Universal Health Coverage for the Poor: A Cost-Benefit Analysis of Mexico's Seguro Popular

Jeremy Barofsky[1]

Abstract

Using unique experimental data, this paper compares the benefits and costs of Seguro Popular, Mexico's health insurance program for the poor. I find that insurance sharply reduced health spending, particularly at the right tail of the distribution, generating gains equal to 21% of program cost. A stylized utility model indicates that less exposure to health-spending risk produced enough welfare to cover almost a quarter of program cost. Overall utilization does not increase, while some types of utilization rise for those with chronic illness. Although these data suggest possible improvements in self-reported health, no beneficial effects are found for chronic diseases like obesity and diabetes. Analyzing supply, I find evidence for contraction in staffing levels and non-significant declines in other forms of health infrastructure. To explain these results, I note that Seguro Popular was under-funded compared to what its designers intended. These findings highlight both the benefits of health coverage and how supply-side responses can mitigate positive effects.

Keywords: health insurance; Mexico; cost-benefit analysis; Seguro Popular; H4, H5, I1, I3.

1. Introduction

In developing nations, health shocks represent a large and unpredictable form of income variability against which low income households cannot self

[1]jbarofsky@brookings.edu, Brookings Institution (Current Affiliation) and Harvard University, School of Public Health. I am indebted to David Canning, David Cutler, Amitabh Chandra, David Laibson, Jacob Bor, Tristan Zajonc, John Scott, and seminar participants at Harvard University, University of Southern California, and AcademyHealth for feedback and helpful comments. I would like to thank the generous funding from the Institute for Quantitative Social Science and the Weatherhead Center for International Affairs for supporting background research. I gratefully acknowledge Gary King for his helpful discussion and providing the data for this analysis. All errors are my own.

insure (Gertler and Gruber, 2002; Townsend, 1994). Even if consumption is maintained during illness, the welfare consequences can be large as families sell assets, borrow, or pull their children from school to finance health care (Chetty and Looney, 2006; Mohanan, 2013). Although health insurance coverage has grown substantially in the developing world since 2000, further expansion to achieve universal health coverage is increasingly advocated as an international development goal (Jamison, et al. 2013; World Health Organization, 2010).[2]

Multiple country evaluations and two recent reviews highlight the uneven evidence on whether, and under what circumstances, health insurance achieves the goals of financial risk protection and health improvement.[3] Acharya, et al (2012) summarizes that "[i]n general, we find no strong evidence of an impact on utilization, protection from financial risk, and health status." That is, mandating free or low cost health care coverage for the poor does not necessarily translate into the claimed benefits even though utilization in developing nations is often inefficiently low because of credit constraints.

In 2004, Mexico instituted a health reform called Seguro Popular to cover more than half the population that was uninsured. Before reform, unemployed or informal-sector Mexicans could access care in public sector health facilities with sliding scale out-of-pocket payments that exposed between 2 and 4 million households per year to catastrophic and impoverishing health spending (Frenk, et al. 2006). Mexico's reform was intended to provide financial risk protection against health shocks and reduce poverty caused by illness for the poor.

This paper investigates the impact of Seguro Popular using experimental data. The reform eliminated out-of-pocket payments to access care at public health facilities and was designed to shift financing to a pre-payment model. I calculate Seguro Popular's effect on out-of-pocket health and non-health expenditure, adjusting for time spent covered, as well as calculate the welfare effect from reduced exposure to health-shock risk. I take advantage of these

[2]The World Health Report 2010 defines universal health coverage as a health system in which "all people have access to services and do not suffer financial hardship paying for them" (WHO, 2010, ix). Health insurance therefore represents one policy tool to realize the goal of universal coverage. In this paper, I use health coverage and insurance synonymously for the sake of lexical variation.

[3]Evaluations of coverage expansions in Mexico (King et al., 2009), Colombia (Miller et al., 2013), Ghana (Powell-Jackson et al., 2014), India (Fan et al., 2012; Sood et al., 2014), and China (Wagstaff and Lindelow, 2008; Wang et al., 2009)), inter alia, are mixed on these metrics. See Giedion et al. (2013) and Acharya et al. (2012) for reviews.

panel data with measured health status to investigate effects on utilization and health overall and, for utilization, by baseline level of chronic disease. I also use a survey not previously analyzed on infrastructure to explore the program's effect on near-term supply. Finally, I combine these effects to compare program benefits against cost.

Overall, I find that Seguro Poplar produces benefits equal to 73% of program social cost, although benefits accruing to the intended recipients represent 44% of cost. These benefits come in approximately equal proportion from reduced out-of-pocket spending and the welfare benefit from less exposure to health-spending risk. Using national account data, I report that approximately 30% of program social cost is captured by the health system through higher wages and salaries. Insurance produces large effects on mean health spending, reducing out-of-pocket expenditure by 49% or 4.3% of non-health expenditure. Quantile effects reveal that health coverage produces a substantial reduction in the right-tail of the health-spending distribution. That is, insurance reduces spending at the 75th and 90th percentile by 25% and 71% of mean health expenditure, respectively. Mixed effects of insurance on health are found, where the number of chronic conditions reported, ability to perform daily activities, and depression show suggestive improvement, while clinical measures of health such as diabetes and obesity show (non-significant) increases. These health status effects are consistent with research, using these same experimental data, which finds that Seguro Popular reduced utilization of preventive care (Spenkuch, 2012). Insurance produced no overall increase in utilization, again if anything a marginal decline, although care increased for specific types of care among individuals likely to need it given baseline illness. Analyzing the supply side, I find that health facilities in areas with more coverage experienced no expansion, and contracted, at least in staffing levels. I suggest that the lack of utilization and supply response to health reform was driven by program underfunding and exacerbated by the loss of patient user fees not being replaced by federal transfers.

This paper extends a previous study on the effect of Seguro Popular using the same data (King et al., 2009), which found the program generated reductions in out-of-pocket spending, but no overall changes to utilization or health. My health spending results are qualitatively similar to those found by King et al. (2009). However, the treatment effects I find are uniformly larger because, instead of treating insurance enrollment as a binary variable, total household exposure to insurance is estimated. It is, to my knowledge, the only attempt in the health insurance literature to account for the fact that as the time and number of household members enrolled increases, these programs should produce larger benefits. In addition to this difference, King

et al. (2009) does not investigate changes to risk exposure nor changes to specific types of utilization using baseline health. This paper also investigates the effect of insurance on a much wider range of health status variables, that, although available in these data, were not presented in King et al. (2009).[4]

This paper provides multiple contributions. First, it extends the literature investigating the effect of health insurance on out-of-pocket spending and risk exposure in developing countries. Extensive observational and quasi-experimental evidence exists on these questions, but examples that control for confounders to identify the causal effect of coverage are rare. Most closely related to this study are experimental evaluations in Nicaragua (Thornton, et al., 2010) and Ghana (Powell-Jackson et al., 2014) in the developing world as well as the Oregon Health Insurance Experiment in the U.S. (Finkelstein et al., 2012; Baicker et al., 2013). Although not experimental, Limwattananon et al. (2015) studies the expenditure risk impact of Thailand's health insurance expansion, finding benefits equal to 80-200% of program deadweight loss. I find comparable effects, as Seguro Popular's gains from financial risk protection are 98% of deadweight loss. Second, these panel data with measured health status allow investigation of utilization for those with chronic disease. Given the epidemiological transition extending increasingly to middle- and low-middle income nations, understanding how health systems manage chronic disease will become essential to accomplish the most ambitious goals of universal coverage. Third, by analyzing the supply response to a primarily demand-side reform, this analysis is able to shed additional light on reasons why the elimination of user fees may not be sufficient to improve outcomes. It adds to the evidence that changes in utilization, and ultimately health, are highly determined by supply-side financial incentives. In Japan, Kondo and Shigeoka (2013) find that variable supply-side responses to insurance expansion reduce the system's ability to respond to new demand. In contrast, Miller et al. (2013) analyze the impact of Colombia's health reform for the poor and find large utilization effects with strong supply side

[4]Multiple, non-experimental studies have explored the impact of Seguro Popular, as well. An early evaluation found the program was associated with reduced catastrophic expenditure, increased utilization, and improved provision of preventive care (Gakidou et al. 2006). Galarraga et al. (2010) use variation in Seguro Popular's national roll-out applied to both the experimental data employed here and national data, finding reductions in out-of-pocket and catastrophic health spending similar to King et al. (2009). Barros (2008) also uses the national phased roll-out to estimate positive effects on health spending and small improvements in self-reported health. Additional non-experimental evaluations of Seguro Popular, which generally find positive effects on utilization, include Sosa-Rubi et al. (2009) which focuses specifically on utilization of obstetric services, and Bleich et al., (2007), which finds a 50% increase in the probability of receiving antihypertensive care.

incentives to provide care. Fourth, while not all program costs and benefits are included (and some are estimated from other research), this paper produces a comprehensive metric by which policymakers can compare this policy's benefits to other health and non-health options. Given the rapid expansion of these programs in the developing world and continued promotion of universal coverage as a development goal, this analysis extends the literature on what cost-benefit ratio health reforms can produce.[5] This paper also relates directly to Finkelstein and McKnight (2008), which, although not in the developing world, calculates Medicare's early impact compared to cost. They find that Medicare's risk exposure benefits constitute about 40% of program costs, larger than, but of the same order of magnitude as the 23% risk exposure benefit found for Seguro Popular.

The rest of this paper proceeds as follows: section 2 provides background on Mexico's health system and Seguro Popular, while defining the data set used. Section 3 explains how time spent covered is estimated and investigates the impact of Seguro Popular on mean out-of-pocket and non-health expenditure. Section 4 estimates quantile treatment effects and translates these changes in health shock risk into welfare benefits. Section 5 reports Seguro Popular's effects on utilization, health, and infrastructure, while section 6 combines aggregate program benefits and compares to costs. Section 7 concludes.

2. Background

2.1. Pre-reform

In Mexico, employment status determines health insurance eligibility. In 2000, 57% of the population was uninsured and without access to formal sector insurance. Consequently, as is common in the developing world, more than 50% of Mexico's health spending occurred out of pocket (OOP) (Pauly, et al., 2006; Frenk, 2006). Those with formal sector employment could access coverage through a vertically-integrated and separate system of hospitals, doctors, and clinics. Although called insurance, each of these institutions operate as a miniature health service.

Before Seguro Popular (SP) began, the uninsured population could pay OOP for private providers or access care through Mexico's Ministry of Health (MoH) facilities, where they were charged income-indexed user fees. The MoH was decentralized in the 1980s and 1990s, meaning that each state

[5]In addition, by tracking how much welfare $1 of spending generates, this exercise allows public programs to be judged based on the 'index fund' of development policy: giving directly to the poor (Blattman and Neihaus, 2014).

controls MoH infrastructure and human resources. In addition to charging user fees, services in MoH facilities were limited, of variable quality, and frequently unavailable (Lakin, 2010). Although it is possible that the uninsured received occasional free care from the MoH system, two empirical facts suggest that user fees faced by the uninsured created (and still create for the uninsured) large barriers to care. First, 12% of Mexican families faced either catastrophic or impoverishing health spending in 2002 (Knaul, 2006).[6] Second, about 50% of utilization in the SP-eligible population analyzed here occurs in public facilities.

2.2. Seguro Popular

Although piloted earlier, SP began its full-scale rollout in 2004. Designed to cover a specific set of medical interventions and medications that comprised 95% of Mexico's disease burden, reform allowed enrollees to access care at public hospitals and clinics without user fees (Frenk, 2006). To mimic Mexico's formal sector institutions, financing for SP was split between three sources: national and state governments and individual premiums. Initially, the system mandated premiums on a sliding scale with the first income quintile exempt. However, early on it became clear that enrollment would be prioritized over premium payment such that virtually no enrollees paid for coverage.[7] During its design phase, the variable cost of providing SP's guaranteed package of services was determined and divided among payers. In practice, because individuals pay no premiums and state funding was substantially less than expected, the program was "severely under-financed" (Lakin, 2010) compared to designers' intentions.[8] In addition, to incentivize higher quality care, federal funding transfers to state MoHs were determined by household enrollment. However, reform ultimately did not change the financial incentives faced by providers. Doctors and nurses remained salaried,

[6]Catastrophic expenditure is defined as health spending 30% or more of household subsistence income, while impoverishing refers to health spending that pushes households into or further into poverty.

