The Effectiveness of Public Health Insurance: Evidence From Rajasthan, India

THE EFFECTIVENESS OF PUBLIC HEALTH INSURANCE: EVIDENCE FROM RAJASTHAN, INDIA

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ABSTRACT

Expanding public health insurance programs and contracting private hospitals for service delivery are common policy strategies to meet the goals of universal health coverage, but evidence from lower income countries on their design and function is limited. My dissertation studies the effectiveness of the BSBY government health insurance program that entitles 46 million low-income individuals to free care at public and empaneled private hospitals in Rajasthan, India. We use a unique dataset of insurance claims linked to post-visit patient surveys that allows us to analyze hospital-patient interactions under insurance. In the first paper, we document substantial out-of-pocket payments (OOPP) at private hospitals under insurance across a range of health care services and find that higher risk and less informed patients pay more. In the second paper we investigate whether hospitals are charging in order to compensate for reimbursement rates that are too low. We exploit a policy reform that discontinuously changed hospital reimbursements for different procedures by varying magnitudes to conduct a difference-in-differences analysis of private hospital responses, and find that less than half of the higher public subsidies are passed through to patients in the form of lower OOPP. In the third paper, we conduct an experiment to test whether providing phone-based information to eligible patients about their entitlements under the program can help them hold hospitals accountable and reduce OOPP. We find that the intervention is effective at increasing patient awareness of the program and leads to dramatic reductions in OOPP, but only at public hospitals. Together this research provides evidence that 1) OOPP is not simply due to problems with eligibility, enrollment, or facility choice, but also hospital charging behavior, 2) hospital capture contributes substantially to the observed high OOPP under public insurance in India, and 3) patient-driven accountability interventions may be important but insufficient to improve the effectiveness of public insurance programs.

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PAPER 1: OUT OF POCKET PAYMENTS UNDER HOSPITAL INSURANCE IN INDIA

Radhika Jain

1.1 Background and Motivation

Out-of-pocket payments (OOPPP) for health care services constitute a substantial financial burden across low- and middle-income countries (LMICs) and can lower health care utilization, with negative consequences for health outcomes, particularly among the poor (Qin et al 2019, Karan et al 2014, Mohanty et al 2012, Shahrawat and Rao 2011). As lifespans lengthen and the burden of non-communicable diseases increases, it is likely that the need for secondary and tertiary hospital care will grow along with the financial burden associated with it (Nandi et al 2014). Public health insurance programs targeting the poor have been a key policy effort aimed at protecting patients from health-related financial risk while ensuring access to quality care. National and state level health insurance programs have scaled up rapidly since 2007 in India, with substantial public financial outlays. However, studies find that high levels of OOPP persist and the effects of these programs on financial protection have been limited (Prinja et al 2017, Nandi et al 2017, Karan et al 2017, Mohanan et al 2014, Acharya et al 2012, Escobar et al 2010).

Identifying the specific factors driving OOPP under these programs is critical for determining where to focus policy reform efforts. OOPP despite the expansion of insurance programs may be due to problems at various points in the causal chain between insurance availability and patient financial outlays, including eligibility, enrollment, facility access and choice, procedures not covered under the program, or hospital charging behavior. Most of the literature has focused on the role of eligibility, enrollment, implementation, and awareness (Rathi 2003, Seshadri 2012, Rao 2014, Nandi 2015 review). This paper focuses on the role of hospital behavior, which is relatively understudied.

We study the BSBY health insurance program in Rajasthan, India, which entitles 46 million poor individuals to free secondary and tertiary care at public and empaneled private hospitals. Hospitals across the state are reimbursed at prospectively set, bundled rates for each patient visit that are supposed to cover all costs of care, including tests, medicines, and diagnostics. However, there is anecdotal evidence that hospitals continue to charge patients under the program. Because BSBY is similar in structure to most other government health insurance programs in India, findings from this study may be generalizable to other programs as well.

We use a unique micro-dataset that links insurance claims records from private hospitals to surveys with patients soon after they receive hospital care under insurance and allows us to analyze the behavior of hospitals participating in insurance. First, because patients are identified through claims filed in their name, we know with certainty that they are enrolled in insurance, have visited a hospital covered by the program, and that the hospital has been reimbursed by the program for their care. Therefore, OOPP cannot be explained by problems in eligibility, enrollment or patient facility choice, but is evidence of hospitals contravening program rules. Second, because we can directly link patient payments to specific hospitals, we can decompose the variation in OOPP into the share explained by differences across locations and hospitals and the residual variation across patients within the same hospital. This allows us to narrow the set of possible explanations for OOPP: variation in OOPP across hospitals may reflect differences in hospital costs, quality, or market characteristics, whereas within-hospital variation in OOPP controls for these hospital-specific differences and reflects differences in hospital charging behavior across patients. We then examine the factors associated with OOPP both within and across hospitals.

We document substantial rates and levels of OOPP at private hospitals across a range of services that are covered under BSBY. 35% of patients pay some amount, and mean OOPP levels vary across services but are highest for angioplasties and c-section deliveries (approximately $40 for both). Of the total variation in OOPP, 30% is explained by differences across hospitals and a striking 70% of the variation is within hospitals. Within hospitals, riskier and higher illness severity patients pay more, while patients that are wealthier, educated, and better informed about BSBY pay less. These associations suggest that hospitals may be charging sicker patients more because their costs of care are higher or because these patients have fewer options and less elastic demand. They also suggest that informed patients are better able to negotiate prices and get their entitlements under BSBY. Nevertheless, patient and care characteristics explain a relatively small share of the variation, suggesting either that other unmeasured aspects of care are contributing substantially to hospital OOPP charges, or that price negotiation plays an important role. For comparison, a recent study of U.S. private hospitals finds over 20% of the total price variation is within hospitals and cannot be explained by hospital, care, or patient characteristics, but is due to differences in contracts negotiated by insurers (Cooper et al 2019). Across hospitals, OOPP is negatively associated with hospital specialization and competition, which is consistent with the theory that hospitals with lower average costs and those that face stronger market incentives charge lower OOPP.

While neither causal nor exhaustive, we provide some of the first analysis of hospital-specific OOPP under insurance in India. We contribute to a growing literature showing that publicly financed health insurance programs in India have had limited effects on financial exposure at the population level and that high OOPP persists despite the expansion of insurance (Prinja et al 2017, Nandi et al 2017, Karan et al 2017, Mohanan et al 2014). We provide new evidence on the specific contribution of hospital charging behavior to the muted impacts of these programs on patient expenditures. The only other largescale study examining this question, to our knowledge, finds 44% of insured patients using RSBY, a similar health insurance program, still faced OOPP (Devadasan et al 2013). Although a large literature in the U.S. has grown around the initial documentation of substantial price variation across hospitals and geographic locations, there is little evidence of this type from the Indian context, where health care markets are largely unregulated and insurers have limited ability to enforce contracts with hospitals (Skinner 2012; Cooper et al 2019).

The paper proceeds as follows. Section 1.2 describes the program context, Section 1.3 presents the theoretical framework guiding our analysis, Section 1.4 describes the data, Section 1.5 discuss our methods, Section 1.6 presents results, and Section 1.7 discusses the limitations of our study as well as the implications of our findings.

1.2 Program Context

The Government of Rajasthan launched the BSBY health insurance program in December 2015. Households below the state poverty line are automatically enrolled and are entitled to free secondary and tertiary in-patient care at public and empaneled private hospitals, and are not required to pay a premium or co-pay. The Insurer, the New India Assurance Company, was chosen through a standard government procurement process. The verification of patients, as well as the filing, review, and reimbursement of hospital claims is managed through a central IT portal managed by the government. Like most public health insurance programs in India, under BSBY hospitals are reimbursed at prospectively set rates for predefined bundles of services, referred to as "packages" – i.e. hospitals are paid a lumpsum per visit based on diagnosis or service provided, regardless of the actual costs they incur for a particular patient. Rates are uniform for all hospitals across the state and are supposed to cover all costs of a hospital visit, including hospital stay, diagnostics, and medicines. In December 2017, the government increased reimbursement rates for several packages; the implications for analysis are discussed in Section 4 and the Appendix. As of mid-2018, the program covered 1400 unique packages for services ranging from child deliveries to heart surgery. The program has scaled up rapidly, with 866 private and 483 public hospitals actively participating and over 2.5 million transactions filed by mid-2018.

1.3 Theoretical Considerations

To narrow down the sets of factors potentially driving OOPP, we first decompose the total variation in OOPP to the share attributable to differences across locations and hospitals, and the share attributable to differences across patients within the same hospital. Variation across locations and hospitals reflects the role of factors such as local input costs, market structure, and hospital structure, while variation within hospitals reflects differences in hospital charging behavior across its patients.

One explanation for OOPP may be that hospitals charge more to patients with higher costs of care. Under fixed reimbursement systems, hospitals are reimbursed a flat amount for all patients receiving a service although the costs of treatment may be heterogeneous across patients because some patients are sicker or receive higher quality care. While hospitals in the U.S. have been found to risk select or skimp on care in response to fixed payment systems, in our context charging OOPP may be an alternate method of covering the higher costs of these patients (Ellis & McGuire 1996). Alternatively, hospitals may be engaging in third degree price discrimination and charging more to patients who are willing and able to pay. Studies of hospital behavior in Uganda and India, where regulation is weak and prices are not negotiated by third party Insurers, find evidence that hospitals charge richer patients more (Hunt 2010; Goodman et al 2018). A third theory is that hospitals simply exploit patients who are unaware of their benefits. To examine the evidence for these behaviors in our context, we test whether higher OOPP within a hospital, conditional on the service type provided, is associated with 1) patient risk, illness severity, and care quality, 2) patient socioeconomic status, and 3) lower patient awareness of the BSBY program.

Variation across hospitals may be driven by differences in the cost of providing care across locations or hospitals, or market characteristics. Because BSBY and most health insurance programs in India do not cost-adjust hospital reimbursements, hospitals in areas with higher local input costs may charge more. Hospital specialization can also increase efficiency, resulting in cost savings that may be passed on to patients (Dranove 1987). Finally, economic theory suggests that competition can induce hospitals to lower costs, profit margins, and patient prices (Dranove & Satterthwaite 2000). To examine the evidence for these theories, we test whether average OOPP is lower among hospitals 1) that specialize in a service and 2) that face higher competition for a service. Although we do not have data on local input costs, the extent to which location fixed effects explain OOPP variation puts an upper bound on the extent to which this factor matters.

1.4 Data

We use administrative insurance claims, which include patient phone number; hospital name, district, and sector (public or private); and the date of visit and package of care provided. We classified packages into clusters of closely related services and selected 12 different types of health care services for the study – hereafter, "clusters" of care - to reflect a range of types of care, as well as services that comprised a large share of all claims. The included clusters are ear procedures (tympanoplasties and mastoidectomies), eye procedures (pterygium removal), PCNL (percutaneous nephrolithomy, or kidney stone removal), PTCA (percutaneous transluminal coronary angioplasty), c-section deliveries, vaginal deliveries, chemotherapy, hemodialysis, laparoscopic appendicectomy (appendix removal), laparoscopic cholecystectomy (gall bladder removal), tooth procedures (scaling and restoration), and ward days (standalone claims for ward days, which typically act as a catch-all for non-specific hospital care). Claims for these services comprised approximately 63% of all claimed transactions at private hospitals over the study period, June 2017 to July 2018.

Using updated claims data received every 2-3 weeks, we stratified claims by cluster and randomly sampled a fixed number of claims within each cluster to be surveyed. In total, we sampled about 6% of the total claims filed in the study period. Surveys were conducted by phone within 3 weeks of the patient's hospital visit to reduce recall bias (Das et al 2012), and collected data on OOPP, details of care services received, technical and perceived quality, and patient demographic and socioeconomic status. Surveys for the two delivery clusters (vaginal and c-section) began in June 2017, while those for the remaining clusters began in late September 2017; all surveys continued until July 2018. Although the core survey was the same across all clusters, the delivery surveys included an additional set of more detailed questions about patient prior risk factors and care quality. We use the claims data to identify the package of care provided and to construct hospital and market characteristics variables, and we use the survey data to construct the OOPP and patient and care characteristics variables as follows and summarized in **Table 1.1**:

Table 1.1 Description of variables

	Variable	Description	Services Covered
	Variables from Claims Data		
Hospital/Market	Urban	Urban hospital location per the census	All
	Hospital size	Total transactions filed by hospital	All
	Hospital specialization	Hospital package or cluster claims as the share of total hospital claims	All
	HHI	Package or cluster-specific district level HHI using claims to determine market share	All
	Hospital market share	Hospital share of all package or cluster-specific claims filed in the district	All
	Variables from Survey Data		
Payment	OOPP at hospital	OOPP paid at the hospital	All
	OOPP elsewhere	OOPP for tests/medicines/supplies associated with a visit but purchased elsewhere	All
	Total OOPP	Total OOPP at the hospital and elsewhere for a visit	All
Risk/Severity	Referred	Indicator for whether the patient was referred from another facility	All
	Risk index	Composite index of indicators for history of high BP, warning of high BP/pre-eclampsia during antenatal visit, prior still birth, prior C-section, and last pregnancy 10+ years ago	Deliveries
	Complications	Composite index of indicators for multiparous birth, high BP, heavy bleeding, convulsions, and fainting	Deliveries
	Length of stay	Continuous measure of self-reported days spent at hospital	All
Quality	Care quality	Deliveries: Composite index of indicators for skin-to-skin care, labor companion allowed, whether seen by a doctor, called for post-delivery check-up, and warned of dangerous symptoms. Non-deliveries: Composite index of indicators for whether seen by a doctor, called for post-delivery check-up, and warned of dangerous symptoms.	All
	Perceived quality	Composite index of indicators if patient reported the staff were very respectful, hospital was very clean, and she was very satisfied with her care	All
	Private room	Indicator for whether patient had a private room	All
SES	BSBY awareness	Indicator for whether patient was aware of BSBY prior to visiting the hospital	All
	Schooling	Years of schooling, standardized over the sample	All
	Low caste	Indicator for whether patient is of scheduled caste or tribe	All
	Assets	Composite index of indicators for ownership of 12 assets	All

- <u>Hospital and Market Characteristics Using Claims Data:</u> Data on hospital physical infrastructure are unavailable, but we use total claims filed under BSBY within the study period as a proxy for hospital size, and a package or cluster's share of a hospital's total claims as a proxy for the hospital's specialization in a service. As measures of competition we construct package- and cluster-specific Herfindahl-Hirschman Indices (HHI) and hospital market share using the claims data and the administrative district as the market boundary. The HHI is a commonly used measure of competition and is the sum of the squares of market share for each hospital within a defined market. Although the appropriate definition of a market is contested in the literature, we use the district as a first approximation because the India health referral system is organized at this level. All hospitals in a district will have the same HHI for a given package or cluster. Market share will vary by hospital. Hospital urban status is based on the census classification.

- Patient Payments: We compute total OOPP for a visit as the sum of total patient-reported OOPP at the hospital for a visit and OOPP for tests and medicines related to the visit (that are supposed to be covered by BSBY) purchased outside the hospital.
- Risk, Complications, and Severity: We use four measures of patient risk and illness severity. For delivery patients, we create an index for prior risk (history of high BP, heavy bleeding, convulsions, and fainting). and an index of complications during the visit index (multiparous birth, high BP, warning of high BP/pre-eclampsia during antenatal visit, prior still birth, prior C-section, and last pregnancy 10+ years ago). We also use an indicator for whether the patient was referred from another hospital and a continuous measure of self-reported length of stay in days for all patients. Referrals to a different hospital are typically made for riskier, complicated cases that a lower level facility cannot handle. All four measures of risk and severity are positively correlated, adding confidence to the interpretation of referral as a measure of risk/severity (see Appendix).
- Quality: Quality measures include a care quality index (skin-to-skin care, labor companion allowed, whether seen by a doctor, called for post-delivery check-up, and warned of dangerous symptoms) for deliveries. For non-delivery services, the index only included the last 3 indicators. We also use a perceived quality index of indicators staff very respectful, hospital very clean, patient very satisfied with care), and whether the patient had a private room.
- BSBY Awareness and Socioeconomic Status: For BSBY awareness we use an indicator for whether the patient knew about BSBY prior to visiting the hospital. Patient demographic socioeconomic status (SES) includes age, sex, years of schooling (standardized over the sample), a dummy for scheduled caste or tribe (the lowest groups in the caste hierarchy), and an asset index (ownership of a list of 12 assets).

All of the indices above are the first component of a principal component analysis of a series of indicator variables that is standardize to have mean zero and a standard deviation of one for ease of interpretation. Because some of our measures of risk, severity, and quality were different for deliveries and non-delivery services, we analyze these categories of care separately where appropriate.

1.5 Methods

In addition to documenting the overall rates and levels of OOPP across the sample and by service type, our analysis consists of 3 stages.

Decomposition of Variation: As a first step to understanding the types of factors driving OOPP, we decompose the total OOPP variation into the share accounted for by differences across districts, towns, and hospitals, and the residual variation within hospitals, after controlling for case-mix and patient and care characteristics. We run transaction-level regressions of total OOPP on the full set of patient and care characteristics to adjust for risk and socioeconomic differences across locations, survey recall period, and month fixed effects. We start with including package fixed effects and add interactions of each of the location (district, town, or hospital) fixed effects with package fixed effects. The R2 for each specification indicates how much of the variation in total OOPP can be explained by the location, after adjusting for case-mix.

Within Hospital Variation: To examine patient and care characteristics associated with higher OOPP within hospitals, we run a series of transaction-level regressions of total OOPP for a patient on each of a series of patient or care characteristics. We include month fixed effects to control for service-specific time trends, as well as controls for patient age, sex, and survey recall period in all regressions. We also include the interaction of hospital and package fixed effects to ensure we are only comparing patients receiving the same type of service within the same hospital. Patient and care characteristics include our measures of patient risk and severity, care quality, and socioeconomic factors. We report the coefficients from regressions with each patient and care independent variable included separately. We also report the results from multivariate regressions that include all of the patient and care characteristics together, but because the independent variables are correlated in complex ways, the multivariate regressions must be interpreted with caution and we focus our attention on the bivariate specifications (see Appendix for correlations). We include several robustness checks in the Appendix: we restrict analysis to 2018 transactions to check whether the December 2017 policy reform changed payment patterns; we use OOPP at the hospital, excluding payments for tests and medicines purchased elsewhere, as the outcome variable; and we present results for the subset of transactions that had a non-zero OOPP.

Across Hospital Variation: We use two methods to examine what factors are associated with variation in mean service-specific OOPP across hospitals. First, we collapse our data to the hospital-package-month level to calculate mean payments and run regressions of the mean hospital-level OOPP on each of a series of hospital and market characteristics. We include month fixed effects to control for time trends, and package fixed effects to ensure we are comparing OOPP across hospitals for the same service. Second, to adjust hospital-level OOPP for differences in patient composition and care quality, we use a second approach that is similar in spirit to Cooper et al (2018). We first run a transaction level regression of OOPP on the full set of patient and care characteristics, along with month fixed effects and the interaction of hospital and package fixed effects. We then recover the hospital-package fixed effects, which we call the Adjusted Hospital Package FE (AHPFE). This controls for differences in case-mix, patient composition, and care quality across hospitals that may confound estimates of OOPP payment, and generates a measure of the variation in the mean adjusted hospital OOPP charge for each package.[1] We then regress these AHPFE on our measures of hospital and market characteristics to examine factors associated with higher OOPP across hospitals.

[1] Since we are interested in exploring the *variation* in OOPP across hospitals, rather than estimating hospital prices directly, the AHPFE is sufficient (whereas Cooper et al calculate hospital mean prices using sample mean values of the patient and DRG).

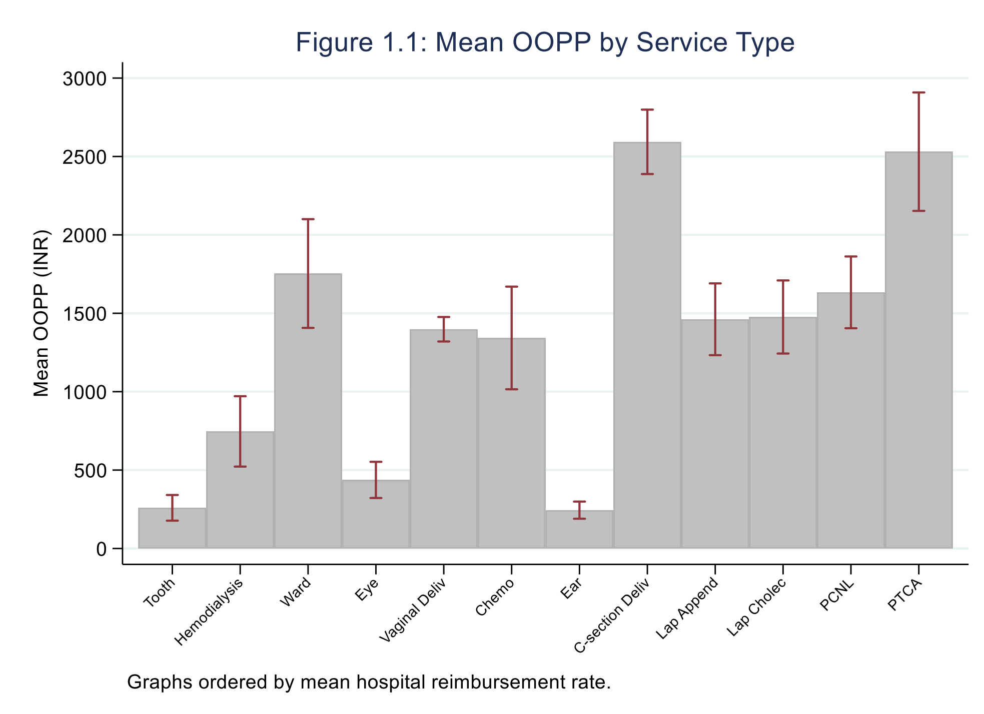
1.6 Results

1.6.1 Descriptive Statistics on Patient Payments

Table 1.2 presents descriptive statistics. We sampled a total 25,648 transactions for survey. Refusals were low at 1.3%, but a substantial share of patients were not reached due to invalid or incorrect phone numbers in the claims data and phone call non-response, resulting in a survey success rate of 70%. The average recall period between claim filing and survey completion was 25 days, or a little over 3 weeks. Our sample includes 627 private hospitals across Rajasthan. Overall, 35% of all patients pay, and OOPP averages INR1435 ($22), including payments directly to the hospital and for tests and medicines related to the transaction that were purchased outside the hospital. Hospital reimbursements for each patient visit average INR 13,562 ($209) overall and OOPP constitutes approximately 10% of the total payment to the hospital (reimbursement plus OOPP). **Figure 1.1** shows mean OOPP by service type. Payments are prevalent across all services, but levels vary substantially and are not systematically related to reimbursement values. C-sections with mean OOPP of INR 2593/ $40 (95% CI 2390 – 2790) and PTCA with mean 2530 / $40 (95% CI of 2150-2900) have the highest payment levels.

Table 1.2: Descriptive Statistics

	Mean	SD
Surveyed	69%	0.46
Recall period (days)	24.67	9.65
Any OOPP at hospital	27%	0.44
OOPP at hospital (INR)	1154	3291
OOPP at hospital (USD)	18	51
Any OOPP elsewhere	13%	0.33
OOPP elsewhere (INR)	291	1665
OOPP elsewhere (USD)	5	26
Any OOPP overall	35%	0.48
Total OOPP (INR)	1435	3817
Total OOPP (USD)	22	58
Hospital reimbursement (INR)	13562	17044
Hospital reimbursement (USD)	209	262
OOPP as share of total hospital payment	10%	0.35
Technical quality (PCA)	0.01	1.13
Perceived quality (PCA)	0.00	1.33
Length of stay (days)	2.57	1.95
Had a private room	11%	0.32
Female	71%	0.45
Aware of BSBY prior to visit	62%	0.48
Asset score (PCA)	0.01	1.78
Years of schooling	5.83	4.90
SC/ST caste	26%	0.44
Observations	25648	



1.6.2 <u>Decomposition of Variation in Payments</u>

In **Table 1.3** we decompose the variation in OOPP payments into the share explained by differences across districts, across census town/village locations within districts, across hospitals, and the remaining unexplained within-hospital variation in OOPP. The table presents the R2 from regressions of transaction level total OOPP on either just package fixed effects or the interaction of package fixed effects and an increasingly granular set of location fixed effects (hospital district, census town, and hospital), as well as controls for recall period and month fixed effects. We report both the total R2 and the additional variation explained in each column relative to the previous one. Models with districts and census town fixed effects explain 15% and 20% of the total variation in OOP, respectively (Columns 2 and 3). Overall, about 30% of the total variation in OOPP is explained by differences across hospitals (Column 4). In other words, 70% of the total variation in OOPP cannot be explained by factors that vary across locations or hospitals, such as market characteristics, patient demographics, local input costs, or hospital structure, and is attributable to variation in charges across patients within the same hospital.

