Did Mexico's Seguro Popular Universal Health Coverage Programme Really Reduce Formal Jobs?

By Enrique Seira, Isaac Meza, Eduardo González-Pier and Eduardo Alcaraz Prous

WIEGO Working Papers

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About the Authors

Enrique Seira is the Frederick S. Addy Distinguished Professor of Economics at Michigan State University. He is a research affiliate at the Abdul Latif Jameel Poverty Action Lab (J-PAL) and founder of Qué Funciona para el Desarrollo (QFD), a non-profit academic organization dedicated to Mexico's social and economic development. Previously, he was the Director of the Centro de Investigación Económica at the Instituto Tecnológico Autónomo de México (ITAM). He can be contacted at: enrique.seira@gmail.com.

Isaac Meza is a PhD student in the Economics Department at Harvard University. He can be contacted at: i.mezal.12@gmail.com.

Eduardo González-Pier is an economist with more than 25 years of experience in the health and social security sectors. He is a Global Fellow at the Wilson Center in Washington D.C. Former appointments include serving as Deputy Minister of Health of Mexico and Finance Director of the Instituto Mexicano del Seguro Social (IMSS). He can be contacted at: egpier@gmail.com.

Eduardo Alcaraz Prous is the Coordinator of Planning and Evaluation of the Directorate of Incorporation and Collection at the Instituto Mexicano del Seguro Social (IMSS). He can be contacted at: eduardo.alcarazp@imss.gob.mx.

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Abstract

Prominent international institutions have written that social protection benefits that are tied to not having formal employment make informal employment more attractive and thus reduce formal employment by shifting workers to the informal sector. While the argument is simple and consistent, the question is an empirical one. We assess this hypothesis by studying Mexico's Seguro Popular (SP). SP has been at the forefront of this debate both because of its large size, covering half of Mexico's population and close to 50 million people, and because it often has been portrayed as the leading example of an informality-inducing social policy. This Working Paper uses the roll-out of Mexico's SP across municipalities to quantitatively assess its impact on private sector formal employment in Mexico, using more detailed data and improved econometric methods compared to previous papers. We find no robust evidence of a decrease in formal employment, suggesting the attraction of SP was not large enough to overcome the benefits of having a formal job. We also find no effects in average salaries of jobs affiliated to the Mexican Institute of Social Security (IMSS), further suggesting that there were no strong shifts in labour supply from the formal to the informal sector. We need more work on the benefits side of SP, as the benefits side should be considered in an assessment of SP welfare consequences.

Preface from WIEGO

Global frameworks and the widely recognized importance of social protection during the COVID-19 crisis have generated momentum towards the realization of Universal Social Protection. At a global level, the Social Protection Floors (SPF) and Universal Social Protection frameworks, which are grounded in human rights principles, International Labour Standards and the Sustainable Development Goals (SDGs), provide a key set of principles and actions that should underpin the extension of social protection to all, including workers in informal employment. They call for the development of social protection systems that are based on rights and provide protection for all throughout the life-course via a mix of equitably financed social assistance and social insurance. Despite these frameworks and important improvements over the last few years, substantive coverage gaps remain, especially for the world's two billion workers in informal employment who remain largely excluded from social protection.

One reason for the slow progress, WIEGO believes, is a set of influential policy ideas that hold back the expansion of social protection to informal workers. Although the Universal Social Protection framework has gained support from stakeholders including governments, the World Bank, International Monetary Fund, UN agencies and civil society, in practice, key principles remain contested at both the level of global financial institutions and within the design, financing and implementation of schemes at national level.

The idea that certain forms of social protection cause substantial increases in informality presents a particular challenge to governments wanting to invest in social protection for workers in informal employment. Specifically, social protection systems that combine employment-linked social insurance with tax-financed social assistance for low-income informal workers are claimed by some to be key drivers of informality, which is then held responsible for low productivity and underdevelopment. This argument was most clearly outlined in Santiago Levy's 2008 book on Mexico's social protection system: 'Good Intentions, Bad Outcomes: Social Policy, Informality, and Economic Growth in Mexico'.[1] Recently, this line of thinking featured notably in UNDP's 2021 Regional Human Development Report for Latin America[2], which declares that "social protection policies contribute to informality" because they "tax formality and subsidize informality" (*p.* 279). In the same year, the IMF's report on 'The Global Informal Workforce: Priorities for Inclusive Growth'[3] dedicates significant space to making the case that "payroll taxation on formal sector workers [..] increase the cost of doing business and create double taxation of labor, thus encouraging informality. Further, means-tested benefits [..] generate severe disincentive effects and often create poverty traps" (p.254).

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[1] Levy, Santiago. 2008. Good Intentions, Bad Outcomes: Social Policy, Informality, and Economic Growth in Mexico.

[2] UNDP.2021. Regional Human Development Report 2021. Trapped: High Inequality and Low Growth in Latin America and the Caribbean.

[3] IMF. 2021. The Global Informal Workforce. Priorities for Inclusive Growth.

While this may sound a bit academic, these claims have real-world impacts. Following a presentation on this issue by WIEGO to the Inter-American Social Security Conference in November 2022, Miguel Ángel Ramírez Villela, the conference's Head of the Projects Division, told us: "Today when someone presents a proposal to create some new benefit, one of the first objections raised is whether it would not create more informality. In general, this idea, that non-contributory programmes encourage informality, was adopted quickly and uncritically. It has been almost a dogma and very few people have dared to question it."[4]

WIEGO believes it is critical to question this claim for several reasons.

First, the argument is based principally on a voluntarist view of informality, implying that workers and firms in the informal economy face few meaningful structural, economic, regulatory or gendered constraints in choosing the nature and status of their employment. In this view, informal workers and firms carefully and rationally weigh the costs and benefits of informality relative to formal work. To minimize costs and maximize benefits, they deliberately seek to avoid regulations and taxation where possible. While this voluntary-choice theory may be helpful in exploring the persistence of informality for a sub-set of actors, such as informal entrepreneurs, ignoring the constraints that workers in informal employment, especially women workers, face risks over-emphasizing informal actors' ability to choose the status of their employment, which is severely limited for many. It can also deflect attention away from the need to reduce barriers to decent work.

Second, uncritically accepting these arguments wholesale would threaten the equitable financing of social protection systems. Social protection systems have multiple objectives, including the smoothing of consumption over the lifetime, providing insurance against economic and demographic risks, protecting against poverty and redistributing between richer and poorer. To effectively meet these objectives, different forms of financing are needed. Financing for consumption smoothing and insurance are generally linked to people's incomes and employment, as they seek to stabilize incomes throughout the life-course. These are therefore generally based on social security contributions. On the other hand, poverty prevention and redistribution objectives are financed through general taxes, as they require the shifting of resources to those who have little. To achieve these multiple objectives, and also recognizing the differences in people's incomes and contributory capacities, both taxes and social security contributions are needed. Mixed systems are particularly important to expand social protection to workers in informal employment who often have low contributory capacities and therefore require both tax-financed schemes and subsidization with social insurance systems. This means that genuine Universal Social Protection should always be the outcome of mixed systems – and therefore subject to the claim that they increase informality.

A social protection system that is based on these claims might still enable universal coverage of some sort but would likely increase the regressiveness of financing and provide more limited protection, in particular to the informal working poor. To avoid theorized "perverse incentives towards informality", such systems would largely do away with social security contributions and rely to a much greater extent on indirect taxation, thereby shifting the overall financial burden from employers and workers towards workers and taxpayers. Capital would make no further contributions to the financing of social protection beyond general taxes. Beyond a basic safety net financed mainly by consumption taxes, people would be encouraged (potentially through subsidies) to purchase private insurance, which would be delinked from work and be "actuarially fair", meaning devoid of redistribution. Such proposals are the logical conclusion placing the "perverse incentives" argument at the centre of discussions on how to achieve Universal Social Protection and have been consistently proposed from Levy (2008) to the World Bank (2019).[5]

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[4] Juergens-Grant, Florian. 2022. Is social protection to blame for informality? New evidence and reflections on inclusive, adequate and fair social protection systems. XXXI Session of the Permanent Seminar on Welfare in the Americas. Inter-American Conference on Social Security (ICSS). Presentation on November 16, 2022.

[5] Packard, Truman, Ugo Gentilini, Margaret Grosh, Philip O'Keefe, Robert Palacios, David Robalino, and Indhira Santos. 2019. Protecting All: Risk Sharing for a Diverse and Diversifying World of Work. Human Development Perspectives. Washington, DC: World Bank. doi: 10.1596/978-1-4648-1427-3. License: Creative Commons Attribution CC BY 3.0 IGO.

Finally, it is worth noting that the logic of this argument can be applied to any form of labour regulation, such as minimum wages and occupational health and safety requirements. Therefore, this line of thinking not only threatens the expansion of equitably financed social protection to workers in informal employment, but also provides a rationale for wider deregulation of labour.

Surprisingly, given the confidence with which these claims are made, they are drawn from a literature that can best be described as mixed. WIEGO is aware of roughly a dozen credible studies that set out to empirically estimate the effects of different social protection programmes – contributory, non-contributory, cash transfers and health insurance schemes – on a variety of measures of employment. Of those, a bit more than half find increases in informality or decreases of formality, although almost always for specific sub-groups of the population, such as older people, parents with young children or men working in particular sectors. Conversely, a handful of studies find increases in formality or reductions in informality. There are also important methodological and data challenges in a number of papers. For instance, all studies except one use surveys to estimate the impacts of Mexico's Seguro Popular universal health coverage scheme, one of the most studied schemes. The problem is that Mexico does not have a representative municipal level survey. The most credible study (Bosch & Campos-Vazquez 2014) therefore relies on municipal level administrative data from Mexico's social security institute to study the impact of SP on formal jobs. They found that the programme decreased formal employment in small firms by four per cent, or about 17,000 formal jobs per year.

As Levy's analysis of SP in 2008 put concerns around incentives on the agenda of social protection policy makers, and Bosch & Campos-Vazquez (2014) is widely cited as in support of the argument that social protection drives informality, we thought it would be a good idea to return to where it all started and take another careful look at the evidence.

WIEGO worked with an outstanding group of Mexican economists and policy makers, Enrique Seira, Isaac Meza, Eduardo González-Pier and Eduardo Alcaraz Prous, who revisited the question: Did Mexico's Seguro Popular universal health coverage programme really reduce formal jobs?

This study aims to contribute to a more nuanced conversation on whether or not social protection programmes generate informality. Another, probably more important question, is whether small-incentives effects, if they do exist, matter at all when compared to the well-documented benefits of social protection for workers in informal employment, as well as the human right to social protection.

Florian Juergens-Grant and Laura Alfers
Social Protection Programme, WIEGO

1. Introduction

According to the International Labour Organization, "Two billion people – more than 61 percent of the world's employed population – make their living in the informal economy" (ILO, 2018). Most do not have access to social protection programmes to insure against risks like unemployment and health shocks. The ILO calculates that just 47 per cent of the global population is effectively covered by at least one social protection benefit. This seems both unfair and inefficient. It works against basic social justice requirements (Dworkin, 2006), and exposes workers to financial and productivity risks derived from health and labour hazards (Hoynes et al., 2016). The COVID-19 pandemic has been a highly regressive income and health shock. The increased unemployment from disrupted labour markets and higher mortality weighed more heavily among low-income populations, most of them not covered by health insurance (Arceo-Gomez et al., 2022). The pandemic has become a stark reminder of the need for stronger more inclusive social protection mechanisms (Schwandt et al., 2022).

Policy makers and international institutions often focus on the cost of social protection policies, not only in terms of fiscal burdens, but specifically on the claim that they shift jobs from the formal to the informal sector of the economy (Levy, 2008; UNDP, 2021). The argument is that social protection systems that combine employment-linked social insurance with tax-financed social assistance for low-income workers in informal employment increase informality because, from an employer's perspective, they increase the cost of creating a formal job relative to an informal one. Moreover, from a worker perspective, the claim is that the introduction of non-contributory social assistance benefits for those in the informal sector increases the attractiveness of informal jobs as they can now access at least some social assistance benefits outside of formal employment. For example, in a recent IMF report (2021), the authors warn that means-tested benefits "generate severe disincentive effects and often create poverty traps".

This argument – that we call the "distortion-towards-informality" – is intuitive and internally consistent, but it carries two caveats. The first is that moving across sectors may not be frictionless or even desired, and the incentive created by SP may not be strong enough to attract substantial numbers of workers from the formal sector to the informal one. Formal-sector jobs are typically better paid and have higher quality medical care. These jobs tend to operate in different industries and geographies, and workers with job-specific human capital in the formal sector may not want to switch to the informal sector just because they have (lower quality) health care in the form of SP.

There may be only a small set of people at the margin between working in a formal versus an informal job and offering health care in the informal sector may not attract a large number of people. The argument cannot be settled by theory alone, it is necessarily an empirical one. However, existing empirical evidence on the claim that social protection causes informality is very weak in the case of Mexico. Most of the evidence finds that there is no effect on informal or formal jobs from Seguro Popular (SP) (Alonso-Ortiz & Leal, 2018; Campos-Vazquez & Knox, 2013; Azuara & Marinescu, 2013; Barros, 2009).[6] These papers are based on executed surveys and suffer from not being representative at the municipality level. An exception on both counts is Bosch & Campos-Vazquez (2014). They find that SP decreased formal employment. But even this paper finds that the effect is only present for firms with fewer than 50 employees, and they find only a four per cent decrease. They estimate that this effect translates into about 171,000 fewer formal jobs cumulatively, in an economy with close to 20 million formal jobs.[7] In other words, none of the papers we reviewed found that SP increased informality.

The second weakness of the distortion-towards-informality argument is that it only looks at one side of the welfare calculus. The cost of informality – if any – has to be compared to the benefits of social protection programmes. In terms of better health, lower mortality, lower exposure to risk, and greater worker productivity from improved health. Given its size and time in operation, one would expect SP to have protected many families from catastrophic spending in health and generated significant improvements in health outcomes.[8] The negative effect of 17,000 fewer formal jobs per year found by Bosch & Campos-Vazquez (2014), would need to be weighed against the benefits of nearly 50 million people with health coverage. Several papers, such as Finkelstein

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[6] See: https://www.worldbank.org/en/results/2015/02/26/health-coverage-for-all-in-mexico.

[7] Aterido et al. (2011) find tiny effects: no exit from formality. They find lower entry to formality, which lowers the number of formal workers by 0.4–0.7. This study uses survey data.

[8] Several authors, e.g. Levy (2008) do acknowledge there is a benefit side that needs to be measured.

et al. (2012) and Goldin et al. (2020), have shown that health insurance protects income, promotes adequate care and improves mental health. All social protection schemes have costs, and one cannot sensibly argue against them simply for this reason without a balanced look at the benefits as well. Although this can seem obvious, the economic analysis emphasis has most often been on costs until very recently. Acknowledging a new research trend Aizer et al. (2022) note that "Economic research on the safety net has evolved significantly over time, moving away from a near exclusive focus on the negative incentive effects of means-tested assistance on employment, earnings, marriage and fertility to include examination of the potential positive benefits of such programs to children." In this paper we focus on the first caveat related to the SP effect on informality for three main reasons. First, its sheer size. Seguro Popular served close to 50 million people and accounted for about 1 percent of GDP, achieving a decline of 30 percent on catastrophic health spending (Knaul et al., 2012). This makes it a plausible candidate to attract workers to the informal sector. Failing to find effects of such a large programme is a valuable contribution to the debate. Second, SP itself has featured prominently in the distortion-towards-informality debate. Third, the fact that it was implemented in a staggered modality across municipalities over several years, allows us to estimate a causal effect by comparing outcomes in municipalities with SP versus those without it.

