Adverse selection does not explain why utilization rises with premiums: evidence from a health insurance experiment in India

Cynthia Kinnan[*], Anup Malani[†], Alessandra Voena[‡], Gabriella Conti[§], and Kosuke Imai[¶]

March, 2020

Abstract

Information asymmetries or community-rating can lead to adverse selection into health insurance. We use a multi-armed RCT that varies insurance premiums to study selection into a health insurance program in India called Rashtriya Swasthya Bima Yojana (RSBY). Limited fiscal capacity in low and middle income countries (LMICs) may necessitate charging premiums, which may exacerbate adverse selection. Moreover, the degree of selection may differ in LMICs due to limited healthcare supply and knowledge, and constraints that interact with selection in *a priori* ambiguous ways, e.g., liquidity. We find mixed evidence on selection into the program. While those who purchase insurance when premiums are high use insurance more, they are no higher risk than those who purchase at lower prices.

To interpret these findings we appeal to a literature in development economics that studies why price sometimes increases utilization of a product. That literature suggests three explanations: selection, price as a signal of quality, and sunk costs. Given this framing, our positive correlation test is not dispositive because it is consistent with all three theories. However, the evidence that enrollment does not vary with risk is inconsistent with selection. We are also able to rule out that price signals quality. Our study employs a two-stage randomization that varies the premium that neighbors pay, holding constant what a household pays. We find that

[*]Tufts University, cynthia.kinnan@tufts.edu.
[†]University of Chicago, amalani@uchicago.edu.
[‡]University of Chicago, avoena@uchicago.edu.
[§]University College London, gabriella.conti@ucl.ac.uk.
[¶]Harvard University, imai@harvard.edu.

people do not utilize insurance more when their neighbors are charged a higher price. These results suggest that sunk costs effects, rather than adverse selection, explain the effect of price on utilization in our context.

1 Introduction

Sickness poses significant health and financial risk in low and middle income countries (LMICs). Roughly 150 million households are pushed into poverty due to health expenditures each year (Shahrawat and Rao, 2011).[1] In response, many LMICs have stated a goal of achieving universal health insurance coverage (Lagomarsino et al., 2012).

Weak state capacity, however, makes it difficult to achieve universal coverage. Administrative constraints make it difficult to enforce private purchase of health insurance (Banerjee et al., 2019), while low fiscal capacity makes it difficult for the government to provide free insurance for everyone. LMICs may want to ease their fiscal burden via cost sharing, i.e., charging households premiums to enroll or imposing deductibles or copays for insurance (Banerjee et al., 2019). Cost-sharing for insurance, however, raises the specter of adverse selection, namely that high-cost individuals may be more likely to participate (Akerlof, 1970; Einav and Finkelstein, 2011). This, in turn, can raise unit costs, further reducing insurance enrollment and threatening the goal of universal coverage.

Adverse selection has been studied in LMICs by several papers, including Banerjee et al. (2019), Asuming et al. (2019) and Fischer et al. (2018).[2] This paper contributes to this literature by leveraging a large sample (∼50,000 person) experiment in Central and South India that gave participants access to an at-scale government-run hospital insurance plan (Rashtriya Swasthya Bima Yojana, or RSBY). Importantly, we randomized the price at which households had access to RSBY. Using a two-stage randomization, we also varied the premiums that neighbors faced, holding constant the premium a given household faced. Our large sample, design, and rich survey data allows us to study selection and its mechanisms.

There are typically two ways to test for adverse selection. The more common approach examines ex post utilization: If those who enroll at higher insurance prices or with more generous plans utilize more, then there is adverse selection (Chiappori and Salanié, 2013; Einav and Finkelstein, 2011).

[1] In India alone, as many as 63 million may fall into poverty each year due to health spending (Berman et al., 2010; Shahrawat and Rao, 2011).

[2] We focus here on papers looking at LMICs. There is also an important literature testing for adverse selection in developed country contexts; see Einav and Finkelstein (2011) for an overview.

This test is also called a positive correlation test. Another approach examines enrollment into insurance: If those who are more likely to consume care—e.g., because they have worse *ex ante* health—are more likely to enroll, there is likely to be adverse selection.

In our setting, these two tests produce opposite results. The utilization or positive correlation test suggests that there is adverse selection: individuals who have to pay full price for insurance are more than 40% more likely to use insurance than those who obtain insurance for free. The enrollment test, however, suggests that that there is no adverse selection. Individuals who are less healthy are no more likely to enroll in insurance, either at the extensive margin (access vs. no access) or as a function of the premium charged. This finding is consistent with Das and Leino (2011).

To reconcile these results, we appeal to another literature – from development economics – that examines why individuals who pay higher prices for products use those products more (Ashraf et al., 2010; Cohen et al., 2010).[3] This literature applies to our study because our implementation of the positive correlation test for adverse selection varies the price of insurance instead of, e.g., comparing insured and uninsured consumers. Moreover, our study is not confounded by moral hazard because insured households face no coinsurance whether they pay a high premium or not.

This second literature tests three explanations for why price may be positively correlated with utilization. First, there is selection: people who will utilize the good more select into purchasing it. Second, price signals quality: higher price convinced people the good is more valuable so they use it more. Third, there are sunk costs or psychological commitment effects: individuals use the product more because they already paid for it.[4]

We are able to test and reject two of these theories. Our utilization test does not distinguish the theories because all three theories are compatible with a positive relationship between price and utilization. Our enrollment test for selection, however, rules out the selection explanation. Moreover, we use a unique feature of our experiment to rule our the price-signals-quality theory. Our

[3]There are also two other related but distinct literatures. One literature examines how price affects targeting, another form of selection (Alatas et al., 2016). That literature asks whether price can screen in subpopulations that welfare programs want to target. Another literature examines how price today affects uptake at a later date. The later literature examines whether low price today reduces future purchases because it serves as an anchor (Fischer et al., 2019) or whether low price today raises future purchases because allows people to try an experience good (Dupas, 2014).

There are also studies that find that higher prices are not associated with greater use (Hoffmann, 2009; Cohen et al., 2010; Dupas, 2014).

[4]Ashraf et al. (2010) directly test but find no evidence for sunk cost effects.

experiment uses a two-stage randomization design to vary both the price that individual households pay for insurance and the average price households in a village pay for insurance.[5] Assuming that one's consumption is a function of not just the price that one paid, but also what neighbors paid (Kremer and Holla, 2009),[6] we use variation in what neighbors pay to test price-signals-quality. We find that neighbor's variation does not affect one's use of insurance, holding own-price constant. By process of elimination, this leaves the sunk cost theory as the likely explanation for our results.

Our findings contribute not just to the literature on adverse selection into insurance and how price affects quality, but also to the literature on how price affects uptake. Others have documented that even small prices affect uptake of health-related products (Kremer and Miguel, 2007; Agha et al., 2007; Cohen et al., 2010; Ashraf et al., 2010; Kremer et al., 2011).[7] We find that, although higher price lowers uptake, there is still substantial uptake at full price[8]. Changing the premium from zero to the amount that the government pays lowers uptake by nearly 20%, but uptake is 60% even at the higher price. One possibility is that this result is a product of the fact that our study looks at populations that are wealthier: our sample is above the poverty line. However, we do not find an income gradient on uptake even at full price.

Section 2 provides background on the health care financing in India and the government's Rashtriya Swasthya Bima Yojana (RSBY) health insurance program. Section 3 describes our experiment. The purpose was to examine the effect of expanding eligibility in RSBY. Our study randomized roughly 11,000, above-poverty-line households ($\sim$ 50,000 individuals) in the state of Karnataka to four treatment arms which varied access to RSBY insurance coverage: (A) free access to RSBY, (B) access to RSBY for roughly INR 200 plus an unconditional cash grant of INR Rs. 200, (C) access to RSBY for roughly INR 200 (the premium for RSBY), and (D) no intervention. To measure spillover effects across households, we employed a two-stage design that randomized villages to different allocations of households across access arms before we randomized households to those four arms. In this manner we are able to vary the share of households that face different

[5]Specifically, we have an arm that pays zero price for insurance and an arm that pays full price. We randomize villages to different allocation of its households to those arms, e.g., 50% get free insurance in one village and 30% get free insurance in another village. We then randomize households in villages to the two arms based on the village allocation.

[6]Fischer et al. (2019) is not to the contrary. They examine the effect of price of one product on the demand for other products. Here we leverage how prices that one person pays for a product affects how much others are willing to pay for that same product.

[7]There are similar findings with respect to education-related products (Duflo et al., 2006; Evans et al., 2008).

[8]This is consistent with the finding in Tarozzi et al. (2014).

prices in each village.

Section 4 describes our outcomes and estimation strategy. We estimate the intent-to-treat (ITT) effect of different methods of access to insurance by regressing outcomes on indicators of our 4 access arms. We estimate complier average treatment effects (CATE) via instrumental variables methods. We regress outcomes on insurance enrollment, an endogenous variable. We we instrument for enrollment with random assignment to one of three access arms. We estimate spillovers by interacting either access or enrollment with (indicators for) the share of households paying different prices.

