Private Hospital Responses to Reimbursement Changes Under Government Health Insurance in India

Pascaline Dupas[1], Radhika Jain[2]

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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. Many of these programs reimburse hospitals using bundled payments, rather than fee-for-service, systems in order to contain program costs. Hospital reimbursement rates are a critical design element that shape hospital incentives under these programs, but the evidence on their effects on hospital behavior in lower income countries is limited. Exploiting a policy-induced natural experiment, and using over 1.6 million insurance claims and 20,000 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 evidence of substantial coding manipulation by private hospitals in response to increased reimbursements. Nevertheless, real service volumes also increase for services with higher reimbursement rates. Although services are supposed to be free for patients, informal out-of-pocket charges are pervasive under the program. Increasing reimbursements reduces these charges significantly, but hospitals capture about half the higher public subsidies instead of passing them through to patients. Pass-through is higher in less concentrated markets and we find no evidence of changes in care quality or patient composition that could explain the incomplete pass-through. This suggests that balance billing to compensate for too-low reimbursement rates only partly explains the observed out-of-pocket charges and that other factors, such as market power, influence the extent to which public subsidies benefit patients.

[1] Stanford University, email: pdupas@stanford.edu
[2] Stanford University, email: rjain19@stanford.edu
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1. Introduction

As achieving universal health coverage becomes a priority, governments in low and middle-income countries are expanding public health insurance programs to increase access to quality hospital care while protecting households from financial burden. In India, national and state governments have been implementing largescale public health insurance programs that aim to provide free care at public and empaneled private hospitals to their poorest people since 2007. In an effort to control costs, these programs typically adopt bundled payment systems that reimburse hospitals a fixed rate per admission, adjusted for diagnosis and procedure, rather than fee-for-service payments.[3] 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). However, there is little evidence on how private hospitals participate in these programs and, in particular, how they respond to changes in reimbursement rates.

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. Two years after the program was launched, 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 coding manipulation, healthcare volumes, hospital entry, patient out-of-pocket (OOP) charges, care quality, and patient composition.

We find substantial evidence of upcoding pre-reform and that this changes in response to reimbursement changes. Hospitals appear to trade off the rewards to coding manipulation against the risk of detection and allocate their upcoding efforts where they gain the most. However, we also find evidence of real volume increases. An INR1000 increase in the reimbursement for a service induces a 2-3% increase in service volume relative to services with no reimbursement change. We find OOP charges under the program are substantial and that 40% of patients paid OOP for insured care in 2017. Although increasing hospital reimbursements decreases patient OOP charges significantly, hospitals capture approximately half of the increased public reimbursements. In other words, for every INR100 paid by the government to hospitals, only approximately INR50 is passed through to patients in the form of lower OOP payments. 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. Exploring heterogeneity by measures of market competition, we find that pass-through is higher in markets with more hospitals and lower market concentration. Finally, we find no meaningful changes in the socioeconomic and demographic composition of patients in the program.

[3] 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).

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) increasing reimbursement rates can encourage hospital entry and increase service volumes respond, but 2) they also change hospital upcoding incentives, 3) balance billing, where hospitals use OOP charges to compensate for too-low reimbursement rates, can only partially explain the observed OOP charges to patients, and hospital capture of subsidies contributes substantially, and 4) 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.

Hospital coding manipulation has been documented in both descriptive and causally identified studies in higher income countries (Dafny 2005, Silverman and Skinner 2004). We contribute evidence from India, a lower income country, where largescale public health insurance programs are relatively new, health care markets are largely unregulated, and the capacity to monitor hospitals limited. We also add 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 provides background information on the insurance scheme under study, and the policy reform we exploit. Section 3 presents the conceptual framework guiding our analysis. Section 4 describes the data, Section 5 provides program descriptive statistics, and Section 5 explains the empirical strategy. The results are presented in Section 7 and we discuss the findings and conclude in Section 8.

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 INR300,000 (~$4500) 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.[4] 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.

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.

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. Reimbursement rates under bundled systems are fixed at Rp for all patients receiving the bundle (in our context, these are referred to as "packages"). The marginal cost of care Cp depends on the patient's illness severity and can vary within a package. If the reimbursement rate for a package Rp is set lower than marginal cost Cp, the hospital has the option to not provide the package ("selection"), reduce the quality of care, provide the service but bill for a higher-reimbursed service

[4] 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.

("upcoding" , Rp' ≥ Cp), provide the service and charge patients additional cash to make up the difference between the reimbursement rate and marginal cost ("balance billing", Rp + cash ≥ Cp), or provide a higher reimbursed service (Rp' ≥ Cp'). However, particularly where information asymmetries exist between the Insurer and the hospital and monitoring capacity is weak, profit maximizing hospitals may choose to upcode to maximize revenues regardless of care costs or patient charges. Given a non-zero threat of detection, hospitals will upcode where the rewards relative to the risk of detection are greatest. When the reimbursement rate for a package increases, the hospital may be more willing to accept patients, lower the amount it charges patients (pass-through into OOP), or improve the quality of care. Both reduced charges and higher quality 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 hospital may also reallocate upcoding to the services where the gains from upcoding are highest (distance between Rp' and Rp is greatest).

The assumption of perfect competition may not be realistic for secondary and tertiary health care markets that have barriers to entry and where other factors, such as low patient awareness about their entitlements or ability/inclination to shop are prevalent. 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.

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.

a. 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 93 fully matched packages across 18 service clusters, ensuring that the highest volume services were included. Although these comprise a relatively small share of all BSBY packages, they account for approximately 70% of all claims filed during the study period. We then calculated the reimbursement rate change across Phases 1 and 2 for each package as well as the predicted rate change for each cluster. A detailed discussion of the package matching and package-level and cluster-level rate change calculation, as well as descriptive information on the services included in our study sample and corresponding reimbursement rates are presented in the Appendix.

b. 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).[5] 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.

5. Descriptive Statistics

Figure 1 demonstrates that private hospitals are playing an increasingly important role in BSBY over time and account for a substantial share of total public spending on the program. Although BSBY assures patients of free hospital care at public and empaneled private hospitals, we find evidence of substantial OOP charges at empaneled hospitals across a range of services, despite the fact that these hospitals have filed claims and been reimbursed for these patient visits (Figure 3). Mean patient payments directly to the hospital were INR2000. To put these numbers in context, this constitutes 13% of total hospital revenue (reimbursement plus OOP charge). Figure 4 presents the results of separate regressions of OOP charges at the hospital on measures of patient risk and illness severity, care quality, awareness of BSBY, and socioeconomic status. We include hospital and package fixed effects to examine variation across patients receiving the same service at the same hospital. Riskier and complicated cases are associated with higher charges, while awareness is strongly associated with lower charges. These patterns are consistent with hospitals charging more to cover the higher costs of complicated cases, but also with hospitals simply exploiting patients who are less informed about their entitlements. One and a half years after program launch, awareness about BSBY details among patients remain low and only about 50% of patients are aware that all costs of care are supposed to be covered by the program, even after they have received care under the program.

[5] 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.

Figure 6 presents time series data on hospital entry, total transaction volumes, and total reimbursements through the study period for the services we study. We separate the sample 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. Total transactions among panel hospitals decrease slightly from the pre-reform mean of approximately 30,000 per month, but increase after April. Entrants have relatively lower average transaction volumes but account for about 15% 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.

6. Empirical Strategy

Our empirical strategy exploits the variation in reimbursement rate changes across 93 packages in 18 service clusters between Phase 1 and 2. Figure 7 demonstrates that there is substantial variation in the magnitude of rate change across packages and clusters, 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. We calculate rate change at the package level as the difference between post-reform and pre-reform rates. We also calculate a cluster-level predicted rate change, which is effectively the average cluster level rate change taking into account the package composition of each cluster across all hospitals in Phase 1 (discussed further in the Appendix). This allows us to run our DID estimations at both the package and cluster level.[6] Our study period spans the months June 2017 through July 2018. To allow effects to change over time, we use two post-reform dummies: Post-reform short run (SR) is a dummy for January through March 2018 and post-reform long run (LR) is a dummy for April 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 the effect of package reimbursement rate increases on claims volume and composition, we collapse the claims data to create a hospital-package-month level balanced panel and use the following specification:

$$Y_{pht} = \alpha_0 + \beta_1 RateChange_p * PostSR_t + \beta_2 RateChange_p * PostLR_t + \gamma_p + \delta_h + \zeta_t + e_{pht}$$

where Y_{pht} is the outcome for package p in hospital h in month t; *RateChange*PostSR and RateChange*LR* are the absolute change in rates between Phase 1 and Phase 2 in rupee terms interacted with each of the Post dummies described above; we include package, period, and hospital fixed effects; e_{pht} is the error term clustered at the hospital-package level. β_1 and β_2 are the coefficients of interest and represent the change in the outcome for every unit increase in package rate change. For most outcomes, we present separate estimates for the pooled sample of all hospitals, including entrants and dropouts, as well as the panel of hospitals that participated in BSBY both before and

[6] This is necessary because we find evidence of substantial reclassification of services across packages within clusters, which could possibly bias our package-level estimates. As discussed in detail in the Appendix, while the package-level reimbursement rate change translates fairly precisely into a one-for-one increase in hospital reimbursements in Phase2, this is not the case for the cluster level predicted rate change. Nevertheless, our methodology ensures that the predicted rate change is orthogonal to hospital-cluster outcomes and is valid for identifying the effects of rate increases. For estimates of pass-through into patient OOP charges, we present both the change in reimbursement rate and the change in out-of-pocket charges as a result of the cluster-level rate increase.

after the reform. We include the pre-reform mean and the p-value on an F-test for joint significance of the Rate Change x PostSR/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. We collapse the claims data to create a hospital-cluster-month level balanced panel and estimate volume effects at the cluster level using a very similar specification with cluster predicted rate change as the treatment and include cluster fixed effects, with errors clustered at the hospital-cluster level.

