Does Access to Health Insurance Reduce the Risk of Miscarriages? Evidence from Mexico's Seguro Popular.

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Abstract

This paper analyzes whether Mexico's de-facto non-contributory health insurance program *Seguro Popular* had an effect on the risk of miscarriage during pregnancy. Using data on pregnancies over the 2004-08 period from the 2009 round of the National Survey on Demographic Dynamics (ENADID), and employing the staggered roll-out of the program as an identification strategy, it is found that it resulted in a significant reduction in the risk of a miscarriage. For the target population, a one percentage point increase in eligibility is found to decrease miscarriages by .04 percentage point at the average.
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1 Introduction and Background

Since the start of its roll-out in 2004, Mexico's health insurance system *Seguro Popular* has received considerable attention in the academic, as well as, policy literatures. Following Santiago Levy's early critique that the program will undermine efforts to bring Mexico's labor force into the formal sector (Levy 2008), several studies have set out to determine whether or not it changed the incentives to pursue formal vs. informal sector employment (Azuara and Marinescu 2010, Barros 2008, Bosch and Campos-Vázquez 2010, Camacho, Conover, and Hoyos 2010, Martínez and Aguilera 2010). They either find no effect or a statistically significant, but fairly small shift toward informality. Other studies focus on changes in the access and use of health services in response to the program (Knox 2008, Sosa-Rubi, Galarraga, and Harris 2009), or the avoidance of catastrophic health expenditures (Grogger, Arnold, León, Ome, and Triyana 2010, Gakidou, Lozano, González-Pier, Abbott-Klafter, Barofsky, Bryson-Cahn, Feehan, Lee, Hernández-Llamas, and Murray 2006, Knaul, Arreola-Ornelas, Méndez-Carniado, Bryson-Cahn, Barofsky, Maguire, Miranda, and Sesma 2006, King, Gakidou, Imai, Lakin, Moore, Nall, Ravishankar, Vargas, Tellez-Rojo, Hernández-Avila, Hernández-Avila, and Hernández-Llamas 2009). Lastly, several studies have tried to determine the program's effect on health outcomes. Thus far, most of these studies have failed to find any significant effect (Barros 2008, Duval-Hernández and Smith-Ramírez 2011, King, Gakidou, Imai, Lakin, Moore, Nall, Ravishankar, Vargas, Tellez-Rojo, Hernández-Avila, Hernández-Avila, and Hernández-Llamas 2009, Knox 2008). However, as most health outcomes, especially in adults, are slowly moving targets, it is doubtful whether one would expect to find any significant effect of a recently established program (Scott and Aguilera 2010). Focusing on shorter-term health outcomes, some authors have recently found evidence that program affiliation results in lower levels of cholesterol (Ruvalcaba and Parker 2010), lower blood pressure (Ruvalcaba and Parker 2010, Bleich, Cutler, Adams, Lozano, and Murray 2007), and lower infant mortality (Pfutze 2014).

The present paper contributes to this last line of research. Similar to infant mortality, any effect of improved health care access on the likelihood of a miscarriage can be expected to manifest itself in the short-run. While there are some data related problems (a relatively rare outcome, combined with a noisy measurement of program exposure) that affect statistical power, it is found that the program significantly reduced the risk of a miscarriage by around .04 percentage points for each percentage point increase in coverage of the target population. It is also shown that this effect is not changed much by potential selection on the outcome variable.

The *Seguro Popular* health insurance program started its roll-out in January 2004, after a pilot program in 2002/03. The roll-out occurred at the level of locality, with localities that had a large proportion of its population uncovered being enrolled first. The insurance is de-facto non-contributory for all beneficiaries as it is free of charge for households in the bottom two wealth quintiles as determined by a uniform questionnaire, and for the immediate family of every child born after December 1, 2006. According to the Mexican health ministry, less than 1% of the affiliated households pay for coverage. Its declared aim is to provide health insurance to the more than 50% of Mexico's population, mostly self-employed and/or in the informal sector of the economy, that did not get coverage through any of the employment linked programs nor through the private market.[1] It does not replace any of the existing public health services, but adds a lowest tier of coverage to the existing system. Its distinguishing feature is that it is run as an insurance, not a direct provider of health services. It certifies health care providers and buys services from a list of close to 300 covered interventions (plus a separate list of interventions that would result in catastrophic health expenditures) at fixed prices. A large number of these interventions are related to pre-natal care.[2] While in theory any provider can be certified, the lion's share is made up of clinics and hospitals run by the federal and state level ministries of health (Lakin 2010).

[1] See Frenk, González-Pier, Gómez-Dantes, Lezana, and Knaul (2006) for a detailed discussion on the motivation for and description of the program.