Section 5 examines evidence of adverse selection. In the process it also reports other findings related to uptake. Uptake is high even when people are charged full premiums. Moreover, while uptake was higher in our study of above-poverty-line households than the main RSBY program, which only enrolled below-poverty-line households, this is not because there is an income gradient on enrollment in our sample.

Section 6 tries to understand the conflicting results on adverse selection by using a framework from the literature on why price may affect utilization. Specifically, we explore whether the positive effect of price on utilization may be a consequence of price signalling the quality of RSBY. This section also explores why we do not find adverse selection. In the appendix, we look for and fail to find evidence on advantageous selection into insurance using data on risk preference among our sample households. We also explore whether lack of information about insurance or limited cognitive capacity can explain limited selection.

2 Background

2.1 Health care context in India

Indians have access to subsidized government health care facilities and non-subsidized private medical providers, but rely primarily on the latter. The government operates a large number of facilities, from Primary Health Centres and Sub-Centres to District Hospitals. The government facilities largely offer free care, though they may not cover all populations, their lack supplies, and their quality has been questioned (Comptroller and Auditor General of India, 2019). Private doctors have offices and run small clinics, and there are private hospitals of various sizes. Private facilities will often request at least a down payment before providing service and, in some cases, do not release the patient until the negotiated bill is paid in full. Nevertheless, the private sector provides 80% of all outpatient treatment and 60% of all inpatient treatment in the country (Ministry of Health and Family Welfare, 2014). Although total medical expenditures are roughly 4.02% of GDP, government expenditures amount to only 1.15% of GDP. Overall, India faces a shortfall in supply in providers. For instance, 47% of children live in villages without any health facility at all (Ma and Sood, 2008).

Aside from RSBY (at the time of the study) and PMJAY (now), there are a limited set of insurance options in India. Treatment at government facilities is largely free.[9] In addition, the central government operates a scheme called Janani Suraksha Yojana (JSY) that provides cash payments to mothers who deliver in an institutional setting (as opposed to their homes). Some state governments have provided insurance programs that cover tertiary care (e.g., Arogyasri in Andra Pradesh and Vajpayee Arogyashri in Karnataka). In addition there are private insurance options, often provided by employers, including the government for its employees. However, private insurance covers less than 4% of all medical expenditures and this too is concentrated in urban centers. Although RSBY and PMJAY are ambitious public insurance scheme, insurance coverage presently accounts for just 8% of government health expenditures (Gupta and Chowdhury, 2014). As a result, 69% of all expenditures remain out-of-pocket.

In the areas of Karnataka where this study takes place, there are typically no insurance options aside from RSBY, Arogyashri Vajpayee, and a plan called Yeshasvini, which is only available to members of certain occupational cooperative societies (i.e., trade associations).

2.2 Rashtriya Swasthya Bima Yojana insurance scheme

RSBY was introduced in 2008 to provide hospitalization insurance to India's poor. Like Medicaid in the U.S., it is largely free to enrollees and is designed and largely funded by the national government, but administered by the state governments.

Eligibility. All households carrying BPL ration cards or those with members in certain occupations[10] are eligible for RSBY. In addition states can expand eligibility to other groups so long as

[9] However, public facilities frequently have shortages of consumables such as anesthetics and x-ray film. In these cases, patients have to purchase them from the private sector and bring them to the public hospital (Comptroller and Auditor General of India, 2019).

[10] These include: (1) building and other construction workers registered with the welfare boards; (2) licensed railway porters; (3) street vendors; (4) MNREGA workers who have worked for more than 15 days during the preceding financial year; (5) beedi workers; (6) domestic workers; (7) sanitation workers; (8) mine workers; (9) rickshaw pullers; (10) rag pickers; and (11) auto/taxi driver. See `http://www.rsby.gov.in/about_rsby.aspx`.

they (as opposed to the central government) pay the full cost of these groups. The scheme covers up to five members of each enrolled household: the head of household, the spouse and up to three dependents[11]. The threshold to define a household as BPL is set at approximately INR 900/month in rural areas, and INR 1,100/month in urban areas in Karnataka.

Coverage. RSBY covers up to INR 30,000 per year per household for over 700 procedures at empaneled hospitals. The covered procedures largely include those that require an overnight stay at a hospital, though there are a number of so-called day surgeries that are also covered[12]. Child delivery is also included. There are no deductibles or co-pays. RSBY covers all pre-existing diseases and there is no age limit for beneficiaries. The rates of most surgical procedures are fixed[13]. Transportation charges are also covered at a rate of INR 100 per hospitalization up to a maximum of INR 1,000 per year. The coverage lasts one year starting the month after the first enrollment in a particular district, but is often extended without cost to beneficiaries.

Administration. RSBY is a completely paperless program which uses biometric-enabled smart cards as a vehicle of delivery. Empaneled hospitals include both private hospitals and government hospitals that meet certain criteria and sign MOUs with the state agency running the scheme; by implication, not all public hospitals are included. Insurance is provided by private companies, but the premium is paid for by the government. Government funding is shared by the central and the state government in a 3:1 ratio. The insurance premium is determined at the state-level based on an open-tender process. (The premium costs approximately INR 200 in the state of Karnataka). The only cost to the beneficiary is that of a registration charge of INR 30 to obtain the smart card.

[11] An exception is in the case of childbirth: the newborn is always covered even if five members of the household are already covered. This coverage continues until the renewal date, at which point the newborn is only covered if the household chooses to include it among the five that are covered. See `http://www.rsby.gov.in/faq_medical.aspx`.

[12] These include: haemo-dialysis; parenteral chemotherapy; radiotherapy; eye surgery; lithotripsy (kidney stone removal); tonsillectomy; D&C; dental surgery following an accident; surgery of hydrocele; surgery of prostrate; few gastrointestinal surgery; genital surgery; surgery of nose; surgery of throat; surgery of ear; surgery of urinary system; treatment of fractures/dislocation (excluding hair line fracture), contracture releases and minor reconstructive procedures of limbs which otherwise require hospitalization; laparoscopic therapeutic surgeries that can be done in day care; identified surgeries under general anesthesia; and any disease/procedure mutually agreed upon. See `http://www.rsby.gov.in/faq_medical.aspx`.

[13] They can be found at `http://www.rsby.gov.in/Documents.aspx?ID=4`.

3 Experimental Design

We carried out a randomized controlled trial (RCT) to test the impacts of different methods of expanding eligibility for RSBY hospital insurance to APL households.[14]

3.1 Intervention is access to RSBY

We evaluate three methods of accessing RSBY:

A. Free RSBY. Households obtain access to RSBY for no charge, not even the INR typically charged to obtain a biometric, smart card that functions as the insurance card.

B. Right to purchase RSBY and an unconditional cash transfer. Households receive the right to purchase RSBY – for the premium the government pays for RSBY in their district plus the cost of an insurance (smart) card – within 3 weeks. Households obtain an unconditional cash transfer equal to RSBY premium plus the cost of the smart card.

C. Right to purchase RSBY. Households receive the right to buy RSBY (for the same price in condition B) in the next 3 weeks, but no cash transfer, conditional or otherwise.

We compare outcomes under these conditions versus a control condition:

D. No intervention.

We choose these interventions because they address important academic and policy questions. Programs such as RSBY in India or Medicaid in the US are actually two conceptually distinct interventions: access to insurance at full price (i.e., pure insurance) and a (conditional) cash transfer equal to the insurance premium. Moreover, different interventions have different budgetary effects; we want to net these out by comparing budget neutral alternatives such as conditional and unconditional cash transfers. We seek to value each of these interventions. These not only have different effects, they are different policy options available to the government. Comparing condition C to condition D yields the value of pure insurance. Comparing condition A to C yields the value of a conditional cash transfer. Finally, comparing conditions A and B yields the difference between a conditional and an unconditional cash transfer.

[14]This study was registered before outcomes were measured in the first post-treatment survey at 18 months. A pre-analysis plan was posted prior to the last follow-up survey at 4 years under American Economic Association Registry Identifier AEARCTR-0001793.

In this paper we mainly focus on the effect of premium on uptake and utilization of insurance, i.e., we compare arm C to arms A and perhaps B.

3.2 Sampling Strategy

For a household to be eligible for the study, it had to meet the following inclusion criteria at the start of the study in 2013:

1. The household resided in a village in Gulbarga or Mysore districts;
2. The household resided in village within 25 km of a hospital empaneled in RSBY; and
3. A member of the household must hold an Above Poverty Line (APL) ration card.

The trial was conducted in two districts selected because they are representative of central and southern India, respectively. We focused on APL households because they were treatment naive: they were not otherwise eligible for RSBY. We use the distance-to-hospital restriction to ensure that insurance would have some value. Hospital insurance is not a useful contract in the absence of hospitals at which to use insurance.