To examine effects on OOP charges to patients, hospital revenue (reimbursements plus OOP charges), 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:

$$Y_{icht} = \alpha_0 + \beta_1 RateChange_c * PostSR_t + \beta_2 RateChange_c * PostLR_t + \gamma_c + \delta_t + e_{icht}$$

where Y_{ipht} is the outcome for patient *i* that received a service in cluster *c* in hospital *h* in month *t.* We include cluster fixed effects 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). Again, we present pooled estimates for all hospitals as well as separate estimates for panel hospitals; in the latter case we include hospital fixed effects in the regressions. Standard errors are clustered at the hospital-cluster level.

For several of the survey-based outcomes, we create indices of closely related outcomes following Anderson (2008), where each outcome is demeaned, normalized by the standard deviation pre-reform, and weighted by the inverted covariance matrix. Indices were calculated separately for each package to allow weights to vary depending on the service received.

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 the several months of pre-reform data allow us to look for evidence of the first assumption. Figure 8 plots interactions of rate change with pre- and post-reform dummies relative to the excluded reference period of October-November 2017 to demonstrate that there were no diferential pre-reform trends across treatment groups in hospital monthly claims volumes at the package and cluster levels and out-of-pocket charges, but that all three outcomes changed significantly in response to the rate change reform.

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 charges. We find no evidence for differential changes in claims rejections, a proxy for monitoring, by package rate change (results in the Appendix).

7. Results

a. Changes in Package Claims

The time series data presented in Figure 6 demonstrates that the total volume of services included in our study increased from a relatively stable mean of approximately 30,000 transactions per month pre-reform to about 45,000 per month by July 2018 (50% increase), of which 7,000 were in newly entering hospitals.[7] However, among panel hospitals, total service volumes decreased initially and only increased beyond pre-reform levels after April 2018. Nevertheless, the DID estimations in Table 2 find that monthly log claims increase by 2.1% for every INR1000 increase in reimbursement rate among panel hospitals in the short run (January to March 2018); additionally, claims volumes for packages with no rate change decrease by a similar amount (coefficient on the Post SR dummy). Examining effects separately for packages with a positive and a negative rate change in Column 2, we find that that panel hospitals significantly increase claim volumes of packages with a rate increase and decrease volumes for packages with a rate decrease and the effect sizes are comparable. Including new hospitals that entered in Phase 2 reduces the size of the coefficients on the interaction terms in Column 3 but increases the size of the response to negative rate changes in Column 4.

These results suggest that panel hospitals already participating in BSBY pre-reform are changing the composition of packages for which they file claims and reallocating towards those with larger increases rather than increasing total volumes, particularly in the short run. While new hospitals contribute to total volumes, they appear to be less likely to participate in services with rate reductions.[8]

b. Evidence of Coding Manipulation

There are three possible explanations of these changes in package claims volumes within panel hospitals: hospitals could attract patients more in need of the services made more profitable by the reform (patient selection); they could change the services they provide without any underlying change in patient health needs (under/overprovision); and/or they could simply change their coding to claim these services without actually changing the care they provide (upcoding). We provide several pieces of evidence to suggest that a substantial share of the change in claims, particularly in the short run, is due to coding changes.

Immediate Changes in Claims Composition Induced by Rate Change: We first provide further evidence that there are large and immediate compositional changes in the claims filed within each cluster and these changes are strongly related to rate changes. We focus on panel hospitals that were participating in BSBY pre-reform. Figure 9, which plot each package's claim as a share of total weekly claims for all packages in the Vaginal Deliveries cluster and the Ear Surgeries (Tympanoplasty/Mastoidectomy) cluster at private panel hospitals, confirm that there are large and immediate changes in cluster package composition at the time of the reform. We observe immediate compositional changes across all the clusters we study and, as shown in Figure 10, these changes are strongly positively correlated with changes in the package rate. To examine compositional changes systematically, we present DID estimates with a package's share of total claims within the cluster of closely related packages as the outcome variable in Table 3. The treatment variable is an INR1000 increase in package reimbursement rate in Column 1; in Column 2 we present this in percent terms for comparability with the following columns; in Column 3 the treatment is the percent change in relative rate; and in Column 4 we include both percent rate change and percent relative rate change. The relative rate change is the percent

[7] Total transactions for all services increased from approximately 40,000 per month pre-reform to 70,000 per month in July 2018.

[8] Figure X shows that the most of the packages with rate reductions are for ear surgeries (Tympanoplasty/ Mastoidectomy Cluster). Hospitals providing these services tend to be smaller specialty hospitals focusing on Ear, Nose, and Throat procedures.

change in a package's rate-share of the total of all package rates in the cluster between Phases 1 and 2. A package with a 10% rate increase will have a higher relative rate increase if all other packages in the cluster had smaller than 10% increases than a package with a 10% increase where all other packages also increased by 10%. This is effectively a measure of the change in the distance between the rates of two packages within the same cluster, rather than the change in a package's rate relative to its pre-reform level, and is therefore a more direct measure of upcoding incentives. Given that there is some risk of detection with upcoding, hospitals have an incentive to upcode most where they gain most in reimbursements, which is a function of the distance between the rates of packages, or the relative rate change.[9] The results make clear that there are systematic compositional changes within clusters and that these are directly induced by changes in reimbursement rate changes. Composition is more responsive to changes in relative rates within the cluster than to changes in package rates relative to their pre-reform levels, even when the two are included together. These results suggest hospitals change their coding behavior strategically to maximize reimbursements subject to the threat of detection.

No Changes in Cluster Patient Composition: Changes in package composition took effect within a week of the reform and correspond directly to reimbursement changes. Any changes in patient selection in the short run are unlikely to 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 – and must be driven by hospital strategies to attract these patients. Although, in our context, there is likely to be a large untapped patient pool and selection mechanisms, such as village health camps organized by hospitals to identify prospective patients and hospital agreements with lower level health care providers for patient referrals and are anecdotally common, it is implausible that hospitals would be able to identify patient health needs with this level of specificity within weeks of the reform. Furthermore, if the changing service composition of clusters reflects successful selection of patients requiring the packages rendered more profitable after the reform, we would also expect to see changes in the patient composition within clusters. However, using the DID specification at the cluster level (the treatment is the cluster level predicted rate change), we find no meaningful change in several measures of patient risk and illness severity in Table 4.[10]

Changes in Survey Confirmation: If the changes in care were real, whether necessary or not for the patient, we should also find no changes in survey confirmation rates of the services received. 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.[11] We then use the same DID specifications to examine whether an increase in the

[9] While the threat of detection likely varies across packages within the same cluster (for example, the threat of detection for upcoding from a basic vaginal delivery to one with an episiotomy is likely to be lower than upcoding it to a delivery with pre-eclampsia management), this is unlikely to change discontinuously with the reform. We also show in Table X that rejection rates do not increase with reimbursement rates, indicating that the Insurer did not start monitoring packages with large increases more.

[10] The risk and complications index is created using the method discussed in Section 6 and a series of dummies for history of high BP and high BP during ANC, woman warned of pre-eclampsia during ANC, prior stillbirth or c-section, woman's age is over 40 years, last delivery 10+ years ago, heavy bleeding, fainting, convulsions, or multiparous birth).

[11] We focused on vaginal and c-section deliveries for survey confirmation because they were the only two service clusters for which we expected to be able to get reasonable survey responses to confirm the care received. It is unlikely that patients can distinguish between a tympanoplasty and a mastoidectomy, for example. The vaginal

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 (decrease) in a package's reimbursement rate leads to a decrease (increase) in the probability of confirmation, it provides evidence that a higher (lower) 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. We also created an indicator for cluster confirmation if the survey confirmed that the broader service type was confirmed for all clusters.

Table 5 presents package composition changes and survey confirmation rates for packages with increasing and decreasing relative rates in the vaginal and c-section delivery clusters. Pre-reform confirmation rates for bottom-coded packages are high and don't change with rate changes, even though their volumes increase substantially. However, for all other packages, packages with decreasing rates have decreasing volume shares and increasing survey confirmation rates, which is consistent with decreases in upcoding into these packages. Unfortunately, there are only two non-bottom coded package with an increasing relative rate, both of which have low volumes and are less represented in the survey data, making the estimates for it noisy. We only have surveys for a small sample of public hospital delivery patients, their survey confirmation rates are also substantially higher for non-bottom coded packages, which is consistent with lower coding manipulation in public hospitals, where financial incentives are weaker. We also show in Column 5 that confirmation at the cluster level is high and unchanged – i.e. there is no evidence of upcoding across clusters, most likely because the threat of detection is substantially higher (for coding a vaginal delivery as a c-section or as an ear surgery, for example) and because coordination across hospitals departments would be harder.

These results further support the explanation that some share of the observed compositional change is due to upcoding changes and not real changes in provision. Importantly, the fact that confirmations increase when volumes decrease indicates that, at least in the vaginal and c-section delivery clusters, upcoding decreased post-reform. 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. We cannot precisely estimate the degree to which the observed claims compositional changes are real or due to coding manipulation alone and, while coding manipulation is the most likely explanation in the short run, it is likely that financial incentives will lead to larger real changes in service provision – either through selection of patients with real needs or overprovision of unnecessary services – in the longer run.

