Pre-Analysis Plan for Endline Data in the Indian Health Insurance Experiment (amended)

Note: This version amends the original version, posted March 14, 2019. The original, time-stamped version is available at https://www.socialscienceregistry.org/trials/1793.

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Abstract

We present the analysis plan for the Indian Health Insurance Experiment (IHIE). The IHIE is a randomized controlled trial examining the impacts of expanding Rastriya Swasthya Bima Yojana (RSBY), India's first large, national public health insurance program. RSBY is an insurance plan that covers INR 30,000 of hospital expenses per year for a family of up to 5 individuals. Begun in 2013, the IHIE randomized roughly 11,000 above poverty line (APL) households in two districts of Karnataka into four treatment arms that provided different levels of access to RSBY. The four arms were: (A) free 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. The study also randomized the fraction of villages within each arm to address spillover effects. We examine impacts on insurance uptake, utilization, finances (including out-of-pocket expenses), and health. We present the questions we address, the outcomes of interest, and our econometric strategy.

Contents

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1 Introduction

Policy context. Each year, some 150 million people worldwide face financial catastrophe due to spending on health. According to a 2010 study, more than one third of them live in India (Shahrawat and Rao, 2011). The number of Indians falling below the poverty line (BPL) due to health spending may run as high as 63 million people, almost 7% of the nation's population (Berman et al., 2010). In recent years, many countries have moved towards Universal Health Coverage (UHC) with various degree of success. In particular, a growing number of low and middle-income countries (LMICs) have rolled out various publicly-funded health insurance (PFHI) schemes. In 2008, the Ministry of Labour and Employment (MoLE) in India initiated RSBY (Rastriya Swasthya Bima Yojana), one of the largest PFHI schemes in the world today, focused on inpatient care. Its aim was to provide coverage for hospitalization to the poor "Below the Poverty Line" (BPL) population. The last official figures report that out of 59,117,989 BPL families, 36,332,475 are enrolled.

RSBY just assumed greater significance with the government's announcement to use the program as a platform to move towards UHC. On February 1, 2018, the Union Budget announced the National Health Protection Scheme (NHPS), popularly known as "Modicare", after PM Narendra Modi. Prime Minister Modi reiterated that it was a government priority in his Indian Independence Day speech on August 15, 2018. As of November 1, 2018, 33 states have agreed to implement the scheme.

If successful, the NHPS will extend health insurance to 500 million people from financially vulnerable households, nearly one-half India's 1.2 billion population.[1] To put that in perspective, when the United States rolled out the Affordable Care Act (ACA) in 2010, the goal was to extend health care to nearly 50 million people, roughly one-sixth of its 320 million population. The success of Modicare—and other related insurance expansions—depends crucially on whether people sign up for the service, whether they use it, whether hospitals participate, and the value of medical care. Our project can help providing timely answers to many of these questions.

Our Project. The Indian Health Insurance Experiment (IHIE) is a large-scale, impact evaluation of expanding eligibility for RSBY. We also wish to estimate the health and financial impacts of health insurance in a low-income setting. Towards this end, the IHIE randomizes roughly 11,000 households (50,000 individuals) in the Indian state of Karnataka to four different methods of providing access to insurance: (A) free 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. In order to address potential spillovers between insured and uninsured households, we employ a two-stage design that randomized villages to different allocations across arms (A) – (D) before we randomize households to those four arms.

Use of this Pre-analysis Plan. This document provides a statistical analysis plan for data from the endline survey conducted roughly 4 years after treatment (and described in Section 5.3.6). The purpose is to commit to an analysis plan prior to obtaining endline data. As such, the document will be posted at the AEA trial registration site before we begin surveying sample households in the endline survey.

In the Appendix, we will explain how this Pre-Analysis Plan influences our analysis of data from two previously conducted surveys—a 12 month follow-up called the Post-Health Event Survey (PHES, Section 5.3.4) and an 18 month follow-up called the midline (Section 5.3.5).

Related Literature. There is a vast literature on evaluating health insurance expansions in developed and developing countries, using both experimental and quasi-experimental designs.

Although insurance has a myriad of effects, here we focus on two categories.[2] Insurance lowers the price of medical care on the margin. This has two effects. First, decisions to take care are inframarginal, i.e., are unchanged by the presence of insurance. For these events, insurance reduces the cost of care. This is manifest as lower out-of-pocket (OOP) spending. This can be unpacked depending on how individuals finance OOP spending; for example they could take out a loan or tap savings, they could sell an asset, or they could defer consumption. Each channel has a different welfare cost.

[1] NHPS also expands RSBY on another margin besides eligibility: it expands the services covered. Whereas RSBY only covered short term treatments at acute care facilities (secondary hospital care), NHPS will also cover longer term treatments at hospitals (tertiary care). The latter is the sort of care covered in programs such as Arogyashree Vajpayee in Andra Pradesh and Karnataka. This expansion in vertical coverage is also reflected in the annual caps for households, which will rise from the INR 30,000 under RSBY to INR 500,000 under NHPS.

[2] There are a number of other consequences of insurance. For example, if it is subsidized either by the government or via pricing regulations such as community rating, it may have so-called *ex ante* moral hazard effects, i.e., it may encourage risky health behaviors because it reduces the financial cost of sickness. Moreover, it may have spillover effects on local credit markets as it is a substitute for savings and borrowing as method of financing care.

Second, the lower price can lead to increased consumption of insured medical care, due to both substitution effects and income effects. It is possible for this care to be excessive, i.e., moral hazard, in the sense that an individual consumes more than they would were they charged the marginal cost of care. There are also income effects from insurance. If this marginal care that is purchased with insurance is productive, in the sense that it improves health care outcomes, we will see improvements in health due to insurance.

The degree to which there are effects on inframarginal and marginal consumption—and their knock-down effects—depends on a number of factors, including demand elasticity and how individuals finance medical care in the absence of the insurance being provided as treatment. For example, if individuals have informal insurance or free access to public hospitals, formal insurance or insurance that covers private providers, respectively, may yield no increase in utilization and thus health because it was merely replacing one method of insurance-like financing with another that covers private care.