The exclusion criteria for the study were:

1. the household resided in a village with ≤ 10 eligible households;
2. The possession of a BPL card by a member of the household;
3. Having a member working in one of the occupations that made the household eligible for RSBY regardless of BPL status; or
4. Having a member with insurance that covered secondary hospital care (most commonly, Yeshasvini).

We eliminated villages with small populations because surveying those villages was not cost-effective.

3.3 Sample size

The target enrollment for each of the 4 household-level arms, unconditional on village-level arm, was 4,500 households for condition A and 2,250 households for each of conditions B, C and D. The 2,250 target for conditions B to D ensured the RCT was powered to detect a 25% change in hospitalization rate across study arms, allowing for a 10% attrition rate. The sample size for condition A was doubled because we thought (correctly) that that woudl be the likely approach used by the government to expand eligibility for public insurance. Due to some attrition between listing and completion of baseline, our final randomized sample amounted to 11,089 households in 424 villages in the two districts.

3.4 Data Collection

We conducted 6 rounds of data gathering: (1) a listing exercise; (2) a baseline survey; (3) an enrollment survey; (4) a 12 month follow-up survey, which uses a novel design we call a Post-Health Event Survey (PHES); (5) an 18 month follow-up survey, which we label a midline survey; and (6) a 4 year follow-up survey, which we call an endline. Here we report results using data from our baseline, midline and endline surveys. The surveys themselves are described in Appendix A. The remainder can be found in our posted pre-analysis plan.

3.5 Treatment assignment

We designed a two-stage randomization process to study both direct treatment and spillover effects of different methods of providing access to health insurance.

In a first stage, we randomly assigned villages to one of five village-level arms. A village-level arm is defined by the percentage of households within the village assigned to each of the four household-level study arms defined in subsection 3.1. The percentage allocations to the 4 household-level arms are given in the last 4 columns of Table 1. The percent of villages assigned to each of the 5 village-level arms are in the second column. Villages were matched before this first stage randomization.[15]

In a second stage, we randomly assigned households within a village to the four arms according to the allocation probabilities assigned to the village. Households were matched before this second stage randomization.[16]

[15]Specifically, we first stratified villages into quintiles of # eligible hhds per village. Within each quintile, we created blocks of 20 villages. Using data from our listing exercise on average values for certain variables (among eligible hhds in a village), create blocks using Mahalanobis matching on: education, age of household head, # children, # rooms in house; and caliper matching on binary variables: major illness, unemployment in household. Within a village block, we randomly assign villages to 5 village-level conditions (without replacement).

[16]Specifically, within each village, we first created blocks of 10 eligible and consented households. Using household-level data from listing survey, create blocks using Mahalanobis matching on: education, age of household head, #

Table 1: IHIE Two-Stage Randomization Design

Village-level arms (%)		Household-level arms (%)			
Arm	Village allocation	Group A	Group B	Group C	Group D
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
Total	100	40	20	20	20

We describe how treatment was delivered to subject households in Appendix B.

3.6 Balance Tests

We conduct balance tests to validate that assignment to treatment was indeed random. We do this in 3 steps. First, we gather baseline measurements on a range of variables on demographics, financial status and health (number of persons and of children in household, age and education of head of household, distance to nearest town, number of rooms and of concrete rooms in home, annual household budget and good expenditure, major sickness) and on a subset of outcomes (visited health care facility, annual hospital and non-hospital expenditures). Second, we estimate multinomial logit models predicting household treatment assignments for each household (A/B/C/D) as a function of outcomes measured at baseline, one outcome at a time. Third, we conduct likelihood ratio tests where the null model is the same multinomial model without the baseline covariate, to determine if we can reject the null that these two models are statistically equivalent, i.e., that the baseline covariate has no explanatory power. We collect the p-values from these LR tests.

If the randomization is successful, then the p-values from these tests should stochastically dominate the uniform distribution. Figure 1 plots these p-values and the CDF of the uniform distrobution. A one-sided Kolmogorov–Smirnov test confirms that our p-values stochastically dominate a uniform ($P = 1.000$).

children, # rooms; and caliper matching on binary variables: major illness, unemployment in household. Within each block of households, we randomly allocate households to the 4 household-level conditions in accordance with the village-level assignment probability condition (without replacement).

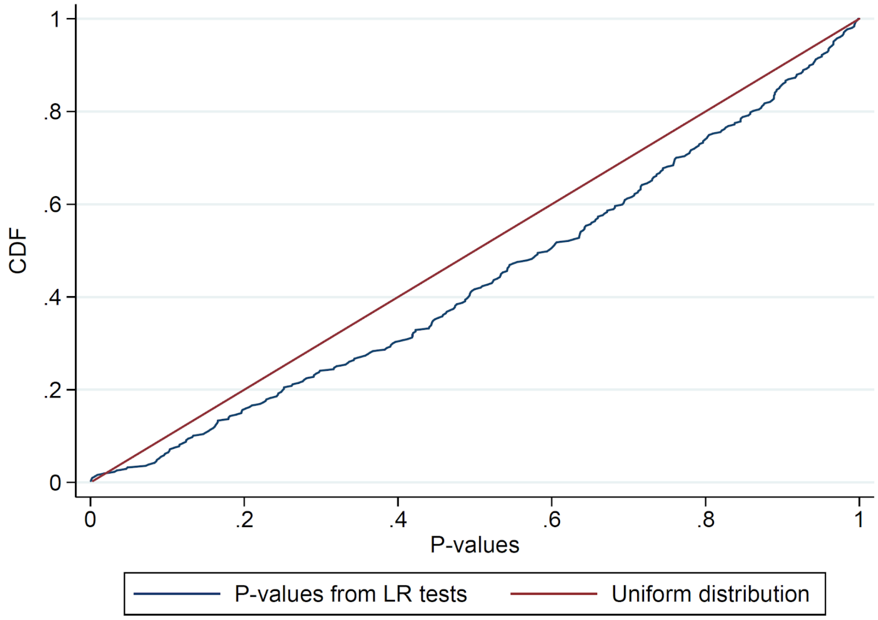

Figure 1: Test of balance.

3.7 Sample Attrition

Randomization was based on a listing of households conducted prior to baseline. Table 2 provides numbers on response rates and attrition over the course of the study. Attrition between randomization and baseline can be attributed to household non-response, inability to locate sample households, households no longer meeting eligibility criteria for the study, and missing baseline data. Attrition between baseline and midline and then midline can be attributed to household non-response and the inability to locate sample households, including their movement outside the village. Some households with missing baseline data were able to be surveyed at midline, and endline.

4 Empirical implementation

4.1 Outcomes

Although the study measures a number of outcomes, including both subjective and objective measures of health for multiple members of each household, this paper focuses on three largely household-level outcomes: enrollment in RSBY, utilization of the RSBY card for payment, and utilization of health care facilities. We describe each outcome in turn.

Enrollment is measured in a straightforward manner. We examine whether sample APL house-

Table 2: Randomized sample, response rate and attrition by survey

	Insurance (access) arm					
	(A) Free insurance	(B) Sale of insurance + transfer	(C) Sale of insurance	(D) No inter-vention	Total	Attrition
Randomized sample	4,401	2,180	2,146	2,152	10,879	
Biometric sample	1,363	718	737	833	3,651	
BASELINE SURVEY						
Main survey	4,155	2,080	2,022	2,033	10,290	589
	(94%)	(95%)	(94%)	(94%)	(95%)	(5%)
Biometric survey	1,324	652	702	748	3,426	225
	(97%)	(91%)	(95%)	(90%)	(94%)	(6%)
MIDLINE SURVEY (18 MO.)						
Main survey	4,091	2,036	2,007	1,977	10,111	768
	(93%)	(93%)	(94%)	(92%)	(93%)	(7%)
Biometric survey	1,195	607	632	667	3,101	550
	(88%)	(85%)	(86%)	(80%)	(85%)	(15%)
ENDLINE SURVEY (4 YR.)						
Main survey	3,879	1,902	1,855	1,874	9,510	1,369
	(88%)	(87%)	(86%)	(87%)	(87%)	(13%)

holds enrolled in RSBY during the 1 or 2 days that we brought a mobile enrollment truck to their village or town during our 2015 enrollment drive. In theory households in arms A to C have the option to enroll either when the RSBY enrollment truck visited their village or by visiting the RSBY office in each district at any time, we have no evidence that enrollment at the district office, which in general is very uncommon, occurred in our sample. Nor did the government conduct any RSBY enrollment drive after 2015 for any group. In any case, group D households were ineligible to enroll in RSBY via any channel.

Utilization of the RSBY card for payment was measured via multiple questions at midline and endline. First, we asked if the household attempted to use the RSBY card for payment in the last 6 months. Second, we asked if they were successful during that attempt. Third, if the household reported that they were no successful, we asked why the household was unable to use the RSBY card for payment.

Utilization of medical care was challenging because there is no agreed upon definition of a hospital. At baseline and endline we asked the male and the female respondents to our surveys if they had visited a hospital in the last year, as has been done on other surveys, including the National Statistical Survey (NSS). Our assumption was that people would recognize that a hospital is a facility in which one can be admitted for overnight stays. However, our examination of the resulting hospital utilization rate and separate but concurrent ethnographic work we did in each district suggests that respondents call even small clinics hospitals. Therefore, we interpret hospital use at baseline and midline as health care facility use, not just hospital use.