[7]Two percent of all enrollees were paying any premium as of January 2010 (CNPSS, 2011) and by 2012, in partial recognition of this fact, the population exempt from premium payment was raised to the top two income quintiles.

[8]Lakin (2010) explains that because of state resistance to reform, the federal government made a series of concessions on mandated payments. Most importantly, states were allowed to count already allocated physical infrastructure funding as their SP contribution. In 2007, Lakin (2010) shows that only 9 of 32 states provided any liquid contributions to the program. He also reports that in 2006 and 2007, 88% and 92%, respectively, of state contributions intended to provide personal health services were instead credited from other state payments, primarily in physical infrastructure.

while facilities were not paid on a service nor per patient basis.[9]

2.3. Data

The experimental panel data used in this analysis includes a household baseline survey collected in 2005 and a follow-up in 2006. To collect these data, Gary King and colleagues divided Mexico into 12,000 health clusters that consisted of the population catchment area around a health facility (King et al., 2007). Health clusters were matched based on 38 demographic, socio-economic, health facility, and geographic characteristics. Once matched, 74 cluster pairs from 7 states were selected and one cluster from each pair was randomly assigned to treatment. Treatment clusters received SP affiliation offices, publicity campaigns educating the public on SP and its eligibility requirements, and government visits to villages in which officials would help with affiliation. In addition, treatment was to include expanded health facilities, medication access, and human resources to provide care. The control cluster in each pair received no additional services. During random assignment, 50 health cluster pairs were included in the baseline household survey (chosen based on similarity of match and likelihood of treatment compliance). Approximately ten months after the baseline survey, the follow-up household survey was implemented in the same 100 health clusters. A survey of health facilities was also conducted at baseline and follow-up for all 74 health cluster pairs included in the treatment.[10]

Table 1 shows the difference between the treatment and control health clusters among SP-eligible households at baseline on socio-demographic, expenditure, utilization, and health characteristics. No statistically significant differences are found between the groups. However, I observe that a high percentage of the study population resides in rural areas, that annual non-health expenditure is MX$35,300 (US$3209) and MX$36,265 (US$3297) in control and treatment clusters respectively, and that this sample has a high prevalence of obesity, hypertension, and diabetes. Table A.11 shows treatment compliance at baseline and follow-up by treatment and control areas. 52% of the eligible in treatment areas are enrolled at follow-up, while 8% of the eligible in control clusters are covered (SP enrollment is voluntary and those in control areas were not excluded, but incurred higher costs to enroll from information acquisition and travel). Take-up is larger in treatment ar-

[9] As designed, the reform intended to separate purchaser and provider functions, but in practice, formal contracting for services represented a small proportion of total services, although there is limited evidence that it has since expanded (Lakin, 2010).

[10] See King et al. (2007) and Imai et al. (2009) for a detailed explanation of the experimental design and additional validation of the randomization.

eas than control areas at baseline because, according to study designers, the baseline survey was executed simultaneously with treatment such that some clusters had short-lived access to SP before baseline.[11]

3. Expenditure Effects

3.1. Empirical Methods

The household survey asks separately about spending in the last three months on inpatient and outpatient care, medications, medical devices, and other health spending categories (imaging and tests, dental, traditional, and any other). These questions are combined and annualized to obtain out-of-pocket health spending used throughout.[12]

Using the cluster randomized experiment, effects are estimated in multiple ways. First, the intent-to-treat (ITT) effect is defined as:

$$Y_{ic,t=2} = \beta_0 + \beta_1 Z_c + \beta_2 V_{ic,t=1} + \epsilon_{ic,t=2} \tag{1}$$

where β_1 represents the ITT effect of residence in treated health clusters for outcome Y on household i, in health cluster c using follow-up survey data, where $t = 1$ and $t = 2$ refer to the baseline and follow-up surveys, respectively. Z_c is an indicator for residence in a treatment cluster. $V_{ic,t=1}$ represents a set of covariates for each household i at baseline and is not included in the main results.[13]

Ultimately, we are interested in the impact of coverage (the treatment effect on the treated, TOT), not the effect of residence in treatment clusters. In this context, a close approximation to the TOT is the local average treatment effect (LATE), which produces causal effects while accounting for treatment noncompliance.[14] Using residence in a treatment area to instru-

[11]This pre-baseline coverage exposure will bias results toward the null and indeed SP's mean effects are larger when the high-baseline enrollment clusters are removed. However, mean effects do not differ in a statistically significant way from the results I present.

[12]This 8 question measure of spending differs from King et al. (2009)'s use of a one item summary question on health spending and produces larger estimates of health spending. A question is asked on insurance premium spending, but only 1% of respondents report any spending in this category, so this question is excluded.

[13]Adding $V_{ic,t=1}$ to equation 1 is unnecessary to produce an unbiased estimate of the relationship between residence in the treatment area and the outcome, but may increase precision of the treatment effect. In the appendix, regressions including $V_{ic,t=1}$ for OOP spending are displayed in table E.13 and, reassuringly, treatment effect estimates do not differ much from those found in table 2.

[14]The LATE is only identified for a subpopulation, called compliers, for whom treatment assignment induces insurance enrollment. Since the TOT represents the weighted

ment for uptake, the effect of coverage is estimated using the following system of equations:

$$D_{ic,t=2} = \delta_0 + \delta_1 Z_c + \delta_3 V_{ic,t=2} + \upsilon_{ic,t=2} \tag{2}$$

$$Y_{ic,t=2} = \pi_0 + \pi_1 Z_c + \pi_3 D_{ic,t=2} + \pi_4 V_{ic} + \mu_{ic,t=2} \tag{3}$$

In addition to the above analysis, there are (non-significant) differences at baseline between treatment and control areas for OOP and non-health expenditure. To control for these, equations (1) and (2)-(3) are also run as differences-in-differences (see Appendix B for specifications).

3.2. Measuring Coverage Time

Generally, studies that investigate the impact of health insurance ignore variation in the time households are exposed to coverage and the number of household members enrolled. However, we would expect the effect of coverage to change as time spent insured rises, irrespective of how we model the arrival of health shocks. Ignoring time insured implicitly assumes that each individual enrolls immediately after the baseline survey and therefore systematically underestimates treatment effects.

To estimate each household's cumulative time with coverage, I use the fact that the date of each household's baseline and follow-up survey is collected. That is, interval censored data on the timing of affiliation is available. The coverage interval is set based on whether enrollment was observed at baseline or between baseline and follow-up. A duration model with an exponential hazard rate is then employed that conditions on baseline characteristics (residence in the treatment area, baseline knowledge of SP, and state of residence) to estimate an enrollment hazard by household. Once a predicted hazard rate of enrollment is obtained, Monte Carlo simulation is used to obtain predicted enrollment time. First, define $S(X,t)$ as the probability of being uninsured for a given covariate group X at time $t \in [0,1]$ for baseline and follow-up, respectively. Survival probabilities are randomly drawn 1000 times in the interval $[\ S(X,0), S(X,1)\ ]$, and since each draw

average of the LATE and treatment effect for always takers (households that would enroll irrespective of treatment assignment), the LATE approximates the TOT when the number of always-takers is small, while these quantities would be equal if no take-up in the control group occurred. As mentioned, 8% of the SP-eligible are always takers. Although the treatment effect among always takers could differ substantially from the LATE, Angrist (2004) simulates the relationship between the LATE and the TOT, showing that for any first-stage, selection on gains makes the TOT larger than the LATE such that the LATE represents a lower bound estimate of the TOT.

implies a separate household coverage time, the median predicted time from these draws is used. Appendix C provides a detailed explanation of how coverage time is estimated and combined with the number of enrolled members to create an index of annual full-household coverage.[15]

Applying this method, I estimate that the median number of days enrolled in insurance among enrollees is 263 with an interquartile range between 236 and 308 days. Figure C.4 shows the bimodal distribution of estimated enrollment time with peaks around 10.5 and 7.5 months. Data on the number of enrolled members at baseline and follow-up are then combined with estimated coverage time to generate an index of what proportion of a year the entire household was covered.

3.3. Mean OOP Expenditure

The effect of coverage on mean OOP expenditure is estimated using equations (1) and (B.1) to find the difference in outcomes between treatment and control areas using a one-period and a differences-in-differences specification, respectively. The effect for those induced to take-up coverage is also measured both using a one-period and differences-in-differences model from equations (2)-(3) and (B.2)-(B.3), respectively. These equations are also run using binary health coverage and estimated coverage time as the endogenous variable.

Table 2 shows the impact of insurance on OOP health expenditure and its components (inpatient, outpatient, drugs, medical devices, and other medical spending). The preferred specification is column (7), which uses annual full-household coverage time as the endogenous variable and controls for baseline spending differences. This specification shows that the mean decline in OOP health expenditure produced by SP is MX$1509, which represents 49% and 4.3% of OOP and non-health expenditure, respectively. These results also indicate that insurance produced statistically significant declines for inpatient and outpatient health spending equal to 99% and 122%, respectively. This is possible because although mean spending was approximately MX$3000, 38% of households incur no health spending at all, while households in the 90th percentile spent MX$8000 out of pocket. The largest component of health spending, drug expenditure, which comprised 40% of OOP payments, decreased because of insurance, but the null hypothesis of no change cannot be rejected.

[15]Thanks go to Jacob Bor for developing this method to estimate coverage time.

3.4. Mean Non-Health Expenditure

Table 3 shows how health coverage affects non-health spending and its components, while controlling for baseline covariates V_{ict} to reduce measurement error, although results without V_{ict} are substantially similar.[16] An overall (non-significant) mean increase in non-health spending is found, but this effect declines once higher baseline non-health spending is controlled for (comparing columns 2-4 to 5-7). Since the household survey does not measure savings directly, we cannot verify if the rest of the reduction in OOP spending from insurance goes to increased savings. Of the components of non-health spending, only housing shows a statistically significant increase, which may represent a form of saving for this low-income population. However, we generally find no portfolio choice effects, potentially because, as suggested by Miller et al. (2013), coverage does not provide full insurance.

4. Quantile Effects and Risk Exposure

4.1. OOP Expenditure Quantile Effects

Next, I turn to characterizing the effects of health coverage by quantile. The empirical distribution of OOP health spending is highly skewed with a mean of MX$3082 and a median of MX$600, while almost 40% of households spend zero. Therefore, investigating mean effects misses important characteristics of how coverage affects the OOP expenditure distribution. Residence in treatment clusters is used as an exogenous variable to instrument for health coverage with treatment noncompliance. To calculate the quantile treatment effect (QTE), nonparametric methods from Abadie (2002) are applied to recover the cumulative distribution of OOP medical spending with and without health coverage. If a set of nonparametric restrictions are fulfilled,[17] then Abadie (2002)'s instrumental variable (IV) model causally identifies the QTE for compliers. The LATE by quantile, $\Delta\tau_{\theta,\ IV}$, represents the difference between health expenditure distributions with and without insurance for those induced to enroll in insurance by the treatment. This approximates the treatment effect of coverage on the treated, as explained with mean effects, and represents a treatment effect lower bound with selection on gains.

[16] Tables E.14 and E.15 in the appendix display non-health expenditure effects that, respectively, exclude control covariates and both exclude control covariates and use the same sample size for all regressions so that component effects sum to the total non-health spending effect.

[17] Assumptions include independence of the instrument, first-stage validity, and monotonicity (such that there are no households that would defy their treatment assignment). The nonparametric equations (5) and (6) from Abadie (2002) are used here to estimate complier CDFs.

After obtaining $\Delta\tau_{\theta,\ IV}$, this change in health shock risk is translated into welfare terms by calculating risk premium changes (the amount a household would pay to completely avoid a given level of uncertainty) using a stylized utility model.