Evaluating insurance is made more difficult due to selection into insurance. For example, adverse selection (advantageous selection) of higher (lower) risk individuals into insurance may cause those that are insured to appear less (more) healthy than those who are uninsured, even if insurance does not cause a reduction (an increase) in health. To address this problem, a number of experiments have been conducted that randomized individuals to access to insurance or insurance itself.

We can divide these experiments by whether they were conducted in high-income countries or not. In the U.S., two notable experiments are the RAND health insurance experiment (Newhouse, 1993) and the Oregon Health Insurance Experiment (OHIE) (Finkelstein et al., 2012). Both experiments find that there was an increase in utilization and a reduction in OOP payments due to insurance. However, RAND found that there were no significant health effects, though one could not rule out health effects for low-income populations. Likewise, OHIE found that Medicaid only had significant mental health effects (including for individuals who did not utilize care) and that one could not rule out effects on diabetes. A limitation of these studies for our research questions is that the health care and financing systems of high-income countries is different than those of low or middle-income countries. For example, the U.S. has a safety net, i.e., the Emergency Medical Treatment and Labor Act, state debtor-creditor laws, and bankruptcy, that less well-off countries may not have. This difference implies that insurance could have a larger impact in places like India. Conversely, less well-off countries may have a less developed health care infrastructure so that many individuals who are given insurance may not have access to facilities at which they can use insurance. This could lead insurance to have a lower impact in places like India.

Health insurance experiments have been conducted in China, Mexico, Cambodia, Nicaragua, Kenya, and India (reported in the next paragraph). A number have found that insurance did not increase utilization (and thereby did not impact health), but did reduce debt or catastrophic medical expenses (King et al., 2009; Levine et al., 2016; Thornton et al., 2010). This result may be due to care being inframarginal and insurance substituting one method of financing care for another. A recent experiment in Kenya, however, found no financial benefits from insurance either (Haushofer et al., 2017). That said, the study did find a "peace of mind effect" wherein individuals with health insurance experienced less stress, even if they did not use the insurance to obtain care.

Work on the impacts of health insurance in India is likewise mixed. Sood et al. (2014) evaluated the effects of a government insurance program covering tertiary care for BPL people in Karnataka (Vajpayee Arogyashree) using a geographic RD design (in 300 villages where the scheme was implemented and 272 neighboring matched villages). They found an increase in health care use, a reduction in mortality rates for covered conditions, and a decrease in OOP. Das and Leino (2011) randomized a pilot information and education campaign (IEC) in six (of 72) administrative circles in Delhi in 2008 (the year RSBY was launched), and found that the IEC campaign increased enrollment but decreased claim rates. Karan et al. (2017) used 3 waves of household level data from the National Sample Survey Organisation (NSSO) and district-level RSBY administrative data on enrollment, and found no effect on inpatient or outpatient expenditures, and a 30% increase in the likelihood of any OOP spending, using a difference-in-differences design. A recent review of the impact of publicly financed health insurance schemes (Prinja et al., 2017) shows that, while all studies reviewed find an increase in the use of health services, only a few studies (in particular those evaluating state-sponsored health insurance schemes) find a reduction in OOP expenditures, with most of them finding no impact. Lastly, Ghosh and Gupta (2017) have recently shown that almost half of the households enrolled in RSBY actually belong to the non-poor category, and that RSBY had hardly any effect on financial protection. Importantly, to date, there has been no study evaluating the health impacts of RSBY.

When evaluating the impact of health insurance, it is important to acknowledge that prior to the introduction of formal insurance, villages may have had informal insurance (Townsend, 1994) or credit

markets to help people cope with health shocks. The introduction of formal insurance may impact the value of informal insurance or affect the operation of informal credit markets. For example, formal insurance may displace informal insurance (Attanasio and Rios-Rull, 2000; Dercon and Krishnan, 2003; Lin et al., 2014), or it may complement informal local insurance if it provides protection against aggregate village-level shocks that informal insurance cannot address (Dercon et al., 2014). Likewise, formal insurance may reduce demand for credit, pushing down interest rates for the uninsured, or it may reduce precautionary savings and thus credit supply, increasing interest rates for the uninsured. These potential spillovers will affect the overall value of insurance.

2 Funding, ethical approval, and trial registration

Funding. The IHIE was funded by the following entities, with earmarked funds indicated in parentheses:

- the University of Chicago via grants from the Law School, the Becker-Friedman Institute, the MacLean Center for Bioethics, and the Neubauer Collegium (for ethnographic work);
- Northwestern University;
- Department for International Development (DFID) (UK) via a grant to the Public Health Foundation of India (PHFI) (for baseline and part of treatment);
- the International Growth Centre (IGC) (for the 12 month follow-up, called the Post-Health Event Survey); and
- the Tata Trusts via a grant to the Tata Centre for Development (TCD) at the University of Chicago (for enrollment, the midline or 18 month follow-up, and endline or 4 year follow-up).

If a source is not followed by a parenthetical about how the funds were used, the funds from the source were not earmarked for a particular use.

Ethical approval. The study received IRB clearance at the University of Chicago (IRB12-2085), Northwestern University and Tufts University (during the periods Kinnan was at each institution), Princeton University and Harvard University (during the periods Imai was at each institution), University College London, University of Pennsylvania, PHFI, and the Institute for Financial Management and Research (IFMR). Presently, University of Chicago is the IRB of Record for the IHIE.

Trial registration. The IHIE was registered with the American Economics Association (AEA) Registry (RCT ID: AEARCTR-0001793) on December 16, 2016 (before the 18 month follow-up) and at ClinicalTrials.gov (ID: NCT03144076) on May 8, 2017. We did not register prior to these dates as it was not the norm in the economics literature to register experiments prior to initiation.

3 Evaluation questions

The IHIE was designed to address the following questions.

1. What is the value of health insurance in a developing country context? Specifically, what is the value of expanding eligibility for RSBY to the non-poor?

Within this broader question we address the following specific questions:

(a) Does access to insurance or insurance itself increase utilization?