At endline we corrected this problem by asking specifically about whether the hospital that the respondent visited was one in which a patient could stay overnight. (We defined these as "big hospitals" for respondents.) [17] We separately asked whether the respondent visited a hospital and had an outpatient or "day" surgery. In addition we asked this question in the abstract and in the context of the most significant illness in the household over the last 6 months.

4.2 Regression specifications

4.2.1 Intent-to-Treat (ITT)

Our analysis will focus on two types of parameters. The first is the ITT estimate of the impact of different forms of access to insurance (free, at cost, etc.). Our basic regression model to estimate this is:

$$y_{ijt} = \gamma_h d_{ij}^h + e_{ijt} \tag{1}$$

where y_{ijt} is the outcome for household i in village j at time t; d_{ij}^h is the indicator for household group $h \in \{1, 2, 3, 4, \}$; and γ_h for $h \in \{1, 2, 3\}$ are ITT estimators for adding a household to groups A, B, and C (D is the baseline). Since we are omitting indicators for village allocation groups, the estimators give the impact of access conditional on the average share of sample households allocated to groups A to D. On average, 80% of these households are in group A to C, and 20% do not have access to RSBY.

We weight each household equally because no household is obviously more informative than any other, especially when examining questions about the impact of insurance on households. We use

[17] While this phrasing lowered the implied utilization rate, it remained higher than the utilization rate on the NSS survey.

the cluster robust HC2 standard errors at the village level. Imai, Jiang, and Malani (2019) shows that this is a conservative variance in two-stage randomized experiments.

Spillovers. To address the possibility of spillovers from, e.g., giving some households access to insurance at a positive price affects households given access to insurance for free, we do a few things.

First, we use modified regression specification. A natural candidate would be

$$y_{ijt} = \sum_{v=1}^{5} \beta_v d_j^v + \sum_{h=1}^{3} \gamma_h d_{ij}^h + \sum_{h=1}^{3} \sum_{v=1}^{5} \lambda_{hv} d_j^v d_{ij}^h + e_{ijt} \tag{2}$$

where d_j^v is the indicator for village group $v \in \{1, 2, 3, 4, \}$, corresponding to I, II, III, IV and V village groups; ; and (β_v, λ_{hv}) measures spillovers. However, this is not the specification we use. In order to facilitate interpretation of village group, we estimate an isomorphic specification that recodes the village groups variables, $(d_j^{v=1}, ..., d_j^{v=5})$, to reflect share of village sample in arms A, B, C, and D, $(\sigma_j^1, ..., \sigma_j^4)$. The mapping from village group indicators to arm shares is given by the last four columns of Table 1. So, for example, the indicator for village group 1, $d_j^{v=1}$, maps to $(\sigma_j^1, \sigma_j^2, \sigma_j^3, \sigma_j^4) = (0.3, 0.5, 0.1, 0.1)$. Our modified regression model is now:

$$y_{ijt} = \sum_{h=1}^{4} \beta_h \sigma_j^h + \sum_{h=1}^{3} \gamma_h d_{ij}^h + \sum_{h=1}^{3} \sum_{h'=1}^{4} \lambda_{hh'} \sigma_j^{h'} d_{ij}^h + e_{ijt} \tag{3}$$

where σ_j^h is the proportion of group $h \in \{1, 2, 3\}$ or {A,B,C} in village j. Unlike the nonparametric approach given in equation (3), this equation is based on a parametric assumption of linear effects in shares. However, this specification increases power as it has fewer parameters to be estimated and allows for extrapolation regarding the effects of varying treatment proportions (see Appendix E.1 of Imai, Jiang, and Malani (2019)).[18]

Second, we measure ITT effects accounting for spillovers at the village level with dummies for the proportion of sample households in a village who are in the no-intervention arm. There are three such proportions: 50, 30, and 10% not treated or, equivalently, 50, 70 and 90% treated (i.e., in arms A, B or C).

Third, we make assumptions required for unbiased estimation of, (γ_h, λ_{hv}) with spillovers as given in Imai, Jiang, and Malani (2019). The key assumption is partial non-interference, i.e., assignments in villages $j' \neq j$ do not influence outcomes in another village j.

Fourth, to test for the existence of spillover effects, we conduct the Wald test with the null

[18] `https://imai.fas.harvard.edu/research/files/spillover.pdf`.

hypothesis that $\beta_v = \beta_{v'}$ and $\lambda_{vh} = \lambda_{v'h}$ for any $v \neq v'$ and $h = 1, 2, 3, 4$. This test can be conducted for both take-up and any outcome variable of interest.

Fifth, in cases where we want to report ITT under sample average spillovers, we will exclude interaction between household arms and, e.g., village arms.

4.2.2 Complier average treatment effect (CATE)

Our second parameter of interest is the CATE estimate of enrolling in RSBY. Our basic regression model to estimate this has a two-stage least squares structure:

$$y_{ijt} = \alpha + \theta s_j + \phi z_{ij} + \rho s_j z_{ij} + u_{ijt} \tag{4}$$

where z_{ij} is the binary enrollment variable by household i in village j, and $s_j = \sum_{i=1}^{n_j} z_{ij}/n_j$ is the enrollment rate in village j. We estimate this equation using instrumental variables, d_j^v, d_{ij}^h, and $d_j^v d_{ij}^h$ (see Appendix E.2 of Imai, Jiang, and Malani (2019), for $(s_j, z_{ij}, s_j z_{ij})$. The remaining variables are defined as in equation (3). Weighting is managed as it is in the previous section.

The exclusion restriction for CATE estimation is that treatment assignment only affects household outcomes through the decision to enroll in RSBY (see Imai, Jiang, and Malani (2019) for details). Note we are not instrumenting *utilization* of RSBY, which would require a stronger exclusion restriction. We are instrumenting enrollment in RSBY.

Spillovers. When we are not interested in estimating spillovers, we omit the terms θs_j and $\rho s_j z_{ij}$ and any instruments that include d_j^v terms when we estimate (4). When we are interested in estimating spillovers, we include those terms and do many of the things we do for the ITT analysis. For example, as discussed by Imai, Jiang, and Malani (2019), we will use the cluster robust HC2 variance or the weighted average of individual and cluster robust HC2 variances. The most important difference is the assumptions required for unbiased estimation with spillovers, as given in Imai, Jiang, and Malani (2019). Beyond partial non-interference as before, we must assume that for noncompliers their treatment assignment does not affect their outcome through the enrollment of other units. Moreover, we test for the existence of spillovers by testing the null hypothesis that $\theta = 0$ and $\rho = 0$.

4.2.3 Testing for heterogeneous effects

Some of our analyses will examine whether the impact of access to insurance and uptake into insurance varies with certain individual or household characteristics – such as health status – at baseline. To do so, we interact the characteristics with household and village group indicators. For example, for ITT estimates and heterogeneous impacts from household-level treatments, the estimated equation would be:

$$
\begin{aligned}
y_{ijt} = \alpha + & \sum_{v=1}^{3} \beta_v \sigma_j^v + \sum_{h=1}^{3} \gamma_h d_{ij}^h + \sum_{h=1}^{3} \sum_{v=1}^{3} \lambda_{hv} \sigma_j^v d_{ij}^h \\
& + \sum_{v=1}^{3} \beta_{vw} \sigma_j^v w_{ij} + \sum_{h=1}^{3} \gamma_{hw} d_{ij}^h w_{ij} + \gamma_w w_{ij} + \sum_{h=1}^{3} \sum_{v=1}^{3} \lambda_{hvw} \sigma_j^v d_{ij}^h w_{ij} \\
& + \phi w_{ij} + \delta X_{ij} + e_{ijt} \qquad (5)
\end{aligned}
$$

where w_{ij} is the continuous or binary characteristic along which we want to test heterogeneous impacts. The test for heterogeneous treatment effects along w_{ij} is whether we can reject $(\beta_{vw} = \beta_v, \gamma_{hw} = \gamma_h, \lambda_{hvw} = \lambda_{hv}) \; \forall \; (h, v)$.

5 Adverse selection

Our first task is to test for adverse selection into insurance. Tests for such selection can be categorized into two types. One type, which we label "enrollment tests," examines whether people who enroll have a high propensity to use insurance. Propensity to use may be measured by health status or prior utilization. The other type, which we label "utilization tests", looks at whether individuals who have enrolled at higher prices or in more generous plans have higher unit costs. Measures of unit costs may be quantity of use or expenditure via insurance.

