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ABSTRACT

Universal health coverage is a widely shared goal across lower-income countries. We conducted a large-scale, 4-year trial that randomized premiums and subsidies for India's first national, public hospital insurance program, RSBY. We find roughly 60% uptake even when consumers were charged premiums equal to the government's cost for insurance. We also find substantial adverse selection into insurance at positive prices. Insurance enrollment increases insurance utilization, partly due to spillovers from use of insurance by neighbors. However, many enrollees attempted to use insurance but failed, suggesting that learning is critical to the success of public insurance. We find very few statistically significant impacts of insurance access or enrollment on health. Because there is substantial willingness-to-pay for insurance, and given how distortionary it is to raise revenue in the Indian context, we calculate that our sample population should be charged a premium for RSBY between INR 500-1000 rather than a zero premium to maximize the marginal value of public funds.

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1 Introduction

Sickness imposes substantial health and financial costs on individuals. Health insurance may mitigate these costs, and so there may be substantial demand for such insurance. Moreover, concerns about the inefficiency of private insurance markets have led many policymakers to support publicly provided insurance. Considerations of equity have resulted in efforts by policymakers to subsidize insurance premiums. These policy reforms are especially pressing in lower-income countries, where a high fraction of health care costs are paid out of pocket (World Health Organization, 2023).

Designing health financing policy requires information on several critical parameters: demand for insurance, utilization, impact on health, and the degree of market inefficiency stemming from issues such as adverse selection. We estimate these parameters using a large-scale, nearly 4-year randomized controlled trial (RCT) where we varied premiums and subsidies for India's first national, public hospital insurance program, Rastriya Swasthya Bima Yojana (RSBY). The RSBY insurance plan covers up to INR 30,000 of treatment at in-network hospitals per annum per household.

Our study made the RSBY plan available to 10,879 above-poverty-level (APL) households (comprising 52,292 individuals) in 435 villages in Karnataka state who were not otherwise eligible for the plan.[2] Our study randomized these households to one of four arms: (A) zero premiums for RSBY insurance (full subsidy), (B) the full premium and a cash transfer equal to that premium, (C) full premiums for the plan (no subsidy), or (D) no access to RSBY insurance. Moreover, to estimate spillover effects across households, we randomized villages to different proportions of households assigned to each arm. We measured utilization of insurance and of healthcare as well as health outcomes for households at three points in time: at a baseline before enrollment, at a midline 18 months after enrollment, and at an endline 3.5 years after enrollment. We estimate both intent-to-treat (ITT) effects of randomization to each arm and complier-average-treatment effects (CATE) of enrollment in insurance at midline and endline.

One critical parameter is we estimate is demand. Demand captures subjective utility from insurance and impacts that the investigator may not measure. It also determines how much subsidies affect insurance consumption. We find substantial demand for insurance. Uptake was 72.24% when households were charged a full premium but were given a cash transfer equal to the amount of the premium, while roughly 60% of our sample purchased insurance

[2]Outside our study, only below-poverty-line (BPL) individuals were eligible for RSBY insurance. The government purchased RSBY coverage from private insurance companies and provided it nearly for free to BPL households. RSBY plans were not offered to APL households in the private market and outside our study.

when charged the full premiums private companies charge for RSBY coverage but were not given the cash transfer. When the price of insurance fell to zero, 78.71% of sample households purchased insurance. The implied price elasticity of (compensated) demand is -0.0896, on the low end of the range of estimates of insurance demand elasticity from higher income countries (Pendzialek, Simic and Stock, 2016a). These results show significant demand for insurance at positive prices, implying that people subjectively believe health insurance helps address some of the costs of illness.

A second critical factor is adverse selection. This impacts whether private markets for community-rated insurance[3] yield sub-optimal levels of consumption. As we will discuss, it is also relevant for determining the optimal subsidy for insurance. We estimate a detectable and economically meaningful extent of adverse selection into insurance: average predicted health costs are economically and significantly higher among those who select into enrollment at positive prices relative to those who enroll at zero price. Our adverse selection estimates are consistent with two other findings. First, households required to pay full price for insurance had higher levels of utilization than households charged a zero price for insurance. Second, households required to pay full price for insurance also reported lower rates of being unable to use their insurance card. These results are suggestive that price serves as a screen for households that have greater knowledge and/or propensity to utilize insurance.

A third critical parameter is health care utilization. This sheds light on why consumers may value insurance, as well as on the cost of providing insurance. We find that insurance enrollment increased utilization of hospital care. However, many beneficiaries were unable to use their insurance cards and the utilization effect fell over time. Access to free insurance increased 6-month utilization of RSBY insurance by 6.73 pp over a control group mean of 3.86% at 18 months. Spillovers effects play a quantitatively important role: they are responsible for 70% of this effect. Evidently, utilization by neighbors have an important effect on utilization by sample households. Nevertheless, among enrolled households, 5.01% tried but were unable to use their insurance card (compared to a control group failure rate of 1.50%).[4] Our surveys suggest that this difficulty may reflect not just supply-side constraints, but also demand-side obstacles such as households forgetting their card or trying to use RSBY

[3]Community-rated insurance is a contract wherein all members of an insurance pool pay the same premium, even if they have different expected costs to the pool. This premium is based on the average across members of members' expected costs. Experience-rated insurance, by contrast, charges beneficiaries premiums based on their specific expected cost.

[4]Although households in the control group did not have access to RSBY at the start of the study, they may have obtained access over time and utilized RSBY. They may obtain RSBY either by falling into poverty and obtaining a BPL card or by joining an RSBY-eligible occupation, and then signing up for RSBY during annual enrollment.

Our finding of failure during attempts to use insurance complements Banerjee et al. (2021b), which documents failure during attempts to obtain insurance in Indonesia.

at non-participating hospitals. Furthermore, utilization decreased over time: utilization at 6 months was only 1.60% greater in the free-insurance group than the no-access control group after 3.5 years. This cannot be explained by earlier hospital use that makes later hospital use less necessary or by earlier failures to be able to use insurance leading to disappointment with insurance.[5]

The fourth critical parameter is impact on health. Like utilization, this outcome sheds light on why consumers might value insurance. We find that insurance had minimal detectable effects on health. Health insurance showed statistically significant treatment effects on only 3 among 84 health-related outcomes across 2 waves of surveys. This remains true even if we do not apply multiple testing adjustments. This specific result is consistent with results from all other RCTS of health insurance conducted in lower-income countries (Haushofer et al., 2020; King et al., 2009; Levine, Polimeni and Ramage, 2016; Thornton et al., 2010). Our estimated standard errors suggest that we cannot rule out clinically-significant health effects, on average equal to 11% (8.8%) of the standard deviation for each health outcome in ITT (CATE) analyses. However, many of our insignificant estimates suggest negative effects of insurance on health.

Finally, we use our demand and adverse selection estimates to ask whether free insurance is a positive value use of public funds and what the optimal price for insurance is.[6] To answer these questions, we employ and extend the marginal value of public funds framework (Hendren and Sprung-Keyser, 2020a).[7] A key insight of these models is that aggregate demand measures the value of insurance, even if one lacks statistical power to validate the impact of insurance across specific dimensions. Applying this framework in our context highlights a central trade-off when setting insurance subsidies. Higher insurance premiums (lower subsidies) reduce uptake of insurance and thus its social benefits. However, high premiums reduce the fiscal externalities from subsidies, although this effect is less than one-for-one because adverse selection increases the average cost of insurance. Our estimates of insurance demand and cost curves imply that the optimal premium is greater than zero for plausible values of the marginal cost of public funds (MCPF) for a lower-income country (Auriol and Warlters,

[5]Although our study focuses on the impact of price on insurance demand and cost, our analysis of utilization suggests that marketing is also an important policy instrument. First, many households may have been unable to use insurance because they did not know how to use an insurance card or where it was valid. Second, spillovers in utilization are consistent with households learning how to use insurance by observing neighbors. These plausible mechanisms suggest that information is critical for utilization of insurance in lower-income countries.

[6]The successor to the RSBY program in India, called Pradhan Mantri Jan Arogya Yojana (PM-JAY), offered insurance for free and to above poverty-line households like those in our sample. It is available to roughly the bottom 40% of the population based on assets and vulnerability reported in the Socio-Economic and Caste Census (2011), roughly 537 million persons.

[7]Our work on the optimal subsidy builds on Dobkin et al. (2018) and Hendren (2021).

2012). We estimate optimal premiums of roughly INR 528-1052 (holding fixed the rates of successful insurance use measured in our study), and INR 1900-2200 if the unsuccessful utilization we observe translates into successful utilization over time. If the MCPF falls over time to the level observed in higher-income countries, free public insurance may become optimal. Another way to state our finding is that, in India, the cost of financing likely swamps the benefits of public insurance to enrollees.[8]

We contribute to a substantial literature on health insurance and other health programs. Our estimate of the demand curve for insurance contributes to a large literature on insurance price elasticity (Pendzialek, Simic and Stock, 2016b). It also relates to literature on how user fees reduce program uptake (e.g., Cohen, Dupas et al., 2010) in lower-income countries. Our finding of less than complete take-up of insurance even at zero price is consistent with other studies of RSBY (Rathi, Mukherji and Sen, 2012; Berg et al., 2019). At the same time, our estimate of substantial demand for insurance at the price the government pays for insurance, contrasts with Banerjee, Duflo and Hornbeck (2014b), which finds no demand for insurance.

In contrast to a vast literature in rich countries[9], only a few papers have studied adverse selection in health insurance markets in lower-income countries. These studies have yielded varying results. On the one hand, Banerjee et al. (2021a) find evidence that subsidies (i.e., lower cost sharing) select for lower-cost households in Indonesia. Asuming et al. (2019) and Wagstaff et al. (2016) find a similar pattern in Ghana and Vietnam, respectively. Fischer, Frölich and Landmann (2023) find that, in Pakistan, adverse selection is seen when the enrollment decision is at the individual level but not when enrollment is at the household or credit group level. On the other hand, Banerjee, Duflo and Hornbeck (2014b) find no adverse selection in the market for a product bundling credit and health insurance in India, driven in that case by very low demand for the insurance. Given that we find an economically significant level of adverse selection, our results buttress the former view.

Consistent with every RCT of health insurance in other lower-income countries, we find robust effects of insurance access and enrollment on utilization (Haushofer et al., 2020; King et al., 2009; Levine, Polimeni and Ramage, 2016; Thornton et al., 2010; Das and Leino, 2011). However, the level of utilization in our sample, even amongst enrollees, is low, as has been under observed under PMJAY, the program that replaced RSBY and covered our sample population (Garg, Bebarta and Tripathi, 2020). This result, along with our

[8]Our primary calculations use estimates of demand measured after consumers had some information about their expected expenditures when they had insurance. We also calculate measure welfare using what Hendren (2021) calls *ex ante* demand, before consumers have any information on their expected costs. In the presence of adverse selection, *ex ante* demand is greater than ex post demand Hendren (2021). We estimate that the optimal price of insurance falls to roughly INR 239-864 (holding constant the rates of successful insurance use) and INR 281-1217 (if unsuccessful use becomes successful use in time).

[9]See Einav and Finkelstein (2011) and Handel and Ho (2021) for overviews.

evidence on failed attempts to use insurance, are in line with Berg et al. (2019), which finds poor knowledge about RSBY. Our spillover effect results contribute to literature on the importance of learning spillovers in program adoption (Chatterjee et al., 2018; Debnath and Jain, 2020; Liu, Sun and Zhao, 2014; Sorensen, 2006).

Our evidence on the impacts of insurance on health contributes to a very large literature on the subject. In the US, RCTs provide mixed results on health impacts. Some find few significant effects (Finkelstein et al., 2012; Newhouse, 1993), while others report large mortality benefits (Goldin, Lurie and McCubbin, 2021). But the US experience may not apply to lower-income countries, which have different disease burdens (Naghavi et al., 2017) and poor quality health care (Das, Hammer and Leonard, 2008; Das et al., 2012; Mohanan et al., 2015). Most RCTs in lower-income countries find effects on utilization, but no systematic effects on health (Haushofer et al., 2017; King et al., 2009; Levine, Polimeni and Ramage, 2016; Thornton et al., 2010).[10] Our study likewise finds significant effects on utilization, but few significant effects on health. Non-randomized studies of health insurance, however, have found positive health effects. Notably, Sood et al. (2014) used a border identification strategy in Karnataka to study another insurance program (Vajpayee Arogyashree) that, in contrast to RSBY, covers long-term but not acute hospital care. The study estimated a significant increase in utilization and reduction in mortality rates due to insurance access over 18 months.[11] However, that study leaves open the question of the medium-term health effects of insurance coverage and the effects of coverage for acute hospital care visits, which are addressed in this study.

Finally, our paper contributes to the public finance literature that employs the marginal value of public funds framework to evaluate the welfare effect of public policies (Hendren, 2016; Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020b). Critically, our work builds on Dobkin et al. (2018) to incorporate adverse selection into calculations of optimal subsidies for public health insurance under this framework. Our paper applies Hendren (2021) to calculate optimal subsidies using the *ex ante* value of insurance, before consumers have any information on their expected costs. It also adjusts the optimal price calculations in Hendren (2021) to account for the cost of public funds in lower-income countries like

[10] Haushofer et al. (2017) finds that insurance lowers stress, but does not improve other health outcomes. (This is consistent with the Oregon Health Insurance Experiment, which found mental health benefits from insurance, but no significant effects on other health outcomes.) Our study, however, finds no significant effects on mental health. Mahal et al. (2013) finds significant reduction in self-reported sickness from providing both insurance and preventative care products such as soap, water purification tablets, and mosquito repellent, but not from insurance alone.

[11] The implied mortality reductions are quite large: the mortality rate fell by two-third from 0.90% to 0.32% after just 18 months of access to insurance that only covered hospital stays longer than 4 days. The estimates imply that the sample had more deaths than covered hospitalizations, suggesting the mechanism was not treatment and thus not insurance utilization.

India. Finally, we connect our calculations to the price-theoretic model of adverse selection in Einav, Finkelstein and Cullen (2010) and Einav and Finkelstein (2011).

Our paper is organized into five parts. Section 2 describes the Indian health care financing system and explains our experiment. Section 3 presents our empirical strategy. Section 4 presents our top-line results on uptake, utilization, health, and adverse selection. Finally, section 5 presents our welfare analysis.

2 Background and study design

2.1 Setting

India spends 3-4% of GDP on health care (World Health Organization, 2023). India's population obtains 75% of its care from private facilities (Government of India and Ministry of Health and Family Welfare, 2015). Before 2008, only 75 million people had health insurance (Prinja et al., 2019). Uninsured patients pay out of pocket for care. If patients lack money to pay for care, a private hospital may deny care. Patients can obtain subsidized care at public facilities. However, these facilities have limited inventories of consumables such as anesthetics (Deka, 2012; Nair, 2018; Government of Uttar Pradesh, 2019), which patients must buy themselves. Over 60 million Indians are impoverished by healthcare expenditures each year (Shahrawat and Rao, 2012).

