How Universal Healthcare Affects Individuals' Choices: The Case of Seguro Popular

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Abstract

This paper assesses the effects of Seguro Popular, a universal healthcare program implemented in Mexico, on individuals labor force participation, family planning, and the take-up of womens' preventative cancer screenings. I find that the implementation of Seguro Popular increases the target demographic for Mammogram take-up of such exams, has no significant effect on take-up of Papanicolaou Tests, and find some evidence to support the claim that Seguro Popular increased the amount of children women wanted to have. Finally, Seguro Popular had a negative effect on labor force participation, most notably among working males.

Introduction

Pushback from universal healthcare has come from experts who critique how the introduction of such programs distorts individuals' decisions. An example of an individual's microeconomic choices shifting from economic theory would labor supply dynamics. In a standard labor-leisure model, the introduction of free or extremely subsidized health coverage introduces an income effect which increases leisure, a normal good, inversely reducing an individual's participation in the labor market.

In the backend of the 20th century, Mexican lawmakers faced a healthcare dilemma. A Mexican Constitution amendment enacted in 1983 entitled every Mexican the "right to health protection". However, no universal healthcare legislation was able to be passed for decades. The Instituto Mexicano del Seguro Social (IMSS) had long been established in Mexico to cover workers in the formal sector. Workers and employers would register and contribute roughly 24% on average of workers' salary, essentially splitting the healthcare bill between workers and employers to fund the IMSS. The IMSS operated as both a healthcare insurer collecting premiums and also a hospital network. This and similar programs insured roughly three-fifths of Mexicans as of 2000. This left 12 million families, mostly households with workers in the informal sector, with little more than underfunded state-run health ministries. (Siera et al., 2023)

This changed when in 2003, Mexico passed the Seguro Popular program and in 2004, Mexicans could start enrolling for Seguro Popular. Seguro Popular was targeted to cover informal workers and their families. Seguro Popular aimed to enroll 1.7 million families per year over the course of seven years. This staggered rollout was due to the fact that not all areas of Mexico had the healthcare infrastructure to immediately accommodate the additional onset of 12 million families. The goal was to give priority to the poorest, families living in rural areas, and indigenous populations. I exploit this exogenous variation in rollout to estimate the effect of Seguro Popular on a set of outcomes such as labor, family planning, and take up of preventative womens' cancer screenings. However, due to the infrastructure constraint, Seguro Popular arrived first in areas with sufficient healthcare facilities and personnel, generally better off areas with disproportionately less of the target population - heterogeneity in those receiving Segiro Popular in an earlier or later group is a possible endogeneity issue to later address. As seen in a balance test (Table 1) showing the differences in individuals and households that received Seguro Popular in between the first and second wave of the Mexico Family Life Survey (roughly 2002-2005) and those that received Seguro Popular in between the second and third wave of the Mexico Family Life Survey (roughly 2006-2009), individuals/households that received Seguro Popular tended to be on average more educated, have higher incomes, and more likely to live in bigger cities. However, there isn't a statistically significant difference in distance to the nearest clinic or use of outpatient services between the two groups, suggesting that availability of healthcare services did not differ drastically contrary to what the literature suggests.

In this paper, I study impacts of the sudden implementation of Seguro Popular on the labor supply, family planning decisions, and preventative cancer screening takeup of individuals eligible for Seguro Popular. I estimate Seguro Popular's intent-to-treat treatment effect using Callaway & Sant'anna's (CS) estimator. (Callaway and Sant'anna, 2021) Then, I check the robustness of this estimator's results using a difference-in-difference estimator and a machine-learning based method, Matrix Completion with Nuclear Norm Regularization (MCNNR). (Athey et. al., 2021)

This paper makes a few contributions to the literature. The first is the nature of our data as this paper makes use of data that spans a decade. Many previous studies into Seguro Popular look at a short time horizon on the effects of Seguro Popular. For example, Knox (2008) uses difference-in-difference estimators on cross-sectional data from 2003/2004 and 2004, only able to capture the short-term one-year effects of Seguro Popular on healthcare utilization and labor. Knox (2008) found insufficient evidence to conclude that the treatment had a discernible effect on most workers, including heads of households, males overall, and females overall. However, Knox (2008) found that Seguro Popular had a statistically significant reduction of roughly 7.8 hours of labor per week of secondary workers. Barros (2009) similarly found that Seguro Popular had no effect on labor force participation. One year might not be enough time to fully realize the long term treatment effects of healthcare coverage, particularly considering that improved health outcomes often unfold gradually over time. If rollout doesn't occur instantaneously within a municipality, the intent-to-treat treatment effect may exhibit a higher impact over time, as more individuals enroll later in the process.

Bosch & Campos-Vazquez (2014) used administrative data to find that employment overall reduced by 4% after four years as a result of Seguro Popular. However, Siera et al. (2023) contested the results found in Bosch & Campos-Vazquez - they replicated the Bosch & Campos-Vazquez study using methods such as estimators that take into account the effect of Seguro Popular being different in different municipalities and found that the Bosch & Campos-Vazquez study was not robust. They instead find no evidence of a decrease in employment as a result of Seguro Popular. As of date, (Siera et al., 2023) is the only study to not use biased two-way fixed effects estimates (biased since they don't take into account possible heterogeneity in treatment effects in staggered treatment times) to estimate the long term effects of Seguro Popular. Other studies either look at short-term effects of Seguro Popular, use biased two-way fixed effects, or have both shortcomings. However, the scope of (Siera et al., 2023) paper was limited to informal/formal sector switching and wages. I expand this use of both to other topics of interest such as family planning, hours worked, and cancer screening take up.