One notable difference between quantile and mean effects is that although $\Delta\tau_{\theta,\ IV}$ provides information on the difference between distributions with and without treatment, it does not provide any information on how a specific household fares under treatment. That is, households could move up and down on the OOP health spending distribution and QTEs cannot tell us how specific households move between policy regimes. However, because this evaluation is done from the social planner perspective, the household effect is not of first order importance.[18]

Figure 1 shows the household CDFs of OOP health expenditure for compliers in treatment and control areas at follow-up. Values shown are constrained to under MX$50,000, which represents 98% of OOP health expenditure distribution. For all values of medical expenditure, the probability of experiencing a given health spending shock is lower for compliers in treatment clusters compared to control and suggests that the treatment CDF stochastically dominates the control. Figure 2 summarizes the effect of coverage on yearly OOP spending by quantile by plotting $\Delta\tau_{\theta,\ IV}$ for $\theta \in [0.4, 0.99]$. The results show sharply increasing treatment effects toward the right tail of the OOP distribution. While most treatment effects are small in the middle of the distribution, the QTEs at the 75th and 90th percentiles represent 25% (MX$780) and 71% (MX$2180) of mean OOP spending, respectively. Standard errors are block bootstrapped by health-cluster pair with 500 repetitions.[19]

4.2. Welfare Effect of Reduced Risk Exposure

Given that health coverage reduces OOP expenditure, particularly for the largest costs, risk averse households benefit from greater financial protection against these payments. Here, I translate this risk reduction into welfare terms using a stylized utility model (Finkelstein and McKnight, 2008; Engelhardt and Gruber, 2010; Shigeoka, 2014; Limwattananon et al., 2015). It is

[18]Another interpretation is that this approach evaluates the impact of coverage from behind the veil of ignorance, where a given household does not yet know their a priori position on the OOP distribution.

[19]Both nonparametric methods from Abadie (2002) and parametric methods from Abadie, Angrist, and Imbens (2002) are applied to calculate QTEs, producing similar results. For computational simplicity, the latter is estimator used to calculate QTEs and bootstrap for figure 2 with a function written by Frolich and Melly (2008).

assumed that households satisfy a per period budget constraint of $c = y - m$ where y represents income, m OOP health spending, c non-health expenditure, and consume with a constant relative risk aversion (CRRA) utility function $u(c) = \frac{c^{1-\rho}}{1-\rho}$. The coefficient of relative risk aversion, ρ, is set to 2.5.[20] The assumption that budget constraints are satisfied each period such that households cannot borrow to pay m is strong. However, evidence suggests consumption smoothing in this population is limited. The survey asks a series of questions on how households finance health care and, at baseline, 13%, 12%, and 28% of households with nonzero medical spending used savings, sold goods, and obtained loans, respectively, to cover health care costs in the last year.

The density of m is defined as $P_k(m)$ with support $[0, \overline{m}]$ where k indexes the policy regime [0,1] without and with health coverage, respectively. This shift from $P_0(m)$ to $P_1(m)$ after treatment determines the risk exposure effect of coverage. Household expected utility can be calculated as:

$$EU[\ y\ ,\ \gamma\ ,\ P_k(m)\ ] = \int_0^{\overline{m}} u(\ max[\ y - m, \gamma y\ ]\ )P_k(m)dm \tag{4}$$

where γ represents an assumed minimum consumption value under which household expenditure does not fall. The risk premium π, to be defined below, represents the quantity of money a risk-averse household would be willing to pay to completely insure against a given risk distribution. The difference between π_0 and π_1, $\Delta\pi$, monetizes the change in risk exposure. A reduction in the risk premium after the imposition of health coverage means reduced willingness to pay to avoid risk, thereby valuing financial risk protection from health shocks.

Health care is luxury good such that low income households will not be subject to the same size health shocks as their relatively wealthy counterparts. For this reason, OOP expenditure distributions with and without health coverage are estimated by expenditure tercile and indexed by $t \in [1, 3]$ as $P_{t,k}(m)$. The consumption floor, γ, is estimated by examining the budget share of OOP spending at the tail of the health spending distribution. The 95th, 96th, and 97th percentile of the distribution of OOP health expenditure

[20]Disagreement on the magnitude of risk aversion persists. Chetty (2006) uses labor supply elasticities to estimate risk aversion, finding a mean coefficient of relative risk aversion of 1 (log utility) and an upper bound of 2. Other estimates find greater variation and higher risk aversion coefficients (Meyer and Meyer, 2005). ρ is varied between 1 and 5 in robustness checks. It is also worth noting that although we did not observe expenditure portfolio effects, additional welfare loss from ex-ante risk mitigation is not taken into account, which would increase the measured benefits of risk reduction.

as a share of non-health expenditure for bottom tercile households is 64%, 75%, and 91% respectively. Consequently, and for consistency with previous analyses (Finkelstein and McKnight, 2008; Limwattananon et al., 2015), a base case expenditure floor of $\gamma = 0.2$ is chosen, but varied between 0.2 and 0.4 in robustness. Therefore, π is calculated as:

$$\pi_k = [\ E_k(y-m) - CE_k\ ] =$$

$$\{\sum_{m=0}^{\overline{m}} max(\ y-m\ ,\ 0.2y\ )P_{t,k}(m)\} - \{u^{-1}[\sum_{m=0}^{\overline{m}} u(\ max(\ y-m\ ,\ 0.2y\ ))P_{t,k}(m)]\}$$

where $E_k(y-m)$ represents the expected value of a household's non-health expenditure and CE_k represents the same household's certainty equivalent under a given risk distribution.[21]

The nonparametric IV quantile regression employed to calculate $P_0(m)$ and $P_1(m)$ permit estimation with binary variables, but are not defined with continuous endogenous variables. Consequently, I use the binary measure of insurance enrollment to estimate quantile effects. Then, I scale these results by calculating the ratio of the LATE using binary insurance divided by the LATE using cumulative household coverage time. That is, I scale the risk-premium effect by the ratio of columns 7 to 6 from table 2. Implicit in this operation is the assumption that the ratio of mean effects between continuous insurance exposure and binary insurance coverage can be linearly scaled throughout the health expenditure distribution.

After the effect of insurance on OOP spending by quantile is found and this change in uncertainty is translated into welfare terms, I vary assumptions on the level of risk aversion ρ and the level of consumption smoothing γ. Figure 3 summarizes the change in risk premium $\Delta\pi$ as a percent of program social cost (to be defined below) as ρ varies from 1 to 5 and γ from 20% to 40% of household expenditure. In general, it shows that the welfare benefit of health insurance increases as risk aversion rises or if less consumption

[21]The standard definition of risk premium, as the difference between the expected value of a given risk distribution and its certainty equivalent, is used here. While, the certainty equivalent of a gamble represents the minimum amount of consumption c for which an individual is willing to accept the gamble instead of c for certain. This differs from the definition used in other analyses (Finkelstein and McKnight, 2008; Shigeoka, 2014) where π is defined as $y - CE = y - u^{-1}[\sum_{m=0}^{\overline{m}} u(\ y-m\ )P(m)]$. This definition, in the case of a degenerate risk distribution but $m > 0$, implies a willingness to pay to avoid risk when no risk exists.

smoothing is possible.[22] The paper reports values of $\Delta\pi$ using a coefficient of relative risk aversion of 2.5 and a minimum expenditure threshold of 20%. This implies that health coverage produces a welfare benefit from reduced exposure to health spending risk equal to 13% of program costs. Then, the ratio between mean effects is used to scale up the effect and obtain an estimate that the risk exposure benefit of SP is equal to 23% of program social cost.

4.3. Non-Health Expenditure Quantile Effects

I also calculate the change in non-health expenditure from health coverage for eligible compliers by quantile. As with OOP quantile effects, confidence intervals are obtained through matched health-cluster pair block bootstrapping with 500 repetitions. Figure E.5 shows that health coverage increases non-health expenditure for households until the 40th percentile of spending. In the lowest two deciles, these increases are large in percentage terms, ranging from 13% of control area non-health expenditure for the 5th quantile to 5.9% for the 20th quantile. These effects have confidence intervals that extend below zero and as such constitute suggestive evidence that the reductions in OOP spending reduce consumption poverty. Moreover, there is a clear pattern in these imprecisely estimated quantile effects, showing the greatest gains at the lowest spending quantiles.

5. Health, Utilization, and Infrastructure

5.1. Utilization Effects

King et al. (2009) examine changes in utilization and find no effects for inpatient, outpatient, and preventive care with negative (non-significant) point estimates. I extend this analysis by running equations (1) and (2)-(3), the latter using both binary and coverage time endogenous variables. I also extend King et al.'s (2009) analysis by investigating changes in utilization for respondents with measured diabetes, obesity, and hypertension at baseline. These characteristics are chosen because chronic diseases, particularly diabetes, represent a large and growing proportion of Mexico's disease burden and these utilization data contain sufficient detail to identify specific types of care likely to benefit those with baseline chronic disease.

Table 4 shows how various measures of overall utilization change because of the introduction of health coverage. Although no effects are significant at

[22]When minimum $C = 20\%$, $\Delta\pi$ peaks at $\rho = 4$ and then declines slightly again until $\rho = 5$. However, given the γ constraint on minimum expenditure, we would not necessarily expect to see completely monotonic increases in risk premia as ρ increases.

the 5% level, large percentage declines are observed over multiple types of utilization, including non-significant declines of 19.6% and 15.4% in the number of inpatient and outpatient visits attributable to one year of household insurance exposure. Two specific types of utilization show non-significant increases however; inpatient surgeries and inpatient birth admissions. Inpatient surgeries expand such that the ITT represents a 15% increase from the treatment and the LATE from one year of household insurance exposure implies a 62% rise in surgeries. Hospital births increase by 15% and 57% for the ITT and LATE with full household coverage, respectively, with significance at the 10% level. This result is consistent with lower out-of-pocket costs raising demand among the poor for these services in a previously credit constrained environment. This imprecisely-measured utilization increase could be inefficient if driven by ex post moral hazard, but these two types of care have a relatively low likelihood of contraindication.

Health coverage may not produce overall utilization changes that can be easily differentiated from noise in this experiment. Yet, the specificity of these data also allow a test of whether those with baseline chronic disease exhibit utilization changes for care particularly likely to benefit them. Specifically, table 5 investigates the effect of insurance on birth-related utilization for those with baseline obesity (a risk factor for birth complications) as well as hypertension- and diabetes-related care for those with high blood pressure and diabetes at baseline, respectively.[23] Each baseline disease condition raises the marginal benefit of the type of care analyzed. Table 5 shows differences in the number of visits between treatment and control clusters with each row displaying model results from an OLS, Poisson event count, and OLS with binary utilization indicator, respectively. In contrast to the overall utilization effects, outpatient birth utilization among the obese increases by 82% with statistical significance at the 5% level, while the likelihood of any outpatient birth utilization among those with baseline obesity increases by 11%. Inpatient birth utilization among those with baseline obesity increases by 46% with statistical significance at the 10% level. A 33% increase, significant at the 10% level, in hypertension-related utilization among those with high blood pressure at baseline is also observed. No increase and negative point estimates are found for the effect of insurance on outpatient diabetes-related utilization among those with baseline diabetes.

In addition, tables F.16, F.17, F.18 show ITT estimates of the effect of insurance on utilization overall and then for those with baseline diabetes,

[23]Birth-, hypertension- and diabetes-related utilization are identified by asking survey respondents the primary reason for seeking each instance of care.

obesity, and hypertension. Still, we observe that overall utilization exhibits non-significant reductions in inpatient stays, inpatient days, and outpatient visits. Consistent with the results from the targeted analysis above on diabetes, table F.16 shows marginally significant declines in inpatient visits for those with baseline diabetes. Tables F.19 and F.21 shows the positive utilization effects are not seen for all utilization types given baseline health conditions.

5.2. Health Effects

King et al. (2009) also analyzes the effect of insurance on self-reported health outcomes, finding no overall effect when controlling for baseline differences, and I extend that analysis here by including a wider range of self-reported and objectively-measured health status variables. The measures of subjective health status investigated include self-reported health, number of self-reported chronic conditions (asthma, arthritis, and angina). In addition, I create an Activities of Daily Living (ADL) score from 17 questions on a respondent's mobility, personal care, cognition, pain / well-being, and affect. Objectively-measured health status includes obesity, diabetes (for a random half of respondents), hyperglycemia, hypercholesterolemia, and hypertension, all of which are measured both during baseline and follow-up.[24] Treatment effects are estimated using the ITT and LATE with household exposure for the follow-up survey and all of these estimators are also used while controlling for baseline differences.