Heterogeneity in Coding Changes: To examine the implications, in Figure 11 we plot the observed cluster level rate change (the actual average cluster level package rate in Phase 2 minus that in Phase

delivery cluster includes packages for "basic vaginal", "vaginal + forceps", "vaginal + episiotomy/tear repair", "vaginal + forceps", "vaginal + tubectomy", "vaginal + pre-eclampsia management", and "vaginal + 3 antenatal care visits"; the c-section delivery cluster includes packages for "basic c-section", "c-section + tubectomy", "c-section + pre-eclampsia management", and "c-section + 3 antenatal care visits". We were able to include survey questions to verify all of these.

1) for each hospital and cluster against the predicted cluster rate (which, as we describe above and in the Appendix, is the average rate change for a cluster weighted by the package composition of that cluster across all hospitals in Phase 1). If hospitals continued with their Phase 1 composition, the two should be equal; if the observed cluster rate change is lower than the predicted one, hospitals are making less on average per transaction in the cluster than they would have as predicted by the Phase 1 composition. We find that, on average, the observed rate change is lower than the predicted rate change for clusters with higher predicted rate changes and the opposite where predicted rate changes are negative or low. The vertical spread of the dots suggests there is also substantial heterogeneity across hospitals within each cluster, with some well above the forty five degree line and others below it. While this is a simple linear fit at the hospital-cluster level and does not account for differences in the volumes in each hospital-cluster, it suggests that a substantial share of hospitals may have decreased upcoding when rates increased and increased it when rates decreased.

Costs of Coding Changes: For a rough assessment of the costs of coding and reimbursement changes, in Figure 12 we plot observed government spending on reimbursements across all services in our study post-reform in Phase 2 (January to July 2018). We also plot spending we would have observed with Phase 2 total volumes but Phase 1 rates and with Phase 2 total volumes but Phase 1 rates and composition. This abstracts away from volume changes and allows us to look at the effects of increased rates and changes in composition. Note that, even if we assume all of the changes in composition to be due to coding manipulation, this is not the full cost of upcoding, but only the net cost to the government of changes in coding manipulation induced by the policy reform. Hospitals that did not change coding will not be captured and increases in upcoding in some areas and decreases in others (we find evidence of both) will offset each other. Comparing the three lines makes clear that increased rates would have increased spending even without any compositional changes, but that compositional changes increased spending by more. Overall, BSBY is clearly spending more in Phase 2 with the new package composition than it would have if package composition had not changed. However, the net magnitude of this greater spending is relatively small – an approximately 2.5% increase over the reweighted estimate.

c. Changes in Service Volumes

Given that we find evidence of coding manipulation across packages, difference-in-differences specifications at the package level may thus bias estimates of service volumes. However, we expect that coding manipulation is restricted to packages for closely related services, but not across totally different clusters of services. For example, a basic vaginal delivery may be upcoded as a vaginal delivery with episiotomy, but not as a c-section delivery or as an ear surgery. As the Insurer requires hospitals to submit supporting documentation for all claims filed and reviews a random selection of these, the threat of detection of fraud is substantially higher when coding manipulation is egregious. Our survey evidence above also finds little evidence of coding manipulation across clusters. Therefore, to evaluate the effects of rate increases on real service volumes we present the DID estimations at the cluster level using the cluster predicted rate change as the treatment variable in Table 5.

Panel A of the table presents changes in hospital cluster-level reimbursements as a result of the cluster predicted rate change and Panel B presents the corresponding changes in hospital cluster-level monthly volumes. As discussed in the Appendix, the predicted cluster level method may not result in an equivalent increase in hospital reimbursement because it is based on the overall Phase 1 composition of clusters and is not hospital-specific, and because hospitals may change their cluster composition in Phase 2. Nevertheless, the estimates are close to one. Interestingly, reimbursements for clusters with negative predicted rate changes also increase, although by much less. This is consistent with the increased hospital upcoding we see in Figure 11 for clusters with negative predicted rate changes. An INR 1000 increase in rate change leads to a 2-3% increase in hospital monthly cluster volumes within 6 months of the reform. Given that hospitals are still making positive average reimbursements on clusters with negative predicted rate changes, it is unsurprising that volume increases are smaller but still positive for these clusters. As with the package-level estimates, effects are smaller when newly entering hospitals are included.

d. Effects on Out-of-Pocket Payments

Table 6 presents the results of the DID specification with hospital reimbursements, patient OOP payments, and total hospital revenue (reimbursements plus OOP payments) as the outcome variables. If there are changes in upcoding between Phases 1 and 2, patients may be reclassified between packages so that the DID specification at the package level may be biased. We thus estimate effects on outcomes at the cluster level. The cluster level rate change is calculated as the mean rate change across all packages in the cluster, weighted by each package's share of all claims within the cluster in Phase 1 (explained further in the Appendix). 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 we use Tobit estimates to allow for censoring of the continuous OOP payment measure at zero.

Among panel private hospitals, an INR1 increase in the cluster level predicted rate change leads to an INR 1.23 increase in hospital reimbursements in the first three months post-reform and an INR 1.40 increase in the next four months. As discussed in the Appendix, the way we calculate our cluster predicted rate change means it is orthogonal to hospital outcomes but does not translate exactly into an equivalent reimbursement increase at the hospital level and this is greater in the survey sample, where estimates are noisier. The rate increase results in a substantially smaller change in OOP payments, which decrease by INR 0.65, or about half the reimbursement increase, in the short run. While the estimates of total hospital revenue are noisy, they allow us to reject complete pass-through. Estimates are similar when we all hospitals, suggesting lower charges by newly entering hospitals are not driving the effects (the sample size is also not much larger because, as discussed earlier, new entrants comprise a relatively small share of all transactions).

e. Heterogeneity in Payment Effects by Competition

We examine whether market structure can explain incomplete pass-through. Hospitals in more competitive markets should have a greater incentive to pass through public subsidies in order to lower prices faced by patients. 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-cluster 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 service cluster within a district. A higher HHI represents higher market concentration (lower competition). Second, we generate a district-cluster level hospital density measures that is the number of hospitals providing a cluster in a district in the pre-reform period. In both cases, creating cluster-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 7 presents results from the same DID specifications, splitting the sample into below and above median HHI and below and above median hospital density. Panel A presents results for private panel hospitals and Panel B presents them for all private hospitals pooled. We present OLS regressions for simplicity (Tobit estimates presented in the Appendix look very similar). Both measures of higher competition are associated with substantially larger decreases in patient OOP charges. 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 of public subsidies.

f. 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. However, as we show in Table 4, we find no evidence of changes in patient risk composition.

Hospitals may also respond to increased reimbursement rates by increasing care 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 create indices of self-reported technical quality (seen by a doctor, skin-to-skin care, labor companion, warned of postpartum symptoms, called back for a checkup for deliveries; seen by a doctor, called for a check-up, and warned of problematic post-visit symptoms for all other services), post-visit complications (a series of conditions, including fever, pain, pus, bleeding, infection, or death), 'luxury' (AC room, private room, own bed), and perceived quality (very respectful, very clean, very satisfied, would recommend). All indices are created as discussed in Section 6. Table 8 presents results. Among panel hospitals, we find evidence of a small increase in care quality: post-visit complications decrease and self-reported technical quality increases slightly. But these effects are small and we find no changes in luxury or patient perceived quality. Including newly entering hospitals does not change the estimates much. Overall, these results suggest hospitals do not substantially increase care quality in response to the reimbursement rates. While these effects may increase in the longer run, they allow us to conclude that accepting costlier patients or improving quality are not adequate explanations for incomplete pass-through.

g. 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. Table 9 presents the effects of rate change on patient gender, age, assets, schooling, low caste (indicator for scheduled caste or tribe), and BSBY awareness. Although OOP charges decrease substantially, we find little evidence that poorer households are now more likely to use BSBY. Instead, the asset index increases, while schooling, caste, and gender remain unchanged, and patient age decreases slightly. Furthermore, awareness of BSBY increases significantly after a few months, which may be the result of increased outreach by the government around the time of the reform as well as by hospitals interested in attracting more patients. To the extent that this was effective, it seems only to have drawn in relatively wealthy patients. The lack of effects on socioeconomic status may be because information about the lower prices hospitals are now charging may take time to spread through the population. It may also be possible that even with the price reductions the cost of seeking hospital care is still too high for the poorest populations.

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.

We use administrative claims data linked to patient surveys, and exploit a policy-induced change in hospital reimbursements to conduct a difference-in-differences analysis of hospital responses to changes in reimbursement rates. 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. We find that increased reimbursements induce an immediate increase in the volume of claims filed for a package relative to packages that did not experience an increase. Particularly in the first few months after the reform, these volume changes reflect a change in the composition of closely related claims (i.e. a reallocation to now more profitable health care services) rather than a total volume increase. We find strong 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. Hospitals appear to trade off the rewards to coding manipulation against the risk of detection and, in many cases, reduce upcoding where it no longer as profitable. We also find evidence of real volume increases, but cannot disentangle whether the volume increase is for necessary care or reflects overprovision of services that are profitable but not necessarily needed. An INR1000 increase in the rate change for a service leads to a similar increase in hospital reimbursements and a 2-3% increase in hospital monthly volumes for that service.

