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IZA DP No. 17185

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ABSTRACT

Evaluating and Pricing Health Insurance in Lower-Income Countries: A Field Experiment in India*

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, called RSBY. We find substantial demand (~ 60% uptake) even when consumers were charged a price equal to the premium the government paid 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, healthcare utilization does not rise substantially, suggesting the primary benefit of insurance is financial. 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 67-95% of average costs (INR 528-1052, $30-60) rather than a zero premium to maximize the marginal value of public funds.

JEL Classification: D1, I13

Keywords: health insurance, adverse selection, spillovers, 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 ($1,712) of treatment at in-network hospitals per annum per household.[2]

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.[3] 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 we estimate is demand. Demand captures subjective utility from insurance and impacts that the investigator may not measure. Moreover, demand elasticity 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

[2]We employ the purchasing-power-parity exchange rate of INR 17.52/dollar (OECD, 2020).

[3]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.

given a cash transfer equal to the amount of the premium, while roughly 60% of our sample purchased insurance 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.[4] The implied price elasticity of demand is -0.314, well within the range of estimates of insurance demand elasticity from higher income countries (Pendzialek, Simic and Stock, 2016a).[5] These results show significant demand for insurance at positive prices, implying that people subjectively believe health insurance helps address some of the costs (financial, health and/or psychological) associated with illness.

A second critical factor is adverse selection. This impacts whether private markets for community-rated insurance 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 enroll 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 how access to insurance effects utilization of both insurance and of health care. This sheds light on why consumers may value insurance, as well as on the cost of providing it. We find that insurance enrollment significantly and substantially increased utilization of insurance. 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, a 74% increase in insurance utilization. However, enrollment had insubstantial and statistically insignificant effects on use of hospital care. Hospital utilization rose only 10-15%, and even that estimate is imprecise. Combined, the insurance and hospital utilization effects suggest the predominant benefit of insurance is reducing financial risk.

[4]Combined, these numbers tell us about two demand curves. First, the difference between uptake in the zero-price arm and the sale-of-insurance arm tells us about the (uncompensated) Marshallian demand curve. Second, when Marshallian demand is combined with the difference between uptake in the zero-price arm and in the sale-plus-transfer arm, we obtain bounds on the (compensated) Hicksian demand curve. When income effects are positive, as is likely with health insurance, Marshallian demand is also a lower bound on Hicksian demand. Because the sale-plus-transfer arm compensated consumers even when they do not buy insurance, it provides an upper bound on Hicksian demand.

[5]This estimate uses data from the free-insurance and sale-of-insurance arms because estimates of demand elasticity in the literature use Marshallian demand curves to estimate elasticity. If we use the sale-plus-transfer arm rather than the sale-of-insurance arm, the implied price elasticity of (compensated) demand is lower. We explore this in Section 4.1.

In addition, we find that the insurance utilization effect is mediated by spillover effects, and that many beneficiaries were unable to use their insurance cards. Spillover effects are responsible for 70% of the effect of enrollment on successful insurance utilization. Evidently, utilization by neighbors has an important effect on utilization by sample households. Moreover, among enrolled households, 5% tried but were unable to use their insurance card (compared to a control group failure rate of 1.5%).[6] We find that this difficulty reflects not only supply-side constraints, but also demand-side obstacles such as households forgetting their card or trying to use RSBY at non-participating hospitals. Together these results suggest a need to educate enrollees on how insurance functions, and to improve the administration of insurance programs.

A 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 nearly all results from 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 standard errors imply 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. Our findings are consistent with the conclusion either that insurance did not substantially increase health care consumption, or that the little it did had no significant impact on the health outcome we measure.

Finally, we use our demand and adverse selection estimates to ask whether free insurance[7] is a positive-value use of public funds, and what the optimal price for public insurance is. To answer these questions, we employ and extend the marginal value of public funds framework (Hendren and Sprung-Keyser, 2020).[8] A feature of these models is that they use aggregate demand to measure the value of insurance, without taking a stand on the specific mechanisms behind the impact of insurance. Applying this framework in our context highlights a central

[6] Although households in the control group did not have access to RSBY at the start of the study, some 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.

[7] 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.

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

trade-off when setting insurance subsidies. Higher insurance premiums (lower subsidies) reduce uptake of insurance and thus its social benefits. However, higher premiums also 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, 2012). We estimate optimal premiums of roughly 67% - 95% of average cost (INR 528-1052 or $30-60) holding fixed the rates of successful insurance use measured in our study, and 48% - 73% of average cost (INR 1900-2200, $108-126) if the unsuccessful utilization we observe translates into successful utilization over time.[9] If the MCPF falls over time to the level observed in higher-income countries, free public insurance may become optimal. This is the case whether the alternative to free public insurance is no insurance or private insurance. This conclusion also holds whether we estimate demand for insurance when households have some information on their health state, or before they have this information—so-called *ex ante* demand for insurance (Hendren, 2021). Another way to state our findings is that, in India, the government's cost of financing likely swamps the benefits of public insurance to enrollees.[10]

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 essentially no demand for insurance at positive prices.

In contrast to a vast literature in rich countries,[11] only a few papers have studied adverse selection in health insurance markets in lower-income countries. These studies have yielded varying results. Banerjee et al. (2021a) find evidence that subsidies (i.e., lower cost sharing)

[9] We calculate the premium as a percentage of average cost using Table A.12 columns 3 (successful use) and 6 (successful and unsuccessful use). The first panel of that table gives regression estimates of average cost as a function of premium.

[10] 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 ($14-49) holding constant the rates of successful insurance use, and to INR 281-1217 ($16-69) if unsuccessful use becomes successful use in time.

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

select for lower-cost households in Indonesia. Asuming, Kim and Sim (2021) 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. 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 levels of both demand and adverse selection, our results buttress the former view.

Consistent with other RCTs 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 observed under PMJAY, the program that replaced RSBY and covered our sample population (Garg, Bebarta and Tripathi, 2020). This result, along with our 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 externalities 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).[12] Our study likewise finds significant effects on utilization, but few significant effects on health. Non-RCT 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 in-

[12] 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.

crease in utilization and reduction in mortality rates due to insurance access over 18 months. 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 (MVPF) framework to evaluate the welfare effect of public policies (Hendren, 2016; Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020). 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 India. Lastly, we connect our MVPF 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 it. 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) in rural and

INR 65,340 ($3,729) 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.[13] Although the government paid the premiums, households had 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.[14] RSBY did not cover primary care. The government was responsible for 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) per household. The government dictated the prices for procedures; this cap was enough to cover, e.g., 4 Cesarean sections or 10 MRIs.[15] 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 ($1712 to 28,538) 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.[16] We decided to pro-

[13]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.

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

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

[16]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 insured households, which 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.

vide 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.

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.[17] 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.[18]

Table 1 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 2 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). Mean medical expenditure across groups was INR 38,115 ($2,191.78), of which INR 8,147 ($465) was spent on hospital care. Conditional on having positive hospital expenditures, average hospital expenditure is INR 16,181 ($925) and 76% had hospital expenditure below the RSBY limit of INR 30,000.

[17] Households 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 the even lower Antyodaya Anna Yojana thresholds get access to a greater quantity of subsidized food than those with APL cards.

[18] 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.

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),

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.[19] 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.[20] 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 3 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 3.[21] 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 Appendix A.1.1.

[19] 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 ($1.71) in each district.

[20] 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 of as having no access to RSBY *at the beginning of the study*.

[21] As the last row of Table 3 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.

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.[22] Second, we conducted likelihood-ratio tests 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.[23] A one-sided Kolmogorov–Smirnov test reveals that covariates are balanced (p=1.000) across insurance-access arms (Appendix Figure A.1).

2.4 Measurement and timing

Our study measures several key outcomes. The first is demand, measured by enrollment in RSBY in our treatment arms. Enrollment is defined as a household enrolling in RSBY during the 1-2 days that we brought a mobile enrollment truck to their village or town during our 2015 enrollment drive.[24] 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 reveal dimensions of value that we do not directly measure, e.g., consumption smoothing.

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 affect 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

[22] 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.

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

[24] 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.

Appendix Section A.4 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 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 4.5.

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 about household finances, 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.

Appendix A.2 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 on insurance enrollment, and the effect of both access to insurance and of enrolling in insurance on insurance utilization; 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 are based on the following specification[25]:

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

where i, j and t index households, villages and time, respectively; y_{ijt} is an outcome; d_{ij}^h is an indicator for assignment to 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);[26] 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.,

[25]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.

[26]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 differ.

shares of the village sample in groups A, B or C).[27] In our setting, s_j ranges between 50–90%. 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).[28] 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%.[29]

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

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

[27] 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 varies with the number (rather than the fraction)) of sample households given any access to insurance via the study. The results are qualitatively similar (not reported).

[28] 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).

[29] 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.

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 $(d^h_{ij}, d^v_j, d^h_{ij}d^v_j)$, where d^v_j 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 household 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. Their discussion justifies the use of the above linear model.

This assumption raises concerns about using the sale-plus-transfer arm B as an instrument. Perhaps the transfer affected outcomes, e.g., utilization or health, through a channel other than enrollment in insurance. We believe this risk is minimal. The amount of the subsidy is quite small, as the premium is small, relative to average household income. The subsidy may affect very short-term liquidity issues on the order of days, but not months or year-long liquidity issues, the latter of which are more likely to affect the outcomes we measure in our 18 month and 3.5 year surveys. Therefore, we report results that use arm B as one of our instruments.

A second exclusion 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. This restriction is satisfied, e.g., if never-takers do not affect others' outcomes. Importantly, this additional restriction

allows for both spillover effects on take-up and on outcomes.

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). In private markets with community-rated insurance, such selection can cause suboptimal levels of insurance consumption and even the collapse of insurance markets.[30] In public insurance programs, adverse selection can reduce fiscal relief governments obtain from charging premiums for public insurance, a topic we explore further in Section 5. Our study induces exogenous variation in the price at which households are offered access to insurance, allowing us to cleanly test for adverse selection.

Figure 1 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.[31] 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.[32]

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

[30] 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.

[31] 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.

[32] 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.

the interaction of premium indicators and expected cost. We first explain how we estimate expected costs and then describe our estimating equation.

3.2.1 Measuring expected costs.

Our goal is to construct a measure of expected total household medical expenditure at midline and endline.[33] 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 a LASSO regression. We fit the model on the control group to avoid predicting additional spending caused by insurance (i.e., moral hazard).[34] 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 predictions for the full sample. See Appendix A.5 for the LASSO procedure details.

3.2.2 Estimating equation.

We test for differential selection using the following regression specification:

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

where $enroll_{ij}$ is the enrollment decision of household i in village j, d_{ij}^h is an indicator for assignment to treatment arm $h \in \{1, 2, 3\}$, $Cost_i$ is the household's expected cost under insurance, and s_j is treatment saturation of village j (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 control for additional baseline covariates (baseline assets, education, Raven's matrix scores, and risk aversion) interacted with treatment status.

[33] 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.

[34] In theory there could be selection based on moral hazard (Einav et al., 2013). But as insurance is a novel product in India 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.

We measure variation in a household's costs ($Cost_i$) with our LASSO-based prediction of either midline or endline medical spending. We scale $Cost_i$ 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 1. 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

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 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 at zero price 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). Finally, enrollment may also be suppressed in India because health insurance is a newer and less well-known product there (Berg et al., 2019).[35]

Sale-of-insurance arm. When households were charged for insurance, the enrollment rate was 59.91%, 18.80 percentage points (pp) lower ($p < 0.001$) than in the free-insurance arm (78.71%). 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

[35]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).

and Stock, 2016a).[36,37]

Our estimate of enrollment in the sale-of-insurance arm sheds light on two questions in the literature. One concerns the level of demand, namely 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.[38] Likewise, Banerjee et al. (2021b) found low uptake ($< 10\%$) of unsubsidized public insurance in Indonesia. By contrast, our study estimates uptake of 59.91% at unsubsidized prices. The other debate concerns the elasticity of demand, i.e., the 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 15% of the full cost. 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.

Sale-plus-transfer arm. In this arm, the enrollment rate was 72.24%, 12.33 pp higher ($p < 0.001$) than in the sale-of-insurance arm (59.91%).[39] If we use the sale-plus-transfer arm (rather than the sale-of-insurance arm) to calculate the price elasticity, the implied elasticity of (compensated) demand is -0.0896, on the low end of the range of estimates of insurance demand elasticity in the literature. This lower elasticity may be the product of

[36](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.

[37]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. The enrollment rate in the official RSBY program was 43.12% in Gulbarga, 59.97% in Mysore (RSBY, 2018). Enrollment among 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.

[38]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; borrowers chose not to renew loans when this condition was imposed.

[39]The 12.33 pp increase in uptake with a transfer is consistent with the view that the sale-plus-transfer treatment relaxed liquidity constraints (Berkouwer and Dean, 2022; Casaburi and Willis, 2018).

rural households in an LMIC having a greater need for insurance because they lack other options for smoothing financial shocks due to health risk.

Our findings have implications for the efficacy of insurance pricing at raising enrollment (with welfare effects we discuss in more detail in Section 5). Because free insurance is akin to pure insurance plus an in-kind subsidy, we infer that in-kind subsidies raise enrollment by twice as much (from 59.91% to 78.71% v. to 72.24%) 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 even free insurance. In our sample, we were able to achieve 59.91% enrollment while paying no additional premiums. 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, such that 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 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* (overnight stay or same day surgery). These capture the effect of insurance at two margins: (i) insurance can change financing of care, holding level of care constant, and (ii) it can affect the level of care. Second, we distinguish successful and unsuccessful efforts to use insurance. Successful use affects insurer costs and beneficiary finances/care. Unsuccessful use reflects either administrative failures or beneficiaries' lack of knowledge about how to use insurance. We report intent-to-treat (ITT) effects of insurance access and complier average treatment effects (CATE) of insurance enrollment. The CATE provides a scaled version of ITT, to facilitate comparisons across different interventions which may change enrollment

[40] 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).

by different amounts. Some measures of use at 3.5 years differ from those at 18 months due to differences between midline and endline surveys.

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. Table 5 reports direct, spillover, and total effects for treatment arms $i = 1, 2, 3$.[43] We find that the total effects of insurance access on our measures of utilization are significant, although ITT estimates of the direct effects of insurance access are not separately significant at conventional levels.[44] 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.[45,46]

This spillover result is unlikely to be due to supply constraints because we find that RSBY access and enrollment increase insurance utilization substantially, but hospitalization only a relatively slight amount. 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]

[42]Recall that it is possible for control households to obtain RSBY coverage, 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]The ITT effect at 18 months for arm 3, and at 3.5 years for arm 2, are significant at the 10% level.

[45]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.

[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

Hospital utilization. We estimate imprecise effects of insurance access and enrollment on utilization of hospital care at 18 months and 3.5 years, with some point estimates positive and some negative (ITT: Tables 5, CATE: 6). Consistent with a null effect on the extensive margin or utilization, we also do not find evidence that insurance increased the intensity of use of hospitalization, as captured by length of stay, which we measured at the 3.5 year endline. We discuss how the contrast between insurance and hospital utilization results sheds light on the value households obtain from insurance in Section 4.4, after we discuss effects of insurance on health.

Failed utilization of insurance. Many households had difficulty using their insurance to pay for healthcare. At 18 months, access to free insurance increased the number of households who tried to use their insurance card but were unable to do so by 3.34pp annually, from a base of 2.68%.[48] This excess failure rate is roughly 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; we document failures in the *use* of insurance.

Lack of knowledge — about the purpose of insurance and how to use insurance — is a potential explanation for failed use. 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.6). 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 hospital 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)

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.5). 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.

[48] Although no household in the control enrolled at baseline, some households in that group may have fallen below the poverty line or switched to an eligible occupation 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

and supply-side (e.g., IT and training) may be important for raising utilization of, and thus demand for, insurance in India and similarly situated countries.[50]

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.7-A.10 report estimates of ITT effects and CATE.[52] We cannot reject the hypothesis that our estimated p-values are drawn from a uniform distribution ($p = 0.31$), the distribution of p-values we would expect if there are no effects of insurance on health outcomes. We also find no effects on a summary index of health outcomes.

These insignificant health effect estimates should, however, be interpreted with care. First, despite our large sample size, we may lack power to detect the effect of insurance on health, either because insurance has a small effect on the level of hospital care, or because 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 prior 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 (i.e., "bad,") outcome, 23.6-47.2%

the card because a service was truly not covered, or incorrectly/strategically did so. Such strategic behavior has been documented in other contexts: Alexander (2020) shows that doctors in the US strategically turn away patients who are unlikely to generate large billable fees and Alexander and Schnell (2024) show that lower benefit generosity increases the rate at which providers turn away beneficiaries.

[50] We find no evidence that the 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.

[51] Even if we do not adjust for multiple testing, only 55 (8.38%) out of 738 estimated parameters are significant (Table A.11).

[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) .

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.2 and A.3).

Our findings are broadly consistent with prior RCTs of health insurance in low- and middle-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 Mechanisms through which insurance generates value

The greater utilization of insurance combined with insignificant effects on hospital utilization, documented in Section 4.2, suggests that insurance access primarily shifts the financing of care, rather than the level of care. The lack of significant effects on health are consistent with this story.

If insurance has mainly financial effects, we would expect that it would reduce out-of-pocket (OOP) payments. A benchmark of how much OOP spending would change if RSBY had purely financial effects, i.e., changed the financing of infra-marginal spending with no additional changes in the level of care, is - INR 1108 ($63), calculated as the control mean of medical OOP spending (INR 28,777) multiplied by the pooled treatment effect on RSBY utilization (times -1 since utilization reduces spending 1 for 1 under this benchmark). However, we do not find any reduction in OOP. Table 8 reports ITT effects and Appendix Table A.4 reports the CATE. At 18 months, the total effects of assignment to free insurance, sale of insurance plus transfer, and sale of insurance were actually positive and significantly greater than -1108 (though not significantly different from zero).[54]

There are several ways to reconcile the utilization, health and OOP 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

[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%.

[54] At the 3.5 year endline, the effects are negative but not significant than either 0 or -1108.

there was a visit). However, the fact that length of stay does not change (Table 5) suggests that number of services were unlikely to increase. Second, respondents may also have increased spending on OOP costs that were complementary with covered care (such as medicines prescribed at the hospital but obtained elsewhere). Third, it is possible that hospitals charge insured patients an amount above and beyond what insurance pays. 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. Jain (2021) finds evidence of such "balance billing" in Rajasthan. As a joint test of these last two possibilities (failed use and balance billing), we examine whether treatment effects vary according to a proxy for the predicted likelihood to successfully use RSBY; however we do not find significant interactions (results on request).[55] A fourth possibility is that failed utilization caused some households to increase OOP spending (e.g., a household decided to go to the hospital because they had insurance, but they were unable to use the insurance and had to pay OOP). In principle, such increases could offset reductions in spending for other households. However, in Appendix Figure A.4, we plot the distribution of quantile treatment effects and do not find evidence of large increases in OOP for some quantiles nor of decreases for others. Instead, throughout the distribution the estimated effect on OOP is (insignificantly) positive. Moreover, below the 80th percentile the effect is significantly greater than -1108.

Our results suggest that, while insurance is likely acting primarily as a financial product in our context, insurance does not translate into reductions in OOP spending. We note that we are not the only paper to observe this puzzle: Wagstaff et al. 2016, for example, also find that insurance does not mitigate OOP spending.

4.5 Adverse selection

We find significant adverse selection into insurance at midline. Recall that our test for adverse selection is whether arms 2 and 3, which impose a positive price on insurance, attract enrollees who have higher predicted cost, compared to arm 1, which offers free insurance. This implies testing for $\phi_2 > 0$ and $\phi_3 > 0$ in regression equation (3).[56] Table 9 presents our

[55] Specifically, we use LASSO to predict the likelihood of successfully using RSBY in the treatment groups as a function of baseline covariates, generating predictions for treatment and control, and adding the resulting predicted use variable (levels and treatment interactions) to a regression predicting midline OOP spending.

[56] We also test for adverse selection a different, but related way. Specifically, we focus on a sample of households that enrolled in insurance and regress their predicted midline cost on separate indicators for assignment to the sale-of-insurance and sale-plus-transfer arms and a constant. We again find evidence of adverse selection, though it is only significant when comparing the sale-of-insurance and free-insurance arms ($p = 0.033$).

estimates. Columns 1 and 2 use predicted midline spending.[57]

Column 1 shows that, while a household with the mean level of predicted spending is 19 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 arm C 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.[58]

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.[59]

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.[60]

Price and utilization. Two additional results in 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 $p = 0.06$. Second, the effect of assignment to free insurance on failed use is significantly larger than that of assignment

[57] We discuss the construction of this measure in Sec. 3.2.1; 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.

[58] A one-sided test of the hypothesis that 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.

[59] 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.

[60] See Appendix A.5 for details.

to paid insurance (3.57% vs. 2.57%); a one-sided test yields a p-value of 0.05.[61]

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, 2020), 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 the derivation of those measures in Appendix 5.

5.1 Does offering (free) public insurance improve welfare?

Our first analysis compares welfare with free, publicly-financed, insurance against a counterfactual where neither the government nor the private sector offer insurance. This is a reasonable counterfactual in India, where private insurance is largely unavailable outside of large cities (Gambhir et al., 2019).

If the counterfactual were private insurance, the MVPF spent on free public insurance might be positive due either to social demand exceeding private demand (i.e., positive externalities from, e.g., controlling infectious diseases) or to adverse selection leading to suboptimal take-up of private insurance. In this analysis, we assume that there are no externalities from consuming insurance. This is in part because we do not estimate all possible externalities from hospital insurance. However, we will explore a counterfactual with private insurance and adverse selection in an extension in Section 5.1.1.

We first calculate the marginal social benefit (MSB) of offering a public insurance product at price p (versus not offering insurance).[62] Then we calculate the marginal social cost (MSC)

[61] 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; we do not have separate variation in offer prices vs. transaction prices as in Ashraf, Berry and Shapiro (2010).

[62] 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 these 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.

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 via PM-JAY (the successor to RSBY).

We do this exercise, first, using information that consumers have at time of enrollment. Then, in an extension in Section 5.1.2, we repeat it assuming consumers do not have any information on their expected costs. The former case generates what Hendren (2021) calls *ex post* demand, the latter *ex ante* demand. In this section, we focus on *ex post* demand, which is the demand curve the literature typically works with.