Some of the claims of the inducement to informality have been based on analysis of reductions in the number of formal jobs. There are many definitions of what it means for a worker to be formal. In this analysis, we define a worker in the private sector as formal if they are registered with Instituto Mexicano del Seguro Social (IMSS) and therefore pay payroll taxes (used to finance social security for workers in the private sector). This definition has the advantages of being clear, measurable with administrative data covering the entire country, and of connecting to the literature, in particular to what we think is one of the best published research on the topic: Bosch & Campos-Vazquez (2014). We will answer the following question: Is there an effect of Seguro Popular on the number of private sector formal jobs? Does the effect differ by gender, age, temporary employment, and pre-programme formal sector salary? In answering that question we will provide ancillary evidence comparing formal and informal jobs using surveys, and an analysis of the robustness of the results of Bosch & Campos-Vazquez (2014).

The main hypothesis we test, **H1**, is that SP causally decreased the number of workers registered with IMSS. Under the distortion-towards-informality view, the prediction is that municipalities introducing SP will experience a decrease in the number of workers in the formal sector. The reason is that formal workers switch to informal work, because having health care services through SP and not having to pay for them makes these jobs more attractive on the margin. Contrary to this hypothesis we cannot reject the hypothesis of a zero effect of SP on the number of formal jobs. While we can replicate the results of Bosch & Campos-Vazquez (2014) who find that SP gradually reduced formal employment in small firms by four per cent four years after its implementation, we find that their result is not robust to *any* of the following changes: (a) including more municipalities in the sample, (b) controlling for differential employment time trends of municipalities that adopt SP at different stages as recommended by (Wooldridge, 2021), (c) using econometric methods that are robust to heterogeneous and dynamic treatment effects (de Chaisemartin & D'Haultfoeuille, 2022), (d) using an instrumental variable strategy, and (e) controlling for all time invariant worker characteristics using worker fixed effects.

We then test other ancillary hypotheses as well. The second hypothesis, **H2**, revisits H1 but using data at the individual level. That is, instead of looking at municipality level outcomes, we can follow a particular individual who was registered in IMSS in 2000 and ask if she or he is more likely to leave the IMSS-registered job when SP starts operating in his or her municipality. Under this approach we can see if *the same worker* switches, whereas in the municipality level analysis there could have been substantial switching across sectors with a net zero inflows that mask SP making some workers switch. The result at the individual level is again that we cannot reject a zero effect of SP.

Finally, the third hypothesis, **H3**, posits that SP increased salaries in the formal sector. If indeed SP made working informally (i.e. without being registered in IMSS) more attractive, then one would expect that, as the supply of workers in the formal sector decreases, salaries registered in IMSS increase to reflect the increased scarcity of formal workers. Intuitively, formal sector jobs would have to compensate workers at the margin of working informally not to leave. We find no such effect, suggesting again that SP did not disrupt formal job markets.

All in all – in accordance with most of the papers studying SP – results show that SP had no effect on the total number of formal private sector jobs or switching out of a formal job for those already registered in IMSS, contrary to one of the predictions of the distortion-towards-informality view.

Under what circumstances would SP have no effect on formal workers switching to the informal sector? There are several possibilities. One is that those working in the formal and informal sectors have different characteristics and are highly imperfect substitutes for each other. For instance, a person selling tacos in the street may be an unattractive hire at Walmart, or as a teacher. Conversely, those working formally at a hotel may not want to work selling fruit in an outdoor market. Indeed, using employment surveys we find that the observable characteristics of workers with and without IMSS are statistically different even for the limited number of observable characteristics.[9] A second possibility is that the services provided by SP are just not valuable enough to convince people to forgo the typically higher formal sector salaries and better health services at IMSS.[10] We document using the employment surveys that find formal sector jobs command higher salaries, and that when a given person switches from a formal to an informal job his or her salary decreases on average by eight per cent. A third possibility is that the empirical methodology we used is not adequate. This is always a possibility. We note, however, that we are using the more robust econometric methods available, two different identification strategies (difference-in-differences and instrumental variables) and good quality data (essentially an administrative dataset from the social security administration).

Our findings do not imply there is no efficiency loss from the introduction of SP.[11] To start, the government needs to raise taxes to finance it. Moreover, inefficiency may manifest in subtle ways, for instance by decreasing the quality of matches between workers and employers, or by causing workers to leave the labour force. We find these two outcomes unlikely in our context. Further work needs to be done in studying these other margins. But we think that, given the existing evidence, the burden of the proof rests on finding distortionary effects. Importantly, we have to recognize that SP is providing a benefit to millions of Mexicans, and that health improvements should lead to a more productive labour force. An evaluation to inform social protection policies should consider both the costs and the benefits. More research on the benefit side is needed.

The report is structured as follows. Section 2 describes SP and the context in which it operates. It also describes the main definition of formality we use. Section 3 reviews literature related to SP. Section 4 explains the sources of data. Section 5 describes the empirical methods. Section 6 estimates the causal effects of SP on the number of formal jobs, the number of firms registering formal jobs, and average formal salaries. Section 7 uses Mexico's employment surveys to explore why SP may not decrease formal sector jobs, although more research is needed in this regard. Section 8 concludes with some reflections on future lines of work.

2. Context

2.1 Health care coverage in Mexico before Seguro Popular

Since its inception, the Mexican health care system has been characterized by its fragmentation, where population coverage is aligned to labour market segmentation. "The insured population received health care from well financed, vertically integrated, federal institutions, whereas the uninsured relied on underfunded, state decentralized 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, costly private sector" (Knaul et al., 2012). Those earning a salary in the formal private sector (and their families) have access to public social security medical care given by IMSS, the largest provider of health care in Mexico. Registering workers at IMSS is mandatory and involves paying a payroll tax of about 24 per cent of the salary on average. IMSS operates as both an insurer that collects premiums and a provider of social security benefits including health care through its own network of hospitals and primary

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[9] There are many characteristics we cannot observe in the survey data that could tie a worker to the formal sector, like experience and skill in a particular profession, if they can keep a fixed working hour schedule, how much they like having a boss, etc. We suspect that there may be large differences in these unobserved variables as well.

[10] There is a presumption that IMSS health services are better than SP's services. Plus, IMSS gives access to day-care services and helps saving for retirement.

[11] Efficiency in the sense that there may be cheaper ways to achieve medical coverage, distorting markets less.

Figure 1. Insurance affiliation 2000-2020
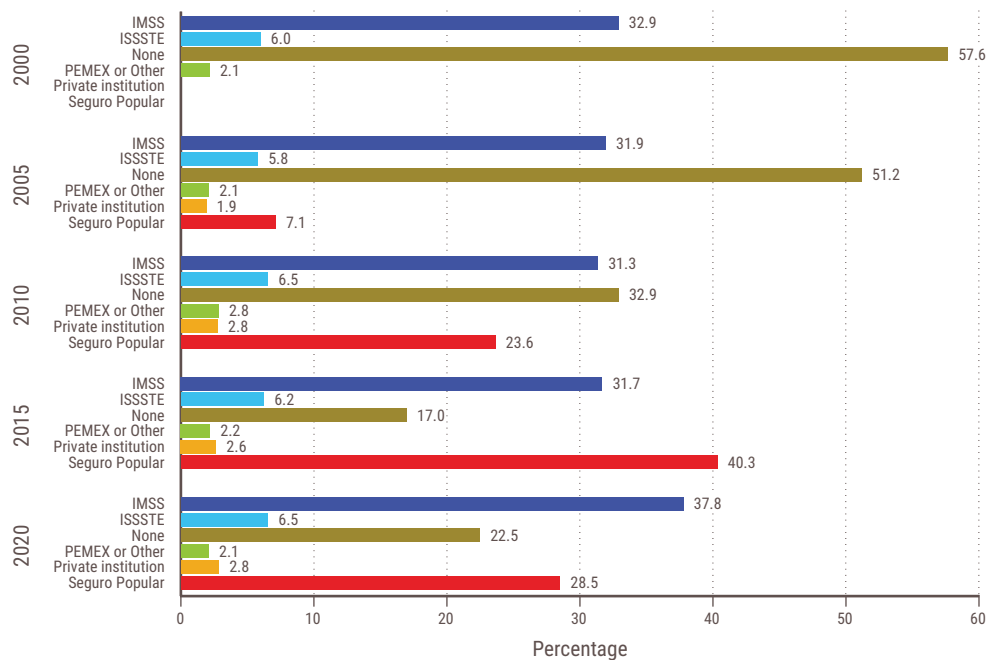
Constructed with data from the 2000, 2010 and 2020 Censo de Población y Vivienda, the 2005 Conteo de Población y Vivienda and the 2015 Encuesta Intercensal, all conducted by INEGI. Each bar represents the proportion of people who claimed being insured by each institution. In particular the question they answered was: Are you affiliated, or do you have the 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.

care clinics.[12] Public servants employed by the federal government are covered by an equivalent but smaller social security institute – ISSSTE. The scheme for the public sector (ISSSTE) is also compulsory. The national oil company PEMEX and the armed services have separate social security ad-hoc arrangements as do the public servants employed by the 32 Mexican states. In general, those having a registered salaried job and their families – roughly half of the population or 60 million people – receive their health care through one of several social health insurance schemes.

The other half of the population not registered in IMSS or ISSSTE, and therefore not paying payroll taxes, "accessed health services through the state ministries of health on a public assistance basis. Health care for this population was funded from uncertain, residual budget allocations that did not have explicit entitlements. Care was not comprehensive, and families paid out-of-pocket, especially for basic services and medicines" (Knaul et al., 2012). Although access to medical care at state-level health ministries was subject to a means-tested user fee, it was

severely underfunded with rationing of services being common. Private health care is accessible for whoever is willing to pay and is unrelated to employment status. For the past 20 years, spending on health has averaged 5.7 per cent of GDP with private spending – most of it out-of-pocket – accounting for half of total spending. Figure 11 in the Appendix plots out-of-pocket expenditure as proportion of current income over time.[13]

Before 2004, when the SP was launched, funding across coverage schemes was very unequal. Comparing IMSS spending to general health spending for the uninsured population, average per capita spending in IMSS was 2.1 times higher than for the uninsured. There was also substantial variation in spending for the different states (Knaul et al., 2012). "The services provided by the states did not ensure access to an explicit package of services and medical procedures and user fees were required for drugs and some medical services" (Azuara & Marinescu, 2013). In 2005, around the time that SP was created, the OECD wrote that public health care spending

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[12] IMSS also covers various benefits beyond health like child care and workers' compensation for work-related injuries.

[13] In 2020, less than 10 per cent of the Mexican population had private health insurance, and about 3 per cent used 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.

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).

Figure 1 provides some detail using census data. One can observe that SP started to gain share in 2005, reached its highest level in 2015 surpassing IMSS, and then decreased as the federal government started to scale it back. Importantly, notice that the share gained by SP comes at the expense of those who were not enrolled in any social health protection scheme.

2.2 The Seguro Popular

The Seguro Popular (SP) – formerly known as the Sistema de Protección Social en Salud – was a Universal Health Coverage scheme enacted by law in 2004 and targeted to the population not covered by social health insurance. SP was devised as a voluntary health insurance made available to those not covered by IMSS or ISSSTE. A new financial architecture was implemented to promote more equitable health budget appropriations across all states. Yearly health budgets to states were made in direct proportion to the number of families they managed to enrol and re-enrol year after year. The SP financial scheme consisted of a tripartite arrangement, seeking to emulate the financing of social health insurance. The federal government contributed in equal terms to SP as it did to IMSS. The state governments used their own tax revenues to substitute for the employer's contribution. These funds were complemented by a means-tested premium paid by families. In practice, the three lower wealth deciles were exempt and small amounts were charged to all others. This matching fund scheme created incentives for state governments to affiliate as many beneficiaries as possible.

In other words, while IMSS medical care was financed mostly by payroll taxes, SP medical care was financed by general taxes. As highlighted by Levy (2008), this was thought to create incentives for working in the informal sector, for the reasons argued in the "distortion-towards-informality" (DTI) argument described in the introduction.[14]
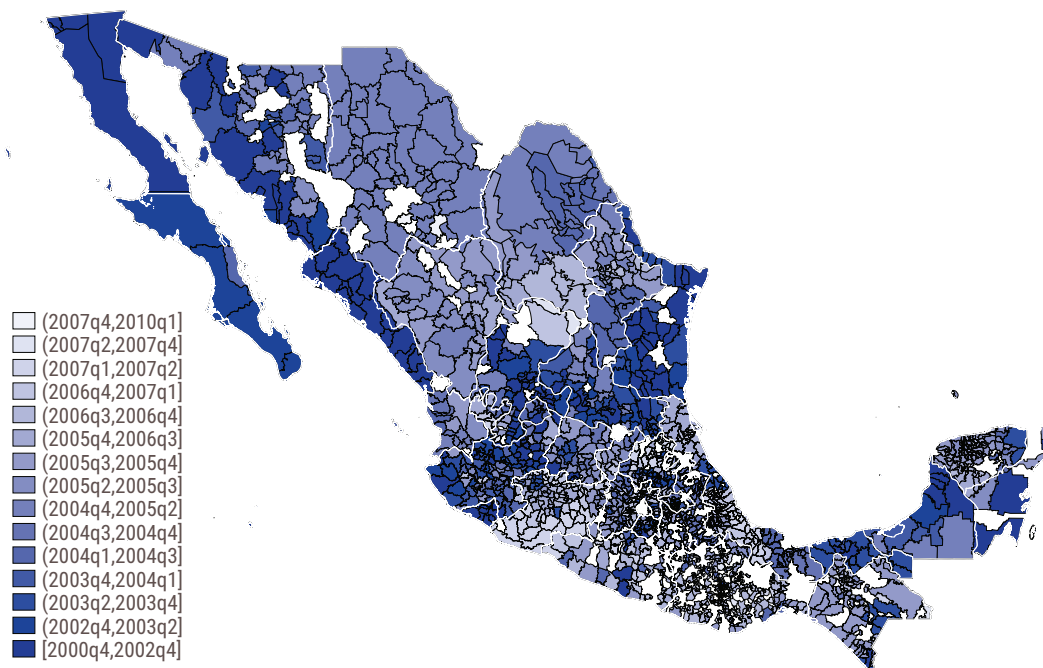
Fresh funds were used by states to increase the supply and quality of services to respond to the health needs and promote re-affiliation. In practice, this meant that more resources would be devoted to locations with less coverage and consequently finance gaps and access gaps were slowly closed. SP covered a basic package of primary health care and essential hospitalization services funded by the per-capita allocation. A concomitant fund for the protection against catastrophic spending provided targeted per-case reimbursement for high specialty care. Thus, universal health care through SP was a combined progression of population coverage, incrementally expanded services and improved financial protection.

The SP became a natural experiment to test the DTI argument. SP was implemented gradually; payment and enrolment mechanisms were first tested with a pilot and, starting in 2004, coverage expanded geographically across states that signed up and within states across municipalities based on health needs, organizational capacity to deliver services, and local budget space. This staggered rollout became an essential component of the empirical strategy to identify the negative effects of social protection programmes like SP on formal employment. Figure 2 presents the geographical expansion across municipalities. By 2011, 29 states had reported universal coverage, while the three remaining states reported 83 per cent coverage.