Table 7 reports the mean prevalence of and health coverage's effect on self-reported health status. Without controlling for baseline differences, self-reported health exhibits improving point estimates for number of reported chronic conditions, depression, and ADL score (with significance at 5% levels). When controlling for baseline differences, the significant improvement in ADL disappears, although all point estimates point in the direction of improving health. However, consistent with results from the expansion of Medicaid in the U.S. (Baicker et al., 2013), non-significant improvements in depression are observed that, even for the intent-to-treat, represent an almost 14% decline.

Table 8 shows that, whether or not differences at baseline are controlled for, there is no impact of insurance on objective health measures at standard significance levels. Moreover, each of the point estimates are pointing toward worsening health, except for hypertension. Given the cluster-randomization, the point estimates imply large changes in health status,

[24]For additional background and health variable definitions, see appendix D.

such as the marginally significant increase in obesity where the ITT estimate implies a 9.7% increase. Diabetes prevalence, identified by measuring glycated hemoglobin (HbA1c),[25] in this population is very high, observed in 19% of respondents at baseline. In addition, I combine the components of self-reported and objectively-measured health status in tables 7 and 8 separately using the average standardized treatment effect (ASTE) approach (see Spenkuch, 2012 and Kling et al., 2007) and find no statistical significance (results not shown).[26] Although none of these results are significant at conventional levels, they indicate that, at best, insurance did not improve chronic disease health status over this approximately ten month follow-up period.

5.3. Infrastructure Effects

I also investigate the impact of health coverage on the supply side using a previously unanalyzed health facilities survey. Although the household survey was implemented in 100 health clusters, the experiment itself was rolled out in 144 clusters nationwide and health infrastructure in each was measured as well. Table 6 shows the mean difference between treatment and control clusters at baseline, follow-up, and the differences-in-differences estimate of change in health infrastructure between treatment and control clusters. Table 6 indicates that the availability of health facility infrastructure contracted, at least over the short term, in response to the health coverage expansion. I observe a 31% reduction in staff availability (doctors and nurses) in treatment areas compared to control, with statistical significance. Substantial, non-significant, percentage declines in office and bed availability are also observed.

6. Cost-Benefit Analysis

In this section, I explain how the social costs of SP are calculated, using both government national accounts and other literature, and compare them to program benefits. Benefits include welfare that risk averse individuals receive from decreased risk exposure and lower OOP health spending. One of the largest potential benefits from insurance is better health. However, at least over the approximately ten month follow-up period in this survey,

[25]HbA1c level is a measure of average blood glucose over the last one to three months and diabetes is defined as HbA1c $\geq$ 6.5%.

[26]In contrast, Spenkuch (2012) combines multiple measures of preventive care utilization, all with negative point estimates, using the ASTE and finds a statistically significant decline in preventive utilization from insurance.

I interpret the health changes described above as an imprecisely measured null effect. In addition, some of the increase in funding allocated for SP was retained by the health system in the form of greater wages and benefits. Although this could have translated into improved quality or more service, any impact should be detectable through utilization and health status. Health system resource capture is interpreted as a social benefit, but one that does not accrue to the program's intended beneficiaries. Costs include direct government spending on health coverage, the social cost from the deadweight loss of raising this funding through general revenue, ex-post or ex-ante moral hazard, and crowd-out of private insurance at the coverage and utilization margin. Another potential cost of expanded health coverage comes from incentives to work in the informal sector or reduce labor supply (Levy, 2008).

6.1. Crowd-out

Public health coverage can replace private or formal-sector coverage and care, reducing program benefits. Multiple papers have explored the extensive margin, whether SP supplants formal-sector coverage, and find evidence for either no or a small effect using SP's nationwide rollout for identification (Arias et al., 2010; Barros, 2008; Aterido et al., 2011; Azuara and Marinescu, 2013). However, another source of inefficiency from government coverage could come from private utilization being replaced with lower quality public care. The household survey allows investigation of this question because in addition to measuring utilization, it also asks facility type for each household's most recent utilization event in the last year. Table 9 shows the extent to which different types of health care utilization change by sector between the baseline and follow-up surveys. Each row shows the proportion of utilization that occurs between MoH government facilities that do not accept SP, MoH facilities that accept SP, formal sector, private, and other facilities. Table 9 demonstrates that the only statistically and economically significant change in utilization occurs between facilities that accept SP compared to those that do not. Given other literature and these findings on the lack of utilization shift, I estimate the social cost of SP switching into the informal sector as null.[27]

[27]Given that employment determines coverage eligibility in Mexico, the effect of expanded health coverage on labor supply is closely related to crowd out. Appendix G describes the literature, while table G.22 shows the limited evidence from this survey on SP's labor supply effect, finding null results as well.

6.2. Moral Hazard

Given the overall null or slight decrease in utilization found and the focused, limited increases among those with baseline chronic conditions, the foregoing analysis does not provide evidence for ex-post moral hazard. Utilizing the same data evaluated here, Spenkuch (2012) finds that SP reduces the use of preventive care like mammograms, pap smears, and eye exams and interprets this decrease as evidence for ex-ante moral hazard. However, given my finding of a limited supply contraction, I advance a different explanation for the marginally significant, but economically large decline in preventive care observed. Instead, I posit that the marginal decline in preventive care reflects the broader, but marginal utilization decrease driven by an inward shift in the supply curve that counteracted any demand expansion. I argue that this inward shift in turn was caused by a combination of health facilities' weak financial incentives to increase care and their concurrent loss of liquid user fees after reform.

6.3. Costs

When it comes to direct costs, approximately $MX8.5 billion ($US780 million) and $MX16.78 billion ($US1.53 billion) were spent on SP in 2005 and 2006 respectively (Secretaria de Hacienda, 2010). This translates into an average of $MX5511 ($US 501) spent per affiliated household in 2005-2006 and constitutes the cost per household benchmark against which I compare benefits. The number of enrolled households is estimated from a nationally-representative survey that was implemented in late 2005 and early 2006 called the National Health and Nutrition Survey (Encuesta Nacional de Salud y Nutricion or ENSANut). According to ENSANut, 2.3 million households were affiliated to SP during this time. Indirect costs include the deadweight loss from taxes used to raise funds for the program. In the U.S., the consensus estimate on the marginal cost of raising public funds is 0.3 (Poterba, 1996) and I apply this estimate to Mexico, as a reasonable approximation, acknowledging that the percentage is likely higher, but that more specific information to provide an upper bound is unavailable. This implies that the annual cost of raising these funds was 0.3 * SP costs per household in 2005-06, which comes to $MX1653 per affiliated household. Therefore, the total social cost of health coverage was MX$7164 (US$648) per household.

Using Mexico's MoH national accounts system, I also explore how SP funding transferred to states was spent in 2005 and 2006 (SINAIS, 2013).[28]

[28]Federal transfers to states in 2006 represented 71% of total MoH spending on SP. Source: SINAIS (2006). Gasto en salud por funciones del sistema de salud de los Servicios Estatales de Salud, 2006, Cuadra I.1.

These accounts show that the second largest line item of SP spending from federal transfers in 2006 was wages and salaries at 38.7% of total federal transfers.[29] They indicate that a significant amount of the resources intended to provide care for low-income workers was captured by the health system. In contrast, in 2006, 8.7% of federal transfers were spent on rehabilitation and construction of physical infrastructure, while 5.3% of SP transfers went toward medical equipment for health facilities. This estimate of medical-system capture is consistent with outside evaluations of the political economy around SP, which reported that over 30% of new SP funds were used on personnel, instead of service provision and in some states the amount was as high as 45% of new resources (Lakin, 2010). These medical system benefits translate into MX$2122 or about 30% of SP's social cost.

Table 10 summarizes the results of the cost-benefit analysis described above. The final column shows that the two benefits accruing to the program's intended beneficiaries represent 23% and 21% of social cost from reduced risk exposure and reduced OOP spending, respectively. That is, program benefits to its intended recipients represent 44% of total program social cost. Table 10 also shows that transfers captured by the health system constitute 30% of social cost. Given that I find no strong evidence for other potential costs such as moral hazard or crowd out, these are not included nor is any benefit from health improvements. The short follow-up period and sample size precludes estimates of mortality effects, but if found to be positive, program benefits would be substantially greater than cost. Another consideration is that this analysis does not value the distributional effects produced by health coverage. Given that the program is financed through general revenue (that is disproportionately from the wealthy), while benefits accrue to disproportionately to the poor, a concave social welfare function that values transfers down the income distribution would imply program benefits larger than cost as well.[30]

7. Conclusion

Previous non-experimental studies of Seguro Popular have found that the program generates financial risk protection, increases utilization, and improves health status (Barros, 2008; Gakidou et al., 2006, Bleich et al.,

[29]Source: SINAIS (2006). Situacion del ejericio de los recursos transferidos para el Seguro Popular, Cuadro I.24.

[30]In addition, health insurance may provide access to medical care that otherwise would not have been available and improve health in the process. However I do not explicitly attempt to measure the impact of the access-motive for health insurance (Nyman, 1999).

2007). The preceding analysis supports those results on financial risk protection. I find that Seguro Popular lowers out-of-pocket spending by 49% or 4.3% of non-health expenditure, with the largest reduction in the right tail of the spending distribution. However, these experimental data provide little evidence for increased utilization or better health. In addition, I calculate that health coverage produces benefits for its intended recipients equal to 44% of program costs, with roughly half coming from reduced out-of-pocket health spending and the other half from less risk exposure. Another 30% of program cost is captured by the medical care system. Like Miller et al. (2013), I find minimal evidence that risk reduction produced changes in the portfolio of expenditure choices. However, housing expenditure did increase, which could be interpreted as evidence for greater saving.

Most studies of insurance expansion find that reduced out-of-pocket prices for care raises utilization (Finkelstein et al., 2012; Kondo and Shigeoka, 2013; Miller, et al. 2013; Powell-Jackson et al., 2014; Limwattananon et al., 2015), yet I find no overall utilization improvement. An analysis of utilization for those with baseline chronic disease reveals increases in care likely to be beneficial. However, even this analysis reveals no improvement for the highest burden chronic disease in Mexico as diabetes-related utilization among those with baseline measured diabetes did not increase. Analyzing a health facilities survey, I show that the increase in coverage coincided with no expansion in supply and a statistically significant shift inward for at least one particularly liquid form of supply, staffing. I suggest that this inward supply shift was driven by the combination of Seguro Popular being underfinanced generally combined with a reduction in user fees after reform.

In contrast to other experimental studies with short follow-up periods (Finkelstein et al., 2012; Baicker, et al., 2013), I find limited evidence for improvements in self-reported health and no gains (and point estimates in the worsening direction) for clinical measures of health like obesity, diabetes, and hypertension. Powell-Jackson et al. (2014) find improved health among children with anemia at baseline and Miller et al. (2013) finds improvements in child health linked to prevention, as reform raised financial incentives for preventive care. Given similarities between the organization of Mexico's and Colombia's health systems pre-reform, noting differences in design may suggest explanations for the discrepancy in results. Even though coverage eligibility is also determined by employment status in Colombia, providers and insurers are separate. Contracts between these entities generate financial incentives through capitated payments or fee-for-service to increase supply when demand increases. Insurers also can institute "utilization review" as a tool to limit wasteful spending. These functions all occur within state Ministries of Health in Mexico, minimizing financial incentives for supply

expansion.

This study's limitations include the fact that these data are not nationally representative, but instead focus on the low-income and primarily rural population that the program was intended to help. Also, these data are from the early introduction of the Seguro Popular program, and the number of diseases and procedures covered has increased since, while costs may have declined through economies of scale. Nevertheless, even by 2005, primary program structure had already been established, while the number of medical interventions covered has not expanded significantly between 2006 and 2014 (CAUSES, 2014).[31]

This study intends to generate a comprehensive picture of the costs and benefits produced by Mexico's health coverage system for the poor. An effort is also made to generate a metric of policy impact that can be compared to other studies and to direct cash transfers. Given the substantial rise in interest and advocacy for expansion of these programs, this paper highlights both successes and challenges for universal health coverage to meet its most ambitious goals. These results show the important benefits that health coverage can provide and how supply-side responses to financing reforms may mitigate these positive effects. As these programs grow in number and scope, more studies that are comparable will help produce evidence on which design elements are effective, such that universal coverage can better fulfill its promise to both improve health and reduce poverty.