Marginal social benefit of public insurance. 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.[63] 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 $MSB(0)$.

Marginal social cost of public insurance. We assume that the government pays for insurance through a combination of charged premium p (i.e., a user fee) and financing at a marginal cost of public funds of r. 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 of these is scaled up by $1 + r$ to account for the government's cost of financing.

MVPF of offering public insurance with (i) *ex post* demand and (ii) no private insurance. The decision to provide RSBY insurance at p is welfare-improving if the marginal

[63] A detailed derivation of MSB(p) (Eq 4) and MSC(p) in Eq. (5) is presented in Appendix Section B.1.

social benefit is greater than the marginal social cost: MSB(p) - MSC(p) ≥ 0. In the case where $p = 0$, which is policy-relevant and one we evaluate with data, MVPF is positive if

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

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 illustrate the MVPF trade-off in Figure 2. Panel A illustrates the calculation using demand and cost curves from midline.[64] 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 ($\approx$ 500 INR, \$29) 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.[65]

[64] Einav and Finkelstein (2023) make an analogous figure using data from an RCT in Pakistan that randomized the price of insurance, to demonstrate adverse selection (Fischer, Frölich and Landmann, 2023). An important difference between Einav and Finkelstein (2023) and this paper is that the former uses the figure to calculate the welfare loss from adverse selection, while we leverage the graph to employ the MVPF framework to determine the optimal price of public insurance.

[65] 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-

Application with estimates of linear demand and average cost curves. 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:

$$g_i(p_i) = g(0) - \gamma p_i + \epsilon_i \tag{7}$$

$$c_i^{AC}(p_i) = c^{AC}(0) + \kappa p_i + \upsilon_i \tag{8}$$

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.[66]

We simultaneously estimate both equations using linear regression. Household demand is a binary variable that indicates enrollment. Price is determined by treatment group. We estimate the demand equation on two samples. Our preferred sample includes (A) the free-insurance group (charged a price of zero) and (B) the sale-plus-transfer group (charged the full premium). This sample produces an upper bound on Hicksian (i.e., compensated) demand, which is the relevant curve for estimating the willingness to pay. The unconditional cash transfer, which is equal to the premium, is a sort of compensation. However, because even households that do not purchase insurance receive the compensation, we may overestimate Hicksian (compensated) demand. Nevertheless, this sample yields our preferred estimates for demand as liquidity constraints are likely important in LMIC contexts (Berkouwer and Dean, 2022; Casaburi and Willis, 2018).

As a robustness check, in the appendix we estimate demand and welfare with a second sample that includes the (A) free-insurance and (C) sale-of-insurance groups. This sample produces estimates of Marshallian demand. That is a lower bound on demand because income effects are surely positive.[67]

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 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.

[66] 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 (7) to translate price to quantity when calculating MVPF, using the delta method to calculate standard errors.

[67] A common assumption in demand estimation in health economics is that there are no income effects. In that case, Marshallian demand is equal to Hicksian demand. We suppose this assumption is mainly for convenience because there is also a sizable literature finding that income increases healthcare expenditure,

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).[68] Neither is the measure we use for estimating adverse selection in Section 4.5. 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 (or government'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). However, we do not include village allocations in our specification. Because allocation is also randomized, its omission does 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 receive some access to insurance across arms.

We can plug our estimates of Eqs. (7) and (8)—which are reported in Table 10 and plotted in the A panels of Figures 2 and 3—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:

$$S_P(0) = \frac{g(0)}{2\gamma} - (1+r)c^{AC}(0) \geq 0 \tag{9}$$

though micro and macro estimates differ on magnitude (Acemoglu, Finkelstein and Notowidigdo, 2013).

[68] 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.

We provide MVPF calculations using using other measures of cost, including midline medical expenditures, in the appendix (Table A.12). While cost curves differ, the MVPF calculations yield similar results.

Using a MCPF of $r = 1.25$ to account for the inefficiency of lower-income country tax systems (such as India[69]) and the free-insurance arm (A) and sale-plus-transfer arm (B) to estimate demand,[70] we calculate that free public insurance reduces welfare by INR 63 ($3.60) per household at midline (though our estimate is imprecise[71]) and increases welfare by INR 516 ($29) per household at endline (Table 10, row 1).

Our findings are driven by the level of utilization, the 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 (to capture the idea that households may utilize successfully more often over time), then free insurance reduces welfare by 534 INR ($30) per household even at endline (Table 10, column 4). Moreover, B panel of Figures 2 and 3 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 value of free insurance using sale-of-insurance arm (C) rather than arm B to estimate *ex post* demand and other measures of hospitalization costs (see Appendix table A.12). Our results are qualitatively similar: free insurance lowers welfare at our realistic marginal costs of public funds (MCPF). Versions of B panels in Figures 2 and 3 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 B of Figure 2.

5.1.1 MVPF of public insurance with *ex post* demand and private insurance

An alternative counterfactual against which to judge public insurance is private insurance, specifically a private insurance plan identical to the public plan and offered at a community-

[69] 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).

[70] We use arm B rather than sale-of-insurance arm C because it is a more realistic estimate of demand. As Table A.12 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 allowed year-round enrollment at hospitals when required. Moreover, the magnitude of the premium in our study ($\sim$ 200 INR, $11.42) is small relative to annual income, so likely has a small income effect.

[71] Standard errors on welfare calculations are obtained using the delta method.

rated price with zero load.[72] Here we calculate the MVPF under free public insurance against this counterfactual in two steps. First, we calculate the surplus S_P from public insurance, and then subtract the surplus from private insurance S_M. Since we have already discussed how we calculate S_P, here we focus on how we calculate S_M.

Marginal social benefit of private insurance. The MSB of private insurance is the aggregate willingness to pay for such insurance sold in a market at a private insurance equilibrium price, assuming that such insurance is sold at all. To determine whether private insurance is sold and at what price, we use the model of Einav and Finkelstein (2011). Under community rating, price is determined by the average cost curve rather than the marginal cost curve. If the average cost curve is always above the demand curve or crosses the demand curve from above, there will be no private policy offered in the market. Either it will be cost-prohibitive from the start or will collapse due to adverse selection, respectively. If average cost intersects demand form below, as in Panel A of Figure 2, the private market price p_m will be the price at the intersection of average cost and demand. In the latter case, the marginal social benefit of private insurance is the social surplus from insurance, i.e., the area (i) under the demand curve to the left of $G(p_M)$ and (ii) above the average cost at $G(p_M)$. Because price is equal to average cost, this is identical to the area under the demand curve and above the price p_M.

Marginal social cost of private insurance. Assuming it is offered in a market, the MSC of private insurance is zero. To see why, note that the cost of insurance is given by equation (5) except where one uses the capital cost r appropriate for a private insurance firm. However, the private firm chooses a (community-rated) price equal to average cost: i.e., $p_m = c^{AC}(p_m)$. Therefore, $MSC(p_m) = 0$. Note, there is a private cost to private insurance, but we have included this in the MSB calculation discussed in the previous paragraph.

MVPF of offering public insurance where there is private insurance. The marginal social surplus with private, market insurance is MSB(p_M) - MSC(p_M), or

$$S_M(p_M) = \max\left\{0, \int_0^{G(p_M)} D^H(q)dq - c^{AC}(p_M)G(p_M)\right\} \tag{10}$$

There are two cases to consider. First, if there is no private insurance offered, the social surplus from private insurance is zero, i.e., $S_M = 0$. Second, if average cost intersects demand

[72]The private insurance policy may not be the same as the public insurance. Because the possibilities are innumerable and difficult to model, to make our problem tractable we assume the private policy is identical to the public policy. We assume zero load because we implicitly assume that in public insurance, the cost of which is equated to out-of-pocket consumer expenditures reduced. We assume community rating (price equal to average cost) because Indian insurers have poor information on individual health risk, especially in rural areas.

from below, then $S_M > 0$.

The MVPF for public insurance when there is private insurance is welfare under public insurance minus welfare under private insurance, or $S_P(0) - S_M(p_M)$. Consider the same two cases we discussed in the last paragraph. First, if there is no private insurance offered, then the incremental value of public insurance taking into account private insurance is the same as when there is no private insurance, i.e., $S_P(0) - S_M(p_M) = S_P(0)$. Second, if private insurance is offered in the market, then the value of public insurance when there is private insurance is greater than public insurance when there is no private insurance: $S_P(0) - S_M(p_M) < S_P(0)$, which flows directly from the fact that $S_M(p_M) \geq 0$.

Application using estimates of linear demand and average cost curves. To calculate the value of public insurance where there is private insurance, we first have to solve for the private market price of insurance. Setting average cost in Eq. (8) equal to price in Eq. (7), we get a market clearing price of $p_M = c(0)/(1-\kappa)$. This is positive if $\kappa < 1$ and there is private sale of insurance if $g(0) > c(0)$. With this price we can plug our estimates of demand from Eq. (7) and average cost from Eq. (8) into Eq. (10) to obtain the surplus from private insurance with linear demand and cost:

$$S_M(p_M) = \frac{1}{2\gamma}[g(p_M)]^2 = \frac{1}{2\gamma}\left[g(0) - \frac{\gamma}{1-\kappa}c(0)\right]^2 \tag{11}$$

Then the condition required for free insurance to be a welfare-improving use of public funds from the perspective of *ex post* demand is that $S_P(0)$ from Eq. (9) minus $S_M(p_M)$ from Eq. (11) is greater than zero.

Using a MCPF of $r = 1.25$ and the free-insurance arm (A) and sale-plus-transfer arm (B) to estimate demand, we calculate that free public insurance reduces welfare by INR 1219 ($70) per household at midline and INR 397 ($23) at endline (second panel of Table 10). Our findings are graphically illustrated in the C Panels of Figures 2 and 3, respectively. The surplus from private insurance is INR 1151 and 912 ($66 and 52) per household at midline and endline, respectively, so (zero-load, community-rated) private insurance is offered in equilibrium without public insurance. But free public insurance crowds out positive-price private insurance and the social surplus from public insurance is INR -69 and 515 (-$3.93 and $29) at midline and endline, respectively, less than the surplus with private insurance.

5.1.2 MVPF of public insurance with *ex ante* demand and no private insurance

Let us return to the counterfactual where there is no private insurance offered in the absence of public insurance. The condition in (6) for whether free insurance is a positive MVPF policy uses an *ex post* demand to value insurance, i.e., values insurance 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 on average higher if measured behind a veil of ignorance, before consumers have any private information on their expected costs (Hirshleifer, 1971; 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

$$D^{H,EA}(G) = D^H(G) + M(G) \quad \text{where} \quad M(G) = G(1-G)\left|\frac{\partial D^H}{\partial G}\right|\beta(G) \tag{12}$$

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.[73] 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,[74] 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.

The condition for the MVPF spent on free public insurance to be positive when we use *ex ante* demand for insurance and assume no insurance in the absence of public insurance is given by Eq. (6), except that one replaces $D^H(q)$ with $D^{H,EA}$ in Eq. (6) to capture *ex ante* demand.

Application using estimates of linear demand and average cost curves. To

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

[74] 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.

calculate the *ex ante* value of free insurance, we take three steps. First, we solve the linear demand in Eq. (7) 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 $D^{H,EA}(G)$.[75] We illustrate this *ex ante* demand with the dashed blue line in the A Panels of Figures 2 and 2. Third, we take a first-order Taylor approximation of the markup. This reveals that, if we define

$$\frac{1}{\gamma^{EA}} = \frac{1}{\gamma}\left(1 + \frac{\sigma}{\gamma}G(0)[1 - G(0)]\right) > \frac{1}{\gamma} > 0 \tag{13}$$

we can replace γ with γ^{EA} in Eq. (9) to approximate the value of free insurance.[76] In our 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 third panel of Table 10. *Ex ante* demand is sufficiently greater than *ex post* demand that free insurance is now always positive for welfare. The D panels of Figures 2 and 3 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:

$$MSB(p) = -\sum_i \mathbb{I}(g_i^* = 1) = -G(p). \tag{14}$$

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 consumer surplus 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.

[75] See Appendix B.1.1 for details.

[76] A derivation is presented in Appendix Section B.1.1.

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:

$$MSC(p) = (1+r)\frac{\partial c^{AC}(p)}{\partial p}G(p) - (1+r)G(p) + (1+r)[c^{AC}(p) - p]\frac{\partial G(p)}{\partial p} \tag{15}$$

The first term is new and reflects the change in average cost as price increase. Importantly, this is positive if 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^*, 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.,

$$-\frac{\partial c^{AC}(p^*)}{\partial p^*}G(p^*) + \frac{r}{1+r}G(p^*) - [c^{AC}(p^*) - p^*]\frac{\partial G(p^*)}{\partial p^*} = 0 \tag{16}$$

(A full derivation is presented in Appendix Section B.2.) 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.[77]

Optimal price using the *ex ante* demand 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. (12). To

[77] An incidental question the reader may have is whether the insurance companies that provided insurance under RSBY made or lost money under the premiums that the government paid them in Gulbarga and Mysore. The answer is that insurance companies lost money at midline because average cost was always above the roughly INR 200 ($11.42) premiums. However, at endline, the companies barely broke even when the government subsidized insurance prices to make it free to consumers.

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. (16) that replaces $G(p^*)$ and $\partial G/\partial p^*$ with $G^{EA}(p^*)$ and $\partial G^{(EA)}(p^*)/\partial p^*$.

Application with linear demand and cost. Using the linear demand (7) and average cost (8) curves we estimated with our experiment, we can solve analytically for the price that balances marginal social costs and benefit using the *ex post* demand value of insurance:

$$p^* = \frac{\left[\kappa - \frac{r}{1+r}\right] g(0) - c^{AC}(0)\gamma}{\left[\kappa - \frac{r}{1+r}\right]\gamma + [\kappa - 1]\gamma} \tag{17}$$

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

$$\kappa < 1 - \frac{1}{2}\left(\frac{1}{1+r}\right) \tag{18}$$

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

With $r = 1.25$, we estimate that the optimal price is INR 1052 ($60) using cost estimates from midline, or INR 528 ($30) using the lower cost at endline (Table 10, row 2). Adverse selection is sufficiently low that the optimal price rises with costs (columns 3 and 4).[78] The B panels of Figures 2 and 3 show that optimal price rises with the MCPF at midline and endline, respectively. 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 takeaway 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.

To calculate the optimal price using the *ex ante* demand 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 (17) and solve for p^*. The resulting welfare calculations for the case where $r = 1.25$ are presented in the third panel of Table 10.

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. The D panels of Figures 2 and 3 show that using the *ex ante* demand function

[78] 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.12.

increases the range of r (MCPF) over which free insurance is optimal at midline and endline, respectively. 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. (15). 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 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, which we reviewed in the introduction.

Our study has, however, a number of shortcomings. 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* predicted expenditure, we are unable to estimate precisely effects on post-enrollment expenditure. (However, 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 3.5 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

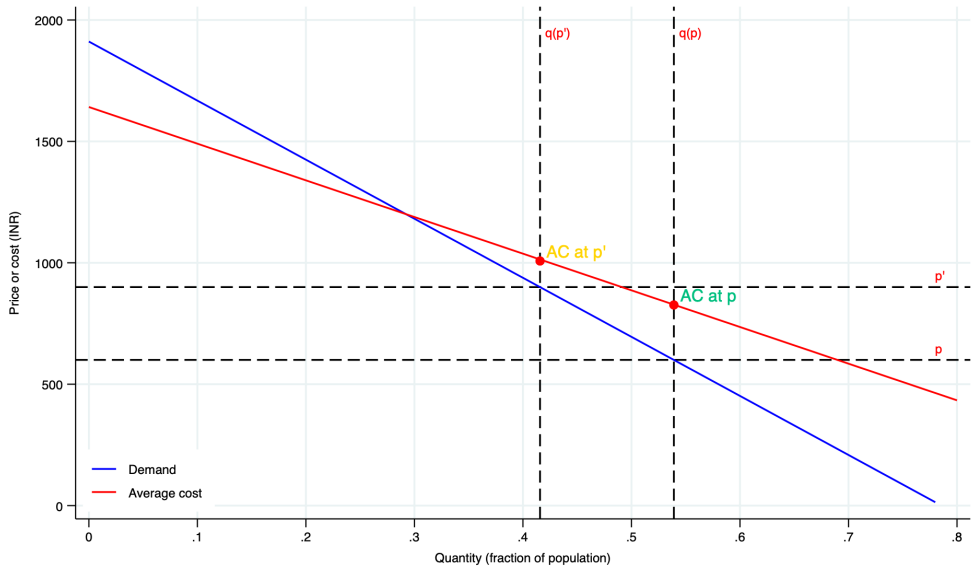
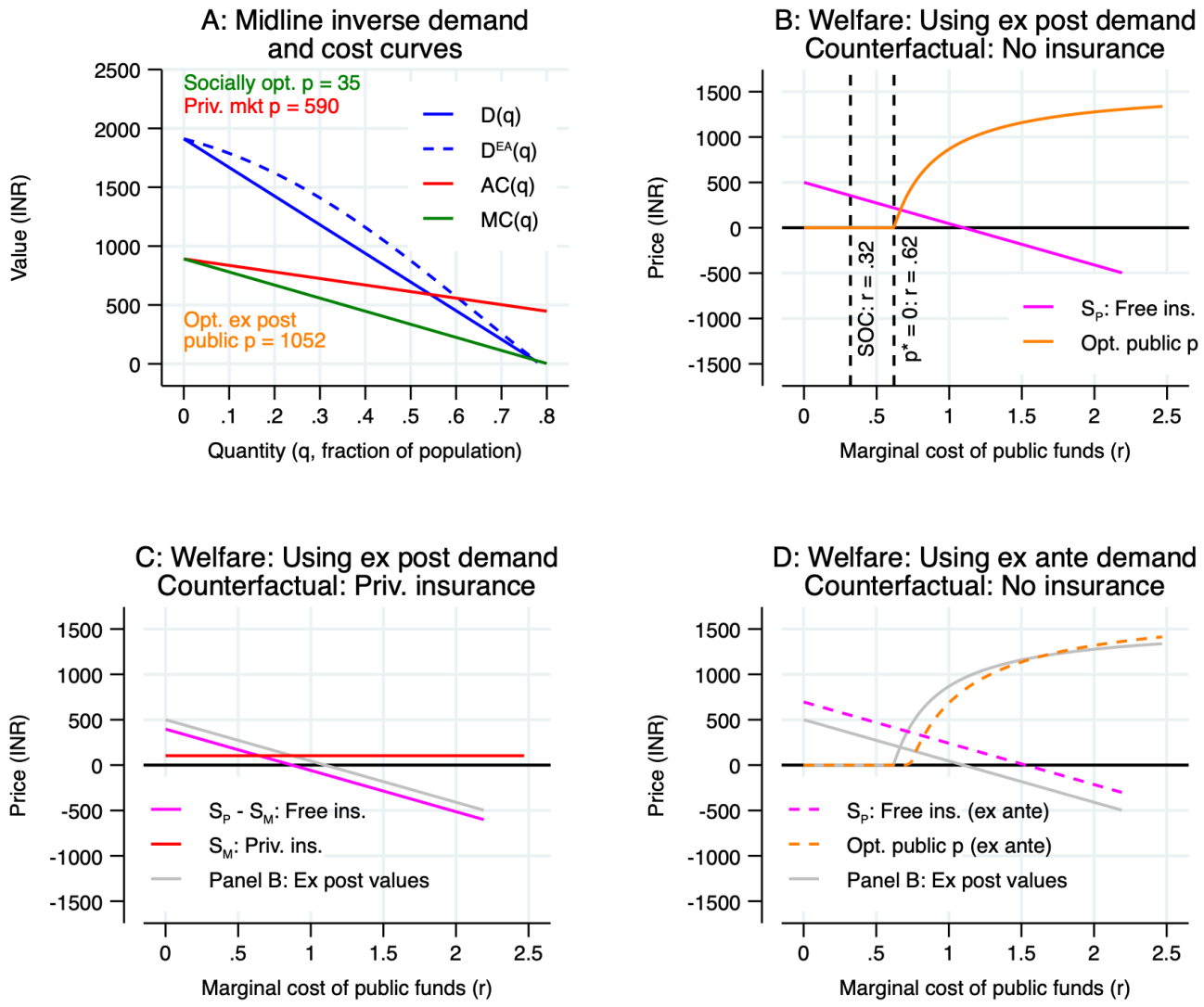


Figure 1: Illustration of adverse selection

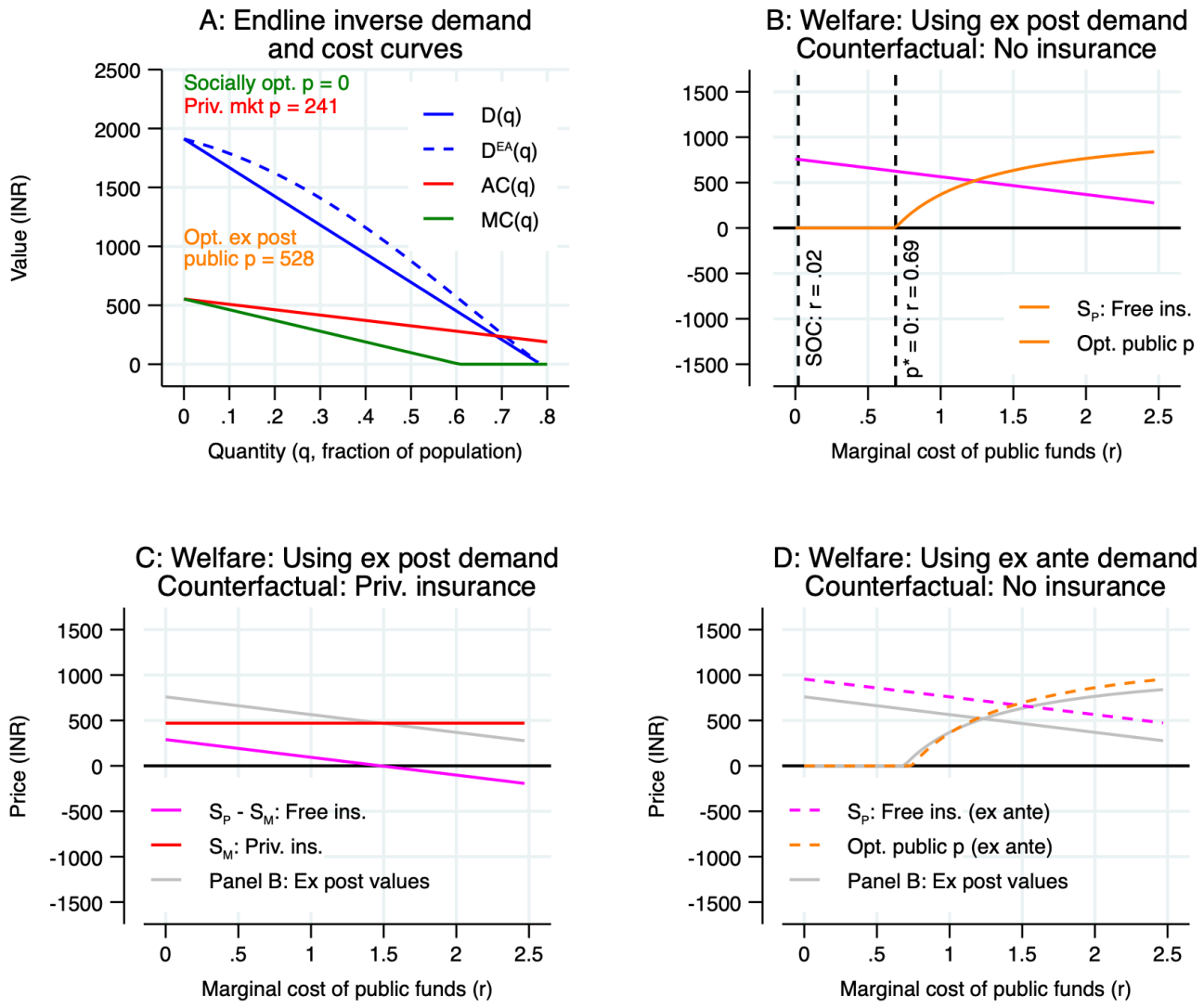
Notes. This figure illustrates how we identify the existence of adverse selection. It mimics demand and average cost lines in Einav and Finkelstein (2011). The population is arranged on the x-axis in declining order of willingness to pay for insurance; we normalize by population size, so the x-axis units 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 2: Inverse demand, cost and welfare calculations at midline.