At the same time as the geographical expansion was taking place, the number and types of procedures covered was increasing. While at the start it only covered 91 interventions, as of 2011 SP's health benefit package (*Catalogo Universal de Servicios de Salud, CAUSES*) covered 275 interventions. This expansion was also significant in terms of spending. Bosch & Campos-Vazquez (2014) explain that while IMSS expenditure declined from 1.7 to 1.5 per cent of GDP from 2003 to 2008, financing to health outside of IMSS increased – basically a SP effect – from 0.8 to 1.2 per cent of GDP in the same period. The expenditure was concentrated in "catastrophic health expenditure", but it also devoted resources to preventive care. Miranda (2012) estimates that household savings from SP can be substantial,

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[14] An oversimplified way to look at this is that, by being registered at IMSS, workers and employers are forced to pay payroll tax. If workers do not value the benefits offered by IMSS to the extent of the tax, there are incentives to avoid it and work informally. To compound the problem, by starting to offer free services, SP makes an IMSS-affiliated job even less attractive for workers and employers as it is no longer tied to receiving health care.

Figure 2. Geographical coverage of SP by municipality

Legend:
(2007q4,2010q1]
(2007q2,2007q4]
(2007q1,2007q2]
(2006q4,2007q1]
(2006q3,2006q4]
(2005q4,2006q3]
(2005q3,2005q4]
(2005q2,2005q3]
(2004q4,2005q2]
(2004q3,2004q4]
(2004q1,2004q3]
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(2003q2,2003q4]
(2002q4,2003q2]
[2000q4,2002q4]

This figure shows the geographic spread of SP. The first municipalities to implement SP are shown in dark blue, while those more recent are shown in white.

with SP achieving a decline of close to 30 per cent in catastrophic health expenditure for every peso spent by households.

SP helped close the gap in health expenditure between those registered at IMSS and ISSSTE and the rest. However, as of 2010, public resources per beneficiary were still about 20 per cent higher in IMSS. Outpatient consultations 40 per cent higher and 2.6 times higher for specialty consultations. Also, IMSS had 30 per cent more nurses and 10 per cent more beds (Table 6 in Knaul et al. (2012)). Having less expense and inputs suggests also that SP provided more basic care than IMSS.

SP was legally repealed on the 1st of January 2020 and replaced by the National Institute of Health for Welfare (INSABI). The INSABI legal reform lacked details on how the financing transition from SP would take place. Its implementation process to date has been highly disorganized and coincided with a Ministry of Health overwhelmed by the need to respond to COVID-19. No formal assessment of the effects of INSABI on health coverage or labour market performance has yet been done.

Formal jobs

This report investigates the effects of SP on formal private-sector jobs. This has several advantages. First, we know exactly when a job is registered at IMSS, thus allowing us to avoid

measurement error in our estimate of private-sector formal jobs – by definition. Second, we have full visibility of the job dynamics across the entirety of the formal salaried private sector, rather than relying on samples as is the case with most papers using employment surveys (e.g. Azuara & Marinescu (2013)) which cover fewer municipalities and are not representative at the municipal level. One might argue that the reason these papers have failed to find an effect on informality is precisely measurement error and small samples. Third, in our view, the best published paper in the question of the labour market effects of SP uses this same data, and we have followed some of their methodological approaches. This is also the only paper we know that has found a negative effect of SP on formal jobs. Using IMSS data allows us to test how robust this result is. The main disadvantage of using IMSS data is that we can only measure effects on private-sector formal jobs; not on public-sector jobs, nor on informal jobs.

2.3 Some labour market statistics

A cursory look at employment trends may provide a clue on whether large changes in the labour market were happening as SP was being rolled out. Figure 3 shows the unemployment rate, the labour force participation rate, and fraction of workers with IMSS from 2005, when ENOE started being collected, to 2015. One can see that these three statistics are steady across these

Figure 3. Unemployment rate, labour force participation rate and fraction of workers without IMSS
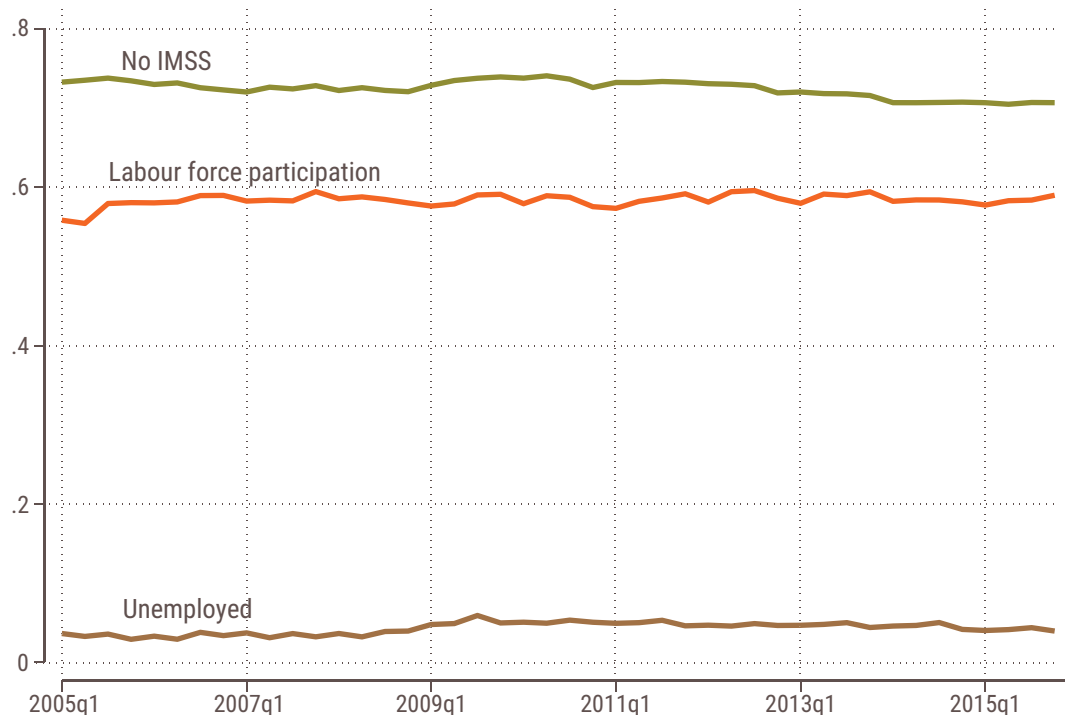
This figure uses Mexico's employment survey ENOE from its inception in 2005 to 2015 to plot three lines: (a) the unemployment rate defined as the proportion of people in the economically active population who do not have a job (either formal or informal); (b) labour force participation rate defined as the proportion of people older than 15 who are either employed, unemployed or looking for a job; and (c) and the fraction of those employed – either in the public or private sector and either in the formal or informal sector – who report not being registered with IMSS. ENOE covers 32 states encompassing 1,584 municipalities between 2005 and 2015.

years, even though SP coverage was growing strongly in this period.[15] One can also see that more than 70 per cent of workers surveyed report not having IMSS coverage.

3. Brief Review of the Literature

The question that frames the larger debate we are interested in, is whether social protection policies like health insurance or health care, income transfer programmes, and others, generate inefficiencies and in particular inflate the size of the informal sector at the expense of the more productive formal one. This is too large a question to be tackled in one paper. It is also an ill-posed question as the details of the policy and its context should matter and have to be specified. We therefore focus on a narrower but important question to do with SP. The question this report is concerned with is whether SP decreases the number of formal sector jobs in the non-governmental sector. In the introduction we discussed why studying SP is important for the larger debate. This brief literature review aims only to discuss some of the most prominent papers

covering this question, and the related question of whether SP increases the number of informal jobs.

With the exception of Bosch & Campos-Vazquez (2014), every paper we reviewed uses surveys to study the effect of SP. As we mentioned earlier, this is problematic since, other than the population census, Mexico does not have a survey that is representative at the municipality level for all its municipalities, and SP was implemented at the municipality level for all of them. Moreover, surveys are self-reported by workers and they may not know whether or not they are enrolled in IMSS as the employer does the registration.

Using survey data. One of the first papers on this issue is by Barros (2009). The paper uses several rounds of Mexico's income-expenditure survey together with the rollout of SP to measure effects on labour market outcomes. The paper finds no effect on labour force participation, hours worked, or relative wages of members of households covered by IMSS versus those not covered. The latter should be more affected by SP, but the paper finds they are not. The author writes, "I find

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[15] From 2005 to 2015, SP incorporated close to 4 million extra beneficiaries.

that SP did not induce a shift of workers into the informal sector". He conjectures that this is due to lower quality of care not being attractive to those with IMSS-provided medical care. Campos-Vazquez & Knox (2013) use employment surveys (which cover 33 cities) and find "little evidence of any correlation between SP and the decision of workers to be employed in the formal or informal sector". Azuara & Marinescu (2013) use employment surveys and "estimate that SP had no effect on informality in the overall population". Partitioning into sub-samples, they find an effect of 0.8 percentage point increase in informality for those with less than nine years of education.[16] Aterido et al. (2011) also use the employment survey. Using its short panel structure, they find that SP decreases the probability that a household is covered by IMSS by 0.3 percentage points on a base of 43 per cent. All these papers show a zero or a negligible effect of SP on informality.

Using administrative data. The only paper we know of that uses administrative data is Bosch & Campos-Vazquez (2014). The paper uses data from IMSS at the municipality level to compare the number of jobs and the number of formal employer[17] in IMSS, across municipalities that had not yet implemented SP versus those that had already implemented it. They don't find any effect of SP on the number of formal jobs on average. But for firms with one to five employees, they find a decrease of 2 per cent of formal jobs one year after implementation and in 4-5 per cent four years after. They also find that one year after implementation the number of formal *employers* decreased by 1.4 per cent and by 4.4 per cent one and four years after, respectively. They estimate that this translates into a cumulative 2000-2011 loss of 36,000 employers and 171,000 employees from those who would have formally registered with IMSS. On this we have three comments. This seems a small number when compared, for instance, to 14 million workers registered with IMSS in 2010. The effect comes entirely from firms with fewer than five workers, which several authors have documented are the least productive. These firms may have similar productivity as firms in the informal sector, implying little change in aggregate productivity. But more importantly, we find that these results are not robust. They disappear once the analysis includes *any* of the following: (1) all municipalities for which one can get data, (2) econometric methods that

are robust to the effect of SP being different in different municipalities, (3) following workers individually and controlling for their time invariant characteristics, (4) other methodologies to estimate causal effects like instrumental variables, which rely on different identification assumptions.

All in all, we conclude that there is not a single robust result in the literature showing that SP reduced formal jobs. If there are distortions from this social protection policy, they are not detectable on this margin.

4. Data

Before testing the three hypotheses we laid out, we describe the sources of data we use. Some of this data had not been put together before this report.

4.1. IMSS data

Our main data source is IMSS. The law mandates that all private-sector employees must be registered at IMSS and pay the corresponding contributions to IMSS. This entitles the workers and their families to a package of social security benefits including health care, life and workers compensation insurance for on-the-job injuries, old age and disability pensions and child care. The following are two sets of data provided by IMSS.

Municipality level data. We were able to obtain data at the municipality level by quarter on the number of formal workers registered in IMSS, and the number of firms registering formal workers, from 2000 to 2015. We observe the number of permanent workers and the number of temporary workers by gender and rural/urban areas. We also observe the average salary of those workers in the municipality. Additionally, we use the data published by Bosch & Campos-Vazquez (2014) in the AEA Policy webpage and complement it with our own. IMSS geographical affiliation and revenue collection structure does not coincide exactly with all municipalities, but most do. This means that we have data for close to 1,700 municipalities.[18] We will use this data to test hypothesis H1. This is the best data Mexico has to offer on formal private-sector jobs. We were also able to obtain average wages for jobs registered with the IMSS. This allows us to test hypothesis H3.

Individual level data. We had access at IMSS premises to worker-level data. The data set

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[16] Partitioning in sub-samples without taking into account that they are estimating multiple hypothesis leads to the wrong standard errors, however.

[17] Firms registering at least one worker in IMSS.

[18] Mexico has 2,454 municipalities, but 762 of them report zero workers to IMSS.

preserved the anonymity of both employers and workers. Our sampling is as follows. We draw a simple random sample of 10 per cent of all workers enrolled in IMSS in January 2000. We then follow them across quarters from January 2000 to December 2019. This amounts to 52 million observations in total and allows us to inquire if the same individual left formal employment as a function of the introduction of SP. This data allows us to test hypothesis H2. It also allows us to test if the effects of SP differ by salary, type of worker, and labour histories.

4.2. Data from Seguro Popular

We have data for the number of enrollees in SP by quarter from 2000 to 2009. This data was published by Bosch & Campos-Vazquez (2014) at OpenIPCSR. It contains the number of beneficiaries enrolled in SP by municipality by quarter-year. We were able to obtain similar data but at the municipality-*year* level directly from government publication at the open government data repository for the full period when SP was active, from 2004 to 2019. We checked that these two datasets were consistent. The data allow us to define when a municipality implemented SP and thus track the roll-out of SP. We define a municipality as implementing SP when at least 10 people are enrolled in the municipality.[19] This data allows us to construct our main explanatory variable, which is whether and when a municipality implemented SP. It allows us to compare job outcomes for municipalities with and without SP at a given point in time.

4.3. Data on clinics and hospitals

We were able to obtain data indicating where Mexico's hospitals and clinics are located, using GPS coordinates. The data includes the number of health facilities, the number of consulting rooms, and hospital beds. It also identifies the institution in charge of such health clinics. There are 32,393 primary care clinics, 5,081 hospitals, and 187 other health facilities (e.g. ambulatory specialty care or diagnostic and imaging health units). Figure 12 in the Appendix maps the stock of medical infrastructure in Mexico. This data allows us to implement a new causal estimation method ("instrumental variables") that uses the presence of clinics to generate earlier adoption of SP. Having different estimation methods is important to assess robustness of results since these methods rely on different assumptions.

4.4. Data from the National Statistics and Geography Institute (INEGI)

Employment surveys. Mexico's Encuesta Nacional de Ocupaci´on y Empleo (ENOE) has been running since 2005. ENOE is a rotating panel, following the same person for five quarters, maintaining an overlap of 80 per cent and rotating 20 per cent of the sample per quarter. It surveys on average about 312,000 people each quarter. We use data from the first quarter of 2005 to the last quarter of 2015. This implies we have 1.4 million individuals but just over 13 million observations. We define a person as working if they answered affirmatively to the following question: *Did you work for at least one hour last week?* And we consider it formal if they are reported to be affiliated to IMSS. We use ENOE to study whether workers with IMSS have different observable characteristics, transition between the formal and the informal sector for the same person, and to estimate by how much their salary decreases when switching to the informal sector.

Population censuses. We use data from the population census in 2000, 2010 and 2020, together with intercensal data in 2005 and 2015. We use this data to extrapolate population on each quarter with a constant growth rate for each of the five-years gaps. We add population as a control in all regression specifications.

4.5. Other data

Lights from space. It is possible that municipalities adopting SP before others do have intrinsically different economic conditions. One would like to isolate every other cause of employment changes like macroeconomic trends at the municipality level, and just focus on the effect of SP on the labour market. That is, we want to avoid attributing to SP changes in formal employment that would be associated with changes in economic conditions unrelated to SP. To do this we needed a measure of economic activity that exists at the municipality level and that is measured at least at the yearly frequency. Luminosity has these two properties. It has been used as a proxy for economic activity in the economics literature (e.g. Michalopoulos & Papaioannou (2013)), and it exists at yearly frequencies for Mexican municipalities. By luminosity we mean night-time light data systematically sensed from satellites.[20] It turns out after doing the analysis that controlling for luminosity does not make much difference in the estimated effects of SP.

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[19] Results are similar when we consider a threshold of 1 up to 100 beneficiaries when defining implementation of SP.