[31]As of 2014, SP covered 285 medical interventions compared to 249 in 2006. The number of medicines covered has substantially increased from 265 in 2006 to 634 in 2014. However, other evidence suggests that statutory access to medications does not equate to access in practice (Kuhn et al., 2015).

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9. Tables

Table 1: **Balance between treatment and control areas at baseline**

	Control	Treatment	(2) - (1)	P value	N
	(1)	(2)	(3)	(4)	(5)
Sociodemographics:					
Male Household Head	0.38	0.37	-0.01	0.34	27093
Age	41.75	42.01	0.27	0.66	27057
Rural	0.93	0.92	-0.004	0.93	27093
Assets Index	0.44	0.44	0.01	0.77	27079
State of Residence:					
Estado de Mexico	0.60	0.57	-0.03	0.78	27093
Morelos	0.23	0.24	0.01	0.87	27093
Other States	0.17	0.19	0.02	0.85	27093
Expenditure:					
OOP Exp.	2585	2669	84.47	0.71	26029
Non-Health Exp.	35300	36265	964.50	0.65	23541
Food Exp.	14661	14833	172.04	0.86	26720
Housing Exp.	3238	3282	44.51	0.88	26785
Health and Utilization:					
Obesity	0.16	0.15	-0.01	0.56	17706
Hypertension	0.17	0.18	0.002	0.83	26455
Diabetes	0.19	0.20	0.01	0.65	12569
Outpatient Visits	1.12	1.16	0.04	0.67	27093
Inpatient Visits	0.08	0.08	-0.0008	0.90	27093

Table 1 shows experimental balance at baseline among eligible households. Columns 1 and 2 indicate the mean level of each outcome variable at baseline in the control and treatment clusters, respectively. Column 3 shows the difference in means between treatment and control at baseline. Column 4 displays the p-values for the difference in means with standard errors clustered at the level of randomization.

Table 2: **Effect of Insurance on Out-of-Pocket Health Spending**

	Mean '06	ITT	2SLS		ITT	2SLS	
Endogenous Variable:			Binary	Coverage Time		Binary	Coverage Time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OOP HE	3081.9	-324.2	-715.7	-1238.6	-392.8	-869.9	-1508.7
		(241.9)	(527.1)	(909.9)	(234.2)*	(518.1)*	(891.5)*
Inpatient	568.6	-119.7	-266.0	-463.1	-145.0	-322.7	-563.0
		(57.5)**	(125.9)**	(215.3)**	(62.0)**	(139.0)**	(237.4)**
Outpatient	469.9	-112.5	-248.2	-436.1	-147.3	-329.2	-572.1
		(66.8)*	(145.3)*	(256.2)*	(81.9)*	(179.8)*	(316.2)*
Drugs	1239.3	-52.5	-114.6	-198.6	-90.1	-196.4	-344.4
		(104.7)	(231.2)	(400.8)	(107.5)	(238.1)	(411.9)
Devices	122.5	11.9	26.2	45.8	23.9	53.0	92.6
		(21.5)	(48.1)	(83.9)	(20.4)	(45.5)	(80.1)
Other	738.8	-68.1	-150.4	-251.9	-23.9	-51.5	-82.3
		(80.4)	(174.7)	(301.2)	(80.2)	(177.6)	(306.8)
Controls for Baseline Differences					X	X	X

Table 2 displays the mean effect of SP coverage on out-of-pocket health expenditure and its components (inpatient, outpatient, drug, medical devices, and all other health expenditure). Column 1 shows the mean of OOP health spending and its components in the control area for the follow-up survey. Columns 2-4 indicate the mean ITT, LATE with binary insurance, and LATE with estimated full-household coverage time using follow-up data, respectively. Columns 5-7 display the ITT, LATE with binary insurance, and LATE with estimated full-household coverage time, respectively, while controlling for differences at baseline. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table 3: **Effect of Insurance on Non-Health Expenditure**

	Mean '06	ITT	2SLS		ITT	2SLS	
Endogenous Variable:			Binary	Coverage Time		Binary	Coverage Time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-Health Exp.	35458	251.8	551.0	910.9	173.1	397.0	622.9
		(1260.3)	(2736.3)	(4764.7)	(1258.8)	(2749.7)	(4799.5)
Basic Food	15060	305.7	673.5	1177.4	429.5	952.0	1672.0
		(578.2)	(1260.5)	(2199.7)	(581.3)	(1255.1)	(2211.8)
Housing	3169	323.8	709.4	1229.4	403.8	895.2	1554.3
		(191.9)*	(430.4)*	(753.7)	(157.7)**	(365.9)**	(642.0)**
Alcoh. & Tob.	448	-87.3	-190.8	-332.2	-48.6	-106.2	-186.8
		(43.4)**	(94.2)**	(169.3)**	(51.9)	(114.5)	(201.1)
HH Assets	901	-46.3	-99.4	-169.8	153.8	347.2	599.4
		(118.0)	(255.8)	(446.0)	(161.7)	(359.9)	(627.0)
Education	1952	-65.1	-142.2	-246.0	-269.1	-606.4	-1036.0
		(200.8)	(435.7)	(760.4)	(370.9)	(834.8)	(1446.5)
Transport	6688	-102.2	-225.1	-462.8	-89.8	-197.1	-420.5
		(370.8)	(804.1)	(1401.5)	(341.2)	(752.2)	(1308.3)
Consumables	7090	-89.7	-194.2	-343.4	-42.0	-80.1	-165.0
		(345.3)	(746.9)	(1302.7)	(339.4)	(743.3)	(1292.3)
N		23201	23195	23156	46380	46339	46335
Controls for Baseline Differences					X	X	X

Table 3 displays the mean effect of SP coverage on non-health expenditure and its components. Each model includes baseline covariates respondent age, sex, urban / rural status, state fixed effects, and household asset index to control for chance differences between treatment and control groups. Column 1 shows the mean of OOP health spending and its components in the control area for the follow-up survey. Columns 2-4 indicate the mean ITT, LATE with binary insurance, and LATE with household insurance exposure using follow-up data, respectively. Columns 5-7 display the ITT, LATE with binary insurance, and LATE with household insurance exposure, respectively, while controlling for differences at baseline. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. Because the regressions using components of non-health spending are run with different sample sizes depending on missingness, the component treatment effects do not sum to the total non-health spending treatment effect (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table 4: **Effect of Insurance on Utilization**

	Mean '06	ITT	2SLS	
Endogenous Variable:			Binary	Coverage Time
	(1)	(2)	(3)	(4)
# Inpatient Visits	.0816	-.0042	-.0092	-.0160
		(.0058)	(.0126)	(.0217)
# Outpatient Visits	1.5316	-.0643	-.1398	-.2363
		(.0915)	(.1987)	(.3434)
# Inpatient Surgeries	.0105	.0017	.0038	.0065
		(.0017)	(.0036)	(.0063)
# Birth Admissions	.0267	.0041	.0088	.0152
		(.0024)*	(.0051)*	(.0089)*
N		23100	23100	23100

Table 4 displays the mean effect of health coverage on multiple measures of utilization. Column 1 shows the mean of each dependent variable in the control area at follow-up. Columns 2-4 provide the ITT, LATE using a binary insurance variable, and LATE using household insurance exposure, respectively. Each model includes baseline covariates (respondent age, sex, urban status, state fixed effects, and household asset index) to control for chance differences between treatment and control groups. Household insurance exposure includes both the estimated time spent insured and the percentage of the household spent insured during that time. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table 5: **Effect of Insurance on Utilization Targeted to those with Need**

Baseline Condition:	Obesity		Hypertension	Diabetes
Utilization:	Birth-related		Hypertension-related	Diabetes-related
	Outpatient	Inpatient	Outpatient	Outpatient
	ITT	ITT	ITT	ITT
	(1)	(2)	(3)	(4)
OLS	.094	.010	.123	-.042
	(.035)***	(.008)	(.067)*	(.115)
Poisson	.557	.233	.287	-.065
	(.224)**	(.269)	(.151)*	(.176)
Any Visits	.013	.012	.027	-.011
	(.007)**	(.006)*	(.015)*	(.019)
Mean Control	.114	.026	.372	.653
N	2513	2513	4158	2121

Table 5 displays the difference in utilization between treatment and control clusters by baseline health status for those types of utilization likely to be beneficial. From left to right, these include outpatient birth utilization and inpatient birth-related visits for those with baseline obesity, outpatient hypertension utilization among those with baseline hypertension, and outpatient diabetes-related utilization for those with baseline diabetes. Each model includes baseline covariates (respondent age, sex, urban status, state fixed effects, and household asset index) to control for chance differences between treatment and control groups. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table 6: **Effect of Insurance on Health Infrastructure Supply**

	Mean	N	Baseline Diff.	Follow-up Diff.	Diff.-in-Diff.
	(1)	(2)	(3)	(4)	(5)
Staff	2.88	133	-.160	-.938	-.778
			(.676)	(.674)	(.289)***
Hours Open	45.26	128	-.601	-1.0	-.398
			(1.763)	(2.039)	(2.073)
Offices	1.88	133	.469	.071	-.398
			(.687)	(.307)	(.680)
Beds	1.54	133	.785	-.215	-.999
			(.650)	(.375)	(.711)

All table 6 data come from a health facility level survey collected at baseline (2005) and follow-up (2006). Column 1 reflects the dependent variable's mean in the control area during the follow-up survey for those facilities present in both surveys. Column 2 reflects the number of facilities reporting in both surveys for a given dependent variable. Columns 3 and 4 reflect the unconditional mean difference in health infrastructure between treatment and control clusters in 2005 and 2006, respectively. Column 5 represents the differential change in health infrastructure between treatment and control areas, accounting for baseline differences. Robust standard errors clustered at the level of randomization are used (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table 7: **Effect of Insurance on Self-Reported Health Status**

	Mean '06	N	ITT	2SLS	ITT	2SLS
Endogenous Variable:				Coverage Time		Coverage Time
	(1)	(2)	(3)	(4)	(5)	(6)
Self Reported Health	2.403	23182	-.040	-.150	.009	.038
			(.028)	(.108)	(.034)	(.130)
Chronic Conditions	.284	22858	-.018	-.068	-.032	-.121
			(.023)	(.050)	(.027)	(.100)
ADL Score	.885	23182	.015	.058	.007	.028
			(.006)**	(.024)**	(.008)	(.028)
Depression	.241	23105	-.020	-.076	-.033	-.126
			(.016)	(.059)	(.022)	(.084)
Controls for Baseline Differences					X	X

Table 7 displays the mean effect of insurance on self-reported health status (self-reported health, total reported chronic conditions among asthma, arthritis, and angina, an Activities of Daily Living (ADL) score (where higher values imply better health), and self-reported depression). Column (1) shows the dependent variable's mean in the control area for the follow-up survey. Columns (3) and (4) indicate the mean ITT and LATE with household insurance exposure using follow-up data, respectively. Columns (5) and (6) display the ITT and LATE with household insurance exposure, respectively, while controlling for baseline differences. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table 8: **Effect of Insurance on Objectively Measured Health Status**

	Mean '06	N	ITT	2SLS	ITT	2SLS
Endogenous Variable:				Coverage Time		Coverage Time
	(1)	(2)	(3)	(4)	(5)	(6)
Obesity	.165	19040	.020	.035	.016	.064
			(.012)	(.048)	(.010)*	(.038)*
Diabetes	.250	10578	.050	.186	.041	.150
			(.026)*	(.099)*	(.026)	(.097)
Hyperglycemia	.036	23367	.005	.020	.003	.010
			(.005)	(.021)	(.004)	(.016)
Hypercholesterolemia	.026	19935	.0002	.001	.0003	.002
			(.004)	(.014)	(.005)	(.018)
Hypertension	.126	24191	-.006	-.024	-.009	-.033
			(.008)	(.030)	(.008)	(.029)
Controls for Baseline Differences					X	X