Note. Midline use equals endline hospital expenditure x midline successful use. Panel A: 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. Private market price is given by intersection of AC line and demand curve. Optimal ex post line is pulled from Panel B at r = 1.25. Panel B: Formulas for MVPF with free insurance (magenta) and optimal price of public insurance (orange) are given in main text. The counterfactual is no insurance in absence of public insurance. Left black dashed line shows highest r at which second-order condition is not satisfied. Right dashed line gives r where p=0 satisfies the first-order condition using ex post demand. Panel C: Value of free insurance when counterfactual is community-rated private insurance without load. Red line shows surplus from private insurance alone. For comparison, gray line shows value of free insurance in panel B, the no insurance counterfactual. Panel D: Value of free insurance and optimal price of insurance using ex ante demand. For comparison, gray line shows value of free insurance in panel B.

Figure 3: Inverse demand, cost and welfare calculations at endline.



Note. Endline use equals endline hospital use x endline successful use. Panel A: 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. Private market price is given by intersection of AC line and demand curve. Optimal ex post line is pulled from Panel B at r = 1.25. Panel B: Formulas for MVPF with free insurance (magenta) and optimal price of public insurance (orange) are given in main text. The counterfactual is no insurance in absence of public insurance. Left black dashed line shows highest r at which second-order condition is not satisfied. Right dashed line gives r where p=0 satisfies the first-order condition using ex post demand. Panel C: Value of free insurance when counterfactual is community-rated private insurance without load. Red line shows surplus from private insurance alone. For comparison, gray line shows value of free insurance in panel B, the no insurance counterfactual. Panel D: Value of free insurance and optimal price of insurance using ex ante demand. For comparison, gray line shows value of free insurance in panel B.

Tables

Table 1: 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 2: 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 3: 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 4: Effects of 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% (4.78%) 0.000	6.99% (5.68%) 0.219	78.71% (0.90%) 0.000
(B) Sale of insurance + transfer	65.19% (7.84%) 0.000	7.83% (9.34%) 0.402	72.24% (1.36%) 0.000
(C) Sale of insurance	48.40% (8.08%) 0.000	12.79% (9.80%) 0.193	59.91% (1.60%) 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 premium & subsidy combinations on use of insurance and on hospital utilization (intent to treat estimates).

			(A) Free insurance			(B) Sale of insurance + transfer			(C) Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	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
Length of hospital stay	9483	1.27 (4.80)	-0.73 (0.63) .24	0.59 (0.56) .30	-0.20 (0.53) .70	-1.03 (0.97) .29	1.28 (1.04) .22	0.12 (0.55) .83	-0.19 (0.81) .81	-0.06 (0.76) .94	-0.24 (0.53) .65
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).

			Direct effect	Spillover effect	Total effect
	Obs.	Control mean (SD)	Coefficient (SE) P-value	Coefficient (SE) P-value	Coefficient (SE) P-value
Insurance use at 18 months					
Successful use (past 6 mos.)	9960	3.88% (19.32%)	3.85% (2.73%) .16	2.60% (4.47%) .56	5.89% (2.54%) .02
Failed use (past 6 mos.)	9960	1.50% (12.15%)	3.46% (1.87%) .06	-0.06% (2.67%) .98	3.41% (1.27%) .007
Insurance use at 3.5 years					
Successful use (past 6 mos.)	9458	0.49% (6.99%)	2.74% (1.16%) .02	-1.58% (1.60%) .32	1.50% (0.58%) .010
Successful use (most serious event)	9450	0.13% (3.59%)	0.50% (0.81%) .53	0.63% (1.06%) .55	1.00% (0.38%) .008
Failed used (most serious event)	9451	0.57% (7.52%)	1.24% (1.64%) .45	0.26% (2.28%) .91	1.45% (0.56%) .009
Healthcare use at 18 months					
Hospitalized on last visit (past 1 yr.)	9053	20.82% (40.61%)	1.00% (6.28%) .87	6.09% (12.47%) .63	5.79% (9.00%) .52
Healthcare use at 4 years					
Overnight stay for treatment	9483	22.97% (42.07%)	-12.42% (5.99%) .04	14.48% (7.36%) .05	-1.02% (4.16%) .81
Length of hospital stay	9483	1.27 (4.58)	-0.87 (0.70) .22	1.26 (0.75) .09	0.12 (0.45) .79
Outpatient (day) surgery	9474	6.76% (25.11%)	-4.07% (3.47%) .24	10.52% (4.34%) .02	4.21% (2.57%) .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: Effect of insurance access on total and out-of-pocket (OOP) medical expenditures (ITT).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
Medical spending at 18 months											
Annual OOP medical spending	10030	28776.72	13620.75	-1.0e+04	4576.07	6980.48	-4069.06	3318.33	10526.08	-6241.70	4908.54
		(31205.85)	(5890.55)	(8508.57)	(4697.45)	(6324.58)	(8612.39)	(4662.33)	(6093.18)	(7872.45)	(4503.38)
			.02	.24	.33	.27	.64	.48	.08	.43	.28
Log annual OOP medical spending	10030	9.59	0.39	-0.68	-0.22	0.79	-1.27	-0.35	0.62	-0.91	-0.20
		(1.91)	(0.24)	(0.40)	(0.39)	(0.42)	(0.53)	(0.39)	(0.36)	(0.44)	(0.39)
			.11	.09	.56	.06	.02	.38	.08	.04	.61
Annual medical non-hospital OOP spending	10030	23980.96	7260.11	-5553.37	2262.07	3161.37	-1758.17	1579.01	5430.70	-3412.59	2359.37
		(24866.47)	(4486.46)	(7033.69)	(4292.37)	(5470.58)	(7562.05)	(4225.99)	(5024.84)	(6823.73)	(4126.61)
			.11	.43	.60	.56	.82	.71	.28	.62	.57
Log annual medical non-hospital OOP spending	10030	9.37	0.23	-0.62	-0.33	0.63	-1.21	-0.47	0.55	-0.99	-0.34
		(2.04)	(0.26)	(0.41)	(0.40)	(0.43)	(0.57)	(0.41)	(0.36)	(0.51)	(0.40)
			.37	.13	.41	.14	.03	.26	.13	.05	.40
Medical spending at 3.5 years											
OOP medical spending (most serious event)	9396	21470.36	-1120.07	-5093.02	-5703.79	30035.21	-4.1e+04	-6866.75	2168.64	-1.2e+04	-8332.90
		(1.4e+05)	(18940.15)	(11620.16)	(15796.81)	(36760.53)	(39959.29)	(16060.03)	(20798.90)	(15004.06)	(15908.22)
			.95	.66	.72	.41	.31	.67	.92	.44	.60
Log OOP medical spending (most serious event)	9396	4.81	-0.04	-0.04	-0.07	-1.20	1.20	-0.12	-1.01	0.57	-0.50
		(4.77)	(0.55)	(0.84)	(0.72)	(0.84)	(1.07)	(0.72)	(0.98)	(1.26)	(0.75)
			.94	.97	.92	.15	.26	.87	.30	.65	.50

Notes. Table presents estimates of the effect of access to insurance at different prices and subsidies on out of pocket medical expenditures. Abbreviations: SD, standard deviation; SE, standard error; 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 changes relative to 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). SEs in parentheses; *p*-values below SEs.

Table 9: 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 (0.013) [0.000]	-0.14 (0.052) [0.005]	-0.066 (0.013) [0.000]	-0.15 (0.052) [0.005]
Pay (C)	-0.19 (0.014) [0.000]	-0.19 (0.057) [0.001]	-0.19 (0.014) [0.000]	-0.19 (0.057) [0.001]
Cost	0.0017 (0.0078) [0.825]	-0.0058 (0.0080) [0.466]	0.012 (0.0077) [0.124]	0.0033 (0.0083) [0.690]
B × Cost	0.029 (0.011) [0.010]	0.026 (0.011) [0.022]	0.023 (0.012) [0.056]	0.016 (0.013) [0.192]
C × Cost	0.060 (0.016) [0.000]	0.047 (0.017) [0.006]	0.034 (0.013) [0.011]	0.028 (0.014) [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
Demog. controls	N	Y	N	Y
Cost variable	EL1 medical exp, predicted	EL1 medical exp, predicted	EL2 medical exp, predicted	EL2 medical exp, predicted
Pr(C × Cost > B × Cost)	0.031	0.106	0.216	0.244
C × Cost × 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. Cost measure in columns 1-2 (3-4) are predicted midline (endline) medical expenditure as defined in Sec. 3.2.1. Spending variables are standardized to have unit 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 10: Welfare calculations using four different measures of cost.

	(1) ML use	(2) EL use	(3) ML attempt	(4) EL attempt
Ex post demand and no insurance counterfactual (p)				
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 post demand and private insurance counterfactual (p)				
Private ins. value	1150.894	912.341	1488.042	1361.131
Free ins. value	-1219.290	-396.718	-2286.548	-1895.617
Ex ante demand and no insurance counterfactual (p)				
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 in the first panel focus on ex post demand and assume no insurance in the absence of public insurance. This panel presents 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. Welfare calculations in the second panel focus on ex post demand and assume private insurance is the alternative to public insurance. The panel presents social surplus under private insurance and the net gain from public insurance (assuming public insurance prevents private insurance). The third panel focuses on ex ante demand (Hendren 2021) and assumes no insurance absent public insurance. It presents the net welfare benefit of free insurance and the price of insurance that satisfies the first-order condition (FOC) for optimal premium. 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.

Online Appendix

"Evaluating the impact and design of health insurance
in low-income countries: A field experiment in India"

Anup Malani, Cynthia Kinnan, Gabriella Conti,
Kosuke Imai, Morgen Miller, Shailender Swaminathan,
Alessandra Voena, and Bartek Woda

A Methods

A.1 Power calculations

Sample size calculations for household-level arms did not consider within village-correlation in outcomes. Moreover, we did not conduct power calculations for the size of our village-level arms.

A.1.1 Implementation of two-stage random assignment

Our two-stage randomization procedure was implemented separately in each district (Gulbarga, Mysore). In each stage, we matched units before randomization because matching reduces orthogonal variation (Imai, King and Clayton, 2009) and thereby increases statistical power.

The first stage was implemented in two steps. First, blocks of 20 matched villages (within each district) were created. Specifically, villages were stratified into quintiles based on the number of eligible households per village. Within each quintile, blocks were constructed using village-level sample averages, Mahalanobis matching on continuous and integer variables (head of household's level of education, age of head of household, number of children and number of rooms in the house), and caliper matching on binary variables (had major illness in last year and head of household was unemployed). These variables were constructed from the listing exercise to determine eligible households in each village of our sample prior to our baseline survey. Second, within each block, villages were randomly assigned to the 5 village-level conditions.

The second stage of our randomization also had two steps. First, households were sorted into blocks of 10 matched households. These blocks were constructed using household-level data gathered during our listing exercise and Mahalanobis caliper matching on the same variables used for constructing village blocks. Second, within each block, households were randomly assigned to 4 household conditions based on the village condition to which we assigned their village in stage one.

A.1.2 Roll-out of intervention

This experiment employed the same enrollment method (mobile enrollment vans that visit villages on a specific day) the government used for RSBY enrollment of below poverty-line households (BPL), a population excluded from our study. However, there were two differences in the marketing of RSBY to the above poverty-line households in our sample relative to the government's marketing to BPL households. First, households were informed of their treatment group assignment and, if appropriate, given a cash transfer roughly 14 days prior to arrival of the enrollment van. Second, study personnel visited households in groups 1-3 the day of enrollment to guide interested households to the enrollment van. By contrast, the government conducted its

enrollment drive by circulating flyers (chits) to eligible households a few days before the van arrived. The government did not guide eligible households to the enrollment van.[1] We collected the purchase price from sample households in groups 2 and 3 (sale of insurance with and without an unconditional cash transfer) that enrolled in RSBY at the van. For all sample households in group 1 (free insurance), we paid for the full purchase price from grant funds.

The Karnataka government automatically renewed RSBY coverage annually for non-study and enrolled study households in 2016 and 2017 without any action—even payment—by enrollees or the study. RSBY technically ended on August 31, 2018, because it was replaced by Pradhan Mantri Jal Arogya Yojana (PMJAY). As of completion of data gathering, Karnataka had agreed to implement PMJAY but had not yet rolled it out. The state effectively transitioned from RSBY to PMJAY in late 2019.[2]

A.2 Data collection

In March to June 2013 and again in November–December 2013, we conducted listing exercises wherein we surveyed 1 adult in 25,000 households in candidate villages to identify eligible households and gather data to construct blocks of similar villages and households prior to randomization.

In August 2013 to January 2014, a baseline survey was administered to the female and male most knowledgeable about household finances and a female of childbearing age in each sample household we could reach. The biomarker survey conducted as part of the baseline was administered to the male most knowledgeable, a woman of childbearing age, and a child under the age of 18 if available in roughly, a randomly-selected one-third of sample households. This subsample was selected in two steps. First, we stratified households by the two districts in our study. Second, we selected one third of villages in each district and conducted the biomarker survey on each sample household in the selected villages. A discrepancy between one-third of households and the number on which the biomarker survey was attempted reflected both the fact that we selected one-third of villages not households and villages have different sample sizes. The actual number of households ultimately surveyed – for both the main and biomarker survey – was less than the number attempted because of non-response. Households were paid INR 250 ($14.27) as a participation incentive for completing major sections of the survey. We performed back checks on 10% of households, a rate known to surveyors ex ante.

In January to May 2017, a midline survey was administered in the same manner as the baseline survey, including the biomarker survey on the same randomly-selected sample households. Households were paid the same participation incentives and back checks were performed in the same manner.

In March to June 2019, we conducted an endline survey of all sample households to measure outcomes after approximately 3.5 years. We asked the female most knowledgeable about household finances from baseline a subset of questions about personal health status and household health care utilization and finances. No biomarker survey was conducted at endline. We paid households INR 50 ($2.85) as a participation incentive since only 1 individual was surveyed and for a shorter period. We performed back checks on 10% of households, a rate known to surveyors ex ante.

Figure A.5 presents the study timeline, both in terms of the intervention and the multiple surveys conducted.

[1] The study's greater advance notice allowed households in the pure insurance condition more time to get funds together to pay the premium, if desired.

[2] Because RSBY ended 6 months before our endline survey, we asked the respondent to recall hospital utilization 6 months before a notable holiday, Dussehra, that occurred just before the intervention ended.

Figure A.6 presents a chart that shows the flow of subjects through each arm of the two-stage design. It also shows attrition at each data collection point.

A.3 Economic calculations

We convert Indian Rupees (INR) to 2019 US Dollars ($) using a purchasing power parity adjusted exchange rate of INR 17.52/dollar (OECD, 2020).

We calculated the demand elasticity for hospital insurance with the following formula:

$$\frac{\text{(Enrollment in Group 1 - Enrollment in Group 2)/Enrollment in 2}}{\text{(Price for insurance in 2 - Price in 1)/Premium in 2}}$$

The price for group 2 is INR 183 ($10.45), including the cost of the insurance card. 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.

A.4 Health data

We examine multiple categories of health outcomes. Each category included multiple individual indicators: self-reported health (2 categories at midline, 3 at endline), objective health indicators (7 midline), chronic disease (15 midline, 17 endline), quality of life (1 midline, 14 endline), mental health (3 endline), childbirth (8 midline, 10 endline), and mortality (4 endline). A final secondary health outcome was an index of health, following Kling, Liebman and Katz (2007), equal to the average of z-scores for individual health outcomes.

Biomarkers. Biomarkers reported in Table A1 were constructed using anthropometric measurements from adult respondents at the midline survey. These included recording an individual's weight, height, waist, hip, and mid-upper arm circumferences, skinfolds, and blood pressure. Mid-upper arm circumference and an aggregate measurement of skinfolds (biceps, triceps, and sub-scapular) were included directly in the model. Using these measurements, we created additional outcomes of interest. Overweight or obese was categorized as a body mass index (BMI) greater than or equal to 25, while underweight was categorized as a BMI less than 18.5 for both adult males and females. Abdominally obese was categorized using an individual's waist-to-hip ratio (WHR); adult males with a WHR greater than 0.9 and adult females with a WHR greater than 0.85 were classified as abdominally obese. Finally, we categorized individuals as having Hypertension, grade 1 or Hypertension, grade 2 based on their systolic and diastolic blood pressure measurements. An individual was considered to have Hypertension, grade 1 if systolic blood pressure was greater than or equal to 130 and less than 140 OR a diastolic blood pressure greater than or equal to 80 and less than 90. Hypertension, grade 2 was defined as systolic blood pressure greater than 140 OR diastolic blood pressure greater than 90.

A.5 LASSO details

Predicted midline medical spending is constructed as follows. Medical spending at midline is winsorized at 0.5% and 99.5%. A model is then estimated which predicts midline medical spending as a function of the full set of available baseline covariates which relate to health and health spending[3], as well as indicators for

[3]The baseline questionnaire is available at https://drive.google.com/file/d/1RAORj0wbnDU1R-QU1j6zAI5UL3FCFkTV/view?usp=sharing.

missing data on each baseline variable. The model is estimated with linear regression. Predictions are fitted via 10-folds cross-validated LASSO on the control group of 2152 (or smaller due to missing outcomes). We then predicted the outcomes in treatment group with the trained model. The penalty parameter is selected under the minimum rule. Figure A.7 shows the resulting distribution of predicted values for midline medical spending. The correlation between predicted midline medical spending and actual, winsorized midline medical spending is 0.119, significantly greater ($p < 0.001$) than the 0.057 correlation between actual, winsorized baseline medical spending and actual, winsorized midline medical spending.

Predicted endline medical spending is calculated and employed in the same manner as predicted midline medical spending, with one change. 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 covariates which are selected by LASSO to predict midline spending. Despite this change, the correlation between predicted endline medical spending and actual, winsorized endline medical spending is 0.071, greater than the 0.054 correlation between actual, winsorized baseline medical spending and actual winsorized midline medical spending, but not significantly so ($p = 0.368$).

A.6 Selection by baseline variables

An alternate approach to testing for adverse selection would be to use baseline health and spending directly as proxies for subsequent health/health costs. Here we describe the this approach and the results, displayed in Table A.3 .

Inasmuch as RSBY insurance covers visits to empanelled facilities, the likelihood of visiting a health care facility is a natural predictor to use when forecasting differential incentives to enroll. Columns 1 to 4 use the baseline realization or predicted midline probability of visiting a medical facility for the male and female respondent.[4] Using baseline values (cols 1 and 2) yields very small and insignificant estimates of the degree of selection (i.e., δ_3); moreover, the sign of the (negligible) coefficient estimate flips sign between specifications using the male vs female respondent. One way to benchmark the magnitude of selection (if any) is to compare the degree of selection implied by the point estimate relative to mean enrollment: using the (insignificant) point estimate for the effect of male facility visits, a one standard deviation (SD) reduction (i.e., lower risk) is associated with a 1.4% increase in the enrollment rate; the number for female health is even smaller in magnitude and opposite in sign (-0.8%). Using the predicted midline values (cols 3 and 4) yields even smaller (and likewise insignificant) estimates of the degree of selection, with a one SD reduction in predicted probability for males leading to a 0.9% reduction in predicted enrollment; and a 0.0% effect associated with changing predicted probability for females. In summary, we find no evidence of selection using either baseline facility visits or predicted midline facility visits. For comparison, the relative average enrollment rates in arm B vs arm C (paid+cash vs. paid) demonstrate that providing a INR 220 unconditional cash transfer causes an increase in enrollment of 22% (71.6pp vs 58.8pp), an order of magnitude larger than any of the (insignificant) selection effects.

Columns 5 to 8 focus on the overall health of the male and female survey respondents. Using baseline health of either (cols 5 and 6), there is no statistically significant selection; δ_3 is not significantly different from zero. In addition to being statistically indistinguishable from zero, the point estimates imply very small

[4] We flip the sign so that a higher number corresponds to better health. We do the same for expenditure, below.

degrees of selection relative to mean enrollment: using the (insignificant) point estimate for the effect of male health, a one standard deviation improvement in baseline health is associated with only a 2.1% reduction in the enrollment rate; the number for female health is similar (2.0%).