[20] Luminosity rasters are from Li et al. (2020) Harmonized Global Nighttime Light Dataset 1992-2018, and we use data from the repository at https://github.com/emagar/luminosity. This repository distributes night-time luminosity data for Mexico, aggregated at the municipal level.

5. Empirical Methods

The challenge of measuring causal effects of an intervention is to find ways to estimate what would have happened without it. In the absence of a randomized experiment, there are several quasi-experimental ways to try to estimate such a counterfactual. We discuss below two methodologies we use.

5.1. Difference in Differences

Difference in Differences (DiD) is a method that can be applied in the context of a differentiated geographical roll-out of a programme. This is the method that Bosch & Campos-Vazquez (2014) used. The methodology of difference-in-differences compares outcomes of municipalities that have implemented the programme against those that have not yet implemented it. To identify a causal effect, the context needs to satisfy a "parallel trends" assumption, which requires that early implementing municipalities would have had the same outcomes as late implementing municipalities if SP had not been implemented.[21]

Figure 2 showed that the timing of adoption differed widely across municipalities. Figure 4 shows the number of workers registered at IMSS (right-hand vertical axis), and the individual affiliation to the SP programme (left-hand vertical axis).[22] As we have discussed above, it is a very large programme in terms of the number of people covered. At a glance, there does not seem to be much substitution across these two categories. The next section explores this more rigorously. The downturn of IMSS-registered jobs in 2008-2009 is associated with the global financial crisis.

Baseline model specification. We implement a DiD strategy by estimating a simple two-way fixed effect regression as follows:

$$Y_{mt} = \sum_{k=-4}^{k=4} \beta k \mathbb{1}(\tau_{mt} = k) + \delta Pop_{mt} + \lambda_m + \lambda_t \quad \textbf{(1)}$$
$$+ q(t) \times s + \varepsilon_{mt}$$

Y_{mt} denotes the log number of jobs registered at IMSS (or else the log number of employers registered) in municipality m at time t. Time may represent a quarter-year or just a year.

Figure 4. Roll-out of Seguro Popular (replication)
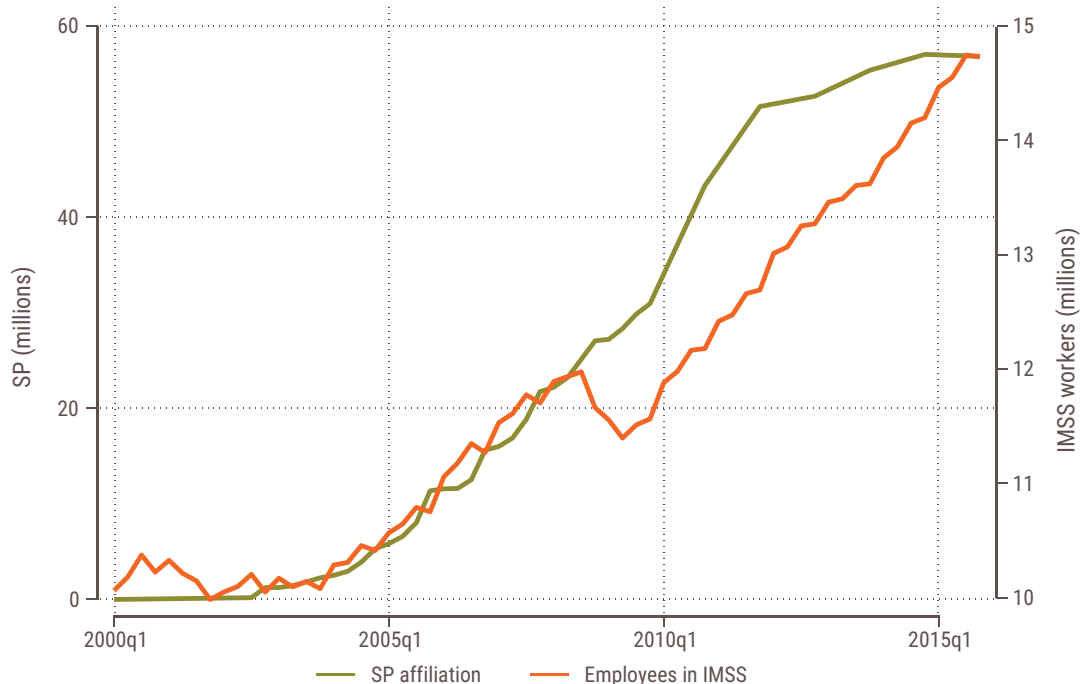
SP refers to the number of beneficiaries enrolled in SP. Employees refers to IMSS-registered employees. The numbers for SP and IMSS are individual level. SP registers all family members separately, while IMSS does not, which may explain the difference with Figure 1. Information is quarterly.

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[21] This assumption is inherently untestable as it involves a counterfactual, but having both sets of municipalities have similar employment trends before SP is a common check.

[22] Recall that IMSS registration entitles the entire family to medical care at IMSS, so to compare across the two lines one would have to multiply the IMSS numbers by family size. The numbers are similar if we assume IMSS beneficiaries have a family size of about 3.7.

τ_m, indicates the time before/after SP adoption, in particular $\tau_{mt} = 0$ indicates the time of SP adoption. We will look at results four years prior and four years after implementation, therefore we have eight estimated coefficients: three for the pre-SP period $(\hat{\beta}_{-4}, \hat{\beta}_{-3}, \hat{\beta}_{-2})$[23]. This allows us to test the assumption that formal job trends were parallel in earlier vs later implementing municipalities *before* implementation.[24] There is one coefficient for the year of implementation $\hat{\beta}_0$, and four coefficients for the years after implementation $(\hat{\beta}_1, \hat{\beta}_2, \hat{\beta}_3, \hat{\beta}_4)$. Thus, we are able to measure effects four years after. We control for the population of the municipality on logs P_{mt}, extrapolating linearly from the population census. We also control for a third-degree polynomial of time $q(t)$ interacted with state s to allow for state-individual trends, and also by X_{mt} which are municipality characteristics. Finally, we also include municipality (λ_m) and time fixed effects (λ_t). Two more details to note. Standard errors are clustered at the municipality level to take into account potential serial correlation in employment. We use weights that are proportional to the population in the municipality at year 2000.

The regression specification in equation 1 is exactly the one used by Bosch & Campos-Vazquez (2014). This allows us to assess the robustness of their results to three changes:

- Include time varying proxies for economic activity: in our case by including a third-degree polynomial of luminosity at the municipality-quarter level.

- Include more municipalities in the analysis. We were able to include another 18 municipalities that Bosch & Campos-Vazquez (2014) dropped for lack of information on the number of enrollees in SP. We were able to obtain such data. Furthermore, we also use new data on employment that we got from IMSS, which enables us to include another 300 municipalities. At all times we use a balanced panel of municipalities where we observe employment from 2000 to 2011, which spans 12 quarters before and 18 quarters after programme implementation.

- Use a more flexible specification that allows earlier and later adopting municipalities to be on different time trends by interacting quarter

of implementation interactions with time, as recommended by Wooldridge (2021).

We use the methodology of de Chaisemartin & D'Haultfoeuille (2022) which allows the dynamic effects of SP to be different in different municipalities. This may be the state of the art in terms of methods to implement a difference in differences design using the municipality rollout of SP.

Flexible model specification. What we call the "flexible specification" follows the advice of Wooldridge (2021) and makes two additions to equation 1 above, represented in equation 2 below:

$$Y_{mt} = \sum_{k=-4}^{k=4} \beta k \mathbb{1}(\tau_{mt} = k) + \delta Pop_{mt} + \lambda_m + \lambda_t \text{ (2)}$$
$$+ q(t) \times s + q(lum)_{m,t} + \gamma \mathbb{1}(QI)_m \times t + \varepsilon_{mt}$$

The additions are reflected in two terms: $q(lum)_m$ a third-degree polynomial of luminosity as proxy for economic activity, $\mathbb{1}(QI)_m$ is an indicator variable for the quarter of implementation of municipality m which we interact with a linear time trend t. These later interactions allow municipalities that implemented earlier to have a different evolution of employment both before and after implementation than those that implemented later. There is no harm in including them: if municipalities turn out to have the same evolution of employment, then the data will manifest that in that the γ's will be statistically zero. Rejecting that they are zero, however, means that the model in equation 1 is mis-specified, since it would be imposing a false assumption on the data, leading to biases in the estimates. We again cluster errors at the municipality level and use weights that are proportional to the population in the municipality at year 2000.

5.2. Individual level data and worker fixed effects

We were able to obtain panel data on a random sample of 10 per cent of the workers who were at IMSS in January 2000. This enables us to follow particular workers and ask, for each of them, if they are more likely to terminate their IMSS-registered job when SP is implemented in their municipality. The ability to do this is important for several reasons. First, it enables us to study

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[23] We omit the coefficient $\hat{\beta}_{-1}$, so the rest of the coefficients are measured relative to the year before implementation.

[24] Finding an "effect" of the programme before programme implementation —that is finding that $(\hat{\beta}_{-4}, \hat{\beta}_{-3}, \hat{\beta}_{-2})$ are different from zero — is a sign of mis-specification in the regression equation and would cast serious doubts on the appropriateness of the DiD method.

switching directly, and isolate gross flows out the formal sector from inflows that may mask the former at the municipality level. Second, it allows us to control for time invariant worker level characteristics, in the form of a worker fixed effect α_i. The outcome variable is now an indicator for the worker i living in municipality m leaving IMSS at period t, $\mathbb{1}(\text{worker leaves IMSS})_{imt}$. We cluster errors at the municipality level.

$$\mathbb{1}(\text{worker leaves IMSS})_{imt} = \alpha_i + \sum_{k=-4}^{k=4} \beta k \mathbb{1}(\tau_{mt} = k)$$
$$+ \delta Pop_{mt} + \lambda_m + \lambda_t + q(t) \times s + q(lum)_{m,t}$$
$$+ \gamma \mathbb{1}(QI)_m \times t + \varepsilon_{imt} \qquad \textbf{(3)}$$

5.3. Difference-in-differences of dynamic effects

Our preferred difference-in-differences method is that proposed by de Chaisemartin & D'Haultfoeuille (2022). We refer the readers to the original paper for detail. As they explain, this estimator is valid even if the treatment effect is heterogeneous across municipalities and across time. This estimation method resolves the problem of negative weights and bias generated by these. We implement this method using their STATA command *did_mutiplegt*.

5.4. Instrumental variables

The above methods use the staggered roll-out of SP to generate counterfactual outcomes; that is, outcomes that the implementing municipalities would have had if SP had not been implemented. This section implements a different empirical strategy; that of Instrumental Variables (IV). This method relies on a different identification assumption. It requires the existence of a variable (called an instrument) that can predict the implementation of the programme at the municipality level (first stage), and that is by itself directly unrelated to the outcome we care about, which in this case is formal employment (the exclusion restriction).

We have a candidate instrumental variable in our context. Because SP needed medical infrastructure to operate and not all municipalities had this, its implementation began in municipalities with enough clinics and hospitals. Bosch & Campos-Vazquez (2014) cite Mexico's Ministry of Health as stating that the geographies were initially chosen for "the capacity of offering the services, large concentration of urban and semi-urban population, and the existence of previous benefit programs from the government". Based on this, we conjectured that the number of clinics and hospitals may predict which municipality implemented first and which implemented later, and therefore satisfy the statistical requirement of the first stage. Indeed, we show later that this is the case. For the instrument to be valid, however, the number of hospitals and clinics does not directly affect the number of formal jobs in a municipality, once we control for population and other covariates. This is a strong assumption; it could be the case that municipalities with more hospitals per capita are richer or less healthy and that the number of clinics proxy for employment opportunities. [25]

First stage. We first test if the medical infrastructure does indeed predict SP adoption at the municipality level. To do that, we start with a set of potential instrumental variables given by the following list of variables: log(Total # clinics), log(# IMSS clinics), log(# 2nd level clinics), log(# 3rd level clinics), log(Total # of rooms), log(Total # beds), Incumbency of political party. Using the above instruments, Z_{mt}, we estimate the following first stage equation that predicts the number of beneficiaries of SP at the municipality level m for a particular quarter t:

$$\log(\#\text{SP beneficiaries} + 1)_{mt} = \alpha_o$$
$$+ \beta' Z_{mt} + V_{mt} \qquad \textbf{(4)}$$

We then employ a Lasso method that selects which are the best predictors of implementation within the set of potential instruments. Lasso selected all but the log(Total # of rooms) or log(Total # of beds) for certain quarters, which we collectively call Z_m and are measured in logs.

We estimate equation 4 separately for each quarter.[26] We test if the coefficients in β' are statistically different from zero using an F-test. Figure 18 plots one estimated $\hat{Z}_m$ coefficient on the left Y- axis; they can be interpreted as elasticity. For instance, between 2006 and 2007, municipalities that had 100 per cent more clinics than others were between 90 and 150 per cent more likely to have implemented SP: a large correlation. The correlation is also statistically significant, with F-stats above 100 (right Y-axis), suggesting strong first stage instruments.

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[25] We provide some suggestive evidence of its plausibility. Figure 17 in the Appendix splits municipalities by terciles of the number of clinics in 2000 and plots the evolution of formal employment. It shows that municipalities with different medical infrastructure had similarly evolving employment trends before SP. Regression Table 2 in the Appendix tests this statistically.

[26] The Lasso stage is also estimated separately for each quarter.

Second stage. To estimate the effect of SP using our instrumental variable strategy, we instrument the number of beneficiaries log(#SP beneficiaries + 1)$_{mt}$ in equation 5 below using the first stage equation 4.

$$Y_{mt} = \beta \log(\#\text{SP beneficiaries} + 1)_{mt} + \delta Pop_{mt} + \lambda_m + \lambda_t + q(t) \times s) + q(lum)_{mt} + \varepsilon_{mt} \quad \textbf{(5)}$$

This equation is similar to the one we have been estimating, with the difference being that the independent variable that interests us is the number of SP beneficiaries in municipality m at time t. The coefficient of interest is β, which can be interpreted as an elasticity: the percentage change in the number of jobs registered at IMSS over the percentage change in the number of SP beneficiaries. We would have full crowding out if $\beta = -1$. We instrument the number of SP beneficiaries in municipality m at time t with the predicted values from equation 4, using two-stage least squares. We estimate equation 5 separately for each quarter and plot the elasticity through time in the main results on Figure 19 in the Appendix. Moreover, we also estimate equation 5 exploiting our panel data structure using a panel IV regression and show results in Table 1.

6. Main Results: Effects of Seguro Popular

This section tests three of the hypotheses implied by the DTI hypothesis. Hypothesis 1 is that when SP is introduced in a municipality, workers switch from the formal sector (IMSS for the case of private-sector formal workers) to the informal. Implying that IMSS has fewer formal-sector jobs registered at that municipality compared to municipalities that have (not yet) implemented. We cannot reject that there were no IMSS job losses. Hypothesis 2 posits that a given individual worker is more likely to abandon IMSS when SP is introduced in his or her municipality. We cannot reject that there is no effect of SP on the probability of abandoning an IMSS-registered job. Hypothesis 3 focuses on the effect on wages. The DTI implies that the supply of workers to the formal sector decreases, as workers now offer their services in the informal sector. Without a change in the demand for workers in the formal sector, we would expect an increase in IMSS registered wages. We cannot reject that wages did not change as SP was introduced. We now present the results.