In 2008, the government adopted the RSBY public health insurance program. From 2008 to 2018, this program paid private insurance companies to provide an RSBY-defined health insurance plan to below-poverty-line households and certain vulnerable occupations. (The poverty line in Karnataka in 2012 was annual income of INR 54,120 ($3,089.04) in rural and INR 65,340 ($3,729.45) in urban areas for a family of five (Prabhavathi and Naveena, 2014).) This program did not cover our sample. In 2015, the government paid an annual premium of INR 133 ($7.59) and INR 173 ($9.87) per household in Gulbarga and Mysore, respectively, the two districts of Karnataka in our study.[12] (We employ the INR 17.52/dollar purchasing-power-parity exchange rate (OECD, 2020).) Although the government paid the insurance company premiums, households has to pay INR 30 ($1.71) for an insurance card to enroll in the program.

The RSBY insurance plan covered inpatient treatments, select outpatient surgeries, and diagnostic tests. It covered care at in-network hospitals, which included both public and private hospitals.[13] RSBY did not cover primary care. The government was responsible for

[12]The premium the government paid was low because the hospital utilization rate of below-poverty-line household is low and the government buys insurance from the insurance company bidding the lowest premium.

[13]Not all public hospitals were in-network because they were run by a different agency than ran RSBY. The

enrolling eligible households via occasional enrollment campaigns and at RSBY offices. Insurance coverage had no deductible or copay, but had an annual cap of INR 30,000 ($1,712.33) per household. The government dictated the prices for procedures; this cap was enough to cover, e.g., 4 c-sections or 10 MRIs.[14] Up to 5 members per household were covered.

In 2019, the RSBY program was replaced by a new program, PMJAY, which expanded RSBY in two critical ways. First, it expanded coverage to include millions of above-poverty line households. Specifically, it used economic and occupational categories based on data from the Socio Economic and Caste Census of 2011 so as to cover a little over 500 million Indians. Second, it increased the cap on covered annual expenditures from INR 30,000 to 500,000 per household (Economic Times, 2021).

Our study was conducted before RSBY was replaced by PMJAY. Survey activities took place between March 2013 and June 2019. Study-related insurance access was provided from May 2015 to August 2018. Specifically, the study provided RSBY coverage to above-poverty-line households that were not eligible for RSBY in collaboration with the national- and Karnataka state-level agencies running RSBY. We chose to experiment with expanding RSBY eligibility rather than providing more than hospital coverage to below-poverty line populations because India did not have the information technology and contractual infrastructure to provide more than hospital insurance. We did not experiment with adding deductibles or co-pays to RSBY's insurance plan for the same reason.[15] We decided to provide RSBY rather than private insurance because RSBY had a network of hospitals that private insurance did not; moreover, we expected that RSBY's structure and network would be the basis for any future expansion. (This largely turned out to be correct for PMJAY.)

2.2 Participants

The study was carried out in one state (Karnataka). We chose two geographically distinct districts within that state – a district in the far north (Gulbarga) and one in the south (Mysore) so that participants would be representative of central and southern India, respectively.

value of RSBY at public hospitals is uncertain. On one hand, public hospitals were supposed to provide care for free. On the other hand, those hospitals are known to have shortage of consumables such as bandages, requiring patients to buy them (Government of Uttar Pradesh, 2019). RSBY revenue might have led public hospitals to purchase those consumables on behalf of insured patients.

[14] There is some concern that the prices the government set were low relative to medical procedure prices in local markets (Jain, 2021).

[15] In the US it may seem natural to expand the range of treatments covered or co-insurance; however, that requires a network of contracts with providers and an electronic billing and payment system to collect money from insureds that exists in the US, but not in India. We explored these avenues of experimentation before choosing our design, but were unsuccessful because of the lack of infrastructure.

Our sampling strategy first identified eligible villages and then eligible households within villages. In each district, villages were eligible if they were within 25 km (15.63 miles) of a hospital that was in-network for RSBY in 2013, to ensure that study participants had local access to covered hospitals. Within eligible villages, households were eligible to participate in the study if they responded to our screening survey and met the inclusion criterion: they had a member who held an Above-Poverty-Line (APL) ration card.[16] Households were excluded from eligibility if, as of 2013, they (i) had a member who possessed a card indicating they were below-the-poverty-line (BPL); (ii) had RSBY coverage; or (iii) had a member that was enrolled in insurance that provided access to hospital care.[17]

Table 2 presents village-level statistics for sample villages. On average 18% of village populations were below-poverty-line (BPL), meaning a large fraction were non-BPL and hence potentially eligible for this study. Villages were on average about 9 kilometers from a hospital.

Table 3 presents characteristics of respondent households by group at baseline. The mean annual household budget (excluding medical expenditures) across groups was INR 98,326 ($5,618.63). (For reference, the poverty line in Karnataka in 2012 was annual income of INR 54,120 ($3,089.04) in rural and INR 65,340 ($3,729.45) in urban areas for a family of five (Prabhavathi and Naveena, 2014).) Mean medical expenditure across groups was INR 38,115 ($2,191.78), of which INR 8,147 ($465.01) was spent on hospital care. Conditional on having positive hospital expenditures, average hospital expenditure is INR 16,181 ($924.63) and 76% had hospital expenditure below the RSBY limit of INR 30,000.

2.3 Intervention and randomization

The study involves two-stage randomization, first at the village level and then at the household level. There were four household-level premium and subsidy conditions:

A. Access to RSBY for free ("free-insurance" arm),

[16] Indians may apply for a so-called ration card that entitles their household to food and other commodities at discounted prices at government sanctioned fair price or "ration" shops. Individuals who meet below-poverty-line or even lower Antyodaya Anna Yojana thresholds get access to a greater quantity of subsidized food than those with above-poverty-line cards.

[17] The sample was identified through a listing exercise and a consent process. The listing was carried out in two waves, in March–June 2013 and in November–December 2013. The actual randomized sample was smaller than the listing because not all listed households were available on the days we approached households for consent and because many households became ineligible by the time they were approached for consent. The main cause of ineligibility is that households obtained BPL cards. A total of 8,866 and 2,013 households were identified and enrolled from the two listing waves, respectively, for a total sample of 10,879 households.

B. The opportunity to purchase RSBY plus an unconditional cash transfer equal to the purchase price ("sale-plus-transfer" arm),

C. The opportunity to purchase RSBY ("sale-of-insurance" arm), or

D. No access to RSBY (control arm).

Households assigned to arm A did not have to pay any amount for RSBY coverage. The purchase price per household for groups B and C was INR 163 ($9.31) and INR 203 ($11.60) in Gulbarga and Mysore, respectively.[18] The transfer to group B was equal to this purchase price. There were two purposes of the transfer. One was to eliminate liquidity constraints when measuring demand. The second was to offset income effects and measure compensated demand for insurance. Because *ex ante* eligibility for RSBY was an exclusion criterion, group D had no access to RSBY via our study.[19] Study households that enrolled in RSBY received coverage from May 2015 to August 2018.

At the village level, the study varied the share of sample households in a village that were assigned to each of the four household groups above. We call these allocations. Table 1 lists the five village conditions, each of which corresponds to a different allocation.

Treatments were assigned in two stages (Hudgens and Halloran, 2008). First, villages were assigned one of five conditions (labeled I, II, III, IV, and V) with probabilities given in Table 1.[20] Second, households in a village were assigned to premiums and subsidy conditions in proportions given by the allocation for their village. In each stage, we stratified units before randomization because doing so balances covariates and reduces orthogonal variation (Imai, King and Clayton, 2009), thereby increasing statistical power. Details of the matching are provided in the Appendix.

We conducted balancing tests to investigate the resulting covariate balance after randomization. First, we estimated multinomial-logit models predicting household group assignment to the 4 treatment groups as a function of outcomes specified in our pre-analysis plan and measured at baseline, one outcome at a time.[21] Second, we conducted likelihood-ratio tests

[18]The purchase price was comprised of an annual premium and a fee for printing an insurance card. The premium was INR 133 ($7.59) and INR 173 ($9.87) in Gulbarga and Mysore, respectively. The smart-card fee was INR 30 in each district.

[19]Because a household could fall under the poverty line over time, it could obtain RSBY outside our study, through annual enrollment by the official RSBY program. Therefore, group D should be thought to have no access to RSBY *at the beginning of the study*.

[20]As the last row of Table 1 explains, across all sample villages, 40% of households are assigned to the free insurance condition (group A), and 20% of households are assigned to each of the other household-level conditions. This implies that, on average across villages, 80% of sample households are given some form of access to insurance.

[21]A pre-analysis plan was posted prior to the last follow-up survey at 3.5 years (AEARCTR-0001793) and data analysis was only conducted after that last survey.

to determine if we can reject a null model equal to the multinomial model without any baseline outcome, i.e., reject that the baseline outcome has no explanatory power. If the block randomization was successful, then the p-values collected from these tests should stochastically dominate the uniform distribution.[22] A one-sided Kolmogorov–Smirnov test reveals that outcomes are balanced (p=1.000) across insurance-access arms (Appendix Figure A.2).

2.4 Measurement and timing

Our study measures several key outcomes. The first is demand, measured by enrollment in RSBY in our treatment arms.[23] Uptake of RSBY measures household benefits in two ways. One is that insurance only (directly) benefits those who take it up. The other is that demand may capture value that we do not directly measure.

The second set of outcomes concerns insurance and health care utilization. This tracks both benefits to consumers and the cost of providing insurance. The latter includes hospital spending paid for by insurance, which depends on i) the rate at which households *utilize* insurance; ii) the average hospital spending of enrolled households. We will distinguish and measure both insurance utilization (use of RSBY to pay for hospital care) and different measures of medical and hospital spending (i.e., actual consumption of care). Insurance may affects the former without changing the latter. Moreover, in the Indian context, perhaps due to lack of knowledge about insurance, many households may attempt to use insurance to pay for care, but fail to obtain care or have the costs covered. We also track this failed use.

The third set of outcomes concerns the downstream effects of health insurance on health. We measure health via self-reported indicators and biomarkers. We also examine validated health scores that use self-reported health as inputs. We describe these metrics in the Appendix Section A.3 and indicate when they are measured in Table A.1. We estimate intent-to-treat effects and complier average treatment effects on each of these outcomes in Section 4. In addition, we examine the effect of price on uptake and costs for the purposes of welfare analysis in Section 5.

In addition to these three sets of outcomes, we measure a number of other household

[22] We expect stochastic dominance relative to the uniform distribution due to stratification (intuitively, the groups will be more balanced than by chance alone).

[23] Enrollment is measured by whether a household enrolled in RSBY during the 1-2 days that we brought a mobile enrollment truck to their village or town during our 2015 enrollment drive. In theory, households in arms 1, 2 and 3 also had the option to enroll by visiting the RSBY office in their district at any time, but we have no evidence that enrollment at the district office, which in general is very uncommon, occurred in our sample. Nor did the government conduct an RSBY enrollment drive for APL households outside the study after 2015. Study households were ineligible to enroll in RSBY via any channel unless they subsequently became eligible, e.g., by obtaining a BPL card.

covariates at baseline, including household composition and finances. We employ these to predict household medical expenditure. This prediction is then used for estimating adverse selection of higher expected cost households into insurance coverage in Section 4.4.

We measured these variables three times during the study. Approximately 18 months prior to the intervention, we conducted a baseline survey of several members in each sample household: the male head of household, a female knowledgeable about household finances, and a female with children below age 18. Those surveys asked about finances and self-reported health. In addition, we selected a random one-third of sample households and conducted a biomarker survey that gathered objective health measures on three members: the male most knowledgeable, a woman of childbearing age, and a child under the age of 18 (if present and available).

At 18 months after the intervention, we conducted a midline survey similar to baseline, including the biomarker survey. At 3.5 years after the intervention, we conducted an endline survey of one household member, the male head or a female knowledgeable about household finances. We did not conduct the biomarker survey at endline. As noted above, because the intervention (i.e., RSBY) ended 6 months before our endline, we asked the respondent to recall hospital utilization 6 months before a notable holiday, Dussehra, that occurred just before the intervention ended.

Figure 1 provides a timeline of our study. The appendix provides additional details about our data gathering during our listing exercise, at baseline, and at midline and endline.

2.5 Sample size and power

Our target sample size was 2,250 households for each of the four household-level groups. According to the National Sample Survey Organisation (2004), the hospitalization rate in 2004 was 10.5% and 11.8% in Mysore and Gulbarga, respectively. At this baseline rate, we estimated that 2,250 households per household-group would provide 80% power to detect a 25% change in hospitalization rate across household-level groups at the 5% significance level, allowing for 10% attrition. We doubled the sample size for the free-insurance group to increase power to detect effects regarding this most likely form for any future insurance expansion.

3 Estimation

We address three sets of research questions. First, what is the effect of access to insurance and the effect of enrolling in insurance on insurance uptake, health care utilization, and health? We explain our estimators for the associated intent-to-treat (ITT) and complier average treatment effects (CATE), respectively, in Section 3.1. Second, is there adverse selection into insurance? To answer this question, we predict health spending at midline using baseline household covariates. We then modify our estimator of ITT effects to determine whether uptake is greater among households with higher predicted spending. We explain this approach in Section 3.2. The results appear in section 4. Third, we ask, does free public insurance improve welfare and what is the welfare-maximizing price of public insurance? To answer these questions, we estimate the relationship between insurance premiums, on the one hand, and insurance demand and hospital expenditures, on the other. This requires parametric assumptions on demand and cost functions. We explain our methods and findings pertaining to this question in Section 5.

3.1 Effects on enrollment, utilization and heath

We estimate the intent-to-treat (ITT) effect of offering different premiums and subsidies for access to RSBY on enrollment, and both the ITT effects and complier average treatment effects (CATE) of enrollment in RSBY on utilization and health at 18 months and 3.5 years.

Intent-to-treat. ITT estimates were based on the following linear regression[24]:

$$y_{ijt} = \alpha + \sum_{h=1}^{3} \beta^h d_{ij}^h + \psi s_j + \sum_{h=1}^{3} \gamma^h (d_{ij}^h \cdot s_j) + \epsilon_{ijt} \tag{1}$$

where i, j and t index households, villages and time, respectively; y_{ijt} is an outcome; d_{ij}^h is an indicator for assignment household-condition $h \in \{1 = A, 2 = B, 3 = C, 4 = D\}$; s_j is the share of the sample in village j that is "treated" (in household conditions A, B or C);[25] and ϵ_{ijt} is an error term. We investigate spillover effects at the village level by examining effects on outcomes for a household in a given household condition at different values of s_j (i.e., shares of the village sample in groups A, B or C).[26] In our setting, s_j ranges between 50–90%.

[24]For binary outcomes, we use a linear probability model (LPM). For our ITT analysis we contrasted the LPM results with those obtained from a logistic model but did not find economically significant differences between the two models.

[25]We also explored a variation of our ITT and CATE specifications where we replaced share given insurance access with number of households given access. We find qualitatively similar effects (not reported), though the exact coefficients on peer behavior variable differ.

[26]Note that the spillover effect on a household in arm h is specified to vary with the fraction s_j of sample households in the village given any access to insurance, not the fraction of households given access via arm h in a village. We have estimated models where the spillover effect in a given arm h is permitted to vary with the fraction of sample households in a village that are in each arm h'. However, we found no systematic patterns in the impact of the fraction in each of the other arms. We also estimated a model where the spillover effect on a household in arm h from the number (rather than the fraction)) of sample households given any access to insurance via the study. The results are qualitatively similar (not reported).

The estimates of spillover effects are unbiased provided that assignment in villages $j' \neq j$ does not influence outcomes in another village j (partial non-interference) (Imai, Jiang and Malani, 2021).[27] The cluster robust HC2 standard error was used to cluster at the village level and account for the two-stage randomization design, an approach that can be shown to be conservative (Imai, Jiang and Malani, 2021).