We next turn to predicted midline health (cols 7 and 8). Using male health (col 7) shows no detectable selection. In the case of female health (col 8), we do observe marginally statistically significant selection; the sign of $delta_3$ is positive, implying *advantageous* selection. However, the point estimate is small, implying that a one standard deviation improvement in predicted midline female health is associated with only a 3.6% increase in the enrollment rate. This is less than one fifth of the 22% effect of the unconditional cash transfer. In addition, as we discuss below, once we control for additional baseline characteristics and/or correct for multiple testing, the estimate is no longer significantly different from zero at conventional levels.

Next we examine total household medical expenditure (columns 9 and 10). As with visiting a medical facility, this is a potentially important variable which households might forecast and use to determine their willingness to pay to enroll in insurance; it is also arguably the key variable from the perspective of the government forecasting its costs. When we examine baseline medical spending, we find a very small and statistically insignificant δ_3 coefficient of -0.005, which would imply that a 1 SD decrease in baseline medical spending is associated with only a 0.8% decrease in enrollment under cost-sharing. Turning to *predicted* midline health spending, the estimated δ_3 is -0.04, significantly different from zero at the 1% level. Consistent with adverse selection, those with lower predicted spending are differentially less likely to enroll under cost sharing.

B Welfare

In this section, we derive the welfare formulas we report in the main text.

B.1 Does free insurance improve welfare?

Marginal social benefit. Assume that a given consumer i obtains utility $u(g_i, x_i)$ from consuming a government service and other goods x_i, where we make the usual assumptions required for u to be well-behaved. In our case, the government service, i.e., RSBY insurance, is discrete or indivisible and the consumer can only consume one unit of it at a price or user fee of p, i.e., $g_i(p) \in \{0, 1\}$. (Except for the case where we calculate the "*ex ante* value of insurance", however, these equations generalize to any government services, not just insurance.) The consumer has income y_i. We normalize the price of other goods to be 1, but we assume that the consumer must pay a consumption tax of rate τ on those goods.[5] Although the government could finance the government service partly with a user fee and partly with taxes (implying that taxes depend on the price of the government service), we assume that the tax rate is fixed. We justify this later.

The consumer's indirect utility is

$$V(y_i, \tau, p) = \max\{ \max_{x_i} u(1, x_i) + \lambda_i[y_i - p - (1+\tau)x_i, \\ \max_{x_i} u(0, x_i) + \lambda_i[y_i - (1+\tau)x_i\} \tag{19}$$

Let $g_i^*(y_i, \tau, p)$ and $x_i^*(y_i, \tau, p)$ be the consumer's optimal choice of government service and other goods, and

[5] In India, individuals must pay a goods and service tax that averages 18%. This generated 53% of government revenue in 2022 (Thakur and Nagarajan, 2023)

let $\lambda_i^*(y_i, \tau, p)$ be the associated Lagrange multiplier. The marginal private benefit of providing insurance (versus not) at price p to consumer i is equal to her willingness to pay for insurance ($WTP_i(y_i, \tau, p)$).[6] In our situation, where the offer price is zero, this is approximately

$$WTP_i(y_i, \tau, 0) \approx \frac{u(1, x^*(y_i, \tau, 0)) - u(0, x^*(y_i, \tau, 0))}{\lambda^*(y_i, \tau, 0)} \tag{20}$$

where we could replace the approximation with an equality if utility were separable in the government service and other goods.

Assuming equal welfare weights, the marginal social benefit at price p is the sum of individual WTP for government service at that price across the population.[7] Let $G(p)$ be aggregate quantity demanded for the government service at price p. If insurance is a small portion of budget or there are no income effects, marginal social benefit at price p is approximately the area under the inverse aggregate Hicksian demand curve $D^H(G)$ and above p.[8] For notational convenience, we will treat aggregate demand curves as continuous, so

$$MSB(p) = \sum_i WTP_i(y_i, \tau, p) \approx \int_0^{G(p)} [D^H(q) - p]dq = \int_0^{G(p)} D^H(q)dq - pG(p). \tag{21}$$

The marginal social benefit from free insurance is this expression evaluated at $p = 0$.

Marginal social cost. The government's net expenditure on consumer i is

$$t_i(y_i, \tau, p) = [c_i^{MC}(p)g_i^*(y_i, \tau, p) - pg_i^*(y_i, \tau, p) - \tau x_i^*(y_i, \tau, p) \tag{22}$$

where $c_i^{MC}(p)$ is the government's marginal cost of providing service g_i^* to consumer i—in our context, the government's expenditure on healthcare for consumer i given her coverage. Marginal cost depends on price because of adverse selection: $\partial c_i^{MC}/\partial p > 0$. We assume income effects are sufficiently small or the user fee is sufficiently small that we can ignore that marginal cost also depends on the tax rate. The government obtains revenue from the price for the government service (the second term in the equation above) and from sales tax (the last term).

The government can finance the provision of its service by a combination of (a) charging for it directly (user fees), (b) taxing other goods, and (c) borrowing the money from other programs or creditors. In this section, we will assume the government relies on user fees and borrowing, and that the government's cost of funds is $R = 1 + r > 1$. (In the next section, we consider a combination of user fees and borrowing.) This assumption is reasonable because the Indian government is unlikely to change the tax rate based on

[6] In the MVPF framework, MSB is equated to WTP for insurance, which we measure using revealed preference for insurance. An alternative, objective measure, might employ the impacts of RSBY access on different dimensions of health and non-medical consumption times WTP for each dimension. We would again face the question of how to measure WTP for each dimension. Moreover, we would have ensure that we do not fail to measure any relevant dimension that might be valued. We have no direct measure of WTP for health and non-medical consumption. Although we have a large number of health measures, there are surely many we miss. But we do measure demand for insurance, so use that our measure of WTP for the insurance.

[7] Since we are dealing with a non-marginal change in government service from 0 to 1, this is better described as an incremental social benefit.

[8] Alternatively, if insurance is a small portion of budget or there are no income effects, marginal social benefit at price p is approximately the area under the inverse aggregate Marshallian demand curve $D^M(G)$ and above p.

the provision of public health insurance, if for no other reason than that government expenditures on such insurance are a tiny fraction of its budget.[9]

The marginal social cost of funds spent on providing the government service is the sum of the government's net expenditure on all consumers who purchase the government service at price p:

$$\begin{aligned} MSC(p) &= (1+r)\sum_i [c_i^{MC}(p) - p] g_i^*(y_i, \tau, p) \\ &\approx (1+r)\left[\int_{\overline{p}}^{p} [c^{MC}(z)G(z)]dz - \int_{\overline{p}}^{p} [zG(z)]dz\right] \quad (23) \\ &= (1+r)\left[\int_0^{G(p)} [c^{MC}(D(w))w]dw - pG(p)\right] \quad (24) \\ &= (1+r)[c^{AC}(p)G(p) - pG(p)] \quad (25) \end{aligned}$$

where $D(q)$ is the inverse demand curve for the government service. We treat these marginal cost curves as continuous so that we can approximate them with continuous functions $c^{MC}(p)$ in Eq. (23). We swap axes of integration and integrate over quantity in Eq. (24). In the final step we use the fact that the area under the marginal cost curve from $G = 0$ to $G(p)$ to in the price-quantity plane is equal to average cost at $G(p)$ times $G(p)$: $c^{AC}(p) = \int_{\overline{p}}^{p} [c^{MC}(q)G(q)]dq/G(p)$.

B.1.1 Valuation with *ex ante* demand and linear demand and cost functions

Even though we estimate a linear demand function, inverting that function to obtain *ex ante* willingness to pay (WTP) for insurance in Eq. (12) yields a non-linear function. We can numerically integrate it, but obtaining standard errors on our estimate is non-trivial. To simplify estimation, we use a first-order approximation of the non-linear *ex ante* WTP for insurance.

Ex ante demand has two parts, *ex post* inverse demand $D(p)$ plus a markup $M(p)$. Here we suppress the H superscript because we will vary how we estimate demand, estimating both a Marshallian demand (with group C) or an upper bound on Hicksian demand (with arm B). We need to linearize both $D(p)$ and $(M(p)$. We will indicate linearized functions with a upper-bar: $D^{\bar{E}A}(G) = \bar{D}(G) + \bar{M}(G)$.

Our estimates of demand are, by assumption, already linear. We start with estimates for the linear *ex post* demand in Eq. (7). The implied inverse *ex post* demand is $\bar{D}(G) = [G(0)/\gamma] - [1/\gamma]G$, where we suppress the i subscript that indexes households and we replace lowercase g with capital G because we will be speaking of demand in terms of fractions of all households rather than for an individual household.

Next we linearize the markup $M(G) = G(1-G) \cdot |\partial D/\partial p| \cdot \beta(G)$. We follow Hendren (2021) and start with approximating $\beta(G)$ with $\tilde{\beta}(G) = \sigma[D^H(G(p)) - \int_{G(0)}^{G(p)} D^H(s)ds]$. This changes the (still non-linear) markup to

$$M(G) = G(1-G) \cdot \left|\frac{\partial D}{\partial G}\right| \cdot \tilde{\beta}(G) \quad (26)$$

If we plug linear demand into $\tilde{\beta}(G)$ we get $\tilde{\beta}(G) \approx \sigma[\bar{D}(G) - (1/2)(G(0) - G)\bar{D}(G)] = \sigma[1 - (1/2)(G(0) - G)]\bar{D}(G)$, where σ is the coefficient of absolute risk aversion.

[9] PMJAY, the successor to RSBY, constitutes less than 10% of the budget of the Ministry of Health & Family Welfare in India. And government spending on health is roughly 3.5% of India's total government budget and less than 1% of India's GDP (National Council of Applied Economic Research, 2023).

We take a first-order approximation around $G(0)$ and obtain $\bar{M}(G)|_{G(0)} \approx M(G(0)) + M'(G(0))[G - G(0)]$. We can zero out the first term $M(G(0))$ right away because $\bar{D}(G(0)) = 0$ implies $\tilde{\beta}(G) = 0$. This leaves us with

$$\bar{M}(G)|_{G(0)} \approx M'(G(0))[G - G(0)] \tag{27}$$

The derivative of the markup evaluated at $G(0)$ is

$$M'(G(0)) = \left[(1 - 2G(0)) \cdot \frac{1}{\gamma} \cdot \tilde{\beta}(G(0))\right] + \left[G(0)[1 - G(0)] \cdot \frac{1}{\gamma} \cdot \tilde{\beta}'(G(0))\right] \tag{28}$$

Let's compute each part. The first term is 0 because $\tilde{\beta}(G(0)) = 0$. The second term depends on $\tilde{\beta}'(G)$, which is

$$\tilde{\beta}'(G) = \sigma \left(\frac{1}{2}\bar{D}(G) - \left[1 - \frac{1}{2}(G(0) - G)\right] \frac{1}{\gamma}\right) \tag{29}$$

Evaluated at $G(0)$, which implies this becomes $\tilde{\beta}'(G) = -\sigma/\gamma$ because $\bar{D}(G(0)) = 0$. This implies $M'(G(0)) = -G(0)[1 - G(0)]\sigma/\gamma^2$. Plugging this into our approximation of the markup yield

$$\bar{M}(G)|_{G(0)} \approx -\frac{\sigma}{\gamma^2} G(0)[1 - G(0)](G - G(0)) > 0 \tag{30}$$

This is positive because $G < G(0)$—quantity is larger at zero price. It converges to 0 as G approaches $G(0)$, maximal demand.

Returning to linearized *ex ante* inverse demand, we plug in linear *ex post* inverse demand and linearized markup to obtain

$$\begin{aligned}
\bar{D}^{EA}(G) &= \left[\frac{1}{\gamma}\right](G(0) - G) - \left[\frac{\sigma}{\gamma^2} G(0)[1 - G(0)]\right](G - G(0)) \\
&= \frac{1}{\gamma}\left(1 + \frac{\sigma}{\gamma} G(0)[1 - G(0)]\right)(G(0) - G) \\
&= \frac{1}{\gamma^{EA}}(G(0) - G)
\end{aligned} \tag{31}$$

where

$$\frac{1}{\gamma^{EA}} = \frac{1}{\gamma}\left(1 + \frac{\sigma}{\gamma} G(0)[1 - G(0)]\right) > \frac{1}{\gamma} > 0 \tag{32}$$

This means that *ex ante* inverse demand as a steeper slope than *ex post* inverse demand. Moreover, because inverse demand is 0 when $G = g(0)$, inverse demand is a rotation around a hinge at $G(0)$ on the x-axis.

B.2 What is the optimal insurance premium?

In this section, we modify our MVPF analysis to ask: what is the optimal price of RSBY insurance, conditional on offering it.

Marginal social benefit. An individual consumer's indirect utility conditional on purchasing the

government service is

$$V(y_i, \tau, p) = u(1, x_i^*) + \lambda_i^*[y_i - p - (1+\tau)x_i^*] \tag{33}$$

where we suppress the reliance of optimal x_i and the multiplier on income, tax rate and government service price. The willingness to pay for an increase in price is marginal utility from increasing price divided by the marginal utility of income: $(\partial V_i/\partial p)/\lambda_i^* = -1$. This is simply Roy's identity. It is negative because higher price lowers utility conditional on buying the government service. Lower price would have the opposite signed effect.

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

$$MSB(p) = -\sum_i \mathbb{I}(g_i^* = 1) = -G(p). \tag{34}$$

This is proportional to aggregate quantity demanded at price p.

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:

$$MSC(p) = (1+r)\frac{\partial c^{AC}(p)}{\partial p}G(p) - (1+r)G(p) + (1+r)[c^{AC}(p) - p]\frac{\partial G(p)}{\partial p} \tag{35}$$

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.,

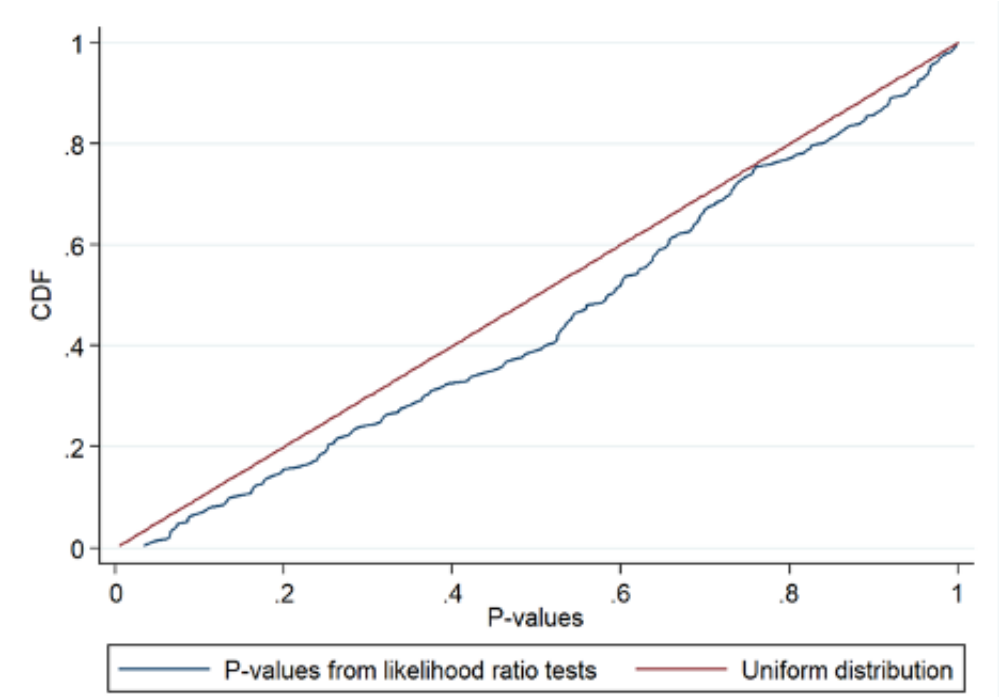
$$-G(p^*) - \left[(1+r)\frac{\partial c^{AC}(p^*)}{\partial p^*}G(p^*) - (1+r)G(p^*) + (1+r)[c^{AC}(p^*) - p^*]\frac{\partial G}{\partial p^*}\right] = 0 \tag{36}$$

$$-\frac{\partial c^{AC}(p^*)}{\partial p^*}G(p^*) + \frac{r}{1+r}G(p^*) - [c^{AC}(p^*) - p^*]\frac{\partial G}{\partial p^*} = 0 \tag{37}$$

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.

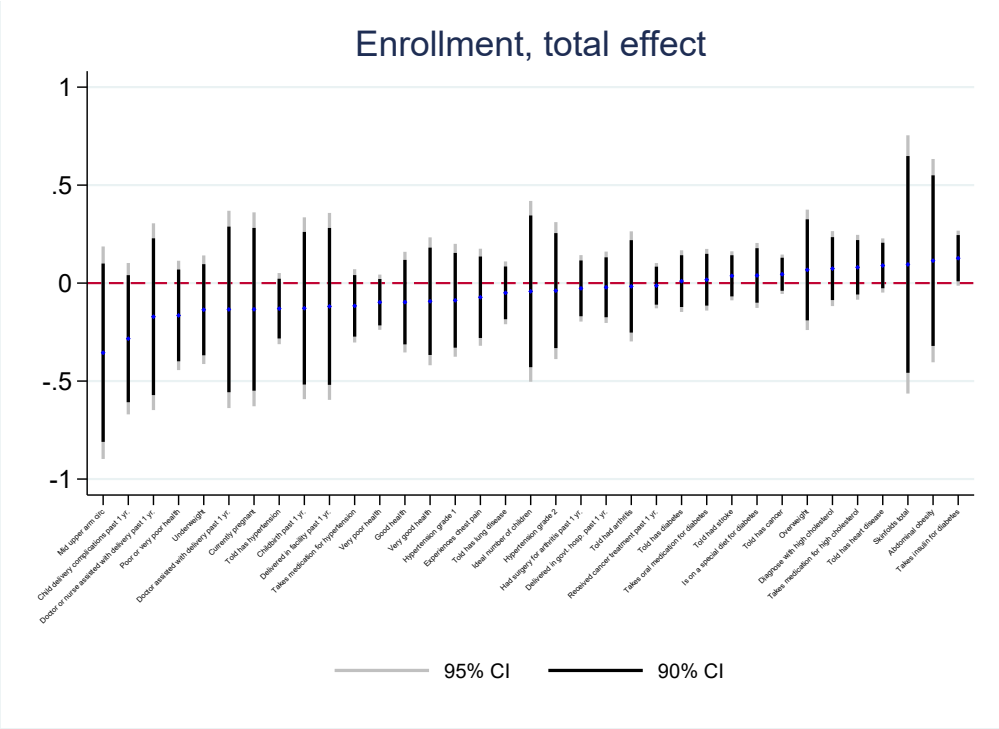
C Figures

Figure A.1: Balancing test result.



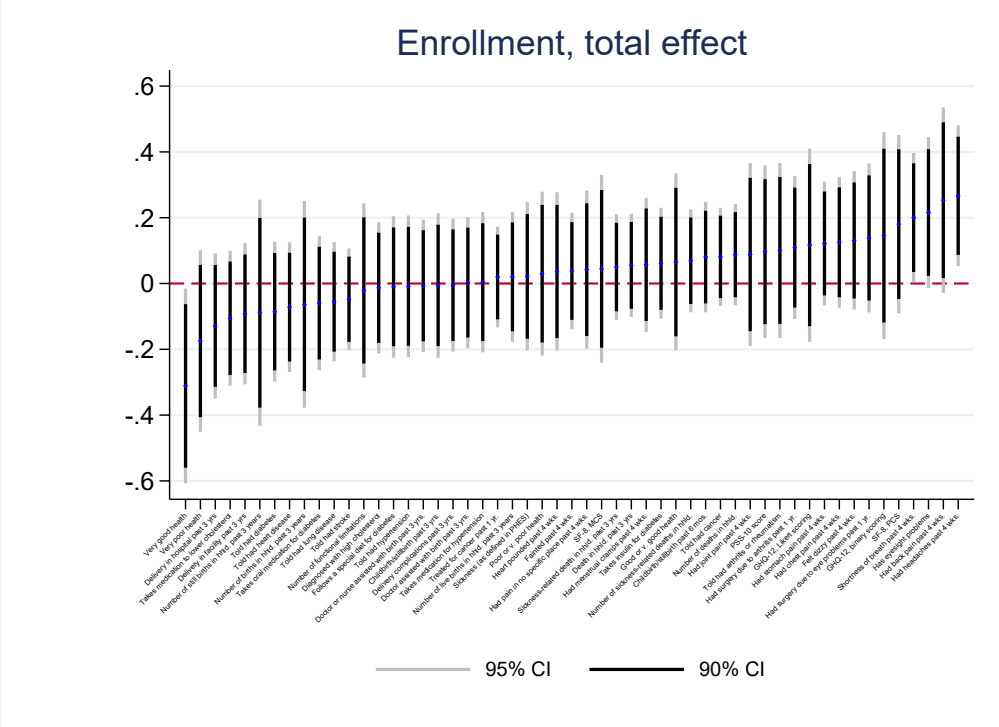
Notes. Figure presents the cumulative distribution of p-values (a) from likelihood ratio tests checking balance in outcomes across arms and (b) from a uniform distribution (capturing perfect randomization).

Figure A.2: Estimates of total effects of insurance (CATE) on a randomly-selected set of health outcomes from midline (18 months).