5.1 Enrollment tests

5.1.1 Uptake of insurance

To provide some background to enrollment tests for adverse selection, Table 3 shows how enrollment varies across different access arms. Enrollment rates were 78.3% for households with free insurance (group A), 71.6% for households sold insurance but given an unconditional cash transfer (group B), and 58.8% for households sold insurance (Group C).[19]

[19] The enrollment rates among our sample were higher than is found for regular RSBY among the BPL population in Karnataka (60%). This is probably not because our sample is APL while the regular RSBY-eligible population is BPL: enrollment rates do not vary by monthly expenditures or asset levels (Appendix Table XYZ). Instead, it is likely because our study employed a more extensive enrollment drive than RSBY typically does. We went door to door to inform households that they were eligible to enroll and when the mobile enrollment truck visited their village.

Table 3: Enrollment.

Free RSBY (A)	0.783***
	(0.008)
Sale of RSBY + Cash (B)	0.716***
	(0.012)
Sale of RSBY (C)	0.588***
	(0.014)
A = C (P-value)	0.000
B = C (P-value)	0.000
Mean enrollment	0.576
SD of enrollment	0.494
N	10879

All models estimated with OLS. Standard errors were clustered at the village level. Significance levels: * 10%, ** 5%, *** 1%.

We can draw a few conclusions from these enrollment rates. First, the demand curve for insurance is downward sloping: uptake is approximately 20 percentage points higher when insurance is free (group A) than when it is sold at the unsubsidized price (group C), a difference which is highly statistically significant. The implied price elasticity for insurance is -0.33 with linear demand. This is in the mid-range of estimates from the health insurance literature, e.g., Pendzialek et al. (2016), Gruber and Lettau (2004).

Second, there is significant demand for insurance at positive, and indeed unsubsidized, prices. This result contrasts with studies that have found that, at positive prices, demand for insurance (health, weather, etc.) and other preventative/*ex ante* health investments falls to extremely low levels (Cohen et al., 2010). One reason may be that our sample is above the poverty line and thus wealthy. However, our sample is just above the poverty line. The poverty line in Karnataka in 2012 was an annual income of INR 54,120 ($3,089.04) in rural and INR 65,340 ($3,729.45) in urban areas for a family of 5 (Prabhavathi and Naveena, 2014). Sample households' average annual budget plus medical expenditures was INR 88,800 ($5,068.49). Moreover, as Appendix Table **??** shows, enrollment rates do not rise with baseline non-medical consumption expenditure.

Third, liquidity constraints may affect uptake. Uptake of insurance is approximately 13 percentage points higher when households who must pay to get insurance receive an unconditional cash

Table 4: Enrollment. Treatment interacted with health risk variables.

	Coefficient (SE)	**Coefficient (SE)**	**Coefficient (SE)**
Access to RSBY (A/B/C)	0.724***	0.737***	0.713***
	(0.008)	(0.008)	(0.013)
Access × health risk	0.005	-0.007	0.029**
	(0.006)	(0.014)	(0.014)
Mean enrollment	0.582	0.590	0.590
SD of enrollment	0.493	0.492	0.492
N	9503	9179	9175
Health risk variable	Medical expend.	Bad or very bad health (male OR female resp.)	Visited med. fac. 1yr. (male AND female resp.)
Mean of health risk var	0.381	0.180	0.770

All models estimated with OLS. Standard errors were clustered at the village level. Significance levels: * 10%, ** 5%, * 10%.

transfer (group B) then when they do not receive an unconditional cash transfer (group C). This difference is highly statistically significant. These findings are consistent with Casaburi and Willis (2018) and Berkouwer and Dean (2019) who find that, in the cases of crop insurance and efficient cookstoves, respectively, demand is significantly higher when liquidity constraints are alleviated.

5.1.2 Adverse selection

Enrollment tests for adverse selection check whether those who are more likely to experience covered health events are more likely to enroll in RSBY. We consider two such tests.

Leveraging hassle costs. Given that even those signing up for free insurance (group A) must pay the "hassle" cost of completing the sign-up process, we expect that those with higher propensity to use insurance would be more likely to enroll in any arm (A, B, or C). Therefore, our first test interacts insurance access with measures of risk. Specifically, we employ our entire sample and regress enrollment on whether a household was give insurance access (via arms A, B or C) and the interaction between access and measures of risk.[20]

[20]These regressions do not include a main effect of risk because enrollment is zero in the control group (D).

Column 1 of Table 4 examines whether those with higher baseline medical expenditure (a proxy for health risk) are more likely to select into insurance. The interaction of RSBY access and baseline medical expenditure is positive, but very small in magnitude (and not significantly different from zero): the average level of baseline medical spending is Rs. 38,100 which corresponds to a mean of .381 for the medical expenditure variable in units of 100,000 Rs.; the 25th percentile of the medical expenditure variable is .075 and the 75th percentile is .396. So, using the estimated coefficient, moving a household from the 25th to the 75th percentile of baseline medical expenditure would only increase their propensity to enroll in RSBY by .00167, or less than a fifth of one percentage point. Using the upper end of the 90% confidence interval, we can rule out with 90% confidence a differential enrollment effect of more than 0.0049 (0.49 percentage points) associated with moving from the 25th to the 75th percentile of baseline medical expenditure.[21]

The remaining columns of Table 4 use different proxies for health risk: namely, a member of the household reported that they were in bad health at baseline (column 2) or that a member of the household had visited a medical facility in the past year (column 3). There is close to no differential selection into insurance by health rating of the male or female respondent in a household. There is is a significant difference in enrollment in households where both the male and female respondents have visited a medical facility, broadly defined, in the last year. However, the magnitude of the different is small: 4% of enrollment[22] in households with low risk.[23]

Grouping the free insurance (A) and sale of insurance (B, C) arms blends the effect of hassle costs and premium on enrollment. To focus just on hassle costs, we estimate the model on a sample of households in the free insurance (A) and control groups (D) only. The results, reported in Table 5 suggest a significant impact of risk as measured by facility visits. However, the magnitude of the effect is just 4.7% of enrollment in the free insurance arm.[24]

Leveraging premiums. Our second enrollment test is a more standard check for adverse selection. Table 6 examines whether higher risk households are more likely to purchase RSBY at higher prices (in our case, at a positive price in group C vs. at a zero price in group A). Specifically, we use a sample that includes only those in group A and C and regress enrollment on assignment

[21] That is, $(.396 - .075) \times (.00517 + 1.645 * 0.0062)$.

[22] 0.029/0.713.

[23] Although we measure health risk by bad health of male *or* female respondent, we measure risk with facility visits by males *and* females. The reason for the difference is that nearly all households have a facility visit if we look at male or female respondents, meaning it would not be a way to distinguish high and low risk households.

[24] 0.036/0.767.

Table 5: Enrollment. Treatment interacted with health risk variables.

	Coefficient (SE)	**Coefficient (SE)**	**Coefficient (SE)**
Free RSBY (A)	0.786***	0.795***	0.767***
	(0.009)	(0.010)	(0.016)
Free RSBY × health risk	0.004	-0.006	0.036**
	(0.008)	(0.018)	(0.018)
Mean enrollment	0.530	0.532	0.533
SD of enrollment	0.499	0.499	0.499
N	5731	5506	5497
Health risk variable	Medical expend.	Bad or very bad health (male OR female resp.)	Visited med. fac. 1yr. (male AND female resp.)
Mean of health risk var	0.368	0.181	0.770

All models estimated with OLS. Standard errors were clustered at the village level. Only households in treatment groups A and C are included. Significance levels: * 10%, ** 5%, * 10%.

to group C, a measure of health risk, and an interaction. We do not find evidence of differential enrollment high premium group C by risk, regardless of our measure of risk. For each of our three measures of risk (baseline medical spending, being in bad health at baseline, visiting a facility the year prior to baseline), the coefficient on C × risk is small, insignificant, and not significantly different from the coefficient on C × risk. We do find that the main effect of risk is significant when risk is measured by whether both male and female respondents visited a facility in the year prior to baseline. Household with more visits were 20.7% more likely to enroll in both group A and C.[25] But this is unaffected by price, and thus does not pass this second direct test of adverse selection.

5.2 Utilization tests

Utilization tests examine whether individuals who enrolled in RSBY at a higher premium utilized insurance at a higher level than those who paid a lower price.[26]

[25] 0.036/0.174.

[26] We examine use of insurance rather than utilization of care because we find no effect of enrollment on utilization, except for outpatient surgeries at 4 years (Malani et al. 2020). We did, however, find an increase in insurance use at 18 months and 4 years. A plausible explanation is that insurance led individuals to substitute insurance for

Table 6: Enrollment. Treatment interacted with health risk variables.

	Coefficient (SE)	**Coefficient (SE)**	**Coefficient (SE)**
Sale of RSBY (C)	-0.187***	-0.180***	-0.174***
	(0.016)	(0.016)	(0.030)
Health risk	0.004	-0.006	0.036**
	(0.008)	(0.018)	(0.018)
Sale × health risk	-0.003	0.005	-0.008
	(0.012)	(0.036)	(0.033)
Mean enrollment	0.726	0.736	0.735
SD of enrollment	0.446	0.441	0.441
N	5711	5483	5486
Health risk variable	Medical expend.	Bad or very bad health (male OR female resp.)	Visited med. fac. 1yr. (male AND female resp.)
Mean of health risk var	0.383	0.178	0.770

All models estimated with OLS. Standard errors were clustered at the village level. Only households in treatment groups A and C are included. Significance levels: * 10%, ** 5%, * 10%.