An additional contribution that this paper makes is its contribution to the literature on Seguro Popular's effects on individual outcomes beyond labor participation. Most economic papers on the effects of Seguro Popular on health outcomes and a few papers such as Knox (2008) and Barros (2008) have looked into the effects of Seguro Popular on individual choices. Knox found little change in spending as a result of Seguro Popular. This paper looks at the effects of individual choices beyond labor participation and is the first paper to explore Seguro Popular's effects on family planning. Furthermore, while the effects of Seguro Popular on healthcare utilization has been studied and shown to increase healthcare utilization (Parker et al., 2018), this is usually measured via a coarse variable such as visits to healthcare facilities. This paper analyzes the effect of a sudden onset of universal healthcare on specific preventative screenings such as Mammograms (breast exams), which screen for breast cancer, and Papanicolaou test (pap smears), which screen for cervical cancer.

This paper also tests the robustness of Machine Learning matrix completion methods on "thin" yet "deep" matrices. In other words, I am testing whether matrix completion methods can overcome the lack of lengthy historical time data with a deep input of covariates. This will be explained in further detail in the methodology section.

Data

Seguro Popular Administrative Data

While the IMSS has individual and municipality level data on Seguro Popular, this data isn't publicly available. However, Bosch & Campos-Vazquez (2014) in their AEA reduplication files include a dataset that lists the number of families enrolled in Seguro Popular by municipality over time. Seria et. al. (2023), who had access to internal government administrative data, was able to confirm that this reduplication dataset was consistent with the official data. I use this dataset to figure out when Seguro Popular was rolled out to a given municipality. Our threshold for when a "roll-out" occurs in a municipality is when at least 10 families are enrolled in that given year.

Mexican Family Life Survey (MxFLS)

The Mexican Family Life Survey (MxFLS) dataset is a longitudinal panel dataset that follows 8,440 families and over 35,600 individuals over a decade period. Data was collected in three batches:

2002, 2005-2006, and 2009-2012. Attrition is low as 91% of respondents in the first survey in 2002 were followed up with. The survey tackles a wide range of questions such household consumption, household healthcare usage, and household finances to name a few categories. The month and year of when the survey was conducted was recorded which we are able to link to the Seguro Popular administrative dataset (however in this paper, I refer to the groups as the 2002-2005 group or the 2006-2009 for simplicity).

General Model

$$Y_{idt} = \gamma_d + \lambda\square + \beta^* d_{dt} \quad (1)$$

Y_{idt} denotes the outcome variable, which based on the specification is labor participation (measured by hours worked per year), family planning (measured by children wanted if were to start over again), or use of preventative womens' cancer screenings (measured by binary outcome whether or not individual has frequent breast exams and pap smear exams)

This study does not aim to measure the average treatment effect of receiving Seguro Popular. Even though, when eligible, enrollment into Seguro Popular is extremely subsidized, or even free for the poorest quintile, measuring the difference between those with Seguro Popular and those without present a self-selection issue. Households that enroll into Seguro Popular may need health insurance more and not be representative of everyone eligible for Seguro Popular.

Instead, linking individuals' municipalities to the Seguro Popular administrative dataset detailing when Seguro Popular was rolled out in a municipality, I am able to measure the intent-to-treat treatment effect of Seguro Popular. As such, I aim to measure β, the intent-to-treat treatment effect of the Seguro Popular program. Treatment assignment refers to informal workers in a municipality that have received Seguro Popular as time $t \leq$ present time. Not assigned treatment refers to informal workers in municipalities who haven't gotten Seguro Popular yet and will receive Seguro Popular in time $t >$ present time and formal workers, the latter of which would never be eligible for Seguro Popular. While I do not estimate an average treatment effect, this intent-to-treat treatment effect can still give us a lower bound on the average treatment effect.

Methodology

Issues with Two-Way Fixed Effects

The bulk of the previous literature on the effect of Seguro Popular such as Knox (2008) and Bosch & Campos-Vazquez (2014) employed two-way fixed effects models. Goodman-Bacon (2021) showed that these estimators are often biased when there is differential timing and heterogeneous treatment effects over time. Two-way fixed effects identify variance-weighted average treatment effect (VWATT), which isn't the ATT, and only when the variance weighted parallel trends assumptions hold. This can be an issue if those receiving Seguro Popular earlier on for example have more pronounced long run treatment effects and are seeing large changes in their outcome between a later period, however are being used as a control during this period. This can even make the VWATT opposite sign as the ATT. (Goodman-Bacon, 2021)

Callaway Sant'Anna Estimator

Families are broken up into three different pools. Informal workers whose municipalities received Seguro Popular in 2002-2005, informal workers whose municipalities received Seguro Popular 2006-2009, and formal workers who would never be eligible for Seguro Popular.

$$ATT(g,t) = E\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{\hat{p}(X)C}{1-\hat{p}(X)}}{E\left[\frac{\hat{p}(X)C}{1-\hat{p}(X)}\right]}\right)(Y_t - Y_{g-1})\right]$$

Given the issues with two-way fixed effects, I employ the Callaway and Sant'anna (CS) estimator. Callaway and Sant'anna (2021) created an estimator that when all assumptions are met can yield a less biased and consistent estimate of each group's individual group-time treatment effect. When working with staggered treatment timing and heterogeneous timing of treatment effects, the CS estimator is less biased than a two-way fixed effects estimator since it never uses an already treated treatment group after its treatment as a comparison group. The CS estimator also uses pre-treatment covariates, where I select all variables that are unbalanced excluding collinear terms to be covariates, to get a propensity score which is used to weight units.