6.1. Hypothesis 1: Effects on formal jobs

Preferred specification. Figure 5 presents our main result, estimated in the *exact sample and data* that Bosch & Campos-Vazquez (2014) use, but using the more robust econometric method of de Chaisemartin & D'Haultfoeuille (2022). Figure 5 presents the result in graphical form, where the horizontal axis indicates the time periods in quarters. Zero indicates the quarter when SP was implemented in a municipality, negative numbers represent quarters before implantation and positive values quarters after implementation. The vertical axis measures the effect of SP on formal jobs in percentage terms. The shaded area represents 95 per cent confidence intervals. If the shaded area crosses zero in the vertical axis, it means that we cannot reject the hypothesis of zero effect.

One would expect to find zero effects before the programme is implemented. This is indeed what we find, lending credibility to the method. Figure 5 – panel (a) shows the event for the number of firms registering workers at IMSS. Panel (c) shows the effect on the number of workers registered at IMSS. Finally, Figure 5 panel (b) focuses on workers in firms with only one employee, which we refer to as a self-employed worker. We focus on this sub-sample. It is where Bosch & Campos-Vazquez (2014) found statistical effects in terms of employment. The main result is that we cannot reject the null hypothesis of zero effects. That is there is no evidence of SP decreasing either the number of firms registering workers, or the number of registered workers themselves.

That our results are different from those of Bosch & Campos-Vazquez (2014) using their exact same data means that their estimate is likely biased and a result of the model not being robust to treatment effect heterogeneity. The new econometric literature summarized in de Chaisemartin & D'Haultfœuille (2022) shows that if early adopters benefit more or less from SP than late adopters, or are subject to different dynamics in treatment effects, then the specification used in Bosch & Campos-Vazquez (2014) gives biased estimates for the average treatment effect; indeed even the sign of the estimated effect could be wrong if the method implicitly uses negative weights.[27] We find diagnostic indications that their methodology uses invalid negative weights. See Figure 12 in the Appendix.

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[27] Negative weights bias the estimate and could imply that the estimate of the effect not only errs in its magnitude, but in its sign also. Negative weights only arise when average treatment effects vary over time. For an explanation of why negative weights arise see Goodman-Bacon (2021).

Figure 5. Event studies - employment

(a) Employers at IMSS

(b) Self-employed workers at IMSS

(c) Total IMSS workers

This figure plots the event studies for the introduction of SP and its impact on employment using our preferred specification, the method of de Chaisemartin & D'Haultfoeuille (2022). Errors are clustered at municipality level.

Replication of Bosch & Campos-Vazquez, 2014. Given that Bosch & Campos-Vazquez (2014) is the only paper that finds negative effects of SP on formal jobs, we replicated their results using their data and their *exact method* and found that it does replicate. Results of this replication are shown in Figures 13 in the Appendix. We then assess the robustness of this result to three changes.

Controlling for time-varying economic activity. The first robustness check involves controlling for economic conditions at the municipality year level. Because Mexico's statistical agency does not produce GDP estimates at the municipality level, we use lights from space as a proxy for economic activity at the municipality level. The results survive almost unchanged (not shown).

More municipalities. A second robustness check is adding more municipalities to the sample. Bosch & Campos-Vazquez (2014) had to drop municipalities because information on the number of SP beneficiaries was missing for some or all periods considered in their analysis. We could

recover 18 of them by accessing the SP census.[28] We could recover another 282 municipalities by complementing their data with new data at the municipality level from IMSS. We ended up with 1,692 municipalities compared to their 1,392. Figure 14 in the Appendix implements their exact estimation method to this larger sample of municipalities, so it evaluates the robustness of their sample using their own method. We find that the estimated negative effect for employers disappears. Moreover, their methodology no longer delivers parallel trends before SP implementation for employees, and therefore does not afford a causal interpretation for employment results.

Flexible specification. The final robustness check involves estimating the more flexible regression specification defined in equation 2 on their exact sample of 1,392 municipalities. Figure 15 in the Appendix shows that their result is not robust to the more flexible specification. The negative effects on IMSS-registered employers and on those single-employee firms disappears. Total

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[28] Accessed from: https://datos.gob.mx/busca/dataset/beneficiarios-de-proteccion-social-en-salud-de-seguro-popular.

IMSS-registered employees show a *positive* trend, but it cannot be interpreted as causal because the trend is present before SP. The lack of robustness to including different time trends for municipalities according to when they implemented SP implies not a failure of their economic theory but of their statistical model. Forcing municipalities to have the same evolution of employment imposes an assumption. We test and reject the assumption of homogeneous trends for earlier versus later adopters by testing that the coefficients are zero in equation 2. We reject this null hypothesis for the case of employers and of workers with p-values of 0.0017 and 0.047, respectively. Imposing an assumption on the data that is false results in biased estimates.[29]

The conclusion is then that the results of Bosch & Campos-Vazquez (2014) are not robust. SP does not have a negative effect on average on the number of workers registered at IMSS (or on the number of employers registering them). Below we explore if this conclusion holds not only on average, but also for particular sub-populations, and whether the method of instrumental variables delivers a different result.

6.1.1. Heterogeneity effect of SP

This sub-section asks if the effect of SP significantly differs by gender, modality of IMSS employment,[30] workers in rural and urban areas, across the wage distribution of formal employees, and by marital status. For ease of interpretation, we split the sample in categories – in cases of continuous variables defined by the median – and estimate our preferred specification separately for each category. In each case, we indicate with

Figure 6. Heterogeneous effects
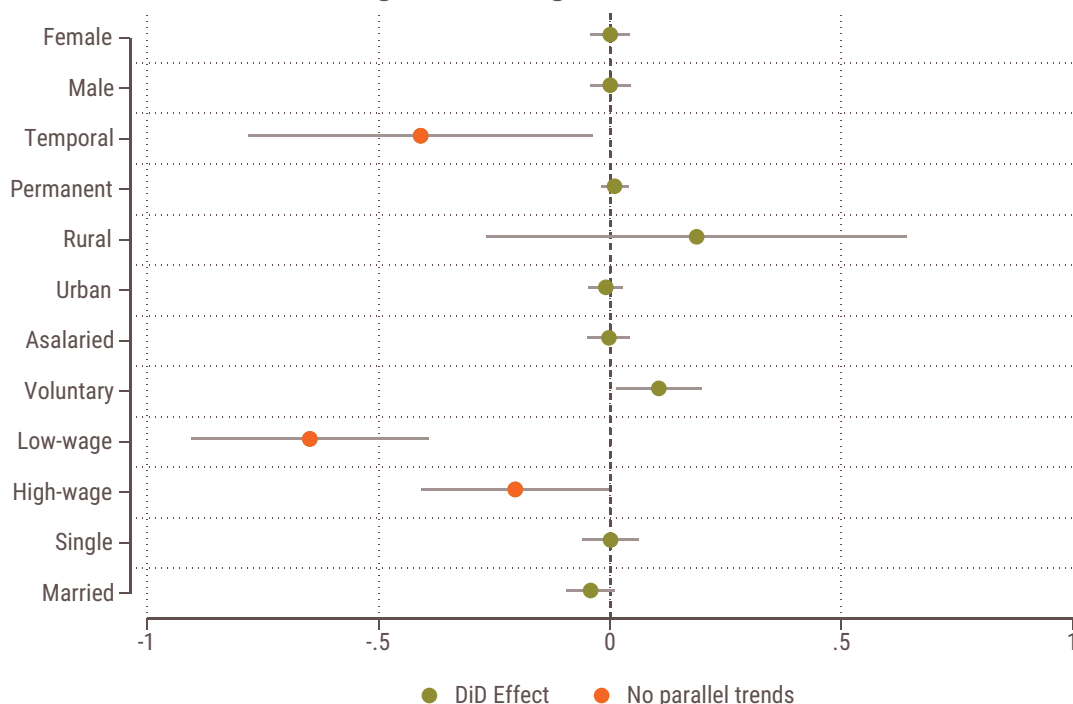
We plot the average dynamic effect for different categories. The command computes a weighted average of the DIDt estimators, giving to each estimator a weight proportional to the number of switchers DIDt applies to. Second, the command computes a weighted average of estimators similar to the DIDt, except that the outcome variable is replaced by the treatment. This weighted average estimates the effect of first switches on the treatments that units receive after their first switch. Finally, the command computes the ratio of these two estimators. This ratio estimates the "intention-to-treat" effect of first switches on the outcome, and scales it by the "first-stage" effect of first switches on the treatments received thereafter. Accordingly, it estimates some average of the change in outcome created by a one-unit change in treatment. Errors are clustered at municipality level.

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[29] Why do municipalities have different evolutions of employment? This is a question outside the scope of the paper, but early implementing municipalities are larger, as Bosch & Campos-Vazquez (2014) show, and may be different in other respects, including industrial composition. This heterogeneity across municipalities makes it critical to allow for flexible estimation approaches (Wooldridge, 2021).

[30] The data has 3 categories of IMSS affiliation: (1) permanent workers – those who have a labour relationship for indeterminate time, (2) temporary workers – those who have a labour relationship for a pre-specified task or time, (3) voluntary affiliates – those who self-enrol in IMSS, presumably to have access to its services.

an orange dot if the estimates do not have parallel trends before SP implementation. This signals that we cannot trust these estimates as average causal effects.

Results. Figure 6 focuses on employees in all firms for 12 categories plotting the average effect through the 16th quarter post-SP period. It finds null effects for all categories that have parallel trends, except for voluntary workers, but the sign is positive. The negative coefficients for temporary and low-wage workers cannot be trusted because they violate the parallel trends assumption (i.e. the number of formal jobs was changing relative to the control group even before SP was implemented, and therefore cannot be attributed to SP).

6.1.2. Results using the method of instrumental variables

Table 1 presents our instrumental variable estimates of the effect of SP on the number of formal employers, the self-employed and the number of employees registered at IMSS, using the largest possible sample of municipalities. On average, we find no effect on the number of IMSS-registered workers. We find that an elasticity of the number of SP enrollees on employers of -0.007, and on formal jobs of -0.003. This means that if the number of enrollees increases by 100 per cent, then the number of jobs in IMSS decreases by 0.7 per cent and 0.3 per cent, respectively. This is tiny. We find that all of the effect comes from firms with 1 employee (self-employment). Figure 19 in the Appendix shows the IV results when estimating equation 5 separately for each quarter.

Summary of the test of Hypothesis 1. Consistent with the overwhelming majority of the literature, we estimate that SP had no effect (or the effect is very close to zero) on the number of formal jobs, in our case measured as jobs registered at IMSS.

6.2. Hypothesis 2: Effects on switching at the individual level

The second hypothesis, **H2**, tests whether workers already registered with IMSS in January 2000 are more likely to leave if SP is implemented in their municipality by estimating equation 3 by Ordinary Least Squares (OLS) regression, while including an individual-level fixed effect (FE). For brevity, Figure 7 plots the average post-treatment effects instead of the complete time-profile of effects. We present results for the entire sample for close to 50 million individual-time observations ("All"), as well as further split the sample by (a) a measure of labour market attachment,[31] (b) by whether the person had wages above or below the median in 2000 ("high wage", "low wage"), and (c) whether the worker worked on a single-worker firm (self-employed). Figure 7 finds tiny (almost null) effects on the probability of leaving an IMSS-registered job, with a *negative* 0.2 percentage point increase.[32] That is, they are less likely to leave IMSS, but the magnitude is negligible.

**Table 1: Effect of SP on formal jobs using IV strategy**

	Employment		
	Employers	**Self-employed**	**Employees**
	(1)	(2)	(3)
Elasticity of SP	-0.0071* (0.0041)	-0.014* (0.0081)	-0.0031 (0.0067)
Observations	30331	30331	30331
Number of municipalities	1626	1626	1626
DepVarMean	3.99	3.01	6.05
Municipality	✓	✓	✓
Economic activity controls	✓	✓	✓

This table presents instrumental variables panel regression estimates of the effect of SP on employers and employees registered at IMSS, as well as employers with one employee, which we call "self-employed".

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[31] Labour market attachment is measured as the percentage of time in a given period the person has not been enrolled at IMSS.

[32] Trends are only parallel for the self-employed, and there the effect is positive.

Figure 7. Effect on the worker level probability of abandoning IMSS

Legend: ● Probability of exit IMSS   ● No parallel trends
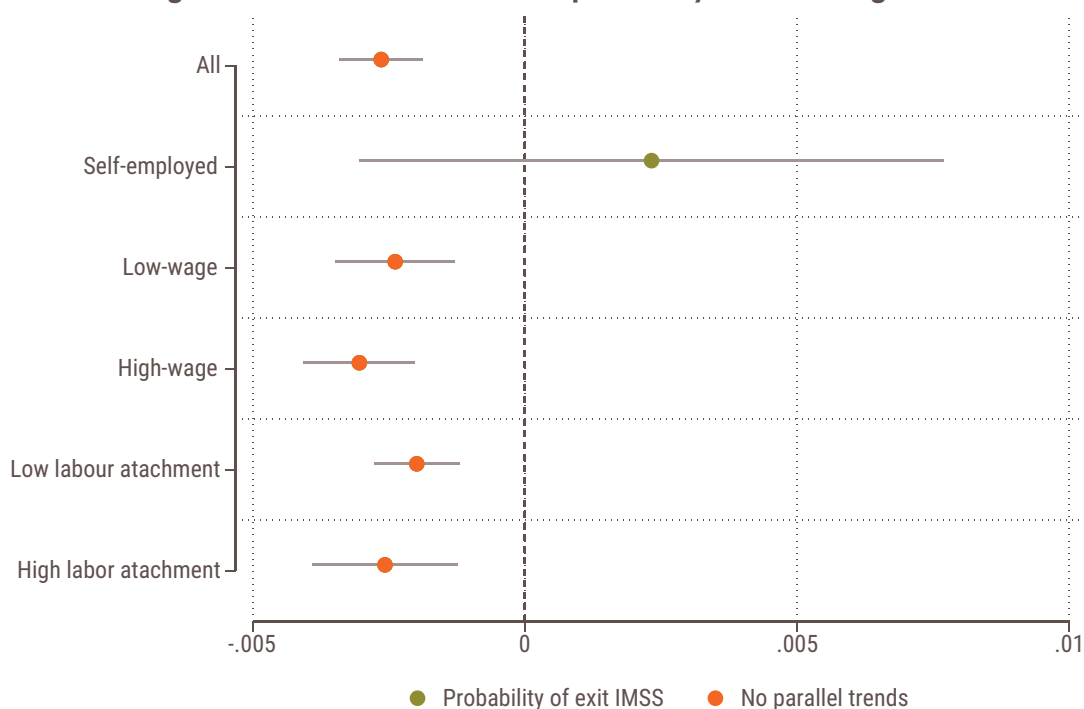
Effects of SP on probability of leaving IMSS estimating equation 3 using individual level data. We estimate equation 3 in all the sample ("All") and also for different sub-samples. Standard errors are clustered at municipality level. Dots in orange fail the parallel trends tests.

6.3. Hypothesis 3: Effects on average wages

Given its significant size, if SP has a disruptive effect on the labour market it could have the potential to change the salaries in the formal labour market. This could occur, for instance, if the supply of labour shifts from the formal and towards the informal market, given the benefits offered by SP only in the informal market. All else constant this would decrease equilibrium wages in the informal market and increase them in the formal market as formal employers try to compensate workers for not leaving. Figure 8 estimates our preferred specification and plots the event study graph. As we can see, if anything we find a *decrease* in wages, not an increase. But it is not statistically different from zero.