Table 1.3: Decomposition of Variation in OOP

	(1)	(2)	(3)	(4)
		Dependent Variable: Total OOP Payment		
Fixed Effects Level	Package	District	Census Town	Hospital
R^2	0.058	0.147	0.199	0.292
Additional variation explained	--	0.089	0.052	0.093
Unexplained variation	0.942	0.853	0.801	0.708
Observations	17551	17474	17148	16734
Month FE	Yes	Yes	Yes	Yes
Package FE	Yes	No	No	No
District x Package FE	No	Yes	No	No
Census Location x Package FE	No	No	Yes	No
Hospital x Package FE	No	No	No	Yes

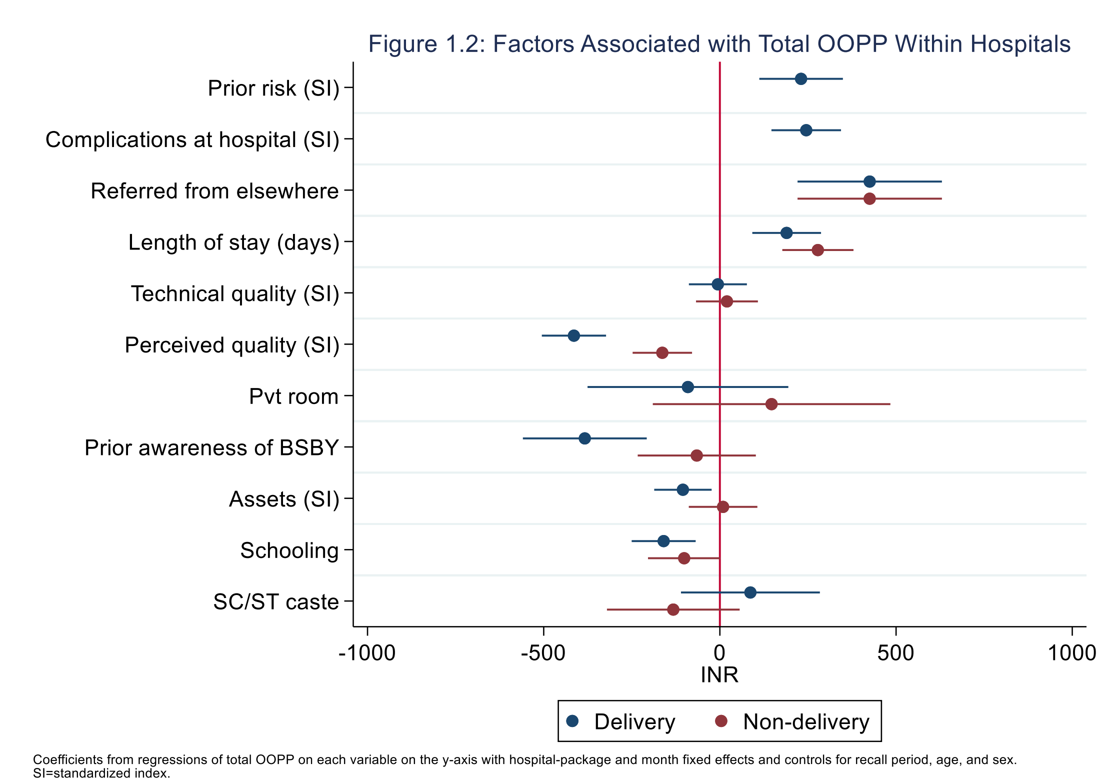
Observations are at the transaction level. The dependent variable is the total OOP payment at the hospital and for tests and medicines purchased outside the hospital. All regressions include controls for survey recall period. Census location is an indicator for the town or village Census 2011 location of the hospital. The unexplained variation is 1-R2.

1.6.3 Within-Hospital Variation in Payments

We next examine which patient and care characteristics are associated with higher payments within hospitals. **Figure 1.2** presents simple bivariate associations of patient and care characteristics with total OOPP, controlling for month fixed effects and hospital x package fixed effects to focus on within-hospital variation. **Table 1.4** presents the same bivariate relationships from **Figure 1.2** in Columns 1 and 3, as well as the full multivariate regression in Columns 2 and 4.[2] All measures of prior risk and illness severity at the hospital are positively and significantly associated with OOPP for deliveries, with coefficients of between INR 190 for each additional day at the hospital to INR 425 if a patient was referred from another facility (these are 11% and 24% of mean OOPP for deliveries). An additional day spent at the hospital is also associated with INR 278 higher OOPP for non-delivery services (23% of the mean OOPP). Care quality and having a private room are unrelated to OOPP, while patient perceived quality is negatively associated with it, suggesting OOPP are not compensating for the higher costs of better care.[3] Surprisingly, higher socioeconomic status is negatively associated with OOPP. A 1 standard deviation increase in the asset index and in years of schooling are associated with INR 105 and INR 160 lower OOPP for deliveries, respectively;

[2] As noted earlier and in the Appendix, measures within each family of explanatory variables – risk/severity, quality, and awareness/SES – are positively correlated with each other as expected, but correlations across families are more complex and the multivariate regressions must be interpreted with caution.

[3] Because perceived quality was measured after service and payment, we cannot disentangle whether these perceptions were formed independently of the amount a patient paid or whether paying more caused patients to perceive quality to be lower. However, the lack of positive association between OOPP and any of our measures of quality suggests that the hypothesis that OOPP is covering the costs of higher quality care is unsupported in the data.



we see a similar relationship for schooling in non-delivery services. It is also striking that delivery patients who were aware of BSBY prior to their hospital visit pay INR 383 less, on average (22% of the mean OOPP), though the coefficient is much smaller and insignificant for non-deliveries. Prior risk, complications at the hospital, and length of stay continue to have large positive coefficients in the multivariate specifications, while higher socioeconomic status and BSBY awareness are associated with lower OOPP even after controlling for risk and complications. These findings are consistent with the theory that hospitals charge more for complicated cases, but not for higher quality care. They are not consistent with standard theories of price discrimination, as wealthier patients pay less, but support the hypothesis that hospitals charge patients who do not understand their benefits or may be less able to negotiate.

1.6.4 Across-Hospital Variation in Payments

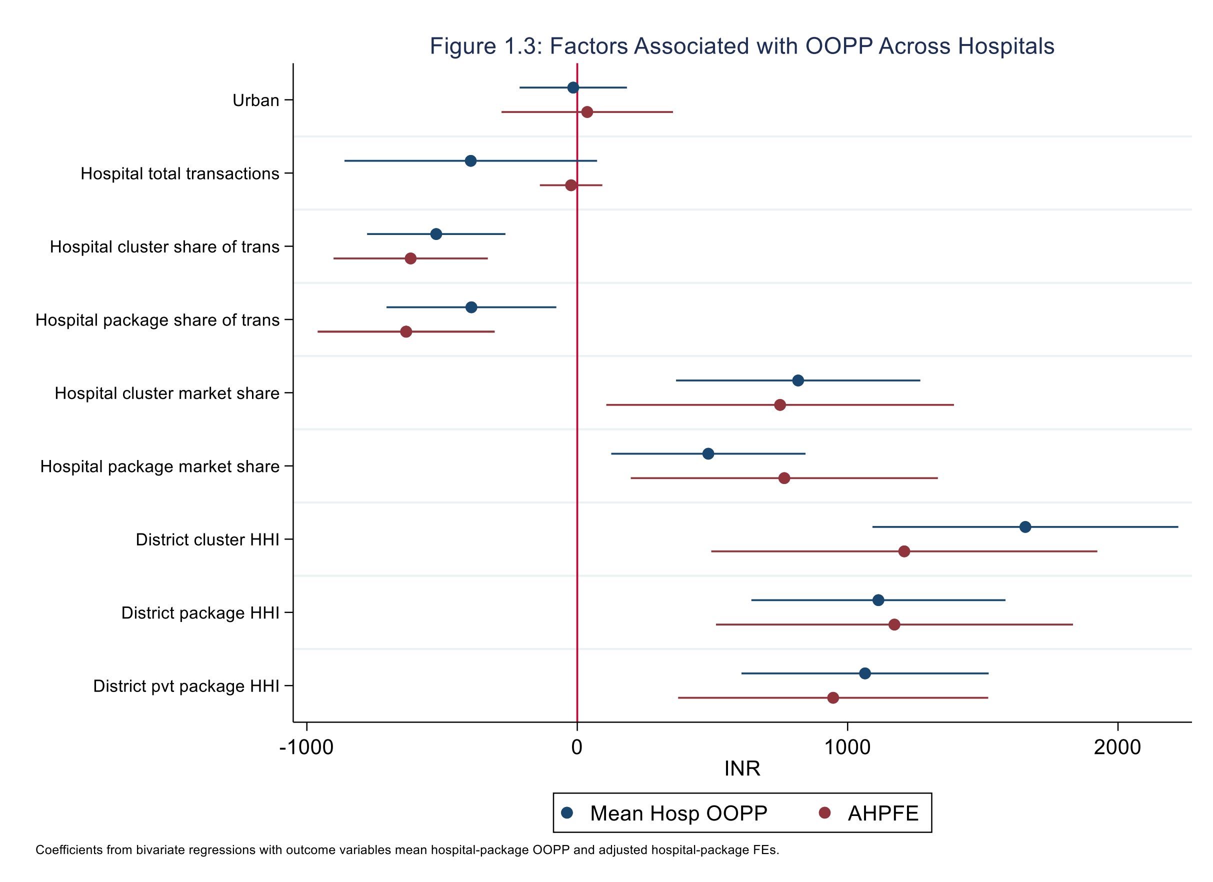
We next examine hospital and market level factors associated with OOPP across hospitals. **Figure 1.3** presents the coefficients from bivariate regressions of the mean package-specific hospital OOPP with

Table 1.4: Within Hospital Variation in OOPP

	(1)	(2)	(3)	(4)
	Dependent Variable: Total OOP Payment			
	Deliveries		Non-Delivery Services	
	Bivariate	Multivariate	Bivariate	Multivariate
Prior risk (SI)	230.51***	186.48**		
	(60.46)	(67.09)		
Complications at hospital(SI)	245.06***	180.14**		
	(50.37)	(79.69)		
Referred from elsewhere	425.26***	166.46	129.11	70.39
	(104.52)	(154.23)	(96.35)	(94.75)
Length of stay (days)	189.47***	141.04**	278.12***	281.82***
	(49.76)	(67.25)	(51.51)	(49.59)
Technical quality (SI)	-5.70	54.50	20.06	12.61
	(41.99)	(54.07)	(44.79)	(46.06)
Perceived quality (SI)	-414.21***	-349.42***	-163.37***	-183.87***
	(46.47)	(59.96)	(43.03)	(44.63)
Pvt room	-90.69	-61.08	146.75	20.26
	(145.36)	(202.52)	(171.97)	(169.53)
Assets (SI)	-104.96**	-78.67	9.07	50.15
	(41.55)	(51.63)	(49.63)	(48.44)
Schooling (S)	-159.49***	-52.52	-101.32*	-67.15
	(46.32)	(62.92)	(52.38)	(54.16)
SC/ST caste	86.68	251.76*	-132.23	-147.73
	(100.52)	(149.37)	(96.07)	(94.51)
Prior awareness of BSBY	-383.35***	-332.26**	-65.54	-26.26
	(89.65)	(118.76)	(85.49)	(87.85)
Constant		1431.74**		838.90***
		(642.58)		(235.01)
Month FE	Yes	Yes	Yes	Yes
Hospital x Package FE	Yes	Yes	Yes	Yes
Observations		3178		8363
Mean OOPP	1746.95	1746.95	1191.37	1191.37
R^2		0.475		0.295

Observations are at the transaction level. The dependent variable is the total transaction OOP payment at the hospital and for tests and medicines purchased outside the hospital. SI indicates a Standardized Index – i.e. an index of several indicator variables standardized over the full sample. All regressions include additional controls for patient age and survey recall time (days between claim filing and survey completion). Columns 3 and 4 also include patient sex. Mean OOPP is the unadjusted mean total OOPP across all patients. Robust clustered standard errors in parentheses.

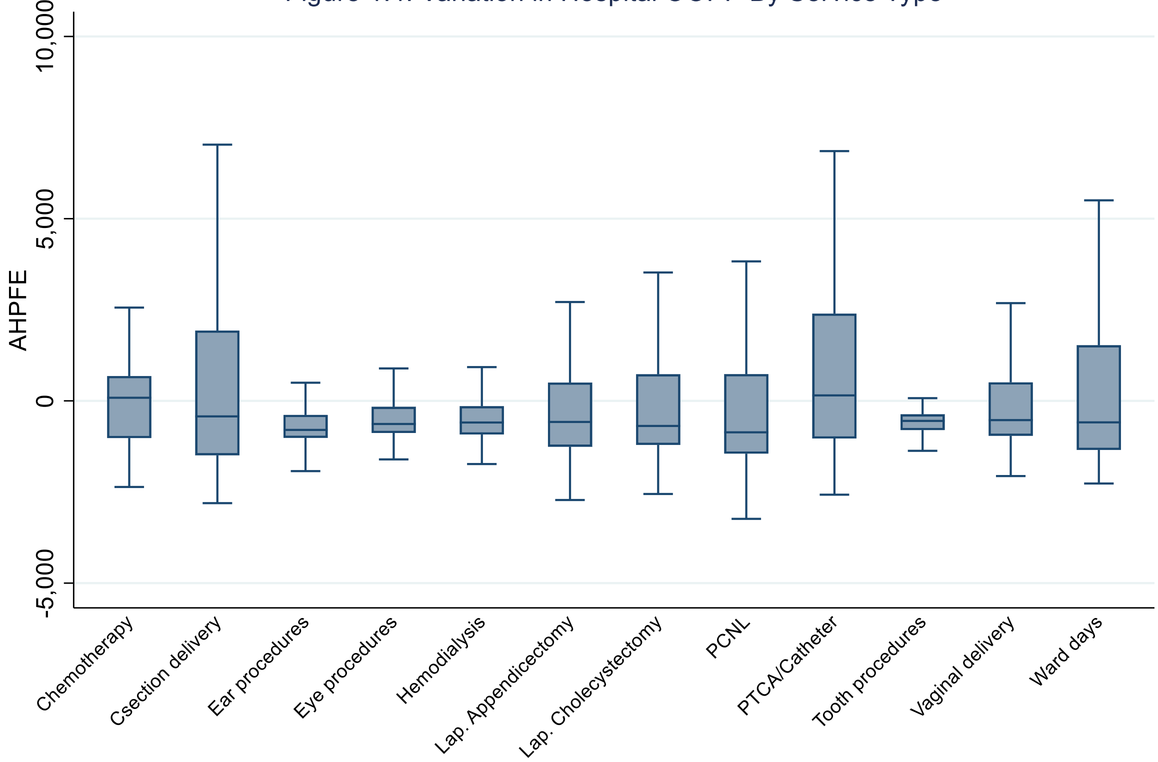
package and month fixed effects, as well as the fully adjusted AHPFE on measures of hospital size, specialization, and market competition. The results are very similar across both specifications, suggesting adjusting for care and patient characteristics does not substantially change hospital-level OOPP. Surprisingly, urban status and total claims, our proxy for hospital size, have no relationship to OOPP. However, the hospital's package and cluster shares of its total claims, our proxy for specialization, are associated with lower OOPP. Hospital market share (its claims as a share of all claims for a service in the district) and the district-level market concentration (the package or cluster specific HHI) are associated with higher OOPP. Although these associations must be interpreted with caution because there may be omitted factors correlated with each of these variables and hospital charges, the results are consistent with economic



theory. Specialization should increase efficiency and reduce the costs of care, which may result in lower charges faced by patients, while hospitals facing less competition are likely to be able to charge more.

In **Figure 1.4** we present the AHPFE by service type to examine whether across-hospital variation is greater for some types of care. Across-hospital variation is clearly lowest for ear, eye, and tooth procedures and dialysis, while it is highest for PTCA (angioplasties) and C-section deliveries, and moderately high for laproscopic procedures, PCNL (kidney stone removal), and ward days. Lower variation is not restricted to lower value services: mean hospital reimbursement rates for ear and eye procedures are comparable to vaginal deliveries, while PCNL rates are much higher than for C-sections (**Figure 1.1**). An alternate explanation may be that relatively standardized types of care have low variation, while surgical procedures have higher variation, and Ward Days vary because they are (reportedly) a catch-all for a range of non-specific hospital care.

Figure 1.4: Variation in Hospital OOPP By Service Type



Graph of AHPFE, or regression adjusted hospital-package OOPP. Outside values excluded. Shaded area is interquartile range.

1.7 Discussion and Conclusion

1.7.1 Limitations

One limitation of our study is that we rely on patient self-reported data for care quality and payments. Although recall up to a month has been found to be reliable for major health events, and we control for survey recall period in all regressions, it is nevertheless likely that noise in our self-reported OOPP measure collected approximately 3 weeks after a hospital visit contributes to the total and unexplained variation in payments (Das et al 2012). Our measures of technical care quality are also limited to factors patients are able to report. However, we do not find that restricting the sample to observations with below-median recall periods changes results substantially (Appendix). While we focused on collecting measures validated in several contexts, including India, we were unable to directly validate our data through direct observations or medical records reviews. There may also be aspects of care complications or quality that contribute to OOPP that we were unable to measure. Furthermore, we use hospital claims data to identify the package of care provided to patients, but these classifications may be incorrect due to hospital error or intentional misreporting. Finally, we do not have information on non-BSBY patients visiting the hospitals we study that could change hospital cost and pricing structure.

1.7.2 Findings and Policy Implications

We use a unique and large dataset of insurance claims linked to post-visit patient surveys to document widespread out-of-pocket payments (OOPP) for care that is supposed to be free under a government health insurance program in Rajasthan, India. Because we focus on OOPP by insured patients that visit an empaneled hospital and receive a service for which the hospital was reimbursed, we can be confident that the reported OOPP is not due to problems in eligibility, enrollment, hospital choice, or type of care received, but is due to hospitals contravening program rules. Furthermore, we can link payments to specific hospitals and are able to decompose the variation in OOPP into the shares across and within hospitals, which allows us to identify the potential sets of factors that could explain OOPP.

We first document substantial levels of OOPP across a range of services that are supposed to be provided for free under BSBY, suggesting that the program is providing incomplete insurance against health-related financial risk. Strikingly, 70% of the total variation in OOPP is across patients receiving the same bundle of services within the same hospital – i.e. it cannot be explained by differences in factors such as local input costs, hospital infrastructure, or market structure. We also examine factors associated with higher OOPP within and across hospitals. Although these relationships cannot be interpreted causally, we examine whether they support different explanations for what drives hospital charges for care under insurance.

Within hospitals, we find positive associations between patient OOPP and measures of patient prior risk and care complexity across services, in support of the hypothesis that hospitals charge more to cover the higher costs of more complicated cases. However, it is also possible that hospitals are able to charge more because these patients have relatively inelastic demand – because they have been referred from elsewhere and have no other options or they arrive too late to go elsewhere. The hypothesis that OOPP is compensating for the costs of higher quality care is unsupported in the data: we also find no relationship with care quality or luxury (private room) and a negative association with patient perceived quality. Although in qualitative interviews, hospitals frequently state that they charge less to patients of lower socioeconomic status who cannot pay, we find that wealth is unassociated with OOPP for non-delivery services and wealth and education are *negatively* associated with OOPP, which is inconsistent with theories of price discrimination. The large negative association with BSBY awareness for child deliveries, even after controlling for other patient and care factors, is consistent with the hypothesis that more informed patients may be able to negotiate lower prices for services that can be planned in advance.

Even with the full set of patient and care characteristics and hospital, package, and time fixed effects, the total explained variation in OOPP is under 50%, leaving a substantial share of the within-hospital variation in OOPP unexplained. This may be because there are aspects of care that we are unable to measure because we rely on patient reports rather than clinical observations. For example, anecdotal evidence and our qualitative interviews find that, particularly for specialty care, hospitals engage visiting consultant clinicians that charge varying prices based on their training and qualifications. Other research finds substantial within-provider variation in the quality of care for child deliveries using direct observation (Sharma et al 2017). The unexplained variation may also reflect the fact that hospitals do not have fixed prices and payment levels are the result of bargaining and patient-specific discounts that we do not measure, as documented by other research in India (Goodman et al 2017).

30% of the overall variation is explained by differences across hospitals, and adjusting for differences in patient and care characteristics does not reduce across-hospital variation much. Variation across hospitals may be driven by differences in the cost of providing care – e.g. differences in local input costs, hospital infrastructure, or hospital productive efficiency. Because BSBY and most health insurance programs in

India do not cost-adjust hospital reimbursements, hospitals with higher average costs may pass some of this on to patients. We do find that measures of hospital specialization, which may increase hospital efficiency and lower its costs, are negatively associated with OOPP. However, our finding that hospital town location only explains 20% of the total variation suggests that local input costs are not a major driver of OOPP. Across-hospital variation may also reflect differences in market structure independent of costs, as competition can induce hospitals to lower profit margins and patient charges. Consistent with this theory, we find higher hospital-level OOPP is positively associated with market power.

We contribute to a growing literature finding that public insurance programs and subsidies for health care provide incomplete financial protection in India and numerous lower income countries (Prinja et al 2017, Nandi et al 2017, Karan et al 2017, Mohanan et al 2014, Acharya et al 2012, Escobar et al 2010). While most studies of the determinants of persistent OOPP have focused on problems in enrollment, implementation, information, and access (Rathi et al 2012, Nandi et al 2013, Rao et al 2014), we provide new evidence on the contribution of hospital charging behavior. We also contribute to a broader literature examining sources of variation in hospital pricing, though these studies have largely focused on developed health systems where prices are negotiated by third party insurers (Cooper et al 2019, Skinner 2012). We find substantially higher within-hospital variation than Cooper et al (2019), which may reflect the fact that hospitals in India are largely unregulated. We also add to the fairly small literature studying the structure of health care markets in lower income countries (Goodman 2018, Das & Hammer 2007, Nakamba 2002), but our findings are consistent with the broader literature on competition (Gaynor 2015 review, Cooper et al 2019).

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APPENDIX

Using only 2018 Claims

In December 2017, the government revised the package list to increase reimbursement rates for many packages (some were left unchanged or decreased). One of the objectives of the reform was to reduce OOPP. For the analysis of variation in OOPP, we pool all survey data from 2017 and 2018 to maximize power, but use month fixed effects in all specifications to ensure we are not comparing across these periods (and are not picking up any other package or cluster specific time trends). However, **Table A1.1 and Figure A1.1** demonstrate that the analysis of overall decomposition and within-hospital OOPP variation is not substantially changed when we use 2018 data alone. Because data collection was ramped up in late 2017, a large share of our data in fact comes from 2018.

Table A1.1: Decomposition of Variation – 2018 Transactions Only

	(1)	(2)	(3)	(4)
	Dependent Variable: Total OOP Payment			
Fixed Effects Level	Package	District	Census	Hospital
R^2	0.042	0.135	0.190	0.293
Unexplained variation	0.958	0.865	0.810	0.707
Observations	13192	13112	12811	12410
Month FE	Yes	Yes	Yes	Yes
Package FE	Yes	No	No	No
District x Package FE	No	Yes	No	No
Census Location x Package FE	No	No	Yes	No
Hospital x Package FE	No	No	No	Yes

Data restricted to claims filed in 2018, after the policy reform to increase hospital reimbursements was in place. Observations are at the transaction level. The dependent variable is the total OOP payment at the hospital and for tests and medicines purchased outside the hospital. All regressions include controls for survey recall period. Census location is an indicator for the town or village Census 2011 location of the hospital. The unexplained variation is 1-R2.

Using OOPP at the Hospital as the Outcome

For the main analysis we use the total OOPP at the hospital as well as for tests and medicines relating to the visit that were purchased outside the hospital. This ensures that we are not comparing different sets of costs across hospitals (if some hospitals systematically do or do not provide the tests and medicines on-site). We present the decomposition of variation (**Table A1.2**) and within-hospital factors (**Figure A1.1**) using only the OOPP at the hospital as the outcome variable below – the results are not substantially different.

Table A1.2: Decomposition of Variation – Payment at Hospital Only

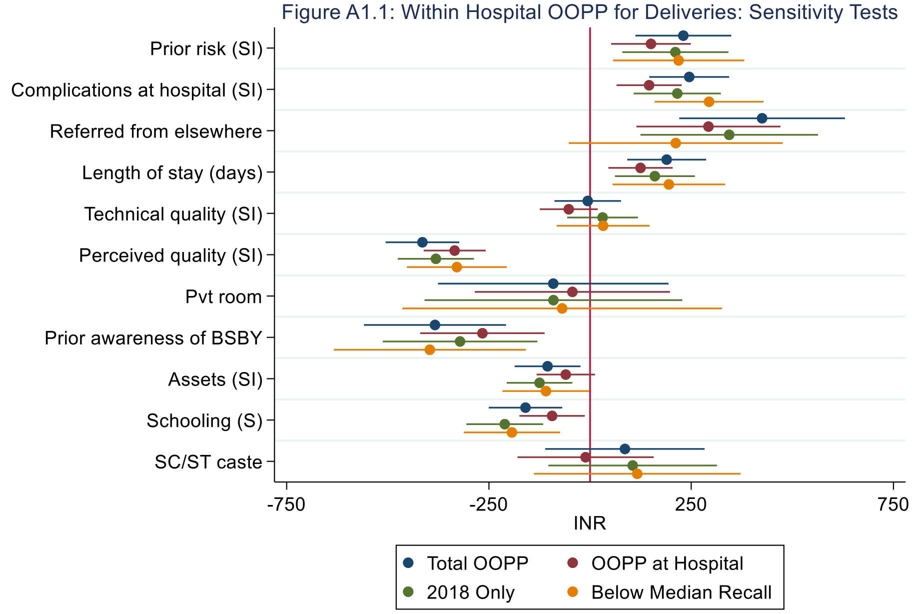
	(1)	(2)	(3)	(4)
		Dependent Variable: OOP Payment at Hospital		
Fixed Effects Level	Package	District	Census	Hospital
R^2	0.058	0.155	0.210	0.298
Unexplained variation	0.942	0.845	0.790	0.702
Observations	17404	17327	17001	16583
Month FE	Yes	Yes	Yes	Yes
Package FE	Yes	No	No	No
District x Package FE	No	Yes	No	No
Census Location x Package FE	No	No	Yes	No
Hospital x Package FE	No	No	No	Yes

OOP payments at the hospital, not including visit-related purchases of tests and medicines off-site, is the outcome variable. Observations are at the transaction level. The dependent variable is the total OOP payment at the hospital and for tests and medicines purchased outside the hospital. All regressions include controls for survey recall period. Census location is an indicator for the town or village Census 2011 location of the hospital. The unexplained variation is 1-R2.