Our primary tests examines a subsample of people who enrolled and asks whether those who had to pay a higher premium had a higher utilization. Because only households in groups A, B, and C could enroll, that means the subsample excludes any households in the no intervention group (D). Specifically, we regress utilization on whether a household was randomized to group B or group C, each of which paid a positive premium. We employ 3 measures of insurance use: successful use of insurance in the last 6 months as measured at midline (18 months after treatment), the same at endline (4 years after), and successful use for the last serious health event at endline.[27]

out-of-pocket payments, but did not lead to increased consumption of care. That interpretation is compatible with other RCTs of insurance in lower and middle income countries(Thornton et al., 2010; King et al., 2009; Levine et al., 2016).

[27]We define a serious health event as (a) childbirth, an accident, or a limitation of multiple activities of daily living that (b) required that a household member missed 2 days of school or work. We specify successful use because some individuals attempted to use their RSBY insurance card but were unable to, e.g., because they forgot it at home or the treatment was not covered. Malani et al. (2020).

Table 7: Utilization among enrolled.

	Successful use at 18 mo. (past 6 mo.)	Successful use at 4 yrs (past 6 mos.)	Successful use at 4 yrs (most serious event)
	Coefficient (SE)	**Coefficient (SE)**	**Coefficient (SE)**
Sale + cash (B)	0.0011	0.0040	0.0005
	(0.0086)	(0.0043)	(0.0029)
Sale (C)	0.0170*	-0.0003	0.0024
	(0.0097)	(0.0039)	(0.0032)
Mean in control	0.039	0.003	0.001
SD in control	0.193	0.057	0.033
N	5891	5583	5576

All models estimated with OLS. Standard errors were clustered at the village level. 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 one that led to the longest hospital stay.

Table 7 presents the results. We find that there was significantly (at the 10% level) higher utilization of insurance at 18 months among those who had to pay a positive premium but did not receive a subsidy (C). Utilization in this group is 43.6% higher than utilization in the comparison group (A)[28]. We do not find higher utilization at 18 months among those who received a unconditional cash transfer, even though they had to pay a higher price. This may be because they viewed the transfer as conditional even though we clarified it was not so. We do not find a significant effect on utilization at 4 years. However, enrollment had a much smaller effect on insurance utilization at 4 years.

We conduct one other version of our utilization test.[29] In one, we examined a subsample that

[28]0.017/0.039.

[29]In our next draft, we will have another test that will examine whether there was a difference in utilization of care within the free insurance group (A) between those who would have enrolled in RSBY had they been assigned to group C and those who would not have enrolled if they had been assigned to group C. We implement this in three steps. First, we construct a prediction model for who, within group C, enrolled in RSBY. Our predictors are drawn from a large set of baseline covariates and selected via LASSO. Second, we apply the prediction model to the sample of households in arm A and construct a "propensity to enroll at a positive price" measure. Third, we regress

Table 8: Utilization among those enrolled, by insurance arm to which they were assigned.

	Successful use at 18 mo. (past 6 mo.)	**Successful use at 4 yrs (past 6 mo.)**	**Successful use at 4 yrs (most serious event)**
	Coefficient (SE)	**Coefficient (SE)**	**Coefficient (SE)**
Enrolled via free RSBY (A)	0.0126	0.0092***	0.0068***
	(0.0090)	(0.0027)	(0.0018)
Enrolled via sale of RSBY (C)	0.0324***	0.0117***	0.0090***
	(0.0120)	(0.0041)	(0.0032)
A = C (p-value)	0.079	0.563	0.510
Mean in unenrolled	0.037	0.005	0.001
SD in unenrolled	0.190	0.069	0.039
N	7945	7567	7560

All models estimated with OLS. Standard errors were clustered at the village level. 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 one that led to the longest hospital stay.

was assigned to groups A, C, or D[30] and regressed utilization on whether a household enrolled, whether the household was assigned to the sale of insurance group (C), and the interaction. Because enrollment and the interaction is endogenous, we instrument for enrollment with household group assignment and examine CATE estimates. The advantage of this test is a larger sample; the disadvantage is greater structure. The net effect on power is unclear. However, the results, reported in Table 8, are very similar to those in Table 7.

utilization in group A on this propensity score. If there is adverse selection, there should be a positive coefficient on this propensity score. This test can also be applied to group B. This test has trade-offs similar to the test in Appendix Table 8: a larger sample but more statistical structure.

[30]We omit the sale plus cash group (B) because it is unclear whether they viewed the cash as conditional or not. As a result, we cannot be sure they perceived that there was a positive price for insurance.

6 Reconciling enrollment and utilization tests

While enrollment tests suggest no adverse selection, our utilization test – at least at the 18 month follow-up – suggests there may be adverse selection. Here we use a different literature to make sense of these results and draw conclusions about adverse selection.

Within development economics, it has occasionally been observed that people who pay a higher price for a product often utilize a product more (Ashraf et al., 2010). There are three theories for why this may be so. First, there may be selection: individuals who are more likely to use the product are more likely to buy it. This theory overlaps with the the theory of adverse selection in health or information economics. Second, the price of product signals it quality. Because individuals are more likely to use a product that they think is higher quality, higher price may lead to more use. Third, individuals may suffer a sunk cost fallacy: the more they pay the more they feel they must use the product. This explanation is occasionally called a psychological commitment theory.

Given this framing, the analysis in Section 5 suggest that there is not adverse selection. The positive utilization test is consistent with any of the three theories above. The enrollment test is the only one that discriminates between the three theories, and it rejects adverse selection.

In this section we test the second theory: price signals quality. Few studies have tested this theory. It is difficult to vary the price a person pays, and thus the signal, without also affecting selection or triggering a sunk cost fallacy. Prior studies have tested the theory by examining whether the price of a good today affects perceptions of quality, as measured by willingness to pay for the good, at some future date. They have not, however, found support for the theory (Cohen et al., 2010).

Our study permits us to test the price-signal theory in a different way. We will use the price that a household's neighbors pay, holding constant the price the household paid, to test the theory. Neighbor's price will serve as a price signal and household's utilization will serve as our measure of a household's perceptions of quality. Our study permits us to vary neighbor's price because we varied the fraction of the sample in each village that receives insurance for at full price.

Specifically, we start with a sample of households enrolled in insurance and regress utilization on an indicator of what price neighbors paid. Our indicator for the price that neighbors paid is the relative share of share of households given access to insurance who received access at full price.

To begin, this share is taken to be the share of a villages households in group C divided by the

Table 9: Utilization by price paid by neighbors.

	Successful use at 18 monthhs (past 6 mos.)	**Successful use at 4 years (past 6 mos.)**	**Successful use at 4 years (most serious event)**
	Coefficient (SE)	**Coefficient (SE)**	**Coefficient (SE)**
Relative share in C	-0.0045	-0.0040	-0.0049
	(0.0318)	(0.0087)	(0.0063)
Mean in control	0.039	0.003	0.001
SD in control	0.193	0.057	0.033
N	5891	5583	5576

All models estimated with OLS. Standard errors were clustered at the village level. 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 one that led to the longest hospital stay.

share in arms A, B, and C:

$$RS_C = \frac{S_C}{S_A + S_B + S_C} \tag{6}$$

We omit group D on the logic that the price signal a household receives from neighbors is only a function of households offered insurance. Those not offered insurance convey no information about price. We do not include group B because the price signal they send is unclear. While they full price, they are also offered an unconditional cash transfer that some may perceive as implicitly a conditional subsidy. To check this assumption, we estimate a version of the model where the share in B is added to the numerator of (6). We again find insignificant results.

The resulting ITT estimates, presented in Table 9, suggest that the relative share in arm C does not significantly affect utilization. This is true for insurance use at 18 months and at 4 years. This is arguably a precise zero at 18 months, where the coefficient is just 11% of mean utilization. But across all outcomes, the sign on our indicator of price is negative, the opposite of what the price-signals-quality literature suggests.

The regression above only uses data on enrolled households. We try an alternative strategy that expands the sample to include those who have been given access to insurance, but may or may not have enrolled. We then regress utilization on enrollment, an indicator for what price neighbors

Table 10: Utilization by enrollment and neighbor's price.