Figure 8. Event studies - wages

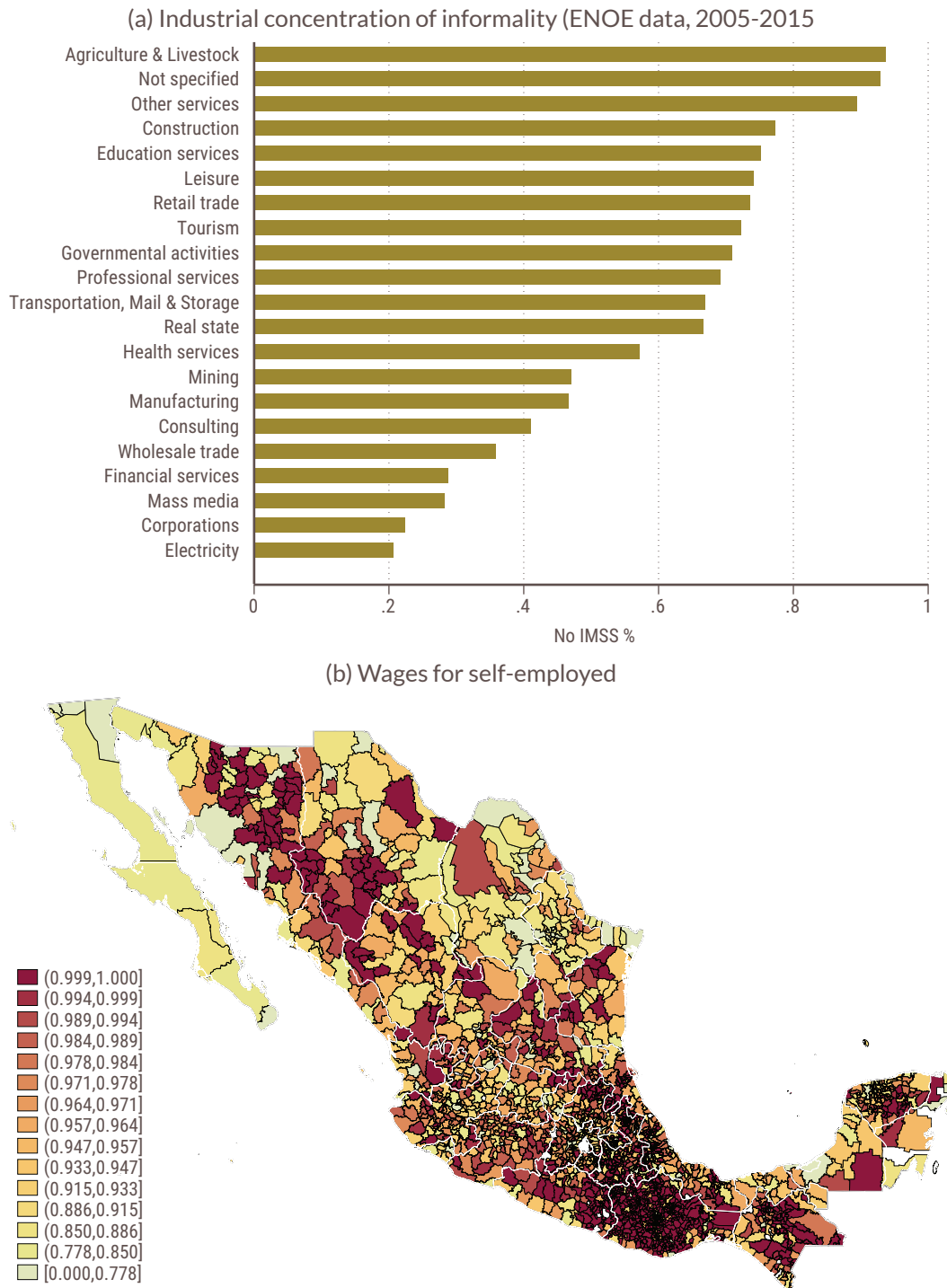(a) Wages for total employees   (b) Wages for self-employed
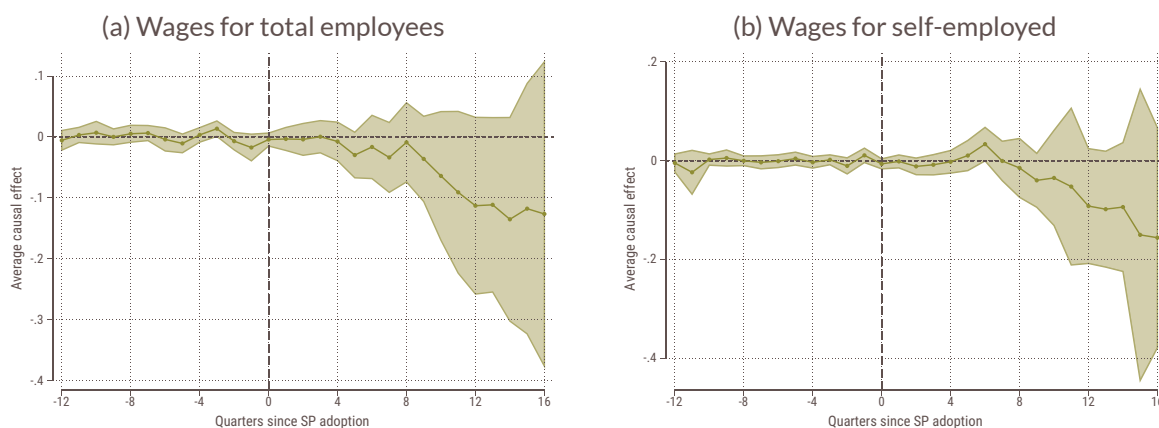
This figure plots the event studies for the introduction of SP and its impact on wages using our preferred specification, the method of de Chaisemartin & D'Haultfoeuille (2022). Errors are clustered at municipality level.

7. Discussion

Given these results, one natural question to ask is why is SP *not* causing a decrease in formal-sector jobs nor an increase in formal-sector salaries? The literature and this paper do not have a rigorous answer. This section puts forward some conjectures.

Is SP not valuable enough? One possible reason is that SP is not perceived as attractive enough to lure workers from the formal sector. SP does not cover all health treatments that medical care at IMSS covers; IMSS also has better infrastructure and anecdotally is perceived to provide higher quality medical care than SP. As of 2010, IMSS had about 20 per cent more public resources per

Figure 9. Industrial and geographical concentration of informality

(a) Industrial concentration of informality (ENOE data, 2005-2015

(b) Wages for self-employed

Legend:
- (0.999,1.000]
- (0.994,0.999]
- (0.989,0.994]
- (0.984,0.989]
- (0.978,0.984]
- (0.971,0.978]
- (0.964,0.971]
- (0.957,0.964]
- (0.947,0.957]
- (0.933,0.947]
- (0.915,0.933]
- (0.886,0.915]
- (0.850,0.886]
- (0.778,0.850]
- [0.000,0.778]

Panel (a) uses the ENOE survey (2005-2015) and plots the fraction of workers reporting that they are NOT affiliated to IMSS. Panel (b) is a map that uses data from IMSS in 2000 to show the fraction of workers not covered by IMSS.

beneficiary than SP. In terms of human resources and infrastructure, IMSS had about 30 per cent more nurses and 10 per cent more beds than SP.[33] If this is the case, then lower quality can be acting as a screening device, attracting only workers with no access to IMSS while probably not tempting for IMSS workers. In addition, IMSS offers a bundled benefit package that is mandatory, including old-age and disability pensions and child care. Opting out of IMSS implies losing out on all social protection coverages.

Different industries and different locations. One reason why IMSS workers may refuse to work in the informal sector is that they work in different industries and have acquired skills that are less useful in the informal sector. Informality is concentrated in certain industries (see Figure 9).[34] Agriculture and livestock have more than 90 per cent of workers not affiliated to IMSS, followed by construction and several services. Informality is also very concentrated geographically, meaning the large shift from formal to informal jobs would mean either migrating to other municipalities or deeply changing the economic structure of a given municipality, which may take time.

Evidence of mobility between formal/informal is likely overstated. One can try to measure flows into and out of formality. This raises an important problem: the only data that contain workers with and without IMSS for Mexico are the employment surveys. The surveys' limitation is twofold: it only follows workers for five quarters, and whether they have IMSS or not is self-reported and potentially subject to measurement error. Using the ENOE, we calculate that the share of workers who switch from having an IMSS-registered job to non-registered ones at some point in a 5-quarter span is 14.2 per cent. Our concern is that it is just misreporting on the IMSS variable on the part of workers. Very often workers do not know whether they do or don't have IMSS.[35] Even though some papers have argued, using this data, that flows are very prevalent across these two sectors, we believe these results are highly suspect because of measurement error. In fact, out of the workers who are in IMSS in quarter one, only 2.9 per cent work without IMSS in quarter two and *stay* there in quarters three, four and five. Moreover, 14.1 per cent of surveyed workers make two or more switches between IMSS and NO-IMSS jobs in a

span of five quarters. This seems too large to be believable.

Workers who report working informally and formally are different. We use the ENOE surveys to study if workers with IMSS versus those without IMSS differ in their observed characteristics. To do this we estimate the following regression.

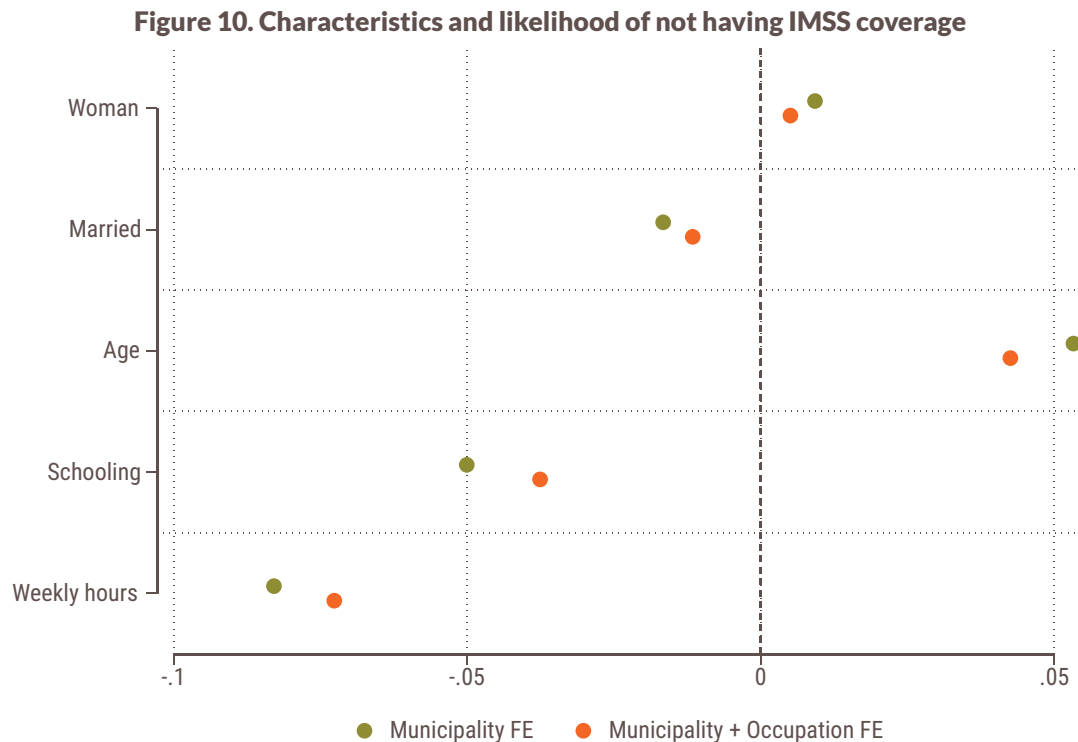$$\mathbb{1}(\text{no-IMSS})_{it} = \gamma_t + \delta_k + \alpha'_m \quad + \beta_j X_j + \epsilon_{it} \quad \textbf{(6)}$$

where i indexes individuals surveyed in ENOE, t indexed calendar quarter of the survey, k indexes industries/occupations, and m indexes municipalities, and X_j are characteristics, indexed by j. We run a separate regression for each characteristic. It could be a dummy for being a woman, years of schooling, age in years, hourly wage (in logs), weekly hours worked, a dummy for holding two or more jobs, or a dummy for being married. We are interested in the coefficient β_j. We are controlling for quarter dummies (γ_t) that absorb macroeconomic trends in IMSS affiliation; this reduces spurious temporal correlations between IMSS affiliation and the business cycle, for example. Some specifications also include municipality-fixed effects (α'_m) and industry-fixed effects (δ_k), which means that we are comparing characteristics of informal vs formal workers/jobs *within* the same municipality and industry.

Figure 10 plots the estimated $\{\hat{\beta}_j\}$ jointly for every characteristic. We statistically reject that coefficients are zero (confidence intervals are small enough to be subsumed in the dots). That is: informal and formal workers are different in their characteristics. This begins to cast doubt on models that assume that workers in both sectors are perfect substitutes. For instance, being a woman increases the likelihood of being informal by about 1.4 per cent. An extra year of age increases the likelihood of informality by 4 per cent. An increase of one standard deviation in schooling (6.2 years) is associated with about 3 per cent lower propensity of being informal. Analogously, workers who work one standard deviation more weekly hours (18.7 hours) are about 7 per cent less likely to be informal. It is likely that they differ even more in characteristics that would sort them into formal/informal occupations, like occupation-specific skills and

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[33] These comparisons are imperfect because IMSS also provides child care and other services, although the overwhelming majority of spending is on health care.

[34] We are forced to use the classification IMSS data collects.

[35] In fact, IMSS has created an app called "Reporte Personalizado de Cotizacion Individual" precisely to inform workers if they are registered in IMSS or not as their employers are responsible for doing this procedure and often do not.

Figure 10. Characteristics and likelihood of not having IMSS coverage

● Municipality FE   ● Municipality + Occupation FE

This graph plots the estimates of β_j using separate OLS estimated regression for equation 6. They measure correlation between not having IMSS and the displayed characteristic X_j.

experience, preference for flexibility in working hours, etc.

Salary changes. Another reason formal workers may not quit their IMSS job and work in the informal sector after SP is implemented is that they may command higher salaries in the formal sector. For those who do switch, we estimate the following regression equation:

$$
\begin{aligned}
\log(\text{wage}_{it}) =\theta_i &+ (\gamma_t \times \alpha'_m) + \delta_k \\
&+ \beta \mathbb{1}(\text{No-IMSS})_{it} + \epsilon_{it}
\end{aligned}
\tag{7}
$$

where i indexes individuals surveyed in ENOE, t indexed calendar quarter, m indexes municipalities, and k indexes industries. Importantly, we include individual fixed effects θ_i. This means we are comparing the same person in two different kinds of jobs, formal and informal. We are interested in the coefficient β, which measures the size of the difference of a given person's wage in the informal sector. We find that the salary earned by *the same worker* is lower by 8 per cent in jobs not registered with the IMSS (see Table OA-2 in the Appendix). Since we cannot know whether they were fired or instead chose the informal job, we unfortunately cannot tell whether 8 per cent is a compensating differential. This number is just meant to illustrate that informal jobs seem to carry lower salaries. If the "No-IMSS" variable was measured with classical measurement error, the true difference

may be even larger. This suggests that for SP to attract workers to switch to the informal sector, its value should overcome the higher wages in the formal sector.

8. Conclusion

This paper evaluates the effect of SP on the number of formal jobs and wages. We find, firstly, that SP did not decrease the number of formal jobs at the municipality level (H1) nor did it cause workers formally employed in the private sector to quit their jobs. We find no evidence of a more restricted supply of workers to the formal sector, in terms of equilibrium wages, as wages in the formal sector did not increase. This report uses more and higher quality data than existing studies of SP but reaches the same conclusion as most of the studies. The only exception to that conclusion is Bosch & Campos-Vazquez (2014), but we find that their results are not robust, and are highly dependent on the municipalities selected, the regression specification used, and the identification strategy implemented. Changes in any of these items makes their result disappear. The most solid conclusion with the best available data and more robust methods is that SP did not decrease the number of formal-sector jobs in Mexico. This does not mean that SP had the best design available; indeed, it is possible that universal health care that is not conditional on

working in the informal sector could be better than SP. It only means that the quantity of jobs in the formal sector did not suffer as a result of its implementation.