Survey Recall Period:

Although we control for survey recall period in all analyses, we also present the results for the analysis of within-hospital variation for the subsample of observations with below-median recall period in Figure A1.1 below and find results do not change substantially, except for referral from elsewhere, which is smaller and no longer significant. Figure A1.1 presents the main results for the within-hospital variation in OOPP from the paper ("Total OOPP") as well as results when we use only OOPP at the hospital as the outcome, when we restrict the sample to 2018 claims, and when we restrict the sample to observations with below-median survey recall period.

Figure A1.1: Within Hospital OOPP for Deliveries: Sensitivity Tests



Coefficients from regressions of OOPP at hospital on each variable on the y-axis with hospital-package and month fixed effects and controls for recall period, age, and sex. S=standardized; SI=standardized index.

Correlations Between Independent Variables:

<u>To facilitate interpretation of the bivariate and multivariate associations between patient and care characteristics and OOPP, we present the correlations between all independent variables used in the analysis of within-hospital variation in Table A1.3. Measures are positively correlated with each other within each family of explanatory variables - risk/severity, quality, and awareness/socioeconomic status - as expected.</u>

Table A1.3: Correlation Matrix of Patient and Care Characteristics

	Risk / Severity				Quality			Awareness / SES			
	Referred	Length of stay	Prior risk	Complications at hospital	Technical quality	Perceived quality	Pvt room	Prior awareness of BSBY	Asset index	Schooling	SC/ST caste
Referred	1.00										
Length of stay	0.07***	1.00									
Prior risk	0.06***	0.16***	1.00								
Complications at hospital	0.04*	0.08***	0.14***	1.00							
Technical quality	0.00	-0.01	0.01	0.06***	1.00						
Perceived quality	-0.08***	-0.00	-0.04*	-0.03	0.07***	1.00					
Pvt room	0.00	0.03	-0.02	-0.01	0.02	0.04*	1.00				
Prior awareness of BSBY	-0.03*	0.06***	0.01	-0.03	0.01	0.09***	0.01	1.00			
Asset index	-0.10***	-0.00	-0.02	-0.11***	-0.03*	0.09***	0.02	0.06***	1.00		
Schooling	-0.05**	0.01	-0.04*	-0.11***	-0.00	0.04*	0.05**	0.04*	0.33***	1.00	
SC/ST caste	0.05**	0.02	0.01	0.01	-0.00	-0.06***	-0.02	0.03*	-0.14***	-0.07***	1.00

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

PAPER 2: PRIVATE HOSPITAL RESPONSES TO REIMBURSEMENT CHANGES UNDER GOVERNMENT HEALTH INSURANCE IN INDIA

Radhika Jain[4], Pascaline Dupas[5]

Abstract

Expanding public health insurance programs and contracting private hospitals for service delivery is a common policy strategy to meet the goals of universal health coverage. Hospital reimbursement rates are a critical design element of these programs, but the evidence on their effects in lower income countries is limited. Exploiting a policy-induced natural experiment, and using administrative claims linked to patient surveys, we provide the first largescale evidence of private hospital responses to changes in reimbursement rates under public health insurance in India. We find hospitals increase volumes of higher-paid services. In our most conservative estimates, only 60% of the higher public subsidies are passed through to patients in the form of lower cash payments four to eight months after the policy change. We find no evidence of changes in care quality or patient composition that could explain the incomplete pass-through. Pass-through is higher in less concentrated markets, suggesting that competition limits hospital capture. Our results are directly relevant to the recently announced expansion of a similarly structured health insurance program in India to cover the poorest 40% of the population.

2.1 Introduction

As achieving universal health coverage becomes a priority, governments in low and middle-income countries are expanding public health insurance programs and contracting the private sector to deliver care.

This research was made possible through a partnership with the Government of Rajasthan, which we thank for its collaboration. We gratefully acknowledge funding from the Weiss Fund and the JPAL Governance Initiative. The data collection protocols were approved by the IRBs of Harvard, Stanford, and the Institute for Financial Management and Research in India. We thank Mantasha Hussain and Niyati Malhotra for outstanding project management and research assistance, JPAL South Asia/IFMR for hosting the project, and Armelle Grondin and Daniel Wainstock for excellent research assistance. All errors are our own.

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In India, national and state governments have been implementing largescale public health insurance programs that aim to provide free care at empaneled private hospitals to their poorest people since 2007. However, several evaluations of these programs have found no reductions in patient health expenditures, despite substantial public financial outlays (Selvaraj & Karan 2012; La Forgia & Nagpal 2012; Mohanan et al 2014; Karan et al 2017).

One explanation for the persistent patient out-of-pocket (OOP) payments is that hospitals may need to charge patients to compensate for reimbursement rates that are set below their participation constraint (*balance billing*). In an effort to control costs, most public health insurance programs in India adopt bundled payment systems that reimburse hospitals a fixed rate per admission, adjusted for diagnosis and procedure, rather than fee-for-service payments.[6] Under these systems, hospital reimbursement rates are a key policy lever to shape hospital incentives, with significant implications for service volumes, quality, patient selection, and health outcomes (Dranove 1987; Ellis and McGuire 1986; Cutler 1995). In contexts like the U.S. Medicare program, hospital reimbursements are risk-adjusted and based on hospital costs in the previous period, leveraging yardstick competition to control expenditure growth (Shleifer 1985). In India, where detailed hospital cost and patient health data are not collected systematically, reimbursements are based on crude estimates of costs from public hospitals and are unadjusted for local variation in health risk or input costs across the state. Balance billing, even if disallowed by program rules, may allow hospitals to participate and provide subsidized care, but results in incomplete insurance against risk.

If balance billing is they key factor driving OOP payments, a potential policy response to reduce patient financial burden is to increase hospital reimbursement rates. However, an alternate explanation for OOP payments is that hospitals simply capture part of the public subsidies as rents (profits) and charge patients

[6] A large theoretical literature establishes the importance of supply-side cost sharing in managing hospital incentives, and empirical work in both advanced and developing economies finds that switching from cost-based reimbursement to bundled or prospective payment systems can effectively improve productive efficiency and control medical expenses (Ellis & McGuire 1993, Cutler & Zeckhauser 2000, Yip & Eggleston 2001).

extra (*double billing*). Particularly in contexts of low institutional capacity, where the ability to monitor hospitals is limited, patients are poorly informed about their entitlements, and grievance redressal systems are missing, hospitals with market power may double-bill patients for services fully reimbursed under insurance. If double-billing is the key driver of OOP payments, a policy response that increases reimbursement rates will simply be an additional transfer of public resources to hospitals with no resulting benefit to patients.

We exploit a policy-induced natural experiment to examine the effects of increased hospital reimbursements in the context of the BSBY public health insurance program that entitles 46 million low-income individuals to free care at public and empaneled private hospitals in Rajasthan, India. However, in other research, we find OOP payments under the program are substantial and that 40% of patients paid OOP for insured care in 2017, two years after program launch. In December 2017, the government implemented a policy reform that discontinuously changed hospital reimbursements for different procedures by varying magnitudes. Using administrative claims data for the 6 months prior to and 7 months following the policy change linked to post-visit patient surveys, we use a difference-in-differences empirical strategy to examine the effects of hospital reimbursement changes on healthcare volumes, hospital entry, and pass-through into lower OOP or better care.

We find that a 1% increase in the reimbursement for a service induces a 0.3% increase in service volume relative to services with no reimbursement change. Although patient OOP payments decrease significantly, hospitals capture approximately 40% of the increased public reimbursements 4 to 7 months after the reform. In other words, for every 100 INR paid by the government to hospitals, only approximately 60% is passed through to patients in the form of lower OOP payments. Exploring heterogeneity by measures of market competition, we find that pass-through is higher in markets with more hospitals and lower market concentration. We find no evidence of changes in care quality or patient risk factors that would suggest that hospitals are improving care or accepting costlier patients as alternate forms of pass-through. Finally, we find no meaningful changes in the socioeconomic and demographic composition of patients in the program.

We provide the first quantitative evidence on how private hospitals respond to changes in reimbursement rates under government health insurance in India. Taken together, our results suggest that 1) health care volumes respond to both absolute and relative changes in prices, 2) balance billing can only partially explain the observed OOP charges to patients, and hospital capture of subsidies contributes substantially, and 3) market structure, a factor rarely taken into account in the design of health insurance in lower income contexts, may affect the extent to which patients benefit from health insurance subsidies.

We contribute to the literature estimating effects of increases in subsidies for public insurance plans, which has been focused on the U.S. to date. Duggan et al. (2016) and Cabral et al (2018) study the extent to which increased government payments to Medicare Advantage insurance private providers benefit patients. Their estimates of pass-through are similar to ours, with 54% of the increased payments resulting in either lower premiums or more generous benefits (Cabral et al 2018). Both studies also find that pass-through is higher in more competitive markets. However, our study focuses on hospitals rather than Insurers and presents evidence from an institutional context where enforcement of government policies is substantially weaker.

We also contribute to the literature on the challenges to implementing public subsidies in settings with weak state capacity. Limited pass-through of government subsidies has been widely documented in the context of food distribution schemes (Olken 2006 and Banerjee et al. 2018 in Indonesia, Nagavarapu and Sekhri (2016) in India), education (Reinikka and Svensson 2004 in Uganda, Ferraz et al. 2016 in Brazil), and maternity benefits (Mohanan et al. 2014). Banerjee et al find that contracting private agents to deliver public benefits was only effective at reducing the prices beneficiaries face when competition was encouraged (Banerjee et al 2017). Much less work has concerned health insurance programs. Gertler and Solon (2002) document substantial capture of health insurance benefits by providers in the Philippines, while other studies find muted or null effects of health insurance on household financial risk, but cannot document the extent to which this is driven by provider capture of benefits (Thornton et al. 2010; Karan et al 2017).

The rest of the paper is organized as follows. Section 2.2 provides background information on the insurance scheme under study, and the policy reform we exploit. Section 2.3 presents the conceptual framework guiding our analysis. Section 2.4 describes the data and Section 2.5 the empirical strategy. The results are presented in Section 2.6 and discussed in Section 7. We conclude in Section 7.

2.2 The BSBY Program and Policy Reform

In December 2015, the Government of Rajasthan, a state of 70 million in western India, launched a statewide public health insurance program that provides cashless secondary and tertiary care to low-income households at public and empaneled private hospitals. Hospitals are reimbursed at fixed rates for predefined bundles of services ("packages") that are supposed to include all procedures, tests, and drugs for a visit. Rates are set by a panel of public health officials and are unadjusted for local input costs or patient case-mix. Hospitals can also choose not to accept some or all patients under BSBY. Households are automatically enrolled based on poverty status, pay no premium, and are entitled to up to INR30,000 (~$460) in secondary and INR100,000 (~$1500) in tertiary care per year with no cash payments. The same package amounts reimbursed to hospitals for care are deducted from the household's annual benefit balance. The New India Assurance Company (hereafter the Insurer), one of India's largest public health insurers, was chosen following a standard public procurement process. Premiums are paid by the government directly to the Insurer on behalf of all eligible households. The Insurer is responsible for empaneling hospitals, publicizing the program, and reviewing hospital claims.[7] Claim filing, Insurer review and approval, and hospital reimbursement for the prespecified package rate are all managed electronically through an IT system designed and managed by the government.

[7] The Insurer's contract requires 2% of the entire premium paid to be spent on information, education, and communication activities, ensuring that it has an incentive to publicize the program. The government also conducted mass media publicity campaigns and tasked village health workers with information dissemination.

In December 2017, the first 2-year phase of the program ended, and the program was renewed for another three years. The primary change between Phases 1 and 2 was the revision of the list of packages covered by the program and corresponding hospital reimbursement rates. Packages that were considered redundant were eliminated or collapsed into single packages and some new packages were added. Rate changes were determined by a panel of government medical staff based on rates used by government insurance programs in other states, estimates of costs of treatment in public facilities, and consultations with private hospitals. The planned reimbursements were finalized and shared with us confidentially in August 2017, shared with hospitals in early December, and went into effect on December 13, 2017. Because reimbursements are managed electronically, all claims filed after this date were immediately and automatically reimbursed at the new rates.

The government issued a new RFP for an Insurer, but decided to continue with the same Insurer it contracted in Phase 1, and the Insurer's responsibilities remained the same. Premiums increased but were paid by the government to the Insurer and did not affect households or hospitals. All other terms of contract remained the same. The program's IT backbone remained the same and continued to be managed by the government. The household annual benefit balance was wiped clear and renewed, with no changes in the annual cap. Hospital empanelment criteria changed slightly to allow smaller facilities to participate. Additionally, public hospitals are no longer reimbursed for child deliveries under BSBY in Phase 2 because they are already paid to provide free maternity services under a national conditional cash transfer program to incentivize institutional deliveries, and the BSBY payments were considered a double transfer to hospitals. The government made a renewed effort to publicize the program through the media and health workers in the months leading up to and soon after the reform.

2.3 Conceptual Framework

We draw on models of hospital incentives under prospective payment systems from the Medicare literature (Dranove 1987), and models of pass-through that have been used, for example, to study effects of increases in subsidies for Medicare Advantage insurance plans (Weyl & Fabinger 2013, Cabral et al 2017). We begin with the assumption of perfect competition, where a hospital sets prices equal to marginal cost. The marginal cost of care depends on the patient's illness severity and can vary within a package, although the package reimbursement rate does not. If the reimbursement rate for a package is set lower than marginal cost, the hospital has the option to not provide the package ("risk selection") or provide it and charge patients additional cash to make up the difference between the reimbursement rate and marginal cost ("balance billing"). When the reimbursement rate for a package increases, the hospital may lower the amount it charges patients (pass-through into OOP). It may also improve the quality of care or accept riskier, higher cost patients that it previously rejected (pass-through into care). Both forms of pass-through could induce a demand response by patients sensitive to price or quality and would result in a change in the composition of the patient pool.

The assumption of perfect competition may not be realistic for secondary and tertiary health care markets that have barriers to entry. Under imperfect competition, firms with market power may not face pressure to reduce prices or improve quality, and may set prices above marginal cost, which could reduce the pass-through rate (Weyl & Fabinger 2013). Studies of Medicare Advantage, for example, find that private insurers pass through as little as 13% of increased government payments, but that this increases considerably in the most competitive markets (Cabral et al, 2014, Duggan et al 2016). Loosening the assumption of perfect competition thus leads to the prediction that pass-through will be higher in more concentrated markets.

2.4 Data

We use a combination of administrative claims data and phone surveys of patients soon after they visit the hospital. We restrict analysis to the period from June 2017 to July 2018, providing us with 6 months of pre-reform data and 7 months of post-reform data, for which we have both claims and survey data.

2.4.1 Administrative Claims Data

As part of our partnership with the government, we received access to the universe of claims filed since program inception, as well as complete, updated claims data on a roughly biweekly basis. These data include unique ID, name, and contact information for the patient; unique ID, name, and district location for the hospital; and unique ID, package of care claimed, reimbursement rate, and filing date for each transaction. Because the package list changed across Phases 1 and 2 (some were eliminated or added), we first matched packages across phases (the process is described in detail in the Appendix).

As with most bundled payments systems, there are typically several packages for a given type of care to cover different types of treatment the patient may need. For example, vaginal deliveries have separate packages for "Vaginal basic", "Vaginal + episiotomy", "Vaginal + forceps delivery", "Vaginal + pre-eclampsia management" and so on. We call groups of closely related packages "clusters" (all the above packages fall into the "vaginal delivery" cluster). For all packages included in our sample, we identified and ensured there was a match for all closely-related packages so that clusters are complete (this is important for survey sampling discussed below). Because our analysis relies on claims linked to patient surveys, we restricted our study sample to 61 fully matched packages across 16 clusters, ensuring that the highest volume services were included. Although these comprise a relatively small share of all BSBY packages, they account for 72% of all claims filed during the study period. We then calculated the reimbursement rate change across Phases 1 and 2 for each package (described in the Appendix). Descriptive information on the services included in our study sample and corresponding reimbursement rates are presented in the Appendix (although our analysis is at the package level, we present the summary at the cluster-level for simplicity).

2.4.2 Survey Data

Using the administrative data that we received from the government approximately every 2 weeks, we restricted claims to those filed at private hospitals, stratified them by cluster, and randomly sampled a fixed number of transactions within each cluster for survey. This ensures that all clusters are equally represented, but higher volume packages are sampled with higher probability (though we adjust for sampling probability in all regressions). We started patient surveys for the vaginal delivery and c-section delivery clusters in late June 2017, and added the remaining non-delivery clusters in mid-September 2017, once the government informed us of the upcoming reform (the full list of clusters and packages is in the Appendix). Our total survey sample was 24,461 transactions, which comprise 27% of all claimed delivery transactions and 7% of all claimed non-delivery transactions.

Surveys were conducted by phone using patient phone numbers included in the administrative data, and were completed within 3 weeks of the claim being filed to reduce recall bias (Das et al 2011).[8] Surveys collected information on patient residence, demographics, care received, cash paid, perceived quality of care, length of hospital stay, knowledge of the insurance program, hospital utilization and morbidity in the previous year, and socioeconomic status (assets, education, caste, and religion). Child delivery-related claim surveys included more detail on facility choice, prior risk factors, complications at the hospital, delivery type (vaginal or cesarean section), care components, and measures of WHO recommended quality.

2.4.3 Summary Statistics

Figure 2.1 shows the number of hospitals filing each month, separated into hospitals that filed before and after December 2017 (Panel hospitals), those that stopped filing in December 2017 (Dropouts), and those that started filing in or after December 2017 (Entrants). We see substantial hospital entry, with approximately 250 new hospitals filing claims between the date of the policy reform and July 2018. This may be partly due to the expansion of hospital eligibility criteria to include smaller facilities discussed earlier. However, **Figure 2.2**, which presents total transactions in the claims data for the packages in our study sample, shows that the new hospitals started out filing very few claims and accounted for about 15%

[8] The average time between claim filing and survey completion was 25 days, and decreased from 27 days in Phase 1 to 24 days in Phase 2, as our surveying procedures got better. We control for recall period in all analysis of survey data.

of total claims by the end of the study period. Hospitals that dropped out in the six months before the policy reform appear to be low-volume hospitals.

Figure 1: Hospitals Filing Each Month

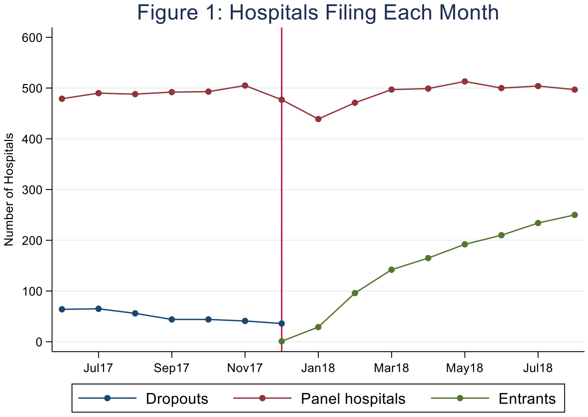


Figure 2: Total monthly transactions

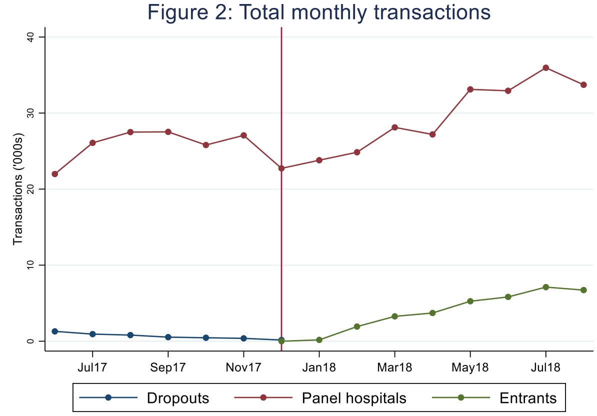


Table 2.1, Panel A, presents descriptive statistics for the panel of hospital-month-package level claims. We have data from 936 hospitals for 61 packages. Hospitals file just over 7 claims per package per month, on average, though the high standard deviation reflects substantial volume differences across hospitals, packages, and months. Note that because we use the claims data, we do not have information on hospitals that are empaneled but not filing any claims. Panel B presents statistics from the survey data. 67% of the transactions sampled for survey were successfully completed, with attrition largely due to incorrect phone

Table 2.1: Summary Statistics

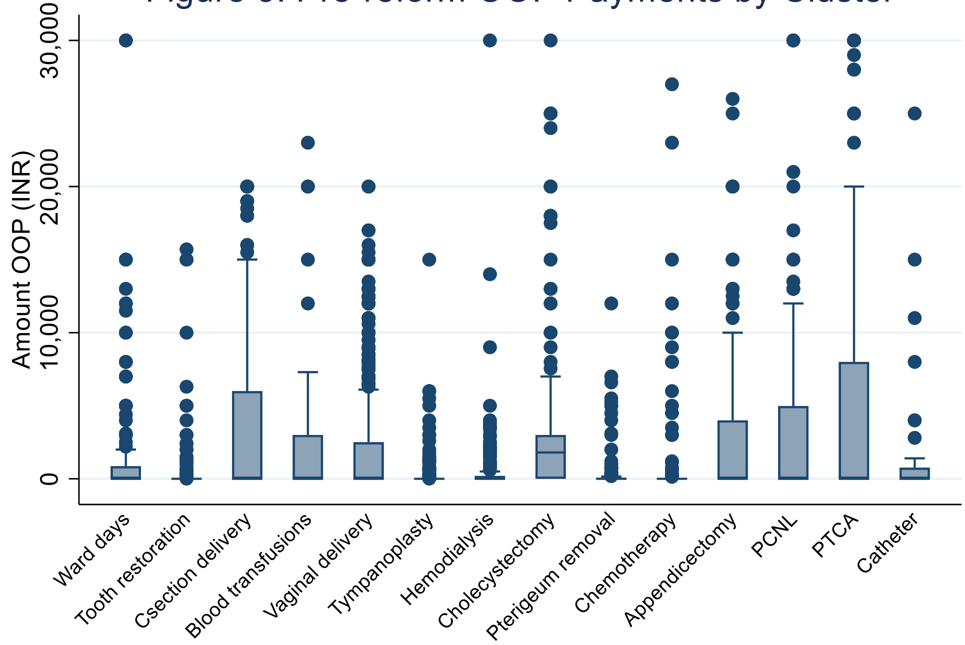
Panel A: Claims Data		Full Sample		
	N	Mean	Median	SD
Phase 1 & 2 panel hospitals	564			
Phase 1 dropout hospitals	90			
Phase 2 entrant hospitals	257			
Number of packages	61			
Monthly hospital package claims		7.67	0.00	62.51
Observations	116536			

Panel B: Survey Data	Pre-Reform			Post-Reform			Full Sample		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Surveyed successfully	0.61		0.49	0.69		0.46	0.67		0.47
Recall period (days)	25.99	25.00	9.91	24.23	23.00	10.95	24.65	23.00	10.74
Any OOP	0.36		0.48	0.22		0.41	0.25		0.43
OOP Amount	1542.46	0.00	3952.23	960.08	0.00	3228.29	1085.19	0.00	3405.16
OOP Amount Among Payers	4488.39	2500.00	5678.07	4552.09	2200.00	5750.67	4532.45	2200.00	5727.85
Hospital Reimbursement	6997.61	3750.00	10815.76	8086.14	3750.00	15323.48	7825.20	3750.00	14379.65
Cash markup	0.40	0.00	1.30	0.34	0.00	1.93	0.35	0.00	1.81
Observations	6977			17484			24461		

The claims data is an unbalanced panel of hospitals, including hospitals that were participating both pre- and post-reform and hospitals that only participated in one of the two periods. There is an observation for every package and every month in the study period for each hospital that ever filed a claim for a package. Hospitals that did not file a claim every month will have zeroes for claim volumes in that month. Cash markup is OOP as a percentage of the hospital reimbursement.

numbers in the administrative data, and average recall period was about 25 days. Patient payments in Phase 1 were substantial: 36% of patients paid some OOP and mean payments were INR 2007 ($31) overall, though the median payment was zero. This constitutes a 40% markup on the hospital reimbursement rate on average. **Figure 2.3** reveals considerable heterogeneity in the distribution of patient payments by cluster. Hospital reimbursements averaged almost INR 7,000 ($108) per transaction in the pre-reform period.