Next, we examine the extent to which increased government reimbursements to hospitals benefit patients in the form of lower out-of-pocket payments or improved services. First, we find evidence of substantial patient OOP payments pre-reform even though BSBY assures patients of free care. Increasing hospital reimbursement rates decreases payments substantially, suggesting that at least part of the reason hospitals charged was to cover their costs. However, hospitals also capture a substantial share of the increased public reimbursements. Four to seven months after the policy reform, only approximately half of every additional rupee paid to hospitals by the government is passed through to patients in the form of lower cash charges. 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 it 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 or because information may take longer to percolate through the eligible household pool.

We find that pass-through is higher in markets with lower concentration and with more hospitals participating under BSBY. Although these results cannot be interpreted causally, as competition may be correlated with other factors affecting pass-through, they suggest that market power plays a role pass-through of public subsidies and is consistent with economic theory and other studies of pass-through in the context of health care (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 the extent to which public subsidies benefit patients rather than hospitals. Although our study only covers the 7 months after the reform, Figure 6 suggests 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.

Our results point to the importance of hospital reimbursement rates as a policy lever that shape hospital incentives and affect program outcomes under bundled payments insurance programs. Increasing reimbursement rates can encourage hospital participation and increase service volumes, but may also constitute a large transfer to hospitals rather than to patients. We cannot disentangle to what extent hospitals would participate in the insurance program without the ability to upcode or to charge patients, but the heterogeneity in effects across hospitals suggests that in some cases reimbursement rates may, in fact, have been too low to allow hospitals to participate in the program and simply cracking down on hospitals may have mixed welfare consequences. Given this, stronger monitoring systems could help ensure hospital compliance with program rules, but will likely need to be accompanied with efforts to rationalize reimbursement rates to accommodate local heterogeneity in input costs and reward care quality. Facilitating competition, including by public sector hospitals, and increasing patient awareness of their entitlements may also be important strategies for disciplining hospitals and ensuring public subsidies benefit patients.

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FIGURES

Figure 1: Public and Private Participation in BSBY Over Time

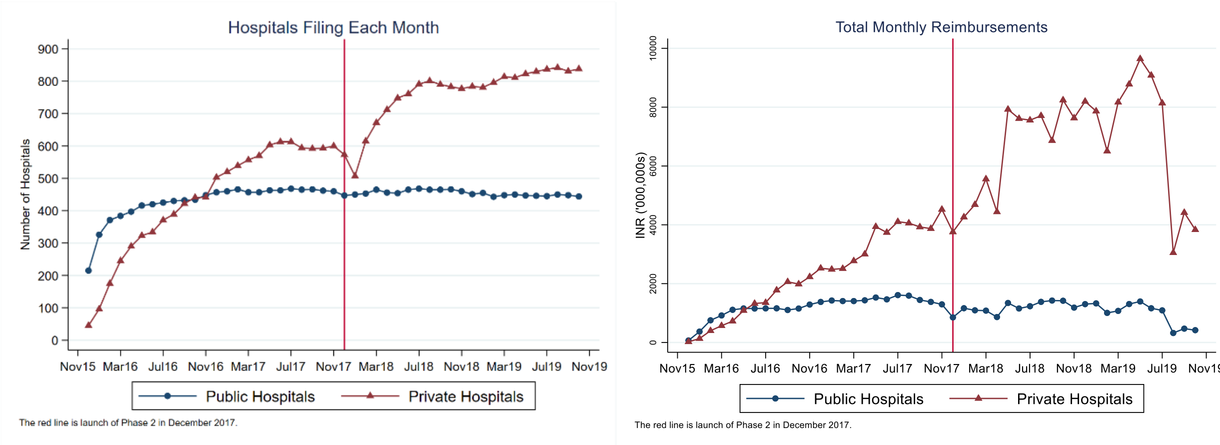


Figure 2: Geographic Distribution of Public and Private Hospitals in BSBY

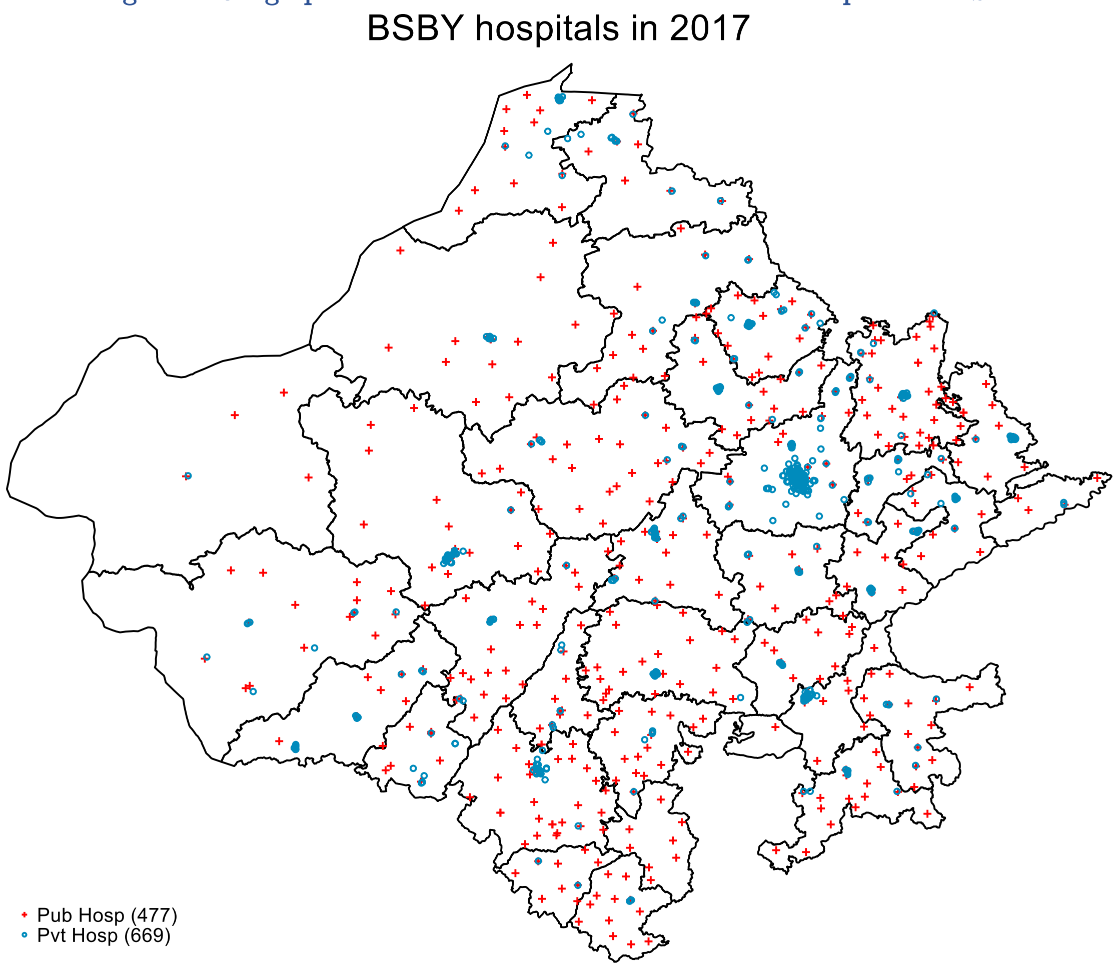
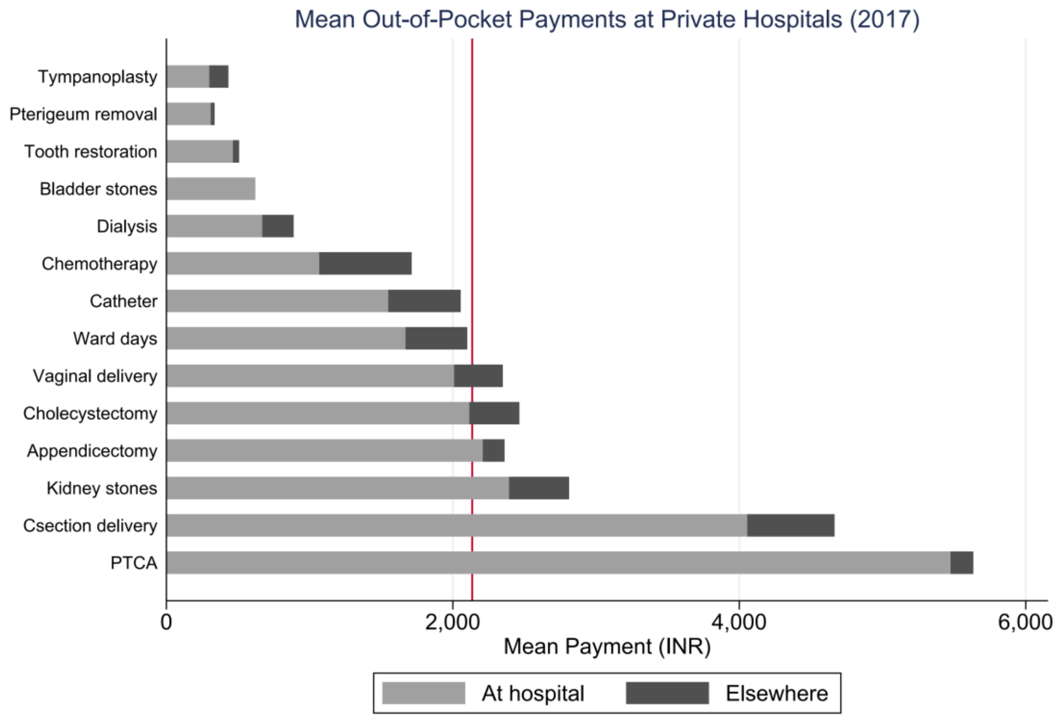
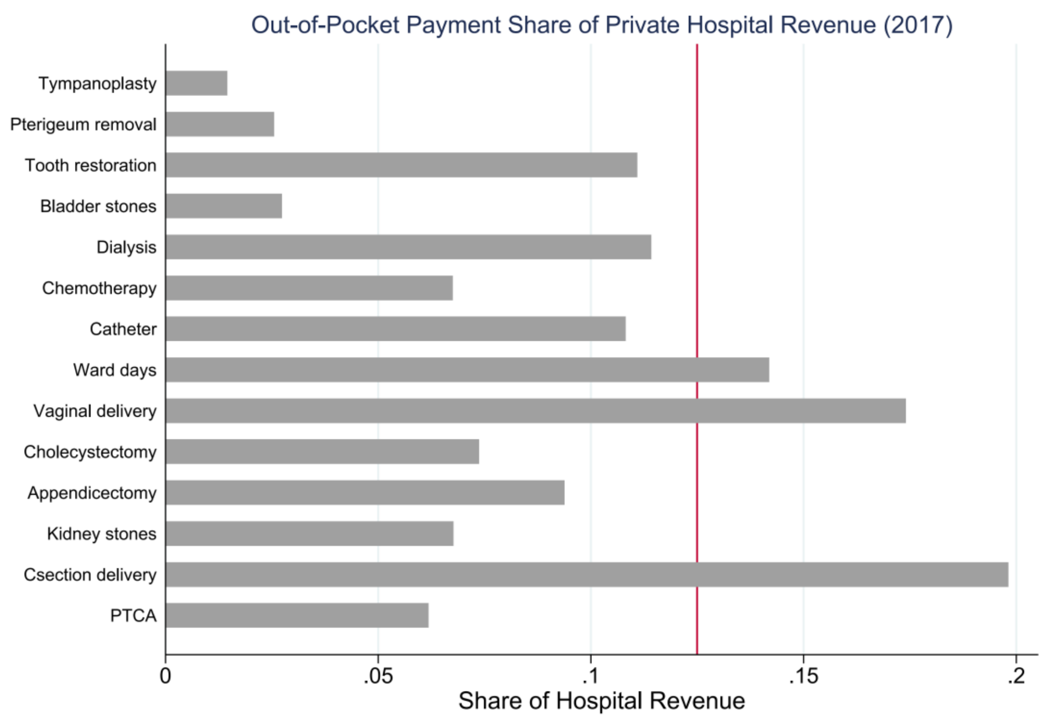