Assumptions: Conditional Parallel Trends and Overlap in Propensity Scores

A key identifying assumption of the Callaway and Sant'anna estimator is the assumption of parallel trends conditional on covariates between group g and groups that are "not-yet-treated" by time t + δ where δ is additional time until group g is treated. The distribution of covariates is different across groups. For example, municipalities that received Seguro Popular earlier tend to be richer and more educated. Since potential outcomes are reliant on covariates and not whether someone got treatment, differences in covariates can affect the potential outcomes and thereby size of treatment effects. This assumption, in the absence of treatment, the mean outcomes for the group initially exposed to treatment in period g and those for the control group would have exhibited parallel trends. There are no municipalities that never "received" treatment, however, in the final comparison 2x2 D-in-D, formal workers are used as a comparison group. So this assumption assumes that formal workers and informal workers in the absence of treatment would have also exhibited parallel trends conditional on covariates. There's only 3 time points, but we're able to compare the pre-trends of the formal worker pool and the pool that received treatment 2006-2009 in between t = 2002 and t = 2005 since the latter group hasn't been treated yet in t = 2005. When performing the chi-squared statistic of the null hypothesis that all pretreatment ATT(g,t)'s, are equal to zero, I fail-to-reject the null hypothesis when the outcomes are births wanted, frequent breast exams, and frequent pap smears. However, the null hypothesis is rejected when the measured outcome is labor outcome; this is a blatant violation of the conditional on covariates pre-trends test. Nonetheless, the interpretability of this pre-trends test is limited due to the examination of only one time period, making it challenging to draw definitive conclusions regarding the presence or absence of conditional parallel trends in either scenario.

Overlap in propensity scores is also met, as seen in Figures 1 and 2 which plots a density distribution of the propensity score between the varying treatment groups.

Matrix Completion Model

I also employ Matrix Completion with Nuclear Norm Regularization (MCNNR). (Athey et al., 2021) This model takes a different approach to estimate the causal effect of Seguro Popular: by imputing the missing potential outcomes when an individual switches from one potential outcome to another. Treatment effect is the difference in outcomes before and after when treated Y(1) minus the potential outcome when not treated Y(0). However both outcomes can't be observed in real life - the Y(0) matrix gets filled up until the time of treatment, where the individual then switches over to treated status (and untreated Y(0) outcomes are no longer observed). Athey, Imben, and co-authors' MCNNR method essentially fill out the rest of the Y(0) matrix. In the situation of "thin" matrixes, matrices without a lot of historical data, such as this case where there's only data on 3 time periods, and some Y(0) only have one unobserved value (although some observations also have two unobserved values unlike unconfoundedness) also have after switching to treatment status, matrix completion finds the estimated missing Y(0) outcomes by using vertical weighted averages of the control group units, similar to unconfoundedness. Once the estimated Y(0) matrix is calculated, the treatment effect denoted by τ is the average difference between $Y_{it}(1)$ and the estimated $Y_{it}(0)$ as in

$$\tau = \frac{\sum_{i,t} W_{it}(Y_{it}(1) - Y_{it}(0))}{\sum_{i,t} W_{it}} \tag{3}$$

The actual matrix Y(0) without covariates is computed by

$$Y_{N\,X\,T} = L_{N\,X\,T} + \varepsilon_{N\,X\,T} \tag{4}$$

and when adding unit specific covariates X_i , time specific covariates Z_t, unit time specific covariates V_{it} computed by

$$Y_{it} = L_{it} + \sum_{p=1}^{P} \sum_{q=1}^{Q} X_{ip} H_{pq} Z_{qt} + \gamma_i + \delta_t + V_{it}\beta + \varepsilon_{it} \tag{5}$$

$$||L||_* = \sum_{j=1}^{min(N,T)} \sigma_i(L) \quad \text{where } \sigma_i(L) \text{ is rank of L} \tag{6}$$

MCNNR employs regularization, which you may be familiar with from LASSO regressions, which also employs regularization. Regularization is basically a penalty term added to the loss function when optimizing model parameters. Nuclear norm regularization however adds a penalty term based on the rank of the matrix as seen in equation (6)- essentially penalizing the complexity of the model by encouraging sparsity and promoting low-rank solutions. Trying to achieve optimal prediction of the counterfactual values while preserving a low rank allows the predicted matrix to capture essential patterns, preserving the underlying relationships in the covariates while minimizing overfitting.

$$min(L)\ \frac{1}{|O|} \sum_{(i,t)\epsilon o} (Y_{it} - L_{it})^2 \ + \lambda_L ||L||_* \qquad (7)$$

The predicted matrix $Y_{it}(0)$ is initialized to be the real $Y_{it}(0)$ matrix with the missing counterfactuals set to zero. The loss function, seen in equation (7), to obtain the estimated $Y_{it}(0)$.