Figure 3: Pre-reform OOP Payments by Cluster



2.5 Empirical Strategy

Our empirical strategy exploits the variation in reimbursement rate changes across 61 packages between Phase 1 and 2. **Figure A2.1** in the Appendix demonstrates that there is substantial variation in the magnitude of rate change, with several remaining unchanged and some experiencing rate decreases. We use a generalized difference-in-differences (DID) empirical strategy, where the treatment is a continuous measure of rate change (treatment intensity) at the package level. Our study period spans the months June 2017 through July 2018, for which we have complete survey data. To allow for effects to change flexibly over time, we use three post-reform dummies. Post-reform short run (SR) is a dummy for January and February

2018, post-reform medium run (MR) is a dummy for March and April and zero otherwise, and post-reform long run (LR) is a dummy for May through July 2018, when our data ends. Because the reform took effect in the middle of the month, we drop December 2017 from the analysis.

To analyze whether increases in the package reimbursement rate led to higher claims volumes, we use the claims data collapsed to the package-hospital-month level and use the following specification:

$$Y_{pht} = \alpha_0 + \beta_1 RateChange_p * PostSR_t + \beta_2 RateChange_p * PostMR_t + \beta_3 RateChange_p * PostLR_t + \gamma_p + \delta_t + e_{hpt}$$

where Y_{pht} is the outcome for package *p* in hospital *h* in month *t*; *RateChange*PostSR/MR/LR* is the absolute change in rates between Phase 1 and Phase 2 in rupee terms interacted with each of the Post dummies described above; are package fixed effects; are *PostSR/MR/LR* fixed effects; and e_{pht} is the error term. Standard errors are clustered at the package level. The outcome is the hospital's monthly volume of claims for a package. β_1, β_2, and β_3 are the coefficients of interest and represent the change in the outcome for every unit increase in package rate change. We also include the pre-reform mean and the p-value on an F-test for joint significance of the three Rate Change x PostSR/MR/LR interaction terms in all tables. To allow for the possibility that responses to positive and negative rate changes may not be symmetric, we also present results of additional specification with separate interactions of positive and negative rate change with the post dummies in the Appendix.

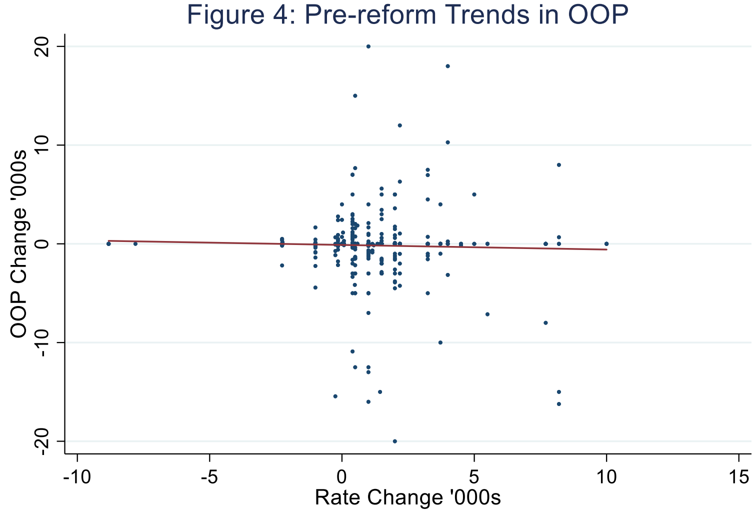
To examine pass-through and changes in patient composition, we use the survey data with patient level transactions linked to the claim data for that transaction. Our main DID specification for analysis of the linked claims-survey data is as follows:

$$Y_{ipht} = \alpha_0 + \beta_1 RateChange_p * PostSR_t + \beta_2 RateChange_p * PostMR_t + \beta_3 RateChange_p * PostLR_t + \gamma_p + \delta_t + e_{ihpt}$$

where Y_{ipht} is the outcome for patient i that received package p in hospital h in month t, and all other terms are as above. All survey-based analysis includes weights for survey sampling probability and controls for recall period (days between claim filing and survey).

The identifying assumption in the DID empirical strategy is that packages that experienced different degrees of rate changes have outcomes on parallel trends pre-reform, and that in the absence of the rate changes, they would have continued on these trends post-reform. We cannot test the second assumption, but our pre-reform data allow us to look for evidence of the first assumption. A visual examination of the change in mean monthly OOP payments between June and November 2017 for each hospital and package suggests that, while OOP did fluctuate over the pre-reform period, this is unrelated to package rate change, our treatment variable (**Figure 2.4**). To examine the parallel trends assumption statistically for each of our key outcomes, we restrict our claims and survey samples to the pre-reform period and use the same DID specification with dummies for each pre-reform month interacted with the Rate Change treatment variable. The results presented in **Table 2.2** show that none of the Rate Change x Month dummy interactions, nor the p-values on the F-tests for joint significance of all the interactions, are significant, giving us confidence that our key outcomes were on parallel trends in Phase 1, prior to the policy reform (estimates are presented graphically in the Appendix).

Figure 4: Pre-reform Trends in OOP



Each dot is a hospital-package. Change in mean monthly OOP over the pre-reform period is on the y-axis.

Table 2.2: Testing for Pre-reform Parallel Trends in Volume and OOP Payments

	(1) Claims Volume	(2) Amount OOP
Jun17 x Rate change	0.191	0.26
	(0.125)	(0.43)
Jul17 x Rate change	0.106	0.16
	(0.099)	(0.20)
Aug17 x Rate change	0.061	0.06
	(0.141)	(0.23)
Sep17 x Rate change	0.094	-0.03
	(0.064)	(0.35)
Oct17 x Rate change	0.054	-0.16
	(0.037)	(0.20)
Nov17 x Rate change	Reference	Reference
Observations	49272	2634
Pre-reform mean	6.293	1963.52
F-test on rate change x month	0.184	0.91

Observations are at the transaction level. Difference in differences specification with sample restricted to pre-reform claims and interactions of the Rate Change treatment variable with month dummies. November 2017 as the excluded reference group. Standard errors clustered at the package level in parentheses.

One concern with our empirical strategy is that the policy-reform may have changed other factors that are correlated with both rate change and our outcomes of interest. In particular, if the Insurer increased monitoring of packages with higher rate changes this could potentially affect claims volumes and OOP. We find no evidence for differential changes in claims rejections, a proxy for monitoring, by package rate change (results in the Appendix).

2.6 Results

2.6.1 Package Volumes

We first examine the effects of the reimbursement rate changes on package volumes in **Table 2.3**. Columns 1-2 reports claims filed by the panel of hospitals that were participating before and after the program, and Columns 3-4 include all claims, including those filed by hospitals that entered after the reform. The supply response was substantial and immediate: among panel hospitals, a 1% increase in a package's reimbursement rate induced a 0.3% increase in monthly hospital claims volumes for that package in the first two months after the policy change. Including new hospitals that entered in Phase 2 to the sample reduces the size of the coefficients, which means that the volume responses to rate changes were driven entirely by hospitals already participating in the program. This suggests that despite the substantial hospital entry observed in Phase 2, the new entrants are not filing more claims for higher reimbursement change packages. This may be because entry was driven more by the change in hospital empanelment requirements to allow smaller facilities or because the new entrants are not yet established enough to file many claims. Given that the volume changes are almost immediate and are specific to packages that experienced a reimbursement increase, it is unlikely that this reflects a sudden increase in demand. Instead, it is possible hospitals are accepting or attracting patients in need of the packages that are now more profitable. This is plausible even in the relatively short run in our context: care seeking is relatively low and the pool of potential new patients may be high (particularly if eligible households are unaware of BSBY benefits). It is common for hospitals to hold health camps in rural areas to identify patients needing care and to pay lower level providers to refer patients to them. Hospitals may also be providing more of these more profitable packages than patients actually need ("overprovision"). A third possibility is that this supply response reflects changes in "upcoding", where a hospital files a claim for a higher-value package than that actually provided. We discuss the evidence for this and its implications for pass-through estimates in Section A3 of the Appendix.

Table 2.3: Effect of Rate Change on Volume

	(1)	(2)	(3)	(4)
	Panel Hospitals		All Hospitals	
	Claims	Log Claims	Claims	Log Claims
% Rate change x Post (SR)	0.050*	0.003	0.037	0.002
	(0.030)	(0.002)	(0.024)	(0.002)
% Rate change x Post (MR)	0.036*	0.003	0.012	0.001
	(0.018)	(0.002)	(0.014)	(0.002)
% Rate change x Post (LR)	0.022	0.003	-0.005	0.002
	(0.018)	(0.002)	(0.016)	(0.002)
Observations	79287	79287	106756	106756
Pre-reform mean	8.190	8.190	6.293	6.293
F-test on rate change x post	0.106	0.494	0.062	0.356

Observations are at the hospital package month level. Rate change x post is the interaction of the package-specific percent change in rates pre- and post-reform and a post reform dummy. Post-reform (SR) is a dummy the short-run period Jan-Feb2018, post-reform (MR) is a dummy for Mar-May2018, and post-reform (LR) is a dummy for Jun-Aug2018. Dec2017 is dropped. In Columns 1-2 the sample is restricted to the panel of hospitals that filed claims both pre- and post-reform; in Columns 3-4 all hospitals, including post-reform entrants, are included. Standard errors clustered at the package level are in parentheses.

2.6.2 Pass-through into OOP Payments

Table 2.4 presents the results of the DID specification with a continuous measure of patient OOP payments and a dummy for any payment as the dependent variables.[9] We present both OLS and Tobit estimates.

[9] We confirm in Table A1 in the Appendix that a 1 INR increase in the package rate change translates into a 1 INR increase in hospital reimbursements, so all DID estimates can be interpreted as the effect of a 1 INR change in hospital reimbursement for a package.

Hospitals that were charging no OOP, or were charging OOP rates below the package rate increase, cannot charge patients negative prices in the post-reform period, so Tobit estimates allow for censoring of the continuous OOP payment measure at zero. Higher reimbursements lead to immediate decreases in OOP: in the two months following the policy reform, patients pay INR 0.30 less for every INR1 increase in package rate (i.e. we observe a pass-through rate of 30% and we can reject rates between 21% and 39% at the 95% confidence level). The estimate increases slightly over time and by the LR period, 4 to 8 months after the policy reform, pass-through is 38%, and we can rule out pass-through of more than 48% with a 95% level of confidence. The probability of any payment also decreases by 2-3 percentage points. Estimates are only slightly lower when we restrict the sample to panel hospitals that were filing before and after the reform, suggesting lower charges by newly entering hospitals are not driving the effects (the sample size is also not much smaller because, as discussed earlier, new entrants comprise a relatively small share of all transactions). The Tobit estimates are higher, with 40% pass-through immediately and 60% pass-through in the LR period, but still imply far from complete pass-through. In other words, in our most lenient specification, we find that for every additional 100 INR paid by the government for a package, 60% of it is transferred to patients and 40% is captured by hospitals.

Table 2.4: Pass-Through Into OOP Payments

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS Estimates				Tobit Estimates	
	All Hospitals		Panel Hospitals		All Hospitals	Panel Hospitals
	Amount OOP	Any OOP	Amount OOP	Any OOP	Amount OOP	Amount OOP
Rate change x Post (SR)	-0.30**	-0.02**	-0.28**	-0.01*	-0.40*	-0.35
	(0.09)	(0.01)	(0.09)	(0.01)	(0.22)	(0.22)
Rate change x Post (MR)	-0.40***	-0.02**	-0.39***	-0.01*	-0.51**	-0.41*
	(0.11)	(0.01)	(0.10)	(0.01)	(0.23)	(0.24)
Rate change x Post (LR)	-0.38***	-0.03**	-0.34***	-0.02**	-0.60**	-0.53**
	(0.10)	(0.01)	(0.09)	(0.01)	(0.24)	(0.22)
Observations	9612	9700	8757	8839	9616	8761
Pre-reform mean	1963.52	0.40	1900.59	0.39	1963.52	1900.59
F-test on rate change x post	0.00	0.05	0.00	0.09	0.08	0.13

Observations are at the transaction level. PostSR is a dummy for January-February 2018, PostMR is a dummy for March-April 2018, and PostLR is a dummy for May-July 2018. The table presents coefficients on the interaction of Rate Change with each post dummy from a DID specification. Survey sampling weights included. Standard errors clustered at the package level in parentheses.

2.6.3 Heterogeneity by competition

We examine whether market structure can explain incomplete pass-through. Hospitals in more competitive markets should have a greater incentive to lower prices faced by patients and pass through public subsidies.

Studies of Medicare Advantage find substantially higher pass-through in more competitive markets (Duggan et al 2016, Cabral et al 2018). We create two measures of pre-reform market competition to examine heterogeneity in pass-through. First, we calculate a district-package level Herfindahl-Hirschman Index (HHI) using the number of pre-reform claims filed. The HHI is the sum of the squares of market-share (in our case, claims share) of all hospitals for each package within a district. A higher HHI represents higher market concentration (lower competition). Second, we generate a district-package level hospital density measures that is the number of hospitals providing a package in a district in the pre-reform period. In both cases, creating package-specific competition measures ensures that we only consider hospitals providing the same service as competitors. We create our competition measures at the district level because the health system in India is roughly organized around them. The district administrative center is typically the largest town, where the largest public and private hospitals are located. Because these facilities attract patients from around the district, particularly those with the most complications, and serve as referral centers for smaller facilities, analysis at a smaller unit would not capture the full market. We only use pre-reform claims to ensure that changes in concentration as a result of the policy reform do not confound our estimates.

Table 2.5, Panel A**,** presents results from the same DID specifications, but splits the sample into HHI terciles, where higher terciles represent higher HHI values. Panel B splits the sample into hospital density terciles, where higher density terciles reflect a higher number of hospitals providing a package. We use Tobit regressions to allow for censoring and a single post dummy for clarity. Both measures of competition are associated with higher pass-through and we observe 71% pass-through (42% to 100% at the 95% level) in the highest competition sub-samples. While these results cannot be interpreted causally, as there may be other factors correlated with competition and OOP payments, they provide suggestive evidence that market structure plays a role in shaping hospital incentives, and that policies to increase competition may be effective at increasing pass-through.

Table 2.5: Heterogeneity by Market Competition

Panel A: Tobit Estimates of Pass Through by Pre-Reform Market Concentration

	(1) Lowest HHI Tercile	(2) 2nd Tercile	(3) Highest HHI Tercile
Rate change x Post	-0.71**	-0.46	-0.51**
	(0.29)	(0.29)	(0.23)
Observations	3222	3128	3266
Pre-reform mean	2023.40	1880.99	1995.11

Panel B: Tobit Estimates of Pass Through by Pre-Reform Number of Hospitals in Market

	(1) Highest Hospital Density Tercile	(2) 2nd Tercile	(3) Lowest Hospital Density Tercile
Rate change x Post	-0.71**	-0.54	-0.40
	(0.31)	(0.33)	(0.35)
Observations	3153	2602	3861
Pre-reform mean	1482.78	2491.48	1868.66

Observations are at the transaction level. We run the same DID specification as elsewhere, but use a single post-reform dummy instead of 3 separate PostSR/MR/LR dummies for clarity. The Herfindahl Index (HHI) is calculated at the district level (where the hospital is located) for each package, to generate a package-specific measure of district-level market concentration. An HHI of 1 represents a single monopolistic hospital, or complete concentration. In Panel A the sample is split into HHI terciles, where higher terciles represent higher HHI values and higher market concentration (lower competition). In Panel B the sample is split into terciles of hospital density. Density is also calculated at the district level and is the pre-reform number of hospitals filing claims for a package. Higher hospital density terciles represent a higher number of hospitals (higher competition). Standard errors clustered at the package level in parentheses.

2.6.4 Quality of Care and Patient Health Risk

If treatment cost is heterogeneous within a package due to patient characteristics, the marginal cost of treating a patient varies though the reimbursement does not, and hospitals benefit less from treating high-cost than low-cost patients (Dranove 1987). This creates incentives for hospital to turn away riskier, high cost patients to the extent that they can be identified before admission. When reimbursement rates increase, hospitals may choose to accept these patients as another form of pass-through. Pass-through may also occur on the intensive margin in the form of higher quality, either because rate increases enable the hospital to spend more per patient or because hospitals engage in quality competition to attract patients to higher reimbursed packages. We use the following measures of quality for our full survey sample: length of stay in nights, a composite index for ‘luxury’ (AC room, private room, own bed), and a composite index for perceived quality (very respectful, very clean, very satisfied, would recommend). We were able to collect more detailed survey data for the sub-sample of delivery patients, and construct an index of self-reported technical quality (seen by a doctor, skin-to-skin care, labor companion, warned of postpartum symptoms, called back for a checkup), a dummy for any prior risk factors (high BP, prior stillbirth or c-section, or last delivery 10+ years ago) and a dummy for any complications at the hospital (heavy bleeding, fainting, convulsions, or multiparous birth).[10] All indices are the first component of a principal component analysis, standardized to have mean 0 and SD 1.

Table 2.6: Effects on Care Quality

	(1)	(2)	(3)	(4)	(5)	(6)
		Full Sample			Deliveries Sub-Sample	
	Reported Length of Stay	Luxury index	Perceived quality index	Technical quality index	Any prior risk factor	Any complication
Rate change (000s) x Post (SR)	0.04	0.03	0.04	-0.05	0.02	-0.04
	(0.03)	(0.04)	(0.03)	(0.05)	(0.02)	(0.02)
Rate change (000s) x Post (MR)	0.04*	0.01	0.04*	-0.06	0.01	0.00
	(0.02)	(0.04)	(0.02)	(0.05)	(0.02)	(0.02)
Rate change (000s) x Post (LR)	0.04*	-0.01	0.05**	-0.04	0.01	-0.00
	(0.02)	(0.03)	(0.02)	(0.05)	(0.02)	(0.02)
Observations	9406	7132	8127	5236	5240	5241
Pre-reform mean	2.43	-0.04	-0.19	-0.09	0.34	0.53
F-test on rate change x post	0.21	0.23	0.12	0.67	0.61	0.20

Observations are at the individual transaction level. Length of stay is the self-reported number of days at the hospital. The indices are the first component of a PCA of several dummies that have then been standardized to have mean 0 and SD 1. Luxury includes dummies for private room, AC room, and own bed; Perceived quality includes dummies for patient reporting the facility is very clean, staff were very respectful, she is very satisfied with her visit, and she would recommend the hospital to others. Technical quality includes dummies for whether the patient was seen by a doctor, encouraged to do skin-to-skin care, was allowed a labor companion, warned of dangerous postpartum symptoms, and called back for a checkup. Any prior risk is a dummy that equals one if the patient had high BP, prior stillbirth, prior C-section, or her last delivery was 10+ years ago. Any complication is a dummy that equals one if the patient had heavy bleeding, fainting, convulsions, or a multiparous birth. Standard errors clustered at the package level in parentheses.

Table 2.6 presents care intensity and quality measures for the full survey sample in the first 3 columns and the additional quality and patient health risk measures available for the delivery sub-sample in the last 3 columns. We detect a significant 4 percentage point increase in LOS, though this is only a 1.6% change over the pre-reform mean, an uptick in patient perceived quality. We find no changes in any other measures of risk, complications, or technical quality. Perceived quality may have increased because patients are paying less for their care or because hospitals increased respectfulness and cleanliness to attract patients. Overall, we interpret these findings to mean that, while patients may be more satisfied with their care, quality improvements were not a channel for pass-through.

2.6.5 Changes in Patient Socioeconomic and Demographic Composition

Because OOP charges are known to deter poorer patients from seeking care, we examine whether lower OOP due to pass-through of higher reimbursements moves households down the demand curve and enables poorer patients to obtain care under BSBY.[11] **Table 2.7** presents the effects of rate change on patient age,

[10] Complications are an outcome of care, but may also reflect prior risk factors not fully captured in our risk measure.

[11] Note that, if lower willingness-to-pay is correlated with higher costs of care (advantageous selection), this may also change the average cost of care within a package and affect the pass-through estimates.

gender, an asset index (the standardized first component of a principal-component analysis of 12 indicators of asset ownership), schooling (standardized), low caste (indicator scheduled caste or tribe), and an indicator for whether the patient was aware of BSBY prior to her visit. We find no meaningful changes in patient demographic or SES composition.

Table 2.7: Effects on Patient Demographic and Socioeconomic Status

	(1) Age	(2) Female	(3) Asset index	(4) Schooling	(5) Low Caste	(6) Knew of BSBY before visit
Rate change (000s) x Post (SR)	-0.11	-0.00	0.02	0.03	-0.01	0.03
	(0.22)	(0.01)	(0.01)	(0.03)	(0.01)	(0.02)
Rate change (000s) x Post (MR)	-0.32	-0.01	0.02	0.02	-0.00	0.01
	(0.32)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
Rate change (000s) x Post (LR)	-0.06	-0.00	-0.01	-0.00	-0.01	0.02*
	(0.21)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Observations	9350	9374	8989	9512	9139	9172
Pre-reform mean	30.71	0.81	-0.10	-0.00	0.28	0.56
F-test on rate change x post	0.68	0.63	0.15	0.31	0.09	0.36

Observations are at the individual transaction level. The Asset Index is the first component of a PCA of 12 indicators of asset ownership that has been standardized to have mean 0 and SD 1. Standard errors clustered at the package level in parentheses.

2.7 Discussion

Given that the costs of provision of a package are unlikely to change discontinuously at the time of the policy reform, the sudden increase in package reimbursement rate provides a shock to the profitability of some packages. Our first finding is that increased reimbursement rates induce an immediate increase in the volume of claims filed for a package relative to packages that did not experience an increase. This most likely reflects a supply response, as it is implausible that eligible patients were immediately aware of the reimbursement changes and sought care for the packages targeted by the policy reform. We cannot disentangle whether the volume increase is for necessary care or reflects overprovision of services that are profitable but not necessarily needed. We do, however, find suggestive evidence that part of the supply response is due to upcoding: that is, increasing the relative profitability of a package resulted in hospitals coding more patients to that package than were actually provided it. Although we cannot estimate implications for pass-through exactly, the presence of upcoding would suggest our pass-through estimates are an upper bound on the true rate.

Our key finding is that pass-through of increased reimbursements is approximately 60% in the most rigorous specifications that account for the fact that hospitals cannot reduce OOP charges below zero. In other words, for every 100 INR paid by the government to hospitals, only about 40 INR reaches patients in the form of lower OOP payments. One explanation for incomplete pass-through into OOP is that hospitals may have started accepting higher risk, higher cost patients (within a package) or invested in improving the quality of care, both of which could increase hospital costs. However, we find no meaningful changes in measures of care technical quality, luxury, or patient risk, suggesting that changes on these dimensions are not affecting our estimates. We note, however, that our measures of quality may not capture improvements in care that are not as easily observable to patients but may be a form of pass-through. Nevertheless, our pass-through estimates are low enough that is implausible that all of the remaining government subsidy is devoted to care improvements. Despite the decrease in OOP payments and increased patient volumes, we do not find that the marginal patient is of lower socioeconomic status. This may be partly because the OOP decreases were not large enough to induce much poorer patients to participate in BSBY. It is also possible that information may take longer to percolate through the eligible household pool.

Because we do not have detailed cost data, we cannot fully explain why pass-through is incomplete. However, we find suggestive evidence that pass-through is higher in markets with lower concentration and with more hospitals participating under BSBY, suggesting that market power may play a role. Although we cannot interpret this finding causally, because our competition measures may be picking up other differences across districts that affect pass-through, it is consistent with economic theory and other studies of pass-through of public subsidies (Duggan et al 2016, Cabral et al 2018). Given this finding, it is possible that other barriers to competition, such as high search costs, poor information on quality and prices, and patient-provider loyalty, that have been well documented in hospital health care markets may also play a role in reducing pass-through. Although our study only covers the 7 months after the reform, Figures 2 and 3 suggest that hospitals may still be entering and ramping up service provision under

BSBY. It is possible that the increased competition will further drive down profits and increase pass-through in the longer run.

A limitation of our analysis is that it relies on patient self-reported data and that attrition was substantial at approximately 30% because we used phone-surveys. Although we attempt to complete surveys within 3 weeks of a patient's hospital visit to reduce recall bias, recall has been found to degenerate as soon as two weeks after treatment (Das et al 2011). We do not, however, find that either survey attrition or the recall period are systematically correlated with our treatment variable (which would bias our effect estimates) and we and include controls for recall period in all regressions.

2.8 Conclusion

Lower income countries around the world are rapidly expanding public health insurance programs and contracting private hospitals for service delivery to meet the goals of universal health coverage. Hospital reimbursement rates are a critical policy lever within these programs. While a large literature examines the effects of hospital payments on healthcare in high (and now middle) income countries, the evidence from lower income contexts with weaker institutional capacity, limited data on private hospitals, and poorer patient populations is relatively limited. We provide the first quantitative evidence from India on how private hospitals respond when the government changes reimbursement rates under public health insurance. Our results are particularly relevant to the recently announced expansion of a similarly structured health insurance program in India to cover the poorest 40% of the population.