(b) How does access or insurance affect health through increased utilization?

(c) Does access or insurance affect health independent of utilization, perhaps through a "peace of mind" effect?

(d) How does access or insurance affect a household's finances, its asset portfolio, its income, its consumption or the variability of its consumption through increased utilization?

(e) Does access or insurance affect finances independent of utilization, perhaps by altering a household's portfolio and thus its income and consumption?

(f) Is there an income effect from health care or health care financing (capital) costs?

(g) Is there an income effect from health insurance premium subsidies?

When exploring these questions we address a number of other cross-cutting questions:

2. Do insurance prices affect utilization, and if so how?
3. Are there spillover effects on non-beneficiaries, or spillovers within beneficiaries?
4. Does access to insurance affect intra-household allocation of resources? How are the benefits of health insurance distributed within the household?

4 Intervention

4.1 Context

Indians have access to both government and private medical providers. The government operates a large number of facilities, from Primary Health Centres and Sub-Centres to District Hospitals. In addition, private doctors have offices and clinics, and there are private hospitals of various sizes. The government facilities largely offer free care, though they may not cover all populations and the quality has been questioned. 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. 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, there are a limited set of insurance options in India. As previously stated, treatment at government facilities is largely free. 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.

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).

4.2 RSBY

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[3] are eligible for RSBY. In addition states can expand eligibility to other groups so long as 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[4]. 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[5]. Child delivery is also included. There are no deductibles or co-pays. RSBY covers all pre-existing diseases and there is

[3] 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.

[4] 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.

[5] 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.

no age limit for beneficiaries. The rates of most surgical procedures are fixed[6]. 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.

4.2.1 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 equal to the premium. Households obtain an unconditional cash transfer equal to the RSBY premium in their district plus the right to buy RSBY in the following 3 weeks.

C. Right to purchase RSBY. Households receive the right to buy RSBY 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.

5 Evaluation Design

We carried out a randomized controlled trial (RCT) to test the impacts of the RSBY expansion to APL households.

5.1 Sampling Strategy

The inclusion criteria for a household to be eligible for the study was that, at the start of the study in 2013:

1. The household resided in Gulbarga or Mysore district;
2. The household resided 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 exclusion criteria for the study were:

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

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

The two districts we selected were representative of central and southern India, respectively. We focused on APL households because they were treatment naive: they were not otherwise eligible for RSBY. The set of all household that met these inclusion criteria was larger than the target sample size for the study. Therefore we restricted enrollment to 25 km around a subset of hospitals in each district. We also omitted villages with $\leq$ 10 eligible households as the amortized fixed costs of reaching those households were very high.

The RCT was powered to detect a 25% change in hospitalization rate across study arms, allowing for a 10% attrition rate.

5.2 Treatment assignment

We designed a two-stage randomization process to study both direct treatment and spillover effects of 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 4.2.1. 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.[7]

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

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.[8]

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. 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.

5.3 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

[7] 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 as follows:

- Mahalanobis matching on: education, age of household head, # children, # rooms in house
- 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).

[8] Specifically, within each village, we first created blocks of 10 eligible and consented households. Using household-level data from listing survey, create blocks as follows:

- Mahalanobis matching on: education, age of household head, # children, # rooms
- 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).

(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.

5.3.1 Listing

In March – June 2013 and again in November - December 2013, we conducted listing exercises to identify candidate villages and households. This round also gathered variables that we used for randomization. Only 1 adult in each household was interviewed.

5.3.2 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*.

5.3.3 Enrollment Survey

A short enrollment survey was conducted during May and June 2015 when households were told their household-level treatment assignment. All households were surveyed, although response rates were lower than other survey rounds because this enrollment survey was not prioritized during the enrollment process.

5.3.4 Post-health Event Survey (PHES)

In May and June 2016, we conducted a 12 month follow-up. We used a different survey design, the PHES. Instead of surveying all households, most of whom may not have had a sickness that required treatment covered by insurance, we only surveyed those households that reported (via a phone interview) a serious sickness. Moreover, we only asked households about sicknesses and treatment in the 2 months prior to the phone screening call. These two design decisions were intended to reduce survey costs and reduce recall bias, respectively. Only 1 member of the household was surveyed. Screening questions and survey questions for PHES were asked to the head of the household or to the most knowledgeable person (from roster). Respondents were asked about the previously identified health event, treatment, and finances.

5.3.5 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,[9] a rate known to surveyors *ex ante*.

5.3.6 Endline Survey

In March - May 2019, we will be conducting a 4 year follow-up survey. We will survey 1 member (our first priority is 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 will be asked about subjective health status, health care consumption and financial status. Households will be 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 will perform backchecks on 10% of households, a rate known to surveyors *ex ante*.

Rounds (1) – (3) were conducted on paper and the remainder on tablets. Rounds (1) – (4) were conducted by IFMR and the midline (5) by Nielsen. The endline will be conducted by Outline India.

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

5.4 Treatment Delivery

The population that is eligible for this study would not be eligible for RSBY under the program's usual rules. To make this study possible, we exchanged letters of cooperation with the state in which we conducted the study (Karnataka), the central government agency (Ministry of Labor) responsible for administration of RSBY, and the development organization (GIZ) providing technical support for RSBY.

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 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.

Roughly 78.4%, 70.4% and 58.6% of households in arms A, B, and C, respectively enrolled in RSBY. Enrollment rates are somewhat higher in Gulbarga than Mysore: 77.9/76.4/64.1% v. 78.8/63.5/52.3% in A/B/C. No one in arm D enrolled.

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 is 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.

5.5 Balance Tests

We conduct balance tests to validate that assignment to treatment was indeed random. We do this in 5 steps.

1. We gather baseline measurements on a subset of important outcome variables (defined in section 8).

2. 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.

3. 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-value from these LR tests.

4. If the randomization is successful, then the p-values from these tests should stochastically dominate the uniform distribution. We will use the one-sided Kolmogorov–Smirnov test to determine if this expectation is supported by the data.

5.6 Sample Attrition

Figure 1 describes household attrition through midline.