[2] The catalog detailing all covered interventions can be found on the Seguro Popular webpage at http://www.seguro-popular.salud.gob.mx/images/contenidos/Causes/CAUSES2012.pdf

2 Data and Empirical Strategy

Two different data sources are used. For one, the data on the Seguro Popular roll-out has been provided directly by the Mexican Health Ministry. Starting in January 2004, it shows the number of affiliated individuals in each municipality on a monthly basis. These monthly observations are available until late 2011, at which point roll-out was considered complete (meaning that every person in the country had the right to enroll). All other data come from the 2009 round of Mexico's National Survey on Demographic Dynamics (ENADID, by its Spanish acronym), conducted by the country's National Statistical Institute (INEGI). The survey was conducted between May 18th and July 10th 2009, based on a stratified sampling frame that was designed to produce representative results at the level of the state and its rural and urban areas. It collected detailed data on each sampled household, and administered an additional extensive questionnaire to all female members of 15-49 years of age. This questionnaire collected detailed data on all pregnancies a woman had, whether or not they resulted in a miscarriage, and if so in which month of gestation.[3]

The unit of observation is a pregnancy, and the binary outcome of interest is whether or not it resulted in a miscarriage. The data available allow for the calculation of the month of onset of each pregnancy observed. In order to avoid truncation in the sample only pregnancies that started between

[3]See www.inegi.org.mx for a detailed description of the ENADID

January 2004 and July 2008 are included. After that date, pregnancies may still not have concluded by the time of the data were collected. A standard probit model is estimated with municipality specific and monthly (for each of the 55 months in the sample) fixed effects, implemented through the inclusion of a set of binary variables. The identifying assumption is thus that once municipality-specific time invariant characteristics are controlled for, the program roll-out can be treated as quasi-random. Put differently, the roll-out did not respond to changes in the outcome of interest or unobserved factors. Several previous studies have used the staggered roll-out as the treatment variable and shown that it is uncorrelated with municipal characteristics other than size (more rural areas were treated earlier) and the proportion of the uncovered population (Bosch and Campos-Vázquez 2010, Pfutze 2014, Conti and Ginja 2014). For this reason this exercise will not be repeated here.

The treatment variable of interest is the relative level of roll-out in each month at the municipal level, constructed as the proportion of the number of beneficiaries in each month relative to the number in September 2011 (the last date available on a monthly basis, and the month when roll-out was supposed to be concluded). Given that actual implementation occurred at the level of the locality, this is not an ideal measure of whether or not a woman was eligible for coverage during the pregnancy, but the best one that is available. The point estimates in the estimation must thus be interpreted as the estimated effect of complete program roll-out. In the most parsimonious specification the only right hand side variables included are the treatment and the fixed effects.

Results will also be presented for a specification with additional control variables. These are: A wealth index, the mother's age at the onset of the pregnancy and its squared value, the number of her previous pregnancies and miscarriages, and a binary indicator for being indigenous. Given that the data was collected in early/mid 2009, only control variables that are time invariant are included. The wealth index is constructed based on a number of binary variables that capture dwelling characteristics and ownership of durable goods using principal components analysis. While the wealth index could be time variant, it can safely be assumed that it would only be changing fairly slowly. More than just being an additional control variable, the wealth index's main purpose is to identify asset poor households, as these are most likely to lack any other insurance coverage. Results will therefore be presented first for the full sample, followed by the subsample of households in the lowest three quintiles of the wealth index, which is interpreted as capturing the target population. Table 1 shows summary statistics on all variables discussed. After municipalities that have no variation in the outcome are excluded (which is necessary due to the municipality level fixed effects which would otherwise perfectly predict the outcome), the sample consists of 27,445 pregnancies. It can be seen that 11.4% of pregnancies in the samples resulted in a miscarriage, implying that the binary outcome variable denotes a relatively rare event. The average level of program roll-out is 25.9%, for a few (small) municipalities this number is larger than one in some months due to an overall decline in their total population. The average age at the onset of a pregnancy is around 25 years of age, and 5.4% of women are indigenous.

(Table 1 about here)

One problem with data on miscarriages is that in many cases the event is unobserved by a woman. No reliable data on the overall incidence of miscarriages exists even for rich countries. This implies that a successful pregnancy (i.e. one resulting in a live birth) is more likely to be observed than one resulting in a miscarriage. This induces an obvious problem of potential selection on the outcome variable, which can be addressed using weighted exogenous sampling maximum likelihood (WESML) estimator first proposed by Manski and Lerman (1977) for the choice-based sampling. This estimator has also been applied by Pfutze (2014) to estimate the program's impact on infant mortality. It consists of applying to each observation n a weight defined as $w(i_n) = \frac{Q(i_n)}{H(i_n)}$; where $Q(i_n)$ denotes the population proportions of the outcome for observation n, and $H(i_n)$ denotes the same proportion in the sample. Since the true population proportions are unknown, the WESML will be run as a robustness check under two different hypothesized true population proportions of miscarriages (15% and 20%) to verify whether results would change. It has to be pointed out that an undersampling of miscarriages would in any case only result in a negative bias on the estimator, i.e. working against finding statistically significant results.