Using administrative claims data linked to patient surveys, and exploiting a policy-induced change in hospital reimbursements to conduct a difference-in-differences analysis, we find that increased reimbursements lead to increased service volumes and lower cash payments. Hospitals do, however, capture a substantial share of the increased public reimbursements. Four to seven months after the policy reform, and using our most demanding specification, only approximately 60% of every additional rupee paid to hospitals by the government is passed through to patients in the form of lower cash charges. We also find suggestive evidence of increased upcoding by hospitals, which means our estimates of pass-through are likely to be an upper bound on the true rate. Consistent with economic theory, pass-through rates are higher in more competitive markets. We find no meaningful change in patient risk factors, complications, or care quality, suggesting that hospitals do not accept costlier patients or provide better care as alternate forms of pass-through.

Taken together, our results reject the possibility that hospitals charge patients extra cash simply to make up for reimbursement rates that are set below the cost of provision. Our results suggest that a large share of further increases in hospital reimbursement rates will accrue to hospitals rather than patients and will not necessarily reduce the widespread OOP payments under insurance. However, our results also suggest that simply cracking down on OOP charges may have mixed welfare consequences. Instead, facilitating competition (including by public sector hospitals), collecting more detailed hospital cost data to set reimbursement rates, and testing the effectiveness of different hospital monitoring mechanisms may be important strategies to increase the effectiveness of health care spending.

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APPENDIX

Claims Data and Package Matching

There were 1,747 packages in Phase 1 and 1,406 packages in Phase 2; some packages were eliminated, some were collapsed into single packages because they were considered redundant, and others were split into more than one package to allow for heterogeneity in care type. When the package list was revised, package names were similar or the same in Phase 2, but were assigned new codes, with no unique package identifier across phases. We first matched package names between phase 1 and 2 using Stata, and then manually verified these matches. If there were several closely related packages for the same broader service, we ensured that all of them were matched and included in our sample. For some packages, there was a many-to-one match across phases if two or more Phase 1 packages were collapsed into a single package in Phase 2. For example, the Phase 1 "C-section basic (INR6500)" and "C-section lower segment (INR6900)" packages were collapsed into a single "C-section basic (INR9000)" package in Phase 2. We do not drop the "C-section lower segment" package, because it is part of the C-section delivery cluster, but to ensure stable package matches across phases, we collapse the 2 Phase 1 packages into a larger "meta-package" that has a one-to-one match with the Phase 2 "C-section basic" package. To calculate the reimbursement rate change for meta-packages, we first create a Phase 1 meta-package rate that is the mean rate across its component packages, weighted by Phase 1 claims. This ensures that the calculated package rate is orthogonal to the case-mix of any specific hospitals. In our example, if there were 6000 total claims for "CS basic (INR6500)" and 4000 claims for "CS lower (INR6900) in Phase 1, the Phase 1 rate and rate change for the collapsed package would be calculated as:

$$(6500^* (6000/10000)) + (6900^* (4000/10000)) = 6600 \text{ P1 rate}$$

$$9000 – 6600 = 2400 \text{ rate change}$$

To check that this method for calculating reimbursement rate changes corresponds correctly to actual changes in reimbursement, we also run the DID specification with hospital reimbursements as the outcome in **Table A2.1**. A 1 INR increase in the calculated package rate change results in a 1 INR increase in the hospital reimbursement for a package. This gives us confidence that the rate change calculations are correct and allows us to interpret the coefficients on the Rate Change x PostSR/MR/LR interaction terms in the DID specifications as the effect of a one-unit change in hospital reimbursements.

Table A1: First Stage Effect of Rate Change on Hospital Reimbursement

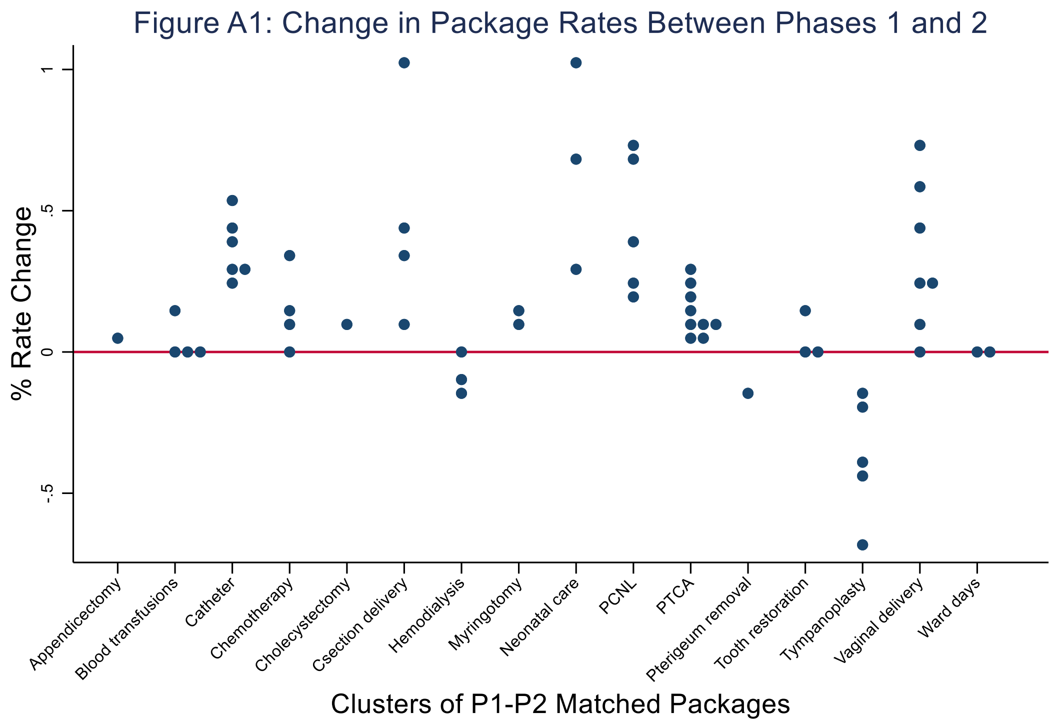
	(1)
	Hospital reimbursement
Rate change x Post (SR)	0.99***
	(0.05)
Rate change x Post (MR)	1.00***
	(0.05)
Rate change x Post (LR)	1.01***
	(0.04)
Observations	13913
Pre-reform mean	7270.18
F-test on rate change x post	0.00

Observations are at the transaction level. Standard errors clustered at the package, and package and hospital level in parentheses.

Table A2.2 presents descriptive information on the reimbursement rates and changes for all packages in our sample (grouped by cluster for clarity) and **Figure A2.1** presents the full variation in rate change across all packages.

Table A2: Packages and Rate Change by Cluster

	Cluster	Number of Packages	Mean Phase 1 Rate	Mean Phase 2 Rate	Mean Rate Change	Mean % Rate Change
1	Ward days	2	1,125	1,125	0	0%
2	Tooth restoration	3	283	308	25	5%
3	Blood transfusions	4	1,050	1,088	38	3%
4	C-section delivery	4	8,148	11,750	3,602	47%
5	Vaginal delivery	7	5,737	7,614	1,878	33%
6	Tympanoplasty	5	15,026	9,400	-5,626	-38%
7	Neonatal care	3	6,833	12,000	5,167	65%
8	Hemodialysis	4	1,870	1,750	-120	-6%
9	Cholecystectomy	1	12,000	13,000	1,000	8%
10	Pterygium removal	1	7,004	6,000	-1,004	-14%
11	Chemotherapy	4	3,464	4,050	586	15%
12	Appendicectomy	1	9,500	10,000	500	5%
13	PCNL	5	17,620	25,200	7,580	45%
14	Catheter	6	4,044	5,500	1,456	37%
15	PTCA	9	47,373	54,167	6,793	14%
16	Myringotomy	2	5,116	5,750	634	13%
	Total	**61**				



Each dot represents a package, and they are grouped by cluster.

Check for Differential Changes in Monitoring and Survey Completion

One concern with our empirical strategy is that there may have been other changes between Phase 1 and Phase 2 that correlate with both reimbursement rate change (our treatment variable) and our outcomes of interest. Although we cannot test for all possible confounders, one potentially key one is if the Insurer changed monitoring patterns in Phase 2 to target packages with higher rate changes. In **Table A2.2**, we examine whether the rate change treatment increased the share of claims rejected by the Insurer. We display coefficients on the interactions of rate change and the post dummies (i.e. the DID estimates of rate change effects), as well as the coefficients on the post dummies, which reflect any general post-reform changes across all packages. Rejections do increase significantly post-reform and spike to almost 20% in the MR period.[12] However, they do not differentially affect packages that experienced different rate changes, which increases our confidence that differential changes in monitoring are not biasing our results.

[12] We were unable to get a clear response from the Insurer on why they increased rejections. In interviews, hospital staff told us the rejections were often for trivial infractions, like spelling mistakes in patient name, or for missing documentation that had not previously been required. Numerous hospitals appealed the April 2018 rejections with the government, which is investigating cases

Table A3: Claims Rejections and Rate Change

	(1)	(2)
	Dependent Variable: % of Monthly Package Claims Rejected	
	Panel Hospitals	All Hospitals
% Rate change x Post (SR)	0.00	0.00
	(0.00)	(0.00)
% Rate change x Post (MR)	-0.00	-0.00
	(0.00)	(0.00)
% Rate change x Post (LR)	-0.00	-0.00
	(0.00)	(0.00)
Post (SR)	0.04***	0.04***
	(0.01)	(0.01)
Post (MR)	0.19***	0.19***
	(0.01)	(0.01)
Post (LR)	0.04***	0.04***
	(0.01)	(0.01)
Observations	33950	40201
Pre-reform mean	0.04	0.04
F-test of rate change x post	0.23	0.19

Observations are at the hospital month package level. Rate change x post is the interaction of the package-specific percent change in rates pre- and post-reform and a post reform dummy. Post-reform (SR) is a dummy the short-run period Jan-Feb2018, post-reform (MR) is a dummy for Mar-May2018, and post-reform (LR) is a dummy for Jun-Aug2018. Dec2017 is dropped. In Column 1 the sample is restricted to the panel of hospitals that filed claims both pre- and post-reform; in Column 2 all hospitals, including post-reform entrants, are included. The dependent variable is the share of all claims filed for a package in a month that was rejected. Standard errors clustered at the package level are in parentheses.

We also check whether survey attrition, which was substantial because we relied on phone-surveys using numbers in the administrative data, and the recall period were differential by our treatment variable. We present the results of the usual DID specification with an indicator for whether the survey was successfully complete and a continuous measure of the days between claim filing and survey completion as the outcome variables in **Table A2.4.** There is no relationship between rate change and the recall period; there is a significant but very small coefficient for survey completion in the LR period (0.08% difference over the pre-reform mean).

Table A4: Survey Completion and Recall Period

	(1)	(2)
	Surveyed successfully	Recall period (days)
Rate change (000s) x Post (SR)	0.01	0.09
	(0.01)	(0.17)
Rate change (000s) x Post (MR)	0.01	0.09
	(0.01)	(0.16)
Rate change (000s) x Post (LR)	0.02**	0.05
	(0.01)	(0.13)
Observations	13913	13913
Pre-reform mean	26.39	26.39

Observations are at the individual transaction level. Standard errors clustered at the package level in parentheses.

and, in some cases, overturning the rejections. However, the appeal process can take several months, which is why our data do not necessarily capture overturned rejections. One possible explanation for a spike specifically in April 2018 is that the end of the tax year in India is May, and the Insurer wanted to minimize outlays or at least postpone them to the next financial year.

Upcoding and Implications for Pass-Through

We observe a substantial and immediate increase in claims volumes in response to reimbursement rate changes. It is unlikely that this could be explained by patient demand – i.e. it is implausible that patients in need of the specific packages that experienced rate increases were immediately aware of it and sought care. However, it could be explained by increased hospital efforts to attract these types of patients – it is common for hospitals to hold village health camps to identify patients in need of care and to make agreements with lower-level health care providers to refer patients to them. In our context, health care seeking is relatively low and the pool of potential patients is likely to be large. Another possibility, however, is that this reflects "upcoding" by hospitals of patients from packages that did not experience a rate increase to those that did and are now more profitable. During field visits we found that hospital staff have the package list and reimbursement rates for their department, so it is plausible that both the medical and administrative staff are aware of the consequences of coding packages differently.

To investigate this, we use the survey data for the vaginal and c-section deliveries clusters, which included more detailed questions on the details of care provided. We create an indicator for whether the claimed package was confirmed by the survey. For example, a "Vaginal + antenatal care" package was considered confirmed if the patient reported having had a vaginal delivery and visited the same hospital for antenatal care. We also created an indicator for cluster confirmation if the survey confirmed that the delivery was vaginal or by c-section. We then use the same DID specifications to examine whether an increase in the reimbursement rate for a package had any effect on the probability of confirmation by survey. While survey confirmation is likely to be noisy because it relies on patient self-reports, there is no reason to believe that this changes discontinuously with the policy or is differential by reimbursement rate change (our treatment variable). Therefore, if we find that an increase in a package's reimbursement rate leads to a decrease in the probability of confirmation, it provides evidence that a higher share of claims in that package were incorrectly coded. Furthermore, because the potential for upcoding is not symmetric across packages – i.e. upcoding can, by definition, only be the incorrect classification of care into higher-rate packages – we examine confirmation separately for bottom-coded and non-bottom coded (all other packages in the cluster) packages. Bottom-coded packages should have higher rates of confirmation and should not be responsive to rate changes.

Table A2.5 presents the results. We first show in Column 1 that confirmation at the cluster level is high and unchanged – i.e. there is no evidence of upcoding across clusters (from vaginal to c-section). Pre-reform confirmation of bottom-coded packages is also high and unchanged in response to the reform. However, Column 4 shows that the pre-reform confirmation rate was lower for non-bottom-coded claims (67%) and that this decreased in response to rate increases. These effects are concentrated in the SR period immediately after the reform, which is also when upcoding is likely to be easier and more effective than efforts to attract patients with real need. Combined with our earlier result that claims volumes increased in response to reimbursement rate increases, this suggests that some share of the observed volume increase is due to upcoding and not real changes in provision. In the presence of increases in upcoding our pass-through estimates are overestimates and that a larger share of the government subsidy is going to hospitals.

Table A5: Survey confirmation of delivery claims

	(1) Share of cluster type confirmed	(2) Share of packages confirmed	(3) Share of bottom-coded packages confirmed	(4) Share of all non-bottom-coded packages confirmed
Rate change (000s) x Post (SR)	-0.00	-0.05**	0.01	-0.04**
	(0.00)	(0.02)	(0.02)	(0.02)
Rate change (000s) x Post (MR)	0.00	-0.03*	0.04	-0.03
	(0.00)	(0.02)	(0.03)	(0.02)
Rate change (000s) x Post (LR)	0.00	-0.02	0.05**	-0.01
	(0.00)	(0.02)	(0.03)	(0.02)
Observations	6847	6847	2024	4823
Pre-reform mean	0.99	0.76	0.99	0.67

Observations are at the individual transaction level. Sample restricted to surveyed delivery transactions. Robust standard errors in parentheses.

Sensitivity Analysis

We examine whether the analysis is sensitive to excluding packages with the most extreme changes. Both the visual examination and the formal test show that the parallel trends assumption still holds when we exclude observations below the 5th and above the 95th percentile in terms of rate change. OLS estimates of pass-through into OOPP are also not substantially different when these packages are excluded.

Figure A2.2: Pre-Reform Trends Excluding Highest / Lowest Change Packages

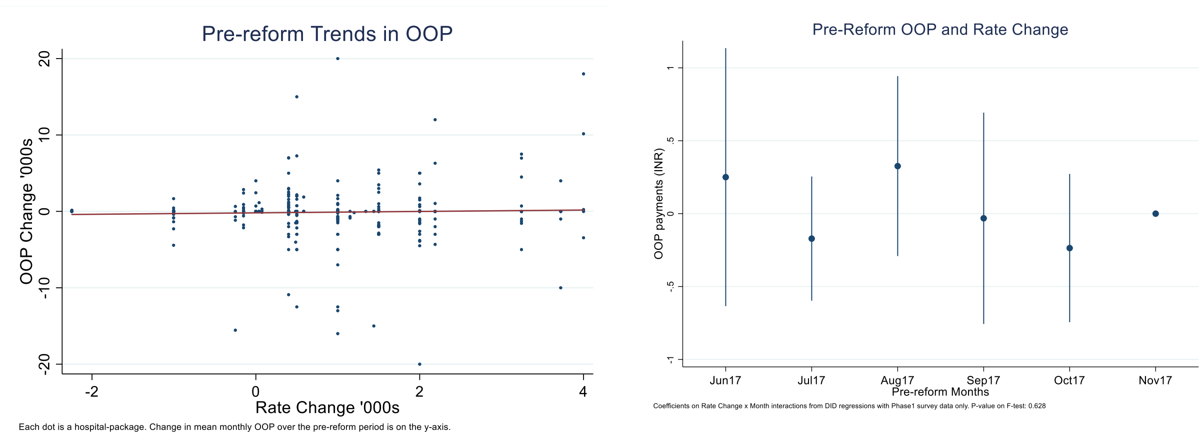
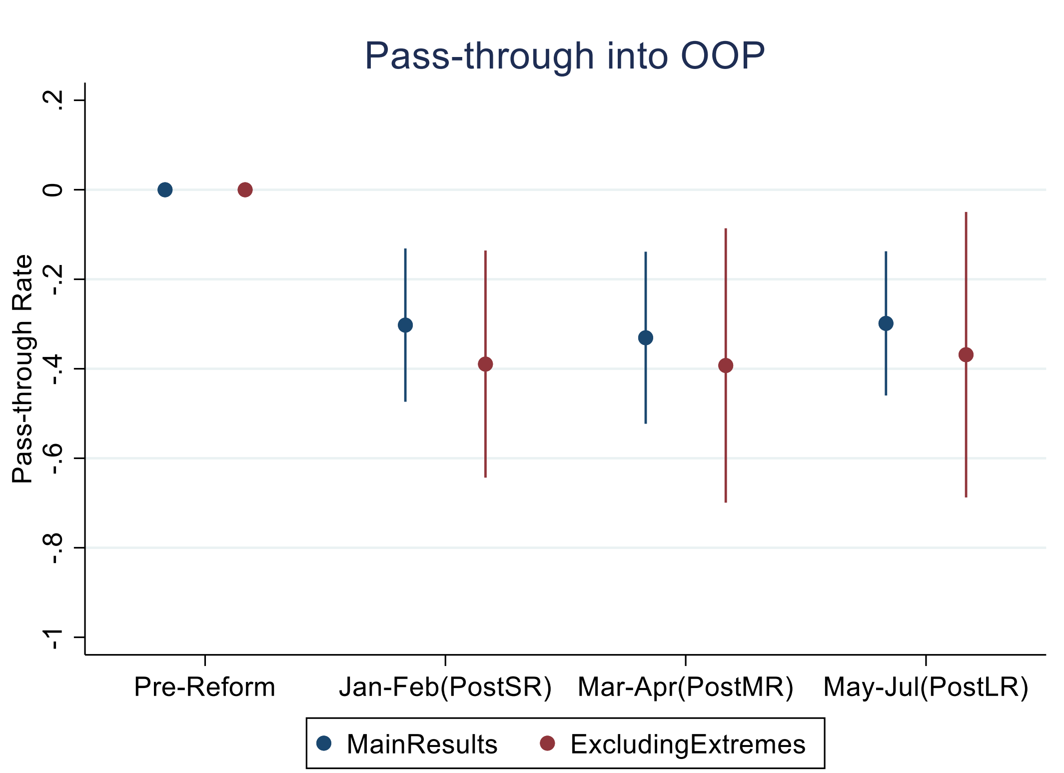


Figure A2.3: Pass-Through Excluding Highest / Lowest Change Packages



CAN CITIZEN INFORMATION IMPROVE HOSPITAL ACCOUNTABILITY? EXPERIMENTAL EVIDENCE FROM A PUBLIC HEALTH INSURANCE SCHEME IN INDIA

Radhika Jain and Pascaline Dupas

Abstract

We study hospital compliance with a public health insurance program in a large Indian state. Using patient surveys, we first document that participating hospitals regularly charge fees to patients eligible to receive free care, resulting in high levels of out-of-pocket payments in and outside the hospital; and that eligible patients lack information about the program. To test whether information is sufficient to enable intended beneficiaries to hold hospitals accountable, we conduct a randomized phone-based information intervention among approximately 1200 patients requiring chronic kidney disease management. We find that the intervention effectively increases program awareness, but has limited impacts on patients' ability to obtain cheaper or more comprehensive care. Subgroup analysis finds that the intervention dramatically reduced patient payments at public hospitals in areas where other hospitals are available.

3.1 Introduction

Health insurance is gaining importance as a policy tool to reduce health-related financial risks in lower income countries. In India, state and central governments have rapidly scaled up public health insurance programs targeting the poor since 2007. Whether these programs are successful at expanding access to care and protection from health-related financial risk depends on whether hospitals comply with program rules. Small-scale surveys and anecdotal evidence suggest that out-of-pocket payments (OOP) are pervasive even for services that are supposed to be free under insurance, which is consistent with recent studies finding that insurance and hospital subsidy programs have had little effect on patient health care expenditures (Rao & Kadam 2009, Grover & Palacios 2011, Mohanan et al 2013, Karan et al 2017).

One explanation for the persistence of OOP payments may be low awareness of program benefits among patients, which may allow hospitals to contravene program rules and charge patients for services that should be free. Particularly in contexts of weak oversight, agency problems, and information asymmetries, efforts to "put poor people at the center of service provision" and increase 'bottom-up' accountability have been advocated as a way of ensuring beneficiaries receive their full entitlements (WDR 2004). Accountability measures may help patients exercise "voice" and claim their benefits from providers and/or exercise choice and "exit" to other providers that better meet their needs (Hirschman 1970, WDR 2004). However, whether information is actually effective depends on the program context. Rent-seeking by public health care providers has been documented even in contexts where patients are aware of their entitlements (Hunt 2010). Competition can reduce rents or profits, but this may be limited by the information frictions and low rates of search common in health care markets (Cohen et al 2017, Lieber 2017, Dranove and Satterthwaite 2000). This paper studies public and private hospital compliance with program rules in the context of a large, state-run public health insurance program in Rajasthan, India. We focus on patients requiring dialysis care because 1) dialysis is a high frequency and expensive service, so the potential gains from information are substantial, and 2) the effects of patient-driven accountability may be different in the context of specialized tertiary care, where hospitals may hold considerable power in the patient-provider relationship. Using insurance claims data, we sampled close to 1,200 hemodialysis patients and conducted phone surveys within 3 weeks of their hospital visit to measure baseline levels of OOP. Because we focus on insured patients who receive eligible health care services at empaneled hospitals, we are able to directly measure hospital non-compliance with program rules that require care to be provided free of cost.

We then conducted a randomized phase-in information experiment to examine whether increasing patient awareness can effectively strengthen 'bottom-up', patient-driven monitoring and increase program compliance by public and private tertiary care hospitals. We provided information by phone to patients about 1) their entitlements under insurance and 2) other hospitals participating under the program to strengthen both the "voice" and "exit" channels of accountability.[13] Given that organizational incentives and factors driving OOP charges are likely to be different in public and private hospitals, we analyze intervention effects by hospital sector.

We first document substantial levels of non-compliance among both public and private hospitals participating in the health insurance scheme. Almost half of all patients have to make some out-of-pocket (OOP) payment at their insurance-covered dialysis hospitals, and payments over the previous 4 weeks average about INR2300 (~$35). In addition, 42% of patients report having to procure and pay for tests and medicines outside their hospital (and this is higher at 50% in public hospitals, largely due to stockouts), even though these are included in the hospital reimbursement rate. This is consistent with research showing that tests and medicines contribute substantially to health-related financial burden in India (Shahrawat & Rao 2012). Total OOP payments over the last 4 weeks, including payments directly to the dialysis facility and for tests and medicines purchased elsewhere, are almost INR4000 (~$60) and are not significantly different across public and private hospitals. Given that dialysis care is required until death, these costs are substantial. Furthermore, although 92% of patients know of BSBY, awareness of program specifics that would be needed to hold hospitals accountable is low: only 55% of all patients know that BSBY covers all dialysis costs and 25% know how much is deducted from their benefit balance to reimburse the hospital.[14] We also find evidence that search and bargaining are limited in our context.