Table 8 displays the mean effect of insurance on measured health status (obesity, diabetes, hyperglycemia, hypercholesterolemia, and hypertension). The survey measured respondent height, weight, HbA1c (for half of respodents), cholesterol, and blood pressure. Column (1) shows the dependent variable's mean in the control area for the follow-up survey. Columns (3) and (4) indicate the mean ITT and LATE with household insurance exposure using follow-up data, respectively. Columns (5) and (6) display the ITT and LATE with household insurance exposure, respectively, while controlling for baseline differences. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table 9: **Effect of Insurance on the Composition of Utilization.**

	Mean '06				
	Control	Treatment	ITT	2SLS	ITT
	(1)	(2)	3)	(4)	(5)
MoH (No Seguro Popular)	.569	.416	-.153	-.305	-.147
			(.036)***	(.077)***	(.035)***
MoH (Accepts Seguro Popular)	.012	.186	.175	.348	.168
			(.025)***	(.046)***	(.024)***
Formal Sector	.036	.035	-.001	-.003	.005
			(.007)	(.014)	(.008)
Private	.318	.316	-.0002	-.0004	.002
			(.028)	(.055)	(.026)
Other	.065	.045	-.020	-.040	-.027
			(.019)	(.037)	(.023)
Controls for Baseline Differences					X

Columns 1 and 2 in table 9 reflect the proportion of health facility visits in the past year by facility type at follow-up in control and treatment clusters, respectively. The first row refers to utilization in health facilities for informal sector workers that do not accept Seguro Popular, while the second row to utilization for informal workers in facilities that accept Seguro Popular. The third row refers to health facilities for formal sector workers, the fourth private facilities, and the fifth to all other types of facilities. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table 10: **Summary of Seguro Popular's cost and benefits**

	$MXN		$MXN	% of Cost
Total Spending per HH	5511	Risk Exposure	1613	23
Cost of public funds	1653	OOP Effect	1512	21
		Transfers	2122	30
Total Cost	7164	**Total Benefit**	5247	73

Table 10 summarizes the costs and benefits of Mexico's Seguro Popular. Benefits are displayed as a percentage of total cost. Risk exposure reflects risk premium benefits from the introduction of Seguro Popular, displayed in figure 3 using a stylized utility model with a coefficient of relative risk aversion of 2.5 and assuming that medical shocks cannot reduce spending below 20% of initial consumption. The OOP effect reflects the reduction in OOP health spending from one year of full-household exposure to Seguro Popular, while controlling for spending differences at baseline (table 2, row 1, column 7). The amount of SP funding transferred to the medical system is determined by using Mexico's national accounts (Secretaria de Hacienda y Credito Publico, 2010) using the percent of federal transfers spent on wages and salaries in 2006 (38.5%). Total spending per household comes from the same source, while the social cost of insurance includes a 30% deadweight loss raising funds through taxation. (Columns may not be equal due to rounding). Ex-post moral hazard is found to be zero since utilization does not increase, as is ex-ante moral hazard. Crowd-out is also null both because of literature and my analysis of how the composition of utilization changes.

10. Figures

Figure 1: **Cumulative distribution of OOP health expenditure for treatment and control areas among eligible compliers.**

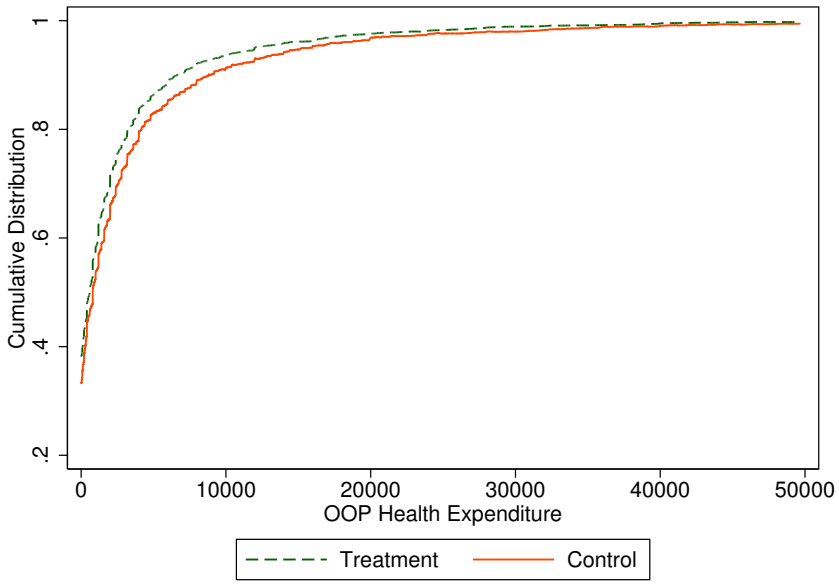


Figure 1 displays the household cumulative density of OOP health expenditure for eligible compliers between treatment and control health clusters. Figure is constrained to values under MX$50,000, which reflects 98% of the OOP health expenditure distribution. Maximum annual OOP health expenditure using the 8-question measure of health expenditure is MX$149,192. CDFs calculated non-parametrically using Abadie (2002).

Figure 2: **Quantile treatment effects by OOP health expenditure quantile for eligible compliers.**

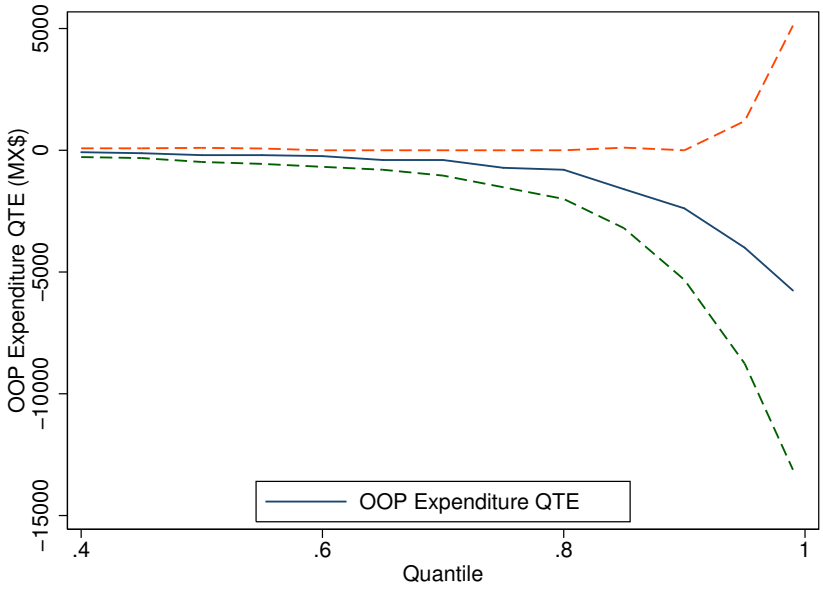


Figure 2 shows the instrumental variable quantile treatment effect of health coverage on out-of-pocket health expenditure using the estimation routine of Abadie, Angrist, and Imbens (2002). Quantiles shown are $Q_{0.4}$-$Q_{0.95}$ by 0.05 quantiles and $Q_{0.99}$. Standard errors are block bootstrapped using matched health-cluster pairs with 500 repetitions.

Figure 3: **Risk premium change ($\Delta\pi$ in %) by risk aversion (ρ) and level of consumption smoothing (minimum C).**

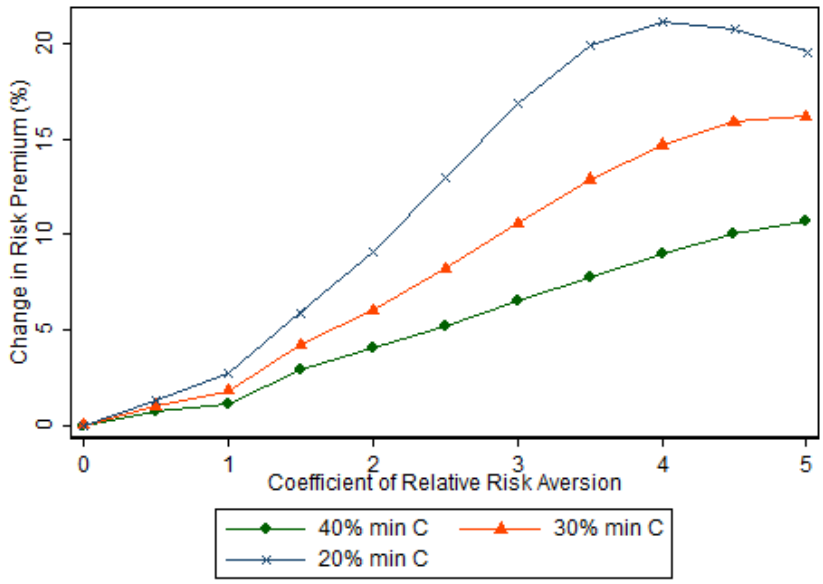


Figure 3 shows the risk premium (a household's willingness to pay to avoid a given level of exposure to medical-expenditure risk) reduction produced by SP among eligible compliers by different levels of risk aversion (ρ refers to the assumed coefficient of relative risk aversion) and the level of consumption smoothing assumed (the minimum level of household expenditure below which we assume medical expenditure will not reduce total household expenditure, set to 20%, 30%, or 40% of total expenditure). The reported $\Delta\pi$ assumes $\rho = 2.5$ and that health expenditure cannot reduce non-health expenditure to less than 20% of total expenditure.

Appendix A. First Stage

Table A.11: **Compliance to treatment assignment (at least one household member with coverage)**

	Baseline 2005		Follow-up 2006	
	Control	Treatment	Control	Treatment
Total	16,219	16,226	14,948	14,949
Eligible	13,728	13,312	12,770	12,389
SP Take-Up (% of eligible)	4%	16%	8%	52%
SP Take-Up (% of total)	4%	13%	7%	45%

Appendix B. Differences-in-differences

ITT and LATE analysis is also performed using the differences-in-differences versions of equations (1) and (2)-(3). These equations are estimated using the following specifications:

$$Y_{ict} = \beta_0 + \beta_1 Z_c + \beta_2 \gamma_t + \beta_3 (Z_c * \gamma_t) + \beta_4 V_{ict} + \epsilon_{ict} \tag{B.1}$$

$$D_{ict} = \delta_0 + \delta_1 Z_t + \delta_1 \gamma_t + \delta_3 (Z_c * \gamma_t) + \delta_4 V_{ict} + \upsilon_{ict} \tag{B.2}$$

$$Y_{ict} = \pi_0 + \pi_1 Z_c + \pi_2 \gamma_t + \pi_3 (D_{ict} * \gamma_t) + \pi_4 V_{ict} + \mu_{ict} \tag{B.3}$$

where Y_{ict} represents the outcome variable for household i, in cluster c, and in time period t. γ_t represents a binary variable denoting follow-up period and our quantity of interest is β_3, the effect on Y_{ict} of coverage in treatment areas for those induced to enroll because of the experiment, controlling for baseline differences in Y_{ict}.

Appendix C. Measuring health coverage time

Using a binary measure of health insurance affiliation, as is done in King et al. (2009), assumes that all households enroll immediately after the baseline survey. However, making this assumption ignores additional information available from the data, which includes the date of each household's baseline and follow-up survey and the share of each household enrolled at each

survey. The variation in household insurance exposure is accounted for explicitly to determine treatment effects. Although the date of enrollment is not observed, since interview dates are given, an interval within which a household enrolls is known, meaning that these data are interval censored. To estimate a household's exposure time to insurance, Monte Carlo simulation is employed using an interval censoring estimation technique with the the INTCENS command in Stata (Griffin, 2005). Given the interval within which a household enrolls, a predicted hazard rate of enrollment is estimated, conditional on baseline covariates (residence in the treatment area, baseline knowledge of SP, and state of residence). An exponential hazard function is assumed. After calculating a household's predicted enrollment hazard, observed enrollment rates at baseline and follow-up (conditional on covariates) are used in Monte Carlo simulation to estimate household insurance exposure. A survival rate between baseline and follow-up is drawn uniformly 1000 times and then an estimated duration of insurance exposure is calculated using the predicted hazard function and the drawn survival rate.