Figure 3: Out-of-Pocket Charges Levels and Share of Private Hospital Revenue

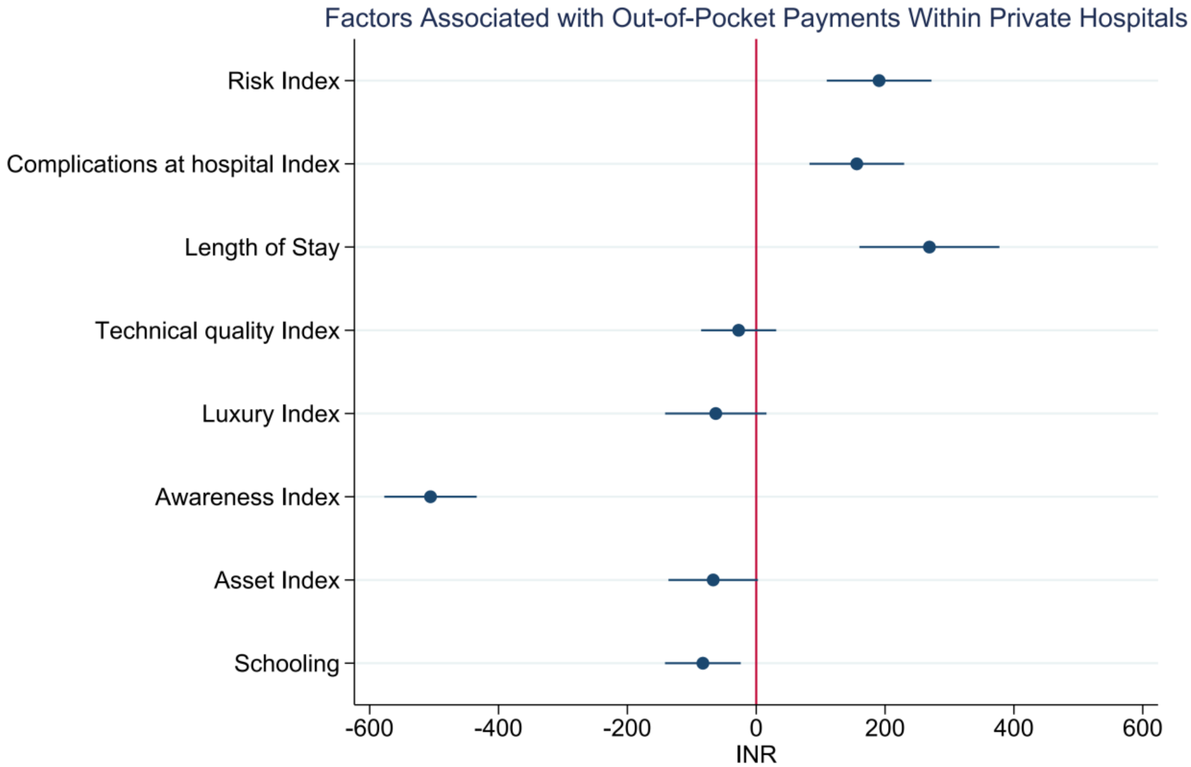


Red line is mean across all services



Hospital revenue is the sum of BSBY reimbursements and out-of-pocket payments to hospital. Red line is mean across all services.

Figure 4: Factors Associated with Out-of-Pocket Charges at Private Hospitals



Coefficients from bivariate regressions of total out-of-pocket payments on each variable on the y-axis with HospitalxPackage and Month fixed effects and controls for recall period and age.