5.6.1 Baseline attrition

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. Randomization accounted for the most updated tracking data from the baseline survey; however, a lag contributed to overall attrition between randomization and baseline.

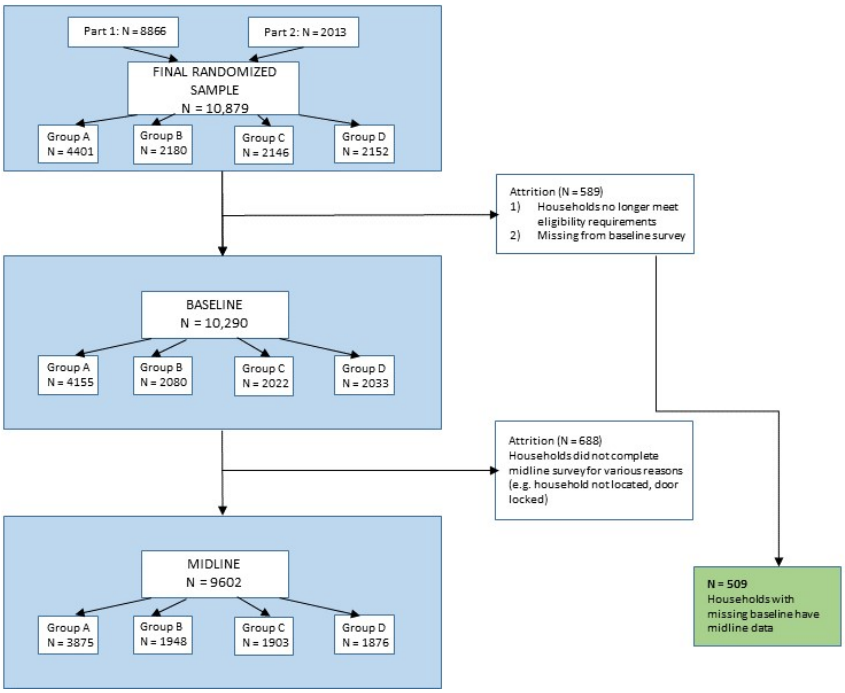

Figure 1: Attrition through midline.

5.6.2 Midline attrition

Attrition between baseline and midline can be attributed to household non-response and the inability to locate sample households, including their movement outside the village. Since many households with missing baseline data were able to be surveyed at midline, a total of 10,111 randomized households have midline data, an attrition rate of 7% between randomization and midline.

6 Companion willingness-to-pay study

We conducted a Willingness-to-Pay study (WTP study) that is distinct from but complements the main experiment. In some of our specified analyses, we will use the data from this companion study.

After conducting the listing exercise explained in Section 5.3.1, we selected a random subsample of 210 households, 105 from each district, to participate in the WTP study. Each of these households met the inclusion and exclusion criteria laid out in Section 5.1. However, these households were then excluded from selection for the main study, i.e., the treatment assignment described in Section 5.2. Instead, these 210 households were administered the baseline survey and a survey that used the Becker-DeGroot-Marshak (BDM) mechanism to measure the household's willingness to pay for RSBY health insurance. (These elicitations were incentivized, i.e., those households whose stated WTP was above the random draw in the BDM procedure received RSBY coverage at their stated WTP.)

7 Identification Strategy

7.1 Intent-to-Treat (ITT)

Our first parameter of interest 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:

$$y_{ijt} = \sum_{v=1}^{5} \beta_v d_j^v + \sum_{h=1}^{3} \gamma_h d_{ij}^h + \sum_{h=1}^{3} \sum_{v=1}^{5} \lambda_{hv} d_j^v d_{ij}^h + \delta X_{ijs} + e_{ijt} \quad (1)$$

where y_{ijt} is the outcome for household i in village j at time t; 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; d_{ij}^h is the indicator for household

group $h \in \{1,2,3,4,\}$; γ_h for $h \in \{1,2,3\}$ are ITT estimators for adding one household to groups A, B, and C (D is the baseline); and (β_v, λ_{hv}) measures spillovers.

Coding treatment variables. 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:

$$y_{ijt} = \sum_{h=1}^{4} \beta_h \sigma_j^h + \sum_{h=1}^{3} \gamma_h d_{ij}^h + \sum_{h=1}^{3} \sum_{h'=1}^{4} \lambda_{hh'} \sigma_j^{h'} d_{ij}^h + \delta X_{ijs} + e_{ijt} \tag{2}$$

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 (2), this equation is based on a parametric assumption. 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)).[10]

In addition, we will estimate two substantively different specifications to abstract away from methods of access to insurance and towards the mere fact of access to insurance. On one, we replace the four household group indicators with an indicator for whether the household was in group A, B or C and we replace the four village-level arm share variables with the share in groups A, B or C.[11] Additionally, we will estimate a specification where σ_j^v is replaced by the total share of the village eligible for RSBY, including both the share assigned to A, B and C as well as the share of the village with BPL cards or in certian eligible occupational classifications (who were therefore RSBY eligible).

Control variables. We first estimate a variant of the regression model above without any controls X_{ijs}, where s indicates a time that may not be equal to t. We then estimate a specification where X_{ijs} is a vector of baseline controls (i.e., X_{ijs} at time $s = 0$). We will use machine learning methods to select these control variables. Finally, we will run specifications where X_{ijs} may include so-called "coloring variables" that shed light on mechanisms of impact, welfare effects, and the dosage of treatment. In the latter case, we will include interactions of treatment variables with coloring variables, the same manner in which we estimate heterogeneous treatment effects. These coloring variables are listed in section 8.

Weighting. This regression can be weighted in multiple ways. We could either weight each household equally or weight each village arm identically. When examining household-level access We choose to 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. When examining spillovers, we will weight villages equally.

Standard errors. We use 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 in one way affects households not given access to insurance in that way, we do a few things.

First, we measure ITT effects accounting for spillovers at the village level by examining the predicted effects on outcomes for a household in a given group at different shares of the village sample in that group. The specific shares selected could be in-sample shares or out-of-sample shares. We will highlight when we make out-of-sample predictions.[12] In cases where we want to report ITT under sample average spillovers, we will exclude interaction between household arms and, e.g., village arms.