The estimation of the matrix itself only has this one underlying assumption. This is basically an unconfoundedness assumption - essentially that the conditional independence assumption holds such that the treatment assignment and the potential outcomes are independent of each other conditional on observed covariates.

This matrix completion method acts as a test for robustness to the Callaway and Sant'anna estimator, however its performance is unclear as this dataset doesn't fit squarely into the matrix completions' usual use cases. When dealing with "fat" matrices containing more historical data points than observations, resembling a wide matrix, matrix completion resembles synthetic control estimators. It involves computing vertical weighted averages of the control group units to predict missing counterfactuals for each unsolved period. Conversely, in "thin" matrices like the MxFLS data with only three periods and numerous observations, matrix completion functions more like unconfoundedness. This involves regressing the final outcome onto lags and inputting values for the last period. However, unlike unconfoundedness models, some time points may have only one historical data point before the counterfactual matrix switches to Y(1). We can evaluate the matrix completion model and assess whether the scarcity of historical data can be compensated for by incorporating many covariates.

2x2 Difference-in-Differences

$$Y_{it} = \gamma_i + \lambda_t + \beta(i * t) + X_i + \varepsilon_{it} \qquad (8)$$

In addition to models that take into account data from multiple periods and staggered treatment implementations, I employ a simple 2x2 Difference-in-Differences to one, check robustness and make sure the previously estimated effects are going the right direction, and two, to estimate the shorter term effects of Seguro Popular. I subset the treatment group to be those working "informal jobs" whose municipalities had Seguro Popular rolled out to in between the first and second wave of the MxFLS surveys and the control being those working “informal jobs” whose municipalities which Seguro Popular wasn’t rolled out until after the second wave of the MxFLS. By using data from the before treatment period of 2002 and after treatment in 2005, I should be able to estimate the medium-term 1-3 year effects of Seguro Popular. In addition to this, I run a version of this model employing propensity score matching to make sure the composure of the characteristics of the treatment group and control group are similar.

Results

Labor Outcomes

The results of the effects of Seguro Popular on labor participation are presented in Tables 2, 3, and 4. We reject the null hypothesis that Seguro Popular has zero effect on total labor participation. In alignment with the economic theory from the labor-leisure microeconomic model, it appears that the implementation of Seguro Popular decreased overall total hours worked per year by all workers by 112.82 hours or roughly 2.2 hours per week. This downturn was especially drastic for males who saw a decrease in labor participation by 156.73 hours per year or roughly 3 hours per week. The breakdown is further broken down by different age and gender demographics. Labor participation decreases across the board for all groups except for women - the ITT treatment effect for women is a positive coefficient, albeit a statistically insignificant result. It’s unclear whether the treatment effect for women is not statistically different from zero as a result of a much lower sample size or a result of a truly null effect. It could possibly be that despite the income effect disincentivizing work, there is another effect in the opposite direction for women where more healthcare coverage makes women healthier and more inclined to stay in the labor force.

The difference-in-difference model with propensity score matching finds a similar magnitude decrease in labor participation, a drop of 78.06 hours per year overall, although this result is insignificant. However, in contrast to the CS estimator, there is a coefficient of 2.58 for males, unlike the steeper decline we see for male labor force participation earlier.

Family Planning Outcomes

I theorized that the advent of Seguro Popular would increase the amount of children that women want to have because the medical expenditures of having children is lessened with free or extremely subsidized healthcare. However, we find insufficient evidence to conclude that Seguro Popular had a discernible effect on the amount of children that women want to have. However, there is a positive coefficient that is produced: 0.11. While this increase of 0.11 doesn't seem that large, with a birth rate of 2.62 children per woman in Mexico in 2002, a 0.11 percentage point increase would represent a 4.3% increase in the birthrate. I find differences in the estimated effect of Seguro popular when subsetting women into two categories: women who were 15-38 in 2002 and women who were 39-55 in 2002. The former represents women who are at an age where they are more likely to reproduce. There is an estimated treatment effect of 0.23 and -0.08 for the former and latter group, respectively. This shows that Seguro Popular could've had a more drastic effect on 15-38 year olds' desire to have children than all women overall. Seguro Popular had a closer to zero effect for women that are at an age where they are less likely to have kids - the introduction of lower medical expenditures of having children doesn't affect their decision to have kids because they weren't going to have anymore kids anyways. There also appears to be a heterogeneity in treatment effect over time as seen in Figure 4. The magnitude of the coefficients, albeit insignificant, get more significant over time.

Similarly in the medium-term, the difference-in-differences with matching model finds positive but statistically insignificant ITT effect on children wanted. Similarly, the younger age group, 15-38, sees a higher magnitude coefficient.

Cancer Screening Takeup Outcomes

We find insufficient evidence to conclude that Seguro Popular had a significant effect on the proportion of women that have frequent pap smears. When further subsetting to the demographic where pap smears are recommended, 21+ year olds, there is still a null result on the effect of women taking up frequent pap smears. However, when subsetting breast exams to the demographic where breast exams are recommended, women beyond their mid-30s, there is a statistically significant 5 percentage point increase in the proportion of women that get frequent breast exams. With 37.7% of 35-55 year old women in this survey getting frequent breast exams in 2002, this 5 ppt increase represents a roughly 13% increase in 35-55 year old women who get frequent breast exams. While it's unclear why these two cancer screenings saw different treatment effects as a result of Seguro Popular, the differences in baseline takeup could play a role. A higher proportion of women who should get pap smears got pap smears in 2002: roughly 56% of 21+ year olds.