The average effect of providing access to insurance via arm h to a given household (and to no other households) is labeled a "direct effect" and is estimated as the coefficient β^h on the indicators d_{ij}^h. The average effect on a given household in arm h of changing the share of other households given access to insurance via any arm (holding the treatment status of the given household constant) is labeled a "spillover" or "indirect effect" and is estimated as the sum of (a) the coefficient on the share s_j of the sample in the village in arms 1, 2, or 3 and (b) the coefficient on the interaction between the household-arm indicator and the share variable, $d_{ij}^h s_j$. This interaction allows the spillover effect to vary depending on the arm h to which a household is assigned.

The "total effect" of providing access to all sampled households in a village is defined as the sum of (a) direct effects and (b) indirect effects times the share of other sample households in a village that are offered insurance. We report total effects setting this share to 90%.[28]

Complier-average treatment effect. We use an instrumental variables approach to estimate the complier average treatment effect (CATE) of RSBY enrollment. This corresponds to a treatment-on-treated (TOT) or local average treatment effect (LATE) of enrolling in RSBY. We employ a two-stage least squares estimator in which the second stage regression is

$$y_{ijt} = \alpha + \phi r_{ij} + \theta z_j + \rho(r_{ij} \cdot z_j) + u_{ijt} \tag{2}$$

where r_{ij} is an indicator for RSBY enrollment and z_j is the share of the sample in village j that enrolled in RSBY. We instrumented for $(r_{ij}, z_j, r_{ij} z_j)$ using variables $(d_{ij}^h, d_j^v, d_{ij}^h d_j^v)$, where d_j^v is an indicator for assignment to village-level arm v for $v \in (I, II, III, IV, V)$. Weighting, controls, and standard errors were handled in the same way as with the ITT estimator.

The CATE has a direct and indirect component. The average effect on a given house-

[27]We tested this assumption for all outcomes by adding to the above specification the share of households given access in the village j' closest to the household i's village; for only 1 outcome (utilization of outpatient surgery) is the share in the nearest neighbor statistically significant (unreported).

[28]Another possible scenario is 100%, because a future insurance expansion might cover all other sample-eligible households. However, we choose 90% because no village arm offered more than 90% of sample households insurance, so reporting 100% would be an out-of-sample prediction.

hold of enrolling that household (and no other household) in insurance is labeled a "direct effect" and is given by ϕ. The average effect on a given household of changing the share of other households enrolled in insurance (holding the enrollment status of the given household constant) is labeled a "spillover" or "indirect effect". This effect is estimated using θ, the coefficient on the share enrolled, and ρ, the coefficient on the interaction of share and the indicator for household enrollment. As with the ITT estimate, the total CATE of enrolling all sample households in a village was estimated by summing (a) the direct effect and (b) the indirect effect of moving the percent of a village sample that was enrolled from 0% to 78.71%, the maximum uptake in any arm accounting for in-sample spillover effects. The cluster robust HC2 standard error is again used to cluster at the village level.

An exclusion restriction is needed for the CATE analysis to identify the direct effects of *enrollment* in insurance. This restriction is that the outcome of any given household is affected by the treatment assignments of households within the same village (including the given household) *only* through the actual enrollment decision of the latter households. This is implied by Assumption 3 of Imai, Jiang and Malani (2021), which discusses the non-parametric identification of CATE estimates in two-stage experimental designs.

An additional restriction, Assumption 5 in Imai, Jiang and Malani (2021), is required to identify spillover effects. That assumption is that, if a unit's treatment assignment does not influence its own take up (holding other units' treatment assignments constant), then its treatment assignment should not affect other units' outcomes. An example that satisfies this restriction is that never-takers should not affect other people's outcomes. Importantly, this additional restriction allows for both spillover effects on take-ups and on outcome.

Both ITT and CATE analyses weight households equally. Because males and females were asked the same health questions at 18 months, we present estimates of a common effect across sexes.

Multiple testing. We report two sets of p-values. To test hypotheses pertaining to groups of related outcomes, we primarily report critical p-values for those groups using a multiple-testing correction procedure (Benjamini and Yekutieli, 2001) to control the false discovery rate. Second, we present p-values for individual outcomes because all endline outcomes were pre-specified in our analysis plan.

3.2 Adverse selection

One concern with charging positive prices for insurance is that willingness to pay (WTP) for insurance may be higher among those with higher *ex ante* expected costs; in other words, that higher prices may cause those with lower anticipated costs to dis-enroll and hence induce adverse selection (Chiappori and Salanié, 2013). Our study induces exogenous variation in the price at which households are offered access to insurance, allowing us to cleaning test for adverse selection.

Figure 3 illustrates an insurance market in which adverse selection is present. Following Einav and Finkelstein (2011), the figure graphs demand for insurance (blue) and downward sloping average cost (red). The fact that that average cost is decreasing in the fraction of the population taking up insurance implies adverse selection: at higher prices only those with higher marginal cost will select into insurance.[29] Our empirical approach asks whether increasing price from p to p' increases average cost among those who enroll, i.e., if the average level of marginal cost (the green line) is greater above p' than above p.[30]

We implement this test by regressing uptake of RSBY insurance on indicators for groups that are charged different insurance premiums; a measure of a household's expected cost, and the interaction of premium indicators and expected cost. We first explain how we estimate expected costs and then describe our estimating equation.

Measuring expected costs. Our goal is to construct a measure of expected total household medical expenditure at midline and endline.[31] To do this, we forecast *realized* midline and endline medical spending in the control group (arm 4) as a function of our full set of baseline covariates using machine learning methods (LASSO). We fit our model on the control group because this avoids predicting additional spending caused by insurance (i.e., moral hazard).[32] Using machine learning to select regressors avoids the need for researcher discretion in choosing what baseline characteristics are most predictive of subsequent usage/valuation of health insurance. To generate the prediction, we fit 10-fold cross-validated LASSO models on the control group. The resulting model is then used to generate predic-

[29]Marginal cost need not be continuous or even monotonic, but should be lower among non-enrollees than enrollees at a given price if there is adverse selection. We depict average cost as continuous (which follows from continuous marginal cost) for simplicity.

[30]In our empirical setting, $p = 0$; however, graphically we depict two nonzero prices to make the graph more legible. The interpretation of the test is unchanged.

[31]We focus on the broader measure of health care spending rather than hospital care for two reasons. First, RSBY covers inpatient care and some day surgeries, as described earlier, and it may be more difficult for households to forecast *ex ante* which conditions would require care of the type covered by RSBY versus forecasting their overall health spending needs. Second, our measure of hospital utilization is subject to measurement error at midline. Households were asked about hospital utilization but the definition of hospital was ambiguous; concurrent ethnographic work by our team revealed that many households referred even to clinics that did not provide inpatient care as hospitals. We provided a precise definition at endline. We focus on *ex ante* predicted healthcare spending, rather than *ex post* realized spending, for two reasons. One, enrollment is an *ex ante* decision; two, differences in realized spending would potentially conflate adverse selection and moral hazard or other *ex post* effects. In Section 5 we examine a realized cost measure that by design incorporates both *ex ante* and *ex post* responses to changes in prices.

[32]In theory there could be selection based on moral hazard (Einav et al., 2013). But insurance is a novel product in India so we suspect that enrollees use at best expected health care needs at current prices, rather than at prices under a novel contract, to determine their willingness to pay.

tions for the full sample. See Appendix A.4 for the LASSO procedure details.

Estimating equation. We test for differential selection using the following regression specification:

$$enroll_{ij} = \alpha + \sum_{h=2}^{3} \beta^h d_{ij}^h + \gamma X_{ij} + \sum_{h=2}^{3} \phi^h (d_{ij}^h \cdot X_i) + \psi s_j + \epsilon_{ij} \tag{3}$$

where $enroll_{ij}$ is the enrollment decision of household i in village j, d_{ij}^h is an indicator for assignment to treatment arm $h \in \{1, 2, 3\}$, X_i is the household's expected cost under insurance, and s_v is treatment saturation of village (i.e., the share of the village sample assigned to arms $1, 2, 3$ as opposed to the control arm, 4). In some specifications, we also control for additional baseline covariates (baseline assets, education, Raven's matrix scores, and risk aversion) interacted with treatment status.

We measure variation in a household's costs (X_i) with our LASSO-based prediction of either midline or endline medical spending. We scale X to have unit standard deviation. The omitted category is the free-insurance group (arm 1). Therefore, γ identifies the correlation between predicted spending and enrollment in the free-insurance group and (ϕ_2, ϕ_3) identify the correlation between predicted spending and enrollment in the sale-plus-transfer (arm 2) and sale-of-insurance group (arm 3) respectively relative to γ in the free-insurance group.

We can connect these coefficients to the test for adverse selection illustrated in Figure 3. Our test, which asks whether increasing the price increases expected cost among those who enroll, is equal to a test of $(\phi_2 > 0, \phi_3 > 0)$. (These coefficients could also be negative, which would imply advantageous selection (Fang, Keane and Silverman, 2008).)

4 Results

4.1 Insurance enrollment

Enrollment is measured by whether a household enrolled in RSBY over the 1-2 days we brought a mobile enrollment truck to their village or town during our 2015 enrollment drive.[33]

Free insurance arm. When insurance was offered for free, 78.71% of households enrolled. Table 4 reports our ITT estimates for RSBY enrollment. One might wonder why all households offered free insurance did not take it up. However, incomplete take-up of a

[33] In theory households in arms 1–3 also had the option to enroll by visiting the RSBY office in their district at any time, but we have no evidence that enrollment at the district office, which in general is very uncommon, occurred in our sample. Nor did the government conduct any RSBY enrollment drive after 2015 for any group. Households in the control group (arm 4) were ineligible to enroll in RSBY through any channel unless they subsequently became eligible, for example, obtaining a BPL card.

nominally free insurance is common. Even in the US, the Medicaid enrollment rate among eligible non-elderly adults was only 62% in 2008 (Sommers and Epstein, 2010). Enrollment rates tend to be higher among those with greater health care needs (Kenney et al., 2012). Partial uptake could be driven by shadow prices, which include the opportunity cost of the time required to enroll and the hassle costs of enrolling (Banerjee et al., 2021b), exceeding nominal prices. Finally, enrollment may also be suppressed in India because health insurance is a newer and less well-known product there (Berg et al., 2019).[34]

Sale-of-insurance arm. Enrollment rates were 18.80 pp ($p < 0.001$) lower when households were charged for insurance (uptake of 59.91%). This reduction in enrollment implies that the price elasticity of demand for insurance is -0.314, well within the range of estimates in the literature (Pendzialek, Simic and Stock, 2016a).[35, 36]

Our estimate of enrollment in the sale-of-insurance arm also sheds light on two questions in the literature. One concerns whether there is any positive demand for insurance with positive premiums. Banerjee, Duflo and Hornbeck (2014a) suggested very low demand for health insurance in India in a context without liquidity constraints.[37] Likewise, Banerjee et al. (2021b) found low uptake (< 10%) of public insurance in Indonesia. By contrast, our study estimates uptake of 59.91% at full price. The other debate concerns the deleterious

[34] That said, we do not find heterogeneous enrollment rates by educational status, specifically an indicator for whether the health of household has more than a 10th grade education (unreported).

[35] (78.71% enrollment with free insurance − 59.91% enrollment with sale of insurance at an average price of INR 183)/59.91% = 0.314. We do not use the usual arc elasticity formula, which normalizes by the midpoint of enrollment and price, because our change in price is going from full price to zero price. Our formula implies this is a 100% reduction in price while the usual arc elasticity formula would imply this is a nonsensical 200% reduction in price.

[36] Although the enrollment rate in the sale-of-insurance group was lower than in the free-insurance group in our above-poverty-line (APL) sample, it was similar to the enrollment rate among below-poverty-line (BPL) households offered free insurance under the official RSBY scheme official. The enrollment rate in the official RSBY program was 43.12% in Gulbarga, 59.97% in Mysore (RSBY, 2018). Moreover, enrollment among our APL households in the free-insurance group is much higher than among BPL households offered free insurance. One possible explanation is that our sample has higher income, meaning higher compensated demand or fewer liquidity constraints. We find mixed evidence for this explanation. A regression of household-level uptake decisions on baseline household expenditures (as a proxy for income) interacted with treatment group indicators (and no main effect of household expenditures) in a sample that excludes the control group finds no significant coefficient on interactions with household expenditures (Table A.2). However, a regression of uptake that uses our wealth index does find a significant coefficient on wealth (Table A.3 from the section on adverse selection). An alternative explanation is that enrollment assistance is responsible for the higher in uptake in our study sample. While our study used the same promotional material that the official RSBY scheme did, our study went a step further and visited each house in person to tell them where the enrollment station was on the designated day(s) of enrollment. Unfortunately, we cannot explore this hypothesis because we do not have any variation in marketing across households. However, Banerjee et al. (2021b) found enrollment assistance increased public insurance uptake in Indonesia.

[37] In Banerjee, Duflo and Hornbeck (2014a), microfinance borrowers were required to purchase insurance as a condition of obtaining a business loan but could use the loan proceeds to pay for insurance. They found that borrowers chose not to renew loans when this condition was imposed.

effect of cost-sharing on uptake of health products and services. For example, Cohen and Dupas (2010) estimate a 75% drop in uptake when they increase price of bed nets from 0 to just 15% the cost of bed nets. Our elasticity estimates are much smaller. The difference between our findings and the earlier literature may be attributable to differences in product, in income, and in enrollment assistance. It is difficult to tease those explanation out across studies.

Sale-plus-transfer arm. Enrollment was 12.33 percentage points (pp) higher ($p < 0.001$) in the sale-plus-transfer arm that offered a cash transfer equal to the premium (72.24%) than in the sale-of-insurance arm (59.91%). This implies a marginal propensity to consume (MPC) insurance of 30.76%.[38] This result resembles the so-called "flypaper effect" (Hines Jr and Thaler, 1995).[39]

Our findings have implications for insurance pricing, a topic we discuss in more detail on Section 5. Because free insurance is akin to pure insurance plus an in-kind subsidy, we infer that in-kind subsidies increase enrollment by nearly a third (from 59.91% to 78.71%). In-kind subsidies raise enrollment by twice as much as unconditional cash transfers of the same amount ($p < 0.001$). However, ignoring concerns about distribution, pure insurance is a much more cost-effective means for the government to promote uptake than free insurance. In our sample, the government was able to pay no additional premiums and achieve 59.91% enrollment. To increase enrollment by a further 18.80 pp required paying premiums for both marginal and infra-marginal households—all told, 78.71% of households. This implies an effective premium roughly 4 times the actual premium per marginal (i.e., incremental free-insurance-complier) household.[40]

Finally, spillover effects in enrollment are not statistically significant (Table 4, col 2). Prior to enrollment, households were given roughly 2-weeks' notice about the treatment arm to which they were assigned. Although it is possible that households communicated, so there could be spillovers, in practice it appears that there were not learning or other spillovers on the enrollment margin.[41] Although we do not find significant spillovers in enrollment, we do

[38] 12.33 pp/40.09% that do not consume under sale of insurance = 30.76%.