Second, 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.

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

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

[11] Berry et al. (2018) consider different subsidies and associated extrapolation issues.

[12] Although our study varies the fraction of subjects in a village who receive access to insurance in some form, we do not – as Table 1 shows – give some treatment to no households or some treatment to all households. Therefore, to estimate the above parameter of interest we have to make out-of-sample predictions.

7.2 Complier average treatment effect (CATE)

Our second parameter of interest is the CATE estimate for the impact from uptake of insurance. Our basic regression model to estimate this has a two-stage least squares structure:

$$y_{ijt} = \alpha + \theta s_j + \phi z_{ij} + \rho s_j z_{ij} + \psi X_{ij} + u_{ijt} \tag{3}$$

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 (2). In particular, the control variables and weighting are managed as they are in the previous section.

Identification. 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. To address the possibility of spillovers, we 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 $\rho = 0$.

7.3 Testing for heterogeneous effects

We will examine whether the impact of access to insurance and uptake into insurance varies with certain individual, household and village characteristics at baseline and enrollment. We will estimate these heterogeneous treatment effects as follows. We will 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:

$$\begin{aligned} y_{ijt} = \alpha &+ \sum_{v=1}^{3} \beta_v \sigma_j^v + \sum_{h=1}^{3} \gamma_h d_{ij}^h + \sum_{h=1}^{3}\sum_{v=1}^{3} \lambda_{hv} \sigma_j^v d_{ij}^h \\ &+ \sum_{v=1}^{3} \beta_{vw} \sigma_j^v w_{ij} + \sum_{h=1}^{3} \gamma_{hw} d_{ij}^h w_{ij} + \gamma_w w_{ij} + \sum_{h=1}^{3}\sum_{v=1}^{3} \lambda_{hvw} \sigma_j^v d_{ij}^h w_{ij} \\ &+ \phi w_{ij} + \delta X_{ij} + e_{ijt} \end{aligned} \tag{4}$$

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 $(\beta_{vw} = \beta_v, \gamma_{hw} = \gamma_h, \lambda_{hvw} = \lambda_{hv})\ \forall\ (h, v)$. In order to ease interpretation, we will make modifications to the above specification analogous to the ones we make to ease interpretation of the ITT regressions.

We will test for heterogeneous treatment effects in two steps. First, we will test for heterogeneity along several basic dimensions[13] we think are the most likely to generate heterogeneous effects:

1. District
2. Size of household
3. Age
4. Wealth (measured by assets)
5. Number of hospitals nearby or distance to closest hospital
6. Hospital utilization in prior year (measured at baseline)

[13] As noted above, for time-varying characteristics we will use the baseline values.

7. Monthly expenditure budget (measured at baseline)
8. Predicted propensity to utilize inpatient healthcare (described below)
9. Village size
10. Urban status

Second, we will use machine learning methods to help predict which dimensions of heterogeneity (from the list below) are most important (Duflo 2018, Wager and Athey (2018)).

1. District
2. Size of household
3. Cast or religion
4. Number of adults in household
5. Number of kids in household
6. Having children
7. Distance from closest public and/or private hospital
8. Number of public and/or private hospitals nearby
9. Presence or number of private hospitals nearby
10. Gender of subject
11. Age or child/adult status
12. Marital status, birth order and relationship to household head
13. Size of village
14. Percent of village assigned to access to RSBY via this RCT
15. Percent of village eligible for RSBY outside the RCT
16. Hospital utilization in prior year (measured at baseline) or predicted hospitalization
17. Health status or serious sickness
18. Cognitive capacity
19. Willingness to pay for insurance (predicted based on data from a randomly selected sample of households removed from main RCT and chosen to participate in a WTP study)
20. Risk and time preferences
21. Assets and savings
22. Urban, peri-urban, and rural status
23. Monthly expenditure budget
24. Experiencing a serious health shock in the past year (provided this is balanced across treatment arms)

Predicting utilization. A natural dimension along which the impact of insurance may differ is the latent propensity of a household to experience health shocks which would require inpatient care. Since this variable is latent, it must be estimated. We will machine learning methods to predict households' likelihood to experience an inpatient hospitalization. We will perform this exercise on two samples: the baseline data (across all treatment arms) and the post-intervention data (using only group D, the control group). The resulting model will then be used to generate predictions for the full sample. Our key hypotheses are (1) that the impact of the offer of insurance will lead to larger treatment effects for those with higher propensity; and (2) that demand for insurance will be less price-elastic among those with higher propensity (i.e., adverse selection). Hypothesis (2) will be tested by examining how takeup of insurance varies between free and not free (group A vs. groups B and C) for high- vs. low-propensity households.

7.4 Accounting for multiple inference

Access to health insurance may affect a number of aspects of households' lives (such as health behaviors, health outcomes, investment decisions, and so on). When testing hypotheses about the impact of treatment on individual outcomes, no multiple testing adjustment is required.

However, our survey instrument in many cases includes multiple questions related to a single behavior or dimension. When those multiple questions are used to test a common hypothesis about a behavior or dimension, an adjustment is required. We will account for multiple inference when testing such hypotheses by using indices of outcome variables and family-wise p-value adjustment.

To be more specific, we have listed in Section 8 the primary groups of outcomes that we intend to consider. For each of these groups, we will construct indices (à la Kling, Liebman, and Katz (2007)) of all the outcomes in the family taken together. Then, for each of these index outcomes, we will report both the standard p-value and the p-value adjusted for multiple hypothesis testing across all the indices. We will calculate the adjusted p-values using the step-down procedure of Hochberg (1988), which controls the family-wise error rate for all the indices. (This is the approach taken in, for instance, Banerjee et al. (2015).)[14]

7.5 Minimizing attrition bias

We address the possibility that attrition affects our ITT or CATE estimates in the following manner. Let a_{ijk} be an indicator that equals 1 if our data contain a non-missing observation on household i in village j at baseline but not at endline. First, we check balance in attrition across household or village treatment arms by estimating the ITT model in equation (2) while setting $y_{ijt} = a_{ij}$. We will also test whether there is differential attrition across insured and uninsured households by estimating system (3) while setting $y_{ijt} = a_{ij}$.