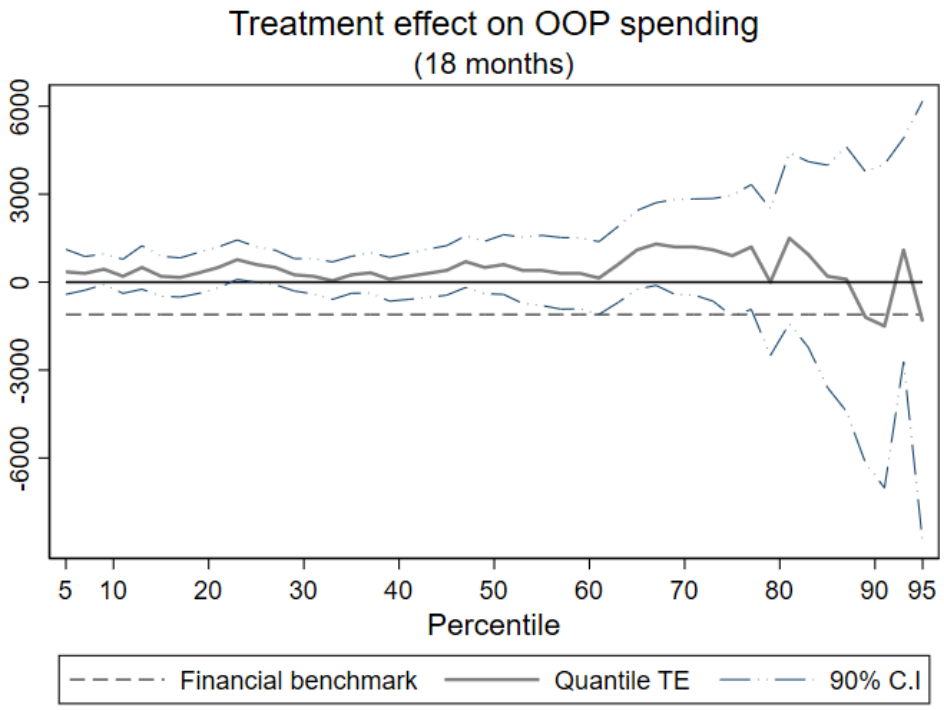
Notes. Figure presents a coefficient plot of estimates of total effects based on IIT coefficients for a randomly-selected set of health outcomes from midline (18 months after treatment).

Figure A.3: Estimates of total effects of insurance (CATE) on a randomly-selected set of health outcomes from endline (3.5 years).



Notes. Figure presents a coefficient plot of estimates of total effects based on CATE coefficients for a randomly-selected set of health outcomes from endline (3.5 years after treatment).

Figure A.4: Quantile treatment effects on OOP spending



Notes. The solid gray line plots the quantile treatment effects of access to RSBY (a pooled indicator for assignment to treatment arms A, B or C) on midline out-of-pocket (OOP) medical spending from the 5th to 95th percentiles. The dashed line shows the effect on OOP spending which would be expected if insurance was purely financial: the control mean of medical OOP spending (INR 28,777) multiplied by the pooled treatment effect on RSBY utilization (3.85 percentage points).

Figure A.5: Timeline of study.

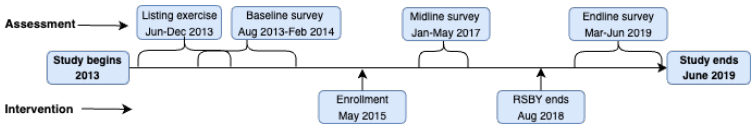
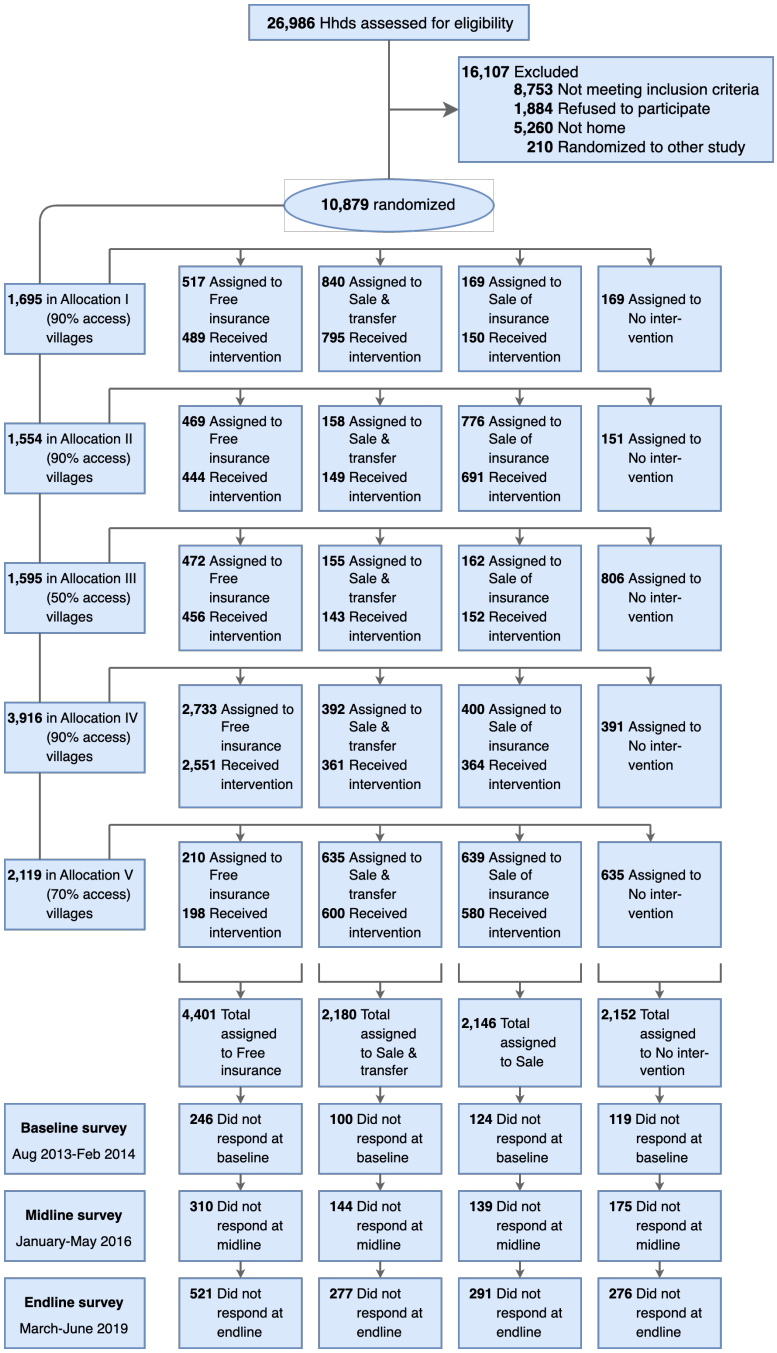
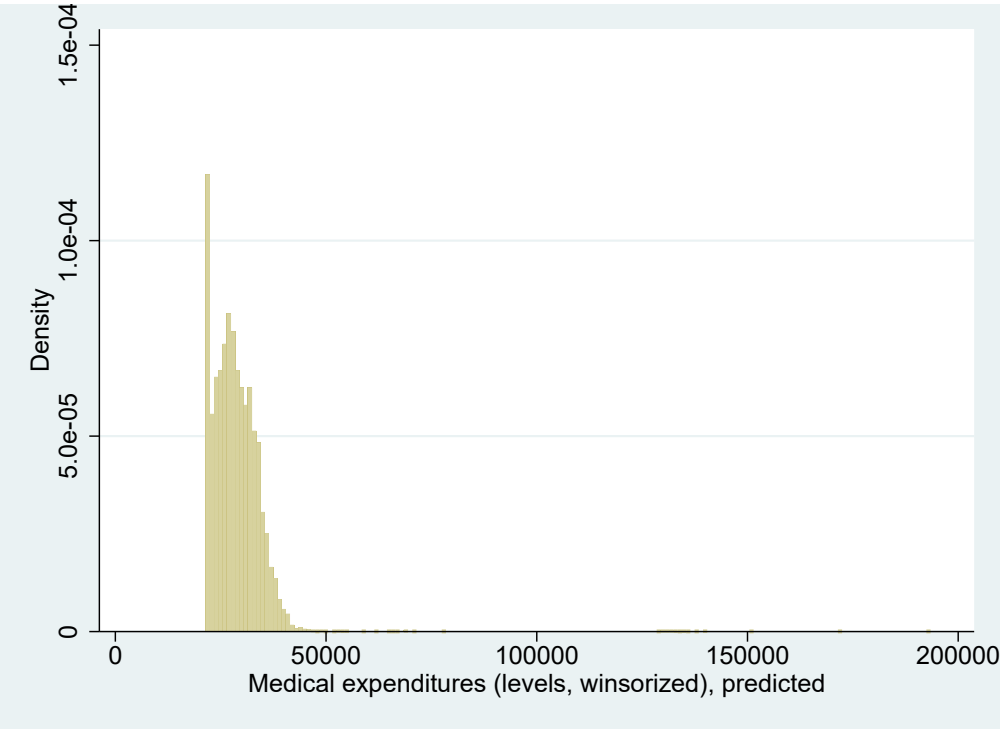


Figure A.6: Subject flowchart, with exclusions and attrition by endline.



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 A.7: Predicted medical expenditures



D Tables

Table A.1: Outcomes measured by topic at midline (18 months post-intervention) and endline (3.5 years post-intervention) surveys.

	Midline (18 months)	Endline (3.5 years)
Utilization		
Inpatient or overnight hospital stay	x	x
Successful RSBY use for treatment	x	x
Other utilization metrics		
Outpatient (day) surgery		x
Failed RSBY use for treatment	x	x
Health value		
Sickness		x
Good or very good health	x	x
Very good health	x	x
Poor or very poor health	x	x
Very poor health	x	x
Chronic disease		
Told had hypertension	x	x
Takes medication for hypertension	x	x
Told had diabetes	x	x
Takes oral medication for diabetes	x	x
Takes insulin for diabetes	x	x
Follows a special diet for diabetes	x	x
Told had cancer	x	x
Treated for cancer past 1 yr.	x	x
Told had lung disease	x	x
Told had heart disease	x	x
Told had stroke	x	x
Told had arthritis or rheumatism	x	x
Had surgery due to arthritis past 1 yr.	x	x
Diagnosed with high cholesterol	x	x
Takes medication to lower cholesterol	x	x
Has eyesight problems		x
Had surgery due to eye problems past 1 yr.		x

Note. Table presents outcomes in first column, and an indicator (x = yes, blank = no) for whether the outcome was measured at midline (column 2) and endline (column 3). Abbreviations: RSBY, Rashtriya Swasthya Bima Yojana; hhld, household.; resp., respondent; yr., year; mo., month; wk., week; kg., kilogram; SF-8, Short-form 8; PCS, physical component score; MCS, mental component score; PSS-10, 10-item Perceived Stress Scale; GHQ-12, 12-item General Health Questionnaire.

Table A.1 (continued): Outcomes measured by topic at midline (18 months post-intervention) and endline (3.5 years post-intervention) surveys.

	Midline (18 months)	Endline (3.5 years)
Mental and emotional health		
GHQ-12, Likert scoring		x
GHQ-12, binary scoring		x
PSS-10 score		x
Quality of life		
Number of functional limitations		x
SF-8, PCS		x
SF-8, MCS		x
Had stomach pain past 4 wks.		x
Had back pain past 4 wks.		x
Had chest pain past 4 wks.	x	x
Had joint pain past 4 wks.		x
Had headaches past 4 wks.		x
Had menstrual cramps past 4 wks.		x
Had pain in no specific place past 4 wks.		x
Fainted past 4 wks.		x
Felt dizzy past 4 wks.		x
Shortness of breath past 4 wks.		x
Heart pounded past 4 wks.		x
Childbirth		
Childbirth/stillbirth past 6 mos.	x	x
Childbirth/stillbirth past 3 yrs.	x	x
Number of births in hhld. past 3 yrs.		x
Number of live births in hhld. past 3 yrs.		x
Number of still births in hhld. past 3 yrs.		x
Delivery in hospital past 3 yrs.	x	x
Delivery in facility past 3 yrs.	x	x
Doctor assisted with birth past 3 yrs.	x	x
Doctor or nurse assisted with birth past 3 yrs.	x	x
Delivery complications past 3 yrs.	x	x
Ideal number of children	x	
Biomarkers		
Overweight or obese	x	
Underweight	x	
Abdominally obese	x	
Mid-upper arm circumference	x	
Hypertension, grade 1	x	
Hypertension, grade 2	x	
Skinfolds total	x	
Mortality		
Death in hhld. past 3 yrs.		x
Number of deaths in hhld.		x
Sickness-related death in hhld. past 3 yrs.		x
Number of sickness-related deaths in hhld.		x

Note. Table presents outcomes in first column, and an indicator (x = yes, blank = no) for whether the outcome was measured at midline (column 2) and endline (column 3). Abbreviations: RSBY, Rashtriya Swasthya Bima Yojana; hhld, household.; resp., respondent; yr., year; mo., month; wk., week; kg., kilogram; SF-8, Short-form 8; PCS, physical component score; MCS, mental component score; PSS-10, 10-item Perceived Stress Scale; GHQ-12, 12-item General Health Questionnaire.

Table A.2: 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	72.96% (7.13%) 0.000	6.88% (8.27%) 0.406	79.15% (1.08%) 0.000
(B) Sale-plus-transfer	65.01% (11.09%) 0.000	5.04% (13.24%) 0.704	69.55% (1.87%) 0.000
(C) Sale-of-insurance	46.80% (13.48%) 0.001	14.03% (16.54%) 0.397	59.43% (2.82%) 0.000
(A) Free-insurance x hhld. expenditure	-0.02% (0.04%) 0.655	0.02% (0.05%) 0.601	0.00% (0.01%) 0.588
(B) Sale-plus-transfer x hhld. expenditure	0.02% (0.07%) 0.756	0.01% (0.08%) 0.879	0.03% (0.01%) 0.003
(C) Sale-of-insurance x hhld. expenditure	0.02% (0.10%) 0.862	0.00% (0.12%) 0.984	0.02% (0.02%) 0.432
N	9080	9080	9080

Notes. Table presents direct and indirect effect of assignment of sample household to different premium and subsidy combinations, interacted with total household expenditures, 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 plus cash transfer (B), and 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 A.3: Enrollment by health (education, raven scores and risk aversion controlled)

	(1) Enroll-ment	(2) Enroll-ment	(3) Enroll-ment	(4) Enroll-ment	(5) Enroll-ment	(6) Enroll-ment	(7) Enroll-ment	(8) Enroll-ment	(9) Enroll-ment	(10) Enroll-ment	(11) Enroll-ment
Pay + Cash (B)	-0.15***	-0.14***	-0.15***	-0.13**	-0.14***	-0.16***	-0.13**	-0.13**	-0.14**	-0.14***	-0.15***
	(0.052)	(0.052)	(0.052)	(0.053)	(0.053)	(0.052)	(0.051)	(0.053)	(0.054)	(0.052)	(0.056)
Pay (C)	-0.19***	-0.19***	-0.19***	-0.19***	-0.17***	-0.20***	-0.19***	-0.20***	-0.17***	-0.19***	-0.20***
	(0.058)	(0.058)	(0.058)	(0.059)	(0.057)	(0.059)	(0.059)	(0.059)	(0.058)	(0.057)	(0.062)
Health		0.0079	-0.0069	0.014**	-0.0022	0.0039	-0.020***	-0.018**	0.0035	-0.0058	-0.00045
		(0.0081)	(0.0046)	(0.0071)	(0.0073)	(0.0068)	(0.0070)	(0.0073)	(0.0061)	(0.0080)	(0.0069)
B × health		-0.0070	-0.012	-0.012	-0.014	-0.0023	0.032**	0.028**	-0.027**	0.026**	0.025**
		(0.012)	(0.0088)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)	(0.013)	(0.011)	(0.010)
C × health		-0.012	-0.017	-0.012	-0.013	0.0097	0.0044	0.0079	-0.0038	0.047***	0.0059
		(0.014)	(0.011)	(0.015)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)	(0.017)	(0.014)
Asset	0.062***	0.065***	0.063***	0.063***	0.078***	0.065***	0.055***	0.066***	0.078***	0.064***	0.065***
	(0.014)	(0.015)	(0.014)	(0.014)	(0.015)	(0.016)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)
B × asset	-0.033	-0.035	-0.034	-0.036	-0.041*	-0.034	-0.023	-0.040*	-0.044*	-0.042*	-0.049**
	(0.022)	(0.023)	(0.022)	(0.022)	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)	(0.023)	(0.024)
C × asset	-0.034	-0.039	-0.035	-0.040	-0.049*	-0.038	-0.031	-0.042	-0.050*	-0.051*	-0.030
	(0.026)	(0.027)	(0.026)	(0.026)	(0.027)	(0.028)	(0.026)	(0.026)	(0.027)	(0.027)	(0.028)
Share in A, B, or C	0.076	0.077	0.076	0.091*	0.072	0.082*	0.075	0.097**	0.074	0.074	0.086*
	(0.047)	(0.047)	(0.047)	(0.048)	(0.047)	(0.047)	(0.047)	(0.048)	(0.048)	(0.047)	(0.051)
Dep. var. mean		0.718	0.718	0.733	0.728	0.719	0.719	0.733	0.728	0.718	0.725
Dep. var. SD		0.450	0.450	0.442	0.445	0.450	0.450	0.442	0.445	0.450	0.446
N	8727	8727	8727	7619	7903	8488	8675	7611	7917	8727	7630
Health variable		General health, male, midline predicted	General health, female, midline predicted	General health, male, baseline	General health, female, baseline	- Pr(visiting medical facility, male)	- Pr(visiting medical facility, female)	- Visited medical facility, male	- Visited medical facility, female	Medical expendi-ture, predicted	Medical expendi-ture, baseline
C × Health × SD/Mean enrollment		-0.021	-0.031	-0.020	-0.022	0.016	0.007	0.013	-0.006	0.079	0.010
Sharpened q-value		1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	0.06	1.00

Notes. *** p<0.01, ** p<0.05, * p<0.1. SE clustered at village level. Health 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. 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 certain equivalence smaller than 180 in a gamble game. Missing indicators of assets, raven scores, risk aversion and education are included.

Table A.4: Effect of insurance access on total and out-of-pocket (OOP) medical expenditures (CATE).

			Direct	Spillover	Total
		Control mean (SD)	Coefficient (SE) P-value	Coefficient (SE) P-value	Coefficient (SE) P-value
Medical spending at 18 months					
Annual medical spending	N = 10030	28717.69	12848.66	-1.6e+04	515.70
		(31601.59)	(5148.08)	(9680.63)	(5047.78)
			.01	.11	.92
Log annual medical spending	N = 10030	9.57	0.59	-1.34	-0.47
		(1.94)	(0.31)	(0.48)	(0.37)
			.06	.006	.21
Annual medical non-hospital spending	N = 10030	24072.39	5794.11	-7181.20	142.23
		(26111.22)	(4200.31)	(8399.11)	(4581.71)
			.17	.39	.98
Log annual medical non-hospital spending	N = 10030	9.35	0.35	-1.09	-0.51
		(2.07)	(0.32)	(0.50)	(0.38)
			.28	.03	.18
Medical spending at 4 years					
OOP medical spending (most serious event)	N = 9396	20444.06	6670.52	-1.7e+04	-6380.67
		(1.3e+05)	(22178.69)	(20511.80)	(12548.28)
			.76	.42	.61
Log OOP medical spending (most serious event)	N = 9396	4.74	-0.93	1.50	0.25
		(4.76)	(0.62)	(1.01)	(0.70)
			.13	.14	.72

Notes. Table presents estimates of the effect of enrollment in RSBY insurance on medical expenditures. Abbreviations: SD, standard deviation; SE, standard error; 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. Mean and standard deviation in the control group are statistics from the control group D. Coefficients were transformed to show changes relative to the control group D. 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.

Table A.5: Effect of insurance enrollment on use of insurance for hospital care and on hospital utilization (CATE), by wealth of household.

			Direct			Spillover			Total		
		Control mean (SD)	High wealth Coefficient (SE) P-value	Low wealth Coefficient (SE) P-value	Difference Coefficient (SE) P-value	High wealth Coefficient (SE) P-value	Low wealth Coefficient (SE) P-value	Difference Coefficient (SE) P-value	High wealth Coefficient (SE) P-value	Low wealth Coefficient (SE) P-value	Difference Coefficient (SE) P-value
Insurance use at 18 months											
Successful use (past 6 mos.)	N = 9391	3.88% (19.32%)	8.59% (4.00%) .03	-1.34% (3.54%) .70	-9.94% (4.83%) .04	-2.10% (5.55%) .71	10.64% (5.66%) .06	12.74% (6.27%) .04	6.94% (3.14%) .03	7.03% (3.07%) .02	0.09% (3.31%) .98
Failed use (past 6 mos.)	N = 9391	1.50% (12.15%)	6.11% (2.73%) .03	0.01% (2.38%) .00	-6.10% (3.41%) .07	-2.81% (3.63%) .44	2.96% (3.71%) .43	5.77% (4.78%) .23	3.90% (1.75%) .03	2.34% (1.56%) .14	-1.56% (2.19%) .48
Insurance use at 4 years											
Successful use (past 6 mos.)	N = 8871	0.49% (6.99%)	1.85% (1.70%) .28	5.08% (1.86%) .006	3.23% (2.57%) .21	-1.19% (2.31%) .61	-4.15% (2.32%) .07	-2.95% (3.27%) .37	0.91% (0.70%) .19	1.81% (0.88%) .04	0.90% (1.05%) .39
Successful use (most serious event)	N = 8863	0.13% (3.59%)	-0.88% (0.65%) .18	1.62% (1.56%) .30	2.50% (1.68%) .14	2.92% (1.14%) .01	-1.33% (1.75%) .45	-4.25% (2.05%) .04	1.42% (0.31%) p<.001	0.58% (0.74%) .44	-0.84% (0.80%) .29
Failed used (most serious event)	N = 8864	0.57% (7.52%)	-0.59% (1.46%) .69	3.68% (2.72%) .18	4.27% (2.52%) .09	2.38% (2.12%) .26	-3.30% (3.64%) .36	-5.68% (3.31%) .09	1.29% (0.76%) .09	1.08% (0.82%) .19	-0.21% (1.07%) .85
Healthcare use at 18 months											
Hospitalized on last visit (past 1 yr.)	N = 8559	20.82% (40.61%)	-3.94% (8.43%) .64	3.31% (10.06%) .74	7.24% (13.22%) .58	6.37% (13.58%) .64	6.88% (15.68%) .66	0.51% (14.14%) .97	1.08% (9.89%) .91	8.72% (10.41%) .40	7.64% (7.83%) .33
Healthcare use at 4 years											
Overnight stay for treatment	N = 8892	22.97% (42.07%)	-13.21% (7.49%) .08	-10.09% (10.51%) .34	3.12% (13.22%) .81	14.16% (8.30%) .09	10.13% (11.57%) .38	-4.03% (12.99%) .76	-2.07% (5.36%) .70	-2.12% (5.92%) .72	-0.05% (7.21%) .99
Outpatient (day) surgery	N = 8887	6.76% (25.11%)	-7.22% (4.48%) .11	0.60% (5.58%) .91	7.81% (7.24%) .28	13.16% (5.08%) .010	6.68% (6.81%) .33	-6.48% (7.78%) .40	3.14% (3.26%) .33	5.85% (3.53%) .10	2.71% (4.16%) .52

Notes. Notes. Table presents estimates of the effect of enrollment in RSBY insurance on use of insurance and visits to medical facilities, stratified by level of wealth. 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. A high (low) wealth household is a household with an above (below) median value of 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.

