evaluating the benefits of Seguro Popular using Difference-in-Differences research designs insist on interpreting their estimates as Intention-to-Treat (ITT) estimates (e.g. Ginja and Conti (2015).) However, interpreting the estimates of a Difference-in-Differences research design in the context of Seguro Popular as ITT estimates would give an inaccurate and biased perspective of the effects the implementation of a program such as Seguro Popular has since the policymaker has to acknowledge the existence of alternatives for health insurance and medical services provision.

A thing worth taking into account when trying to estimate treatment effects on mortality is that detection of them requires enormous statistical power, so failing to find statistically significant effects should not be interpreted as effects being non-existent. This is particularly relevant when assessing the effectiveness of (one of the many) facets of programmes similar to SP.

5.1 All cause mortality rate

According to Ginja and Conti (2015), Seguro Popular did not decrease the mortality rate in the mexican population. However, the empirical strategy used in their study has now been shown to be sensitive to treatment effect heterogeneity. Because of this, I re-study whether SP had an effect on mortality rates using the estimator discussed in section 3.

Table 5.1 shows SP decreased the all cause mortality rate by 0.052 deaths per 1000 people on average. The effect is statistically significant (p = 0.068) and it is economically large in magnitude since it accounts for a 5.1 % reduction over the 2000–2011 outcome mean. On the other hand, however, even though confidence intervals include 0 for all estimates pre SP implementation, Figure 5.1 shows one cannot reject the existence of a decreasing trend in mortality during the period of study. Because of this the null result presented in Ginja and Conti (2015) is likely plausible.

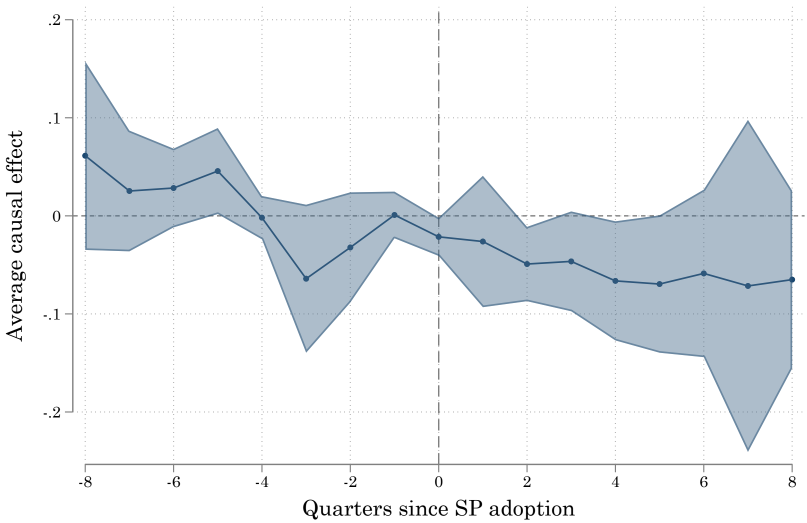
Table 5.1. SP effect on the overall mortality rate

	Mortality Rate
SP	-0.0526*
	(0.0289)
Municipalities	1410
Balanced Panel	Yes
Outcome Mean	1.015
Controls	No

Note: This table shows the weighted average of dynamic ATT's for the all-cause mortality rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 5.1. Event study

(a) All cause mortality



Note: This figure shows an event-study plot of the effect of SP implementation on the all cause mortality rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

5.1.1 Three main SP expenditure targets

I now investigate whether the mortality rates associated to the three main SP expenditure targets were impacted by SP implementation.

Breast cancer SP seems to have *increased* breast cancer mortality rates by 0.0098 deaths per 1000. Economically speaking, effects are huge compared to a baseline mean[1] of 0.0176 deaths per 1000. This raises the question of whether breast cancer diagnoses were accurate before SP implementation. Another important feature of breast cancer is that it metastasizes to other organs such as the brain, liver and bones, which might make it difficult to pin down the exact death cause. If Seguro Popular helped in properly diagnosing breast cancer as the death cause, then that would rationalize the increase in breast cancer mortality rates. King et al. (2009) do not find increases in mammograms conducted which could be interpreted as evidence against the better diagnoses hypothesis, but this evidence should not be taken as conclusive —given the short duration of their study— and further evaluations should be made.

Newborns SP also seems to increase newborns MR, although the point estimate is not statistically significant ($p = 0.148$). SP implementation causes around 0.136 more newborn deaths per 1000. This increase is economically meaningful since it represents a 9.5 % increase over a baseline MR of 1.429 deaths per 1000. Given that SP also included attention to abortions and birth-giving, and in order to help rationalize this result, it is worth looking into whether SP caused any changes in the number of abortions and live-births. I test whether this is the case in section 6.

AIDS There seems to be no effect on AIDS MR. Note in Figure 5.2 that over all the period of study the confidence interval is somewhat constant and includes 0 for every period, which supports the evidence in favor of there being no effect. It is important to recall that AIDS is a stigmatized infection (see St Lawrence et al.

[1] Baseline mortality rates can be found in appendix section II.

(1990),) which might deter people from getting either tested or treated at public —and even private— facilities.

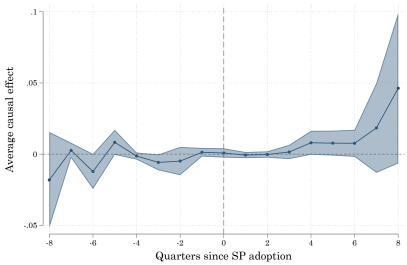
Table 5.2. SP effect on mortality rates

	Breast Cancer	Newborns	AIDS
SP	0.0098*	0.1363	0.0001
	(0.0057)	(0.0944)	(0.0008)
Municipalities	1410	1410	1127
Balanced Panel	Yes	Yes	No
Outcome Mean	0.013	0.596	0.042
Controls	No	No	No

Note: This table shows the weighted average of dynamic ATT's for each of the three main targets of SP expenditure. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications. The sample used for the AIDS estimation is not a balanced panel since AIDS-related deaths were not registered for certain municipalities.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

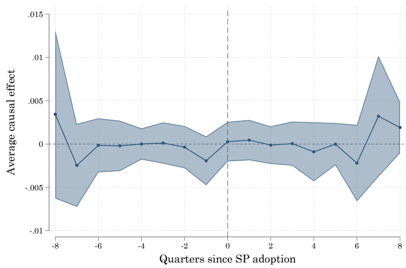
Figure 5.2. Event study

(a) Breast Cancer



(b) Newborns

(c) AIDS



Note: This figure shows event-study plots of the effect of SP implementation on the mortality rate of breast cancer, newborns and AIDS using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

5.1.2 Two main death causes

Another important question is whether SP was effective in reducing the mortality rate by effectively preventing and/or treating diabetes or high blood-pressure, the two most common death causes. Before getting to the estimates, when studying mortality rates it is worth recalling that statistics —and hence the data generating process— depend on what doctors state as the death cause. While some deaths can be directly attributed to a particular cause (e.g. homicides) there are other death causes that are hard to pin down since an initial disease can cause the development of other complications. Particularly in this section it is important to keep in mind that diabetes and high blood-pressure are two very closely related conditions since people with diabetes are very likely to develop high blood-pressure. Anecdotal evidence from doctors suggests there is no consensus among mexican doctors regarding when to diagnose diabetes as the sole or main death cause[2].

High Blood-Pressure SP reduced high blood-pressure MR by around 0.036 deaths per 1000, an economically significant reduction accounting for 23.5 % of the baseline MR. The large effect SP has on high blood-pressure mortality rate can be explained by taking into account that there has also been a large effort by the mexican authorities to make people conscious about their weight and diet. Then the implementation of SP along with people's behavioural changes might be driving the reduction in this particular mortality rate.

Diabetes SP effects on diabetes are very volatile and there seems to be no evidence of changes in the diabetes-related mortality rate. In the case of diabetes, even when people might *have* insulin at home for their health care, they still need to *apply* the treatment to themselves. Because of this, it is possible that SP implementation is an inaccurate measure of diabetes treatment and prevention, which might be the driving force behind these null results. Two

[2] I interviewed doctors at Medica Sur, a private hospital in Mexico City, who confirmed diabetes deaths are regularly listed with other possible death causes and that there is no consensus among doctors regarding when a diabetes-provoked disease should cause doctors to label diabetes as the main death cause.

other possible —non-exclusive— scenarios might be happening: (i) given diabetes' slow-paced development, treatment effects might not be detectable even 2 years after programme implementation or (ii) individuals with diabetes *and* blood-pressure conditions are the ones being treated effectively by SP, hence reflecting in the reduction of the high blood-pressure MR.