In the introduction we noted that to decide rationally whether a social protection programme should or should not be implemented, one has to look at both the costs and the benefits. Most of the literature on SP has looked at the costs, but, in the light of evidence from health insurance in the US, the benefits could be large and important. The next step in evaluating SP should be measuring its benefits, and more particularly the effects on financial protection and health outcomes. After all, this was the rationale for implementing it.

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Appendix

Figure 11. Appendix. Health expenditure as proportion of current income
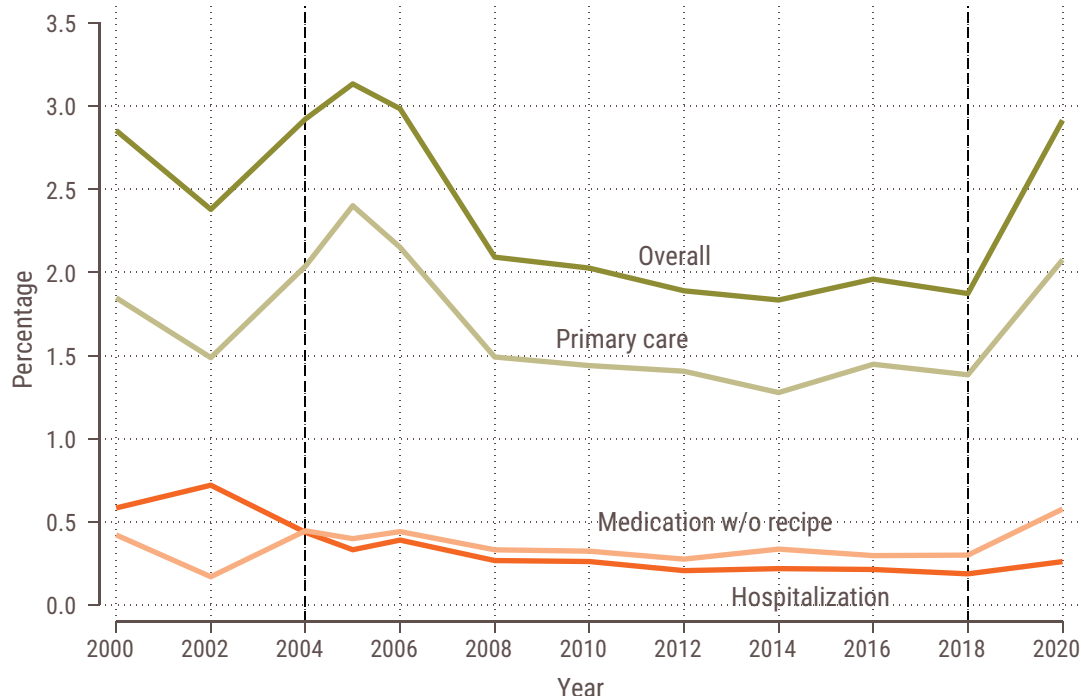
Vertical dashed lines denote beginning and ending of SP programme. This figure uses Mexico's income and expenditure survey ENIGH – conducted by INEGI – from 2000 to 2020 to plot the mean health expenditure as a proportion of a household's current income. To construct each variable, we take the quarterly reported expenditure on each of the expenditure categories and make them annual quantities, multiplying them by 4 as suggested by INEGI. We then standardize expenditure to 2018 Mexican pesos to make quantities comparable over time. Lastly, we divide each category's expenditure by the annualized current income to compute health expenditures as proportions of current income. For computing the mean, we 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.

## A.1   Two-way fixed effect (TWFE) diagnostics

Figure 12. Appendix. Two-way fixed effect (TWFE) weights
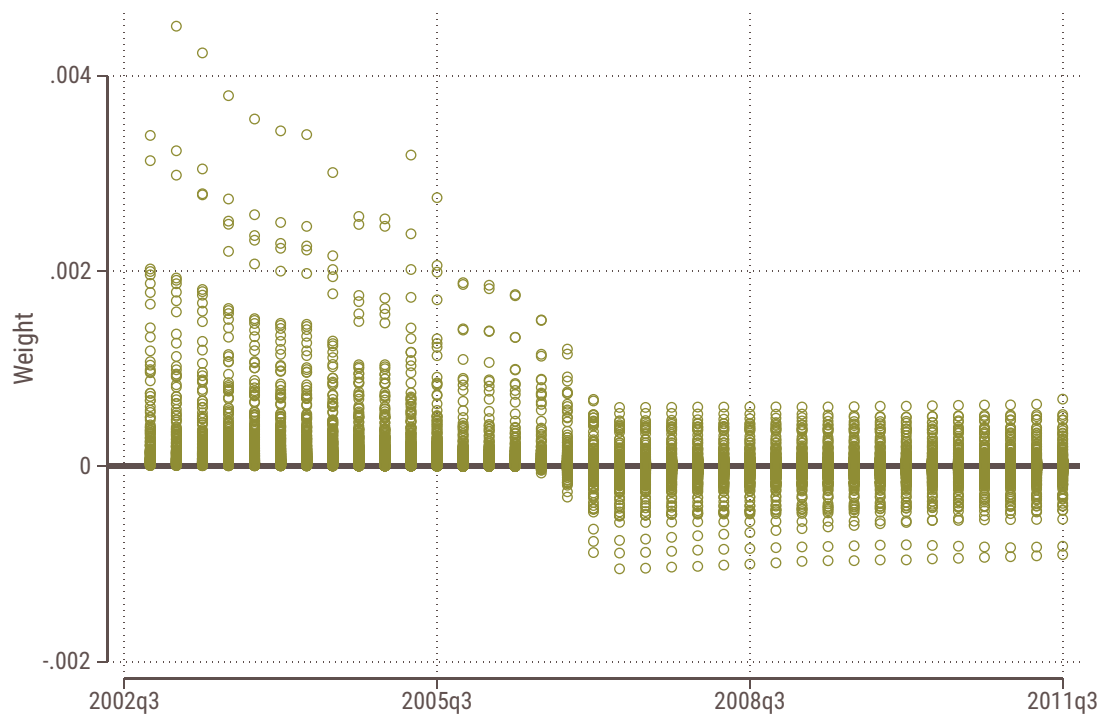
We compute the weights attached to the two-way fixed effects regressions studied in de Chaisemartin & D'Haultfœuille (2020), and using their STATA command – twowayfeweights. Under the common-trends assumption, beta estimates a weighted sum of 36741 ATTs. 26018 ATTs receive a positive weight, and 10723 receive a negative weight. The sum of the positive weights is equal to 1.37. The sum of the negative weights is equal to -0.37. Observe that for the later quarters we find negative weights, so that the DiD estimates become biased for estimates 3-5 years after implementation. Exactly when Bosch & Campos-Vazquez (2014) find their larger effects.

A.2 Replication Bosch & Campos-Vazquez (2014)

Figure 13. Appendix. Event studies - Bosch & Campos-Vazquez (2014) replication

(a) Employers

(b) Self-employed

(c) Employees
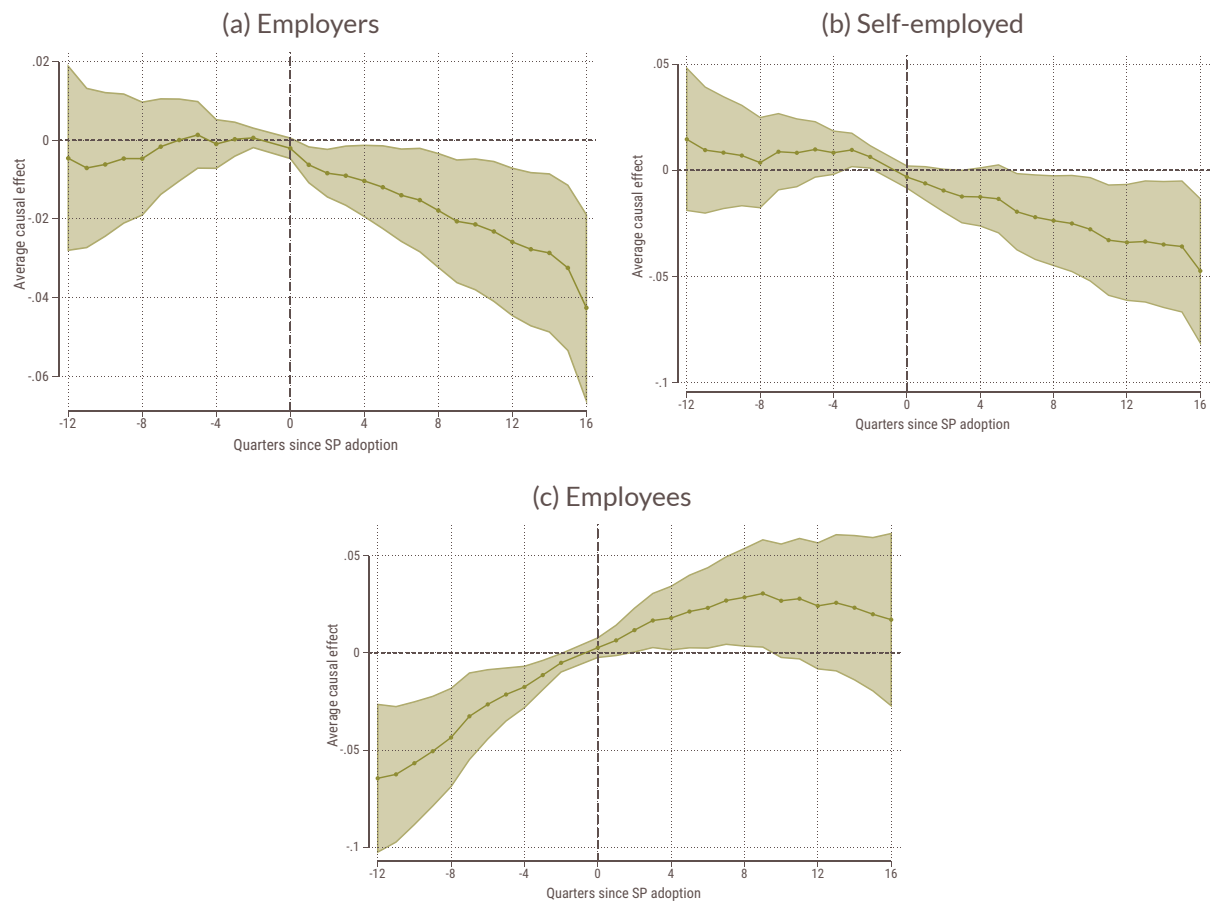
A.3   Robustness analysis of Bosch & Campos-Vazquez (2014) specification

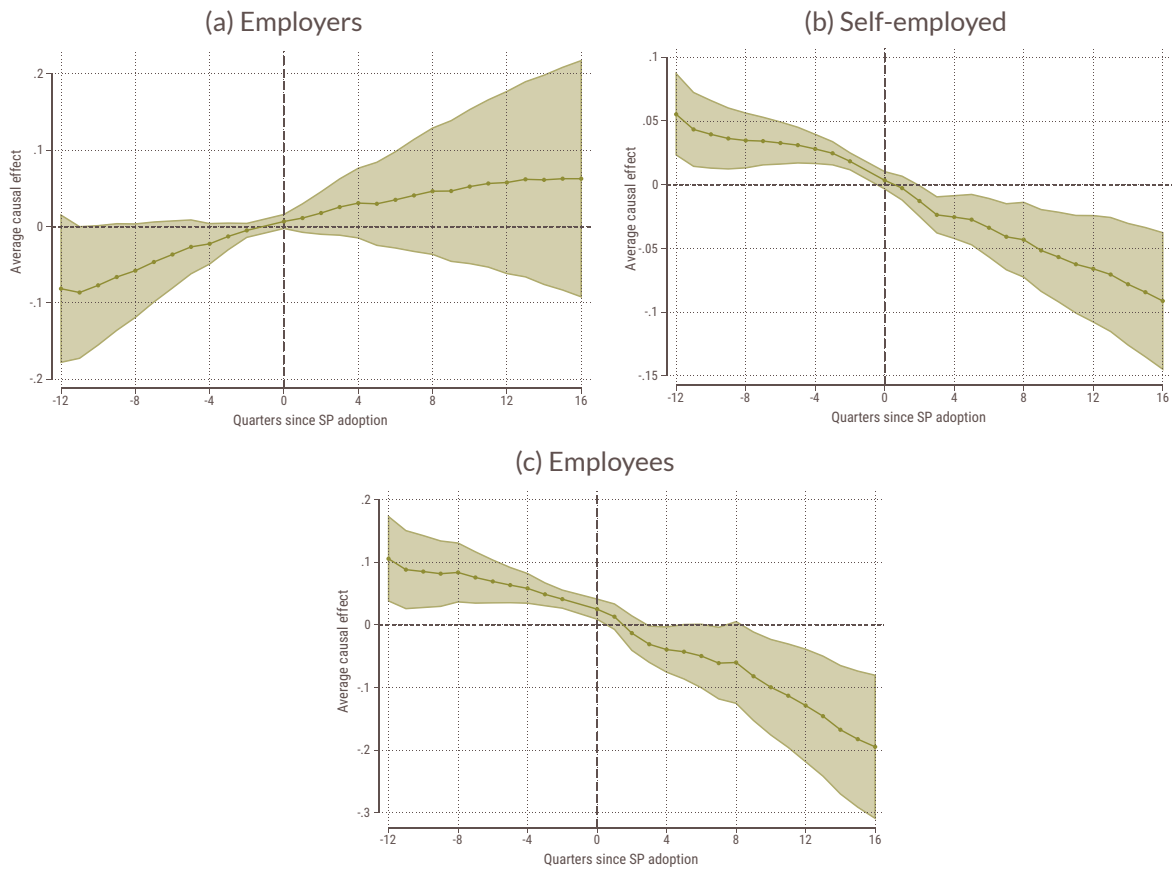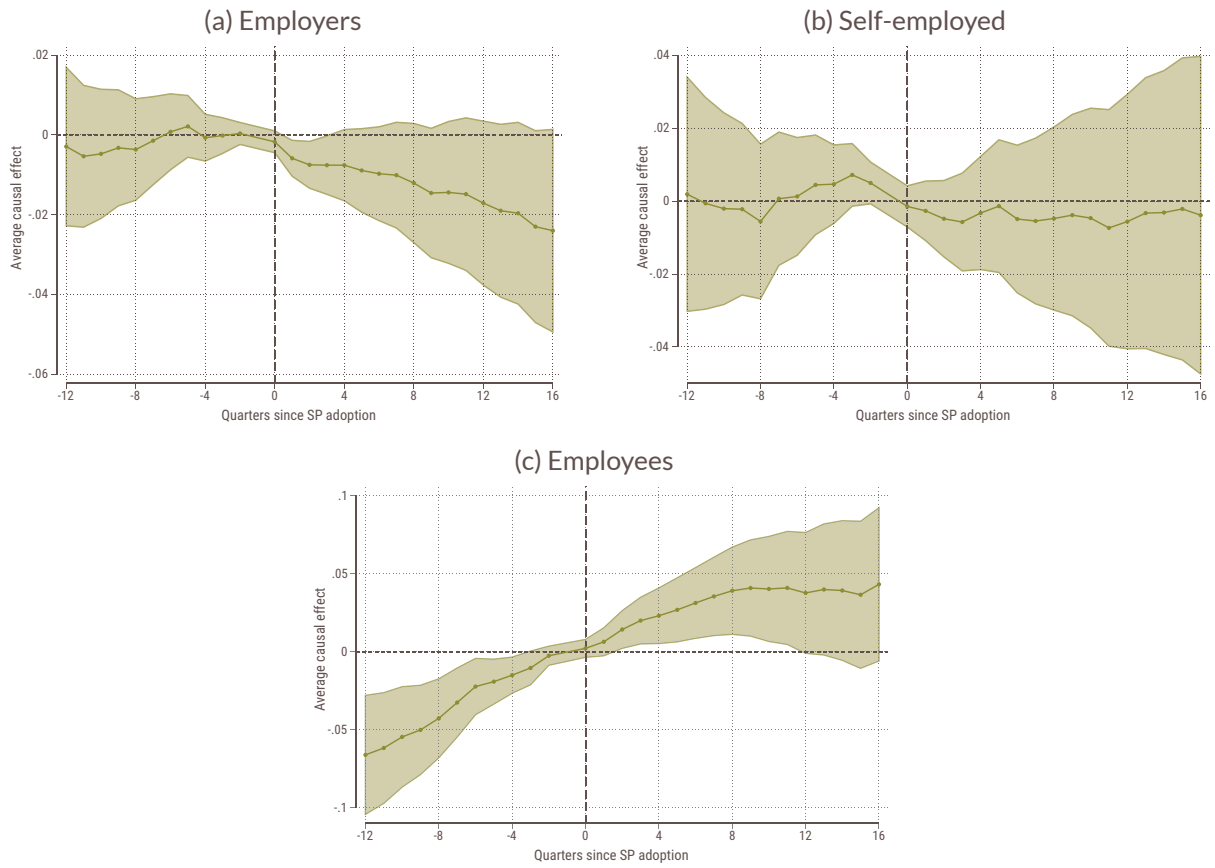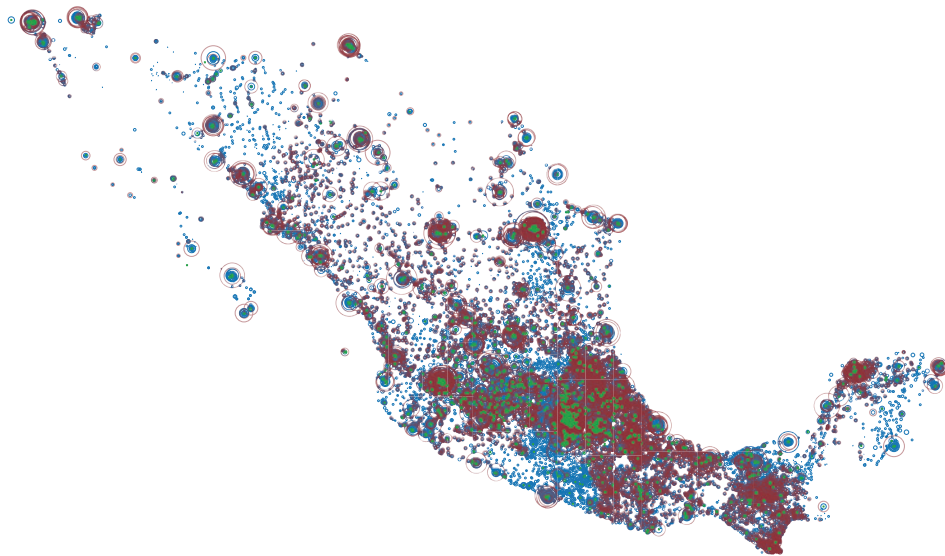**Figure 14. Appendix. Bosch & Campos-Vazquez (2014) replication. More municipalities**