Figure 5: Awareness of BSBY Among Patients Who Have Used the Program

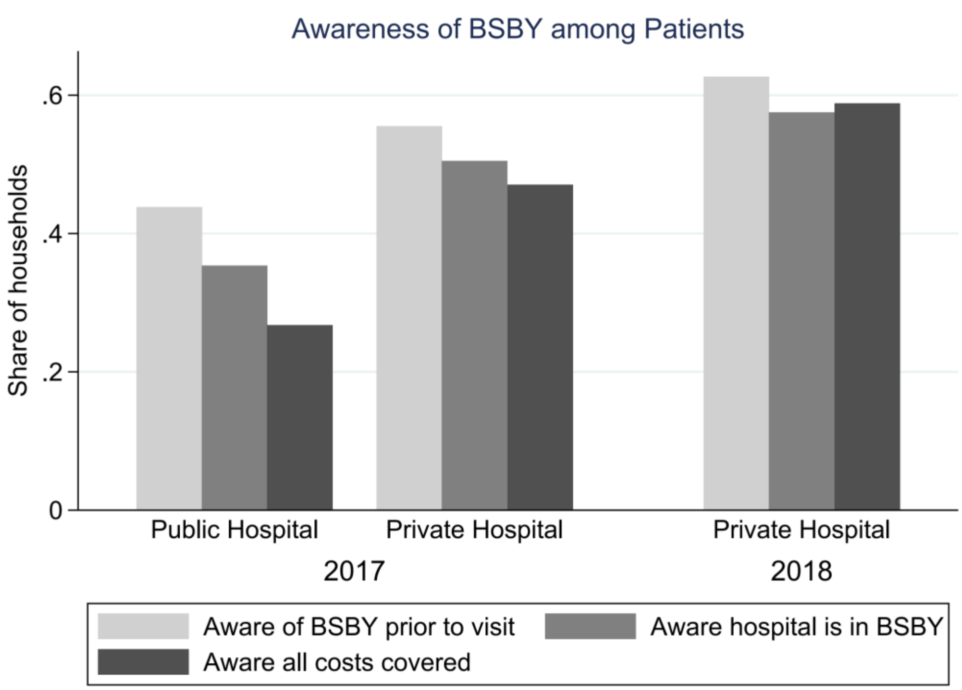
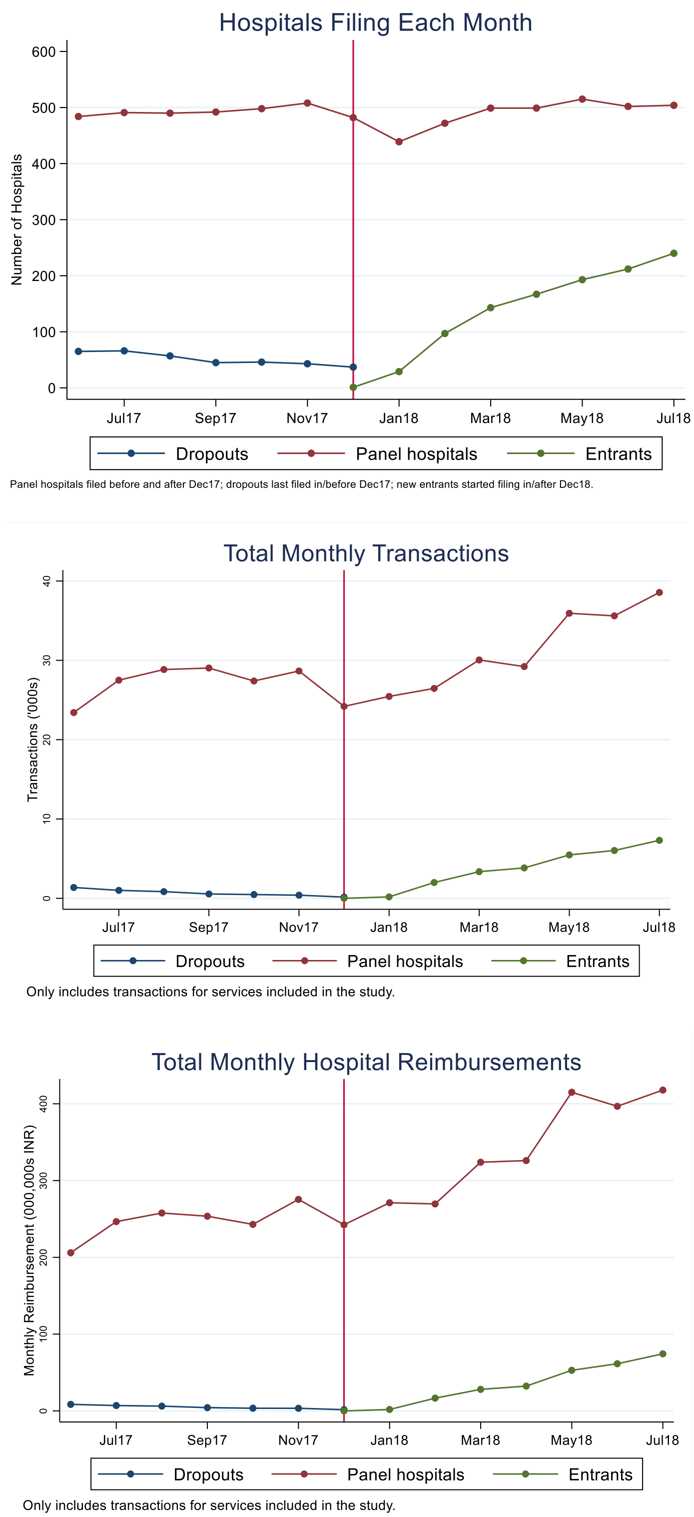
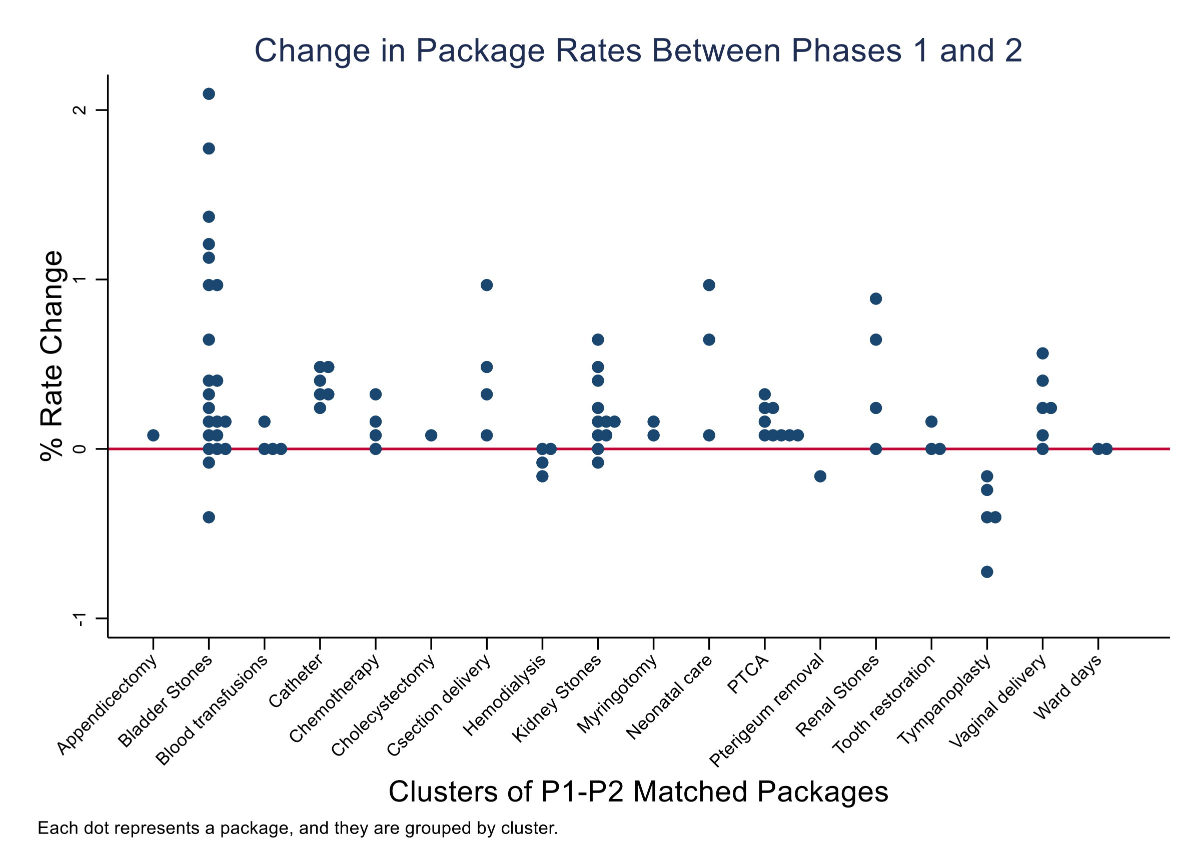


Figure 6: Private Hospital Entry and Transaction Volumes Pre- and Post-Reform



Note: We classify hospitals as "Panel" if they filed a claim at any point in the study period before and after December 2017, the reform month; as "Dropouts" if their last claim was before in or before December 2017; and as "Entrants" if their first claim was in or after December 2017. Figure 1 presents the number of hospitals of each type that filed any claims in a month. Figure 2 presents the total number of transactions filed each month by hospitals of each type.

Figure 7: Identifying Variation in Package and Cluster Reimbursement Rates



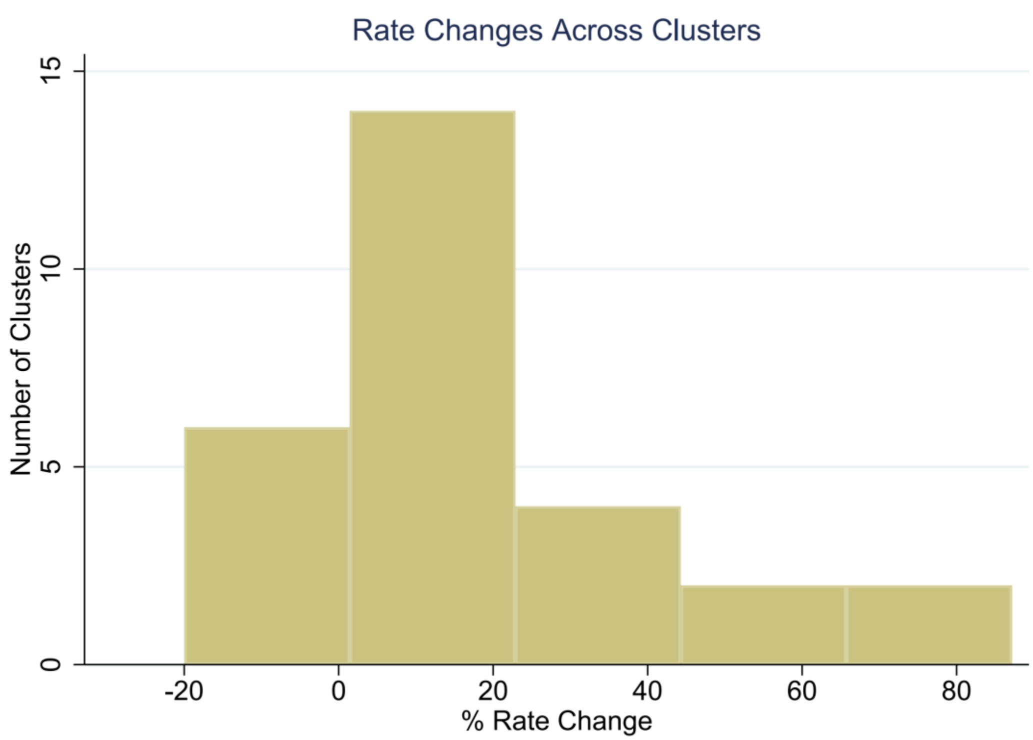


Figure 8: Pre- and Post-Reform Trends in Package and Cluster Volumes and Out-of-Pocket Charges