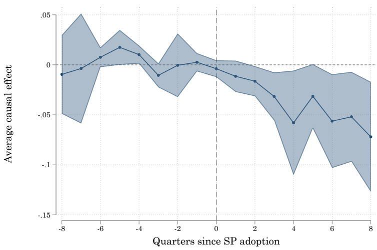
Table 5.3. SP effect on mortality rates

	High BP	Diabetes
SP	-0.0366***	0.0051
	(0.0139)	(0.0097)
Municipalities	1410	1410
Balanced Panel	Yes	Yes
Outcome Mean	0.239	0.137
Controls	No	No

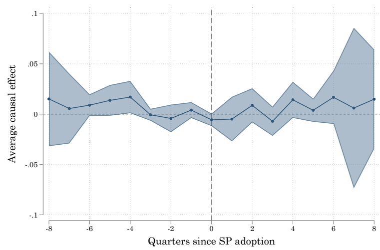
Note: This table shows the weighted average of dynamic ATT's for the two most common causes of death in Mexico from 2000 to 2019. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 5.3. Event study

(a) High BP



(b) Diabetes



Note: This figure shows event-study plots of the effect of SP implementation on the mortality rate of high blood-pressure and diabetes using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

5.2 Effect heterogeneity across municipality marginalization

Even if on the aggregate there seem to be no statistically significant effects for some mortality rates, Seguro Popular might have been more effective in poorer municipalities and thus noise introduced by rich municipalities in the estimation might be masking SP effects. If poorer municipalities had less health infrastructure and worse quality of health services overall, then implementation of SP should have improved the quality of services and the infrastructure for healthcare for these poor municipalities more than it did for the richer ones.

Motivated by this, I study whether there exist heterogenous treatment effects across municipalities because of their marginalization levels. For the estimations presented in this section I also use the estimator proposed in de Chaisemartin and d'Haultfoeuille (2022).

Throughout this section I label a municipality as poor if its 2000 marginalization index is "High" or "Very High"[3] according to CONAPO (CONAPO (2000)), following Ginja and Conti (2015).

5.2.1 All cause mortality

Contrary to what one could believe *ex ante*, the reduction seen on the all-cause mortality rate is driven by reductions in rich municipalities rather than in poor ones. However, one cannot reject the existence of a declining mortality trend in rich municipalities even before SP implementation. King et al. (2009) find that SP affiliation within communities is larger for poorer areas. If we believe[4] their result can be extrapolated to the whole mexican population then it might be the case that, in poor municipalities, both people who can benefit (in health-related terms) from SP and people who cannot benefit are getting affiliated to SP, which would attenuate results in these type of municipalities. If within richer municipalities only those who can benefit the most from SP are the ones affiliating then that

[3] The results are robust to also labeling municipalities whose marginalization index is "Medium" as poor, as shown in Appendix section IV.

[4] Big if.

would explain the reductions in the mortality rate for rich municipalities.

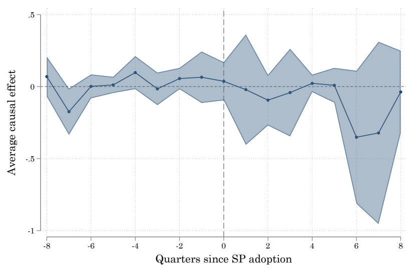
Table 5.4. SP effect on the all cause mortality rate - Poor vs Rich municipalities

	Mortality Rate	
	Poor	Rich
SP	-0.0778	-0.0573**
	(0.1218)	(0.0281)
Municipalities	172	1238
Balanced Panel	Yes	Yes
Controls	No	No

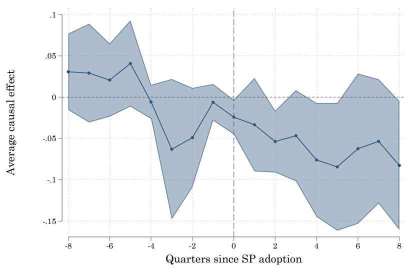
Note: This table shows the weighted average of dynamic ATT's for the all-cause mortality rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 5.4. Event study

(a) Poor



(b) Rich



Note: This figure shows an event-study plot of the effect of SP implementation on the all cause mortality rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

5.2.2 Three main SP expenditure targets

Breast Cancer In line with the result observed for the all-cause mortality rate, the effects detected in the breast cancer MR can be fully explained by increases in rich municipalities rather than in poor ones. One possibility is that, as documented in King et al. (2009), there do not exist changes in medical services usage but nonetheless if SP caused increases in the *quality* of medical provision then it is possible that richer municipalities, where baseline quality was better overall, benefited more from the improvement of the medical infrastructure and staff. King et al. (2009) document that "among SP enrolees, 69 % rated the quality of health services as very good or good," which could be interpreted as SP being of overall good quality.

Newborns Serván-Mori et al. (2015) find that lower-income women are more likely to receive timely ante-natal care, but they find no SP effect on it. Although the coefficient for the SP effect in rich municipalities is now statistically significant, this is likely a mechanical result once the data is split and one considers the estimate obtained when pooling both types of municipalities. However, looking at panels (c) and (d) of Figure 5.5 one can notice that only two ATT_{gt}'s are statistically significant and their corresponding confidence intervals are very close to zero. Apart from that, SP effects on newborns might reflect in the birth rate rather than in the mortality rate since the birth rate tells us the number of live births per 1000 people and SP aimed at improving the conditions in which women gave birth. I test whether the birth rate significantly increased in section 6 to complement this result.

AIDS As expected given the stigmatized status of AIDS there is no effect of SP on either type of municipality. Also, AIDS is nowadays a more controlled disease than it was years before, so another outcome of interest for evaluating the effectiveness of SP expenditure on AIDS antiretrovirals or HIV might be HIV incidence rather than mortality.

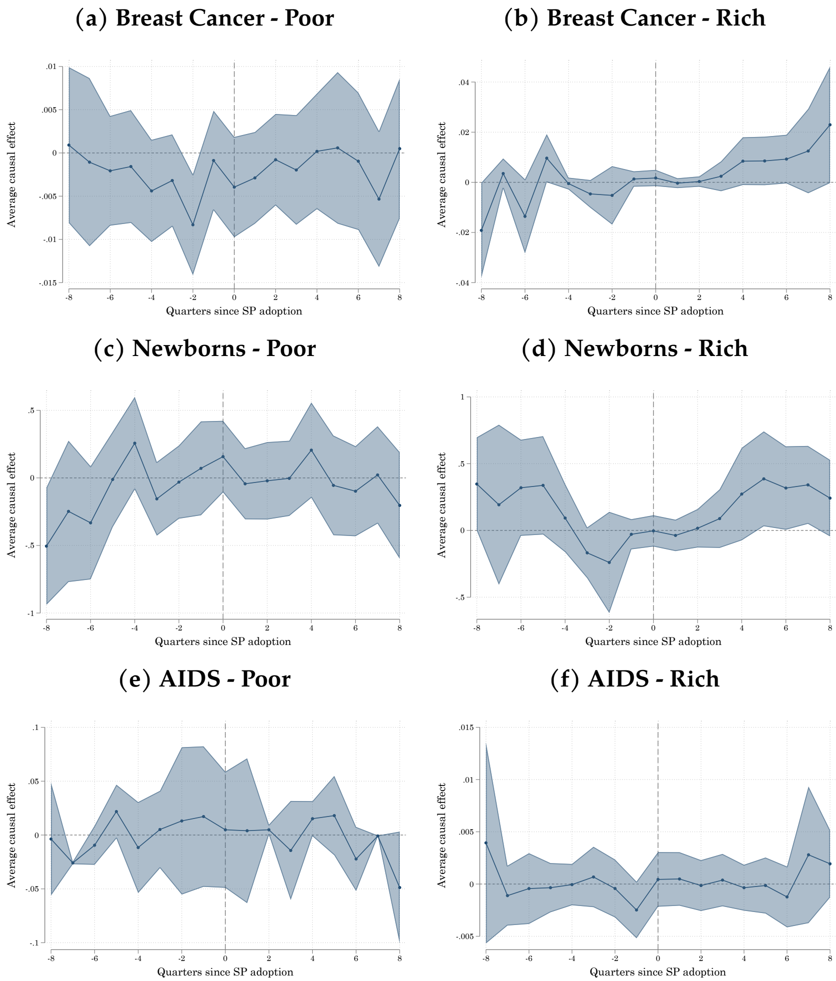
Table 5.5. SP effect on mortality rates - Poor vs Rich municipalities

	Breast Cancer		Newborns	
	Poor	Rich	Poor	Rich
SP	-0.0016	0.0072*	0.0074	0.1774*
	(0.0024)	(0.0039)	(0.1240)	(0.1054)
Municipalities	172	1238	172	1238
Balanced Panel	Yes	Yes	Yes	Yes
Controls	No	No	No	No

	AIDS	
	Poor	Rich
SP	-0.0050	0.0003
	(0.0088)	(0.0008)
Municipalities	152	975
Balanced Panel	No	No
Controls	No	No

Note: This table shows the weighted average of dynamic ATT's for each of the three main targets of SP expenditure. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 5.5. Event study



Note: This figure shows an event-study plot of the effect of SP implementation on the mortality rate of the three main SP expenditure targets using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

5.2.3 Two main death causes

Diabetes and High Blood-pressure The null effect found for diabetes is due to null effects across both municipality types and not because of the effects having opposite signs and cancelling each other out. However, as noted in the main estimation, the effect of SP on diabetes might be reflecting in the high blood-pressure mortality rate. Table 5.6 shows the entirety of the reduction in the high blood-pressure mortality rate is due to reductions in rich municipalities. Although Parker, Saenz, and Wong (2018) do not find increases in diabetes nor high blood-pressure *treatment*[5], they do find that SP affiliates are more likely to receive diagnostic and preventive care. Because of this one could conjecture that SP effects on high blood-pressure should be increasing over time, which is supported by estimates in panel (b) of Figure 5.6.