[39] Our MPC estimate is unlikely to be a pure MPC for insurance: per capita incomes increased by more than insurance premiums over the last decade, yet India's out-of-pocket spending rate hovered around 75% during that period. There are more plausible, alternative explanations for the high estimated MPC: the sale-plus-transfer treatment relaxed liquidity constraints Berkouwer and Dean (2022); Casaburi and Willis (2018); it triggered goodwill towards the surveyors, who were also selling insurance; or it increased the salience of insurance through a "labeled transfer" effect (Benhassine et al., 2015).

[40] Of course, an additional issue with providing free insurance is distortions associated with raising revenue, while a concern with raising price is adverse selection; we return to these trade-offs below.

[41] This contrasts with the findings of Cai, De Janvry and Sadoulet (2015), who find local spillover effects in the decision to enroll in weather insurance in China. In that context, adoption spillovers were driven by diffusion of information from intensive information sessions. Our study did not provide equally intensive information, and baseline knowledge about RSBY is low (Berg et al., 2019).

find spillovers in utilization, which we discuss below.

4.2 Insurance and hospital utilization

We measure utilization along two dimensions. First, we examine utilization of insurance (use of the RSBY insurance card to pay for care) separately from utilization of hospital care (over-night stay or day surgery). These capture the effect of insurance at two margins: insurance can change financing of care, holding level of care constant; and affect the level of care. In addition, we distinguish successful and unsuccessful efforts to use insurance. The former affect insurer costs and beneficiary finances or care. The latter reflects either administrative failures or beneficiaries lack of knowledge about how to use insurance. In contrast to our enrollment analysis, we report effects of insurance access (ITT effect) and of insurance enrollment (CATE). The CATE provides a scaled version of ITT, to facilitate comparisons across different interventions which may change enrollment by different degrees. Some measures of use at 3.5 years differ from those at 18 months because we revised our endline surveys to address questions left unanswered in our midline survey.

Insurance utilization. We find that access to insurance substantially increased utilization of insurance for payment (ITT: Table 5). Access increased successful use of insurance at 18 months and 3.5 years in all arms. Because we measure utilization over the last 6 months, the annualized rate is double our estimates if rates are constant over a year. So, the average annual insurance utilization rate at 18 months (3.5 years) is 13.46% (2.56%) in the free-insurance arms versus 7.72% (0.64%) in the control arm. On average this effect amounts to a 74.35% (400%) increase in insurance utilization at 18 months (3.5 years).[42]

Insurance enrollment also increased use of insurance for payment (CATE: Table 6). It raised annual insurance utilization at 18 months to 12.04% (from a base of 7.76%) and at 3.5 years (for the most serious event only) to 3.04% (from a base of 0.98%).

Spillover effects play a quantitatively important role in utilization.[43] Table 5 reports direct, spillover, and total effects for treatment arms $i = 1, 2, 3$. [44] We find that the total effects of insurance access on our measures of utilization are significant, although ITT estimates of

[42]It may seem surprising that utilization is positive in the no-intervention arm given they did not get insurance through the study. However, it is possible households obtained RSBY coverage in the interim, e.g., by obtaining a BPL card or switching into an RSBY-eligible occupation.

[43]These effects correspond to β^h (direct), $\gamma^h + .9 \times \phi$ (spillover), and $\beta^h + .9 \times \phi + .9 \times \gamma^h$ (total) from estimating equation 1 with measures of utilization as outcomes. The terms for the village-level treatment intensity, ϕ, and its interaction with the household-level treatment indicators γ^h, are scaled by 0.9, the maximum in-sample share assigned to receive access to insurance.

[44]Recall that our exclusion restriction is that one household's access to insurance cannot affect enrollment or outcomes of other households, but the former's enrollment decision can. This permits, for example, compliers' enrollment and outcomes to affect the enrollment and outcomes of other households.

the direct effects of insurance access are not separately significant at conventional levels.[45] The CATE effects largely follow a similar pattern. In terms of magnitudes, the spillover effects are of comparable magnitudes as the direct effects: indirect effects, even though they are imprecisely estimated, are quantitatively important for generating the significant total effects for insurance utilization.[46]

This spillover result is unlikely to be due to supply constraints because we find that RSBY access and enrollment increase insurance utilization, but not hospitalization. These spillover effects are consistent with people learning about insurance via word-of-mouth from neighbors, as reported in prior studies of insurance in Karnataka and Andhra Pradesh (Chatterjee et al., 2018; Debnath and Jain, 2020).[47]

Hospital utilization. We estimate a positive but imprecise effect of insurance access and of enrollment on utilization of hospital care at 18 months (ITT: Tables 5, CATE: 6). One reason for the difference in significance between insurance and hospital use effects may be inadequate power. Our study was powered to detect a 25% change in hospitalization, off a baseline hospitalization rate of 10% annually per household of five persons and assuming 10% attrition from our sample. However, our point estimate suggests hospital utilization effects in the range of 10-15% of the control mean.

The greater utilization of insurance, combined with small and insignificant effects on hospital utilization, suggests that insurance access and enrollment might be expected to reduce out-of-pocket (OOP) payments. However, we do not find a large or significant effect on out-of-pocket payments (Tables A.4 (ITT) and A.5 (CATE)). There are several ways to reconcile these results. First, it is possible that insurance does increase healthcare utilization along the intensive margin (number of services per hospital visit), whereas we only measure the extensive margin (whether there was a visit). Second, respondents may also have increased spending on OOP costs that were complementary with covered care (such

[45]The ITT effect at 18 months for arm 3, and at 3.5 years for arm 2 are significant at 10% level.

[46]Total effects of insurance are estimated more precisely than the component parts — direct and spillover effects — for two reasons. Conceptually, the total effect leverages all of the variation in the data, from both village- and individual-level randomization. Econometrically, the covariance terms between β_h, γ_h and ϕ are negative, making the sum more precisely estimated than the components.

[47]To explore learning, we examined how direct and indirect utilization varies by wealth of households — wealth may be a proxy for experience with financial products. (We define a high (low) wealth household as a household with an above (below) median value of on a wealth index created by taking the average of the following: z-score of the value of farm animals, z-score of the amount of silver, z-score of the amount of gold, the average of z-scores of different durable goods (stoves, fridges, etc.), z-score of land, z-score of the number of rooms in the household, and z-score of savings.) We find that, at 18 months, high-wealth households experience a direct effect, but no indirect effect, on insurance use; low-wealth households experience the opposite (ITT: not reported, CATE: Table A.6). This evidence is consistent with uninformed, low-wealth households learning from others but informed, high-wealth households being relatively unaffected by the behavior of neighbors.

as medicines prescribed at the hospital but obtained elsewhere). Third, it is possible that hospitals increase the price charged to insured patients. RSBY pays below-market rates for health care and hospitals may charge patients for all or part of the difference between market and RSBY reimbursement rates. Dupas and Jain (2019) find evidence of such "balance billing" in Rajasthan.

Failed utilization of insurance. Many households had difficulty using their insurance to pay for healthcare. Access to free insurance increased by 3.34 pp annually the number of households who tried to use their insurance card by 18 months but were unable to do so, from a base of 2.68% in the control group.[48] This excess failure rate represents 50% of the successful utilization ITT effect. CATE results are qualitatively similar. This result is congruent with Banerjee et al. (2021b), who examine failure to enroll in insurance while this examines use of insurance.

Lack of knowledge about the purpose of insurance and how to use insurance are potential explanations for the failure rate. Because insurance is a relatively new product, both hospitals and beneficiaries may not know how to use it (Nandi et al., 2016; Rajasekhar et al., 2011; Berg et al., 2019). In our midline and endline surveys, we asked why households did not try to use their insurance card to pay for care and why they were unable to use the card even when they tried (Table A.7). The most frequent reasons given for not using the card were not knowing that the card could be used for insurance (15% at 18 months, 20% at 3.5 years), forgetting the card at home (13% at 18 months), and not knowing how or where to use the card (29% and 30% at 3.5 years). Besides these beneficiary-side problems, there were also supply-side problems. Of those who tried to use the card, 55% and 69% said that the doctor did not accept the card at 18 months and 3.5 years respectively, and 12% said that the insurance company did not accept the card (i.e., did not approve use) at 3.5 years.[49] This finding suggests that interventions on both the demand-side (e.g.,education) and supply-side (logistics) may be important for raising utilization of, and thus demand for, insurance in India and similarly situated countries.[50]

[48] Although no household in the control enrolled at baseline, some households in that group may have fallen below the poverty line and become eligible for RSBY by midline.

[49] These should be interpreted with caution because we do not know if doctors correctly did not approve the card because a service was truly not covered, or incorrectly/strategically did so. Such strategic behavior has been documented in other medical contexts: Alexander (2020) shows that doctors in the US strategically turn away patients who are unlikely to generate large billable fees.

[50] We find no evidence that a lower absolute effect of insurance access or enrollment on insurance utilization at 3.5 years was due either to earlier negative experience with failed efforts to use insurance, or to reduced need for care due to earlier successful use of insurance. Specifically, we find no sizable or significant correlation between insurance use at 3.5 years and either (a) failed or (b) successful insurance use at 18 months (unreported). The absence of a significant effect of insurance access or enrollment on hospital use underscores this conclusion.

4.3 Health outcomes

Access to insurance had few significant effects on health at 18 months or 3.5 years. Having measured (a) 3 parameters (direct/indirect/total effects) for (b) 3 ITT effects (one per treatment arm) and one CATE effect (effect of insurance enrollment) for (c) 82 specified outcomes over 2 surveys, only 3 estimated coefficients (0.46% of all estimated coefficients concerning health outcomes) were significant after multiple-testing adjustments.[51] Table 7 provides a summary and Tables A.8-A.11 report estimates of ITT effects and CATE.[52] We cannot reject the hypothesis that the distribution of p-values from these estimates is consistent with no differences ($p = 0.31$). We also find no effects on our summary index of health outcomes.

Take care when interpreting our insignificant health effect estimates. First, despite our large sample size, we lack power to detect the effect of insurance on health, because insurance has a small effect on the level of hospital care or hospital care has small effects on each health outcome (Das, Hammer and Leonard, 2008). Recall, our study was powered to detect a change in hospitalization rates, not health.

Second, our estimated confidence intervals cannot rule out medically significant effects for many outcomes. On average, the absolute value of an estimated ITT effect (CATE) for an outcome equals 11% (8.8%) the standard deviation of the outcome. In the appendix, we provide 95% confidence intervals for each outcome so that one can see what effects on health can be ruled out if one's null hypothesis is that there are positive effects on health.

Third, we cannot rule out both positive effects for some outcomes and negative effects for others. Depending on what we classify as a negative outcome, 23.6-47.2% of midline ITT estimates and 41.3-57.8% of endline ITT estimates imply negative (though typically insignificant) effects on health.[53] To visually illustrate the roughly balanced positive and negative range of nominal effects, we report coefficient plots of CATE estimates of the total effect (direct plus spillover effect) of insurance enrollment on a range of randomly selected outcomes at midline and endline (Figures A.3 and A.4).

Our findings are roughly consistent with prior RCTs of health insurance in low and middle

[51] Even if we do not adjust for multiple testing, only 55 (8.38%) out of 738 estimated parameters are significant (Table A.12).

[52] Table 7 reports results where the impact of access or enrollment on a given outcome is the same for women and men and each person is given equal weight. We obtain similar results – in terms of fraction of significant coefficients – if we estimate separate regression models for women and for men (not reported) .

[53] These numbers include direct effects, indirect effects, and total effects, as we define them for our ITT estimates. If we classify being diagnosed with a condition as a negative statement about health (i.e., falling ill), but taking medicine as positive for health (i.e., getting treatment), then the percentage of ITT estimates implying a reduction in health is 47.2% and 57.8% at midline and endline, respectively. If neither diagnosis nor being on medication for an ailment is treated as a negative outcome, these percentages are 23.6 and 41.3%, respectively. If being diagnosed is treated as neutral for health (i.e., diagnosis may be good or bad), but being given medicine as positive for health, then the numbers are 34 and 50%.

income countries, none of which find significant effects on health. Our findings contrast with one other (observational) study in India that does find significant mortality effects from health insurance (Sood et al., 2014); however, that study examined a more generous insurance product that covered long-term or chronic hospital stays rather than short-term, acute-care stays.

4.4 Adverse selection

We find significant adverse selection into insurance at midline. Recall that our test for adverse selection is whether enrollees in arms 2 and 3, which impose a positive price on insurance, have higher expected cost than enrollees in arm 1, which offers free insurance. This test is equivalent to tests for whether $\phi_2 > 0$ and $\phi_3 > 0$ in regression equation 3. Table 8 presents estimates of this regression. Columns 1 and 2 use predicted midline spending; this is our preferred measure as households likely find it easier to predict expenses in the year following enrollment than expenses 3 years in the future. Column 1 shows that, while a household with the mean level of predicted spending is 19 percentage points (pp) less likely to enroll under the sale-of-insurance treatment (arm C) than under the free-insurance treatment (arm A), a one standard deviation (SD) increase in predicted spending increases enrollment in that arm by 6 pp ($p < .01$) – equivalent to 10.1% of the mean enrollment rate in arm C. Moreover, while an average expected-spending household in sale-plus-transfer treatment (arm B) is 6.6 pp less likely to enroll in RSBY, a one SD increase in predicted spending in that arm increases enrollment by 2.9 pp [54]

Our measure of predicted health spending may be correlated with other household characteristics that may affect willingness/ability to pay for insurance. To investigate whether costs *per se* rise with price, Column 2 includes controls for the household's baseline asset holdings, the education level of the household head, the head's score on a Raven's matrices test, and a measure of risk aversion. The magnitude and significance of the interactions between the positive-price insurance arms (B and C) and the predicted spending measure remain quite similar, suggesting that predicted health spending is not simply proxying for wealth, education, financial sophistication, or risk tolerance.[55]

[54] A one-sided test of the hypothesis that the degree of adverse selection is greater under Arm C than Arm B has a p-value of 0.031. Adverse selection may be mitigated by provision of a cash transfer if those with lower predicted spending are more liquidity constrained or more responsive to flypaper or labeled transfer effects.

[55] Conceptually, adverse selection is present to the extent that increased price causes those with higher costs to remain in the enrolled pool with higher probability than those with lower costs, regardless of what the drivers of cost are (worse health, greater sophistication about using insurance, etc). The results in Columns 2 and 4 demonstrate that selection on predicted cost is present after controlling for the direct effects of wealth, cognitive measures, education and risk aversion.

Columns 3 and 4 show results obtained using predicted endline spending rather than midline. The magnitude of adverse selection is roughly half as large, consistent with the fact that it is likely more difficult for households to predict their spending 3.5 years in the future relative to 1.5 years out.[56]

Price and utilization. An additional feature of the data are consistent with adverse selection. The need to pay a higher price may select in those with greater motivation/ability to navigate the process of successfully utilizing insurance and, on the flip side, those who are less likely to experience failed utilization attempts. As shown in Table 5, the direct effect of access to paid insurance (arm 3) on successful utilization of insurance is 7.67%, almost 3 times as great as for free insurance (arm 1); a one-sided test of equality vs. the alternative of greater successful utilization in the paid arm is rejected with $p = 0.06$. Likewise, the effect of assignment to free insurance on failed use is significantly larger than that of assignment to paid insurance (3.57% vs. 2.57%); a one-sided test yields a p-value of 0.05.[57]

5 Welfare analysis

We use the marginal value of public funds (MVPF) framework (Hendren, 2016; Finkelstein and Hendren, 2020) to calculate whether offering free RSBY insurance improves welfare as well as to estimate the optimal premium. The MVPF framework is commonly used in the public economics literature for estimating the welfare benefits of government policies (Hendren and Sprung-Keyser, 2020b), especially public insurance (Finkelstein, Hendren and Luttmer, 2019; Goodman-Bacon, 2021; Deshpande and Lockwood, 2022). We present our measures of marginal social benefits and costs here, and provide details on our derivation of those measures in Appendix 5.