The phone-based information intervention generated large and significant increases in patient awareness of their entitlements and the hospitals available to them under insurance. Effects are larger among patients visiting private hospitals, but are sizeable and significant among those visiting public hospitals as well. Although there is no overall change in OOP payments, subgroup analysis by hospital sector finds that

[13] Particularly if hospitals hold more socioeconomic power than patients – The WDR 2004 refers to "nurses hitting mothers during childbirth, doctors refusing to treat patients of a lower caste" at Indian hospitals – exit may be the preferred option.
[14] Patients receive free care up to an annual limit. The same amount reimbursed to hospitals for each visit is deducted from the patient's benefit balance, so knowledge of this amount is important for patients and can help them hold hospitals accountable. This is discussed in more detail in Section 3.

patients visiting public hospitals see dramatic decreases that are driven both by decreased payments at the hospital and lower rates of having to procure tests and medicines elsewhere. Patients visiting public hospitals also report being more satisfied with their care and treated more respectfully, but we observe no improvements in technical quality. Patients visiting private hospitals were more likely to exercise choice and switch to a different hospital, but this does not result in lower OOP payments. Heterogeneity by hospital sector does not imply a causal relationship, and we cannot fully disentangle why patients visiting private hospitals were unable to lower payments despite being better informed. However, our results are consistent with the theory that public hospital OOP charges at baseline were largely due to informal charges by frontline staff and were reduced in the face of greater patient accountability, while at private hospitals OOP charges may have reflected higher level hospital pricing behavior that patients could not change.[15]

We contribute to the literature in several ways. First, we provide new descriptive evidence on widespread OOP payments at public and private hospitals, even among patients and services that are supposed to be fully insured and are paid for by the government. These findings contribute both to the research on health insurance implementation and effectiveness in India and broader studies documenting substantial leakage in public benefits programs (see Olken and Pande 2012 for a review). In contrast with our results, Dizon-Ross et al (2017) find low rates of extortion in bed-net subsidy programs in Sub-Saharan Africa and attribute this to a combination of high intrinsic and extrinsic motivation among workers in the health system. There is mounting evidence that the organizational structure of public service delivery systems is important for implementation effectiveness, but that agency problems and weak incentives are common in the Indian context (Chaudhury et al 2006, Banerjee et al 2008, Das et al 2016, Dhaliwal & Hanna 2017). Our findings also suggest that simply outsourcing service delivery to private agents to sidestep organizational problems in the public sector is insufficient to ensure program effectiveness. Contracting private agents can successfully leverage market incentives, but it comes with its own monitoring challenges and success

[15] We find some evidence that OOP is at least partially compensating for reimbursement rates that are set too low in other research on the same insurance program.

depends critically on understanding and designing programs around costs and market frictions (Muralidharan & Sundararaman 2011, Das et al 2016, Banerjee et al 2017).

Our findings also join a newer literature in showing that mobile phones can be used as a low-cost and effective tool for both disseminating information and collecting data for program monitoring (Barrington et al 2010, Raifman et al 2014, George et al 2018, Muralidharan et al 2018). In particular, our results support Muralidharan et al (2018) in showing that phone-based data collection is a potentially effective tool to reduce the "high cost of obtaining credible high-frequency data on last-mile service delivery at enough of a spatially disaggregated level to enable holding appropriate staff accountable" that has limited top-down monitoring efforts to date.

Finally, we add to the large body of research on social accountability with evidence from tertiary hospital care, which has received relatively little attention. Particularly after the WDR 2004, numerous studies have evaluated the effectiveness of efforts to empower citizens to hold service providers accountable but reviews of the literature find mixed results (Joshi 2013, Fox 2015). Bjorkman and Svensson (2009) find that facilitating community meetings and monitoring effectively improved the performance of frontline public health workers and health outcomes in Uganda, but a replication modeled on this study did not find similar effects, possibly due to higher baseline levels of health outcomes and differences in implementation intensity (Raffler et al 2018). An experimental evaluation of a government-implemented social accountability initiative to strengthen primary health care in India finds that information about entitlements improved care-seeking and health outcomes, but that information combined with facilitation of community meetings to address grievances was substantially more effective (World Bank 2018). Studies comparing citizen monitoring and top-down monitoring find that the bottom-up monitoring is far less effective, but these were in the context of road building and public education, which may both be subject to free-riding concerns (Olken 2007, Muralidharan et al 2017). Overall, the literature suggest that the effectiveness of different accountability measures likely depends on the details of intervention design, service type, and institutional context. Whereas these studies have focused on primary health care by public providers, we study accountability in the context of specialized, life-saving care provided by both public and private hospitals, where hospitals may hold substantial power and market incentives may be more salient than the threat of being fired. Furthermore, we test the effect of providing just a short phone-based information intervention without additional efforts to coordinate citizens for collective action. Our results suggest that patient-driven accountability interventions are an important but insufficient to improve the effectiveness of public health insurance programs for specialized hospital care. In these contexts, "top down" monitoring and the careful design of incentives for participating hospitals, both public and private, may be an important supplementary intervention to ensure target beneficiaries receive their full program benefits.

The paper proceeds as follows: Section 3.2 describes the insurance program, dialysis care, and the hospital context we study; Section 3.3 presents the conceptual framework underlying our intervention; Section 3.4 describes our data; Section 3.5 presents intervention impacts; Section 3.6 discusses the findings; and Section 3.7 concludes.

3.2 The BSBY Program

In December 2015, the Government of Rajasthan (GoR), an Indian state of 70 million people, launched the Bhamashah Swasthya Bima Yojana (BSBY) health insurance program to provide poor households with free secondary and tertiary health care in public and empaneled private hospitals. Households below the state poverty line are eligible and are automatically enrolled in the program once they obtain a Bhamashah card.[16] Beneficiaries pay no premium or co-pay, and are entitled to an annual household limit of INR30,000 (~$460) in secondary and INR100,000 (~$1500) in tertiary care. Hospitals are reimbursed by the government at prospectively set rates for predefined bundles of care. In 2018, a total of 1401 services (738

[16] The Bhamashah card, which includes the names of all household members and is linked to their national biometric unique identification numbers (Aadhar), is issued to all residents of Rajasthan and is used for delivery of a range of public benefits besides BSBY, including pensions and food assistance (see Gelb et al 2018 for a description).

secondary and 663 tertiary) were covered under the program. The same amount reimbursed to the hospital is deducted from the patient's annual benefit balance. The New India Assurance Company, one of the country's largest public sector insurance companies, is the insurer and administers the program. Program implementation has ramped up rapidly; at the time our experiment was launched, over 2.5 million claims had been filed by 1280 hospitals.

The BSBY program has been widely publicized through billboards, radio and print advertisements, and government frontline health workers. While automatic enrollment eliminates one of the barriers to take-up, it may reduce the likelihood that patients know of their eligibility and available benefits. Hospitals are required to display government-issued posters indicating that they are empaneled under BSBY. However, our field visits revealed that most promotional materials do not clearly specify the details of program benefits, such as which particular types of care are covered under the program, which costs associated with a visit are covered, or the reimbursement rate. Village level health workers are provided lists of nearby empaneled facilities, but these are not updated when hospitals are added or removed from the program, and do not include facilities in towns further away, which may be the sole providers of specialized services. Hospitals are also supposed to provide printed notification slips, automatically generated by the BSBY IT system, that specify the services provided and the corresponding reimbursement rate that will be deducted from the patient's annual benefit balance, but interviews suggest this was not enforced.

3.2.1 Hemodialysis Care and Prices

Hemodialysis is the process of removing impurities from the blood required by patients with loss of kidney function. Tubes inserted into a vascular access point, a fistula, are attached to a dialysis machine to filter blood. Patients typically require dialysis sessions 2 to 3 times a week, with each session lasting 3 to 4 hours, and must continue dialysis for the rest of their lives or until they get a kidney transplant. Treatment also requires additional medicines and regular blood tests. In our context, referrals from a doctor are not required to get dialysis and walk-ins are accepted. Data on dialysis prices are limited, but studies of two large public tertiary hospitals estimate that patients pay between INR2000 and 2600 per dialysis visit, while a study from a large private hospital in South India estimated expenses of INR2500 per visit and INR29000 per month (Kaur 2018, Suja 2012); all three find tests and medicines comprise a sizeable share of the monthly total OOP payments. Hospitals are reimbursed INR2000 per dialysis visit under BSBY, inclusive of all tests, medicines, dialysis, and hospital costs. Similar public insurance programs in other Indian states pay between INR2000-2500 per visit (Kaur 2018).

We focus on dialysis patients in this study for several reasons. The frequent and long-term nature of dialysis care, along with its high prices outside insurance, means households have several opportunities to use new information and the potential gains to patients from reduced OOP payments are substantial. Dialysis is also a standardized service - there is relatively low variation in the required frequency and duration of sessions, and the basic procedure is the same across patients – so that treatment quality across patients and hospitals is less heterogeneous than complex services like heart surgery. Finally, it provides an opportunity to study the effectiveness of 'bottom-up' accountability in the context of specialized, tertiary care, which has received less attention than primary care, but likely contributes substantially to patient financial risk and may have characteristics that make it less amenable to patient-driven accountability (in particular, because these tend to be life-threatening illnesses and care is concentrated in large and relatively few hospitals, patients may be less willing or able to negotiate with hospitals).

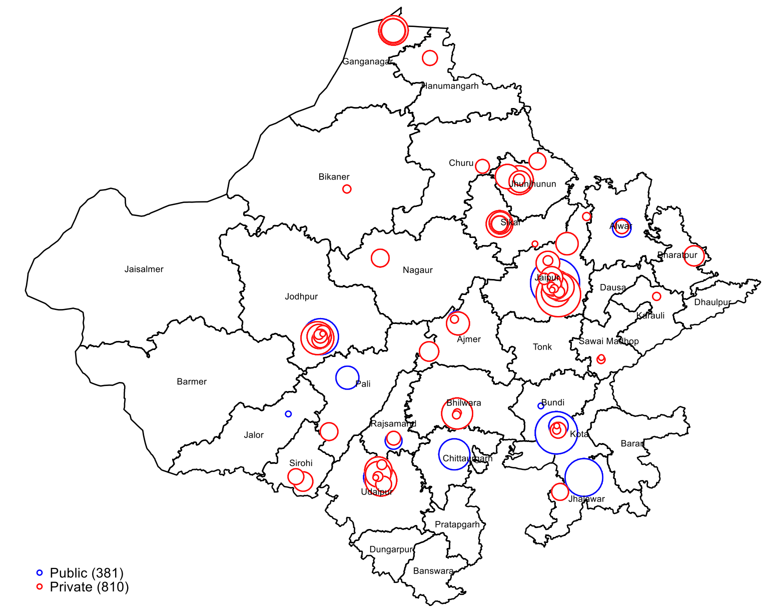
3.2.2 Public and Private Hospitals Under BSBY

Public and private hospitals have very different operational and financial structures under BSBY, which potentially affect organizational and individual incentives in important ways. We describe the context in detail here and discuss theoretical implications for the effects of the intervention in **Section 3**.

Private hospitals are typically profit-maximizing and must ensure that revenues at least cover their costs. Private dialysis hospitals, in particular, tend to be large, multi-specialty hospitals with formal price-setting procedures and internal management systems (in other words, stronger internal principal-agent relationships). If BSBY reimbursements are too low, charging OOP may be one way of meeting the hospital's participation constraint. However, OOP charges may also contribute to profits. While hospitals in competitive markets should have an incentive to lower prices (OOP) faced by patients, facilities for specialized care like dialysis tend to be concentrated in large cities, leaving many markets with a sole provider with market power (**Figure 3.1** presents a map of BSBY hospitals doing dialysis). Furthermore, frictions, such as search costs, patient-provider loyalty, imbalance in patient-provider power, and product differentiation, are well documented in health care markets and can reduce effective competition even in areas with several hospitals. OOP payments at private hospitals, then, likely reflect pricing decisions at the hospital level and either be due to costs of care that are higher than the BSBY reimbursement rate or to hospitals taking advantage of market frictions to overcharge.

Public hospitals do not generate their own revenue; equipment and staff salaries are covered under annual public budgets and medical drugs and supplies are centrally procured. Services are supposed to be provided free of charge, but patients are frequently required to purchase tests and medicines elsewhere due to unavailability at the hospital. Fixed salaries, relatively low central monitoring, and low threat of firing all contribute to weak financial incentives to exert effort and improve the quality of care (Chaudhury et al 2006). Although public hospitals are reimbursed under BSBY, as private hospitals are, hospitals cannot use these funds for staff salaries, so there is little incentive for them to attract more BSBY patients. Per program rules, the funds may be used to purchase tests, medicines, and supplies for BSBY patients if they are unavailable at the hospital, but this requires effort and facilitation by hospital staff. Because public hospitals typically serve as safety net hospitals in the health system, catering to the poorest and sickest patients who cannot afford care at or are turned away by the private sector, hospital staff may have considerable power (Parameswaran 2011). OOP payments in public sector hospitals, then, are unlikely to be a form of official revenue generation, but may reflect informal charges by patient-facing staff in exchange for care, particularly if patients are unaware of their entitlements under BSBY; they may also reflect low effort by staff to procure tests and medicines that are stocked out at the hospital from off-site pharmacies.

Figure 3.1: BSBY dialysis hospitals



Markers proportional to number of patients in RCT sample. Total sample is 1191 patients across 94 hospitals.

3.3 Conceptual Framework

Top-down monitoring of hospitals by the government may be difficult, particularly in low institutional capacity settings. Providing patients with information may enable 'bottom-up', or patient-driven, monitoring and accountability. Patient-driven accountability can work through two mutually reinforcing mechanisms: patients can exercise “voice” with their providers, where they demand their entitlements from their current provider, and/or they may “exit”, where they leave their provider for other options, thus leveraging competition across providers (Hirschman 1970, WDR 2004). In the context of BSBY, providing patients with information on the specific benefits available to them under the program may equip them to demand their entitlements (free dialysis care and treatment) or threaten to complain to higher authorities (“voice”). However, if hospitals have more power in the patient-provider relationship, patients may be reluctant to express voice for fear of retaliation through higher prices, lower quality care, or the refusal to provide services. Providing patients with information on other hospitals that provide dialysis under BSBY may then be a more effective strategy that increases ability to exercise choice and exit to other hospitals that better meet their needs. Because BSBY funds follow patients, exit has financial implications for the hospital. Information on available options may also strengthen patients' bargaining power with their current hospital by enabling them to credibly threaten exit.

Heterogeneity by hospital market density: The feasibility and effectiveness of these patient strategies is likely to be heterogeneous by market density. In high density markets where there are several other hospitals, the cost of exit is likely lower than in low density markets with no other hospital options nearby. The availability (or lower cost) of outside options is also likely to increase the effectiveness of voice in high density markets relative to low density ones. We, therefore, expect to see more of both strategies in high density markets, but which strategy patients will favor within high density markets is uncertain.

Heterogeneity by hospital sector: We also expect patient strategies in response to information and their effects on payment outcomes to vary by hospital sector. Although OOP payments may be substantial in both public and private hospitals, the drivers of these payments are likely to be different across sectors, as we document in **Section 3.2.2**. Public hospitals have weak organizational incentives to respond to patient exit, but if illegal charges by frontline hospital staff are the key driver of observed cash payments, they may be more responsive to the threat of patient complaint (voice), which could cost them their jobs. In private hospitals, cash payments are more likely to reflect hospital-level decisions about prices. If additional patient payments are necessary to cover hospital costs because reimbursements are too low, increasing patient power may have little effect. However, if the payments contribute to hospital profits, the threat of patient exit may induce hospitals to reduce OOP payments to retain BSBY patients.

3.4 Experimental Design and Data

3.4.1 Sampling and Randomization

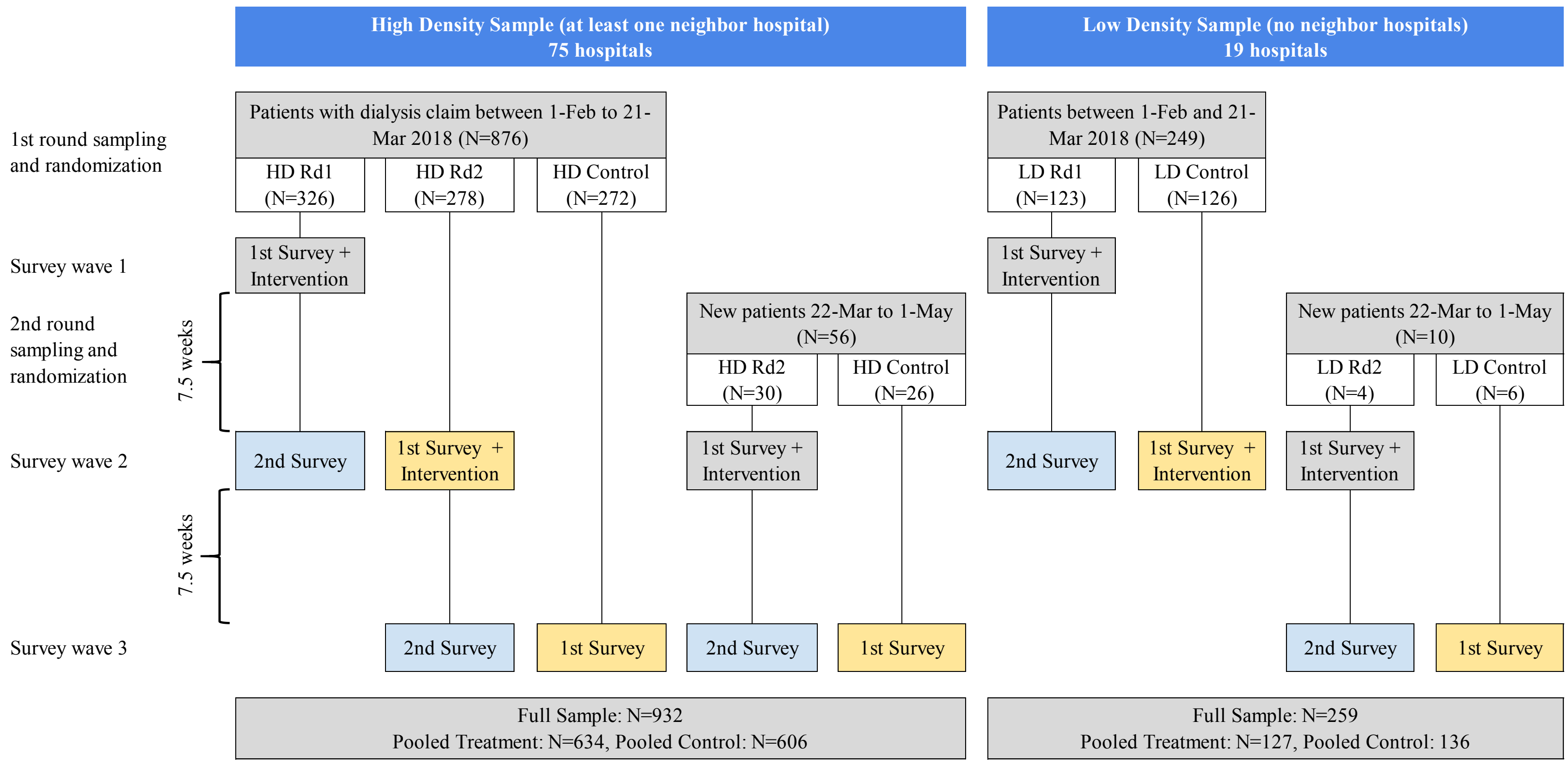
Figure 3.2 presents the study design. We conducted a randomized control experiment where patients were phased into the intervention in two rounds.[17] Using the universe of administrative insurance claims data, we identified 1164 patients with an insurance claim for a dialysis visit between February 1, 2018 and March 21, 2018. 1125 of these patients had a phone number recorded along with their claim. Because rollout of the information intervention was staggered in two rounds, with the first round rolled out in April 2018 and the second round in May 2018, we used fresh claims data received between the first and second rounds to identify an additional 66 "new" patients with dialysis claims between March 22, 2018 and May 1, 2018 that were added to our sample, bringing the total to 1191 patients.

Claims for these 1191 patients were filed by 94 hospitals. We geocoded hospital locations through the Google Maps API and computed Euclidean distances between hospitals as a proxy for travel distance. We then identified up to 3 closest hospitals within a 10-kilometer radius of each hospital ("neighbor hospitals"). Hospitals were split into "low density" (LD) hospitals, with no neighbors within 10 kilometers, and "high density" (HD) hospitals, with one or more neighbor hospitals within 10 kilometers. Of the 94 hospitals, 75 were classified as HD and 19 as LD hospitals.[18] Based on their claims filed during the sampling period, we identified each patient's "primary hospital", or the hospital they visited most often for dialysis. 932 patients in our sample had an HD primary hospital and 259 had an LD primary hospital. Patients overwhelmingly visit only one hospital (only 8% visited more than one hospital in the last 4 weeks at baseline), so the identified primary hospital is where patients get most or all of their dialysis care under BSBY.[19]

The HD and LD patients received slightly different sets of information and were randomized separately. The original HD patient sample was randomly assigned to receive information in round 1, information in

[17] Rollout was staggered into two stages because we had initially planned two different information interventions. We planned to use data collected in the baseline for the first-stage group to compile information on hospital-specific mean out-of-pocket payments for dialysis under BSBY and include this in the information given to the second-stage group. We decided against the second information intervention due to concerns that focusing on prices may induce patients to shift to lower quality hospitals. Note that this makes the study similar to a step wedge design, except that patients who receive treatment in the first round do not stay in the study until the final endline.
[18] Among HD hospitals, 26 have only 1 neighboring hospital, 11 have 2, and 47 have 3 or more.
[19] Although it would have been optimal to define the patient's closest hospitals based on their residence location, rather than the hospital they had been visiting at baseline, but we were unable to do this because accurate residence location was unavailable.

Figure 3.2: Study Design



round 2, or no information ("pure control"), and the 66 new patients identified after round 1 were assigned to information in round 2 or control. The original LD patients were randomly assigned to receive information in round 1 or no information, and the 10 new patients were assigned to information in Round 2 or no information.[20] Patients were stratified by their pre-intervention primary hospital before randomization. 18 patients that were the sole patients in their hospital were grouped into a single HD or single LD stratum before randomization. Tables A1 and A2 in the Appendix examine the balance in average baseline characteristics across the assigned treatment and control groups, as well as those reached at endline (non-attriters).

3.4.2 Information Intervention

All patients were provided information about their entitlements under BSBY; those in the HD sample were provided additional information on up to 3 hospitals closest BSBY dialysis hospitals within 10 kilometers of their pre-intervention primary dialysis hospital (where they had most claims filed during the sampling period). Information was provided to patients over the phone after a short survey that confirmed their identity and collected data on pre-treatment dialysis care and BSBY awareness. Surveyors were trained to read directly from the following scripts:

<u>HD and LD Treatment Groups:</u> *"I would like to give you information about the Government of Rajasthan's BSBY scheme. The program covers the full costs of dialysis, including hospital care, tests, and medicines. You and your household are eligible. You just need to show your Bhamashah card number. All public hospitals and many private hospitals are included in the program. [Primary Hospital], where you have gone for dialysis before, is included. The hospital receives between 1500 and 2000 rupees from the BSBY for each of your dialysis visits."*

[20] Because the purpose of two-stage rollout was to provide information on neighboring hospitals to patients in the second round, this does not apply to LD patients that have no neighboring hospital per our classification. This is why the original LD sample was only split into 2, not 3, groups.

<u>HD only:</u> "These are the names of other hospitals that are within 10 kilometers of that hospital, and that are also included in the BSBY scheme: [Hosp 1], [Hosp 2], [Hosp 3]. Dialysis and related tests and medicines should be free under BSBY at all these places."

After reading out the information, the surveyor asked the patient to confirm how much she/he is supposed to pay for dialysis, including tests and medicines, at a BSBY hospital with her Bhamashah card. If the patient did not say care should be free, the surveyor repeated the information up to 2 times. After that, the surveyor read the following script:

<u>HD and LD Treatment Group:</u> "If you have any other questions about the Yojana, you can ask the Anganwadi center or at any public hospital or you can call the 1800-180-6127 number for free information."

Following the phone call, the following SMS message was sent to the patient:

<u>HD and LD Treatment Groups:</u> "Under BSBY your dialysis, tests, and medicines should be free. The hospital receives between 1500 and 2000 rupees from the scheme for each of your dialysis visits."

<u>HD only:</u> "These hospitals close to you do dialysis and are included in the scheme: [Primary Hospital], [Hosp 1], [Hosp 2], [Hosp 3]"

3.4.3 Data

We received the universe of administrative insurance claims microdata approximately every two weeks.[21] These data include the name and code of services filed, dates of filing and processing, and the reimbursement rate for every patient visit, as well as the name, code, sector (public or private), and district location of the hospital filing the claim. We geocoded hospitals using the Google Maps API and hospital

[21] These data were received as part of a data sharing agreement and MOU between the Government of Rajasthan and JPAL South Asia/IFMR.

names and district locations. Claims also include the patient's name, age, gender, Bhamashah ID number, and phone number. We used the claims data to identify dialysis patients and call them for phone-based surveys. Surveyors were instructed to call each phone number a minimum of five times over at least three different days before declaring it unreachable. Surveys were conducted directly with the dialysis patient to the extent possible, or with a proxy aware of the details of treatment if the patient was unable or unwilling.[22] Very sick patients and female patients were less likely to respond themselves. Surveys collected data on services received, cash OOP payments, measures of care quality, patient risk factors, SES, education, and awareness of BSBY benefits. The core survey was identical across all three waves, which allows us to pool outcomes across waves.