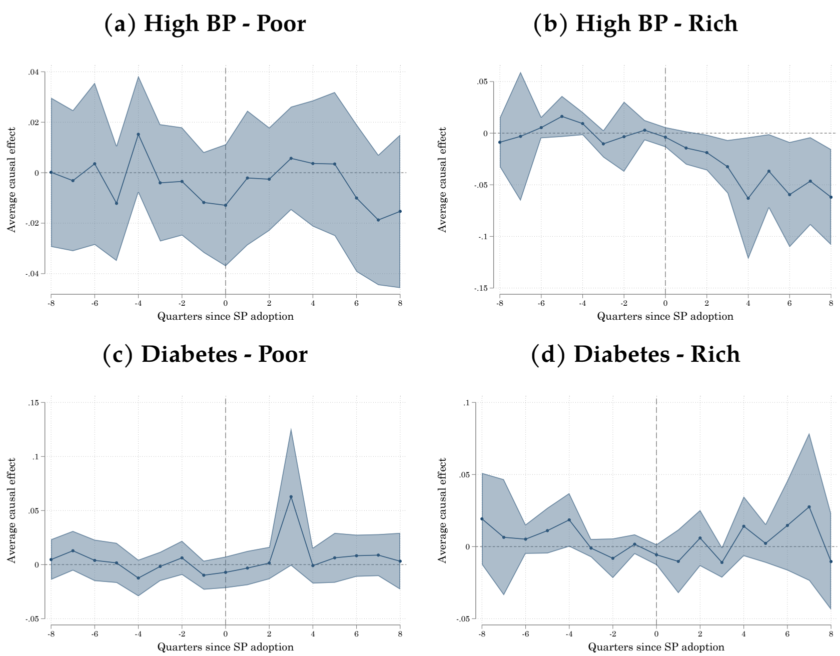
Table 5.6. SP effect on mortality rates - Poor vs Rich municipalities

	High BP		Diabetes	
	Poor	Rich	Poor	Rich
SP	-0.0045	-0.0371***	0.0086	0.0029
	(0.0093)	(0.0143)	(0.0072)	(0.0085)
Municipalities	172	1238	172	1238
Balanced Panel	Yes	Yes	Yes	Yes
Controls	No	No	No	No

Note: This table shows the weighted average of dynamic ATT's for the two most common causes of death in Mexico from 2000 to 2019. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

[5] Although they use a longitudinal survey, which might lack validity regarding the long-run effects of the programme.

Figure 5.6. Event study



Note: This figure shows an event-study plot of the effect of SP implementation on the mortality rate of the two main death causes from 2000–2019 using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

5.3 Robustness Checks

Although the estimator proposed in de Chaisemartin and d'Haultfœuille (2020) is robust to heterogeneous treatment effects across municipalities and time, there could still be some doubt of whether a simple post- vs pre-SP implementation comparison across implementation cohorts really identifies the ATT. In this section I present the same estimation as done before but including covariates in the estimation as suggested in de Chaisemartin and d'Haultfoeuille (2020) as a robustness check for the estimates shown in the previous section. For every estimation in this section I control for the municipality-quarter level of the (log) population, median luminosity, share of women, share of insured population, share of informal workers, average educational attainment and industry shares. These are variables that could potentially bias the estimate if they are confounders of the causal relationship between SP and mortality rates.

As it can be seen in the subsequent tables and figures, the point estimates remain practically unchanged and there is almost no gain in estimation precision due to the inclusion of covariates (the AIDS estimation actually becomes noisier for poor municipalities when including covariates.) These results suggest that either the estimation without controls indeed identifies the causal effect of SP implementation on mortality rates, or rather that the former and these estimations including covariates are both similarly biased. However, the identification assumptions are likely to hold in my context (see section 3,) so I interpret the negligible difference between these estimates and the ones presented in the Main Results section as evidence in favour of a correct identification strategy.

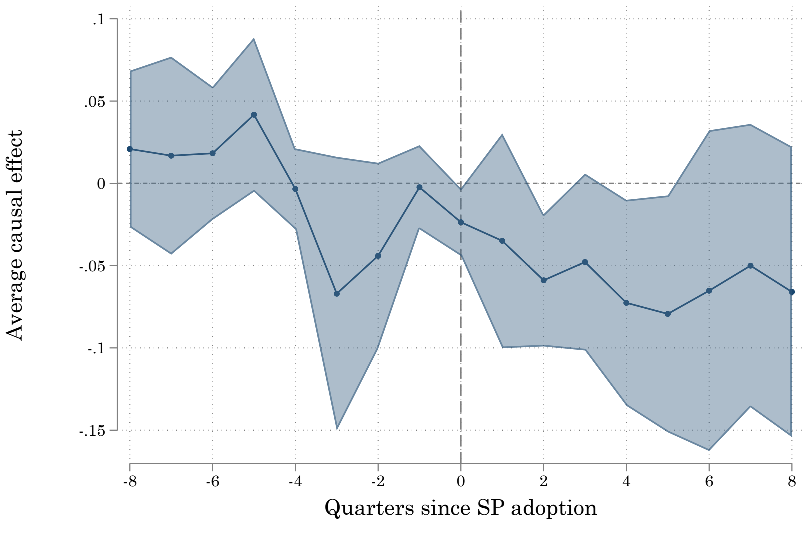
Table 5.7. SP effect on the overall mortality rate

	Mortality Rate
SP	-0.0553**
	(0.0281)
Municipalities	1410
Balanced Panel	Yes
Outcome Mean	1.015
Controls	Yes

Note: This table shows the weighted average of dynamic ATT's for the all-cause mortality rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 5.7. Event study

(a) All cause mortality



Note: This figure shows an event-study plot of the effect of SP implementation on the all cause mortality rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

Table 5.8. SP effect on mortality rates

	Breast Cancer	Newborns	AIDS
SP	0.0071*	0.1795*	-0.0002
	(0.0042)	(0.1028)	(0.0012)
Municipalities	1410	1410	1127
Balanced Panel	Yes	Yes	No
Outcome Mean	0.013	0.596	0.042
Controls	Yes	Yes	Yes

Note: This table shows the weighted average of dynamic ATT's for each of the three main targets of SP expenditure. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

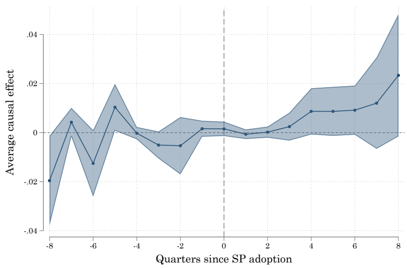
Table 5.9. SP effect on mortality rates

	High BP	Diabetes
SP	-0.0353***	0.0047
	(0.0134)	(0.0089)
Municipalities	1410	1410
Balanced Panel	Yes	Yes
Outcome Mean	0.239	0.137
Controls	Yes	Yes

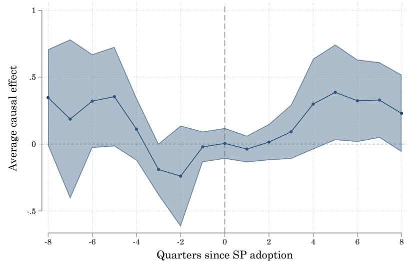
Note: This table shows the weighted average of dynamic ATT's for the two most common causes of death in Mexico from 2000 to 2019. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 5.8. Event study