Second, if we find evidence of differential attrition, we will take either of two approaches. One is to impute outcomes for attrited households using multiple imputation (King et al., 2001). In this approach, we will compare ITT and CATE effects with imputation and without. The other approach is to bound our parameter of interest using Lee bounds (Lee, 2009).

If we observe differential attrition, we will estimate all of our regressions with a correction for sample selection inspired by DiNardo, Fortin, and Lemieux (1996). Their procedure re-weights the data using the inverse of the propensity to be observed at endline, so that the distribution of observable characteristics at baseline among households observed at endline resembles that in the entire baseline sample.

7.6 Method of estimation

In the main, we will use OLS to generate our ITT and CATE estimates. For some variables such as medical expenditures, debt, and monthly expenditure budget, we will be interest not in the effect on the mean of outcome variables but on the distribution of the outcome or on tails. For those variables we will use quantile regressions or distribution regressions when the treatment variable is continuous, e.g., share of village sample in a given arm or set of arms (Chernozhukov et al., 2013). We will compare CDFs of outcome variables when we compare outcomes across two or more discrete groups.

8 Outcomes of interest

The following are outcome variables, organized into groups by subject, that we will examine. We shall indicate those which will be analyzed as part of an index.

In each group, we will also list coloring variables that shed light on mechanisms behind causal relationships, on dosage of treatment, or on the welfare implications of treatment. We describe how we use these controls in Sections 7.1 and 7.2. Note, in some cases, outcome variables also serve as coloring variables.

8.1 Demand for insurance

8.1.1 Uptake

- Take up RSBY at enrollment (in 2015)

[14] We will also consider multiple testing approaches as raised in Lee and Shaikh (2014).

- Possession of RSBY card at endline
- Possession of RSBY card in past
- No longer have RSBY card
- Number of household members on RSBY card

Coloring variables

- Date RSBY card acquired
- Why household no longer has RSBY card
- Expected RSBY card but did not receive it

8.2 Utilization

The following were asked separately for a sickness (as screened using the same criteria as our PHES instrument) and the most serious treated case (defined as largest expense or longest length of stay).

- Seek treatment for an illness (with illness defined by PHES screens)
- Visit a clinic
- Visit a hospital
- Visit a private (versus government) hospital
- Length of stay
- Any medical expenses

The following outcomes were measured on all subjects

- Had a day surgery
- Number of day surgeries
- Type of day surgery
- Tried to use RSBY card
- Were able to use RSBY card
- How many times did you visit a hospital?
- How many nights did people stay in a hospital across all hospital visits?
- Knowledge about RSBY (number correct out of 4 questions)
- Subjective willingness-to-pay for RSBY

Coloring variables for last 2 sets of outcome variables

- Did you have an illness (as defined by PHES screens)
- Number of household members on RSBY card
- Age (child or adult) and gender of household members on RSBY card
- Household has BPL card
- When RSBY stopped being valid
- Unable to use RSBY card
- Expected RSBY card but did not receive it
- Why household did not try to use RSBY card

- Why household was unable to use card
- Age (child or adult) and gender of person who was ill (with illness defined by PHES screens)
- Did RSBY card pay for day surgery
- Verified eligibility for NHPS (National Health Protection Scheme or Ayushman Bharat)
- Verified claims under NHPS

8.3 Financial value

8.3.1 Financing care

The following were asked for the most serious treated case (defined as largest expense or longest length of stay) and for all medical treatments.

- Total cost of treatment
- Total amount paid by RSBY card for treatment
- Total cost of treatment net of RSBY payments
- Total amount paid for care with money outside households (other than RSBY)
- Borrowed money to pay for treatment
- Amount borrowed to pay for treatment
- Borrowed money to pay lost income
- Amount borrowed to pay for lost income
- Interest rate (or duration to repay and amount repaid) on largest loan
- Total amount paid for care with money from within household
- Amount paid with own savings
- Sold assets in response to sickness
- Value of assets sold in response to sickness
- Household worked more to pay for treatment
- Canceled or delayed expense to pay for treatment

The following were asked only of all medical treatments

- Spending on health care, by provider (hospital, clinic, medication, traditional medication, tests, other)
- Medical expenses, variability
- How household would pay for medical expenses from a hypothetical hospitalization

Coloring variables for the last set of outcome variables

- Spending on health care, by provider (hospital, clinic, medication, traditional medication, tests, other)

Coloring variables for the last 2 sets of outcome variables

- Why household did not try to use RSBY card
- Why household was unable to use card
- Interest rate (or duration to repay and amount repaid) on largest loan

- Number of months permitted to repay largest loan
- Total amount borrowed on largest loan
- Total amount to be repaid on largest loan
- Expense canceled or delayed to pay for treatment was business (versus household) expense
- Asset that could be sold to raise Rs. 8000 in emergency: existence, purchase price, use for asset, earnings from asset, loss of earnings from sale of asset, earnings from urgent (versus non-urgent sale)
- When, in last 5 years, was last item or service worth INR 8000 purchased? (If none, then item greater than INR 2000?) Surplus from that item (willingness to pay - amount paid)?

8.3.2 Consumption

- Monthly expenditure, typical month
- Monthly expenditure (net of medical expenses), typical month
- Monthly expenditure, variability
- Delay consumption

Note: To improve precision, we will estimate expenditure specifications in which we impose a household-level budget constraint, as in Kaboski et al. (2018).

8.3.3 Income

The following outcomes were measured on all subjects.

- Now have BPL card
- Missed 2 or more days of work or school (as defined in PHES)
- Earn money from a business
- Net business income, last month
- Net business income, variability
- Average daily income of ill person
- Business income, typical day
- Number of people working for household business (versus outside the business)
- Work outside household business: number of days and total income

Coloring variables for the last set of outcome variables

- Number of people working for household business (versus outside the business)
- Work outside household business: typical daily income

The following were asked separately for a sickness (as screened using the same criteria as our PHES instrument) and the most serious treated case (defined as largest expense or longest length of stay).