Table A.6: Reasons given for respondents' inability to use RSBY insurance card, by survey and utilization status.

	Midline (18 months)		Endline (3.5 years)	
	Did not try to use RSBY (N=2551)	Tried but failed to use RSBY (N=160)	Did not try to use RSBY (N=2763)	Tried but failed to use RSBY (N=84)
Forgot papers at home	13%	10%	9%	2%
Has not received papers	5%	3%	21%	1%
Didn't know card could be used for this purpose	15%	13%	20%	2%
Patient wasn't on the card	2%	1%	1%	1%
Card used up	1%	1%	0%	0%
Didn't want to use card for treatment	21%	9%	1%	6%
Doctor didn't accept card	15%	55%	9%	69%
Card machine didn't work	2%	1%	1%	8%
Treatment not covered	5%	8%	2%	7%
Hospital that accepted card was too far	3%	6%	0%	1%
Didn't know card would reduce payments	3%	3%	2%	0%
Didn't know card would make treatment free	12%	6%	3%	0%
Other reasons	2%	0%	2%	10%
Didn't know where to use card			30%	5%
Didn't know how to use card			29%	6%
Insurance company didn't accept card			1%	12%
Didn't need to use card			2%	1%
Didn't trust doctor would accept card			3%	1%
Didn't trust the card would work			3%	0%

Note. Table only includes respondents who enrolled in RSBY insurance. At midline, all respondents who reported having an RSBY card were asked about utilization attempts. At endline, all respondents whose households had a serious health event were asked about utilization attempts. Percentages do not add up to 100 as respondents were allowed to report multiple reasons.

Table A.7: Effect of different premium & subsidy combinations on midline (18 mos.) health outcomes (intent to treat estimate).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
Health value											
Good health	18983	0.662	0.055	-0.091	-0.027	0.061	-0.046	0.020	0.146	-0.192	-0.027
		(0.473)	(0.040)	(0.070)	(0.061)	(0.069)	(0.091)	(0.061)	(0.074)	(0.099)	(0.060)
			.18	.19	.65	.38	.62	.74	.05	.05	.65
Very good health	18983	0.155	0.064	-0.086	-0.014	0.044	-0.038	0.009	0.043	-0.033	0.013
		(0.362)	(0.036)	(0.064)	(0.063)	(0.069)	(0.094)	(0.065)	(0.068)	(0.093)	(0.066)
			.07	.18	.83	.52	.68	.89	.53	.72	.84
Poor or very poor health	18983	0.056	-0.012	-0.022	-0.032	-0.029	-0.006	-0.034	-0.012	-0.020	-0.030
		(0.231)	(0.024)	(0.030)	(0.034)	(0.033)	(0.038)	(0.034)	(0.038)	(0.045)	(0.035)
			.62	.46	.35	.39	.87	.32	.75	.66	.39
Very poor health	18983	0.008	-0.000	-0.004	-0.003	0.008	-0.014	-0.005	0.002	-0.005	-0.003
		(0.090)	(0.010)	(0.010)	(0.007)	(0.011)	(0.010)	(0.006)	(0.012)	(0.013)	(0.007)
			.00	.72	.60	.47	.16	.40	.86	.69	.69
Chronic disease											
Told had hypertension	18939	0.190	0.028	-0.098	-0.060	-0.140	0.114	-0.038	-0.026	-0.032	-0.055
		(0.392)	(0.046)	(0.049)	(0.036)	(0.057)	(0.069)	(0.038)	(0.053)	(0.062)	(0.037)
			.55	.05	.10	.02	.10	.32	.63	.61	.14
Takes medication for hypertension	18960	0.131	-0.018	-0.044	-0.057	-0.125	0.089	-0.045	-0.082	0.037	-0.048
		(0.338)	(0.037)	(0.045)	(0.033)	(0.047)	(0.060)	(0.034)	(0.043)	(0.051)	(0.033)
			.63	.34	.08	.009	.14	.19	.06	.47	.15
Told had diabetes	18913	0.068	-0.005	-0.003	-0.008	-0.040	0.047	0.002	-0.032	0.033	-0.002
		(0.252)	(0.023)	(0.028)	(0.023)	(0.037)	(0.043)	(0.024)	(0.036)	(0.036)	(0.023)
			.85	.90	.75	.28	.28	.95	.37	.36	.92
Takes oral medication for diabetes	18948	0.050	-0.002	-0.000	-0.002	-0.023	0.030	0.004	-0.027	0.025	-0.004
		(0.217)	(0.020)	(0.023)	(0.020)	(0.031)	(0.037)	(0.021)	(0.031)	(0.032)	(0.020)
			.93	.99	.92	.47	.42	.84	.39	.43	.84
Takes insulin for diabetes	18952	0.018	-0.001	0.019	0.016	-0.016	0.042	0.021	-0.006	0.027	0.019
		(0.133)	(0.011)	(0.011)	(0.011)	(0.016)	(0.018)	(0.012)	(0.017)	(0.018)	(0.011)
			.94	.09	.14	.30	.02	.08	.73	.13	.10
Is on a special diet for diabetes	18962	0.046	-0.000	-0.003	-0.003	-0.044	0.055	0.005	-0.038	0.047	0.005
		(0.210)	(0.018)	(0.024)	(0.021)	(0.028)	(0.032)	(0.021)	(0.030)	(0.030)	(0.021)
			.98	.91	.89	.12	.09	.82	.21	.12	.83
Told had cancer	18946	0.004	-0.006	0.010	0.002	-0.003	0.009	0.004	-0.005	0.005	0.000
		(0.064)	(0.005)	(0.006)	(0.004)	(0.009)	(0.010)	(0.004)	(0.008)	(0.009)	(0.004)
			.22	.11	.54	.69	.40	.23	.55	.55	.94

Notes. Table presents (in all panels but the last one) estimates of intent-to-treat effects of access to insurance at different prices and subsidies on specific midline (18 mos.) health outcomes. The outcomes are listed in the first column. The last panel provides critical p-values that control the false discovery rate for different groups of outcomes listed in the first column. Each observation in our regressions 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). The omitted category is the control group (D). Standard errors were clustered at the village level. 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 group D without access to insurance. Abbreviations: SD, standard deviation; SE, standard error; mos, months; yr, year.

Table A.7 (cont'd): Effect of different premium & subsidy combinations on midline (18 mos.) health outcomes (intent to treat estimate).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
Received cancer treatment past 1 yr.	18998	0.002	0.002	-0.002	0.000	-0.004	0.005	0.000	0.003	-0.002	0.001
		(0.049)	(0.005)	(0.005)	(0.003)	(0.004)	(0.004)	(0.003)	(0.007)	(0.009)	(0.003)
			.66	.67	.95	.38	.28	.87	.70	.81	.75
Told had lung disease	18907	0.034	-0.017	0.014	-0.005	0.012	-0.022	-0.008	0.002	-0.002	-0.000
		(0.181)	(0.020)	(0.017)	(0.017)	(0.028)	(0.028)	(0.017)	(0.028)	(0.030)	(0.017)
			.39	.42	.78	.66	.43	.66	.95	.95	.99
Told had heart disease	18868	0.020	0.009	0.008	0.016	0.023	-0.014	0.010	-0.001	0.018	0.016
		(0.141)	(0.017)	(0.017)	(0.011)	(0.019)	(0.020)	(0.010)	(0.017)	(0.018)	(0.011)
			.60	.63	.15	.24	.48	.33	.97	.31	.13
Told had stroke	18826	0.007	0.003	0.003	0.006	0.009	-0.005	0.004	-0.000	0.006	0.005
		(0.086)	(0.010)	(0.009)	(0.007)	(0.012)	(0.012)	(0.007)	(0.011)	(0.012)	(0.007)
			.76	.77	.42	.45	.67	.50	.99	.60	.42
Told had arthritis	18894	0.176	-0.093	0.060	-0.038	-0.067	0.017	-0.051	-0.127	0.092	-0.044
		(0.381)	(0.038)	(0.065)	(0.056)	(0.076)	(0.094)	(0.057)	(0.058)	(0.072)	(0.055)
			.01	.36	.49	.38	.85	.38	.03	.20	.42
Had surgery for arthritis past 1 yr.	18933	0.018	-0.015	0.004	-0.011	-0.035	0.024	-0.013	-0.053	0.048	-0.010
		(0.135)	(0.014)	(0.018)	(0.014)	(0.019)	(0.022)	(0.014)	(0.017)	(0.019)	(0.014)
			.29	.81	.41	.07	.28	.35	.002	.01	.47
Diagnose with high cholesterol	18871	0.019	-0.001	0.016	0.013	0.010	0.007	0.016	0.005	0.014	0.017
		(0.137)	(0.011)	(0.018)	(0.015)	(0.019)	(0.029)	(0.016)	(0.019)	(0.022)	(0.015)
			.91	.37	.39	.62	.81	.31	.81	.53	.26
Takes medication for high cholesterol	19032	0.014	0.004	0.008	0.010	-0.006	0.019	0.011	-0.009	0.027	0.015
		(0.116)	(0.012)	(0.013)	(0.011)	(0.017)	(0.020)	(0.012)	(0.017)	(0.022)	(0.012)
			.76	.56	.34	.73	.35	.34	.60	.22	.20
Health-related quality of life, ADLs, and physical health symptoms											
Experiences chest pain	18888	0.081	-0.029	0.021	-0.011	0.003	-0.018	-0.014	-0.008	-0.002	-0.010
		(0.273)	(0.024)	(0.041)	(0.035)	(0.044)	(0.053)	(0.036)	(0.040)	(0.049)	(0.034)
			.23	.61	.76	.95	.73	.71	.84	.97	.78
Fertility											
Currently pregnant	9262	0.055	0.020	-0.050	-0.025	-0.002	-0.031	-0.030	-0.071	0.078	-0.000
		((0.227))	(0.026)	(0.068)	(0.061)	(0.039)	(0.068)	(0.062)	(0.057)	(0.105)	(0.068)
			.44	.46	.68	.97	.65	.63	.21	.46	.99
Childbirth past 1 yr.	8299	0.049	0.028	-0.056	-0.022	-0.009	-0.006	-0.015	-0.023	-0.009	-0.030
		((0.217))	(0.026)	(0.060)	(0.051)	(0.034)	(0.051)	(0.052)	(0.038)	(0.039)	(0.051)
			.28	.35	.66	.79	.90	.78	.55	.83	.55
Delivered in govt. hosp. past 1 yr.	8297	0.015	0.009	-0.013	-0.002	-0.002	0.014	0.011	-0.016	0.018	0.000
		((0.120))	(0.017)	(0.015)	(0.012)	(0.022)	(0.023)	(0.013)	(0.017)	(0.019)	(0.013)
			.57	.41	.86	.94	.53	.38	.34	.34	.00
Delivered in facility past 1 yr.	8297	0.047	0.023	-0.051	-0.023	-0.000	-0.016	-0.015	-0.032	0.002	-0.031
		((0.211))	(0.026)	(0.059)	(0.051)	(0.034)	(0.049)	(0.052)	(0.036)	(0.037)	(0.051)
			.36	.39	.66	.99	.75	.77	.37	.96	.55
Doctor assisted with delivery past 1 yr.	8299	0.038	0.023	-0.051	-0.023	0.007	-0.029	-0.019	-0.038	0.014	-0.025
		((0.191))	(0.022)	(0.059)	(0.050)	(0.031)	(0.046)	(0.050)	(0.037)	(0.036)	(0.050)
			.31	.38	.64	.82	.53	.70	.31	.71	.61

Table A.7 (cont'd): Effect of different premium & subsidy combinations on midline (18 mos.) health outcomes (intent to treat estimate).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
Doctor or nurse assisted with delivery past 1 yr.	8299	0.043	0.012	-0.052	-0.035	-0.011	-0.023	-0.031	-0.036	-0.004	-0.040
		((0.203))	(0.026)	(0.059)	(0.050)	(0.032)	(0.047)	(0.051)	(0.037)	(0.039)	(0.051)
			.64	.38	.49	.73	.63	.54	.33	.91	.43
Child delivery complications past 1 yr.	8293	0.015	-0.010	-0.021	-0.029	-0.007	-0.020	-0.024	-0.020	-0.007	-0.026
		((0.122))	(0.013)	(0.026)	(0.021)	(0.018)	(0.031)	(0.022)	(0.017)	(0.018)	(0.022)
			.44	.41	.17	.72	.52	.28	.25	.70	.23
Ideal number of children	7881	2.516	-0.133	0.063	-0.076	-0.136	0.167	0.015	0.013	-0.086	-0.064
		((1.215))	(0.159)	(0.318)	(0.295)	(0.278)	(0.385)	(0.294)	(0.278)	(0.424)	(0.299)
			.41	.84	.80	.63	.66	.96	.96	.84	.83
					Biomarkers						
Overweight or obese	4853	0.271	-0.034	0.003	-0.031	-0.065	0.073	0.001	-0.113	0.137	0.010
		(0.445)	(0.088)	(0.100)	(0.073)	(0.128)	(0.141)	(0.075)	(0.118)	(0.130)	(0.071)
			.70	.97	.67	.61	.61	.99	.34	.30	.89
Underweight	4853	0.084	0.059	-0.123	-0.051	-0.097	0.057	-0.046	-0.045	-0.005	-0.050
		(0.277)	(0.055)	(0.075)	(0.041)	(0.067)	(0.080)	(0.041)	(0.059)	(0.073)	(0.042)
			.28	.10	.22	.15	.48	.26	.44	.94	.23
Abdominally obese	4723	0.713	-0.108	0.204	0.076	-0.046	0.051	-0.000	-0.052	0.091	0.030
		(0.452)	(0.104)	(0.101)	(0.118)	(0.155)	(0.186)	(0.124)	(0.154)	(0.175)	(0.120)
			.30	.05	.52	.77	.79	.00	.74	.60	.80
Mid-Upper Arm Circumference	4815	27.032	-1.680	0.215	-1.486	-2.213	0.504	-1.760	-1.772	0.086	-1.695
		(3.562)	(0.656)	(1.234)	(0.990)	(1.008)	(1.314)	(0.935)	(1.144)	(1.385)	(0.923)
			.01	.86	.14	.03	.70	.06	.12	.95	.07
Hypertension, grade 1	4878	0.449	-0.073	-0.010	-0.082	-0.092	0.000	-0.092	-0.176	0.102	-0.084
		(0.498)	(0.078)	(0.095)	(0.081)	(0.100)	(0.119)	(0.082)	(0.111)	(0.142)	(0.087)
			.35	.91	.31	.36	.00	.27	.12	.47	.34
Hypertension, grade 2	4878	0.191	-0.083	0.086	-0.005	0.030	-0.031	0.002	-0.078	0.070	-0.015
		(0.393)	(0.068)	(0.072)	(0.077)	(0.090)	(0.112)	(0.078)	(0.085)	(0.106)	(0.077)
			.22	.23	.95	.74	.78	.98	.36	.51	.85
Skinfolds total	4065	26.326	-2.131	2.868	0.450	-1.608	1.477	-0.279	-0.994	1.845	0.666
		(9.464)	(2.100)	(3.953)	(3.374)	(3.565)	(4.701)	(3.297)	(3.824)	(5.551)	(3.425)
			.31	.47	.89	.65	.75	.93	.80	.74	.85
					FWER crit. values						
Health value			0.013	0.013	0.013	0.013	0.013	0.013	0.013	0.013	0.013
Chronic disease			0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003
Health-related quality of life, ADLs, and physical health symptoms			0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05
Fertility			0.006	0.006	0.006	0.006	0.006	0.006	0.006	0.006	0.006

Notes. Table presents (in all panels but the last one) estimates of intent-to-treat effects of access to insurance at different prices and subsidies on specific midline (18 mos.) health outcomes. The outcomes are listed in the first column. The last panel provides critical p-values that control the false discovery rate for different groups of outcomes listed in the first column. Each observation in our regressions 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). The omitted category is the control group (D). Standard errors were clustered at the village level. 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 group D without access to insurance. Abbreviations: SD, standard deviation; SE, standard error; mos, months; yr, year.

Table A.8: Effect of different premium & subsidy combinations on endline (3.5 yrs.) health outcomes (intent to treat estimate).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
Health value											
		(0.340)	(0.043)	(0.047)	(0.042)	(0.058)	(0.068)	(0.043)	(0.060)	(0.079)	(0.044)
			.67	.77	.89	.65	.88	.69	.62	.65	.95
Good health	9492	0.516	0.014	-0.011	0.005	-0.172	0.225	0.031	0.015	-0.026	-0.008
		(0.500)	(0.077)	(0.089)	(0.073)	(0.098)	(0.110)	(0.074)	(0.097)	(0.108)	(0.074)
			.86	.90	.95	.08	.04	.67	.87	.81	.91
Very good health	9492	0.076	0.027	-0.120	-0.081	-0.061	-0.016	-0.075	-0.047	-0.043	-0.086
		(0.265)	(0.044)	(0.058)	(0.042)	(0.052)	(0.054)	(0.043)	(0.046)	(0.053)	(0.043)
			.54	.04	.05	.24	.77	.08	.30	.41	.04
Poor health	9492	0.102	-0.033	0.036	-0.001	-0.036	0.033	-0.006	-0.037	0.038	-0.003
		(0.303)	(0.048)	(0.048)	(0.043)	(0.061)	(0.063)	(0.044)	(0.062)	(0.063)	(0.043)
			.49	.45	.99	.55	.60	.89	.54	.54	.94
Very poor health	9492	0.021	-0.042	0.006	-0.037	-0.052	0.021	-0.033	-0.033	-0.007	-0.040
		(0.143)	(0.022)	(0.018)	(0.023)	(0.025)	(0.027)	(0.024)	(0.027)	(0.023)	(0.023)
			.05	.75	.11	.04	.44	.16	.22	.75	.08
Chronic disease											
Told had hypertension	9476	0.233	-0.039	0.013	-0.027	-0.110	0.099	-0.021	-0.169	0.178	-0.008
		(0.423)	(0.057)	(0.060)	(0.053)	(0.076)	(0.080)	(0.055)	(0.083)	(0.085)	(0.056)
			.50	.83	.61	.15	.22	.70	.04	.04	.88
Takes medication for hypertension	9495	0.197	-0.041	0.037	-0.008	-0.062	0.064	-0.004	-0.117	0.141	0.010
		(0.398)	(0.053)	(0.053)	(0.049)	(0.070)	(0.076)	(0.050)	(0.073)	(0.076)	(0.051)
			.44	.49	.87	.38	.40	.93	.11	.06	.84
Told had diabetes	9479	0.121	-0.049	-0.019	-0.066	-0.131	0.082	-0.058	-0.149	0.104	-0.055
		(0.326)	(0.048)	(0.048)	(0.040)	(0.059)	(0.061)	(0.041)	(0.057)	(0.056)	(0.041)
			.30	.70	.10	.03	.18	.16	.009	.06	.18
Takes oral medication for diabetes	9499	0.107	-0.046	-0.003	-0.049	-0.109	0.073	-0.044	-0.126	0.098	-0.037
		(0.310)	(0.048)	(0.046)	(0.036)	(0.055)	(0.059)	(0.037)	(0.052)	(0.054)	(0.036)
			.33	.95	.18	.05	.22	.24	.02	.07	.30
Takes insulin for diabetes	9489	0.024	0.018	-0.007	0.011	0.018	-0.007	0.011	-0.005	0.022	0.014
		(0.152)	(0.019)	(0.019)	(0.015)	(0.026)	(0.029)	(0.015)	(0.025)	(0.026)	(0.015)
			.35	.70	.46	.49	.80	.46	.84	.41	.35
Follows a special diet for diabetes	9495	0.077	-0.035	0.012	-0.024	-0.075	0.064	-0.017	-0.073	0.064	-0.015
		(0.267)	(0.041)	(0.039)	(0.032)	(0.048)	(0.051)	(0.033)	(0.048)	(0.051)	(0.033)
			.40	.76	.47	.12	.21	.61	.13	.21	.65

Notes. Table presents (in all panels but the last one) estimates of intent-to-treat effects of access to insurance at different prices and subsidies on specific endline (3.5 yrs.) health outcomes. The outcomes are listed in the first column. The last panel provides critical p-values that control the false discovery rate for different groups of outcomes listed in the first column. Each observation in our regressions 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). The omitted category is the control group (D). Standard errors were clustered at the village level. 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 group D without access to insurance. Abbreviations: SD, standard deviation; SE, standard error; mos, months; yr, year.