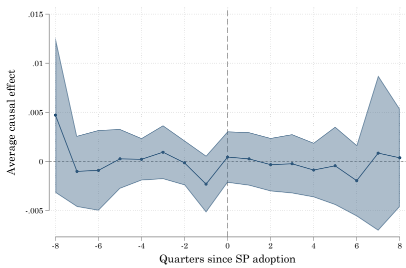
(a) Breast Cancer



(b) Newborns



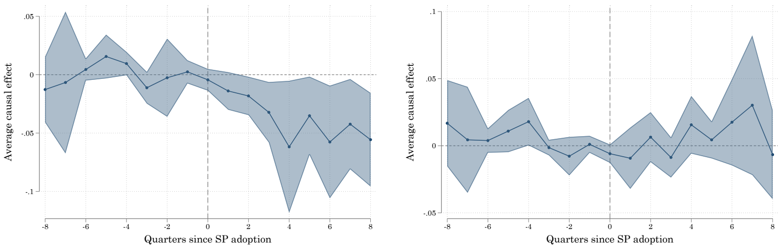
(c) AIDS



Note: This figure shows event-study plots of the effect of SP implementation on the mortality rate of breast cancer, newborns and AIDS using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

Figure 5.9. Event study

(a) High BP **(b) Diabetes**



Note: This figure shows event-study plots of the effect of SP implementation on the mortality rate of high blood-pressure and diabetes using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

5.4 Effect heterogeneity across municipality marginalization

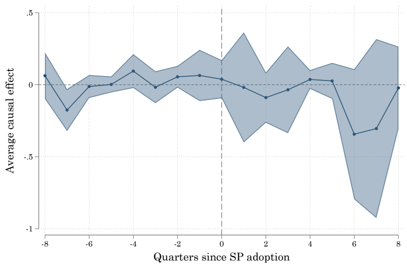
Table 5.10. SP effect on the all cause mortality rate - Poor vs Rich municipalities

	Mortality Rate	
	Poor	Rich
SP	-0.0691	-0.0532**
	(0.1205)	(0.0248)
Municipalities	172	1238
Balanced Panel	Yes	Yes
Controls	Yes	Yes

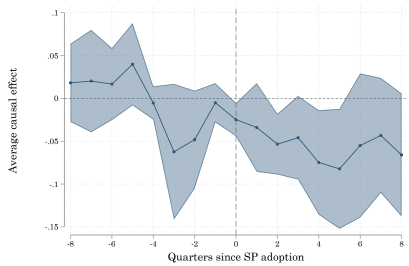
Note: This table shows the weighted average of dynamic ATT's for the all-cause mortality rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 5.10. Event study

(a) All-cause MR - Poor



(b) All-cause MR - Rich



Note: This figure shows an event-study plot of the effect of SP implementation on the all cause mortality rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

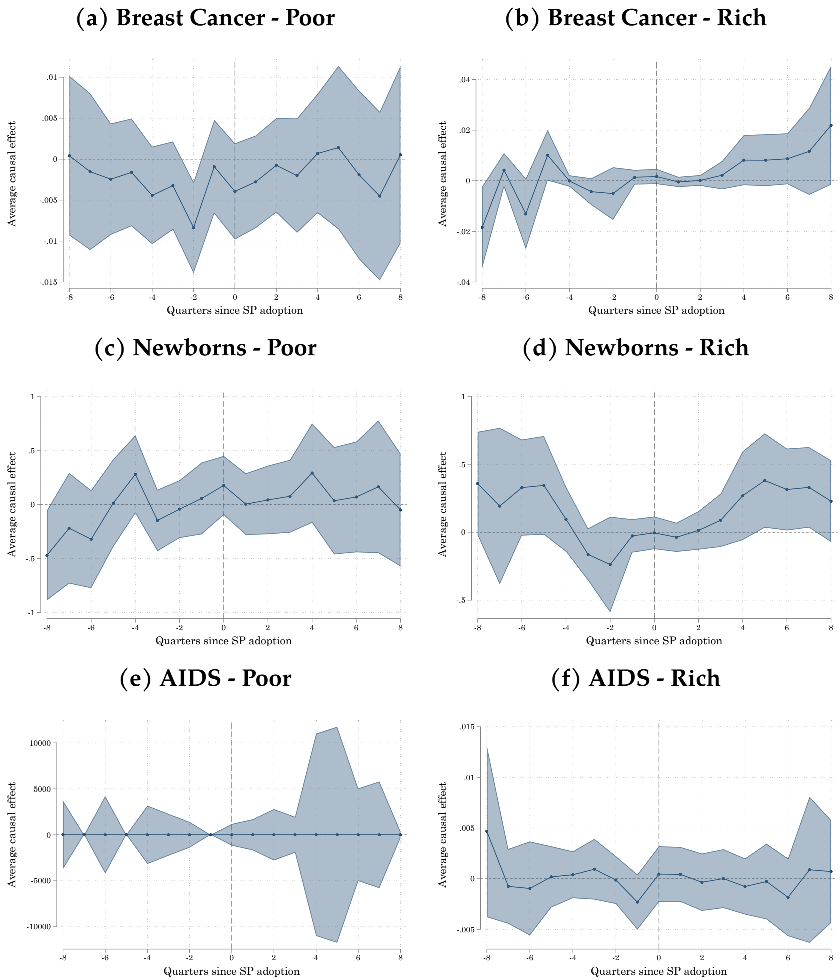
Table 5.11. SP effect on mortality rates - Poor vs Rich municipalities

	Breast Cancer		Newborns	
	Poor	Rich	Poor	Rich
SP	-0.0015	0.0067*	0.0918	0.1729*
	(0.0029)	(0.0040)	(0.1724)	(0.1008)
Municipalities	172	1238	172	1238
Balanced Panel	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

	AIDS	
	Poor	Rich
SP	0.0319	-0.00009
	(1042.6144)	(0.0012)
Municipalities	152	975
Balanced Panel	No	No
Controls	Yes	Yes

Note: This table shows the weighted average of dynamic ATT's for each of the three main targets of SP expenditure. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
p < 0.10 * p < 0.05 ** p < 0.001 ***

Figure 5.11. Event study



Note: This figure shows an event-study plot of the effect of SP implementation on the mortality rate of the three main SP expenditure targets using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

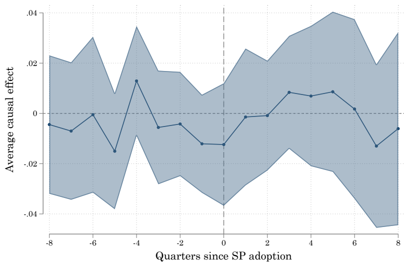
Table 5.12. SP effect on mortality rates - Poor vs Rich municipalities

	High BP		Diabetes	
	Poor	Rich	Poor	Rich
SP	-0.0005	-0.0346***	0.0106	0.0040
	(0.0108)	(0.0121)	(0.0078)	(0.0081)
Municipalities	172	1238	172	1238
Balanced Panel	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

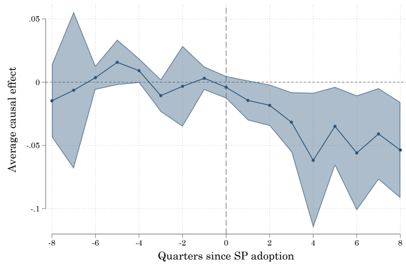
Note: This table shows the weighted average of dynamic ATT's for the two most common causes of death in Mexico from 2000 to 2019. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 5.12. Event study

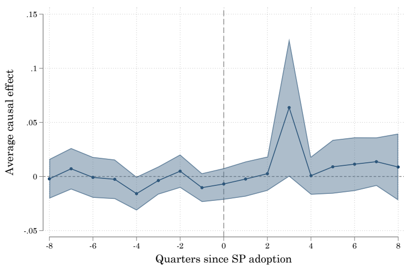
(a) High BP - Poor



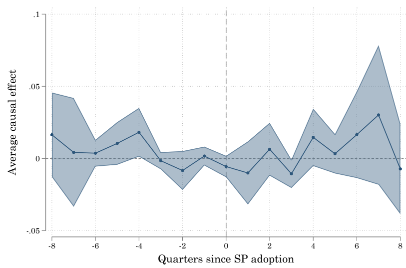
(b) High BP - Rich



(c) Diabetes - Poor



(d) Diabetes - Rich



Note: This figure shows an event-study plot of the effect of SP implementation on the mortality rate of the two main death causes from 2000–2019 using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

Chapter 6

Did SP increase the birth rate and abortions? Complementary results on the null SP effect for newborn's mortality

6.1 The effect of SP on abortions and live-births

One of the main targets of Seguro Popular was to provide women with better ante-natal care as well as to improve the birth-giving conditions and the attention received by newly born babies and women after giving birth.