5.1 Does offering public insurance improve welfare?

We first calculate the marginal social benefit (MSB) of offering a public insurance product at price p (versus not offering insurance). This will be equal to the consumer surplus for that

[56]Consistent with the difficulty of forecasting medical spending 3.5 years in the future, LASSO models select only the intercept when we predict endline spending with baseline variables. We therefore construct predicted endline spending using the set of covariates which are selected by LASSO to predict midline spending.

[57]Unlike our core test of adverse selection, these results on successful vs. failed utilization use an *ex post* measure and as such cannot separate selection effects — differential selection of those who are, at enrollment, more vs. less likely to successfully utilize — from treatment effects, wherein being offered a higher or lower price causes changes in, for instance, motivation to study and remember the rules via a mechanism such as sunk cost effects or price signaling value. Disentangling these is beyond the scope of our analysis as we do not have separate variation in offer prices vs. transaction prices.

insurance product.[58] Then we calculate the marginal social cost (MSC) of offering insurance at price p (versus not). This is equal to the cost of covered healthcare minus revenue from premiums. We then estimate whether MSB is greater than MSC at a zero premium using our estimates of demand and insurance cost; we evaluate welfare at $p = 0$ because that is the price the government charged when it expanded RSBY (the program we evaluate) to include above-poverty-line households in PMJAY (the successor to RSBY). We do this exercise, first, using information that consumers have at time of enrollment. Then we repeat it assuming consumers do not have any information on their expected costs. The former case generates ex what Hendren (2021) calls *ex post* demand, the latter *ex ante* demand.

Marginal social benefit. Our measure of the marginal social benefit of providing public insurance at price p (versus not providing public insurance) is aggregate willingness to pay for insurance with that price, equivalent to the area under the aggregate Hicksian demand curve for insurance above p:

$$MSB(p) \approx \int_0^{G(p)} [D^H(q) - p]dq = \int_0^{G(p)} D^H(q)dq - pG(p). \tag{4}$$

where $D^H(q)$ is the inverse demand curve for the government service and $G(p)$ is quantity demanded. Without loss of generality, normalize quantity by population so that the units of G are fraction of the population. The marginal social benefit from free insurance is this expression evaluated at $p = 0$.

Marginal social cost. We assume that the government pays for insurance thorough a combination of charged premium p (i.e., a user fee) and financing at a marginal cost of public funds of r, rather than taxes. The marginal social cost of funds spent on providing the government service is then the sum of the government's net expenditure on all consumers who purchase the government service at price p:

$$MSC(p) = (1 + r)[c^{AC}(p)G(p) - pG(p)] \tag{5}$$

where $c^{AC}(p)$ is the average cost across all beneficiaries that enroll in insurance at price p. The first element in square brackets is the average cost of providing insurance amongst those who take it up at price p; the second element is the government's revenue from user fees. The net is scaled up by $1 + r$ to account for the government's cost of financing.

[58] Marginal social impact in the MVPF framework is not our estimate of health impacts of insurance. Due to issues of power, we may not have power to estimate that with adequate precision. Moreover, there may be health benefits we did not attempt to measure, or could not measure well (e.g., stress). Insurance may have non-health benefits, e.g., smoothing non-medical consumption. Finally, in a subjective utility framework, a product may offer subjective benefits that do not match objective benefits, but which still count towards utility and welfare.

MVPF of offering public insurance. The decision to provide RSBY insurance at p is welfare-improving if the marginal social benefit is greater than the marginal social cost: MSB(p) / MSC(p) ≥ 1. In the special case where $p = 0$, which is policy-relevant and one we evaluate with data, MVPF is positive if

$$\int_0^{G(0)} D^H(q)dq \geq (1+r)c^{AC}(0)G(0) \tag{6}$$

This condition is not exactly the same as the condition that social surplus should be positive at $G(0)$ because the costs are inflated by $1+r$ due to government borrowing on behalf of consumers. This is the so-called fiscal externality in the MVPF framework.

We can illustrate the MVPF trade-off in Figure 4. Panel A illustrates the calculation using demand and cost curves from midline. The MSB is the area under the aggregate inverse demand curve between quantity demanded at zero price ($q = 0.79$) and zero quantity. The MSC is (i) the area under the marginal cost curve in the same range of quantity times (ii) $(1+r)$, which captures the cost of public funds. The difference between the marginal cost curve in this figure and in our calculation ($c^{MC}(p)$) is that the figure shows the marginal cost curve as a function of quantity demand rather than price, i.e., it shows $c^{MC}(D(G(p))$. The MVPF can also be illustrated with the average cost curve. The MSB is the same. But the MSC is now (i) the average cost at the zero offer price (≈ 500 INR) times the quantity demanded at that price ($q = 0.79$), times (ii) $(1+r)$. The MSC, and thus the result of the MVPF calculation, is the same whether one uses the marginal or average cost curve.[59]

[59] This figure also allows us to connect the MVPF calculation to the framework of Einav, Finkelstein and Cullen (2010) and Einav and Finkelstein (2011), which use price theory graphs to illustrate adverse selection in insurance markets. That paper shows, first, that adverse selection can be captured by the downward sloping marginal cost curve, which implies a downward sloping average cost curve. In a market with community-rated insurance, price is equal to average cost, which lies above the marginal price curve. This means a sub-optimal number of individuals will purchase insurance in private market ($q \approx 0.30$). In Panel A, we show a marginal cost curve always below demand, implying everyone with positive WTP should buy insurance in a private market.

Second, Einav and Finkelstein (2011) notes that if a law mandated that everyone purchase insurance (a so-called insurance mandate), it may increase insurance consumption beyond the point at which marginal cost intersects demand (to, e.g., 100% of the population), i.e., it would force inefficient consumption. The net welfare effect might still be positive if the area under the demand curve were greater than the area under the marginal cost curve over the entire population. In our context, there is free insurance, not a mandate. Assume there is zero price private insurance. This causes everyone with positive WTP – not 100% of the population – to take up insurance. Although Panel A does not depict it, if the marginal cost curve intersected demand above zero price, then free private insurance would cause excess consumption of insurance. However, that could be a net positive for welfare (relative to no insurance) if the area under the demand curve were greater than the area under the marginal cost curve.

An important difference between the MVPF framework and the Einav-Finkelstein-Cullen framework is that MSC is greater than the area under the marginal cost curve because the cost of public funds. The Einav-Finkelstein framework considers the value of private insurance paid for by the consumer, so it does not need to include the cost of public funds. The implication is that whereas the Einav-Finkelstein framework says

MVPF of offering public insurance with *ex ante* value of insurance. The condition above for whether free insurance is a positive MVPF policy uses an *ex post* valuation of insurance, *i.e.*, after individuals have some private information about their expected costs, though before the insurance contract is signed and all covered costs are revealed. The willingness to pay for insurance may be higher if measured behind a veil of ignorance, before consumers have any private information on their expected costs (Hendren, 2021), and if there is adverse selection. After some information is revealed, those with low expected cost may decide not to buy insurance, raising the average cost and thus premiums for the remaining population. Before information is revealed, consumers know there is a risk that information will reveal they have high expected costs, causing them to face a higher premium. If they are risk averse, consumers will be willing to pay some amount to avoid the risk of a higher premium. This amount is proportional to the difference in the marginal utility of those who buy insurance after the cost realization and those who do not, because the former pay higher prices (and have lower consumption) and the latter get the opposite.

Hendren (2021) provides an adjustment to *ex post* inverse demand – demand after some information is revealed – that transforms it into *ex ante* (EA) inverse demand before information is revealed. This adjustment shifts demand from $D^H(G)$ to

$$D^{H,EA}(G) = D^H(G) + M(G) \quad \text{where} \quad M(G) = G(1-G)\left|\frac{\partial D^H}{\partial G}\right|\beta(G) \tag{7}$$

where the additive markup $M(G)$ is intended to capture the additional amount households are willing to pay to avoid variation in insurance prices after their expected costs become apparent, and $\beta(G)$ is the percentage difference in marginal utilities of income for the insured relative to the uninsured.[60] If there is adverse selection, then information revelation increases the premiums for those who enroll. This implies that enrollees have lower consumption and higher marginal utility of consumption than non-enrollees, so that $\beta(G)$ is positive. Moreover, under some assumptions,[61] a first-order Taylor approximation of $\beta(G(p))$ is $\sigma[D^H(G(p)) - \int_{G(0)}^{G(p)} D^H(s)ds]$, the difference between willingness to pay of the marginal enrollee at price p and the average willingness to pay of non-enrollees at that price, scaled by σ, the coefficient of absolute risk aversion.

Replacing $D^H(q)$ with $D^{H,EA}$ in Eq. (6) gives the condition for when free insurance

the optimal level of consumption is where the marginal cost curve intersects the demand curve, the MVPF framework says the optimal consumption level is where $(1+r)$ times the marginal cost curve intersects the demand curve.

[60] See Propositions 1 and 2 in Hendren (2021).

[61] The two assumptions are that the utility from non-medical consumption and health are separable and that there are no differences in average income of the insured and uninsured. Propositions 3 and 4 in Hendren (2021) derive the approximation.

improves welfare from an ex ante perspective.

Application. *Ex post value.* We apply the analysis above to our context by, first, estimating individual demand curves and average cost curves. We impose a linear specification for both curves, partly because we only observe behavior at two prices (zero premium in group A and full premium in groups B and C) and partly to keep things simple:

$$g_i(p_i) = g(0) - \gamma p_i + \epsilon_i \tag{8}$$

$$c_i^{AC}(p_i) = c^{AC}(0) + \kappa p_i + \upsilon_i \tag{9}$$

where i indexes households, $g_i(p_i) \in \{0, 1\}$ is household i's demand, p_i is the premium in the insurance-access treatment group to which the households is assigned, c_i^{AC} measures an enrolled household's cost to the insurance pool, $\gamma > 0$ measures the slope of demand, and $\kappa > 0$ if there is adverse selection.[62]

We simultaneously estimate both equations using linear regression. Household demand is a binary variable that indicates enrollment. We estimate the demand equation on two samples. One is a sample that includes the (A) free-insurance and (C) sale-of-insurance groups. This measures Marshallian demand. If there are no income effects, a common assumption in demand estimation in health economics, this also measures compensated, Hicksian demand. Since income effects are likely positive, this demand curve can also be considered a lower bound on Hicksian demand. The second sample includes the (A) free-insurance group and (B) the sale-plus-transfer group. This second sample provides an upper bound on Hicksian demand because individuals are "compensated" with an unconditional transfer equal to the premium even if they do not purchase insurance.

Household average cost to the insurance pool is measured in two ways. One is hospital expenditure at midline (18 months), the other is hospital expenditure at endline (3.5 years).[63]

[62] Our average cost equation does not link average cost to quantity but to price. However, we can use it just the same in our MVPF calculation. We cannot identify a cost-quantity equation because quantity is not exogenous. Instead, we estimate the reduced form equation (5) and then use the demand equation (8) to translate price to quantity when calculating MVPF use the Delta Method to calculate the asymptotic standard error of our MVPF estimate.

[63] Our measure of midline cost is two times the product of (a) a household's hospital expenditure at midline and (b) whether the household used RSBY insurance to pay for care in the last 6 months before midline. We use endline hospital expenditures rather than midline medical expenditure because we did not define hospital expenditure at midline and many households labeled non-acute clinic care as hospital care. We corrected the survey at endline by defining hospital care as care at a facility with overnight stays. Our measure of midline hospital costs assumes midline and endline hospital costs are roughly the same given the long time-span between them. We convert endline hospital expenditures to midline costs from use by multiplying endline hospital expenditures by midline use of RSBY to pay for hospital care. We multiply by two to get annual costs because endline use, a binary variable, covers use over 6 months rather than 12 months.

Our measure of endline cost is two times the product of (a) a household's hospital expenditure at endline and (b) whether the household used RSBY insurance to pay for care in the last 6 months before endline.

Neither is the measure we use for estimating adverse selection in Section 4.4. There the goal was to use predicted expenditure based on information available prior to enrollment and to avoid including ex post moral hazard in our prediction; here want to use actual expenditure to measure actual cost, including moral hazard, because that contributes to the insurance company's cost. The sample for the cost regression only includes households in treatment arms (groups A and C, not the control group) that actually enrolled in insurance. We exclude the sale-plus-transfer group because, outside this experiment, uncompensated Marshallian demand determines enrollment. We exclude the control group because they cannot enroll under our experiment.

In both regressions, the free insurance group A is coded as paying a price of 0 and the sale-plus-transfer and sale-of-insurance groups B and C are coded as paying a price of 203 in Mysore and 173 in Gulbarga. This is the government premium in each district plus the INR 30 paid for the insurance card. We cluster standard errors at the village level (as in our ITT model) to address within-village spillovers. However, we do not include village allocations in our specification. Because allocation is also randomized, its omission should not cause bias. One should interpret the results as the total effect of price including a full direct effect and 80% of an indirect effect, because on average 80% of other sample households receiving some access to insurance across arms.

We can plug our estimates of Eqs. (8) and (9) (which are reported in Table 9 and plotted in panels A and B of Figure 4) into Eq. (6) to obtain the condition required for free insurance to be a welfare-improving use of public funds from the perspective of *ex post* demand:

$$\frac{g(0)}{2\gamma} \geq (1+r)c^{AC}(0) \tag{10}$$

Using a MCPF of $r = 1.25$ to account for the inefficiency of lower-income country tax systems (like the one in India[64]) and the free-insurance arm (A) and sale-plus-transfer arm (B) to estimate demand,[65] we calculate that free public insurance reduces welfare by INR 63 at

We provide MVPF calculations using using other measures of cost, including midline medical expenditures, in the appendix (Table A.14). While cost curves differ, the MVPF calculations yield similar results.

[64]Our estimate is at the lower end of the range used for MCPF for such countries. Historical data on the MCPF from India suggest an $r = 1.5$, but those estimates are from the mid-1980s (Ahmad and Stern, 1987). A more recent survey of the MCPF from Africa suggests a median estimate of $r = 1$ (Auriol and Warlters, 2012). (Basri et al., 2021), studying the optimal tax rate in Indonesia, focus on the cases where $r = 1.5$ and 2. These estimates are higher than convention for US-focused studies, which use $r = 0.3$ (Finkelstein and Hendren, 2020).

[65]We use arm B rather than sale-of-insurance arm C because it is a more realistic estimate of demand. As Table A.14 shows, demand is greater and has flatter slope when using arm B enrollment. While group B overestimates Hicksian demand, group C underestimates it. We believe that the latter error is more serious. Some of the lower uptake in C is driven by liquidity constraints, which we think would be relaxed in an expansion of RSBY: enrollment drives would be predictable and the PMJAy program that replaced RSBY

midline (though our estimate is imprecise[66]) and increases welfare by INR 516 at endline (Table 9, row 1).