3 Results

Table 2 shows estimation results. The first two columns use the entire sample, and the remaining four only observation in the lowest three wealth quintiles. Specifications with and without controls are presented for the standard probit models in columns (1)-(4). The last two columns show results for the WESML estimator assuming that the true proportion of pregnancies that result in a miscarriage in the population is 15% and 20%, respectively. The tables show parameter estimates and standard errors in parentheses. For ease of interpretation, at the bottom the table also shows the estimated marginal effect at the mean for the treatment variable.

As explained above, the treatment variable is an imperfect measure of whether or not a woman as eligible for the program during the pregnancy. It has rather to be interpreted as the probability of being eligible. The estimates are, therefore, intention to treat effects. Moreover, since actual eligibility is not observed, the effect can be expected to be statistically weaker. This is apparent in the table: While all the control variables are highly statistically significant, the treatment variable is so only at the 10% level. That said, the results on it are very consistent. As would be expected, the effect becomes stronger if the estimation is reduced to the target population in columns (3)-(6). The point estimates stay practically constant for result within each sample, despite adding a number of highly significant controls. Neither does the WESML change the point estimates.

The implied marginal effects at the mean need to be interpreted carefully, since the numbers presented refer to the counterfactual of complete program roll-out, evaluated at the point of the largest marginal effect. Referring back to table 1, for the lowest three wealth quintiles, a one percentage point increase in roll-out, would imply a reduction of .04 percentage point, or around .33% relative to the sample mean. Assuming different population proportions, the marginal effect continues to be around .33%. In terms of standard deviations, the reduction of a one standard deviation increase in program coverage implies roughly a one percentage point (or slightly less than 10%) reduction in miscarriages.

The point estimates on the control variables are of no direct interest, but merit some brief discussion. The risk of a miscarriage follows an U-shaped pattern in age (larger at low ages and high ages), and decreases in the number of previous pregnancies. Having had more miscarriages prior to the current pregnancies increases the risk of having another one, which probably captures woman specific risks. All these results would be expected. The two somewhat counterintuitive findings are the positive effect in the wealth index and the negative one on the dummy of being indigenous. The likely reason is that they may proxy for other unobserved factors such as better medical knowledge (and general awareness whether or not a miscarriage occurred) or cultural factors that affect whether the respondent is willing to admit to prior miscarriages. The important insight from the control variables is that their inclusion does not change the estimates on the treatment, providing strong evidence for the exogenous nature of program roll-out.

4 Conclusions

This study provided convincing evidence that access to the de-facto non-contributory health insurance program Seguro Popular in Mexico resulted in decreased risk of miscarriages. This result is important in two respects. Firstly, it confirms that access to basic health care has an important effect on pregnancy risks. Secondly, it also shows that the program did have a measurable positive effect on health outcomes that can be expected to change in the short-term. This is an important result for the future design of Seguro Popular, and also for policy makers in other countries that try to extend basic heath care services to the entire population. It also bodes well for possible effects on longer-term health outcomes, which will be much more difficult to establish and should be the focus of future research.

Table 1: Summary statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Miscarriage	0.114	0.318	0	1	27445
Insurance at Pregnancy	0.259	0.23	0	1.222	27445
Wealth	-0.336	2.235	-6.553	2.277	27445
Age	25.113	6.459	10	50	27445
Age Squared	672.389	346.237	100	2500	27445
Previous Pregnancies	1.398	1.545	0	15	27445
Previous Miscarriages	0.177	0.478	0	7	27445
Indigenous	0.054	0.226	0	1	27445

Table 2: Results

	(1)	(2)	(3)	(4)	(5)	(6)
Insurance at Pregnancy	-.134	-.140	-.237*	-.232*	-.245*	-.259*
	(.117)	(.119)	(.135)	(.137)	(.142)	(.148)
Wealth		.012**		.006	.006	.007
		(.006)		(.007)	(.007)	(.008)
Age		-.068***		-.058***	-.060***	-.061***
		(.011)		(.013)	(.013)	(.014)
Age Squared		.002***		.002***	.002***	.002***
		(.0002)		(.0002)	(.0002)	(.0003)
Previous Pregnancies		-.046***		-.054***	-.056***	-.058***
		(.009)		(.011)	(.011)	(.011)
Previous Miscarriages		.155***		.135***	.141***	.148***
		(.022)		(.028)	(.029)	(.030)
Indigenous		-.111*		-.139**	-.146**	-.154**
		(.064)		(.068)	(.070)	(.073)
Obs.	27445	27445	19100	19100	19100	19100
Marginal Effect at Mean	-.025	-.025	-.041	-.039	-.051	-.067
Wealth Quintiles	All	All	1-3	1-3	1-3	1-3
Assumed True Population Risk	None	None	None	None	15%	20%

Notes: ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Results for probit model with robust standard errors on binary variable indicating a miscarriage. Parameter estimates shown in main table with p-values in parenthesis, the corresponding marginal effect at the mean for exposure to insurance is shown in below the number of observations. All specification include year-month and municipality level fixed effects.

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