In section 5 I show that SP had no effect on the newborn mortality rate. One could argue that SP being unable to reduce the newborn mortality rate is a negative outcome since a lot of resources were devoted to that goal. However, this null result can be complemented by analyzing the effects SP had on both the abortion mortality rate and the birth rate. *Ex ante* one would expect SP to increase the birth rate if it improved the quality of the medical staff responsible for birth-giving duties and of the infrastructure used, assuming a constant fertility rate (given the population age distribution in Mexico, the fertility rate likely increased over time while SP was being rolled out.) Simultaneously, if SP improved the conditions on which abortions could be made then, conditional on wanting an abortion, SP makes it more attractive to have an abortion at medical facilities and hence being reported. If the birth rate increases and there is no effect on abortions one could argue that SP effectively improved newborn's life by increasing the probability of any given birth being a live birth.

6.1.1 Aggregate results

SP does not seem to have affected the abortion mortality rate. This can be interpreted as SP not changing the medically recommended abortions during the SP-implementation period. In this case, even when the abortion mortality rate mean is so low —and hence realistic effect sizes being low as well—, it does not seem that the null result is due to the study being underpowered. Nonetheless, voluntary abortions were not legal in any mexican state until 2007, a fact that helps explaining why SP had no effects on the abortion mortality rate.

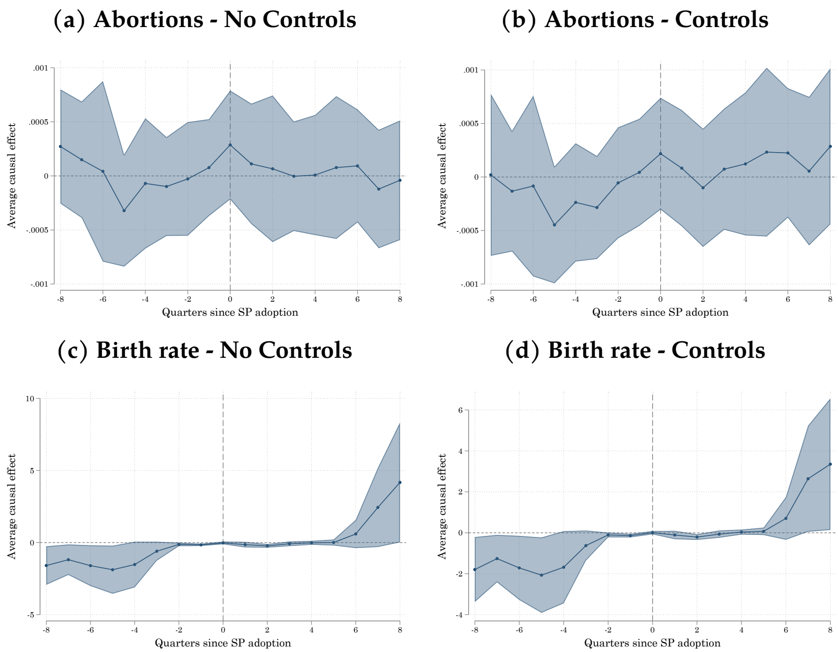
SP did increase the birth rate by 0.71 more live-births per 1000 people, on average. This result implies that, even if SP in fact increased the newborn's mortality rate, the increase in live-births per 1000 is 5 times as large as the mortality rate increase (0.13 more newborn deaths per 1000.) The three results together, namely the (insignificant) increase on newborn's mortality rate, the null effect on abortions and the increase in the birth rate, can be interpreted as SP having overall positive effects on newborn's welfare. Still some caution is needed when associating these results to conclusive positive welfare impacts since it could be the case that, given SP's implementation, couples might expect birth-giving to be safer and hence increase the number of children they want to have. Ideally I would need a record of pregnancies in order to assess whether the number of pregnancies increased because of SP but to the best of my knowledge such data do not exist and the closest one would be the birth registry, so I cannot differentiate between SP causing pregnancy increases or SP effectively increasing the live births per 1000, holding pregnancies constant. The results are robust to the inclusion of controls (see Table 6.1.)

Table 6.1. SP effect on abortions and birth rate

	Abortions		Birth Rate	
SP	0.00005	0.00013	0.7474*	0.7136*
	(0.00022)	(0.00024)	(0.4168)	(0.3671)
Municipalities	1410	1410	1410	1410
Balanced Panel	Yes	Yes	Yes	Yes
Outcome Mean	0.001		4.184	
Controls	No	Yes	No	Yes

Note: This table shows the weighted average of dynamic ATT's for abortions and the birth rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 6.1. Event study



Note: This figure shows an event-study plot of the effect of SP implementation on the abortion mortality rate and the birth rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

6.1.2 Heterogeneity by municipality marginalization

Once again, estimates show SP had effects only in rich rather than poor municipalities[1]. As Knox (2018) documents, it does not seem to be the case that SP was effective in targeting indigenous communities, so no SP effect on poor municipalities' birth rate might be related to the fact that poor municipalities are more populated with indigenous citizens who might prefer to give birth using traditional birth-giving methods rather than hospital-related ones. This hypothesis contradicts findings in Ginja and Conti (2015), where they show suggestive evidence of SP increasing the number of deliveries that occur in a hospital rather than at home particularly in poor municipalities[2].

Table 6.2. SP effect on abortions - Rich vs Poor

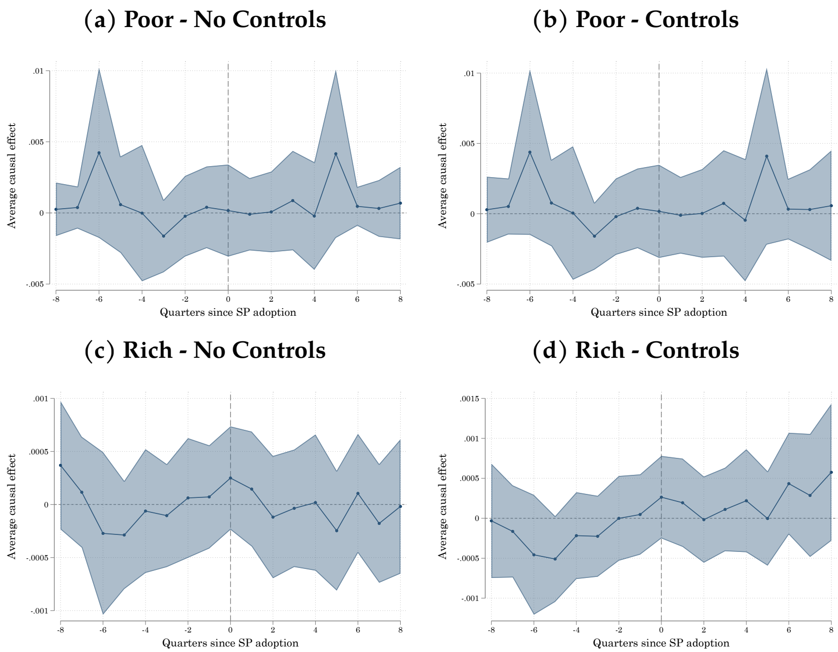
	Abortions			
	Poor	Rich	Poor	Rich
SP	0.00070	-0.00001	0.00062	0.00023
	(0.00120)	(0.00022)	(0.00148)	(0.00024)
Municipalities	172	1238	172	1238
Balanced Panel	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes

Note: This table shows the weighted average of dynamic ATT's for the abortion mortality rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

[1]Results are robust to the alternative definition of "Poor" that includes municipalities labeled as having a marginalization index of "'Medium" in 2000 as poor as well. See Appendix section V..

[2]Our definition of "Poor municipality" is the same.