Our findings are driven by the level of utilization, costs that the government must finance, and by the marginal cost of public funds. If we include in our measure of utilization not just successful utilization, but also failed attempts at utilization, then free insurance reduces welfare by 534 INR even at endline (Table 9, column 4). Moreover, panels C and D of Figure 4 plot the welfare from free insurance (magenta line) as the MCPF increases. Welfare falls as r rises because the costs of financing utilization increases. An important policy implication is that even if public insurance is not valuable today, it may become valuable as the tax system becomes more efficient.

To further determine how robust our findings are, we calculate the *ex post* value of free insurance use sale-of-insurance arm (C) rather than arm B to estimate demand and other measures of hospitalization costs (see Appendix table A.14). Our results are qualitatively similar: free insurance lowers welfare at our realistic marginal costs of public funds (MCPF). Versions of panels C and D in 4 for those alternative demand and cost curves (not reported) show that free insurance loses value as MCPF rises. However, in some cases, when MCPF is sufficiently low (usually around levels observed in higher-income countries), free insurance can improve welfare, much as what we see in panel C.

Ex ante value. Observed measures of demand/willingness to pay for insurance derived from households' choices are, necessarily, based on the information set those households have at the time they make the choice. The fact that households have some information about their risk/expected costs may drive adverse selection; however, this information, which is known at the time of the observed insurance choice, would have been insurable *ex ante* (Hirshleifer, 1971; Hendren, 2021). Thus the *ex ante* value of insurance may be greater than the *ex post* value. To calculate the *ex ante* value of free insurance, we take three additional steps. First, we solve the linear demand in Eq. (8) for price. Second, we estimate the markup $M(G)$ using the first-order Taylor approximation for $\beta(G)$ suggested by Hendren (2021). We add this to the price estimated in the first step to obtain the function $D^{H,EA}(G)$.[67] We illustrate this *ex ante* demand with the dashed blue line in Panels A and B of Figure 4. Third, we take a first-order Taylor approximation of the markup. This reveals that, if we

allowed year-round enrollment at hospitals when case was required. Moreover, the magnitude of the premium in our study ($\sim$ 200 INR) is small relative to annual income, so likely has a small income effect.

[66]Standard errors on welfare calculations are obtained using the Delta method.

[67]See Appendix B.1.1 for details.

define

$$\frac{1}{\gamma^{EA}} = \frac{1}{\gamma}\left(1 + \frac{\sigma}{\gamma}G(0)[1 - G(0)]\right) > \frac{1}{\gamma} > 0 \tag{11}$$

we can replace γ with γ^{EA} in Eq. (10) to approximate the value of free insurance. In calculations, we assume a coefficient of risk aversion of $\sigma = 5 \times 10^{-3}$, in the range of estimates in India and other lower-income countries (Binswanger, 1981; Just and Lybbert, 2012). The resulting welfare calculations for the case where $r = 1.25$ are presented in the second panel of Table 9. *Ex ante* demand is sufficiently greater than *ex post* demand that free insurance is now always positive for welfare. Panels C and D of Figure 4 show that using ex ante demand to value free insurance raises the welfare value of free insurance at all levels of r. However, using midline estimates of cost, free insurance remains a welfare-reducing policy for MCPF above $r = 1.5$.

5.2 What is the optimal public insurance premium?

In this section, we modify our MVPF analysis to ask: what is the optimal price of RSBY insurance, conditional on offering it. We first derive optimal price using *ex post* demand, and then with *ex ante* demand.

Marginal social benefit. The marginal social benefit of *increasing* the price of the government service is the sum of private benefits across individuals who buy the service:

$$MSB(p) = -\sum_i \mathbb{I}(g_i^* = 1) = -G(p). \tag{12}$$

where we are again working with ex post (Marshallian) demand. This is proportional to aggregate quantity demanded at price p due to Roy's identity. Intuitively, raising price by INR 1 does not change demand much, but does reduce WTP by 1 INR for each unit purchased. Note that this MSB is different than the MSB of offering a free service, which was the area under the demand curve. The reason for the difference is the counterfactual. Before it was no service at all, while here it is differently (lower) priced service.

Marginal social cost. The social cost of providing the government service at price p is equal to $SC(p) = (1 + r)[c^{AC}(p) - p]G(p)$. This is identical to the marginal social cost in the last section because the comparison in the last section was providing insurance or not. Here we assume insurance is provided, but compare value at a higher or lower price. So the marginal social cost is the derivative of social cost in this paragraph with respect to price:

$$MSC(p) = (1+r)\frac{\partial c^{AC}(p)}{\partial p}G(p) - (1+r)G(p) + (1+r)[c^{AC}(p) - p]\frac{\partial G(p)}{\partial p} \tag{13}$$

The first term is new and reflects the change in average cost as price increase. Importantly, this is positive if there the service is insurance and there is adverse selection. The second term is the reduction in expenditure as revenues rise. The third term reflects the reduction in sales with higher price. This is negative if average costs exceed price. (If price were zero to start, then the first term would be positive, but the latter two negative.)

Optimal price. The first-order, necessary condition for p^* to be the optimal price for the government service is that the marginal social benefit equals the marginal social cost of that service: $MSB(p^*) - MSC(p^*) = 0$, i.e.,

$$-\frac{\partial c^{AC}(p^*)}{\partial p^*}G(p^*) + \frac{r}{1+r}G(p^*) - [c^{AC}(p^*) - p^*]\frac{\partial G(p^*)}{\partial p^*} = 0 \tag{14}$$

The incremental cost to consumers ($G(p^*)$) of raising the price of government service is just a transfer to the government, reducing its expenses, so those terms cancel in the second line. What remains is the government's saving on borrowing costs ($rG(p^*)$). Dividing by $(1+r)$ gives the trade-off in rupees before financing costs. An important constraint on the optimal price is that it must be greater than 0. If it falls below 0, price is below marginal cost, which is non-negative, and incremental consumption produces a social loss.

Optimal price using the *ex ante* value of insurance. The condition above for the optimal price for the government service uses an *ex post* valuation of insurance, *i.e.*, after individuals have some private information about their expected costs. The willingness to pay for insurance is higher if measured behind a veil of ignorance. This will affect both the marginal social benefits and costs of raising the price of insurance. To implement this change we need to invert the Marshallian analogue to the demand function in Eq. (7). To do so define $D^{EA}(p) = D^M(G) + G(1-G)[\partial D^M(G)/\partial p]\beta(G)$ and $G^{EA}(p)$ as the inverse of this function. The optimal price is given by a version of Eq. (14) that replaces $G(p^*)$ and $\partial G/\partial p^*$ with $G^{EA}(p^*)$ and $\partial G^{(EA)}(p^*)/\partial p^*$.

Application. *Ex post value.* Using the linear demand (8) and average cost (9) curves we estimated with our experiment, we can solve analytically for the price that balances marginal social costs and benefit using the *ex post* value of insurance:

$$p^* = \frac{\left[\kappa - \frac{r}{1+r}\right]g(0) - c^{AC}(0)\gamma}{\left[\kappa - \frac{r}{1+r}\right]\gamma + [\kappa - 1]\gamma} \tag{15}$$

This is a maximum if the second order condition is satisfied:

$$\kappa < 1 - \frac{1}{2}\left(\frac{1}{1+r}\right) \tag{16}$$

i.e., adverse selection is not too severe.

With $r = 1.25$, we estimate that the optimal price at is 1052 INR ($p = 0.084$) and and 528 INR ($p = 0.639$) (Table 9, row 2). Adverse selection is sufficiently low that the optimal price rises with costs (columns 3 and 4).[68] Panels C and D of figure 4 show that optimal price rises with the MCPF. As financing costs rise, the government wants to reduce public expenditures. Since we have fixed the RSBY contract, the government cannot reduce its healthcare spending via cost-sharing. But it can raise more revenue by charging consumers a higher premium.

Our main take-away from our MVPF calculations using the *ex post* value of insurance is that, although estimated levels of adverse selection limits the value of raising premiums, high financing costs swamp the benefits of subsidized public insurance in India.

Ex ante value. To calculate the optimal price using the *ex ante* value of insurance, before consumers have any information on their own expected cost, we take three steps. First, we invert our estimate of the linear function $D^{H,EA}$ from our value-of-free-insurance calculations, so we have quantity as a function of price $G^{EA}(p)$. Second, we replace $G(p^*)$ with $G^EA(p^*)$ in the first-order condition (15) and solve for p^*. The resulting welfare calculations for the case where $r = 1.25$ are presented in the second panel of Table 9.

Although free public insurance is a positive MVPF policy, the optimal price for public insurance is still positive, though generally below where the optimal price using *ex post* demand. Panels C and D of Figure 4 show that using the *ex ante* demand function increases the range of r (MCPF) over which free insurance is optimal. However, for higher r, the optimal price actually increases. The reason is that *ex ante* demand is greater, but has lower slope – which exerts mixed effects on the marginal social cost of raising premiums in Eq. (13). Moreover, the marginal social cost function weights the effects of demand and the slope of demand with the MCPF.

6 Conclusion

This paper reports on a large-scale RCT that estimates a range of policy-relevant parameters concerning health insurance in India. We estimate substantial demand for insurance, but also

[68] We obtain qualitatively similar optimal prices when we use the sale-of-insurance arm (C) to and/or other measures of ex post costs to estimate demand and average costs. See Table A.14.

adverse selection into insurance. We estimate that insurance enrollment increases insurance utilization, but that many beneficiaries are unable to use insurance to pay for care. Finally, although there is demand for insurance, we do not find systematic positive effects of insurance on health. Combining these findings with realistic estimates of the cost of public funds in India, we calculate that the optimal premium for insurance in our study sample is not zero, which is the price of insurance under the RSBY program we study as well as its successor program PMJAY. These findings contribute to several strands of the literature on health insurance.

Our study has a number of limitations. First, it examines a limited product (hospital insurance that excludes primary care or drug coverage) for a limited population (above-poverty line households rather than below-poverty line households). Second, while our study is able to estimate a Marshallian demand function for insurance, it is based on just two price points and our study is only able to set identify, rather than point identify, a Hicksian demand function. Third, our measures of ex post medical expenditure, including out-of-pocket (OOP) expenditure, are limited or noisy. While we are able to demonstrate adverse selection based on ex ante expenditure, we are unable to estimate precisely effects on post-enrollment expenditure. (All the ex post measures of cost show similar optimal prices for public insurance, at reasonable values of the marginal cost of public funds for India.) Fourth, although our RCT has a large sample size, it is still not powered to detect many health effects. Moreover, we only measure effects on health out to roughly 4 years. There may be longer-term effects of insurance.

Our study also raises a number of questions that warrant further investigation, beyond research that just addresses the limitations of this paper. First, does experience with insurance increase utilization over time? Second, are supply constraints a limit on the utilization effects of insurance? Third, how much would co-insurance (deductibles or co-pays) affect utilization of insurance? Relatedly, how does raising the maximum coverage, which PMJAY (the program that replaced RSBY does) affect utilization? Fourth, is lack of power, low-productivity of healthcare, or substitution of insurance for OOP payments responsible for the common finding that health insurance does not significantly and systematically improve health? Finally, because policy-making is sensitive to the marginal cost of public funds, what is the government's cost of capital in India, and in lower-income countries?

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Figures

Figure 1: Timeline of study.

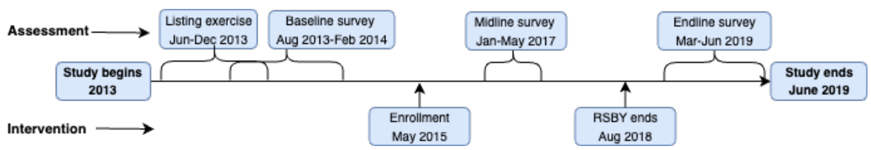
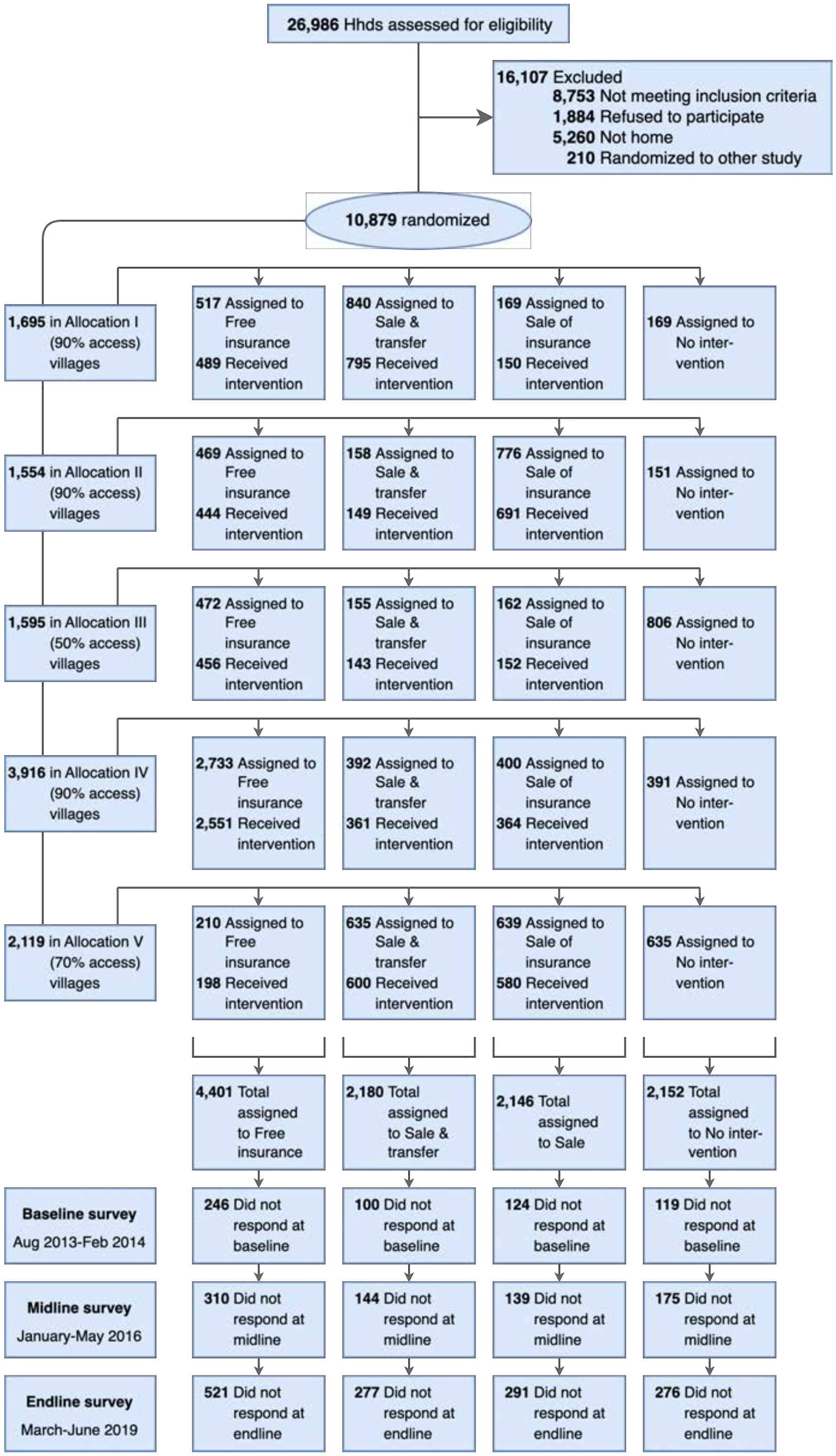


Figure 2: Subject flowchart, with exclusions and attrition by endline.