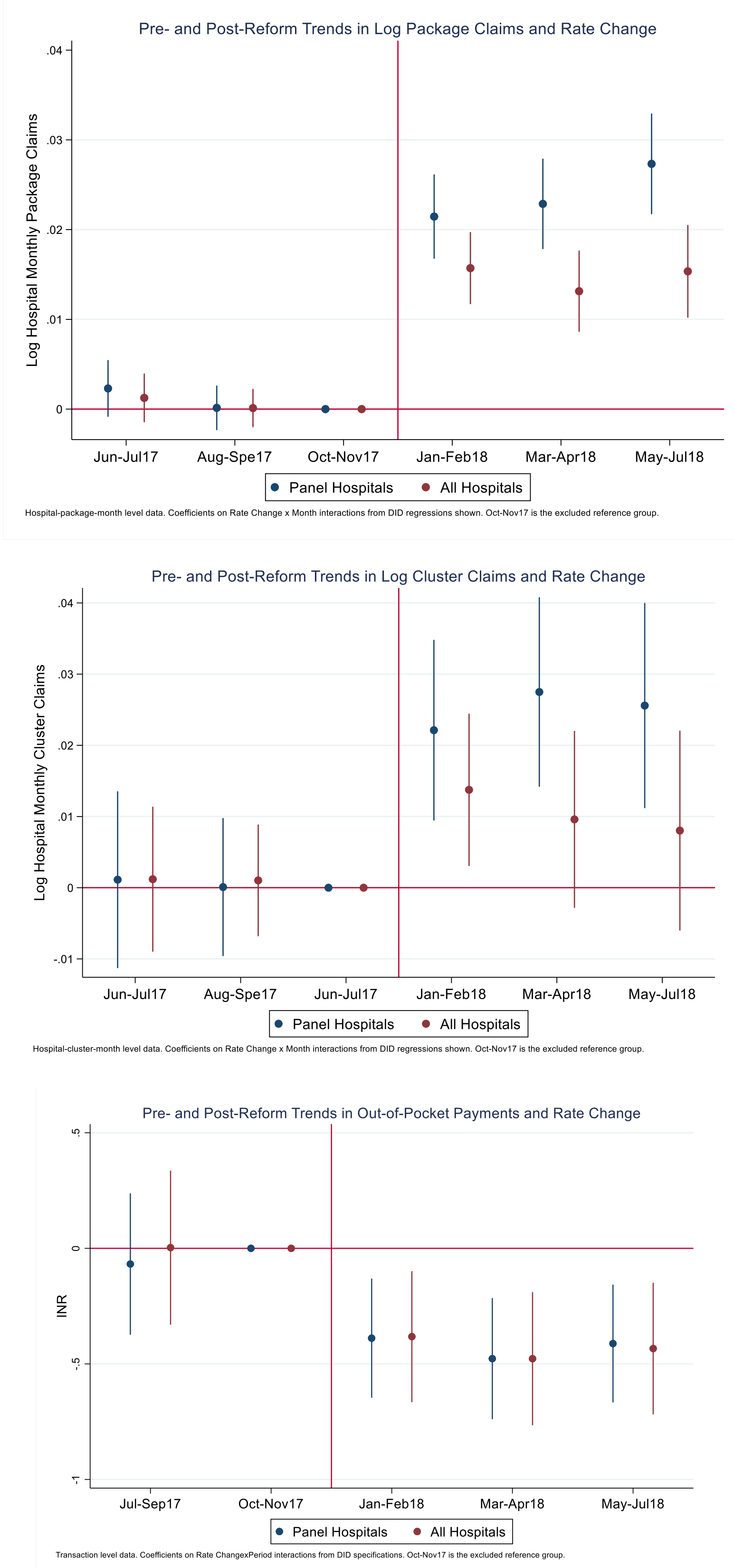
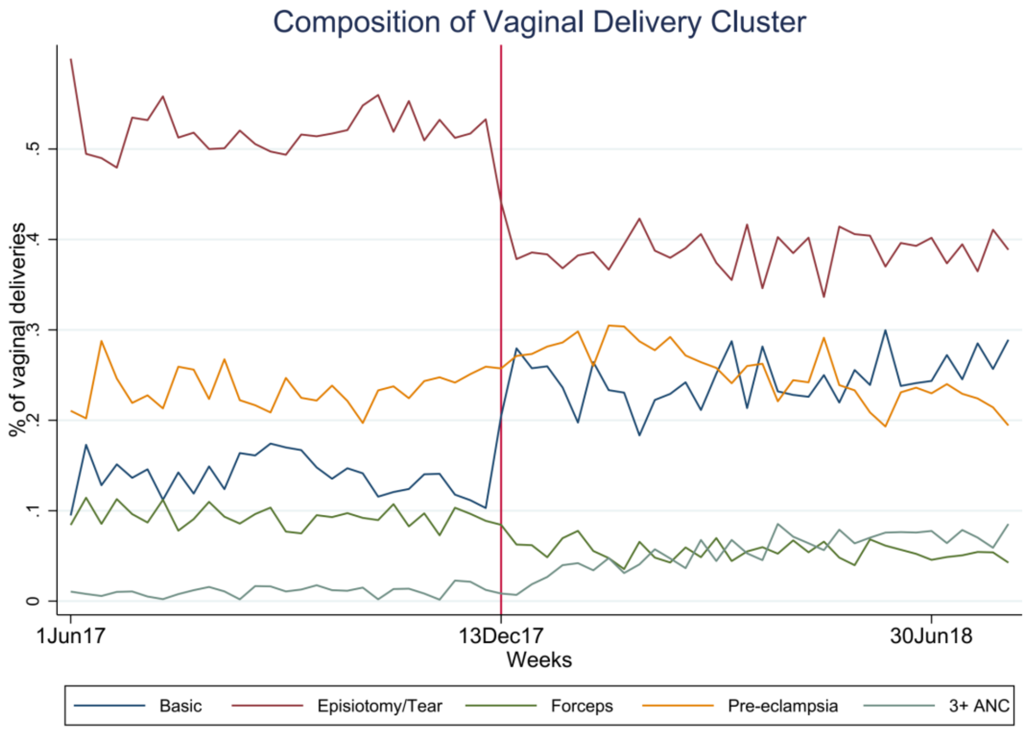


Figure 9: Package Composition of Vaginal and C-section Delivery Clusters



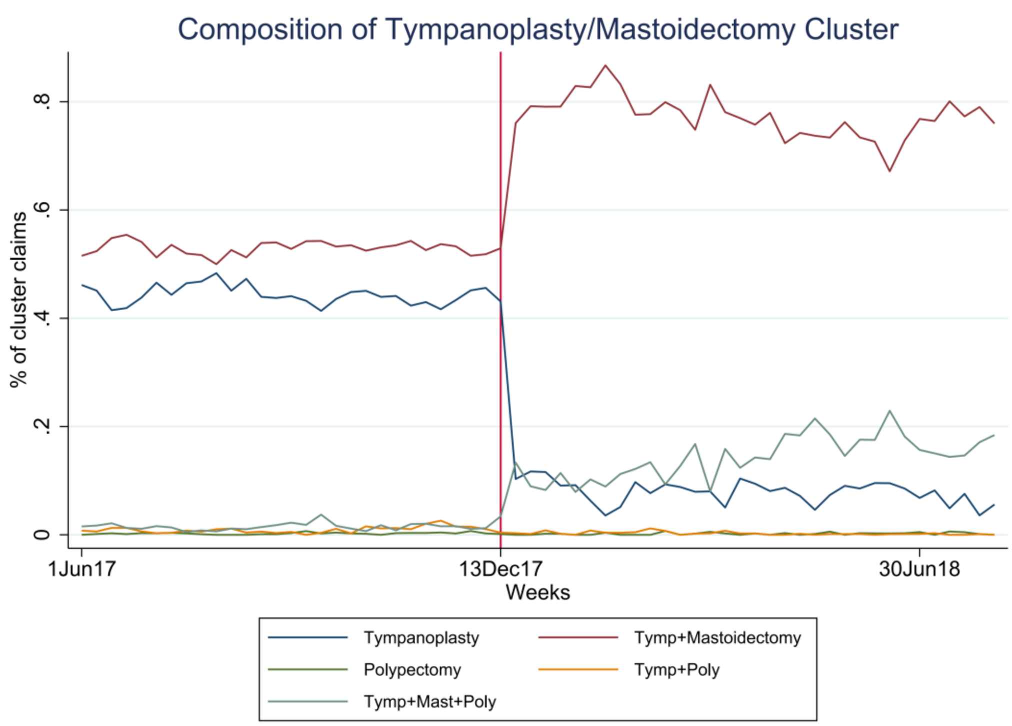


Figure 10: Change in Package Composition of Clusters by Change in Package Rate

Figure 11: Heterogeneity in Composition Changes: Cluster Observed Versus Predicted Rate Change