Table A.8 (cont'd): Effect of different premium & subsidy combinations on endline (3.5 yrs.) health outcomes (intent to treat estimate).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
Told had cancer	9470	0.004	-0.005	0.012	0.006	0.006	0.001	0.007	0.001	0.001	0.002
		(0.065)	(0.008)	(0.006)	(0.006)	(0.014)	(0.018)	(0.007)	(0.008)	(0.007)	(0.006)
			.51	.06	.35	.68	.96	.32	.93	.84	.73
Treated for cancer past 1 yr.	9496	0.002	0.000	0.000	0.000	-0.007	0.009	0.001	-0.002	0.002	0.000
		(0.046)	(0.006)	(0.006)	(0.004)	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)	(0.004)
			.99	.94	.91	.14	.08	.87	.75	.69	.97
Told had lung disease	9479	0.060	-0.035	0.021	-0.016	-0.004	-0.017	-0.019	-0.038	0.017	-0.022
		(0.238)	(0.030)	(0.029)	(0.024)	(0.037)	(0.045)	(0.025)	(0.039)	(0.037)	(0.024)
			.25	.46	.50	.91	.71	.45	.34	.64	.35
Told had heart disease	9473	0.047	0.018	-0.048	-0.026	-0.069	0.049	-0.025	-0.072	0.050	-0.027
		(0.211)	(0.035)	(0.033)	(0.025)	(0.033)	(0.037)	(0.025)	(0.036)	(0.035)	(0.025)
			.62	.15	.30	.04	.19	.32	.05	.16	.28
Told had stroke	9446	0.006	-0.011	0.005	-0.006	-0.022	0.016	-0.008	0.002	-0.017	-0.013
		(0.080)	(0.013)	(0.011)	(0.009)	(0.012)	(0.011)	(0.009)	(0.015)	(0.014)	(0.009)
			.41	.63	.48	.06	.15	.36	.88	.21	.13
Told had arthritis or rheumatism	9482	0.193	-0.064	0.118	0.043	0.038	-0.013	0.026	0.031	0.002	0.033
		(0.395)	(0.062)	(0.063)	(0.055)	(0.082)	(0.096)	(0.054)	(0.076)	(0.090)	(0.056)
			.31	.06	.44	.64	.89	.63	.68	.99	.56
Had surgery due to arthritis past 1 yr.	9497	0.042	0.024	0.010	0.033	0.005	0.021	0.024	0.027	0.007	0.033
		(0.200)	(0.028)	(0.029)	(0.022)	(0.032)	(0.035)	(0.023)	(0.034)	(0.041)	(0.024)
			.38	.74	.14	.87	.55	.29	.44	.86	.16
Diagnosed with high cholesterol	9449	0.026	0.012	-0.003	0.010	0.003	0.007	0.010	0.015	-0.005	0.010
		(0.160)	(0.021)	(0.020)	(0.016)	(0.024)	(0.022)	(0.016)	(0.024)	(0.022)	(0.016)
			.56	.89	.55	.89	.75	.55	.54	.82	.53
Takes medication to lower cholesterol	9496	0.013	-0.002	-0.004	-0.005	-0.017	0.015	-0.003	-0.006	0.000	-0.006
		(0.115)	(0.014)	(0.009)	(0.011)	(0.014)	(0.011)	(0.012)	(0.014)	(0.010)	(0.012)
			.88	.70	.65	.23	.15	.79	.66	.00	.61
Has eyesight problems	9480	0.329	-0.025	0.145	0.106	-0.031	0.168	0.120	0.017	0.099	0.106
		(0.470)	(0.067)	(0.069)	(0.064)	(0.088)	(0.093)	(0.064)	(0.095)	(0.096)	(0.065)
			.71	.04	.10	.72	.07	.06	.86	.30	.11
Had surgery due to eye problems past 1 yr.	9497	0.074	-0.042	0.091	0.040	0.020	0.014	0.032	-0.020	0.061	0.035
		(0.261)	(0.039)	(0.036)	(0.035)	(0.054)	(0.056)	(0.036)	(0.055)	(0.074)	(0.040)
			.28	.01	.26	.72	.80	.36	.71	.41	.38
Health-related quality of life, ADLs, and physical health symptoms											
Number of functional limitations	9413	0.674	-0.170	0.034	-0.139	-0.389	0.257	-0.158	-0.378	0.295	-0.112
		(1.523)	(0.192)	(0.247)	(0.224)	(0.271)	(0.323)	(0.230)	(0.281)	(0.333)	(0.230)
			.38	.89	.53	.15	.43	.49	.18	.38	.63
SF-8, PCS	9510	2.260	-0.120	0.273	0.125	-0.066	0.212	0.125	-0.206	0.425	0.176
		(0.930)	(0.172)	(0.151)	(0.140)	(0.180)	(0.200)	(0.141)	(0.176)	(0.197)	(0.143)
			.48	.07	.37	.71	.29	.38	.24	.03	.22
SF-8, MCS	9510	1.491	-0.012	0.042	0.025	0.085	-0.121	-0.024	-0.287	0.377	0.052
		(0.789)	(0.107)	(0.140)	(0.129)	(0.153)	(0.176)	(0.131)	(0.168)	(0.190)	(0.134)
			.91	.77	.84	.58	.49	.85	.09	.05	.70
Had stomach pain past 4 wks.	8999	0.061	0.065	-0.034	0.035	0.005	0.037	0.039	-0.014	0.070	0.049
		(0.240)	(0.039)	(0.045)	(0.025)	(0.041)	(0.048)	(0.026)	(0.047)	(0.054)	(0.027)
			.10	.45	.17	.90	.43	.14	.76	.20	.07

Table A.8 (cont'd): Effect of different premium & subsidy combinations on endline (3.5 yrs.) health outcomes (intent to treat estimate).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
Had back pain past 4 wks.	8999	0.254	0.079	0.044	0.119	0.052	0.060	0.106	-0.068	0.219	0.129
		(0.435)	(0.060)	(0.083)	(0.069)	(0.085)	(0.099)	(0.070)	(0.079)	(0.096)	(0.071)
			.19	.59	.09	.54	.54	.13	.38	.02	.07
Had chest pain past 4 wks.	8999	0.074	0.056	-0.020	0.038	0.011	0.029	0.037	-0.016	0.069	0.045
		(0.262)	(0.042)	(0.039)	(0.031)	(0.047)	(0.046)	(0.031)	(0.046)	(0.044)	(0.031)
			.18	.61	.22	.81	.53	.23	.72	.12	.15
Had joint pain past 4 wks.	8999	0.578	-0.065	0.108	0.032	-0.004	0.049	0.040	-0.107	0.202	0.075
		(0.494)	(0.076)	(0.078)	(0.076)	(0.090)	(0.112)	(0.077)	(0.099)	(0.115)	(0.077)
			.39	.17	.68	.96	.66	.60	.28	.08	.33
Had headaches past 4 wks.	8999	0.194	0.007	0.132	0.126	0.099	0.059	0.152	0.040	0.101	0.131
		(0.396)	(0.053)	(0.057)	(0.047)	(0.079)	(0.090)	(0.048)	(0.071)	(0.083)	(0.050)
			.90	.02	.008	.21	.51	.002	.57	.23	.009
Had menstrual cramps past 4 wks.	8999	0.038	0.021	-0.001	0.020	0.025	-0.009	0.017	0.022	0.010	0.031
		(0.190)	(0.023)	(0.029)	(0.021)	(0.030)	(0.036)	(0.021)	(0.036)	(0.043)	(0.021)
			.36	.97	.34	.41	.81	.41	.55	.81	.15
Had pain in no specific place past 4 wks.	8999	0.044	0.016	-0.022	-0.004	-0.015	0.008	-0.008	-0.073	0.071	-0.008
		(0.205)	(0.034)	(0.035)	(0.030)	(0.039)	(0.043)	(0.030)	(0.037)	(0.038)	(0.030)
			.65	.52	.89	.70	.86	.79	.05	.06	.78
Fainted past 4 wks.	9451	0.012	0.010	-0.004	0.007	0.021	-0.015	0.007	0.011	-0.006	0.005
		(0.110)	(0.016)	(0.014)	(0.012)	(0.020)	(0.025)	(0.012)	(0.021)	(0.021)	(0.011)
			.51	.80	.53	.30	.54	.56	.60	.76	.65
Felt dizzy past 4 wks.	9451	0.133	0.058	-0.012	0.047	0.032	0.036	0.064	-0.058	0.159	0.085
		(0.339)	(0.047)	(0.049)	(0.040)	(0.057)	(0.064)	(0.040)	(0.057)	(0.067)	(0.042)
			.22	.81	.23	.57	.58	.11	.30	.02	.04
Shortness of breath past 4 wks.	9451	0.049	0.036	0.011	0.045	0.038	0.004	0.042	-0.029	0.094	0.055
		(0.216)	(0.029)	(0.028)	(0.025)	(0.040)	(0.041)	(0.025)	(0.037)	(0.039)	(0.026)
			.22	.70	.07	.34	.93	.10	.43	.02	.03
Heart pounded past 4 wks.	9451	0.064	0.018	-0.023	-0.003	-0.025	0.027	-0.001	-0.060	0.076	0.009
		(0.245)	(0.034)	(0.043)	(0.035)	(0.046)	(0.052)	(0.036)	(0.053)	(0.057)	(0.037)
			.60	.59	.93	.59	.60	.99	.26	.18	.81
Mental and emotional health and cognitive function											
GHQ-12, Likert scoring	9510	9.329	0.385	0.165	0.533	-0.428	0.711	0.212	-2.151	3.292	0.812
		(6.728)	(0.895)	(1.270)	(1.061)	(1.219)	(1.480)	(1.072)	(1.144)	(1.467)	(1.101)
			.67	.90	.62	.73	.63	.84	.06	.03	.46
GHQ-12, binary scoring	9510	2.331	0.679	-0.258	0.447	-0.195	0.512	0.265	-0.828	1.573	0.588
		(3.297)	(0.467)	(0.714)	(0.557)	(0.633)	(0.771)	(0.562)	(0.603)	(0.844)	(0.589)
			.15	.72	.42	.76	.51	.64	.17	.06	.32
PSS-10 score	9510	17.785	0.036	0.128	0.151	0.127	-0.225	-0.076	-1.031	1.245	0.089
		(3.494)	(0.458)	(0.580)	(0.511)	(0.722)	(0.913)	(0.523)	(0.695)	(0.809)	(0.527)
			.94	.83	.77	.86	.81	.88	.14	.12	.87
Fertility											
Childbirth/stillbirth past 6 mos.	9493	0.046	0.020	0.002	0.022	-0.016	0.050	0.029	0.033	-0.013	0.021
		(0.210)	(0.031)	(0.032)	(0.020)	(0.038)	(0.041)	(0.020)	(0.034)	(0.040)	(0.021)
			.53	.95	.28	.67	.22	.16	.34	.75	.31

Table A.8 (cont'd): Effect of different premium & subsidy combinations on endline (3.5 yrs.) health outcomes (intent to treat estimate).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
Childbirth/stillbirth past 3 yrs.	9510	0.192	0.032	-0.015	0.018	-0.095	0.120	0.013	0.069	-0.075	0.002
		(0.394)	(0.058)	(0.064)	(0.048)	(0.072)	(0.078)	(0.048)	(0.070)	(0.086)	(0.050)
			.58	.81	.70	.18	.12	.80	.33	.39	.97
Number of births in hhld. past 3 years	9510	0.315	0.124	-0.111	0.024	0.063	-0.094	-0.022	0.160	-0.161	0.016
		(0.920)	(0.133)	(0.237)	(0.139)	(0.159)	(0.235)	(0.136)	(0.162)	(0.273)	(0.152)
			.35	.64	.86	.69	.69	.87	.32	.56	.92
Number of live births in hhld. past 3 years	9510	0.205	0.049	-0.027	0.025	-0.153	0.197	0.024	0.060	-0.055	0.010
		(0.487)	(0.069)	(0.079)	(0.058)	(0.083)	(0.088)	(0.059)	(0.086)	(0.092)	(0.059)
			.48	.73	.67	.06	.03	.68	.48	.55	.86
Number of still births in hhld. past 3 years	9510	0.110	0.075	-0.084	-0.001	0.216	-0.291	-0.046	0.100	-0.106	0.005
		(0.777)	(0.102)	(0.210)	(0.127)	(0.140)	(0.223)	(0.127)	(0.133)	(0.237)	(0.140)
			.46	.69	.00	.12	.19	.72	.45	.66	.97
Delivery in hospital past 3 yrs	9510	0.141	-0.008	-0.034	-0.039	-0.078	0.042	-0.040	-0.027	-0.021	-0.046
		(0.349)	(0.052)	(0.049)	(0.045)	(0.065)	(0.067)	(0.045)	(0.062)	(0.066)	(0.046)
			.87	.49	.39	.23	.54	.37	.66	.75	.32
Delivery in facility past 3 yrs	9510	0.152	-0.008	-0.031	-0.036	-0.128	0.109	-0.029	-0.008	-0.047	-0.050
		(0.359)	(0.052)	(0.052)	(0.045)	(0.067)	(0.069)	(0.046)	(0.063)	(0.071)	(0.047)
			.88	.55	.43	.06	.11	.53	.90	.51	.28
Doctor assisted with birth past 3 yrs.	9510	0.156	-0.001	0.011	0.009	-0.101	0.125	0.011	0.012	-0.020	-0.007
		(0.363)	(0.052)	(0.053)	(0.042)	(0.067)	(0.069)	(0.043)	(0.055)	(0.067)	(0.044)
			.99	.84	.83	.13	.07	.79	.84	.76	.88
Doctor or nurse assisted with birth past 3 yrs.	9510	0.164	0.007	-0.003	0.005	-0.123	0.142	0.005	0.031	-0.048	-0.012
		(0.371)	(0.056)	(0.056)	(0.044)	(0.067)	(0.069)	(0.045)	(0.061)	(0.072)	(0.046)
			.90	.96	.91	.07	.04	.91	.61	.51	.79
Delivery complications past 3 yrs.	9510	0.039	-0.005	0.018	0.011	0.047	-0.047	0.005	-0.004	0.023	0.016
		(0.194)	(0.027)	(0.024)	(0.023)	(0.037)	(0.038)	(0.023)	(0.034)	(0.032)	(0.023)
			.86	.46	.64	.21	.21	.84	.90	.48	.50
						Mortality					
Death in hhld. past 3 yrs	9482	0.121	0.010	0.043	0.049	-0.018	0.067	0.043	0.090	-0.068	0.029
		(0.327)	(0.049)	(0.046)	(0.029)	(0.051)	(0.057)	(0.031)	(0.062)	(0.067)	(0.030)
			.84	.34	.09	.72	.24	.17	.15	.31	.35
Deaths in the HH past 3 yrs	9474	0.125	0.022	0.049	0.066	0.004	0.063	0.060	0.111	-0.067	0.050
		(0.345)	(0.051)	(0.048)	(0.030)	(0.052)	(0.059)	(0.032)	(0.068)	(0.075)	(0.032)
			.67	.31	.03	.94	.28	.06	.10	.37	.12
Sickness-related death in hhld. past 3 yrs	9482	0.079	-0.009	0.049	0.035	-0.013	0.055	0.037	0.102	-0.097	0.014
		(0.270)	(0.042)	(0.036)	(0.025)	(0.043)	(0.046)	(0.026)	(0.052)	(0.055)	(0.026)
			.84	.18	.16	.77	.23	.16	.05	.08	.59
Number of sickness-related deaths in hhld.	9482	0.081	-0.005	0.053	0.042	-0.008	0.055	0.042	0.104	-0.090	0.023
		(0.281)	(0.044)	(0.038)	(0.026)	(0.044)	(0.046)	(0.027)	(0.055)	(0.059)	(0.027)
			.90	.16	.10	.86	.24	.12	.06	.13	.39

Table A.8 (cont'd): Effect of different premium & subsidy combinations on endline (3.5 yrs) health outcomes (intent to treat estimate).

			Free insurance			Sale of insurance + transfer			Sale of insurance		
	N	Control mean (SD)	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value	Direct Coefficient (SE) P-value	Spillover Coefficient (SE) P-value	Total Coefficient (SE) P-value
FWER crit. values											
Health value			0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Chronic disease			0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003
Health-related quality of life, ADLs, and physical health symptoms			0.004	0.004	0.004	0.004	0.004	0.004	0.004	0.004	0.004
Mental and emotional health and cognitive function			0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017
Fertility			0.005	0.005	0.005	0.005	0.005	0.005	0.005	0.005	0.005
Mortality			0.013	0.013	0.013	0.013	0.013	0.013	0.013	0.013	0.013

Notes. Table presents (in all panels but the last one) estimates of intent-to-treat effects of access to insurance at different prices and subsidies on specific endline (3.5 yrs.) health outcomes. The outcomes are listed in the first column. The last panel provides critical p-values that control the false discovery rate for different groups of outcomes listed in the first column. Each observation in our regressions 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). The omitted category is the control group (D). Standard errors were clustered at the village level. 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 group D without access to insurance. Abbreviations: SD, standard deviation; SE, standard error; mos, months; yr, year.

Table A.9: Effect of insurance enrollment on midline (18 mos.) health outcomes (complier average treatment effect estimates).

			Direct	Spillover	Total
		Control mean (SD)	Coefficient (SE) P-value	Coefficient (SE) P-value	Coefficient (SE) P-value
Health value					
Good health	18983	0.657	0.084	-0.165	-0.046
		(0.475)	(0.049)	(0.092)	(0.062)
			.09	.07	.46
Very good health	18983	0.155	0.100	-0.169	-0.034
		(0.362)	(0.048)	(0.080)	(0.060)
			.04	.04	.58
Poor or very poor health	18983	0.053	-0.013	-0.028	-0.035
		(0.225)	(0.029)	(0.036)	(0.031)
			.65	.44	.25
Very poor health	18983	0.008	0.003	-0.014	-0.008
		(0.088)	(0.010)	(0.013)	(0.006)
			.76	.28	.18
Chronic disease					
Told had hypertension	18939	0.189	-0.028	-0.029	-0.051
		(0.392)	(0.047)	(0.062)	(0.036)
			.56	.64	.16
Takes medication for hypertension	18960	0.130	-0.059	0.026	-0.039
		(0.336)	(0.037)	(0.052)	(0.032)
			.11	.61	.22
Told had diabetes	18913	0.069	-0.013	0.020	0.003
		(0.253)	(0.026)	(0.032)	(0.021)
			.62	.53	.90
Takes oral medication for diabetes	18948	0.051	-0.010	0.018	0.004
		(0.220)	(0.022)	(0.027)	(0.018)
			.64	.51	.83
Takes insulin for diabetes	18952	0.019	-0.003	0.026	0.017
		(0.135)	(0.012)	(0.014)	(0.010)
			.80	.06	.08
Is on a special diet for diabetes	18962	0.048	-0.010	0.023	0.008
		(0.214)	(0.020)	(0.025)	(0.018)
			.62	.35	.64
Told had cancer	18946	0.005	-0.007	0.013	0.003
		(0.071)	(0.006)	(0.008)	(0.004)
			.27	.12	.38
Received cancer treatment past 1 yr.	18998	0.003	0.002	-0.004	-0.001
		(0.051)	(0.005)	(0.005)	(0.003)
			.64	.49	.82
Told had lung disease	18907	0.035	-0.003	-0.008	-0.009
		(0.185)	(0.023)	(0.025)	(0.015)
			.91	.74	.54
Told had heart disease	18868	0.022	0.015	-0.003	0.013
		(0.147)	(0.016)	(0.020)	(0.010)
			.35	.88	.20
Told had stroke	18826	0.008	0.005	-0.002	0.003
		(0.091)	(0.010)	(0.012)	(0.006)
			.61	.85	.56
Told had arthritis	18894	0.177	-0.102	0.122	-0.006
		(0.381)	(0.048)	(0.082)	(0.055)
			.03	.14	.91

Notes. Table presents (in all panels but the last one) estimates of complier average treatment effects of enrollment in insurance at different prices and subsidies on specific midline (18 mos.) health outcomes. The outcomes are listed in the first column. The last panel provides critical p-values that control the false discovery rate for different groups of outcomes listed in the first column. Each observation in our regressions is a household. Treatment variables are enrollment in insurance, the share of sample households in the village that enrolled in insurance, and the interaction of these variables. Enrollment is instrumented with household assignments: free insurance (A), sale of insurance + cash transfer (B), sale of insurance (C). Share enrolled is instrumented with the share of study households assigned to arms (A)-(C) in the same village. The interaction is instrumented with the interaction of household assignment and share of households assigned to (A)-(C). Standard errors were clustered at the village level. The direct effect only includes the coefficient on enrollment. The spillover effect is the estimated effect on an enrolled household of enrolling all other sample households in RSBY insurance. The total effect is constructed as the sum of the direct effect and 0.787*(spillover effect), since 78.7% is the maximum share of households that enroll in insurance in any arm. Mean and standard deviation in the control group are statistics from group D without access to insurance. Abbreviations: SD, standard deviation; SE, standard error; mos, months; yr, year.

Table A.9 (cont'd): Effect of insurance enrollment on midline (18 mos.) health outcomes (complier average treatment effect estimates).

			Direct	Spillover	Total
		Control mean (SD)	Coefficient (SE) P-value	Coefficient (SE) P-value	Coefficient (SE) P-value
Had surgery for arthritis past 1 yr.	18933	0.020 (0.140)	-0.024 (0.015) .12	0.026 (0.020) .19	-0.004 (0.012) .76
Diagnose with high cholesterol	18871	0.022 (0.145)	0.013 (0.014) .34	-0.003 (0.022) .89	0.011 (0.014) .45
Takes medication for high cholesterol	19032	0.016 (0.124)	0.008 (0.015) .58	0.003 (0.018) .88	0.010 (0.011) .34
Health-related quality of life, ADLs, and physical health symptoms					
Experiences chest pain	18888	0.078 (0.268)	-0.023 (0.029) .42	0.005 (0.048) .91	-0.019 (0.034) .57
Fertility					
Currently pregnant	9262	0.058 (0.233)	0.036 (0.037) .33	-0.085 (0.065) .20	-0.030 (0.057) .60
Childbirth past 1 yr.	8299	0.040 (0.195)	0.010 (0.029) .74	-0.044 (0.050) .38	-0.025 (0.046) .59
Delivered in govt. hosp. past 1 yr.	8297	0.013 (0.115)	0.006 (0.016) .73	-0.010 (0.018) .56	-0.003 (0.011) .82
Delivered in facility past 1 yr.	8297	0.038 (0.191)	0.005 (0.028) .85	-0.035 (0.048) .47	-0.022 (0.046) .63
Doctor assisted with delivery past 1 yr.	8299	0.031 (0.174)	0.008 (0.026) .76	-0.040 (0.048) .41	-0.023 (0.044) .60
Doctor or nurse assisted with delivery past 1 yr.	8299	0.036 (0.187)	-0.004 (0.028) .88	-0.035 (0.049) .48	-0.032 (0.045) .48
Child delivery complications past 1 yr.	8293	0.011 (0.105)	-0.013 (0.016) .41	-0.020 (0.026) .44	-0.029 (0.020) .15
Ideal number of children	7881	2.479 (1.235)	-0.005 (0.207) .98	-0.060 (0.393) .88	-0.052 (0.291) .86
Biomarkers					
Overweight or obese	4853	0.282 (0.450)	0.014 (0.096) .89	0.031 (0.134) .82	0.038 (0.073) .60
Underweight	4853	0.088 (0.283)	-0.001 (0.050) .98	-0.042 (0.082) .61	-0.034 (0.043) .43
Abdominally obese	4723	0.700 (0.458)	-0.158 (0.134) .24	0.233 (0.180) .20	0.025 (0.126) .84
Mid-Upper Arm Circumference	4815	26.883 (3.507)	-2.363 (0.889) .008	1.363 (1.676) .42	-1.291 (0.995) .19
Hypertension, grade 1	4878	0.449 (0.498)	-0.113 (0.087) .19	0.067 (0.120) .58	-0.060 (0.078) .44
Hypertension, grade 2	4878	0.201 (0.401)	-0.075 (0.067) .26	0.090 (0.092) .33	-0.005 (0.075) .95
Skinfolds total	4065	26.851 (9.859)	-0.884 (2.627) .74	2.357 (5.267) .65	0.972 (3.504) .78
FWER crit. values					
Health value			0.013	0.013	0.013
Chronic disease			0.003	0.003	0.003
Health-related quality of life, ADLs, and physical health symptoms			0.05	0.05	0.05
Fertility			0.006	0.006	0.006

Notes. See Table A.9

Table A.10: Effect of insurance enrollment on endline (3.5 yrs.) health outcomes (complier average treatment effect estimates).