Notes. In column 1, X% "access villages" means that in these villages, X% of households were assigned to either groups A, B, or C. In rows 1-5, the difference between N "Assigned to" a group and M "Received intervention" is that our randomization algorithm assigned M to the relevant group but that, when we went to the village to inform households of their method of access and to enroll household, we were only able to contact M our of N of those households.

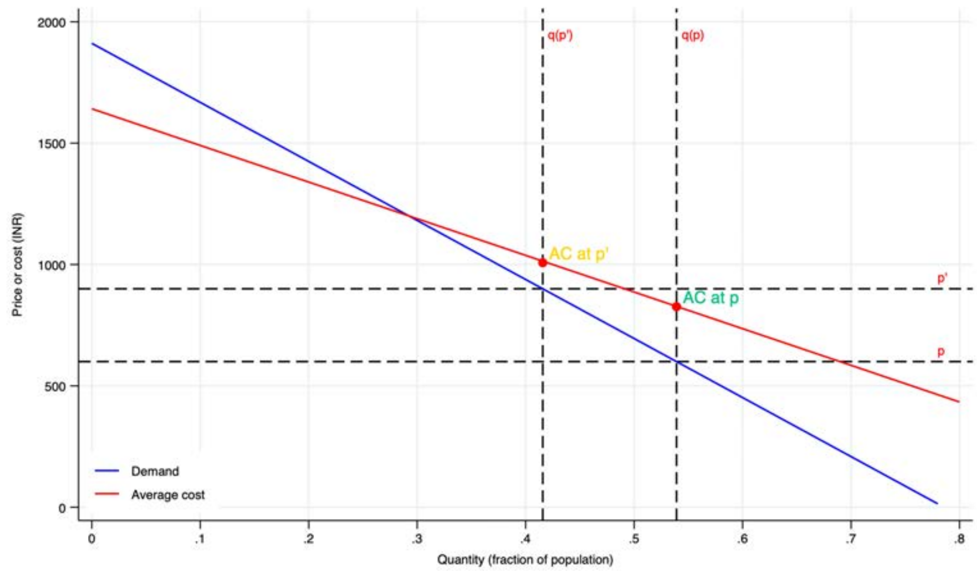
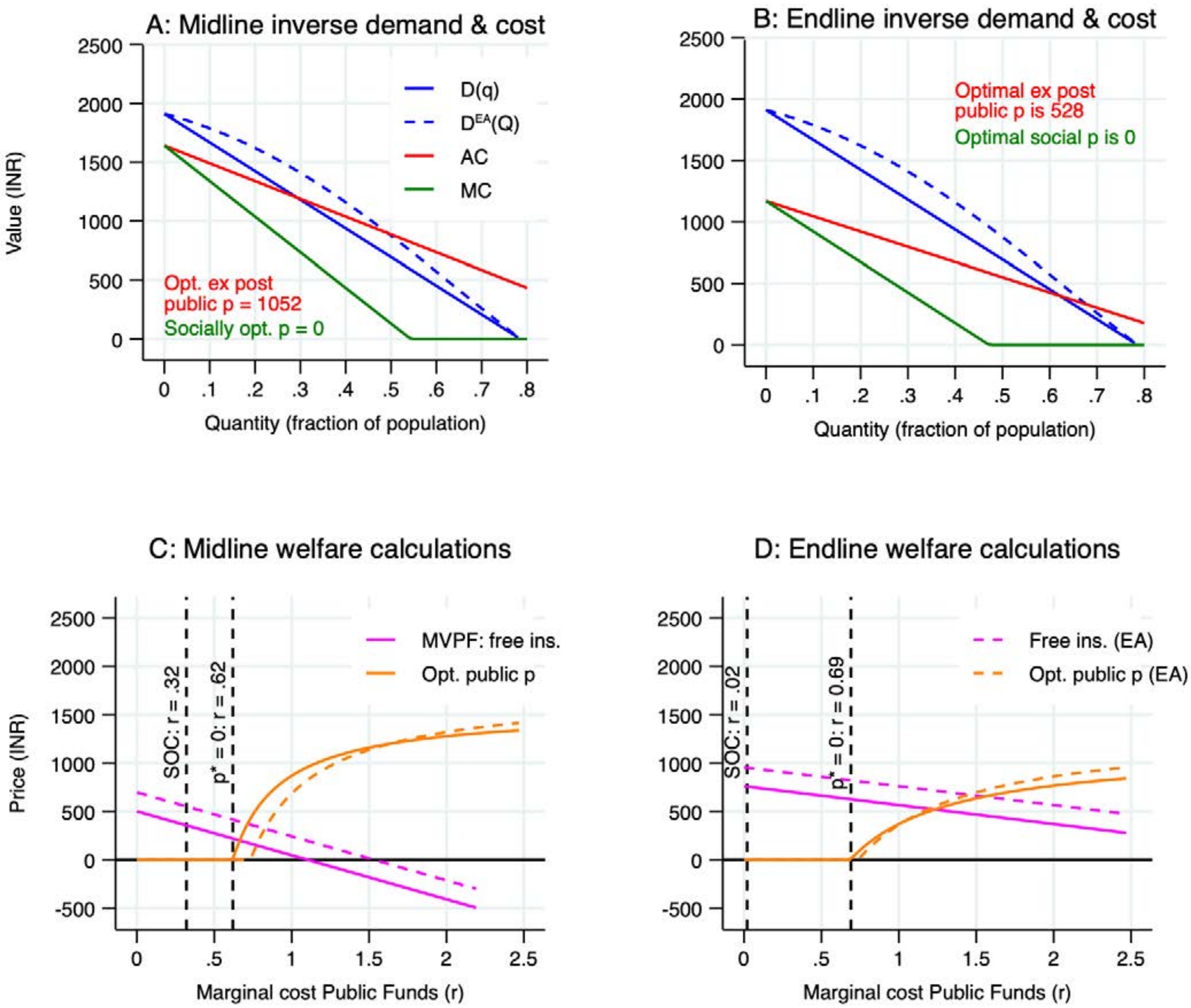


Figure 3: Illustration of adverse selection

Notes. This figure illustrates how we identify the existence of adverse selection. It mimics demand and average cost lines in Einav and Finkelstein (2011). The population is arranged on the x-axis in declining order of willingness to pay for insurance; we normalize by population size, so the x-axis units are fraction of population. The blue line gives inverse demand for insurance; the red line gives average cost (on y-axis) in an insurance pool that includes all individuals to the left of a given point on the X-axis. Average cost is drawn assuming that marginal costs decline smoothly with x-axis quantity. Adverse selection exists if average cost of enrollees in an insurance plan sold at a higher price p' (see red dot labeled AC at p') is higher than average cost of enrollees in a plan sold at a lower price p (red dot marked AC at p). We derive average cost for a plan sold at, e.g., price p in 2 steps. First, we determine the quantity $q(p)$ demanded at price p using the inverse demand curve. Second, we determine average cost among all person who enroll in an insurance plan sold at price p, i.e., the value of the average cost curve at q(p).

Figure 4: Inverse demand, cost and welfare calculations at midline and endline.



Note. ML use equals endline hospital expenditure x midline successful use. EL use equals endline hospital use x endline successful use. Panels A and B: Demand is an upper bound on Hicksian demand; it is calculated using trial arms A and B. Solid blue line gives ex post demand, dashed blue line gives ex ante demand (Hendren 2021). MC curve is derived from AC curve. Optimal prices are calculated using plotted demand and assuming r = 1.25. Socially optimal price is intersection of plotted demand and MC curve. Panels C and D: Formulas for MVPF with free insurance and optimal price of public insurance are given in main text. Solid lines give values using ex post demand. Dashed magenta and orange lines give values using ex ante demand. Left black dashed line shows highest r at which second-order condition is not satisfied using ex post demand. Right dashed line gives r where p=0 satisfies the first-order condition using ex post demand.

Tables

Table 1: Trial design in each district.

		Percent of sample households assigned to each household condition in this village condition			
Village condition label	Percent of villages assigned to condition	(A) Free insurance	(B) Sale of insurance + transfer	(C) Sale of insurance	(D) No intervention
I	15	30	50	10	10
II	15	30	10	50	10
III	15	30	10	10	50
IV	35	70	10	10	10
V	20	10	30	30	30
All conds.	100	40	20	20	20

Notes. Table presents the design of the experiment, including percent of sample villages and sample households in each village assigned to each condition. This design was independently and identically applied to each of the 2 districts in the study. Column 1 gives the labels of the village conditions. Column 2 gives the percent of villages assigned to the village condition. The headers of columns 3-6 give the names of the household conditions (i.e., insurance-access arms). Each village condition is defined by the percentage of sample households assigned to each household condition. Numbers in columns 3-6 give the percentage of households assigned to each household condition in the village condition given in column 1. The last row gives totals. The last row of column 2 total shows that all sample villages are assigned to one of the 5 villages condition. The last rows of columns 3-6 give the percentage of all households in the study that were assigned to each of the 4 household conditions. For example, the table reports that 15% of villages were assigned to village condition II, and that in that condition, 30%, 10%, 50% and 10% of households were assigned to household conditions A), (B), (C), and (D), respectively. The last row says that, across all village conditions, 40%, 20%, 20%, 20% of sample households were were assigned to household conditions A), (B), (C), and (D), respectively.

Table 2: Characteristics of sample villages.

	Mean	SD	10th pct	90th pct
Population (2011 Census)	2,754	2,040	887	5,420
Poverty rate	0.18	0.11	0.06	0.33
Annual hhld. Income, INR 1000s	127.56	66.15	60.58	212.28
Mean hhld. size	5.09	0.95	4.00	6.32
Distance to any hospital	9.04	4.75	2.80	15.24
Distance to a public hospital	14.03	9.33	5.39	32.15
Distance to a private hospital	18.24	14.94	3.32	42.68

Notes. Table presents statistical characteristics of 435 sample villages. Abbreviations: SD, standard deviation; pct, percentile; hhld, household; INR, Indian rupee.

Table 3: Household characteristics in different access-to-insurance conditions.

	N	(A) Free insurance Mean (SD)	(B) Sale of insurance + transfer Mean (SD)	(C) Sale of insurance Mean (SD)	(D) No intervention Mean (SD)
Demographic					
Number of hhld. members[a]	10850	5.05 (2.37)	5.12 (2.40)	5.09 (2.51)	5.03 (2.35)
Number of children in hhld.[b]	10156	1.45 (1.47)	1.45 (1.49)	1.40 (1.42)	1.42 (1.42)
Age of head of hhld., yrs.[b]	10156	51.41 (13.43)	51.16 (13.59)	51.36 (13.41)	50.75 (13.39)
Educ. of male head of hhld., yrs.[a]	10834	6.52 (4.96)	6.69 (4.93)	6.65 (4.94)	6.55 (4.98)
Dist. to nearest town, km.[c]	10404	9.93 (5.09)	9.78 (4.79)	9.71 (4.98)	9.61 (4.89)
Financial					
Number of rooms in house[a]	10816	3.23 (1.62)	3.18 (1.52)	3.24 (1.62)	3.24 (1.63)
Number of concrete rooms[b,d]	10184	1.21 (1.99)	1.17 (1.99)	1.16 (1.94)	1.15 (1.95)
Annual hhld. exp., INR 1000s[b,e]	9080	98.49 (90.44)	99.20 (93.98)	97.75 (78.40)	97.66 (90.27)
Annual food exp., INR 1000s[b,f]	9919	41.03 (24.02)	41.33 (25.84)	41.18 (24.45)	40.33 (23.35)
Healthcare utilization					
Visited provider past 1 yr., male or female[b,h]	10067	0.98 (0.13)	0.98 (0.12)	0.98 (0.12)	0.98 (0.14)
Annual non-hosp. med. exp., INR 1000s[b,i]	9554	30.17 (72.18)	29.76 (57.17)	32.96 (118.52)	28.42 (64.76)
Annual hospital exp., INR 1000s[b,i]	9982	7.08 (21.46)	9.78 (55.49)	8.66 (39.76)	8.15 (37.38)
Health[a]					
Major illness in household[g]	10833	0.16 (0.37)	0.16 (0.37)	0.16 (0.37)	0.17 (0.37)
Good or very good health[j]	19355	0.63 (0.48)	0.61 (0.49)	0.63 (0.48)	0.63 (0.48)
Told had hypertension[k]	19348	0.11 (0.32)	0.12 (0.32)	0.11 (0.32)	0.12 (0.33)
Told had diabetes[k]	19350	0.05 (0.22)	0.05 (0.23)	0.05 (0.22)	0.06 (0.24)
Told had heart disease[k]	19368	0.05 (0.22)	0.04 (0.20)	0.04 (0.20)	0.04 (0.20)
Delivered in facility past 1 yr.[l,m]	7921	0.18 (0.38)	0.17 (0.37)	0.16 (0.36)	0.19 (0.39)

Notes. Table presents statistical characteristics of sample hosueholds by the premium/subsidy arm to which they are assigned. Abbreviations: Abbreviations: SD, standard deviation; hhld., household; yrs., years; km., kilometer; INR, Indian rupee; hosp., hospital; med., medical; exp., expenditure.

a From listing survey. b From baseline survey. c From GPS coordinates. d Concrete room is “pucca” room. e Sum of monthly expenses on non-medical items x 12. f Sum of monthly food expenses items x 12. g A major illness is an illness, injury or hospitalization of any household member in the last year that required missing school or work. h A healthcare provider is an allopathic or traditional provider, institution-based or otherwise. i Hospital medical expenditures are expenditures on inpatient care. Non-hospital medical expenditures are all other medical expenditures. j Good or very good health corresponds to a value of 1 or 2 on a five-point Likert scale of self-reported health; measured at the respondent-level (up to 2 obs per hhd). k Doctor-diagnosed conditions; measured at the respondent level. l A facility is defined as any medical facility, such as a public/private hospital or a clinic. m Fertility questions administered to most knowledgeable female at baseline and limited to those who responded to having ever given birth.

Table 4: Effects of different premium & subsidy combinations on insurance enrollment.

	Direct effects	Spillover effects	Total effects
	Coefficient (SE) P-value	Coefficient (SE) P-value	Coefficient (SE) P-value
(A) Free insurance	72.42%	6.99%	78.71%
	(4.78%)	(5.68%)	(0.90%)
	0.000	0.219	0.000
(B) Sale of insurance + transfer	65.19%	7.83%	72.24%
	(7.84%)	(9.34%)	(1.36%)
	0.000	0.402	0.000
(C) Sale of insurance	48.40%	12.79%	59.91%
	(8.08%)	(9.80%)	(1.60%)
	0.000	0.193	0.000
N	10879	10879	10879

Notes. Table presents direct and indirect effect of assignment of sample household to different premium and subsidy combinations on enrollment in RSBY insurance. The first column lists the premium/subsidy group. The second column reports direct effects of assigning a household to a premium/subsidy group on enrollment of that same household into RSBY. The third column reports the effect, for a given household assigned to the arm listed in column 1, of assigning other sample households in the village to arms A - C on enrollment of the given household. The fourth columns gives the sum of the direct effect and 90% of the indirect effect. We chose 90% of the indirect effect because in the study the maximum percentage of households assigned to arms A-C in any village was 90%. Each observation is a household. Treatment variables are household assignments: free insurance (A), sale of insurance + cash transfer (B), sale of insurance (C). Estimates are intent-to-treat effects; all models estimated with OLS. Each treatment arm was interacted with the share of the village allocated to A, B, or C (combined). Standard errors were clustered at the village level. Coefficients were transformed to show percentage point changes from the control group D. Enrollment rate is 0 in the control group D. The direct effect only includes the coefficient on treatment A, B, or C. The spillover effect is the estimated effect on a treated household of assigning all other sample households to at least some form of access to RSBY insurance. The total effect is constructed as the sum of the direct effect and 0.9*(spillover effect).