It is assumed that all households are uninsured at the initial time, which is defined by cluster to be the first interview date in that cluster. The data are assumed to be single-spell and therefore households cannot re-enter their initial state of uninsurance. The small number of households that are observed enrolled at baseline and then unenrolled at follow-up (404 or 1.5% of the sample) are therefore dropped. Although most households that enroll do so between baseline and follow-up surveys, 16% of eligible households in the treatment area report some enrollment at baseline. For these households, a failure duration is estimated between the assumed initial time in their cluster (the date of the first interview in their cluster) and their baseline interview. Most pre-basline enrollment of households occurs in a small number of clusters; 50% of pre-baseline enrolled households occur in 7 of 100 clusters and 75% in 14 clusters, while 40 clusters exhibit no households enrolling pre-baseline.

Insurance exposure for household i, $SP_{EXP,i}$, is estimated using the following formula:

$$SP_{EXP,i} = T_{0,i} * HH\ SH_{0,i} * 1\{SP\ Enrolled\}_{0,i} + T_{1,i} * HH\ SH_{1,i} * 1\{SP\ Enrolled\}_{1,i} \quad \text{(C.1)}$$

where $T_{t,i}$ represents estimated exposure time for household i by insurance coverage period indexed by t. The coverage periods are defined where $t = 0$ implies the baseline period and $t = 1$ implies the period between baseline and follow-up survey. $1\{SP\ Enrolled\}_{t,i}$ represents a binary variable for

insurance enrollment at coverage period t, and $HH\ SH_{t,i}$ constitutes reported share of household i covered in period t.

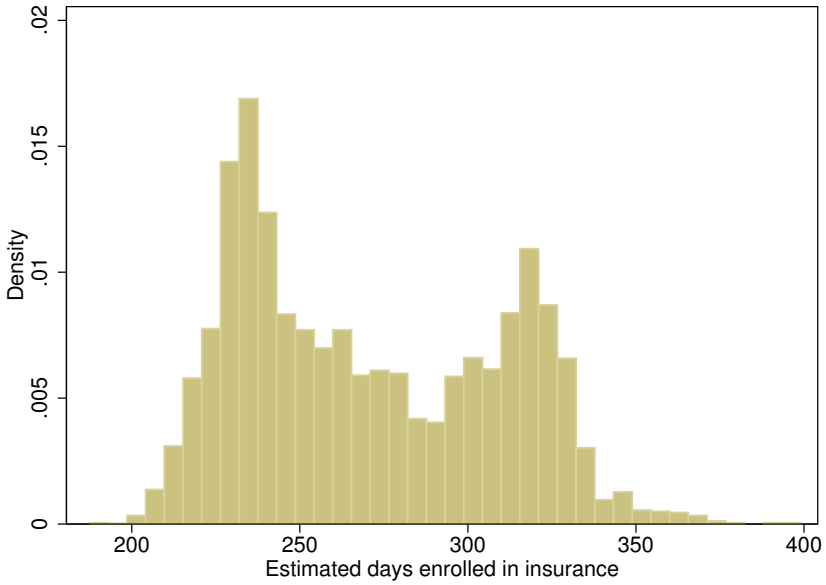
To estimate T_i, an exponential hazard is assumed and estimated using the INTCENS command in Stata, with covariates that predict likelihood of enrollment. The coefficients that indicate each household characteristic's contribution to a groups enrollment ("failure") hazard are displayed below in table C.12. Once each household's failure hazard is estimated $h(X)$, a Monte Carlo process is employed to approximate each household's enrollment time. Using each covariate group's enrollment hazard, $h(X)$, a survival probability is drawn $\in$ [$S(X,0), S(X,1)$] where $S(X,t)$ represents the survival probability for a given covariate group at time t. Survival probabilities (probability of uninsurance) are drawn 1000 times between [$S(X,0), S(X,1)$] and each implies a given T_i. The median of this exposure time process is used as the estimate of insurance enrollment time. Median estimated T_i among those that enroll at any time is .72 of a year with an interquartile range of [.65 , .84]. Figure C.4 displays the distribution of estimated days covered. The bimodal distribution is centered around the most common baseline and follow-up survey dates.

Table C.12: **Duration Model of Insurance Exposure**

Residence in Treatment Cluster	1.4
	(.3)***
Baseline SP knowledge	.7
	(.1)***
Mexico State (Morelos reference)	.7
	(.2)***
Other States	1.1
	(.3)***
Obs.	24437

Table C.12 shows the contribution of each covariate to the constant hazard of failure ("enrollment"). Exponential model of duration to failure is employed with heteroskedastic-robust standard errors.

Figure C.4: **Distribution of estimated days enrolled in health insurance.**



Appendix D. Health variables and other definitions

The following appendix section defines the construction of the health and other variables used in the various regressions described above.

ADL Score: The activities of daily living (ADLs) score is created using 17 questions on mobility (2 questions), personal care (3), pain and well-being (3), memory / cognition (2), usual activities (3), sleep and energy (2), and affect (2). Each question reflects respondent difficulty in performing a given task in five categories from no to extreme difficulty. These questions are combined following the RAND Medical Outcomes Study (Stewart et al, 1990) using the formula:

$$ADL_i = \frac{score_i - min\ score}{max\ score - min\ score}$$

where ADL_i represents households i's ADL score, coded such that 0 indicates the inability to perform all activities and 1 no difficulty.

Obesity: Respondent height and weight were also measured. BMI is defined as weight (in kg) / height (in m)^2 and obesity is defined as BMI > 30.

Hypertension: Systolic blood pressure is measured twice, at the beginning and end of the interview and the average of these measures is used. Hypertension is defined as systolic blood pressure above 140 mmHg (Carretero and Oparil, 2000).

Diabetes: Glycated hemoglobin (HbA1c) levels were measured in a random half of households, which is a measure of average blood glucose levels over the last one to three months and diabetes is defined as HbA1c $\geq 6.5\%$ (Diabetes Care, 2010). Diabetes is associated with heart and kidney disease, and nerve disorders.

Hyperglycemia: This condition is characterized by abnormally elevated levels of blood glucose and defined by the American Diabetes Association as blood sugar $\geq$ 180 mg/dl (ADA, 2012). Although blood glucose levels exhibit higher daily variance than HbA1c measures, one definition of diabetes is chronic hyperglycemia, meaning that a high correlation exists between diabetes and hyperglycemia.

Hypercholesterolemia: Prolonged high cholesterol is associated with coronary artery disease and increases heart attack risk. Hypercholesterolemia is de-

fined as blood cholesterol > 240 mg/dl (NIH, 2012).

Definition of components of non-health expenditure: "Basic Food" refers to expenditure on food consumed in the household only, "housing" refers to rent, mortgage payments, and utilities, "Alcoh & Tab" refers to spending on alcohol and tobacco products, "HH Assets" refers to consumer durables like large appliances, and motor vehicles. "Consumables" refers to spending on communication, entertainment, and clothes.

Appendix E. Additional Spending Regressions

Figure E.5: **Annual OOP Health Expenditure CDFs by Treatment and Control areas among eligible compliers.**

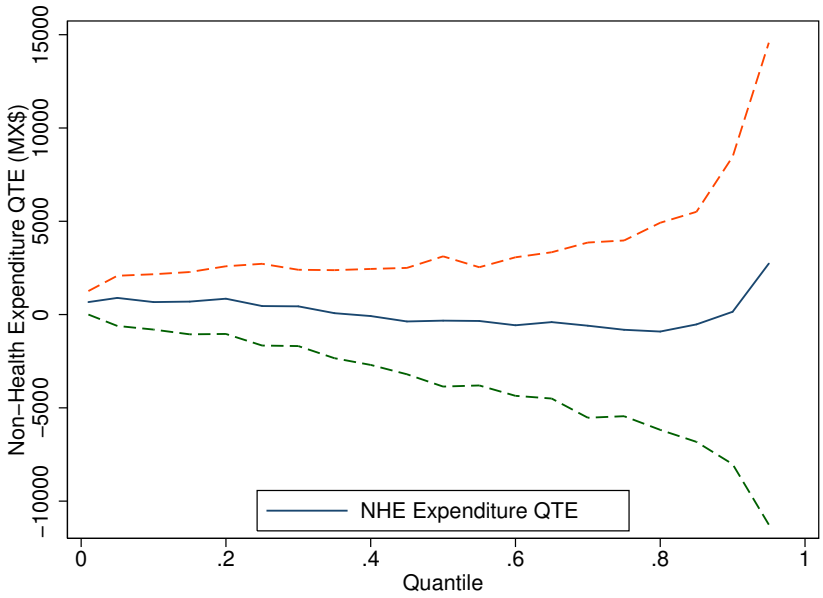


Figure E.5 displays the quantile treatment effect (QTEs) of Seguro Popular health insurance on non-health expenditure. QTEs for compliers where calculate using the routine developed by Abadie, Angrist, and Imbens (2002). Quantiles shown are 0.05 to 0.95 in 5 centile increments. Standard errors are block bootstrapped with 500 repetitions using health cluster matched pairs.

Table E.13: **Seguro Popular's OOP Health Expenditure Effect with Baseline Covariates**

	Mean '06	ITT	2SLS		ITT	2SLS	
Endogenous Variable:			Binary	Coverage Time		Binary	Coverage Time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OOP HE	3081.9	-352.6	-767.0	-1331.5	-394.4	-873.4	-1512.3
	(.0)	(182.7)*	(389.1)**	(683.0)*	(235.0)*	(519.2)*	(894.0)*
Inpatient	568.6	-117.7	-257.8	-450.2	-146.2	-324.8	-566.2
	(.0)	(49.7)**	(105.4)**	(183.8)**	(62.2)**	(139.1)**	(237.8)**
Outpatient	469.9	-114.6	-249.2	-439.2	-148.0	-330.2	-574.3
	(.0)	(63.2)*	(134.3)*	(238.5)*	(82.0)*	(179.7)*	(316.3)*
Drugs	1239.3	-57.3	-123.5	-213.9	-90.2	-196.6	-343.5
	(.0)	(84.5)	(183.3)	(320.3)	(107.9)	(238.5)	(412.7)
Devices	122.5	8.1	17.5	30.9	23.3	51.4	90.2
	(.0)	(17.9)	(39.2)	(68.6)	(20.5)	(45.6)	(80.2)
Other	738.8	-89.4	-194.7	-330.4	-23.7	-51.5	-81.6
	(.0)	(55.8)	(120.1)	(208.7)	(80.6)	(178.0)	(307.7)
Controls for Baseline Differences					X	X	X

Table E.13 displays the mean effect of SP coverage on out-of-pocket health expenditure and its components (inpatient, outpatient, drug, medical devices, and all other health expenditure). Each model includes baseline covariates respondent age, sex, urban / rural status, state fixed effects, and household asset index to control for chance differences between treatment and control groups. Column 1 shows the mean of OOP health spending and its components in the control area for the follow-up survey. Columns 2-4 indicate the mean ITT, LATE with binary insurance, and LATE with household insurance exposure using follow-up data, respectively. Columns 5-7 display the ITT, LATE with binary insurance, and LATE with household insurance exposure, respectively, while controlling for differences at baseline. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table E.14: **Seguro Popular's Non-Health Expenditure Effect**