			Direct	Spillover	Total
		Control mean (SD)	Coefficient (SE) P-value	Coefficient (SE) P-value	Coefficient (SE) P-value
Health value					
Good health	9492	0.516	0.000	0.041	0.033
		(0.500)	(0.080)	(0.106)	(0.068)
			.00	.70	.63
Very good health	9492	0.068	-0.023	-0.069	-0.078
		(0.252)	(0.041)	(0.052)	(0.038)
			.57	.19	.04
Poor health	9492	0.104	-0.039	0.061	0.009
		(0.305)	(0.051)	(0.056)	(0.038)
			.44	.28	.81
Very poor health	9492	0.019	-0.050	0.032	-0.025
		(0.136)	(0.025)	(0.024)	(0.020)
			.04	.18	.21
Chronic disease					
Told had hypertension	9476	0.235	-0.074	0.089	-0.004
		(0.424)	(0.063)	(0.068)	(0.046)
			.24	.19	.94
Takes medication for hypertension	9495	0.196	-0.066	0.086	0.002
		(0.397)	(0.058)	(0.059)	(0.042)
			.26	.15	.97
Told had diabetes	9479	0.119	-0.077	0.063	-0.027
		(0.324)	(0.049)	(0.051)	(0.034)
			.11	.22	.43
Takes oral medication for diabetes	9499	0.106	-0.071	0.067	-0.018
		(0.308)	(0.047)	(0.051)	(0.031)
			.13	.18	.57
Takes insulin for diabetes	9489	0.027	0.020	-0.013	0.009
		(0.161)	(0.020)	(0.022)	(0.013)
			.33	.54	.48
Follows a special diet for diabetes	9495	0.078	-0.044	0.053	-0.003
		(0.268)	(0.042)	(0.044)	(0.029)
			.29	.23	.93
Told had cancer	9470	0.005	-0.001	0.008	0.006
		(0.068)	(0.008)	(0.009)	(0.005)
			.93	.36	.29
Treated for cancer past 1 yr.	9496	0.002	-0.003	0.005	0.001
		(0.048)	(0.006)	(0.006)	(0.003)
			.58	.36	.80
Told had lung disease	9479	0.055	-0.049	0.046	-0.012
		(0.229)	(0.031)	(0.036)	(0.021)
			.12	.20	.55
Told had heart disease	9473	0.043	-0.028	0.017	-0.015
		(0.202)	(0.034)	(0.039)	(0.021)
			.41	.67	.47
Told had stroke	9446	0.006	-0.012	0.010	-0.004
		(0.077)	(0.013)	(0.012)	(0.007)
			.34	.40	.54
Told had arthritis or rheumatism	9482	0.199	-0.052	0.116	0.040
		(0.399)	(0.064)	(0.086)	(0.054)
			.42	.17	.46
Had surgery due to arthritis past 1 yr.	9497	0.042	0.019	0.003	0.022
		(0.201)	(0.029)	(0.035)	(0.022)
			.50	.93	.32

Notes. Table presents (in all panels but the last one) estimates of complier average treatment effects of enrollment in insurance at different prices and subsidies on specific mendline (3.5 yrs.) health outcomes. The outcomes are listed in the first column. The last panel provides critical p-values that control the false discovery rate for different groups of outcomes listed in the first column. Each observation in our regressions is a household. Treatment variables are enrollment in insurance, the share of sample households in the village that enrolled in insurance, and the interaction of these variables. Enrollment is instrumented with household assignments: free insurance (A), sale of insurance + cash transfer (B), sale of insurance (C). Share enrolled is instrumented with the share of study households assigned to arms (A)-(C) in the same village. The interaction is instrumented with the interaction of household assignment and share of households assigned to (A)-(C). Standard errors were clustered at the village level. The direct effect only includes the coefficient on enrollment. The spillover effect is the estimated effect on an enrolled household of enrolling all other sample households in RSBY insurance. The total effect is constructed as the sum of the direct effect and 0.787*(spillover effect), since 78.7% is the maximum share of households that enroll in insurance in any arm. Mean and standard deviation in the control group are statistics from group D without access to insurance. Abbreviations: SD, standard deviation; SE, standard error; mos, months; yr, year.

Table A.10 (cont'd): Effect of insurance enrollment on endline (3.5 yrs.) health outcomes (complier average treatment effect estimates).

			Direct	Spillover	Total
		Control mean (SD)	Coefficient (SE) P-value	Coefficient (SE) P-value	Coefficient (SE) P-value
Diagnosed with high cholesterol	9449	0.022	0.004	-0.007	-0.002
		(0.147)	(0.022)	(0.023)	(0.014)
			.87	.77	.90
Takes medication to lower cholesterol	9496	0.010	-0.014	0.006	-0.009
		(0.097)	(0.015)	(0.012)	(0.009)
			.37	.62	.31
Has eyesight problems	9480	0.324	0.011	0.115	0.101
		(0.468)	(0.075)	(0.082)	(0.055)
			.89	.16	.07
Had surgery due to eye problems past 1 yr.	9497	0.080	-0.012	0.065	0.039
		(0.272)	(0.045)	(0.051)	(0.033)
			.78	.20	.23
Health-related quality of life, ADLs, and physical health symptoms					
Number of functional limitations	9413	0.681	-0.241	0.266	-0.032
		(1.528)	(0.207)	(0.268)	(0.203)
			.24	.32	.88
SF-8, PCS	9510	2.250	-0.068	0.291	0.161
		(0.899)	(0.171)	(0.186)	(0.124)
			.69	.12	.19
SF-8, MCS	9510	1.508	-0.027	0.079	0.035
		(0.789)	(0.121)	(0.153)	(0.116)
			.82	.61	.76
Had stomach pain past 4 wks.	8999	0.071	0.052	-0.028	0.031
		(0.256)	(0.038)	(0.051)	(0.024)
			.16	.59	.21
Had back pain past 4 wks.	8999	0.264	0.074	0.047	0.111
		(0.441)	(0.064)	(0.090)	(0.063)
			.25	.61	.08
Had chest pain past 4 wks.	8999	0.075	0.038	-0.009	0.032
		(0.264)	(0.043)	(0.046)	(0.026)
			.37	.85	.22
Had joint pain past 4 wks.	8999	0.586	-0.031	0.095	0.044
		(0.493)	(0.077)	(0.098)	(0.070)
			.68	.33	.53
Had headaches past 4 wks.	8999	0.202	0.056	0.065	0.108
		(0.401)	(0.060)	(0.076)	(0.044)
			.34	.39	.01
Had menstrual cramps past 4 wks.	8999	0.044	0.035	-0.030	0.011
		(0.206)	(0.024)	(0.033)	(0.020)
			.14	.36	.58
Had pain in no specific place past 4 wks.	8999	0.041	-0.006	0.019	0.009
		(0.198)	(0.035)	(0.039)	(0.025)
			.86	.63	.73
Fainted past 4 wks.	9451	0.012	0.014	-0.012	0.004
		(0.108)	(0.016)	(0.018)	(0.010)
			.38	.49	.67
Felt dizzy past 4 wks.	9451	0.132	0.082	-0.048	0.044
		(0.338)	(0.047)	(0.057)	(0.036)
			.08	.40	.22
Shortness of breath past 4 wks.	9451	0.056	0.035	0.011	0.044
		(0.230)	(0.032)	(0.034)	(0.022)
			.28	.74	.05
Heart pounded past 4 wks.	9451	0.071	0.011	-0.003	0.009
		(0.256)	(0.035)	(0.045)	(0.031)
			.75	.95	.77
Mental and emotional health and cognitive function					
GHQ-12, Likert scoring	9510	9.259	0.091	0.863	0.771
		(6.821)	(0.926)	(1.348)	(0.990)
			.92	.52	.44
GHQ-12, binary scoring	9510	2.451	0.424	0.065	0.476
		(3.323)	(0.482)	(0.739)	(0.524)
			.38	.93	.36
PSS-10 score	9510	17.842	-0.162	0.648	0.348
		(3.502)	(0.515)	(0.751)	(0.481)
			.75	.39	.47

Notes. See Table A.10

Table A.10 (cont'd): Effect of insurance enrollment on endline (3.5 yrs.) health outcomes (complier average treatment effect estimates).

			Direct	Spillover	Total
		Control mean (SD)	Coefficient (SE) P-value	Coefficient (SE) P-value	Coefficient (SE) P-value
Fertility					
Childbirth/stillbirth past 6 mos.	9493	0.045 (0.207)	0.009 (0.029) .74	0.010 (0.034) .78	0.017 (0.018) .35
Childbirth/stillbirth past 3 yrs.	9510	0.187 (0.390)	-0.005 (0.059) .93	0.003 (0.073) .96	-0.002 (0.045) .96
Number of births in hhld. past 3 years	9510	0.310 (0.927)	0.097 (0.133) .46	-0.199 (0.273) .47	-0.059 (0.148) .69
Number of live births in hhld. past 3 years	9510	0.202 (0.491)	-0.006 (0.067) .93	0.020 (0.079) .80	0.010 (0.051) .84
Number of still births in hhld. past 3 years	9510	0.109 (0.778)	0.103 (0.105) .33	-0.219 (0.242) .37	-0.069 (0.136) .61
Delivery in hospital past 3 yrs	9510	0.136 (0.343)	-0.039 (0.054) .46	-0.007 (0.055) .90	-0.045 (0.039) .25
Delivery in facility past 3 yrs	9510	0.149 (0.356)	-0.054 (0.053) .31	0.026 (0.055) .63	-0.033 (0.040) .40
Doctor assisted with birth past 3 yrs.	9510	0.152 (0.359)	-0.038 (0.052) .47	0.049 (0.058) .39	0.001 (0.037) .98
Doctor or nurse assisted with birth past 3 yrs.	9510	0.161 (0.368)	-0.036 (0.054) .51	0.043 (0.059) .47	-0.003 (0.039) .95
Delivery complications past 3 yrs.	9510	0.038 (0.192)	0.011 (0.029) .71	-0.015 (0.030) .62	-0.001 (0.019) .96
Mortality					
Death in hhld. past 3 yrs	9482	0.127 (0.333)	-0.005 (0.047) .92	0.029 (0.053) .58	0.018 (0.026) .49
Deaths in the HH past 3 yrs	9474	0.133 (0.362)	0.018 (0.050) .72	0.016 (0.057) .78	0.031 (0.028) .26
Sickness-related death in hhld. past 3 yrs	9482	0.080 (0.271)	-0.018 (0.041) .66	0.040 (0.044) .37	0.013 (0.022) .54
Number of sickness-related deaths in hhld.	9482	0.084 (0.290)	-0.012 (0.043) .77	0.040 (0.046) .38	0.019 (0.022) .39
FWER crit. values					
Health value			0.01	0.01	0.01
Chronic disease			0.003	0.003	0.003
Health-related quality of life, ADLs, and physical health symptoms			0.004	0.004	0.004
Mental and emotional health and cognitive function			0.017	0.017	0.017
Fertility			0.005	0.005	0.005
Mortality			0.013	0.013	0.013

Notes. See Table A.10

Table A.11: Number of significant health outcomes per category, without multiple testing adjustments.

	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
Significant direct effect	0	0	0	1	0	1	0	1
Significant spillover effect	0	0	0	1	1	0	0	0
Significant total	0	0	0	0	0	0	1	1
Chronic disease								
Total outcomes	15	15	15	15	17	17	17	17
Significant direct effect	1	2	2	1	0	3	4	0
Significant spillover effect	1	1	1	0	2	0	1	0
Significant total	0	1	0	1	0	1	0	1
Quality of life								
Total outcomes	1	1	1	1	14	14	14	14
Significant direct effect	0	0	0	0	0	0	0	0
Significant spillover effect	0	0	0	0	1	0	5	0
Significant total	0	0	0	0	1	1	4	2
Mental and behavioral health								
Total outcomes	0	0	0	0	3	3	3	3
Significant direct effect					0	0	0	0
Significant spillover effect					0	0	1	0
Significant total					0	0	0	0
Childbirth								
Total outcomes	8	8	8	8	10	10	10	10
Significant direct effect	0	0	0	0	0	0	0	0
Significant spillover effect	0	0	0	0	0	2	0	0
Significant total	0	0	0	0	0	0	0	0
Biomarkers								
Total outcomes	7	7	7	7	0	0	0	0
Significant direct effect	1	1	0	1				
Significant spillover effect	1	0	0	0				
Significant total	0	0	0	0				
Mortality								
Total outcomes	0	0	0	0	4	4	4	4
Significant direct effect					0	0	0	0
Significant spillover effect					0	0	0	0
Significant total					1	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 traditional critical p-value at the 5% significance level without making any multiple testing adjustments.

Table A.12: Estimates of demand and average cost using multiple different measures of cost.

	(1) Predicted	(2) Midline	(3) Mixed	(4) Endline	(5) ML attempt	(6) Mix attempt	(7) EL attempt
Price	0.3024	0.0219	0.6208	0.5095	0.1228	0.6260	0.6577
	(0.597)	(0.961)	(0.809)	(0.592)	(1.217)	(1.066)	(1.064)
Constant	754.9922	625.4481	455.1337	195.5689	1018.1084	779.6266	662.2843
	(60.463)	(97.754)	(81.974)	(60.045)	(123.958)	(108.652)	(107.910)
Observations	5008	4709	4988	4446	4685	4668	4447
AC curve:							
Constant	1332.916	667.235	1641.703	1169.459	1252.814	1976.070	1919.300
Quantity	-735.525	-53.182	-1510.150	-1239.472	-298.711	-1522.718	-1599.807
Welfare							
Demand LB: Group C (p)							
Free ins.	-1347.255	-1055.780	-672.573	-88.552	-1939.266	-1402.682	-1138.662
	0.000	0.000	0.001	0.504	0.000	0.000	0.000
Opt. price	981.233	661.854	1303.684	424.839	1009.435	2404.939	2458.271
	0.280	0.079	0.826	0.007	0.368	0.879	0.898
SOC	-0.475	-0.756	-0.157	-0.268	-0.655	-0.152	-0.120
	0.504	0.437	0.874	0.689	0.571	0.897	0.914
Demand UB: Group B (p)							
Free ins.	-743.078	-451.604	-68.397	515.624	-1335.090	-798.506	-534.486
	0.004	0.151	0.792	0.014	0.001	0.016	0.077
Opt. price	1302.995	1088.416	1052.488	528.483	1408.625	2124.629	1944.579
	0.008	0.000	0.084	0.639	0.068	0.812	0.827
SOC	-0.475	-0.756	-0.157	-0.268	-0.655	-0.152	-0.120
	0.504	0.437	0.874	0.689	0.571	0.897	0.914

Note. Top panel presents regressions of average cost (measured 7 ways, corresponding to column headings) on price. Each cell provides coefficient estimate and standard error in parentheses. Units of price and cost are INR. The 7 measures of cost are:

1. Predicted midline medical expenditures (same as outcome in Table 9) × midline successful use over 6 months × 2
2. Midline medical expend. × hospital share of endline medical expend. × midline successful use over 6 months × 2
3. Hospital expend. at endline × midline successful use over 6 months × 2 (same as column 1 in Table 10)
4. Hospital expend. at endline × endline successful use over 6 months × 2 (same as column 2 in Table 10)
5. Midline medical expend. × hospital share of endline medical expend. × midline attempted (successful or failed) use over 6 months × 2
6. Hospital expend. at endline × midline attempted use over 6 months × 2 (same as column 3 in Table 10)
7. Hospital expend. at endline × endline attempted use over 6 months × 2 (same as column 4 in Table 10)

Second panel presents estimates of a linear average cost curve: AC = Constant - β Quantity, where Constant and β are derived from estimates from top panel. Each cell provides a coefficient estimate. Third panel presents welfare calculations associated with a lower bound estimate of the Hicksian demand curve and each measure of cost. 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. Units are INR. Demand is estimated using data on households in the free-insurance arm (A) and the sale-of-insurance arm (C) only. The demand curve is assumed to be linear. This demand curve is a Marshallian demand because arm C is not compensated; if there are income effects, this is a lower-bound on Hicksian demand. Fourth panel provides same calculations as the thurd panel, except that it employs an estimate of an upper bound on the demand curve. Specifically, demand is estimated using data on arm A and the sale-plus-transfer arm (B). The demand curve is assumed to be linear. This demand curve shows demand that is greater than the Hicksian demand curve. The Hicksian demand curve would compensate those household who purchase insurance; however, this demand curve compensates all households, even those who do not purchase insurance. Those non-complier households experience a raw income effect.

Table A.13: Sample size per insurance access group and response rates by survey and insurance access group.

	Insurance access group						
	(A) Free-insurance	(B) Sale-plus-transfer	(C) Sale-of-insurance	(D) No access (control)	Total	Non-response	Attrition
			Initial sample				
Main surveys	4,401	2,180	2,146	2,152	10,879		
Biomarker surveys	1,363	718	737	833	3,651		
			Baseline survey				
Main survey	4,155	2,080	2,022	2,033	10,290	558	31
	(94%)	(95%)	(94%)	(94%)	(95%)	(5%)	(0.2%)
Biomarker survey	1145	588	615	692	3040	611	
	(84%)	(82%)	(83%)	(83%)	(83%)	(17%)	
			Midline (18 mos.) survey				
Main survey	4,091	2,036	2,007	1,977	10,111	439	329
	(93%)	(93%)	(94%)	(92%)	(93%)	(4%)	(3%)
Biomarker survey	1043	546	555	628	2772	879	
	(77%)	(76%)	(75%)	(75%)	(76%)	(24%)	
			Endline (3.5 yr.) survey				
Main survey	3,879	1,902	1,855	1,874	9,510	1,369	
	(88%)	(87%)	(86%)	(87%)	(87%)	(13%)	

Note. Table presents number of households in initial sample (top panel) and the number of households that respond to each survey (remaining panels), by insurance access arm (columns 1-4) and in total across arms (column 5). Column 6 provides the number of households that do not respond to a survey. Column 7 presents numbner of households that attrited out of the study at each survey. Abbreviations: mos., months; yr., year. Cells in top panel report the number of households. Cells in remaining panels report the number of households and (in parentheses) the percent of initial sample in an arm or in total sample that respond to a given survey. Percentages for non-response are number of households that do not respond to a survey devided by the total sample for that survey (from top panel), times 100.

Table A.14: Respondent and response rate by module and insurance access group.

	Insurance (access) group				
	(A) Free-Insurance	(B) Sale-plus-Insurance	(C) Sale-of-Insurance	(D) No access (control)	Total
Initial sample					
Main survey	4,401	2,180	2,146	2,152	10,879
Biomarker survey	1,363	718	737	833	3,651
Module & Respondent					
Baseline survey					
1. Most knowledgeable female.	4,063 (92%)	2,033 (93%)	1,980 (92%)	1,979 (92%)	10,055 (92%)
2. Most knowledgeable female. If none present, most knowledgeable male.	4,125 (94%)	2,070 (95%)	2,007 (94%)	2,010 (93%)	10,212 (94%)
3. Most knowledgeable male. If none present, most knowledgeable female.	3,831 (87%)	1,955 (90%)	1,873 (87%)	1,892 (88%)	9,551 (88%)
4. Female respondent.	1114 (82%)	566 (79%)	603 (82%)	656 (79%)	2939 (80%)
5. Male respondent.	1053 (77%)	545 (76%)	565 (77%)	634 (76%)	2797 (77%)
Midline (18 months) survey					
1. Baseline respondent. Otherwise, most knowledgeable male.	3,972 (90%)	1,977 (91%)	1,930 (90%)	1,922 (89%)	9,801 (90%)
2. Baseline respondent.	4,091 (93%)	2,034 (93%)	2,006 (93%)	1,977 (92%)	10,108 (93%)
3. Baseline respondent.	3,887 (88%)	1,913 (88%)	1,913 (89%)	1,868 (87%)	9,581 (88%)
4. Baseline respondent. Otherwise, male respondent.	970 (71%)	514 (72%)	525 (71%)	574 (69%)	2583 (71%)
5. Baseline respondent. Otherwise, female respondent.	928 (68%)	483 (67%)	473 (64%)	526 (63%)	2410 (66%)

Note. Different modules of the baseline and midline surveys are directed towards different household members. This table presents number of households in initial sample (top panel) and the respondent to whom each module is directed (remaining panels). Columns 2-7 present number of households where respondent answered the survey module (in parentheses) the implied response rate, for each of the 4 study arms and overall. Respondent and response rates are reported for baseline and midline surveys only since the endline survey was administered to a single respondent. Module 1 measures intra-household allocation, fertility and family planning, maternal and newborn health, child care and development, child health and investment, adult health behavior, adult health, cognitive survey, expectations (midline only). Module 2 measures household composition, household characteristics, household income, household expenditures, household indebtedness, savings, demonetization (midline only). Module 3 measures cognitive task, trust, cognitive survey, adult health behavior, adult health, risk unincentivized, expectations (midline only). Module 4 measures female biomarkers, child biomarkers. Module 5 measures male biomarkers.