3.4.4 Empirical Specifications

Due to concerns that completing the survey may itself affect our outcomes of interest, we did not conduct baseline surveys in the control groups and treatment effects are estimated by comparing at endline. The information was delivered in two rounds and patients assigned to receive information in round 2 served as the control group for round 1 patients before switching into treatment. We, therefore, pool estimates from survey waves 2 and 3 and run the following regression:

$$y_{ist} = \alpha_1 + \beta_1 * Treatment_{it} + Z'\gamma_1 + \delta_{1s} + \varepsilon_{1i}$$

where y_{ist} is the Survey Wave t outcome for patient i sampled from hospital s; $Treatment_{it}$ is a dummy equal to 1 if the patient was assigned to receive the information treatment prior to wave t; Z is a vector of patient covariates from the claims data at sampling (gender, age, average number of dialysis visits per week, number of weeks on dialysis, and whether the patient was drawn from the sample of new patients identified in May), and δ_{1s} is a set of Hospital x Survey Wave fixed effects (which ensure we are only comparing outcomes across the Treatment and Control groups within a wave and our estimates are unaffected by time trends across waves).[23] We also test for heterogeneity by hospital sector with the following regression:

[22] Approximately 40% of surveys were directly with the patient herself and we include controls for this in all regressions.
[23] In other words, we are only comparing the blue to the yellow boxes within each Survey Wave in Figure 2.

$$y_{ist} = \alpha_2 + \beta_2{*}Treatment_{it} + \beta{*}Treatment_{it}{*}Public + Z'\gamma_2 + \delta_{2s} + \varepsilon_{2i}$$

The key outcomes of interest are awareness of BSBY, patient response strategies (voice and/or exit), OOP payments, and quality (outcomes, and care technical and perceived quality at the primary hospital at endline). We create family-wise indices for several sets of related outcome indicators, following the methods described in Anderson (2008), to reduce the number of outcomes tested. The summary index is the mean of all of the outcomes, weighted by the inverse of the covariance matrix, where each component outcome has been demeaned and then normalized by dividing it by its control group standard deviation. Results for the component indicators of each index are presented in the Appendix.

We create a composite BSBY awareness index of indicators for whether the patient knows that BSBY covers all costs of care, the costs of dialysis, and the costs of tests and medicines; whether she knows how much a hospital is reimbursed for each dialysis visit under BSBY; and whether she knows of at least one BSBY participating hospital near her pre-intervention primary hospital.[24] Patient response strategies are classified into voice and exit strategies. To measures voice, we use survey responses for whether the patient bargained to lower prices with any of the facilities she visited for dialysis in the past 4 weeks at endline. To measure exit, we use survey questions to identify whether she switched to a different primary hospital for dialysis by endline and what kind of facility she switched to (public or private, one of the information intervention hospitals, or a non-BSBY hospital).

Measures of OOP payments cover the 4 weeks prior to the survey and include the probability of any payment and the total amount paid at all BSBY hospitals visited, whether these payments included payments for tests/medicines and direct payments to medical staff, and the probability of any payment and amount paid for additional tests/medicines obtained at other facilities. Quality measures include indicators

[24] Because the amount reimbursed to a hospital for a dialysis visit is also deducted from the patient's annual benefit balance, patients should be informed of the reimbursement amount. To this end, the government requires hospitals to provide patients a printout of an auto-generated invoice from the BSBY system that specifies the service(s) provided and the amount paid to the hospital by BSBY. However, this is difficult to enforce and patient interviews suggest that many hospitals are not doing this.

for whether the patient had any infection or any bleeding from the fistula (where the dialysis tubes are inserted). We create an index of care quality at the patient's primary hospital at endline, which includes indicators for whether the patient had no wait time (includes up to half hour wait), had dialysis for 3 or more hours (generally considered the minimum sufficient duration), had an AC ward, and was attended to by medical staff (doctor, nurse, or dialysis staff); and an index of perceived quality at the primary hospital, which includes indicators for whether a patient reports that the facility she visited most often in the previous 4 weeks was very clean, staff were very respectful (options for both were very good, good, okay, not good, and bad), and she was very satisfied with the price and quality of care (options were very satisfied, somewhat satisfied, somewhat dissatisfied, and very dissatisfied).

3.5 Results

3.5.1 Summary Statistics

Table 3.1.1 presents summary statistics for the HD sample at the hospital level (Panel A) and patient level (Panel B). 75 of the 94 hospitals in the study are HD hospitals and most have between 2 and 3 other hospitals providing dialysis services within 10 km. Hospitals have 11 dialysis patients on average and the majority are private (79% of HD hospitals). To compile pre-intervention patient descriptive statistics, we pool the first surveys across all patients.[25] 22% of households could not be reached at all in the HD group. This is largely due to wrong or invalid phone numbers in the administrative claims data (20%) and only 0.1% were due to refusals.[26] An additional 17% of HD patients were confirmed dead at the time of survey by someone else in the household. This may be an underestimate of deaths: when patients die, their phone numbers may be deactivated, resulting in classification of the call as an invalid phone number. Confirmed deaths are significantly higher in patients that had a public sector primary hospital prior to the intervention (31% in

[25] As shown in Figure 2, the first surveys for each group were staggered. We did not want to conduct surveys earlier in the Rd2 and Control groups because of concerns that the survey may have an informational effect.

[26] Note that this is considerably lower than non-response in phone surveys in other contexts. For example, the non-response for telephone polls in the United States is 91% (source: http://www.pewresearch.org/2017/05/15/what-low-response-rates-mean-for-telephone-surveys/); and 25% in Tanzania even though respondents were given cellphones (Croke et al 2012). Because the number of patients on dialysis under BSBY was small and all patients were included in the study, we could not replace unreachable patients with others prior to randomization.

public compared to 12% in private), though this may reflect both differential selection of patients and differences in treatment quality across public and private hospitals. In total, 39% of the HD sample could not be surveyed at all and was not included in the intervention or estimation of treatment effects. Among those surveyed, the likelihood that the patient herself was surveyed was around 40%, with the rate lower among very sick patients and female patients, who typically have less access to phones and for whom their male spouses are more likely to respond. The patient population is largely male, middle-aged, and has low education levels (6 years on average). Public hospital patients are significantly more likely to be low caste (31% in public relative to 18% in private) and less wealthy.

Care, Search, Bargaining: Care is long term and high frequency. Patients have been on dialysis for an average of 14 weeks and had 8 sessions in the 4 weeks prior to survey, though the means are lower in public sector patients. Although visits are frequent, search and bargaining appear to be low. Only 51% have ever visited a hospital besides their primary hospital for dialysis, 8% have visited another hospital in the previous 4 weeks, and 20% have ever bargained with their dialysis hospital to reduce prices (though bargaining is 11 percentage points higher in private).

BSBY Knowledge: While general awareness of the BSBY program is high, at 92%, information about program specifics is much lower: 55% of patients knowing that it covers all dialysis-related costs of treatment, and 25% know how much a hospital is reimbursed for each dialysis visit.[27] Half of all patients have heard of at least one of the neighboring BSBY dialysis hospitals we identified from the claims data. Of those who have, 36% believe their current primary hospital is higher quality (significantly higher among private patients) and 22% believe it has lower prices (significantly higher among public patients). This suggests that patients in public hospitals may favor low (perceived) prices over quality in their decision on where to go for dialysis.

[27] As discussed earlier, patients should know the reimbursement amount because this is also deducted from their annual benefit balance and because hospitals are supposed to give them a printed receipt of this amount, though they do not always do so.

Table 3.1.1: Summary Statistics (HD Sample)

	Total Mean	SD	Private Mean	SD	Public Mean	SD	Priv=Pub P-value
Panel A: Hospital Characteristics							
BSBY dialysis hospitals in 10km	2.34	0.87	2.39	0.84	2.16	0.96	0.39
Months in BSBY	22.20	6.30	20.90	6.53	26.99	0.38	0.00
Total BSBY patients	11.22	11.57	11.16	10.31	11.42	15.79	0.95
Observations	75		59		16		75
Panel B: Patient Characteristics							
Household not surveyed	0.22	0.41	0.23	0.42	0.17	0.38	0.04
...Because wrong/invalid number	0.20	0.40	0.22	0.41	0.17	0.37	0.09
...Because survey refused	0.01	0.11	0.02	0.12	0.00	0.07	0.10
Patient dead at time of call	0.17	0.37	0.12	0.32	0.31	0.46	0.00
Patient is the respondent herself	0.40	0.49	0.44	0.50	0.28	0.45	0.00
Female	0.32	0.47	0.33	0.47	0.30	0.46	0.44
Age	44.91	14.84	44.94	14.88	44.85	14.76	0.95
Years of schooling	6.34	4.85	6.39	4.90	6.16	4.65	0.63
Scheduled caste/tribe	0.21	0.41	0.18	0.39	0.31	0.46	0.00
Asset index (std PCA)	0.02	1.03	0.09	1.01	-0.24	1.06	0.00
			Dialysis Care, Search, Bargaining				
Weeks on dialysis	14.08	11.51	15.25	11.27	10.22	11.45	0.00
Dialysis visits in 4 wks	8.01	2.85	8.31	2.74	6.84	2.98	0.00
Visited >1 hospital in 4 wks	0.08	0.27	0.08	0.27	0.10	0.30	0.50
Ever bargained with hospital	0.20	0.40	0.23	0.42	0.12	0.33	0.01
Ever got dialysis elsewhere	0.51	0.50	0.52	0.50	0.47	0.50	0.33
			BSBY Knowledge				
Has heard of BSBY	0.92	0.27	0.93	0.26	0.89	0.32	0.17
Knows BSBY covers all costs	0.55	0.50	0.53	0.50	0.62	0.49	0.13
Knows BSBY hospital payment	0.25	0.43	0.27	0.44	0.16	0.37	0.01
Knows any neighbor hospitals	0.51	0.50	0.51	0.50	0.47	0.50	0.41
Believes PH is highest quality	0.36	0.48	0.39	0.49	0.26	0.44	0.00
Believes PH is lowest price	0.22	0.41	0.20	0.40	0.28	0.45	0.08
			Out of Pocket Payments in Last 4 Weeks				
Any OOP payment at hospital	0.48	0.50	0.51	0.50	0.34	0.48	0.00
Amount paid at hospital	2288.52	4543.07	2364.96	4513.25	1996.89	4662.91	0.44
Got tests/medicines elsewhere	0.42	0.49	0.40	0.49	0.50	0.50	0.04
...Because unavailable at hospital	0.41	0.49	0.34	0.48	0.60	0.49	0.00
...Because cheaper elsewhere	0.23	0.42	0.27	0.45	0.10	0.31	0.00
Amount paid elsewhere	1508.19	3631.66	1342.13	2839.98	2148.69	5716.67	0.15
Total OOP payment	3709.73	5773.79	3628.61	5061.34	4019.18	7945.56	0.61
			Quality				
Fistula infection in 4 wks	0.21	0.41	0.19	0.40	0.25	0.44	0.29
Fistula bleeding in 4 wks	0.15	0.36	0.14	0.35	0.17	0.38	0.51
PH care quality (std PCA)	-0.06	1.02	0.10	0.88	-0.71	1.26	0.00
PH perceived quality (std PCA)	0.07	1.02	0.18	1.00	-0.33	0.98	0.00
Observations	932		714		218		932

PH refers to the patient's primary hospital at the time of survey. Neighbor hospitals are up to 3 BSBY dialysis hospitals within 10km of the PH.
The assets, care quality, and perceived quality indices are the first component of a principal component analysis of several indicators that are normalized to be expressed in standard deviation terms. Care quality includes indicators for no infection, no bleeding, no more than half hour wait time, dialysis for 3+ hours (generally considered the minimum sufficient duration), AC ward, and attended to by medical staff at the patient's primary hospital in the last 4 weeks. Perceived quality includes indicators for whether the patient reported very respectful staff, very clean facility, being very satisfied with care and cost, and that she would recommend the facility to others. The asset index is based on indicators for ownership of 12 assets. P-values are reported from a two-sided t-test comparing means in the public and private sub-samples.

Out-of-Pocket (OOP) Payments: The magnitude and prevalence of patient financial outlays, even under health insurance, is striking. Almost half of all patients pay OOP at their BSBY dialysis hospitals, paying INR2300 (~$35) over the last 4 weeks on average, and 42% of patients pay for tests and medicines procured outside their hospital. The composition of payments is heterogeneous by hospital type. Patients at public hospitals are less likely to have to pay at their hospital (34% and ~INR2000 compared to 51% and ~INR2400 in private), but are significantly more likely to have to purchase tests and medicines outside their hospital, typically due to stockouts at the hospital. Total OOP payments are almost INR4000 (~$60) and are, surprisingly, not significantly different across public and private hospitals.

Quality at Current Primary Hospital: Summary scores of hospital-specific care quality (attended by medical staff, wait time, dialysis duration, and AC ward) and perceived quality (patient reported cleanliness, respect, and satisfaction) have significantly higher mean values among patients with a private primary hospital. 21% of patients reported bleeding from the fistula, the entry point for the dialysis needle, and 15% reported infection at the fistula in the last 4 weeks; these rates are slightly higher in public, though the difference is not significant.

LD Sample: **Table 3.1.2** presents similar summary statistics for the LD sample - patients whose pre-intervention primary dialysis hospital has no other BSBY hospital within 10km. We discuss them in brief, highlighting differences from the HD sample. Among LD patients, the rates of unsuccessful survey and death were lower, at 14% and 13%, respectively, resulting in a total 27% that was not surveyed. Differences across the public and private sector are somewhat different in the LD sample: public sector patients are more likely to be younger and female, but socioeconomic characteristics are not different from those in the private sector. Public sector patients have been on dialysis for longer (but go less frequently), are 35 percentage points less likely to have ever visited another hospital, and are much less likely to have ever bargained to lower prices that patients with a private primary hospital at baseline. OOP payments at the hospital are very low in the public sub-sample (7% probability of payment and INR67, or about a dollar,

Table 3.2.2: Summary Statistics (LD Sample)

	Total Mean	SD	Private Mean	SD	Public Mean	SD	Priv=Pub P-value
Panel B: Hospital Characteristics							
Months in BSBY	21.75	7.79	18.64	8.40	27.10	0.09	0.01
Total BSBY patients	12.92	14.23	8.00	5.78	21.35	20.36	0.14
Observations	19		12		7		19
Panel B: Patient Characteristics							
Household not surveyed	0.14	0.35	0.17	0.37	0.13	0.34	0.42
...Because wrong/invalid number	0.13	0.33	0.14	0.34	0.12	0.33	0.77
...Because survey refused	0.01	0.11	0.02	0.14	0.01	0.08	0.36
Patient dead at time of call	0.13	0.33	0.06	0.24	0.16	0.37	0.02
Patient is respondent herself	0.43	0.50	0.42	0.50	0.44	0.50	0.83
Female	0.30	0.46	0.38	0.49	0.26	0.44	0.08
Age	42.56	15.07	45.71	15.10	40.82	14.81	0.02
Years of schooling	6.35	4.89	5.73	5.26	6.73	4.63	0.18
Scheduled caste/tribe	0.27	0.44	0.26	0.44	0.27	0.45	0.87
Asset index (std PCA)	-0.07	0.91	-0.18	0.96	0.01	0.87	0.18
Dialysis Care, Search, Bargaining							
Weeks on dialysis	18.24	10.52	15.96	10.73	19.58	10.20	0.01
Dialysis visits in 4 wks	7.74	2.19	8.22	2.67	7.44	1.79	0.03
Visited >1 hospital in 4 wks	0.04	0.20	0.07	0.25	0.03	0.16	0.20
Ever bargained with hospital	0.08	0.27	0.17	0.38	0.03	0.16	0.00
Ever got dialysis elsewhere	0.63	0.48	0.85	0.36	0.50	0.50	0.00
BSBY Knowledge							
Has heard of BSBY	0.88	0.32	0.92	0.27	0.86	0.35	0.18
Knows BSBY covers all costs	0.57	0.50	0.50	0.51	0.61	0.49	0.28
Knows BSBY hospital payment	0.23	0.42	0.22	0.42	0.23	0.42	0.93
Out of Pocket Payments in Last 4 Weeks							
Any OOP payment at hospital	0.24	0.43	0.51	0.50	0.07	0.25	0.00
Amount paid at hospital	815.68	2305.36	2042.36	3317.40	67.20	644.90	0.00
Got tests/meds elsewhere	0.53	0.50	0.47	0.50	0.56	0.50	0.22
...Because unavailable at hospital	0.59	0.50	0.61	0.50	0.58	0.50	0.78
...Because cheaper elsewhere	0.02	0.14	0.03	0.17	0.02	0.12	0.66
Amount paid elsewhere	1505.23	2503.73	1768.12	3227.07	1347.50	1945.28	0.33
Total OOP payment	2273.38	3417.41	3736.81	4525.42	1380.45	2077.47	0.00
Quality							
Fistula infection in 4 wks	0.28	0.45	0.28	0.45	0.28	0.45	0.95
Fistula bleeding in 4 wks	0.21	0.41	0.20	0.41	0.22	0.42	0.82
PH care quality (std PCA)	0.02	1.02	-0.22	1.18	0.16	0.88	0.02
PH perceived quality (std PCA)	0.14	0.99	0.04	0.97	0.19	1.01	0.32
Observations	259		96		163		259

See notes for Table 1.1.

on average), and much lower than in the private sample (51% pay and cash is INR2000, or $30, on average). Higher payments for tests and medicines for public patients partially offset this, but overall OOP payments are still significantly lower among public hospital patients than private, and are almost INR1500 lower than total payments in the HD sample. Infection and bleeding outcomes, care quality, and perceived quality are not significantly different across public and private hospitals in LD hospitals.

3.5.2 Information Intervention

Table 3.2 presents statistics on delivery of the information treatments. The information treatment was provided by phone at the end of a short survey. Due largely to a combination of invalid or unreachable phone numbers and patient deaths between the time of sampling and of survey, 66% of the 634 patients in the HD treatment group and 76% of the 127 patients in the LD treatment group were successfully provided the information treatment. Survey success rates are higher among private hospital patients in the HD treatment group, largely due to higher death rates in the public sample. We do not find evidence of differential attrition across treatment and control groups in the HD sample and pre-intervention characteristics of those reached at endline are balanced (discussed in the **Appendix**). In approximately a third of cases (33% in HD and 38% in LD), the information was provided directly to the patient, while in the remaining cases, the information was provided to a close relative involved with the patient's care and treatment. Patients in the HD sample were typically given information about 2 or 3 neighboring hospitals.

Table 3.2: Information Treatment

Panel A: HD sample

	Total Mean	SD	Private Mean	SD	Public Mean	SD	Priv=Pub P-value
Wrong/invalid number	0.22	0.41	0.23	0.42	0.18	0.38	0.20
Refused survey	0.01	0.10	0.01	0.10	0.01	0.08	0.69
Patient dead at time of call	0.15	0.36	0.10	0.30	0.31	0.46	0.00
Information delivered to household	0.66	0.47	0.69	0.46	0.57	0.50	0.01
Information delivered to patient herself	0.33	0.47	0.36	0.48	0.25	0.43	0.01
Number of neighboring hospitals mentioned	2.59	0.70	2.54	0.72	2.75	0.63	0.00
Observations	634		489		145		634

Panel B: LD sample

	Total Mean	SD	Private Mean	SD	Public Mean	SD	Priv=Pub P-value
Wrong/invalid number in claims	0.14	0.35	0.11	0.31	0.16	0.37	0.36
Refused survey	0.02	0.15	0.04	0.20	0.01	0.11	0.36
Patient dead at time of call	0.10	0.31	0.10	0.31	0.11	0.31	0.96
Information delivered to household	0.74	0.44	0.74	0.44	0.74	0.44	0.93
Information delivered to patient herself	0.39	0.49	0.36	0.49	0.40	0.49	0.67
Observations	127		47		80		127

3.5.3 Impacts

3.5.3.1 High density (HD) sample

We examine effects of the intervention at endline, 7 to 8 weeks after the information was provided, on BSBY awareness, patient responses, OOP payments, and care quality. We focus our attention largely on the HD sample, where we expect the intervention to have the largest effects and the larger sample allows more precise estimates. All results are presented as pooled estimates, as well as broken down by the sector (public or private) of the patients' pre-intervention primary BSBY dialysis hospital. However, differences across public and private hospitals may not necessarily be causal, as hospital sector was not randomly allocated and we observe significant differences in patient and care characteristics across sectors prior to the intervention. For all regressions, we also report the Control group means at endline for the pooled sample, as well as for the public and private hospital sub-samples.

The intervention was effective at increasing BSBY awareness. Column 1 of **Table 3.3** reports a significant increase of 0.3 standard deviations in the composite index of BSBY awareness (detailed results for each of the components of the awareness index are reported in the Appendix). Effects are larger among private hospital patients, but substantial in the public sector as well. Patients in private hospitals are also 5.5 percentage points more likely to be aware of any of the neighboring hospitals they were told of in the intervention.

Table 3.3: Treatment Effects: BSBY Awareness (HD Sample)

	(1) BSBY awareness index	(2) Knows any intervention hospital
Treatment	0.300***	0.036
	(0.075)	(0.028)
Heterogeneity by sector		
Treatment	0.321***	0.055*
	(0.082)	(0.031)
Treatment x Public	-0.111	-0.105
	(0.187)	(0.071)
Hospital x Wave FE	Yes	Yes
Observations	753	753
Public Treatment p-value	0.000	0.152
Control Mean	0.003	0.793
Control Mean (Pvt)	0.016	0.803
Control Mean (Pub)	-0.051	0.753

HD sample only. Public Treatment p-value is for the F-test of joint significance of Treatment and Treatment x Public. BSBY awareness index is a composite index of dummies for whether knows all costs, dialysis costs, and tests/medicines costs are covered, and knows the hospital BSBY reimbursement rate per dialysis visit. All regressions include hospital x survey wave fixed effects, as well as the following controls from the claims data: gender, age, dummies for whether the patient had been on dialysis for 5+ weeks at sampling, whether the patient was getting dialysis more than once weekly during the sampling period, and whether the patient was newly sampled in phase 2.

Table 3.4 reports patient responses to the information. Using survey data on the hospital visited most often at endline, we observe a 5.3 percentage point (39% Column 2) increase in the probability of switching away from their pre-intervention primary hospital, and into public hospitals (Column 3), though this effect is driven entirely by patients in the private sub-sample. Patients do not necessarily switch into one of the information intervention hospitals (Column 5), but their endline hospital is 7km away from their baseline primary hospital on average (Column 6), suggesting that the intervention encouraged patients to search for a new hospital rather than simply go to the ones we told them about.[28] We find very little effect on bargaining, our measure of voice, overall or by hospital sector (Column 7).

Table 3.4: Treatment Effects: Patient Response in Last 4 Weeks (HD Sample)

	(1) Visited >1 hospital	(2) Switched away from Primary Hospital	(3) Switched into public hospital	(4) Switched into private hospital	(5) Switched into an Info hospital*	(6) Distance baseline to endline Primary Hospital (km)	(7) Bargained with hospital(s)
Treatment	-0.014	0.053**	0.036**	0.024	0.007	4.916	0.007
	(0.017)	(0.026)	(0.017)	(0.022)	(0.012)	(3.371)	(0.029)
Heterogeneity by sector							
Treatment	-0.011	0.068**	0.036*	0.039	0.003	7.314**	0.025
	(0.018)	(0.029)	(0.019)	(0.024)	(0.014)	(3.706)	(0.032)
Treatment x Public	-0.017	-0.080	0.003	-0.076	0.025	-13.263	-0.096
	(0.042)	(0.065)	(0.044)	(0.054)	(0.031)	(8.566)	(0.074)
Hospital x Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	753	753	753	753	753	723	753
Public Treatment p-value	0.638	0.061	0.115	0.197	0.599	0.105	0.417
Control Mean	0.058	0.136	0.042	0.089	0.026	8.553	0.183
Control Mean (Pvt)	0.059	0.108	0.039	0.066	0.020	8.542	0.197
Control Mean (Pub)	0.052	0.247	0.052	0.182	0.052	8.602	0.130

*Info hospital is any of the BSBY hospitals included in the information intervention. See Table 3 notes. The outcome variable in Column 6 is the distance between the patient's pre-intervention and endline primary hospitals in kilometers.

Columns 1 to 4 in **Table 3.5** report the probability of payment, whether this included payments directly to the doctor or nurse or for tests and medicines, and the amount paid at all hospitals visited for dialysis treatment in the 4 weeks prior to endline (the distribution of treatment and control payments is presented graphically in Figure A1 in the Appendix). Column 5 reports the probability of payment for tests and medicines that patients had to purchase outside their dialysis facility. Column 6 reports the total amount of OOP payment. We find no substantial effects of the information intervention in the pooled sample, but this masks considerable heterogeneity by hospital sector. Patients in the public sub-sample are 11 percentage points (27%) less likely to have to pay OOP at their dialysis hospital. Keeping in mind that this sample of patients did not exhibit significant switching away in **Table 3.4**, this means that most of these patients were

[28] Although the information included the closest 3 hospitals within a 10-kilometer radius of their pre-intervention primary hospital, many hospitals in the HD sample had several other nearby BSBY hospitals. We do not find significant switching to hospitals outside BSBY.

paying less at endline at the same public hospital they were visiting most before the intervention. This reflects a substantial decrease in payment to medical staff of 16 percentage points (49%), as well as a small decrease in payments for tests and medicines at the dialysis hospital. Public hospital patients are also 12 percentage points (21%) less likely to have to purchase tests and medicines elsewhere, although this is not significant (Column 5). As a result, public hospital patients pay INR1582 (69%) less at their hospital and INR2784 (63%) less for overall dialysis related care and treatment in the last 4 weeks. However, patients in the private hospital sub-sample had no significant reduction in the probability or amount of OOP payments for dialysis-related costs, either at their hospital or elsewhere.