There again appears to be a heterogeneity in treatment effect over time as seen in Figure 6. The magnitude of the treatment effect on frequent breast exams get more significant over time.

The difference-in-difference estimator with matching finds statistically significant increases in both pap smear and breast exams interestingly.

Limitations

In order to assess whether an individual is a formal or informal worker, I use the questions: "Then, when you work as [...], are you a [...]?" where the options are: 1) Peasant on your plot; 2) Family worker in a household owned business, without remuneration; 3) Non-agricultural worker or employee; 4) Rural laborer, or land peon (agricultural worker); 5) Boss, employer, or business proprietor; 6) Self-employed worker (with or without non remunerated worker); 7) Worker without remuneration from a business or company that is not owned by the household. Those that selected either option 3 or 5, but not other options over the three waves of the study were categorized as "formal workers". Those that selected option 2, 4, 6, not options 3 or 5 were categorized as "informal workers" for the purposes of this analysis. While the most common informal roles include street vendors, which would fall under option six, and agricultural workers, which would fall under options one or four, many informal and formal jobs will be inevitably miscategorized for the other. An alternative to this approach that could be implemented in the future is using a question asking about which insurance plan people have. Individuals receiving IMSS and similar insurance would have to be formal workers as all formal workers are eligible for IMSS at no cost. Every other individual (excluding those with private insurance) can be assumed to be an informal worker.

Furthermore, there is not evidence for pre-trends conditional on covariates for labor outcomes which presents a possible violation of the parallel trends assumption. However, even if this data passed the pre-trend test as it did with cancer screening and wanted children outcomes, the lack of lengthy historical data doesn't make this pre-trends test a strong indicator of parallel trends. This weak identifying assumption weakens the causal inference of the presented results.

Conclusion

This paper aims to assess the effects of Seguro Popular on individuals labor force participation, family planning, and the take-up of womens' preventative cancer screenings. I find that the implementation of Seguro Popular decreases the labor force participation overall, and especially certain demographics such as males. I also find sizable but insignificant increases in children wanted as a result of Seguro Popular. While there was a statistically significant increase in breast exams takeup, I found no chance in pap smear take-up by women. While the extent of external validity of these results is debatable, it's crucial to consider the broader implications of these results, particularly in the context of developing countries that implement sudden universal healthcare programs. Many economic studies into the effects of Seguro Popular and similar universal healthcare programs only study its adverse or neutral effects on labor force participation. However, this distortion is only one piece of the pie when analyzing the costs and benefits of such a program. Other positive aspects such as those individual outcomes measured in this study need to be weighed against the potential drop in labor force participation. In future studies, I hope to improve the methodology of this study and track other individual outcomes that are often overlooked by the economic literature.

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Appendix

Table 1: Balance Check

	SP Rollout 2002-2005	SP Rollout 2006-2009
Household Demographics		
Household Head Female	0.20	0.20
SE	(0.006)	(0.007)
N	4342	2916
Household Size	4.33	4.41
SE	(0.03)	(0.04)
N	4342	2916
Household Children	1.70	1.82*
SE	(0.02)	(0.03)
N	4342	2916
Household in City (bigger 15k)	0.53	0.36*
SE	(0.008)	(0.009)
N	4342	2916
Economy		
Income	9803.04	7314.99*
SE	(196.81)	(180.70)
N	18790	12859
Household Head Income	42483.07	32274.67*
SE	(891.31)	(858.87)
N	4334	2914
Education		
Secondary	0.42	0.35*
SE	(0.004)	(0.004)
N	17359	11813
Highschool	0.18	0.13*
SE	(0.003)	(0.003)
N	17359	11813
College	0.07	0.05*
SE	(0.02)	(0.02)
N	17359	11813
Head Household Secondary	0.39	0.31*
SE	(0.007)	(0.009)
N	4315	2906
Head Household Highschool	0.19	0.14*
SE	(0.006)	(0.006)
N	4315	2906
Head Household College	0.09	0.07*
SE	(0.004)	(0.005)
N	4315	2906
Hospital		
HH Outpatient Visits last 4 wk	1.32	1.30
SE	(0.02)	(0.02)
N	1492	937
Distance to Clinic (min)	49.43	50.45
SE	(4.9)	(5.7)
N	1465	914

* indicates statistically significant difference at 5% level.