- Missed work due to sickness and any treatment
- Days of work lost due to sickness and any treatment
- Loss of income due to sickness and any treatment

Coloring variables

- Average daily income of ill person

8.3.4 Assets and investment

- Business asset: existence, change in amount, illiquidity, loss of value from quick sale
- Financial well-being, as measured on financial ladder and by reduction in consumption due to financial difficulty
- Own or rent home
- Rooms in home
- Number of pucca rooms, semi-pucca rooms
- Index[15] of select business and non-business assets.
- Index[16] of livestock
- Acres of land owned
- Cash (in savings accounts or other locations)

Coloring variables for the last set of outcome variables

- Nature of business
- Business asset: loss of value from quick sale

8.4 Health value

- Sickness (as defined in PHES)
- Pregnancy or stillbirth (as defined in PHES)
- Self-reported health rating (1 to 5)
- Mortality (all cause and sickness-associated)
- Weight

8.4.1 Chronic disease

- High blood pressure: diagnosed, treated
- Diabetes: diagnosed, treated (at all, insulin, special diet)
- Cancer: diagnosed, treated
- Chronic lung disease: diagnosed, treated
- Heart condition: diagnosed
- Stroke: diagnosed
- Arthritis or rheumatism: diagnosed, treated (arthritis)
- High cholesterol: diagnosed, treated
- Eyesight problems: diagnosed, treated

[15] We describe how we construct this index and what assets we include in the index in an appendix to this document.
[16] See prior footnote.

8.4.2 Infectious disease

- TB
- Malaria
- Dengue
- Chikungunya
- HIV/AIDS
- Chickenpox
- Typhoid
- Hepatitis
- Pneumonia
- Diarrheal diseases
- Influenza
- Jaundice

8.4.3 Health-related quality of life, ADLs, and physical health symptoms

- ADL index (as defined in PHES)
- SF-8 battery
- Types of pain (after being screened in for pain by SF-8)
- Physical health symptoms

  Dizziness

  Fainting

  Shortness of breath

  Feeling your heart pound or race

8.4.4 Mental and emotional health and cognitive function

- GHQ-12 (to measure depression)
- PSS-10 (to measure stress)

8.4.5 Health behaviors

- Smoking: existence, amount
- Drinking: existence, amount
- Physical activity: amount of moderate, amount of vigorous
- Composition of diet: frequency of cereals, pulses, milk, leafy vegetables, fruits, eggs/poultry/fish/meat, salty and fatty foods, soft drinks

8.4.6 Fertility

- Number of total births
- Number of live births
- Number of still births
- Location of births
- Who assisted during birth
- Complications during births
- Ideal fertility (midline)

8.4.7 Coloring variables

Because utilization and financial impacts are mechanisms through which insurance may affect health, the coloring variables in those sections may also serve as coloring variables for health outcomes.

8.5 General coloring variables

The following is a non-exhaustive list of variables that may be used as control variables that measure dosage, specific margins of response or dosage. This list is non-exhaustive as it lists only those variables measured at endline and not otherwise included as a coloring variable above.

- Gender of respondent
- Age of respondent
- Role of respondent in household (relationship to head of household)
- Size of household
- Number of adults in household
- Number of children in household
- Knowledge of BPL households and how many
- Knowledge of BPL households with RSBY cards and how many
- Knowledge of household that successfully used RSBY
- Knowledge about RSBY (number correct out of 4 questions)
- Knowledge of when RSBY scheme expired
- Subjective willingness-to-pay for RSBY
- Knowledge about NHPS
- Does respondent think household is covered by NHPS
- One item test of NHPS (does respondent know about amount of coverage)
- Did any other individuals assist with answering questions during the survey? If so, what is the gender and relationship to respondent? [17]

[17] Additional respondents may affect the response quality in both positive and negative ways. For finance questions, these individuals may improve the precision of the responses. For health questions, these individuals may lead to decreased precision. Please note that enumerators were instructed to administer health questions without any additional individuals present to minimize this concern.

9 Structural modeling

We plan to develop an economic model of household financial decisions and health status. We will structurally estimate the model using the experimental variation, to quantitatively reconcile the rich set of empirical results and perform counterfactual analysis.

10 Why is higher price for insurance associated with higher utilization?

At midline we found that individuals in group C, which had to pay a premium for insurance but did not get an unconditional cash transfer, had higher rates of utilization of the RSBY card. We will explore three explanations. First, individuals that value insurance more because they are more likely to use it are more likely to take up insurance, i.e., there is selection. Second, a higher price for a good sends a positive signal about the quality of the good, and higher quality goods are consumed more. Third, there is some sunk cost effect.

We will test this finding again at endline, and we will test which of these explanations for the midline finding (and potential endline finding) have merit. Our specific analyses are listed below.

10.1 Higher price causes greater utilization

We test whether the midline finding persists at endline in two ways:

- We examine whether group C households have higher rates of attempts to use RSBY and successful efforts to use RSBY.
- Because households with more members have a lower effective price per member[18], larger households should have greater utilization per capita (as opposed to per household).

10.2 Selection

We test this explanation by testing its assumptions or predictions:

- Individuals who were differentially sick or risk averse at baseline are more likely to enroll in RSBY in the first place and/or retain the RSBY card at midline and endline, conditional on household group assignment.
- Total utilization should be weakly higher in household group A than C. Equivalently, average utilization including both enrollees and non-enrollees, i.e., the ITT estimates of utilization, should be higher in A.[19]
- After predicting those in group A likely to enroll if they were in group C, we test to see if they have higher utilization conditional on getting insurance.

10.3 Price signaling

We test this explanation by testing its prediction that, assuming individuals learn about prices not just from their own experience but also from the experience of their peers, individuals who live in villages where there are more people who received insurance at a lower price (e.g., zero) should perceive insurance as having less value and thus purchase it less themselves. We test this prediction by examining the following:

- Holding constant the group assignment of a household, the household should attempt to or utilize care more the higher is price paid by other households that are both in the study sample and in the same village.