	Successful use at 18 monthhs (past 6 mos.)	**Successful use at 4 years (past 6 mos.)**	**Successful use at 4 years (most serious event)**
	Coefficient (SE)	**Coefficient (SE)**	**Coefficient (SE)**
Enrolled	0.0301**	0.0104**	0.0071**
	(0.0145)	(0.0047)	(0.0031)
Rel. share in C	0.0347	0.0015	-0.0013
	(0.0432)	(0.0111)	(0.0049)
Enrolled ×	-0.0592	0.0009	-0.0018
rel. share in C	(0.0553)	(0.0174)	(0.0082)
Mean in unenrolled	0.039	0.005	0.001
SD in unenrolled	0.193	0.070	0.036
N	9960	9458	9450

All models estimated with 2SLS. Standard errors were clustered at the village level. 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 one that led to the longest hospital stay.

paid, and the interaction of the last two. Our indicator for the price that neighbors paid remains the relative share of share of household given access who received access at full price. Because enrollment and the interaction of enrollment with relative share are endogenous, we instrument by the insurance access arm to which households are randomized and the allocation arm to which their village is assigned. Given that we have to use IV estimation, it is not obvious that this approach will have more power than the ITT analysis on only the enrolled sample.

Table 10 presents our CATE estimates. We find that that an increase in neighbors price has an insignificant effect on utilization. Moreover, it remains negative in two specifications. These results are consistent with the previous ITT results.

7 Conclusion

We have cast doubt on cast doubt on adverse selection and price signalling quality. That leaves sunk cost or psychological commitment – or any other theory that may not have been proposed yet. Ashraf et al. (2010) vary offer price and transaction price for a water purification product to separate the screening effect of price from psychological effects of the amount paid and find no evidence for the sunk costs theory.[31] Unfortunately, we are unable to test that theory in our context.

Instead we focus here on why we fail to find evidence of adverse selection, especially since other studies have found such selection with health insurance in a LMIC country(Banerjee et al., 2019).[32] One possibility is that this finding reflects a true null. In a context where the exposure to health risk is arguably very high for all households, and the price of the insurance is relatively low, it may be unsurprising that even households with low (forecastable) risk nonetheless see value in the insurance. Just as there cannot be adverse selection if there is no demand, as noted by Banerjee et al. (2014a), there likewise cannot be (much) adverse selection when demand is very high. Of course, demand among the paying group is well below 100%, but this may reflect frictions rather than low underlying willingness to pay, as suggested by the approximately 13 percentage point increase in takeup when households who must pay to get insurance receive an unconditional cash transfer (group B vs. group C).

However, there are other possible explanations. Because the households in our sample are being offered the chance to sign up for RSBY for the first time, perhaps they were relatively unfamiliar with the details of its coverage and thus unsure what their willingness to pay should be. In a previous paper, we documented that many households who enrolled in RSBY reported an inability to use the insurance because they did not know how to use insurance, what was covered or where to obtain care (Malani et al 2020). This would dampen the potential for adverse selection.

Another possibility, noted by Jack et al. (2016) in the context of adverse selection into uncollateralized vs. uncollateralized loans, arises from the fact that any household whose risk type would make it optimal for them to take up insurance at a positive price (group C) would also optimally take up insurance for free (group A). Therefore, the characteristics of the two groups will overlap to a large extent even if some degree of adverse selection is present, which makes it more statistically

[31] But see Arkes and Blumer (1985), who find a sunk cost effect with theater tickets.

[32] An exception is Banerjee et al. (2014b), which find no adverse selection because there was no demand for health insurance in an experiment that encouraged health insurance in India by bundling it with microfinance loans.

challenging to detect adverse selection.

A final possibility is, of course, that our proxies for health risk are imperfect; however, the fact that all three measures yield similar conclusions gives some reassurance that the finding is not driven by one poor measure of health risk.[33]

[33] In a future draft we will report the results of an analysis that attempts to use machine learning to identify health factors that may predict enrollment.

References

Agha, S., Van Rossem, R., Stallworthy, G., and Kusanthan, T. (2007). The impact of a hybrid social marketing intervention on inequities in access, ownership and use of insecticide-treated nets. *Malaria Journal*, 6(1):13.

Akerlof, G. A. (1970). The market for" lemons": Quality uncertainty and the market mechanism. *The Quarterly Journal of Economics*, 84(3):488–500.

Alatas, V., Purnamasari, R., Wai-Poi, M., Banerjee, A., Olken, B. A., and Hanna, R. (2016). Self-targeting: Evidence from a field experiment in Indonesia. *Journal of Political Economy*, 124(2):371–427.

Arkes, H. R. and Blumer, C. (1985). The psychology of sunk cost. *Organizational Behavior and Human Decision Processes*, 35(1):124–140.

Ashraf, N., Berry, J., and Shapiro, J. M. (2010). Can higher prices stimulate product use? evidence from a field experiment in zambia. *The American Economic Review*, 100(5):2383–2413.

Asuming, P., Kim, H. B., Sim, A., et al. (2019). Long-run consequences of health insurance promotion when mandates are not enforceable: Evidence from a field experiment in Ghana. Technical report.

Banerjee, A., Duflo, E., and Hornbeck, R. (2014a). Bundling health insurance and microfinance in India: There cannot be adverse selection if there is no demand. *American Economic Review*, 104(5):291–97.

Banerjee, A., Duflo, E., and Hornbeck, R. (2014b). Bundling health insurance and microfinance in india: There cannot be adverse selection if there is no demand¡/p¿. *American Economic Review: Papers and Proceedings*.

Banerjee, A., Finkelstein, A., Hanna, R., Olken, B. A., Ornaghi, A., and Sumarto, S. (2019). The challenges of universal health insurance in developing countries: Evidence from a large-scale randomized experiment in Indonesia. Technical report, National Bureau of Economic Research.

Berkouwer, S. B. and Dean, J. T. (2019). Credit and attention in the adoption of profitable energy efficient technologies in Kenya.

Berman, P., Ahuja, R., and Bhandari, L. (2010). The impoverishing effect of healthcare payments in India: new methodology and findings. *Economic and Political Weekly*, pages 65–71.

Casaburi, L. and Willis, J. (2018). Time versus state in insurance: Experimental evidence from contract farming in Kenya. *American Economic Review*, 108(12):3778–3813.

Chiappori, P.-A. and Salanié, B. (2013). *Asymmetric Information in Insurance Markets: Predictions and Tests*, pages 397–422. Springer New York, New York, NY.

Cohen, J., Dupas, P., et al. (2010). Free distribution or cost-sharing? evidence from a randomized malaria prevention experiment. *Quarterly journal of Economics*, 125(1):1.

Comptroller and Auditor General of India (2019). Hospital management in Uttar Pradesh for the year ended 31 march 2018. Report.

Das, J. and Leino, J. (2011). Evaluating the RSBY: lessons from an experimental information campaign. *Economic and Political Weekly*, pages 85–93.

Duflo, E., Dupas, P., Kremer, M., and Sinei, S. (2006). *Education and HIV/AIDS prevention: evidence from a randomized evaluation in Western Kenya.* The World Bank.

Dupas, P. (2014). Short-run subsidies and long-run adoption of new health products: Evidence from a field experiment. *Econometrica*, 82(1):197–228.

Einav, L. and Finkelstein, A. (2011). Selection in insurance markets: Theory and empirics in pictures. *Journal of Economic Perspectives*, 25(1):115–38.

Evans, D., Kremer, M., and Ngatia, M. (2008). *The impact of distributing school uniforms on children's education in Kenya.* World Bank.

Fischer, G., Karlan, D., McConnell, M., and Raffler, P. (2019). Short-term subsidies and seller type: A health products experiment in uganda. *Journal of Development Economics*, 137:110–124.

Fischer, T., Frölich, M., and Landmann, A. (2018). Adverse selection in low-income health insurance markets: Evidence from a RCT in Pakistan.

Gruber, J. and Lettau, M. (2004). How elastic is the firm's demand for health insurance? *Journal of Public Economics*, 88(7-8):1273–1293.

Gupta, I. and Chowdhury, S. (2014). Public financing for health coverage in India. *Economic Political Weekly*, 49(35).

Hoffmann, V. (2009). Intrahousehold allocation of free and purchased mosquito nets. *American Economic Review: Papers and Proceedings*, 99(2):236–41.

Jack, W., Kremer, M., De Laat, J., and Suri, T. (2016). Borrowing requirements, credit access, and adverse selection: Evidence from Kenya. Technical report, National Bureau of Economic Research.

King, G., Gakidou, E., Imai, K., Lakin, J., Moore, R. T., Nall, C., Ravishankar, N., Vargas, M., Téllez-Rojo, M. M., Ávila, J. E. H., et al. (2009). Public policy for the poor? a randomised assessment of the Mexican universal health insurance programme. *The Lancet*, 373(9673):1447–1454.

Kremer, M. and Holla, A. (2009). Pricing and access: lessons from randomized evaluations in education and health. *What Works in Development: Thinking Big and Thinking Small*, pages 91–129.

Kremer, M. and Miguel, E. (2007). The illusion of sustainability. *The Quarterly journal of economics*, 122(3):1007–1065.

Kremer, M., Miguel, E., Mullainathan, S., Null, C., and Zwane, A. P. (2011). Social engineering: Evidence from a suite of take-up experiments in kenya. *Work. Pap., Univ. Calif., Berkeley.*

Lagomarsino, G., Garabrant, A., Adyas, A., Muga, R., and Otoo, N. (2012). Moving towards universal health coverage: health insurance reforms in nine developing countries in africa and asia. *The Lancet*, 380(9845):933–943.