	Mean '06	ITT	2SLS		ITT	2SLS	
Endogenous Variable:			Binary	Coverage Time		Binary	Coverage Time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NHE 20Q	35458	821.8	1820.8	3085.1	-69.5	-118.4	-312.7
		(2144.6)	(4758.5)	(8241.8)	(1283.4)	(2812.4)	(4895.1)
Basic Food	15060	606.1	1355.6	2354.2	464.7	1046.3	1817.7
		(952.0)	(2144.2)	(3729.1)	(588.7)	(1273.7)	(2240.4)
Housing	3169	449.4	1000.6	1726.5	412.3	920.8	1590.5
		(322.9)	(757.0)	(1316.2)	(158.2)***	(368.3)**	(645.4)**
Alcoh. & Tob.	4478	-75.3	-166.6	-287.8	-50.2	-109.8	-192.1
		(81.6)	(179.4)	(313.0)	(52.5)	(116.2)	(204.3)
HH Assets	901	-.06	2.3	4.6	152.3	345.7	593.1
		(143.8)	(317.4)	(550.8)	(160.2)	(357.1)	(621.5)
Education	1952	-27.5	-59.9	-98.5	-261.1	-587.4	-1002.5
		(217.4)	(479.1)	(830.3)	(369.9)	(834.3)	(1445.4)
Transport	6688	-54.2	-120.3	-288.7	-62.9	-130.6	-323.7
		(468.1)	(1030.0)	(1781.5)	(343.8)	(760.4)	(1321.3)
Consumables	7090	41.0	92.4	154.5	-102.1	-210.3	-391.2
		(465.6)	(1020.9)	(1768.2)	(349.5)	(764.0)	(1325.5)
N		23241	23235	23192	46447	46406	46398
Controls for Baseline Differences					X	X	X

Table E.14 displays the mean effect of SP coverage on non-health expenditure and its components without baseline covariates. Column 1 shows the mean of OOP health spending and its components in the control area for the follow-up survey. Columns 2-4 indicate the mean ITT, LATE with binary insurance, and LATE with household insurance exposure using follow-up data, respectively. Columns 5-7 display the ITT, LATE with binary insurance, and LATE with household insurance exposure, respectively, while controlling for differences at baseline. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. Because the regressions using components of non-health spending are run with different sample sizes depending on missingness, the component treatment effects do not sum to the total non-health-spending treatment effect (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table E.15: **Effect of Insurance on Non-Health Expenditure (samples sizes equal between models)**

	Mean '06	ITT	2SLS		ITT	2SLS	
Endogenous Variable:			Binary	Coverage Time		Binary	Coverage Time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NHE 20Q	35458	821.8	1820.8	3085.1	193.3	478.0	717.0
		(2144.6)	(4758.5)	(8241.8)	(1248.9)	(2712.6)	(4731.8)
Basic Food	15095	584.2	1290.7	2243.2	269.6	605.8	1054.3
		(959.3)	(2138.4)	(3715.5)	(614.6)	(1324.2)	(2317.1)
Housing	31801	426.7	940.1	1623.4	431.9	947.1	1645.2
		(323.7)	(747.7)	(1296.3)	(152.1)***	(353.9)***	(619.5)***
Alcoh. & Tob.	453	-71.6	-156.9	-270.8	-55.5	-120.2	-209.6
		(84.1)	(183.5)	(319.3)	(57.5)	(125.9)	(220.2)
HH Assets	910	11.5	28.3	49.1	118.7	270.2	459.6
		(144.8)	(317.6)	(550.2)	(165.2)	(361.6)	(628.1)
Education	1965	-49.5	-109.4	-187.6	-444.7	-984.6	-1694.5
		(223.3)	(487.6)	(842.8)	(404.1)	(912.7)	(1566.9)
Transport	67578	-125.5	-275.3	-547.7	-77.6	-153.7	-357.1
		(465.0)	(1010.5)	(1746.9)	(351.8)	(771.1)	(1331.5)
Consumables	7097	45.9	103.3	175.5	-49.0	-86.7	-180.8
		(465.5)	(1022.2)	(1770.9)	(315.8)	(688.1)	(1197.2)
N				23192			40588
Controls for Baseline Differences					X	X	X

Table E.15 displays the mean effect of SP coverage on non-health expenditure and its components without baseline covariates. The sample is constrained to households without missingness such that the same sample for all regressions. Column 1 shows the mean of NHE spending and its components in the control area for the follow-up survey. Columns 2-4 indicate the mean ITT, LATE with binary insurance, and LATE with household insurance exposure using follow-up data, respectively. Columns 5-7 display the ITT, LATE with binary insurance, and LATE with household insurance exposure, respectively, while controlling for differences at baseline. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Appendix F. Additional utilization regressions

To better understand targeted utilization, I investigate the effect of the introduction of SP on specific types of inpatient and outpatient care. The household survey provides information on the reason why an individual sought care and therefore outpatient utilization such that general check-ups and visits related to birth, diabetes, and hypertension can be differentiated. Investigating these outcomes allows investigation of how SP changes targeted medical care - for example, outpatient diabetes utilization for those with diabetes at baseline. And we would expect this type of care to be most responsive to changes in insurance coverage. Tables F.19, F.20, and F.21 reflect changes to outpatient birth-, diabetes-, and hypertension-related visits. For table F.19 we observe a statistically and economically significant increase in birth-related utilization for those with baseline obesity, a known risk factor for birth complications. This represents an over 80% increase in birth-related utilization compared to the control area and even larger for compliers as this is an ITT. This increase is consistent with an overall non-significant increase in birth-related utilization of about 6%, which is driven by the right tail of the distribution since the point estimate for any use declines slightly. Mean birth-related outpatient visits is low for those with baseline diabetes and hypertension and we observe small and non-significant effects for them. Taken together, these results show that utilization does not increase overall, but that insurance expansion does generally raise utilization for types of care that are likely to be particularly beneficial. However, for the disease state most prevalent in Mexico, we do not observe similar changes.

Table F.16: **Seguro Popular's Effect on Inpatient Visits**

	Baseline Health Condition			
	All	Diabetes	Obesity	Hypertension
	(1)	(2)	(3)	(4)
OLS	-.005	-.025	-.008	-.013
	(.006)	(.013)*	(.014)	(.010)
Poisson	-.057	-.294	-.108	-.202
	(.071)	(.163)*	(.160)	(.147)
Any Use	-.002	-.015	.001	-.011
	(.005)	(.009)	(.010)	(.008)
Mean Control	.082	.094	.092	.070
N	23469	2121	2513	4158

Table F.16 displays the difference in hospital visits between treatment and control clusters by baseline health status (all respondents and respondents with diabetes, obesity, and hypertension at baseline). Each model includes baseline covariates respondent age, sex, urban status, state fixed effects, and household asset index to control for chance differences between treatment and control groups. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table F.17: **Seguro Popular's Effect on Inpatient Days**

	Baseline Health Condition			
	All	Diabetes	Obesity	Hypertension
	(1)	(2)	(3)	(4)
OLS	-.002	-.082	.001	-.044
	(.020)	(.054)	(.054)	(.038)
Poisson	-.010	-.334	-.015	-.210
	(.102)	(.215)	(.258)	(.182)
Any Use	-.002	-.015	.001	-.011
	(.005)	(.009)	(.010)	(.008)
Mean Control	.197	.281	.209	.230
N	23469	2121	2513	4158

Table F.17 displays the difference in hospital lengths of stay between treatment and control clusters by baseline health status (all respondents and respondents with diabetes, obesity, and hypertension at baseline). Each model includes baseline covariates respondent age, sex, urban status, state fixed effects, and household asset index to control for chance differences between treatment and control groups. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table F.18: **Seguro Popular's Effect on Outpatient Visits**

	Baseline Health Condition			
	All	Diabetes	Obesity	Hypertension
	(1)	(2)	(3)	(4)
OLS	-.064	-.146	-.114	.084
	(.092)	(.162)	(.116)	(.134)
Poisson	-.042	-.074	-.065	.049
	(.061)	(.083)	(.066)	(.078)
Any Use	-.017	-.039	-.034	.005
	(.023)	(.029)	(.024)	(.024)
Mean Control	1.530	2.003	1.818	1.681
N	23480	2121	2514	4160

Table F.18 displays the difference in outpatient visits between treatment and control clusters by baseline health status (all respondents and respondents with diabetes, obesity, and hypertension at baseline). Each model includes baseline covariates respondent age, sex, urban status, state fixed effects, and household asset index to control for chance differences between treatment and control groups. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table F.19: **Seguro Popular's Effect on Outpatient Birth Utilization**

	Baseline Health Condition			
	All	Diabetes	Obesity	Hypertension
	(1)	(2)	(3)	(4)
OLS	.010	.008	.094	-.004
	(.019)	(.025)	(.035)***	(.009)
Poisson	.052	.055	.557	-.099
	(.116)	(.492)	(.224)**	(.423)
Any Use	-.0003	.004	.013	-.003
	(.004)	(.006)	(.007)**	(.003)
Mean Control	.157	.051	.114	.026
N	23472	2121	2513	4158

Table F.19 displays the difference in outpatient birth-related utilization between treatment and control clusters by baseline health status (all respondents and respondents with diabetes, obesity, and hypertension at baseline). Each model includes baseline covariates respondent age, sex, urban status, state fixed effects, and household asset index to control for chance differences between treatment and control groups. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table F.20: **Seguro Popular's Effect on Outpatient Diabetes Utilization**

	Baseline Health Condition			
	All	Diabetes	Obesity	Hypertension
	(1)	(2)	(3)	(4)
OLS	-.009	-.042	-.010	.054
	(.018)	(.115)	(.055)	(.045)
Poisson	-.057	-.065	-.026	.219
	(.134)	(.176)	(.256)	(.177)
Any Visits	-.001	-.011	-.007	.010
	(.003)	(.019)	(.009)	(.008)
Mean Control	.140	.653	.232	.228
N	23472	2121	2513	4158

Table F.20 displays the difference in outpatient diabetes-related utilization between treatment and control clusters by baseline health status (all respondents and respondents with diabetes, obesity, and hypertension at baseline). Each model includes baseline covariates respondent age, sex, urban status, state fixed effects, and household asset index to control for chance differences between treatment and control groups. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Table F.21: **Seguro Popular's Effect on Outpatient Hypertension Utilization**

	Baseline Health Condition			
	All	Diabetes	Obesity	Hypertension
	(1)	(2)	(3)	(4)
OLS	.011	-.001	.014	.123
	(.018)	(.042)	(.042)	(.067)*
Poisson	.101	.008	.080	.287
	(.137)	(.226)	(.229)	(.151)*
Any Use	.0003	-.014	.006	.027
	(.005)	(.015)	(.014)	(.015)*
Mean Control	.125	.184	.179	.372
N	23472	2121	2513	4158

Table F.21 displays the difference in outpatient hypertension-related utilization between treatment and control clusters by baseline health status (all respondents and respondents with diabetes, obesity, and hypertension at baseline). Each model includes baseline covariates respondent age, sex, urban status, state fixed effects, and household asset index to control for chance differences between treatment and control groups. All models are run using heteroskedastic-robust standard errors and clustered at the level of randomization. (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).

Appendix G. Effect on labor supply

The theoretical effect of health insurance on labor supply is a priori ambiguous when employment status is linked to eligibility. Insurance could increase work by the health channel through reduced illness, sick days, disability or care giving, while insurance expansion could shift employment to the less productive informal sector. Both Barros (2008) and Campos-Vasquez and Knox (2010) find little or no effect of SP on the formal labor market where identification comes from the timing of SP roll-out and affiliation targets. del Valle (2014) finds that expansion of SP raises labor supply, especially among women, and attributes this increase to reduced care-giving.

Using the household experimental data, I perform a simple test of whether SP affects labor supply using a linear probability model and a binary indicator of work. Table G.22 shows the intent-to-treat impact of health coverage on probability of any work and no economically nor statistically significant effects are observed. In addition, there is no corroboration of the results from del Valle (2014), in which labor supply results were driven by increases among females, as their point estimates are negative. However, these data were collected in a primarily rural setting in which 45% of all males are farmers and another 18% report working for themselves, and so may not generalize to urban areas.

Table G.22: **Effect on self-reported work by gender.**

	Mean	ITT	ITT	ITT
	(1)	(2)	(3)	(4)
Male	.904	.004	.007	.007
		(.013)	(.011)	(.010)
N		8371	8117	8098
Female	.241	-.021	-.015	-.014
		(.023)	(.019)	(.015)
N		15181	14985	14954
Baseline Covariates			X	X
Controls for Baseline Y Differences				X

The columns of table G.22 reflect the mean of a binary variable for employment and the intent to treat estimate of health coverage, respectively, without controls, controlling for baseline covariates, and controlling for baseline covariates and differences in self-reported employment at baseline. A linear probability model is used and controls include respondent sex, age, education, and marital status (* $p < 10\%$, ** $p < 5\%$, *** $p < 1\%$).