Table 5: Effect of different premium & subsidy combinations on use of insurance and on hospital utilization (intent to treat estimate).

	N	Control mean (SD)	(A) Free insurance Direct Coefficient (SE) P-value	(A) Free insurance Spillover Coefficient (SE) P-value	(A) Free insurance Total Coefficient (SE) P-value	(B) Sale of insurance + transfer Direct Coefficient (SE) P-value	(B) Sale of insurance + transfer Spillover Coefficient (SE) P-value	(B) Sale of insurance + transfer Total Coefficient (SE) P-value	(C) Sale of insurance Direct Coefficient (SE) P-value	(C) Sale of insurance Spillover Coefficient (SE) P-value	(C) Sale of insurance Total Coefficient (SE) P-value
Insurance use at 18 months											
Successful use (past 6 mos.)	9960	3.86% (19.26%)	2.51% (2.13%) .24	4.70% (2.69%) .08	6.73% (2.21%) .002	2.13% (3.74%) .57	5.78% (4.80%) .23	7.33% (2.25%) .001	7.67% (4.24%) .07	-0.25% (5.23%) .96	7.44% (2.20%) $p < .001$
Failed use (past 6 mos.)	9960	1.34% (11.49%)	3.57% (1.91%) .06	-0.29% (2.21%) .90	3.31% (1.24%) .008	3.47% (2.57%) .18	-0.40% (3.13%) .90	3.12% (1.30%) .02	2.57% (2.12%) .23	0.03% (2.54%) .99	2.60% (1.18%) .03
Insurance use at 3.5 years											
Successful use (past 6 mos.)	9458	0.32% (5.67%)	2.16% (1.30%) .10	-0.99% (1.41%) .48	1.28% (0.55%) .02	3.36% (1.99%) .09	-2.18% (2.32%) .35	1.40% (0.60%) .02	-1.19% (1.07%) .27	3.06% (1.44%) .03	1.57% (0.65%) .02
Successful use (most serious event)	9450	0.11% (3.28%)	0.38% (0.92%) .68	0.21% (0.99%) .83	0.57% (0.42%) .17	0.61% (0.99%) .54	-0.15% (1.11%) .89	0.48% (0.43%) .26	-1.30% (0.86%) .13	2.30% (1.06%) .03	0.77% (0.45%) .09
Failed used (most serious event)	9451	0.21% (4.63%)	2.18% (2.16%) .31	-1.57% (2.41%) .52	0.77% (0.50%) .13	-1.33% (1.13%) .24	1.94% (1.34%) .15	0.41% (0.52%) .43	-1.12% (1.61%) .49	2.18% (1.99%) .27	0.84% (0.59%) .16
Healthcare use at 18 months											
Hospitalized on last visit (past 1 yr.)	9053	20.31% (40.24%)	4.42% (5.47%) .42	-2.19% (9.97%) .83	2.44% (8.95%) .79	-11.54% (9.43%) .22	19.93% (14.68%) .18	6.39% (9.47%) .50	-0.50% (9.37%) .96	3.90% (12.69%) .76	3.01% (9.04%) .74
Healthcare use at 3.5 years											
Overnight stay for treatment	9483	22.59% (41.83%)	-5.42% (5.54%) .33	2.30% (5.75%) .69	-3.34% (4.66%) .47	-15.29% (7.43%) .04	14.27% (8.04%) .08	-2.45% (4.63%) .60	-7.53% (8.05%) .35	4.19% (8.55%) .62	-3.76% (4.69%) .42
Outpatient (day) surgery	9474	6.52% (24.69%)	-2.07% (3.54%) .56	4.39% (3.51%) .21	1.89% (2.69%) .48	-9.49% (4.17%) .02	12.99% (4.78%) .007	2.20% (2.78%) .43	-5.95% (4.17%) .15	9.48% (5.28%) .07	2.58% (2.81%) .36

Notes. Table presents estimates of intent-to-treat effects of access yo insurance at different prices and subsidies on use of insurance and visits to medical facilities. Abbreviations: SD, standard deviation; CI, confidence interval; mos, months; yr, year. Each observation is a household. Treatment variables are household assignments: free insurance (A), sale of insurance + cash transfer (B), sale of insurance (C). Estimates are intent-to-treat effects; all models estimated with OLS. Each treatment arm was interacted with the share of the village allocated to A, B, or C (combined). Standard errors were clustered at the village level. Coefficients were transformed to show percentage point changes from the control group D. The direct effect only includes the coefficient on treatment A, B, or C. The spillover effect is the estimated effect on a treated household of assigning all other sample households to at least some form of access to RSBY insurance. The total effect is constructed as the sum of the direct effect and 0.9*(spillover effect). Mean and standard deviation in the control group are statistics from the group without access to insurance. Successful use means the household used RSBY to pay for medical treatment. Failed use means that the household attempted to use RSBY to pay for care but were unable to (for many possible reasons, Table A6). The most serious event is defined as an accident which caused a household member to miss at least two days of work, a childbirth or a stillbirth, or three functional limitations. If none of those occurred, it is defined as the most expensive health event or the one that led to the longest hospital stay. Hospitalized on last visit (past 1 yr.) indicates that the respondent was hospitalized on their most recent visit to a healthcare provider within the past year. This question was administered to both the male or the female respondent within the adult health module.

Table 6: Effect of insurance enrollment on use of insurance and on hospital utilization (complier average treatment effect).

	Obs.	Control mean (SD)	Direct effect: Coefficient (SE) P-value	Spillover effect: Coefficient (SE) P-value	Total effect: Coefficient (SE) P-value
Insurance use at 18 months					
Successful use (past 6 mos.)	9960	3.88%	3.85%	2.60%	5.89%
		(19.32%)	(2.73%)	(4.47%)	(2.54%)
			.16	.56	.02
Failed use (past 6 mos.)	9960	1.50%	3.46%	-0.06%	3.41%
		(12.15%)	(1.87%)	(2.67%)	(1.27%)
			.06	.98	.007
Insurance use at 3.5 years					
Successful use (past 6 mos.)	9458	0.49%	2.74%	-1.58%	1.50%
		(6.99%)	(1.16%)	(1.60%)	(0.58%)
			.02	.32	.010
Successful use (most serious event)	9450	0.13%	0.50%	0.63%	1.00%
		(3.59%)	(0.81%)	(1.06%)	(0.38%)
			.53	.55	.008
Failed used (most serious event)	9451	0.57%	1.24%	0.26%	1.45%
		(7.52%)	(1.64%)	(2.28%)	(0.56%)
			.45	.91	.009
Healthcare use at 18 months					
Hospitalized on last visit (past 1 yr.)	9053	20.82%	1.00%	6.09%	5.79%
		(40.61%)	(6.28%)	(12.47%)	(9.00%)
			.87	.63	.52
Healthcare use at 4 years					
Overnight stay for treatment	9483	22.97%	-12.42%	14.48%	-1.02%
		(42.07%)	(5.99%)	(7.36%)	(4.16%)
			.04	.05	.81
Outpatient (day) surgery	9474	6.76%	-4.07%	10.52%	4.21%
		(25.11%)	(3.47%)	(4.34%)	(2.57%)
			.24	.02	.10

Notes. Table presents complier average treatment estimates of the effect of enrollment in RSBY insurance on use of insurance and visits to medical facilities. Abbreviations: SD, standard deviation; CI, confidence interval; mos, months; yr, year; hosp., hospital. Each observation is a household. Treatment variable is enrollment. Estimates are complier average treatment effects. Observations are weighted so each household has equal weight. Standard errors were clustered at the village level. Coefficients were transformed to show percentage point changes from the control group. Mean and standard deviation in the control group are statistics from the unenrolled group. The direct effect is the estimated effect of enrolling one household, assuming no other sample households in the village are enrolled. The spillover effect is the estimated effect on an enrolled household of enrolling all other sample households in the village. Total effects are the sum of direct and (spillover effects)*(uptake into free insurance). The total effect of free insurance on uptake is estimated as 0.7871 in Table 3. Successful use means the household used RSBY to pay for medical treatment. Failed use means that the household attempted to use RSBY to pay for care but were unable to (for many possible reasons, Table A6). The most serious event is defined as an accident which caused a household member to miss at least two days of work, a childbirth or a stillbirth, or three functional limitations. If none of those occurred, it is defined as the most expensive health event or the one that led to the longest hospital stay. Hospitalized on last visit (past 1 yr.) indicates that the respondent was hospitalized on their most recent visit to a healthcare provider within the past year. This question was administered to both the male or the female respondent within the adult health module.

Table 7: Number of Significant health outcomes per category.

	18 months (midline)				3.5 years (endline)			
	ITT			CATE	ITT			CATE
	(A) Free insurance	(B) Sale of insurance + transfer	(C) Sale of insurance	Enrollment	(A) Free insurance	(B) Sale of insurance + transfer	(C) Sale of insurance	Enrollment
Self-reported health								
Total outcomes	2	2	2	2	3	3	3	3
Sig. direct effect	0	0	0	0	0	0	0	0
Sig. spillover effect	0	0	0	0	0	0	0	0
Sig. total	0	0	0	0	0	0	0	0
Chronic disease								
Total outcomes	15	15	15	15	17	17	17	17
Sig. direct effect	0	0	1	0	0	0	0	0
Sig. spillover effect	0	0	0	0	0	0	0	0
Sig. total	0	0	0	0	0	0	0	0
Quality of life								
Total outcomes	1	1	1	1	14	14	14	14
Sig. direct effect	0	0	0	0	0	0	0	0
Sig. spillover effect	0	0	0	0	0	0	0	0
Sig. total	0	0	0	0	0	1	0	0
Mental and behavioral health								
Total outcomes	0	0	0	0	3	3	3	3
Sig. direct effect					0	0	0	0
Sig. spillover effect					0	0	0	0
Sig. total					0	0	0	0
Childbirth								
Total outcomes	8	8	8	8	10	10	10	10
Sig. direct effect	0	0	0	0	0	0	0	0
Sig. spillover effect	0	0	0	0	0	0	0	0
Sig. total	0	0	0	0	0	0	0	0
Biomarkers								
Total outcomes	7	7	7	7	0	0	0	0
Sig. direct effect	0	0	0	1				
Sig. spillover effect	0	0	0	0				
Sig. total	0	0	0	0				
Mortality								
Total outcomes	0	0	0	0	4	4	4	4
Sig. direct effect					0	0	0	0
Sig. spillover effect					0	0	0	0
Sig. total					0	0	0	0

Notes. Treatment variables in intent-to-treat (ITT) analysis are household assignments: free insurance (A), sale of insurance + cash transfer (B), sale of insurance (C). Treatment variables in complier averate treatment effect (CATE) analyses are enrollment in insurance; enrollment is instrumented with assignment to groups A, B or C. All models estimated with OLS. Each treatment arm was interacted with the share of the village allocated to A, B, or C (combined) to estimate spillover effects in ITT analysis. Each treatment arm was interacted with the share of the village enrolled to estimate spillover effects in CATE analysis; the instrument for the share enrolled is the fraction allocated to groups A, B, or C. Standard errors were clustered at the village level. Significant health outcomes are identified using family-wise error rate (FWER) critical p-value at the 5% significance level.

Table 8: Enrollment by predicted spending

	(1) Coeff (SE) [p-value]	(2) Coeff (SE) [p-value]	(3) Coeff (SE) [p-value]	(4) Coeff (SE) [p-value]
Pay + Cash (B)	-0.066	-0.14	-0.066	-0.15
	(0.013)	(0.052)	(0.013)	(0.052)
	[0.000]	[0.005]	[0.000]	[0.005]
Pay (C)	-0.19	-0.19	-0.19	-0.19
	(0.014)	(0.057)	(0.014)	(0.057)
	[0.000]	[0.001]	[0.000]	[0.001]
Cost	0.0017	-0.0058	0.012	0.0033
	(0.0078)	(0.0080)	(0.0077)	(0.0083)
	[0.825]	[0.466]	[0.124]	[0.690]
B × Cost	0.029	0.026	0.023	0.016
	(0.011)	(0.011)	(0.012)	(0.013)
	[0.010]	[0.022]	[0.056]	[0.192]
C × Cost	0.060	0.047	0.034	0.028
	(0.016)	(0.017)	(0.013)	(0.014)
	[0.000]	[0.006]	[0.011]	[0.051]
Dep var mean	0.718	0.718	0.718	0.718
Dep var SD	0.450	0.450	0.450	0.450
Obs.	8727	8727	8727	8727
Wealth/Educ/Raven/Risk controls	N	Y	N	Y
Cost variable	EL1 medical exp, predicted	EL1 medical exp, predicted	EL2 medical exp, predicted	EL2 medical exp, predicted
$p(C \times Health > B \times Health)$	0.031	0.106	0.216	0.244
CXHealth × SD/Mean enrollment	0.101	0.079	0.059	0.048

Notes. Outcome is enrollment in RSBY during the enrollment drive. SEs clustered at village level. Spending variables are standardized by the sample mean and standard deviation. Controls for levels and interactions with education, raven score, and risk aversion are included but not reported in columns 2 and 4. Education is a categorical variable recording the educational attainment of household head (1 = Never attended class 1, 2 = Class 1-5, 3 = Class 6-8, 4 = Class 9-10, 5 = Class 11-12, 6 = Graduate and above). Risk aversion is a dummy variable that equals 1 if respondent had a certainty equivalence smaller than 180 in a gamble game. Missing indicators of assets, raven scores, risk aversion and education are included. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Welfare calculations using four different measures of cost.

	(1) ML use	(2) EL use	(3) ML attempt	(4) EL attempt
Ex post welfare calcs.				
Free ins. value	-68.397	515.624	-798.506	-534.486
Opt. price	1052.488	528.483	2124.629	1944.579
SOC	-0.157	-0.268	-0.152	-0.120
Ex ante welfare calcs.				
Free ins. value	1888.483	2472.504	1158.374	1422.394
Opt. price	238.883	864.175	1216.728	280.769
Observations	4988	4446	4668	4447

Note. Table presents welfare calculations associated with demand based on (1) uptake in groups A and B and (2) four different measures of cost. Welfare calculations include the net welfare benefit of free insurance, the price of insurance that satisfies the first-order condition (FOC) for optimal premium, and the value of the second-order condition (SOC). If the SOC is negative, then the price that satisfied the FOC is the welfare-maximizing price. The four measures of cost are: (1) ML use = endline hospital use * midline successful use for most serious event over 6 months * 2; (2) EL use = endline hospital use * endline successful use for most serious event over 6 months * 2; (3) ML attempt = endline hospital use * midline (successful or failed) use for most serious event over 6 months * 2; (2) EL use = endline hospital use * endline (successful or failed) use for most serious event over 6 months * 2.