Figure 6.2. Abortions - Event study



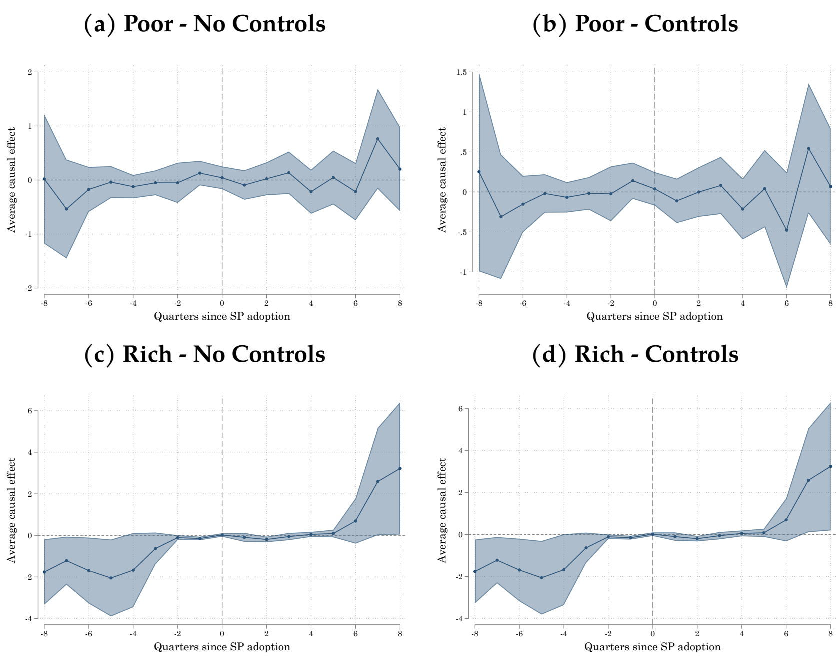
Note: This figure shows an event-study plot of the effect of SP implementation on the abortion mortality rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

Table 6.3. SP effect on Birth Rate - Rich vs Poor

	Birth Rate			
	Poor	Rich	Poor	Rich
SP	0.0464	0.7004*	-0.0215	0.7042*
	(0.1800)	(0.3674)	(0.1747)	(0.3514)
Municipalities	172	1238	172	1238
Balanced Panel	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes

Note: This table shows the weighted average of dynamic ATT's for the birth rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure 6.3. Birth Rate - Event study



Note: This figure shows an event-study plot of the effect of SP implementation on the birth rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

Discussion and conclusions

Discussion

Having analyzed the effects of SP on the all-cause MR and MR's of breast cancer, newborns, AIDS, high blood-pressure and diabetes using state-of-the-art econometric methods, it is not evident that SP helped in reducing them —with the exception of the all-cause and high blood-pressure MR's—. An important question is then why could it be that such a large-scale program failed to reduce the mortality rates of the 3 main expenditure targets while being able to achieve some of its main goals such as reducing catastrophic expenses and having targeted fairly well its intended population (Knox (2018), Campos-Vázquez and Knox (2013), King et al. (2009), Barros (2009), and González-Pier et al. (2006).) I present a few hypotheses on this regard.

Do present costs exceed expected benefits for individuals? It has been documented that while SP increased demand for preventive care services among the poor (Knox (2018),) this demand increase was accompanied by an increase in waiting times at health care facilities. Longer waiting times imply an increased opportunity cost and so individuals may prefer not to visit medical facilities even after affiliating to SP. This is particularly relevant for poor individuals, who are most likely to rely on their daily income for making ends meet. Even when a negative health shock is likely to reduce the lifetime earnings of any individual, it is plausible that people give a larger weight to costs incurred in the present and thus underestimate the benefits of having good health over their lifetime span.

Is AIDS still taboo? Sexually transmitted diseases have long been taboo. During the 80's, HIV began spreading within particular groups of people who later on became stigmatized by others (St Lawrence et al. (1990).) If it is still the case that being infected with HIV/AIDS is stigmatized, then people will be less confident in attending medical clinics for diagnosis and treatment. Rural and lowly populated communities where people know each other better might suffer most from stigma.

Are local practices a preferred alternative? In the case of newborns it might be the case that those who could mostly benefit from attending prenatal care are the ones least attracted to it (Knox (2018).) It is worth noting that *affiliation* differs from actual *usage*, so even if indigenous people affiliate to SP they could still prefer to abide to local practices pre-birth and give birth as their social norms suggest them to. This is a policy opportunity area, where policymakers could try not only to make modern technology available to indigenous people, but rather make sure they have the supplies necessary to treat themselves according to their customs.

Disagreement on the main death cause Diabetes is not only a death cause itself, but it also induces other diseases such as cardiovascular diseases and cancer. If there does not exist an agreement among the medical staff —as anecdotal evidence suggests— of whether to classify a death as being caused by diabetes or by another disease previously caused by diabetes then that would explain the null effect found for this death cause. Additionally, SP effects on diabetes might be reflected in the high blood-pressure mortality rate, given the close relationship between both diseases.

Conclusion

This thesis adds to the literature on the effects of SP on health-related outcomes by using state-of-the-art econometric methods to evaluate the impact SP had on the all-cause mortality rate and the mortality rates of diabetes, high blood pressure, newborns, AIDS and breast cancer.

On the aggregate it seems that SP is not effective in improving health. The only exceptions are a reduction on the all-cause mortality rate of 0.052 deaths per 1000 (5.1 % over the 2000–2011 mean) and a reduction on the high blood-pressure mortality rate of 0.036 deaths per 1000, which accounts for a reduction of 23.5 % over the baseline MR. Importantly, this study only analyses one facet of the many different ones that a programme such as SP aims to alleviate. In order for policymakers to conclusively state whether a programme was useful or not in increasing welfare, comprehensive analyses studying more facets of the programme should be made. When possible, partnerships between government officials and researchers should be made in order to design and evaluate aspects of the programme that might be of general interest.

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Appendix

I. Seguro Popular implementation across municipalities

Figure A-4. Municipalities with SP implemented per quarter

	Implemented SP?				Implemented SP?		
	No	Yes	Total		No	Yes	Total
2000q1	2427	0	2427	2006q1	821	1606	2427
2000q2	2299	128	2427	2006q2	806	1621	2427
2000q3	2294	133	2427	2006q3	718	1709	2427
2000q4	2291	136	2427	2006q4	526	1901	2427
2001q1	2288	139	2427	2007q1	294	2133	2427
2001q2	2286	141	2427	2007q2	142	2285	2427
2001q3	2284	143	2427	2007q3	87	2340	2427
2001q4	2281	146	2427	2007q4	58	2369	2427
2002q1	2279	148	2427	2008q1	45	2382	2427
2002q2	2276	151	2427	2008q2	36	2391	2427
2002q3	2276	151	2427	2008q3	22	2405	2427
2002q4	2105	322	2427	2008q4	17	2410	2427
2003q1	2088	339	2427	2009q1	15	2412	2427
2003q2	2023	404	2427	2009q2	13	2414	2427
2003q3	1962	465	2427	2009q3	13	2414	2427
2003q4	1908	519	2427	2009q4	6	2421	2427
2004q1	1861	566	2427	2010q1	0	2427	2427
2004q2	1840	587	2427	2010q2	0	2427	2427
2004q3	1723	704	2427	2010q3	0	2427	2427
2004q4	1608	819	2427	2010q4	0	2427	2427
2005q1	1420	1007	2427	2011q1	0	2427	2427
2005q2	1317	1110	2427	2011q2	0	2427	2427
2005q3	1236	1191	2427	2011q3	0	2427	2427
2005q4	973	1454	2427	2011q4	0	2427	2427

Note: This table shows the implementation of SP across municipalities over time. This table also helps as a reference for knowing how many *switchers* and *stayers* are available at each quarter for the comparison in the estimation using the method proposed by de Chaisemartin and d'Haultfoeuille (2022).

II. Baseline (2000) mortality rates

Table A-4. Quarterly mortality rates in 2000

	Q1		Q2		Q3	
Disease	Mean	SD	Mean	SD	Mean	SD
Breast Cancer	.0173	.0238	.0176	.0252	.0175	.0264
Newborns	1.3549	1.5217	1.4504	1.6028	1.4845	1.6653
AIDS	.0192	.0168	.0167	.0147	.0186	.0161
Diabetes	.1421	.0933	.1048	.069	.112	.0745
High BP	.1779	.107	.1382	.0871	.1376	.0888

Note: This table shows mean quarterly mortality rates and their standard deviations for the year 2000, which serves as baseline. Means and standard deviations are weighted analytically with municipality population levels in 2000. Computations are done with data from the Population Census, conducted by INEGI.

Table A-5. Quarterly mortality rates in 2000

	Q4		Year
Disease	Mean	SD	Mean
Breast Cancer	.0179	.0252	.0176
Newborns	1.4281	1.7361	1.4295
AIDS	.0182	.0161	.0182
Diabetes	.1281	.0848	.1217
High BP	.1587	.0977	.1531

Note: This table shows mean quarterly mortality rates and their standard deviations for the year 2000, which serves as baseline. Means and standard deviations are weighted analytically with municipality population levels in 2000. The last column shows the simple average of quarterly mortality rates in 2000. Computations are done with data from the Population Census, conducted by INEGI.

III. Baseline mortality rates by municipality marginalization

This tables show mean quarterly mortality rates and their standard deviations for the year 2000 by type (poor - rich) of municipality, which serves as baseline. Means and standard deviations are weighted analytically with municipality population levels in 2000. The column labeled "Year" shows the simple mortality rates average. Computations are done with data from the Population Census, conducted by INEGI.