Figure 1: Propensity Score Distribution of All Workers

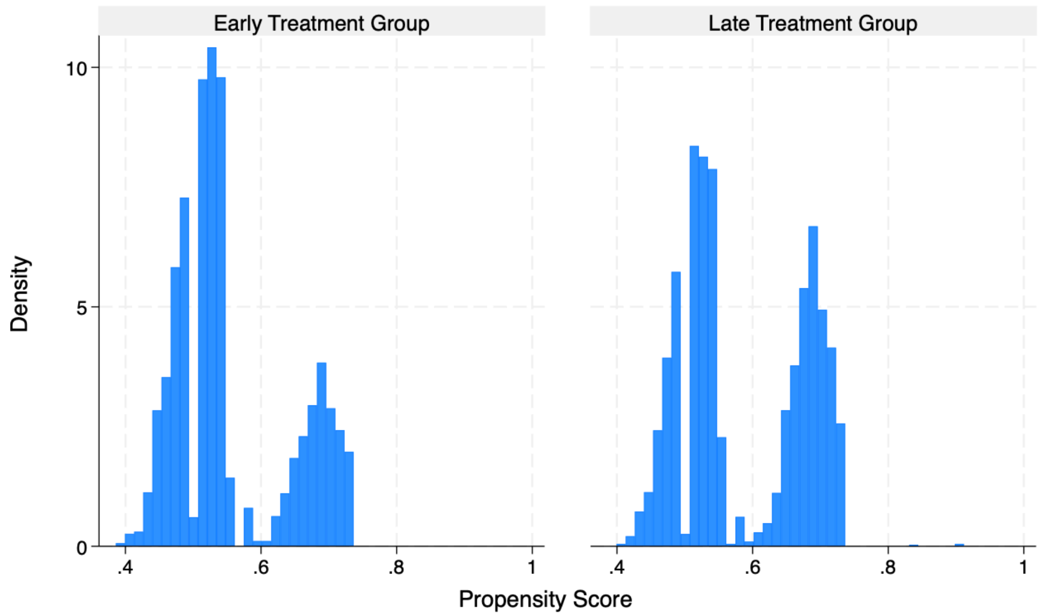

Figure 2: Propensity Score Distribution of Women

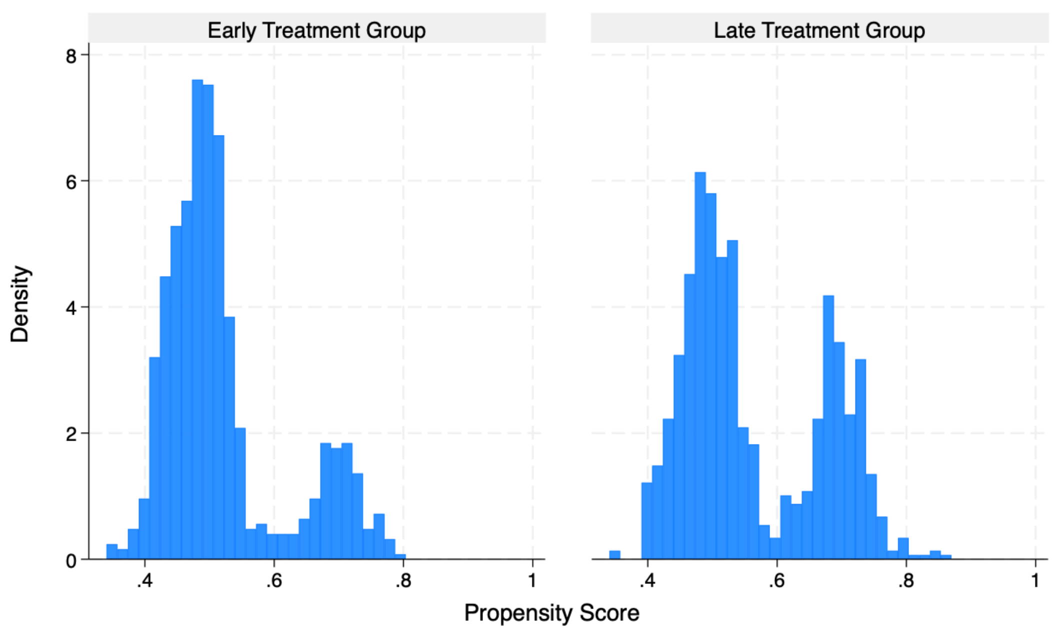

Table 2: Effect of Seguro Popular on Hours Worked per Year

	(1) CS Estimator	(2) MCNNR	Observations
All Jobs Total Worked	-112.82^{*}		11,113
	(51.01)		
Primary Job Worked	-84.65		11,113
	(48.17)		
Household Head	-91.13		5,568
	(-69.25)		
Non Household Head	-69.88		5,545
	(76.42)		
Male	-156.73^{***}		7,189
	(60.80)		
Female	35.76		3,924
	(97.27)		
Male Head of Household	-85.81		4,884
	(71.77)		
Female Head of Household	-116.59		684
	(345.39)		

Table 3: Effects of Seguro Popular on Hours Worked per Year

	(3) 2x2 DiD	(4) 2x2 DiD w/ Matching	Observations
Total Hours Worked	-77.09	-78.06	3,435/2,120
	52.95	53.11	
Primary	-75.33	-75.84	3,435/2,120
	51.041	51.01	
Household Head	-30	9.59	1,535/920
	80.00449	78.19	
Non Household Head	-104	-122.50^{*}	1,900/1,200
	67.47957	68.90	
Male	-23.12	2.58	2,261/1,440
	60.99	61.66	
Female	-102.79	-93.65	1,174/680
	95.18	96.04	
Male Head of Household	-37.49	4.57	1,372/829
	-24.55	80.21	
Female Head of Household	-13.60	151.07	163/91
	-138.50	283.11	

Table 4: Effect of Seguro Popular on Hours Worked per Year by Age and Gender

	(1) CS Estimator	(2) MCNNR	Observations
15-25 yr olds	-3.00		2,822
	(112.12)		
26-40 yr olds	-90.64		5,057
	(76.23)		
41-55 yr olds	-52.73		3,234
	(86.95)		
15-25 yr male	-54.46		1,772
	(133.85)		
26-40 yr male	-131.46		3,172
	(93.82)		
41-55 yr male	-101.73		2,245
	(98.52)		
15-25 yr female	39.00		1,050
	(187.30)		
26-40 yr female	40.25		1,885
	(133.79)		
41-55 yr female	150.25		989
	(192.61)		