(a) Employers

(b) Self-employed

(c) Employees

Figure 15. Appendix. Bosch & Campos-Vazquez (2014) replication. Flexible specification

(a) Employers

(b) Self-employed

(c) Employees
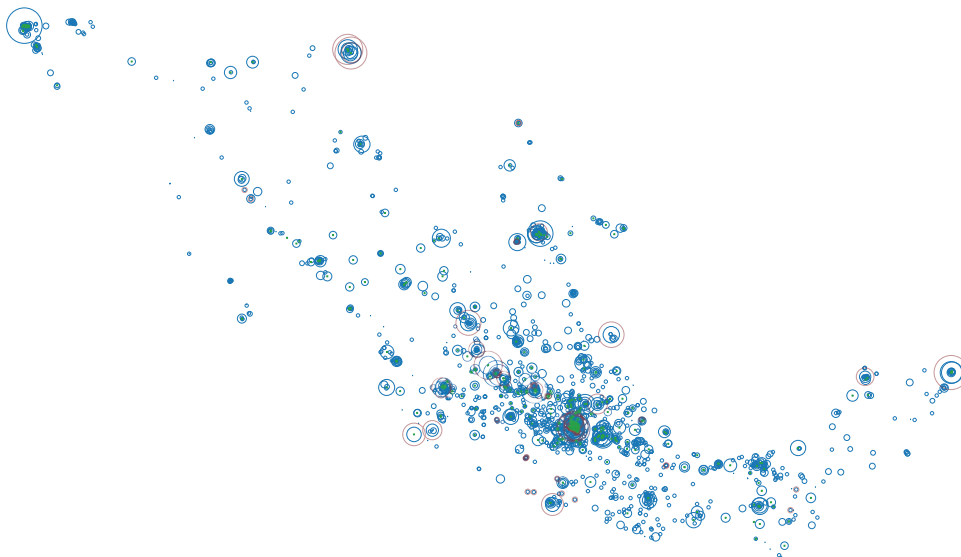
A.4 Instrumental variables

Figure 16. Appendix. Instrumental Variables. Clinics in Mexico

(a) Clinics in 2004

∘ All clinics   ∘ IMSS   · Second level

(b) New clinics in 2007

∘ All clinics   ∘ IMSS   · Second level

**Figure 17. Appendix. Instrumental Variables. Trends of IMSS employment
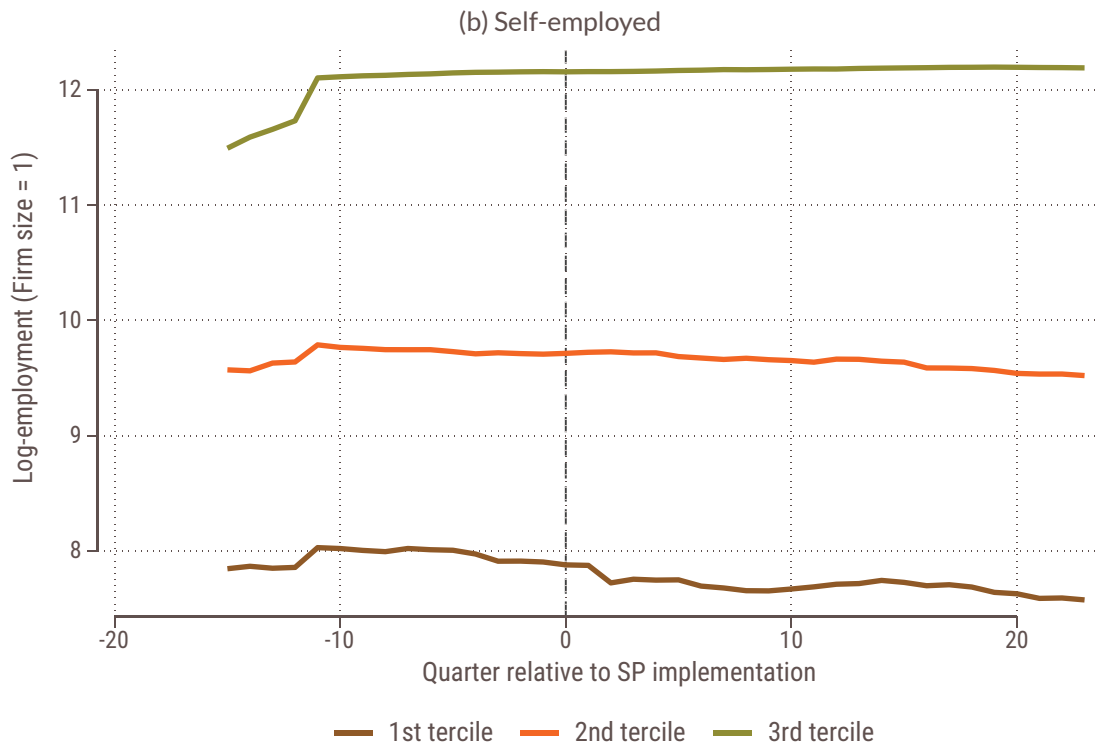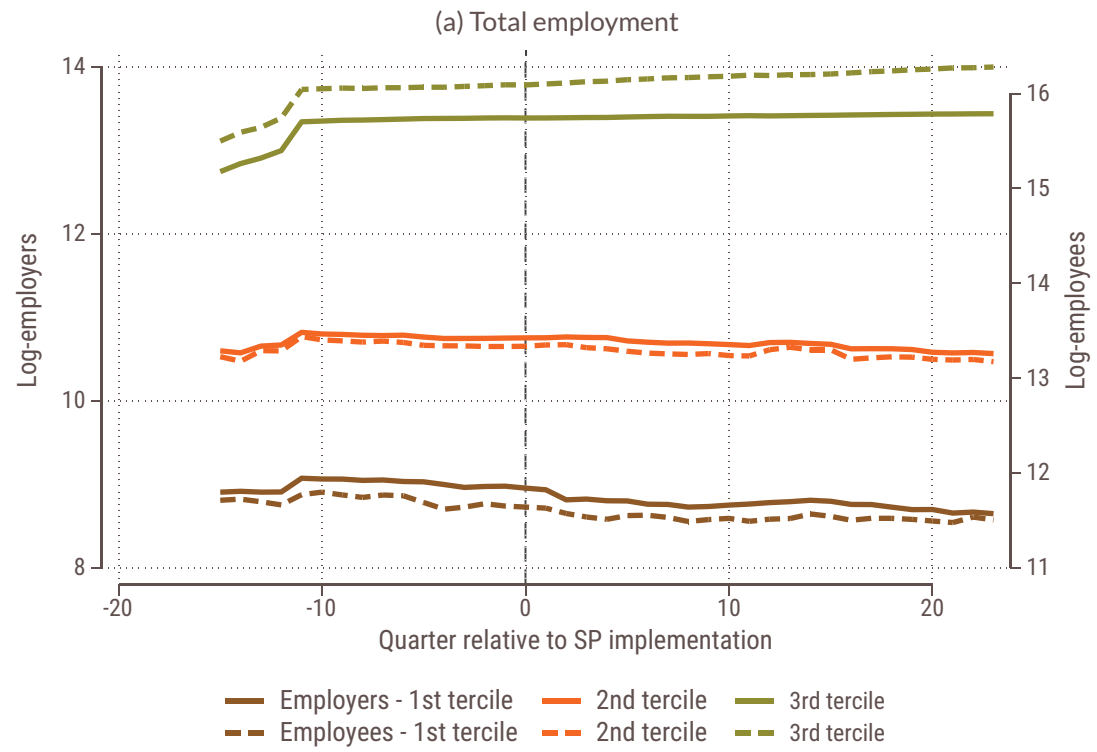by terciles of the number of clinics**

(a) Total employment

(b) Self-employed

Table 2. Appendix. Instrumental Variables. Pre-time trends (1-year)

	Employment					
	Employers		Self-employed		Employees	
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Total # clinics)	-0.070 (0.060)		-0.23* (0.12)		0.0097 (0.089)	
Log(# IMSS clinics)	0.033 (0.032)		0.074 (0.061)		0.010 (0.064)	
Log(# 2nd level clinic)	-0.012 (0.029)		-0.011 (0.057)		-0.010 (0.051)	
Log(# 3rd level clinic)	0.00030 (0.032)		0.0027 (0.055)		-0.042 (0.061)	
Log(Total # of rooms)	-0.032 (0.072)		0.088 (0.17)		-0.090 (0.075)	
Log(Total # of beds)	-0.00066 (0.011)		0.0088 (0.024)		0.016 (0.020)	
PAN	-0.020 (0.013)		-0.011 (0.023)		-0.058** (0.030)	
PRD	0 (.)		0 (.)		0 (.)	
Log-population	0.026 (0.11)	0.020 (0.11)	0.032 (0.26)	0.019 (0.26)	-0.13 (0.28)	-0.14 (0.28)
Luminosity	0.017** (0.0065)	0.016** (0.0066)	0.027** (0.013)	0.027** (0.013)	0.0098 (0.014)	0.0093 (0.014)
Luminosity^2	-0.00061** (0.00024)	-0.00060** (0.00024)	-0.00064 (0.00048)	-0.00065 (0.00048)	-0.00083* (0.00048)	-0.00080* (0.00048)
Luminosity^3	0.0000064** (0.0000027)	0.0000062** (0.0000027)	0.0000054 (0.0000055)	0.0000055 (0.0000055)	0.000011** (0.0000053)	0.000011** (0.0000053)
Gender	-0.40*** (0.072)	-0.40*** (0.072)	0.065 (0.097)	0.064 (0.097)	-1.27*** (0.23)	-1.27*** (0.23)
Observations	16884	16884	16884	16884	16884	16884
R-sq	0.218	0.217	0.147	0.147	0.211	0.210
F-statistics	8.16	8.44	3.01	3.26	3.07	3.16
p-value	0.15		0.20		0.39	
Municipality FE	✓	✓	✓	✓	✓	✓
LASSO selection		✓		✓		✓

In this table we test for the exclusion restriction of our instrument(s). This assumption states that the instruments are uncorrelated with outcome (employment) except that through its effect of SP implementation. To 'test' for this assumption, we estimate $\log \frac{y_{t+1}}{y_t} = \alpha + \beta_k Z_{k,t} + \epsilon_t$ in the pre-implementation periods. Our hypothesis is that $\beta_k = 0$. Odd-numbered columns estimate the previous equation and do not reject $H_0 : \beta_k = 0$. Even-numbered columns run the same specification but with variable selection using Lasso.

Figure 18. Appendix. Instrumental Variables. First stage
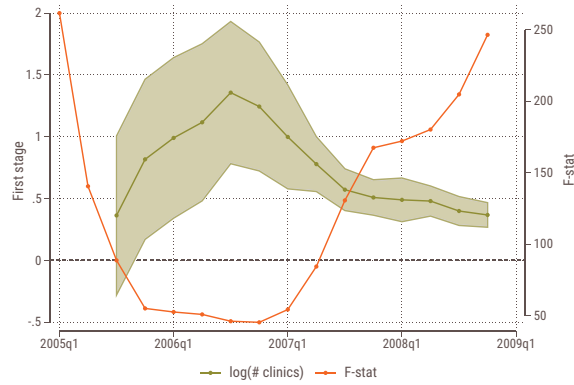
This figure shows the F-statistic and an estimated coefficient for the first stage plotted for each quarter. Errors are clustered at municipality level.

Figure 19. Appendix. Instrumental Variables. Dynamic second stage

(a) Employers

(b) Self-employed

(c) Employees

This figure shows the second stage separately for each quarter using the first stage from Figure 18. Errors are clustered at municipality level.

A.5 Difference of salaries for IMSS/no IMSS

Table 3. Appendix. Instrumental Variables. Salary changes

	Log (hourly wage)
	(1)
No IMSS	-0.08***
	(0.00)
Observations	7.367e+06
Population	2057112833
R-squared	0.57
Dep var mean	2.238
Municipality × Date FE	✓
Occupation FE	✓
Individual FE	✓

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