Table A-6. Quarterly mortality rates in 2000 - Poor municipalities

	Q1		Q2		Q3	
Disease	Mean	SD	Mean	SD	Mean	SD
Breast Cancer	.0066	.0295	.0051	.0341	.0043	.0306
Newborns	.7745	2.1771	.834	2.0047	.7996	1.9871
AIDS	.0322	.0441	.0326	.0528	.0377	.0413
Diabetes	.0516	.0795	.0433	.0671	.0482	.0677
High BP	.1412	.1513	.1074	.126	.1125	.1332

Table A-7. Quarterly mortality rates in 2000 - Rich municipalities

	Q1		Q2		Q3	
Disease	Mean	SD	Mean	SD	Mean	SD
Breast Cancer	.0164	.023	.0168	.0239	.0167	.0259
Newborns	1.3462	1.4697	1.4373	1.5977	1.5079	1.6847
AIDS	.0177	.0167	.0152	.0135	.0177	.0163
Diabetes	.142	.0879	.1039	.0646	.1094	.0689
High BP	.1707	.0989	.134	.0817	.1316	.0815

Table A-8. Quarterly mortality rates in 2000 - Poor municipalities

	Q4		Year
Disease	Mean	SD	Mean
Breast cancer	.0063	.0312	.0056
Newborns	.8976	2.3817	.8264
AIDS	.0458	.057	.0371
Diabetes	.0526	.0743	.0489
High BP	.1251	.1428	.1215

Table A-9. Quarterly mortality rates in 2000 - Rich municipalities

	Q4		Year
Disease	Mean	SD	Mean
Breast Cancer	.0164	.0237	.0166
Newborns	1.4225	1.7128	1.4285
AIDS	.0175	.0154	.017
Diabetes	.1256	.0785	.1202
High BP	.152	.0894	.1471

IV. Robustness Check - Alternative definition of marginalization

In this section, municipalities are labeled as poor if their marginalization index in 2000 according to CONAPO was “Very High”, “High” or “Medium” (CONAPO (2000)). The results are practicall unchanged for point estimates and significance with respect to the definition taking “Medium” marginalized municipalities as rich is also no different.

IV.1 Without controls

Effect heterogeneity across municipality marginalization

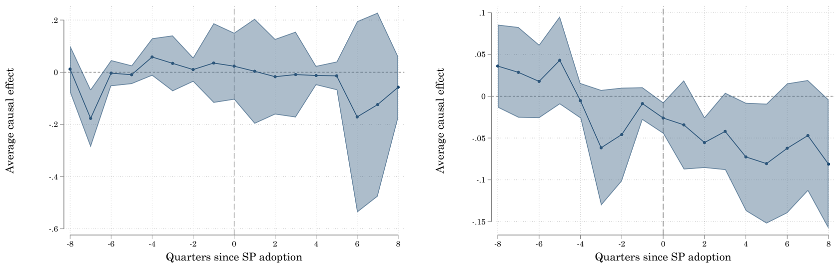
Table A-10. SP effect on the all cause mortality rate - Poor vs Rich municipalities

	Mortality Rate	
	Poor	Rich
SP	-0.0348	-0.0557**
	(0.0748)	(0.0246)
Municipalities	467	943
Balanced Panel	Yes	Yes
Controls	No	No

Note: This table shows the weighted average of dynamic ATT's for the all-cause mortality rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure A-5. Event study

(a) Poor (b) Rich



Note: This figure shows an event-study plot of the effect of SP implementation on the all cause mortality rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

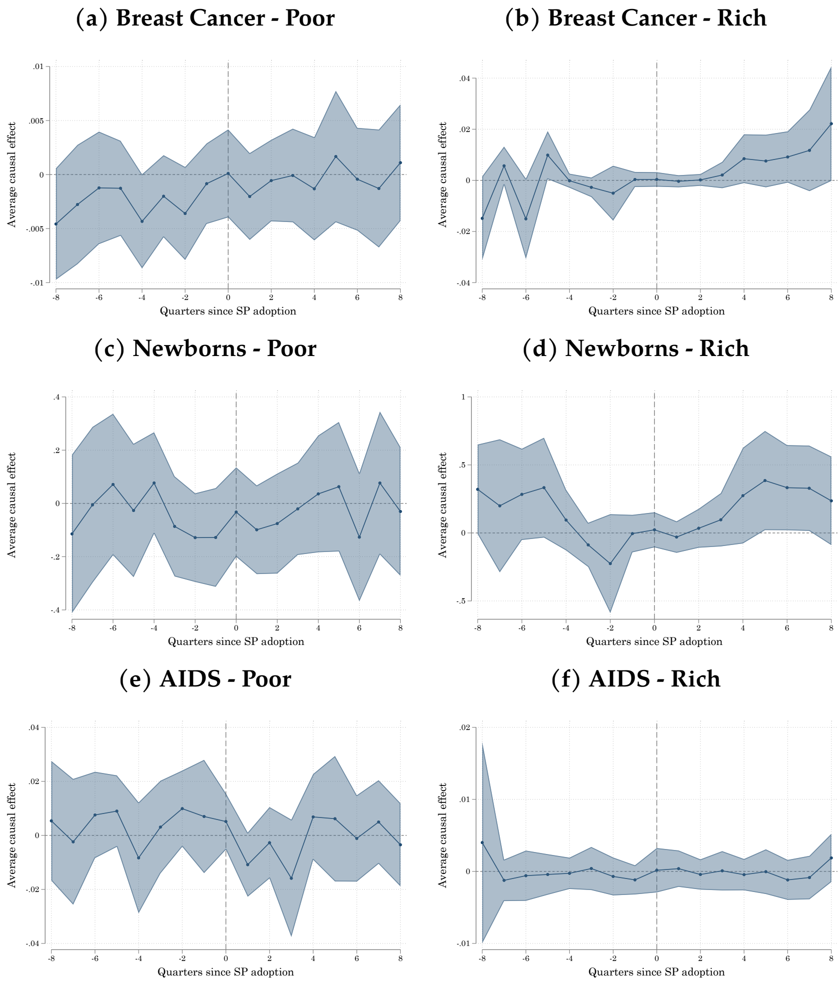
Table A-11. SP effect on mortality rates - Poor vs Rich municipalities

	Breast Cancer		Newborns	
	Poor	Rich	Poor	Rich
SP	-0.0003	0.0067*	-0.0296	0.1840*
	(0.0016)	(0.0038)	(0.0807)	(0.1077)
Municipalities	467	943	467	943
Balanced Panel	Yes	Yes	Yes	Yes
Controls	No	No	No	No

	AIDS	
	Poor	Rich
SP	-0.0015	-0.00008
	(0.0036)	(0.0008)
Municipalities	378	734
Balanced Panel	No	No
Controls	No	No

Note: This table shows the weighted average of dynamic ATT's for each of the three main targets of SP expenditure. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure A-6. Event study



Note: This figure shows an event-study plot of the effect of SP implementation on the mortality rate of the three main SP expenditure targets using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

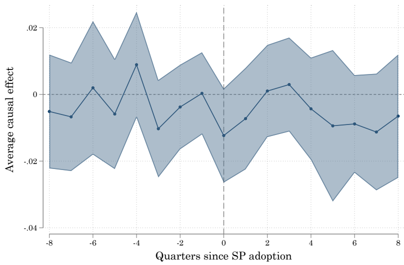
Table A-12. SP effect on mortality rates - Poor vs Rich municipalities

	High BP		Diabetes	
	Poor	Rich	Poor	Rich
SP	-0.0060	-0.0344***	0.0039	0.0022
	(0.0057)	(0.0124)	(0.0050)	(0.0078)
Municipalities	467	943	467	943
Balanced Panel	Yes	Yes	Yes	Yes
Controls	No	No	No	No

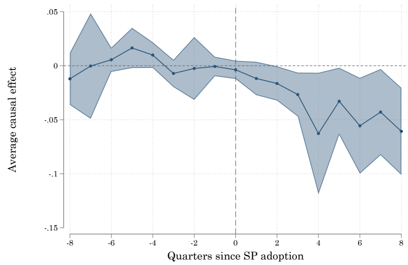
Note: This table shows the weighted average of dynamic ATT's for the two most common causes of death in Mexico from 2000 to 2019. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure A-7. Event study