Table 5: Effect of Seguro Popular on Children Wanted

	(1) CS Estimator	(2) MCNNR	Observations
All female	0.11		3,735
	(0.18)		
15-38 yr olds	0.23		2,294
	(0.22)		
39-55 yr olds	−0.08		1,099
	(0.33)		

Table 6: Effect of Seguro Popular on Children Wanted

	(3) DiD	(4) DiD w/ Matching	Observations
All Female	0.34^{**}	0.16	1,470/830
	0.15	0.16	
Age 15-38	0.38^{**}	0.14	758/375
	0.18	0.19	
Age 39-55	0.22	0.05	712/455
	0.23	0.24	

Table 7: Effect of Seguro Popular on Take-up of Frequent Womens' Cancer Preventative Screenings

	(1) CS Estimator	(2) MCNNR	Observations
Pap smear (all females)	−0.0052		6,753
	(0.0259)		
Pap smear (21-55 yr female)	−0.0182		6,297
	(0.0265)		
Mammogram (all female)	0.0229		9,132
	(0.0179)		
Mammogram (35-55 yr female)	0.0495^{*}		4,285
	(0.0286)		

Table 8: Effect of Seguro Popular on Take-up of Frequent Womens' Cancer Preventative Screenings

	(3) DiD	(4) DiD w/ Matching	Observations
Pap smear (all females)	0.0874^{***}	0.0853^{***}	2,288/1,309
	(0.0298)	(0.0297)	
Pap smear (21-55 yr female)	0.0920^{***}	0.09182^{***}	2,105/1,225
	(0.0307)	(0.0307)	
Mammogram (all female)	0.0224	0.0428^{***}	3,324/2,079
	(0.0209)	(0.0198)	
Mammogram (35-55 yr female)	0.0252	0.0556^{*}	1,675/1,137
	(0.0324)	(0.0329)	