[18] The price of RSBY is fixed under our study—and for BPL families in the actual scheme—regardless of the size of the household.

[19] By contrast, average utilization could be lower in A than C if you only look at enrollees, i.e., the CATE estimates for A could be lower than for C even if there is selection.

- Holding constant the group assignment of a household, the household should attempt to or utilize care more the higher the price paid by other households that are in the same village, including both other households in the village and the fraction of households in the village that are below the poverty line.

10.4 Sunk costs

We do not pre-specify any specific assumptions or predictions we are able to test under this explanation. Therefore, we attribute any failure to support the above two explanation as evidence in favor of this third explanation.

11 Are there spillovers from formal insurance?

RSBY may have spillovers that run from program participants to other RSBY participants and to non-participants. As for the former, it has been shown that per-capita program utilization increases with the number of people who utilize the scheme. For example, people hear that the program works by word of mouth so they are more likely to utilize it (Sood et al., 2014). (This is not unique to health insurance schemes.) More sophisticated analyses of technology or program adoption via social networks suggests the same (Banerjee et al., 2013).

As for non-participants, there are at least two ways they may be impacted. First, formal Formal health insurance may impact alternative methods of financing health care expenditures and of financing generally. For example, formal health insurance may substitute for informal health insurance, or it may complement it if formal insurance covers village-level shocks while informal insurance covers household-level shocks. Moreover, formal insurance may impact credit markets by reducing precautionary savings, and thus the supply of credit in village-level credit markets, or reduce the demand for borrowing, and thus the demand for credit in those markets. Moreover, formal insurance may impact asset markets if formally insured people change their asset allocations, and frictions in those markets mean that prices for assets are set at the village level. Second, provision of insurance may lead to congestion at covered providers, to the extent that capacity constraints are present. This would impact both participants and non-participants.

11.1 Estimation

We examine both the existence of spillovers and the mechanisms behind those spillovers. Our source of variation is the village-level group to which a household is indirectly allocated via the first stage of randomization. These analyses will take the form of examining whether outcomes (e.g., utilization, consumption, assets, health) and intermediating variables (e.g., incidence of gifts and loans, knowledge of RSBY, asset prices) within a given treatment assignment (groups A–D) differ according to the stratum to which the village was assigned.

We will estimate spillovers directly from our ITT and CATE analyses. Because both positive and negative spillovers are possible, we will use two-sided tests. We weight villages equally as that is the level at which we posit that spillovers occur.

11.2 Mechanisms

For all groups together (main effect of insurance penetration) and specifically for household group D (interaction between penetration and household-level group indicator), we examine whether insurance penetration impacts:

- Utilization
- Interest rates for borrowing
- Borrowing
- Savings
- Ownership of non-durable or liquid asset
- (Fire) sale price of assets

Further, we will examine whether effects vary across variables that may affect the density of social networks, the extent to which hospital capacity constraints apply, and the impartance of the sample to local informal insurance or credit markets:

- Distance to and size of local hospital
- Interest rates for borrowing
- Size of village
- Fraction of village BPL
- Share of village that is RSBY eligible

12 Does insurance affect intra-household allocations?

Because RSBY does not necessarily cover all the members of a household, and because the household may decide how to distribute the resources freed up by the availability of insurance between its members, we examine the intra-household implications of RSBY. In particular, we test whether access to insurance or insurance differentially affects household members—including utilization measured by medical expense or by amount of hospitalization and including monthly expenditure budget—depending on their gender and their standing in the household, such as birth order or spouses' birth order.

By examining heterogeneity in treatment effects according to these observable characteristics, as well as heterogeneity in the characteristics of the local marriage market and labor market, we can uncover information about intra-household allocations.

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Appendix 1: Analysis of previously-collected data

This document provides a statistical analysis plan for data from the endline survey conducted roughly 4 years after treatment (and described in Section 5.3.6). However, this plan will also be used to guide analysis for data from two previously conducted surveys—a 12 month follow-up called the Post-Health Event Survey (PHES, Section 5.3.4) and an 18 month follow-up called a midline (Section 5.3.5). While this document was not prepared prior to collecting and commencing analysis of these prior data, it does lay out a consistent plan to analyze all follow-up data. Because the outcome variables measured in each follow-up survey are not identical, we indicate in a separate document the outcome variables we examine from previously conducted surveys and the surveys in which each variable is measured (P=PHES, M=Midline, E=Endline).

Appendix 2: Assets measured at endline

Due to the shortened length of the endline survey, we were unable to ask the full battery of household asset questions (18 categories) or the full inventory of livestock questions (4 categories). We reduced the total number of asset and livestock questions by employing polychoric principal components analysis (PCA) to isolate the most predictive categories for each group of questions.

For household assets, we ran a polychoric PCA on whether the household owned any of the 18 assets measured at midline. The analysis indicated that 6 assets provided 78% of the predictive power of the full battery, and we selected this subset for the endline survey: stove, sewing machine, silver jewelry, internet/dongle, tempo, and bullock cart.

To select a subset of livestock categories to ask at endline, we similarly ran a polychoric PCA on whether a household owned any of the 4 types of livestock at midline. The analysis indicated that 2 livestock categories provided 74% of the explanatory power of the full battery, and we included these questions at endline: birds (chicken, ducks, etc.) and cows or buffalo.

- Assets at midline: pressure cooker, mixer/grinder, pots, stove, fridge, sewing machine, color television, silver jewelry, gold jewelry, mobile phone, smartphone/tablet, computer, internet/dongle, bicycle/motorcycle or moped/scooter, tempo, food cart/push cart/bicycle van, bullock cart, and thresher/tractor
- Assets at endline: stove, sewing machine, silver jewelry, internet/dongle, tempo, and bullock cart
- Livestock at midline: birds (chicken, ducks, etc.), goats and sheep, cows or buffalo, and ox/bull
- Livestock at endline: birds (chicken, ducks, etc.) and cows or buffalo