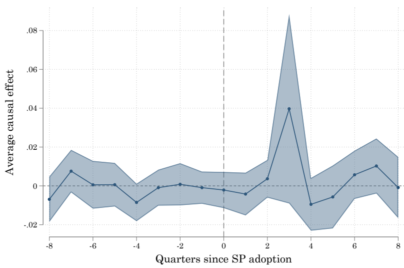
(a) High BP - Poor



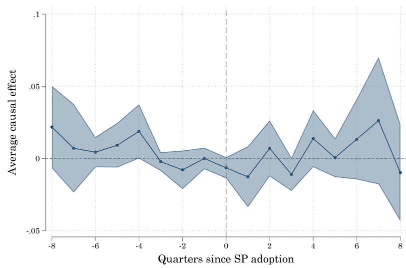
(b) High BP - Rich



(c) Diabetes - Poor



(d) Diabetes - Rich



Note: This figure shows an event-study plot of the effect of SP implementation on the mortality rate of the two main death causes from 2000–2019 using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

IV.2 With controls

Effect heterogeneity across municipality marginalization

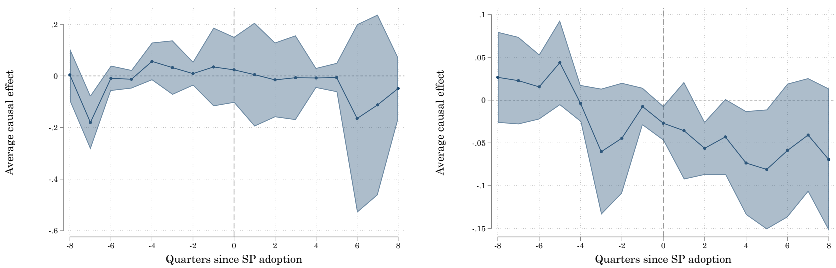
Table A-13. SP effect on the all cause mortality rate - Poor vs Rich municipalities

	Mortality Rate	
	Poor	Rich
SP	-0.0303	-0.0557**
	(0.0746)	(0.0246)
Municipalities	467	943
Balanced Panel	Yes	Yes
Controls	Yes	Yes

Note: This table shows the weighted average of dynamic ATT's for the all-cause mortality rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure A-8. Event study

(a) Poor **(b) Rich**



Note: This figure shows an event-study plot of the effect of SP implementation on the all cause mortality rate using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

Table A-14. SP effect on mortality rates - Poor vs Rich municipalities

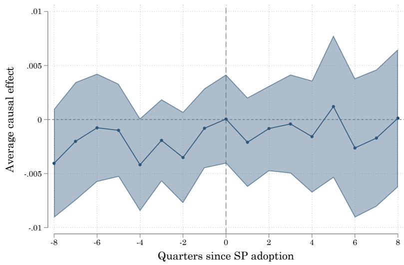
	Breast Cancer		Newborns	
	Poor	Rich	Poor	Rich
SP	-0.0008	0.0065	0.0122	0.1842*
	(0.0019)	(0.0042)	(0.0950)	(0.1103)
Municipalities	467	943	467	943
Balanced Panel	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

	AIDS	
	Poor	Rich
SP	0.0012	-0.0007
	(0.0112)	(0.0013)
Municipalities	378	734
Balanced Panel	No	No
Controls	Yes	Yes

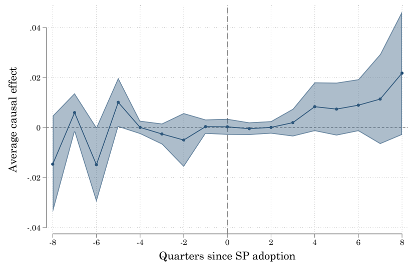
Note: This table shows the weighted average of dynamic ATT's for each of the three main targets of SP expenditure. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure A-9. Event study

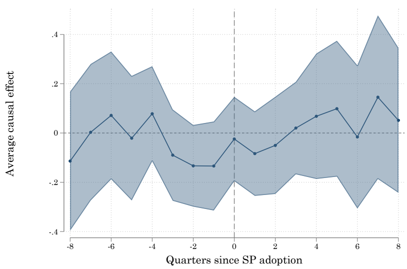
(a) Breast Cancer - Poor



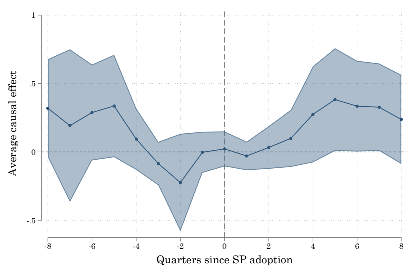
(b) Breast Cancer - Rich



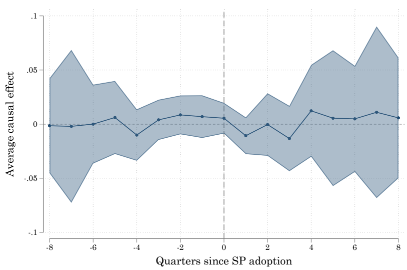
(c) Newborns - Poor



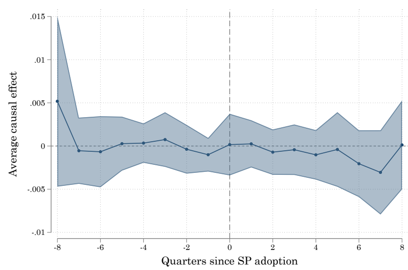
(d) Newborns - Rich



(e) AIDS - Poor



(f) AIDS - Rich



Note: This figure shows an event-study plot of the effect of SP implementation on the mortality rate of the three main SP expenditure targets using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

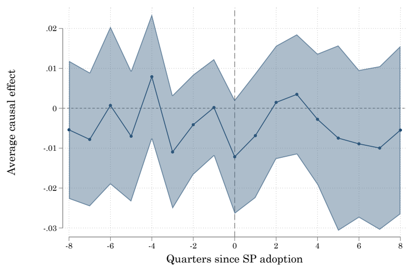
Table A-15. SP effect on mortality rates - Poor vs Rich municipalities

	High BP		Diabetes	
	Poor	Rich	Poor	Rich
SP	-0.0052	-0.0322***	0.0045	0.0034
	(0.0064)	(0.0120)	(0.0052)	(0.0087)
Municipalities	467	943	467	943
Balanced Panel	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

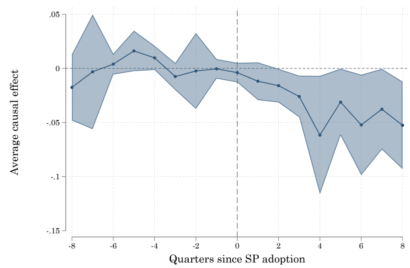
Note: This table shows the weighted average of dynamic ATT's for the two most common causes of death in Mexico from 2000 to 2019. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Figure A-10. Event study

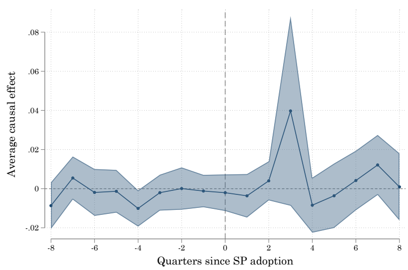
(a) High BP - Poor



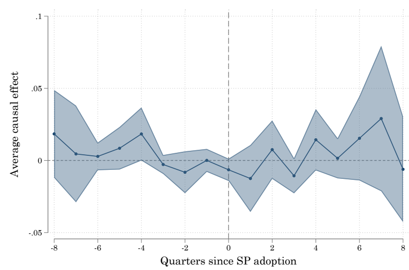
(b) High BP - Rich



(c) Diabetes - Poor



(d) Diabetes - Rich



Note: This figure shows an event-study plot of the effect of SP implementation on the mortality rate of the two main death causes from 2000–2019 using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2022). Standard errors are clustered at the municipality level and generated via 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.

V. Abortions and Birth Rate - Alternative definition of marginalization

Table A-16. SP effect on abortions - Rich vs Poor

	Abortions			
	Poor	Rich	Poor	Rich
SP	-0.00049	0.00010	-0.00050	0.00034
	(0.00075)	(0.00022)	(0.00081)	(0.00025)
Municipalities	467	943	467	943
Balanced Panel	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes

Note: This table shows the weighted average of dynamic ATT's for the abortion mortality rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***

Table A-17. SP effect on Birth Rate - Rich vs Poor

	Birth Rate			
	Poor	Rich	Poor	Rich
SP	0.0457	0.6539**	0.0191	0.6374*
	(0.0950)	(0.3194)	(0.0914)	(0.3272)
Municipalities	467	943	467	943
Balanced Panel	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes

Note: This table shows the weighted average of dynamic ATT's for the birth rate. The weights are given by the number of switchers used in the estimation of each dynamic effect. Standard errors are clustered at the municipality level and computed using 300 bootstrap replications. Standard errors remain almost unchanged when computed with 100 and 200 bootstrap replications.
$p < 0.10$ * $p < 0.05$ ** $p < 0.001$ ***