Figure 3: Treatment Effect of Total Hours Worked by Period

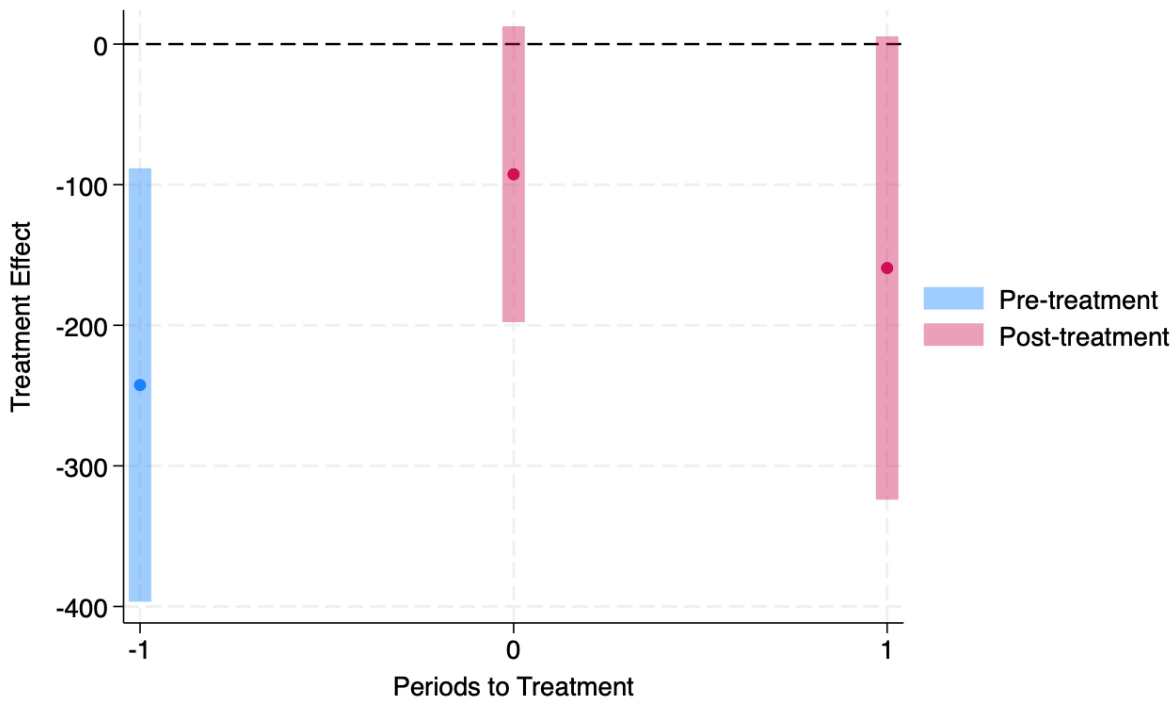

Figure 4: Treatment Effect of Kids Wanted by 15-38 Year-old Women by Period

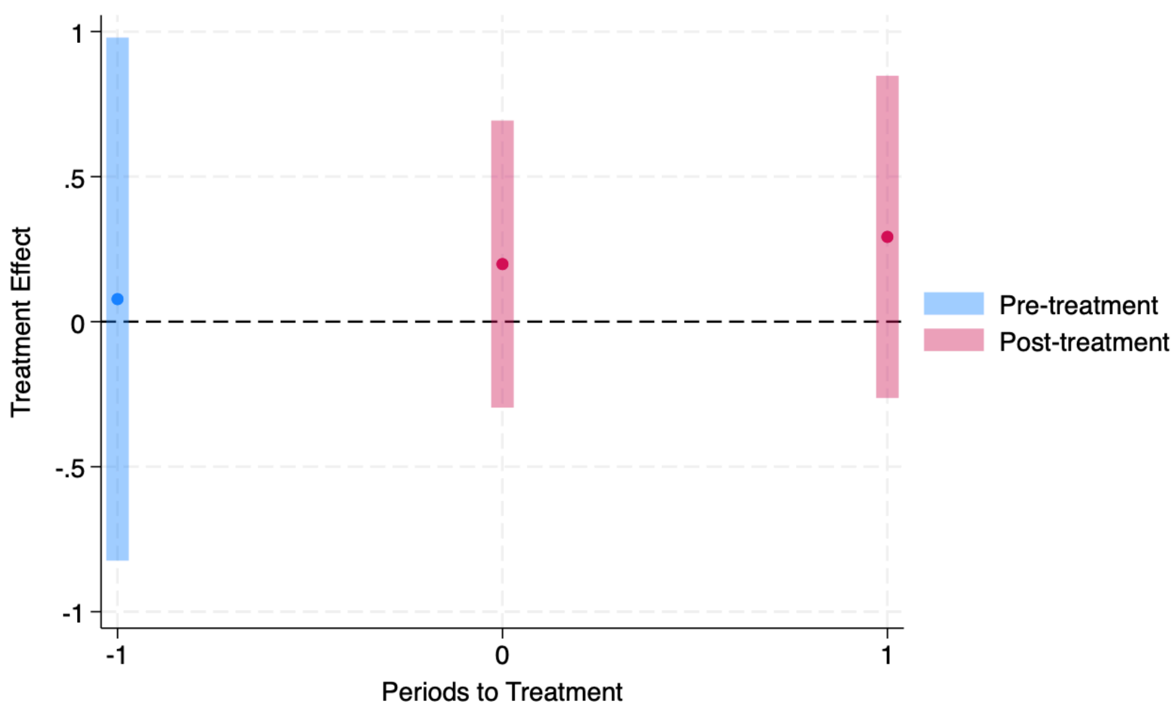

Figure 5: Treatment Effect of Frequent Pap Smears by 21-55 Year-old Women by Period

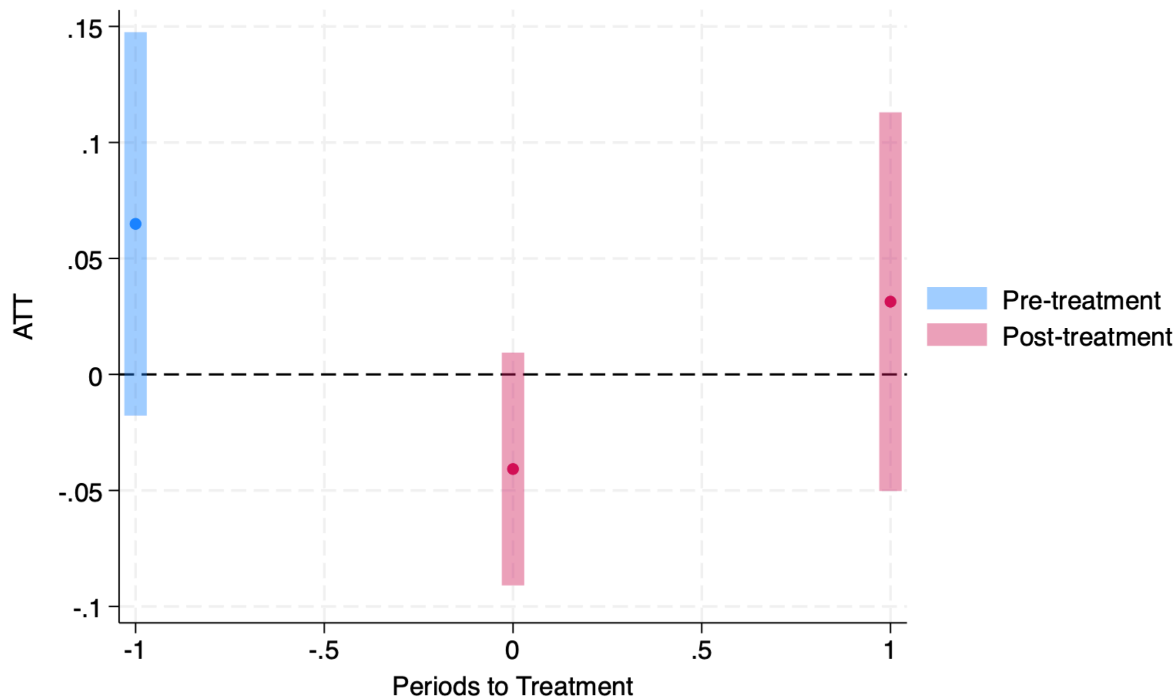

Figure 6: Treatment Effect of Frequent Breast Exams by 35-55 Year-old Women by Period