Table 3.5: Treatment Effects: Out-of-pocket Payments in Last 4 Weeks (HD Sample)

	(1) Any OOP payment at hospital	(2) OOP included test/meds	(3) OOP included payment to med staff	(4) Amount paid at hospital	(5) Got test/meds elsewhere	(6) Total OOP payment
Treatment	-0.013	-0.039	-0.040**	-306.888	-0.051	-508.776
	(0.036)	(0.036)	(0.015)	(297.641)	(0.038)	(399.600)
Heterogeneity by sector						
Treatment	0.01	-0.01	-0.04**	-12.84	-0.04	15.87
	(0.04)	(0.04)	(0.02)	(328.10)	(0.04)	(439.27)
Treatment x Public	-0.11	-0.14	0.01	-1569.02**	-0.08	-2799.41**
	(0.09)	(0.09)	(0.04)	(746.04)	(0.10)	(998.84)
Hospital x Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	723	723	723	723	714	723
Public Treatment p-value	0.42	0.16	0.03	0.06	0.29	0.01
Control Mean	0.50	0.45	0.07	2340.77	0.47	3917.93
Control Mean (Pvt)	0.53	0.48	0.07	2349.94	0.44	3784.53
Control Mean (Pub)	0.41	0.33	0.04	2305.53	0.58	4430.46

See Table 3 notes.

Finally, we examine effects of the intervention on the quality of care at endline in **Table 3.6**. Columns 1 and 2 report the probability of infection or bleeding at the fistula in the last 4 weeks. Columns 3 and 4 report composite indices of hospital-specific perceived quality (self-reported cleanliness, respectfulness, and satisfaction) and care quality (attended to by a doctor, no wait time, sufficient duration of dialysis per session, and AC ward) at the patient's endline primary hospital. Detailed results for each of the components of the indices are reported in the Appendix. We find small and insignificant changes in the probability of infection or bleeding, both in the pooled and hospital sector-specific samples. Perceived quality decreased significantly among private hospital patients, and increased significantly among public hospital patients. This is driven by positive changes in perceived respectfulness of staff and overall patient satisfaction in the public sub-sample, and by negative changes in perceived cleanliness in the private sub-sample (results in Appendix).

Table 3.6: Treatment Effects: Quality in Last 4 Weeks (HD Sample)

	(1) Fistula infection	(2) Fistula bleeding	(3) Primary Hospital perceived quality index	(4) Primary Hospital technical quality index
Treatment	0.039	0.012	-0.097	0.035
	(0.031)	(0.028)	(0.073)	(0.077)
Heterogeneity by sector				
Treatment	0.046	0.018	-0.169**	0.020
	(0.035)	(0.030)	(0.081)	(0.085)
Treatment x Public	-0.036	-0.033	0.387**	0.076
	(0.079)	(0.070)	(0.183)	(0.194)
Hospital x Wave FE	Yes	Yes	Yes	Yes
Observations	747	748	731	753
Public Treatment p-value	0.410	0.814	0.045	0.838
Control Mean	0.206	0.148	-0.012	-0.037
Control Mean (Pvt)	0.195	0.141	0.098	0.076
Control Mean (Pub)	0.253	0.173	-0.455	-0.484

See Table 3 notes. Primary Hospital technical quality is a composite index of dummies for no more than half hour wait time, dialysis for 3 or more hours (generally considered the minimum sufficient duration), AC ward, and attended to by medical staff at the patient's primary hospital in the last 4 weeks. Primary Hospital perceived quality is a composite index of dummies for whether the patient reported very respectful staff, very clean facility, being very satisfied with care and cost, and that she would recommend the facility to others.

3.5.3.2 Low density (LD) sample

Table 3.7 presents effects of the information intervention for the LD sample on patient awareness and their voice / exit strategies. As their pre-intervention primary hospital had no neighboring BSBY dialysis hospitals within 10 kilometers, these patients were only provided information on their entitlements and how much the hospital is paid under BSBY. We had hypothesized that patients at these hospitals would not choose to exit to other hospitals, both because we did not explicitly provide this information and because the costs of switching to another hospital would be high. In fact, we find that switching rates are high in the private control group (18% switch on average; Column 2), that they are mostly visiting other private hospitals (Column 4), and the distance between patients' pre-intervention and endline primary hospitals is a little over 12km on average (so beyond the 10-km radius we used; Column 5). The information intervention results in a 7-percentage point *reduction* in hospital switching, driven entirely by private hospital patients. In other words, the intervention resulted in patients visiting the same hospital more consistently, but because we do not have patient residence location, we cannot determine whether this means they are traveling a shorter or longer distance from their homes for dialysis treatment. Both public and private hospital patients were also substantially more likely to bargain with their hospitals in response to the information intervention (11.8 percentage point increase on average; Column 6).

Table 3.7: Treatment Effects (LD Sample)

	(1) BSBY awareness index	(2) Switched away from Primary Hospital	(3) Switched into public hospital	(4) Switched into private hospital	(5) Distance baseline to endline Primary Hospital	(6) Bargained with hospital(s)
Treatment	-0.055	-0.070*	-0.031	-0.060**	-5.403*	0.118**
	(0.156)	(0.037)	(0.022)	(0.029)	(2.977)	(0.045)
Heterogeneity by sector						
Treatment	0.074	-0.197**	-0.062*	-0.166***	-14.988**	0.135*
	(0.260)	(0.060)	(0.037)	(0.048)	(4.870)	(0.076)
Treatment x Public	-0.203	0.200**	0.049	0.167**	15.085**	-0.028
	(0.326)	(0.075)	(0.046)	(0.060)	(6.120)	(0.095)
Hospital x Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	188	188	188	188	186	188
Public Treatment p-value	0.775	0.005	0.217	0.003	0.010	0.036
Control Mean	-0.010	0.096	0.038	0.067	6.016	0.067
Control Mean (Pvt)	-0.128	0.175	0.050	0.150	12.236	0.150
Control Mean (Pub)	0.064	0.047	0.031	0.016	2.226	0.016

LD sample only. See Table 3 notes. BSBY awareness index is a composite index of dummies for whether knows all costs, dialysis costs, and tests/medicines costs are covered, and knows the hospital BSBY reimbursement rate per dialysis visit. The outcome variable in Column 5 is the distance between the patient's pre-intervention and endline primary hospitals in kilometers.

Table 3.8 presents the outcomes of these strategies. Patients, particularly at private hospitals, saw increased OOP payments in the private sample (Columns 1 and 2), which was partially offset by lower payments for tests and medicines off-site, resulting in a small, insignificant increase in total OOP payments for private hospital patients and no change for public hospital patients, and no meaningful changes in quality. Because the LD sample is small (note that this was not due to our sampling strategy but because most dialysis hospitals are concentrated in the same area), these estimates are not very precise.

3.6 Mechanisms and Discussion

Overall, we find that the intervention substantially increased awareness of the details of BSBY benefits and hospital reimbursement rates. Patients with a public primary hospital at baseline showed no increases in switching or bargaining, but saw large and significant declines in OOP, both at the hospital and for tests/medicines purchased outside it. One possible reason that OOP declined without explicit bargaining is

Table 3.8: Treatment Effects (LD Sample)

	(1) Any OOP payment at hospital	(2) Amount paid at hospital	(3) Got test/meds elsewhere	(4) Total OOP payment	(5) Primary Hospital perceived quality index	(6) Primary Hospital technical quality index
Treatment	0.092	491.960*	-0.141**	145.141	-0.073	-0.006
	(0.056)	(249.795)	(0.071)	(396.243)	(0.150)	(0.139)
Heterogeneity by sector						
Treatment	0.140	907.034**	-0.068	467.155	0.108	0.081
	(0.093)	(413.373)	(0.118)	(658.188)	(0.249)	(0.231)
Treatment x Public	-0.074	-655.305	-0.116	-508.385	-0.286	-0.137
	(0.117)	(520.528)	(0.148)	(828.804)	(0.314)	(0.290)
Hospital x Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	183	183	181	183	185	188
Public Treatment p-value	0.212	0.068	0.106	0.775	0.586	0.894
Control Mean	0.231	850.192	0.524	2231.394	0.043	0.134
Control Mean (Pvt)	0.500	2029.500	0.513	3702.000	0.017	-0.010
Control Mean (Pub)	0.063	113.125	0.531	1312.266	0.060	0.224

LD sample only. See Table 3 notes. Primary Hospital technical quality is a composite index of dummies for no more than half hour wait time, dialysis for 3 or more hours (generally considered the minimum sufficient duration), AC ward, and attended to by medical staff at the patient's primary hospital in the last 4 weeks. Primary Hospital perceived quality is a composite index of dummies for whether the patient reported very respectful staff, very clean facility, being very satisfied with care and cost, and that she would recommend the facility to others.

because patients were able to signal their awareness of benefits more implicitly – e.g. if they simply asked about their entitlements under BSBY– in ways that changed the behavior of public hospital staff. Alternatively, the bargaining may have happened immediately after the intervention and was not captured in our endline survey, which was conducted 7 to 8 weeks after the intervention and only collected information on the previous 4 weeks. Public hospital patients, who are poorer and likely to be more price sensitive, may have chosen not to switch to other private hospitals because of the widespread perception that they are more expensive than private hospitals. Finally, we find patients at public hospitals were more likely to be satisfied with their care at endline – since they did not switch facilities, this likely reflects increased satisfaction with getting the same services at lower prices.

Patients with a private primary hospital at baseline were significantly more likely to switch to a different hospital by endline in response to the intervention, and most of this was into public hospitals. They did not necessarily switch into one of the hospitals named in the information intervention, but visited other nearby BSBY hospitals, suggesting the information encouraged patients to search more broadly for a hospital that meets their needs. This did not result in lower OOP overall, possibly because 1) most patients neither switched nor bargained, and 2) those who switched to other private hospitals saw an OOP increase that offset the OOP decreases for those switching to public hospitals.[29] The failure of the information intervention to affect OOP among private hospital patients may be due to a combination of demand and supply side factors that we cannot disentangle. On the demand side, it is possible that these patients were willing to pay OOP if they believed it compensated for high quality care: the private HD sub-sample is wealthier, more likely to already have bargained with their hospital, and more likely to think their hospital was higher quality (but not lower priced) than other nearby BSBY hospitals. Pre-intervention measures of care technical and perceived quality are also higher in the private sub-sample. On the supply side, it is possible that hospitals charge OOP to compensate for BSBY reimbursements that are too low to cover their costs, or because they know demand for dialysis care is relatively inelastic within their patient pool. Treated patients in the private sub-sample see no substantial changes in technical quality, but report significantly lower perceived quality, which may be driven by those who switched to public hospitals, where quality is typically lower, or because more informed patients are now less satisfied with having to pay for their care.

A small number of dialysis patients in our sample had a "low density" (LD) primary hospital at the beginning of the study – i.e. where there is no other BSBY hospital within 10km, so only information on BSBY entitlements was provided. These patients were not given the second information component on neighboring hospitals and we analyze them separately. However, we note that the sample size is much smaller and estimates for some outcomes are imprecise. We find the information intervention significantly *reduces* hospital switching in the private sub-sample of LD treatment group relative to the control group, where switching rates were surprisingly high. Examining the distances between pre-intervention and endline primary hospitals suggests that LD patients travel beyond the 10-km radius around the hospital that we use to identify neighbors to seek treatment at multiple facilities, and that the intervention reduces this.

[29] In a simple comparison of mean OOP levels among private hospital patients who switched primary hospitals at endline, we find that those who switched to a public hospital paid INR 3309 and those who switched to a private hospital paid INR 6500, but cell sizes are very small.

As predicted by our theory, LD patients at both public and private hospitals are also more likely to exercise voice by bargaining with their hospitals in response to the treatment. However, these strategies result in *higher* OOP payments at the hospital for the private sub-sample, which is partially offset by lower likelihood of payments for tests and medicines elsewhere, resulting in no change in total payments. There were no meaningful changes in OOP payments for the public sub-sample. One possible explanation for these findings is that LD patients travel further to get cheaper care when they can, and that the information treatment encouraged them to instead stay and bargain with their hospitals. This did not reduce OOP payments at the hospital, but may have reduced the distance patients have to travel and associated costs (however, because we do not have patient residence locations we cannot verify this).

One limitation of our study is that we only focused on dialysis. Nevertheless, the evidence from dialysis may generalize to other tertiary care services better than the accountability literature that has largely focused on primary care, because tertiary care is typically delivered in specialized facilities where the hospital holds substantial power and patient-driven accountability may have limited potential. However, because dialysis care can be planned in advance and requires repeated visits with numerous opportunities for shopping and negotiation, the effects of information may larger than for emergency tertiary care. A second limitation is that we were unable to provide information on hospital quality due to the unavailability of sufficiently reliable data. Research from the U.S. finds that quality information is a critical component of interventions to encourage hospital comparisons, although this is in a context where patients are largely insured against prices. Future research in India could directly compare the effects of providing price information with or without quality information. Finally, we rely on patient recall for our measures of patient behaviors, OOP charges, and technical quality. Although recall of major health events has been found to be reliable, it is possible that we did not capture some of the ongoing expenditures that patients may be less likely to remember but that are common for dialysis care (tests and medicines) and that our measures of exit and bargaining did not capture the full range of strategies patients actually used with hospitals (Das 2012).

3.7 Conclusion

Public health insurance programs are being rapidly scaled up across India and other low- and middle-income countries. However, without appropriately designed incentives, including adequate monitoring and accountability systems, these programs may not achieve their goals and benefit the target population. Our study finds high OOP expenditures in both public and private hospitals in a large public health insurance program in Rajasthan, India, although care is supposed to be provided free. Providing patients information about their entitlements and health facilities available to them is a low-cost and scalable intervention with the potential to improve 'bottom-up' accountability.

We experimentally test a phone-based information intervention among dialysis patients under health insurance. The intervention improved beneficiary awareness of their entitlements and helped them try to bargain and/or switch to other hospitals to obtain care that meets their needs at lower cost. These changes did not lead to significant reductions in patient financial outlays overall, but we find substantial heterogeneity by hospital sector. Patients visiting public hospitals in high density markets experience a large and significant reduction in OOP payments, which may reflect decreases in side payments to hospital staff and for expenses that the program covers. However, patients visiting private hospitals, despite being significantly more aware of their entitlements under BSBY, see no effect on OOP charges. Our findings suggest patient-driven accountability may be an important tool in improving the effectiveness of health insurance programs, but may not substitute for improved top-down monitoring and appropriate incentive-setting for hospitals.

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APPENDIX

Baseline Balance

Table A3.1 presents t-tests comparing pre-intervention characteristics of patients assigned to the treatment and control groups, split by whether they were in the HD or LD sub-sample. Because we did not conduct a baseline survey in the control groups, we use characteristics from the administrative claims data to test balance. We find no significant differences between the treatment and control groups in either of the sub-samples. The p-values of joint tests of significance for all baseline characteristics are 0.447 in the HD sample and 0.229 in the LD sample.

Table A3.1: Baseline Balance Across Intervention Groups

Panel A: HD sample

	Treatment Mean	SD	Control Mean	SD	T=C P-value
Private hospital	0.77	0.42	0.76	0.43	0.66
Dialysis hospitals within 10km	2.58	0.74	2.57	0.76	0.91
Hospital dialysis patients	23.05	17.31	24.33	17.34	0.19
Female	0.32	0.47	0.33	0.47	0.71
Age (yrs)	45.54	15.19	46.10	15.17	0.52
Weeks on dialysis	14.22	11.47	13.95	11.52	0.68
Weekly dialysis visits	1.65	0.52	1.62	0.51	0.36
Observations	634		606		1240

Panel B: LD sample

	Treatment Mean	SD	Control Mean	SD	T=C P-value
Private hospital	0.37	0.48	0.36	0.48	0.87
Dialysis hospitals within 10km	0.00	0.00	0.00	0.00	.
Hospital dialysis patients	28.06	18.65	27.18	18.85	0.70
Female	0.31	0.47	0.31	0.46	0.91
Age (yrs)	44.27	14.24	43.57	14.89	0.70
Weeks on dialysis	18.72	10.24	17.69	10.77	0.43
Weekly dialysis visits	1.67	0.41	1.68	0.44	0.99
Observations	127		136		263

The p-value on the F-test for joint significance of coefficients is 0.447 for the HD sample and 0.229 for the LD sample. Because we did not conduct a baseline survey in control groups, we check balance using baseline administrative claims data.

Attrition

Attrition was substantial and due largely to wrong or invalid phone numbers in the claims data, or to deaths by the time of the survey. **Table A3.2** compares attrition at endline across the treatment and control groups for the HD and LD sub-samples. We find no differential attrition in the HD group. However, the LD treatment group was 10 percentage points less likely to be reached at endline than the LD control. Age and how long a patient had been on dialysis within BSBY at the time of sampling are significantly associated with attrition at endline. We control for these characteristics in all regression estimates of treatment effects. Even if attrition is not differential across groups, it may have caused compositional differences across them by endline.

Table A3.2: Attrition at Endline

	(1) Household reached at endline	(2) Household reached at endline	(3) Respondent is dialysis patient herself	(4) Respondent is dialysis patient herself
High Density Treatment	-0.037	-0.038	0.041	0.027
	(0.026)	(0.026)	(0.033)	(0.031)
Low Density Treatment	-0.100*	-0.102*	0.045	0.038
	(0.056)	(0.056)	(0.068)	(0.063)
Female		0.024		-0.311***
		(0.026)		(0.030)
Age		-0.029***		-0.053***
		(0.008)		(0.010)
On dialysis for 5+ wks at sampling		0.104***		0.085**
		(0.031)		(0.036)
Multiple visits per wk at sampling		0.027		0.087**
		(0.028)		(0.033)
New sample patient		0.071		-0.038
		(0.060)		(0.076)
Hospital x Wave FE	Yes	Yes	Yes	Yes
Observations	1502	1502	1107	1107
R^2	0.197	0.217	0.157	0.281
Control Mean (HD)	0.630	0.630	0.378	0.378
Control Mean (LD)	0.765	0.765	0.397	0.397

All variables come from administrative claims data at the time of sampling. New sample patient is a dummy for whether the patient was sampled from fresh claims data between rounds 1 and 2 of the intervention.

Table A3.3 presents balance tests for pre-intervention characteristics from the administrative claims data for patients observed at endline (non-attriters). We find no significant differences in these characteristics across endline treatment and control groups in either the HD or LD sub-samples, and p-values for joint tests of significance are not significant at conventional levels (the p-value is 0.613 in the HD sample and 0.263 in the LD sample).

Table A3.3: Baseline characteristics of those reached at endline - HD sample

	Treatment Mean	SD	Control Mean	SD	T=C P-value
Private hospital	0.82	0.39	0.80	0.40	0.54
Dialysis hospitals within 10km	2.55	0.74	2.51	0.78	0.44
Hospital dialysis patients	20.59	14.70	21.43	14.71	0.43
Female	0.33	0.47	0.35	0.48	0.68
Age (yrs)	43.84	14.30	45.10	14.56	0.23
Weeks on dialysis	15.80	11.22	15.59	11.39	0.79
Weekly dialysis visits	1.68	0.49	1.65	0.47	0.39
Years of schooling	6.55	4.81	6.47	4.76	0.81
Asset index (PCA)	-0.02	1.67	0.11	1.75	0.34
Scheduled caste/tribe	0.17	0.38	0.19	0.39	0.55
Observations	380		382		762

Panel B: LD sample

	Treatment Mean	SD	Control Mean	SD	T=C P-value
Private hospital	0.37	0.49	0.38	0.49	0.86
Dialysis hospitals within 10km	0.00	0.00	0.00	0.00	.
Hospital dialysis patients	28.14	18.73	28.35	19.15	0.94
Female	0.30	0.46	0.33	0.47	0.72
Age (yrs)	44.79	14.40	42.75	14.53	0.33
Weeks on dialysis	19.12	9.90	17.61	10.81	0.32
Weekly dialysis visits	1.67	0.38	1.66	0.39	0.80
Years of schooling	6.63	4.54	6.38	5.02	0.73
Asset index (PCA)	-0.08	1.54	-0.08	1.41	0.99
Scheduled caste/tribe	0.25	0.44	0.27	0.45	0.78
Observations	86		104		190

The p-value on the F-test for joint significance of coefficients is 0.613 for the HD sample and 0.263 for the LD sample. Because we did not conduct a baseline survey in control groups, we check balance using baseline administrative claims data.

Spillovers

Given that we stratified patients by hospital before assigning them to treatment (which was necessary to increase power), it is possible that those who received the information talked to those in the control group within their hospital. Results shown in **Table A3.4** suggests that patients do discuss among each other: about 16% of patients in the control group report discussing about BSBY with other patients. This increases by about 20 percentage points among treatment patients. This could be in part due to treatment patients speaking to each other, but most likely some discussions also happened with control patients. Any such spillovers would mean we are underestimating the effects of information. The large and differential effects on BSBY awareness, a "first stage" outcome, across treatment and control increase confidence that the lack of substantial effects on downstream outcomes (patient responses, payments, and quality) for some subgroups is not due to the presence of spillovers.

Table A3.4: Spillovers (Pooled Sample)

	(1) Heard about Primary Hospital from patient on dialysis	(2) Knows dialysis patients at own hospital(s)	(3) Discussed BSBY with other dialysis patients	(4) Discussed dialysis/test/meds prices with other dialysis patients
Treatment	0.036	-0.021	0.186***	0.236***
	(0.027)	(0.031)	(0.031)	(0.029)
Heterogeneity by sector				
Treatment x Private	0.054*	-0.009	0.201***	0.278***
	(0.031)	(0.036)	(0.036)	(0.034)
Treatment x Public	-0.009	-0.051	0.147**	0.128**
	(0.050)	(0.058)	(0.058)	(0.054)
Hospital x Wave FE	Yes	Yes	Yes	Yes
Observations	941	941	941	941
Control Mean	0.171	0.660	0.245	0.163
Control Mean (Pvt)	0.180	0.643	0.241	0.151
Control Mean (Pub)	0.149	0.702	0.255	0.191

Detailed Results for Composite Indices

Tables A3.5, A3.6, and A3.7 provide results for each of the components of the composite indices for BSBY awareness, perceived quality, and technical quality.

Table A3.5: Details of information index (HD Sample)

	(1) Knows BSBY covers all costs	(2) Knows BSBY covers dialysis costs	(3) Knows BSBY covers medicines	(4) Knows BSBY hospital payment	(5) BSBY awareness index
Treatment	0.063*	0.102**	0.082**	0.112**	0.300***
	(0.036)	(0.034)	(0.032)	(0.038)	(0.075)
Heterogeneity by sector					
Treatment	0.061	0.106**	0.062*	0.143***	0.321***
	(0.040)	(0.037)	(0.035)	(0.041)	(0.082)
Treatment x Public	0.015	-0.021	0.106	-0.185*	-0.111
	(0.092)	(0.085)	(0.080)	(0.100)	(0.187)
Hospital x Wave FE	Yes	Yes	Yes	Yes	Yes
Observations	753	753	753	628	753
Public Treatment p-value	0.218	0.010	0.016	0.002	0.000
Control Mean	0.547	0.675	0.720	0.257	0.003
Control Mean (Pvt)	0.528	0.669	0.728	0.271	0.016
Control Mean (Pub)	0.623	0.701	0.688	0.190	-0.051

Table A3.6: Details of quality indices (HD Sample)

	(1) Very satisfied	(2) Very respectful	(3) Very clean	(4) PH perceived quality index
Treatment	-0.035	0.010	-0.081**	-0.097
	(0.036)	(0.034)	(0.036)	(0.073)
Heterogeneity by sector				
Treatment	-0.062	-0.018	-0.103**	-0.169**
	(0.039)	(0.037)	(0.040)	(0.081)
Treatment x Public	0.148*	0.148*	0.113	0.387**
	(0.089)	(0.085)	(0.091)	(0.183)
Hospital x Wave FE	Yes	Yes	Yes	Yes
Observations	731	730	730	731
Public Treatment p-value	0.159	0.209	0.039	0.045
Control Mean	0.638	0.273	0.408	-0.012
Control Mean (Pvt)	0.693	0.301	0.439	0.098
Control Mean (Pub)	0.419	0.162	0.284	-0.455

Table A3.7: Details of quality indices (HD Sample)

	(1) No wait for treatment	(2) Dialysis session was over 3 hours	(3) AC ward	(4) Attended by medical staff	(5) PH technical quality index
Treatment	0.009	-0.002	0.021	0.025	0.035
	(0.027)	(0.027)	(0.016)	(0.029)	(0.077)
Heterogeneity by sector					
Treatment	0.002	0.003	0.017	0.016	0.020
	(0.030)	(0.030)	(0.018)	(0.032)	(0.085)
Treatment x Public	0.038	-0.027	0.025	0.052	0.076
	(0.067)	(0.070)	(0.041)	(0.072)	(0.194)
Hospital x Wave FE	Yes	Yes	Yes	Yes	Yes
Observations	735	715	717	753	753
Public Treatment p-value	0.810	0.925	0.362	0.522	0.838
Control Mean	0.848	0.798	0.940	0.806	-0.037
Control Mean (Pvt)	0.872	0.859	0.955	0.807	0.076
Control Mean (Pub)	0.750	0.536	0.878	0.805	-0.484