Levine, D., Polimeni, R., and Ramage, I. (2016). Insuring health or insuring wealth? an experimental evaluation of health insurance in rural Cambodia. *Journal of Development Economics*, 119:1–15.

Ma, S. and Sood, N. (2008). *A comparison of the health systems in China and India.* RAND, Santa Monica, CA.

Ministry of Health and Family Welfare (2014). National health policy 2015 draft.

Pendzialek, J. B., Simic, D., and Stock, S. (2016). Differences in price elasticities of demand for health insurance: a systematic review. *The European Journal of Health Economics*, 17(1):5–21.

Prabhavathi, P. and Naveena, N. (2014). An analysis of poverty in karnataka: A study. *IOSR Journal Of Humanities And Social Science*, 19(3):72–31.

Shahrawat, R. and Rao, K. D. (2011). Insured yet vulnerable: out-of-pocket payments and India's poor. *Health policy and planning*, 27(3):213–221.

Tarozzi, A., Mahajan, A., Blackburn, B., Kopf, D., Krishnan, L., and Yoong, J. (2014). Micro-loans, insecticide-treated bednets, and malaria: Evidence from a randomized controlled trial in orissa, india. *American Economic Review*, 104(7):1909–41.

Thornton, R. L., Hatt, L. E., Field, E. M., Islam, M., Solís Diaz, F., and González, M. A. (2010). Social security health insurance for the informal sector in Nicaragua: a randomized evaluation. *Health Economics*, 19(S1):181–206.

8 Tables

A Description of surveys

A.1 Baseline Survey

The baseline survey took place in August 2013 – February 2014. This round included a consent form for the overall study as well as the survey. We administered surveys to up to 3 distinct members of each household (the female and male most knowledgeable about household finances and a female of childbearing age) for the entire sample. These individuals were asked modules about subjective health status, health care consumption and financial status. In addition, for a subsample of roughly 4000 households, we also conducted an anthropometric survey that gathered objective health status (e.g., BP, body fat, weight, lung capacity), on up to 3 members of the household (the male most knowledgeable about household affairs, a female with childbearing capacity, and a child under the age of 5. Households were paid INR 250 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*.

A.2 Midline Survey

In November 2016 – February 2017, we conducted an 18 month follow-up survey. The format was nearly identical to the baseline survey, including the anthropometric survey. Households were paid INR 250 as a participation incentive for completing major sections of the survey. We performed backchecks on approximately 15% of households,[34] a rate known to surveyors *ex ante*.

A.3 Endline Survey

In March - May 2019, we conducted a 4 year follow-up survey. We surveyed 1 member (our first priority was to interview the female most knowledgeable from baseline (or midline for households with missing baseline data), followed by the current female most knowledgeable, and then the male most knowledgeable) of each household in the sample. Respondents were asked about subjective health status, health care consumption and financial status. Households were paid a participation incentive comprised of bars of soap and tubes of toothpaste valued at approximately INR 50 for completing major sections of the survey. We performed backchecks on 10% of households, a rate known to surveyors *ex ante*.

[34]The actual rate is 4 households per village on each module of the the survey.

B Treatment Delivery

Sample households were randomized in August 2014 using data from the listing exercise. They were informed of their treatment assignment and, depending on which arm to which they are assigned, given access to insurance or cash, all in May and June 2015. This step took place roughly 18 months after the baseline due to government constraints. Due to challenges associated with evaluating a government program, we were only able to enroll sample households when the government held its RSBY enrollment drive in early 2015.

We enrolled study households much in the same way the government enrolled RSBY (non-study) eligible households into the program: via a mobile enrollment truck that visited each village. One difference is that we went to each household to ask them if they would like to enroll and took them to the enrollment truck, which was parked in one place in the village. By contrast, the government informed non-study households by delivering paper notices (chits) to each home about when and where the enrollment truck would be in the village.

For all study households that take up insurance, we paid the government their premiums. For households in arms A, this was funded by grant funds. For those in B and C that purchase insurance, this was funded by the respondents' money.

The Karnataka government renewed RSBY automatically for non-study and study households in 2016 and 2017 without any action—even payment—by enrollees. RSBY ended in the state on August 31, 2018, to make way for the new NHPS scheme, which was scheduled to start in late 2018 or early 2019. As of this writing, Karnataka had agreed to roll out NHPS but had not begun the roll-out.

C Supplementary Tables and Figures

C.1 Enrollment by risk aversion

We examine whether the decision to enroll in RSBY by treated households is related to a proxy for risk aversion. In the baseline survey, male respondents were given a set of hypothetical choices between an amount x and a lottery that would provide 50 INR or 350 INR with equal probability. The offered amount x increased sequentially from 40 INR (a dominated answer for monotonic preferences) to 80 INR, then to 120 INR, then to 160 INR and finally to 220 INR, which corresponds to risk-loving preferences.

Table 11 reports the distribution of choices: a large fraction (almost half of the sample) of households chose the the dominated option, and another significant fraction (29%) refused an amount greater than the expected value of the lottery (220 INR as opposed to the expected value of 200 INR) to pursue the lottery itself. This pattern of answers suggests that a large proportion of households may not have interpreted the questions correctly, and hence indicates that any finding that uses such answers need to be interpreted with caution. Indeed, we find that choosing the dominated or the risk-loving option is negatively correlated with a cognitive ability measure (the Raven score).

Table 11: Certain Equivalent by treatment group (Percentage)

certain_equiv	Treatment				
	A	B	C	D	Total
40	46.83%	44.22%	48.28%	42.75%	45.79%
80	6.48%	9.22%	8.20%	7.34%	7.54%
120	8.16%	7.89%	7.60%	7.16%	7.79%
160	6.29%	6.10%	5.92%	6.32%	6.19%
220	4.16%	4.08%	3.59%	4.00%	4.0%
999	28.08%	28.49%	26.42%	32.43%	28.70%
Total	100.00%	100.00%	100.00%	100.00%	100.00%

Table 12 shows that enrollment rates in RSBY are comparable across certain equivalent choices between 40 and 220, at around 60%. Households of respondents who report a risk loving option have lower enrollment rates (53%). This finding is confirmed in our regression tables 13 and 14.

Table 12: Enrollment rate by Certain Equivalent

Choice	Fraction Enrolled in RSBY		
	Mean	Std. Dev.	Freq.
40	60%	0.49	4,981
80	60%	0.49	820
120	60%	0.49	848
160	58%	0.49	673
220	60%	0.49	435
Lottery always	53%	0.50	3,122
Total	0.58	0.49	10,879

Table 13: Enrollment, Treatment interacted with Risk Aversion

	(1) Enrollment	(2) Enrollment	(3) Enrollment	(4) Enrollment
Access to RSBY	0.69***	0.69***	0.69***	0.69***
	(0.011)	(0.011)	(0.011)	(0.011)
Access to RSBY	0.038***	0.041**	0.046***	0.051***
× Risk aversion	(0.012)	(0.017)	(0.013)	(0.016)
Mean enrollment	0.576	0.559	0.576	0.559
SD of enrollment	0.494	0.497	0.494	0.497
N	10879	5898	10879	5898
Risk aversion variable	CE <= 120	CE <= 120	CE <= 220	CE <= 220
CE=40 Dropped	NO	YES	NO	YES

.9 All models estimated with OLS. Standard errors were clustered at the village level. Significance levels: *** 1%, ** 5%, * 10%.

Table 14: Enrollment, Treatment interacted with Risk Aversion

	(1) Enrollment	(2) Enrollment	(3) Enrollment	(4) Enrollment
Free (A)	0.76***	0.76***	0.75***	0.75***
	(0.013)	(0.013)	(0.014)	(0.014)
Pay + Cash (B)	0.70***	0.70***	0.70***	0.70***
	(0.019)	(0.019)	(0.020)	(0.020)
Pay (C)	0.55***	0.55***	0.54***	0.54***
	(0.021)	(0.021)	(0.021)	(0.021)
A × Risk aversion	0.043***	0.060***	0.051***	0.066***
	(0.015)	(0.021)	(0.015)	(0.019)
B × Risk aversion	0.022	0.014	0.023	0.016
	(0.022)	(0.029)	(0.023)	(0.031)
C × Risk aversion	0.059**	0.055	0.070***	0.070**
	(0.025)	(0.033)	(0.024)	(0.033)
A × Risk Aversion = C × Risk Aversion	0.574	0.899	0.484	0.909
B × Risk Aversion = C × Risk Aversion	0.233	0.322	0.130	0.192
Mean enrollment	0.576	0.559	0.576	0.559
SD of enrollment	0.494	0.497	0.494	0.497
N	10879	5898	10879	5898
Risk aversion variable	CE <= 120	CE <= 120	CE <= 220	CE <= 220
CE=40 Dropped	NO	YES	NO	YES

.9 All models estimated with OLS. Standard errors were clustered at the village level. Significance levels: *** 1%, ** 5%, * 10%.