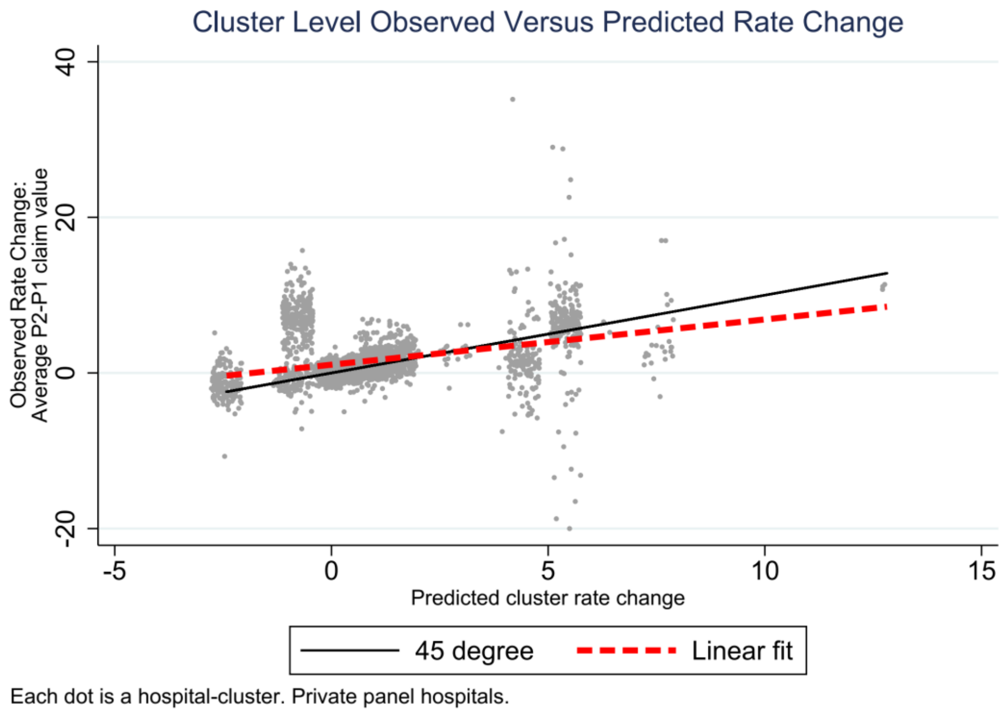
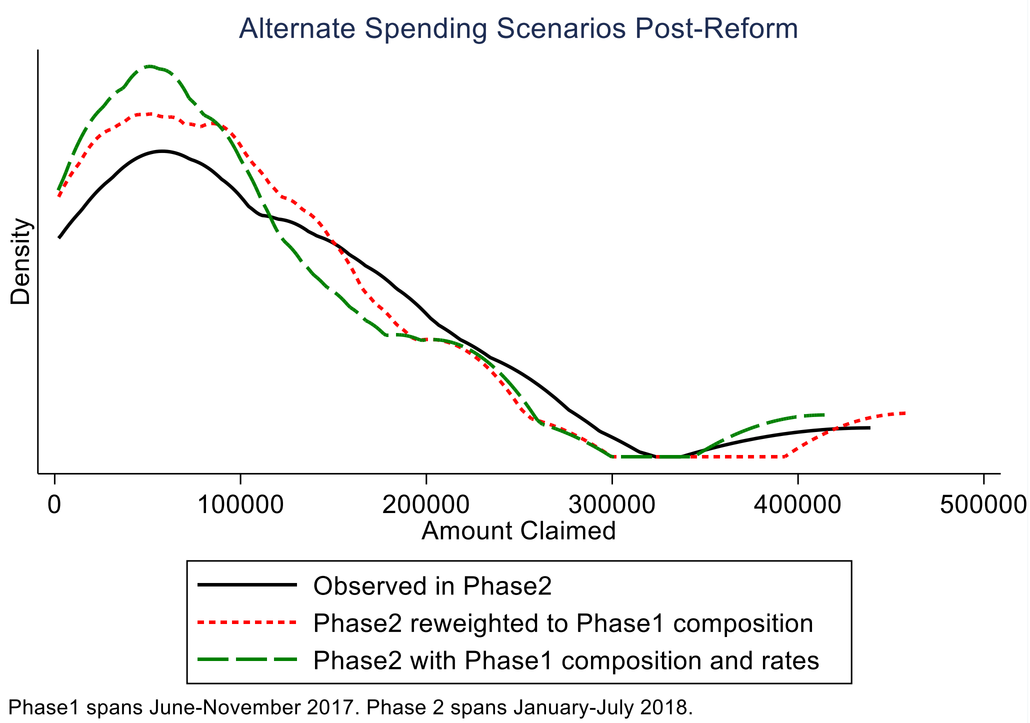


Figure 12: Public Spending Implications of Compositional Changes



TABLES

Table 2: Effect of Rate Change on Package Claims

	(1)	(2)	(3)	(4)
	Panel Hospitals		All Hospitals	
	Log Claims	Log Claims	Log Claims	Log Claims
Rate change (000s) x Post (SR)	0.021***		0.014***	
	(0.002)		(0.002)	
Rate change (000s) x Post (LR)	0.026***		0.014***	
	(0.003)		(0.002)	
Positive rate change x Post		0.102***		-0.014
		(0.028)		(0.026)
Negative rate change x Post		-0.141***		-0.194***
		(0.041)		(0.039)
Post (SR)	-0.026**		0.011	0.068**
	(0.010)		(0.009)	(0.025)
Post (LR)	0.096***		0.201***	0.258***
	(0.012)		(0.011)	(0.026)
Package FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
Observations	95836	95836	123682	95836
Pre-reform mean	7.893	7.893	6.303	6.303
F-test	0.000	0.000	0.000	0.000

Observations are at the hospital-package-month level. Rate change x post is the interaction of the package-specific change in rates (in 1000s of INR) pre- and post-reform and a post reform dummy. Post-reform (SR) is a dummy for Jan-Mar2018 and post-reform (LR) is a dummy for Apr-Jul2018. Dec2017 is excluded. 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 hospital-package level are in parentheses.

Table 3: Effect of Absolute and Relative Rate Change on Cluster Composition in Panel Hospitals

	(1)	(2)	(3)	(4)
	Package Share of Cluster Claims			
Rate change(000s) x Post (SR)	0.019***			
	(0.002)			
Rate change(000s) x Post (LR)	0.022***			
	(0.002)			
% Rate change x Post (SR)		0.003***		0.001***
		(0.000)		(0.000)
% Rate change x Post (LR)		0.003***		0.001***
		(0.000)		(0.000)
% Relative rate change x Post (SR)			0.005***	0.004***
			(0.000)	(0.000)
% Relative rate change x Post (LR)			0.006***	0.004***
			(0.000)	(0.000)
Post (SR)	-0.031***	-0.051***	0.001	-0.023***
	(0.005)	(0.005)	(0.005)	(0.006)
Post (LR)	-0.034***	-0.056***	0.003	-0.023***
	(0.005)	(0.005)	(0.005)	(0.006)
Package FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
Observations	61718	61718	61718	61718

Sample restricted to private panel hospitals. Observations are at the hospital-package-month level. Post-reform (SR) is a dummy for Jan-Mar2018 and post-reform (LR) is a dummy for Apr-Aug2018. Dec2017 is excluded. Rate change x post is the interaction of the package-specific change in rates pre- and post-reform and a post reform dummy.

Table 4: Cluster-level Patient Risk/Severity Composition in Panel Hospitals

	(1) Risk and Complications Index	(2) Referred from elsewhere	(3) Length of Stay
Cluster rate change x Post	0.01	0.01	-0.01
(SR)	(0.13)	(0.01)	(0.03)
Cluster rate change x Post	-0.04	0.00	0.04
(LR)	(0.12)	(0.01)	(0.04)
Cluster FE	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes
Observations	6735	15442	15661
Pre-reform mean	0.01	0.42	2.51
F-test	0.84	0.83	0.03

Observations at the individual level. Standard errors clustered at the hospital-cluster level in parentheses. Indices are computed separately for each package by demeaning, normalizing by the pre-reform standard deviation and weighted by the inverse of the covariance matrix. The risk and complications index is only available for deliveries. Prior risk includes prior high BP, warning of pre-eclampsia in ANC, last pregnancy over 10 years ago, prior stillbirth, and prior c-section. Complications at the hospital include multiparous birth, heavy bleeding, fainting, convulsions, and placenta complications. Because measures of prior risk and complications at the hospital are likely to be correlated, we include all of them in a single index.

Table 5: Changes in Package Composition and Survey Confirmation: Vaginal and C-section Deliveries

	(1)	(2)	(3)	(4)	(5)
	Package Share of Cluster Claims			Survey Confirmation	
	Bottom-coded Packages	Non-bottom coded packages	Bottom -coded Packag es	Non-bottom coded packages	Cluster level (All Clusters)
Positive rate change x Post	0.116***	0.084***	-0.01	0.05	
	(0.021)	(0.018)	(0.01)	(0.08)	
Negative rate change x Post	0.065**	-0.075***	0.02	0.11***	
	(0.026)	(0.012)	(0.02)	(0.02)	
Cluster level rate change x					0.00**
Post					(0.00)
Package FE	Yes	Yes	Yes	Yes	No
Cluster FE	No	No	No	No	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes
Observations	1923	4847	1980	4687	14621
R^2	0.356	0.319	0.301	0.214	0.130
Pre-reform mean: Private			0.99	0.67	0.95
Pre-reform mean: Public			0.98	0.78	0.97

Table 5: Cluster Level Changes in Reimbursements and Volumes

	(1)	(2)	(3)	(4)
	Dependent Variable: Hospital Reimbursements			
	Panel Hospitals		All Hospitals	
Cluster rate change x Post (SR)	1.08***		1.09***	
	(0.07)		(0.07)	
Cluster rate change x Post (LR)	1.16***		1.19***	
	(0.08)		(0.07)	
Positive rate change x Post (SR)		5.27***		4.93***
		(0.34)		(0.31)
Negative rate change x Post (LR)		2.82***		2.71***
		(0.41)		(0.40)
Post (SR)	1.75***	-1.36***	1.71***	-1.13***
	(0.19)	(0.26)	(0.19)	(0.25)
Post (LR)	2.00***	-1.02***	2.00***	-0.75**
	(0.20)	(0.25)	(0.20)	(0.24)
Cluster FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
Observations	26735	26735	30854	30854
Adjusted R^2	0.650	0.648	0.654	0.651
Pre-reform mean	14.81	14.81	14.52	14.52
F-test on rate change x post	0.00	0.00	0.00	0.00

	(1)	(2)	(3)	(4)
	Dependent Variable: Log Claims			
	Panel Hospitals		All Hospitals	
Cluster rate change x Post (SR)	0.02***		0.01**	
	(0.01)		(0.01)	
Cluster rate change x Post (LR)	0.03***		0.01	
	(0.01)		(0.01)	
Positive rate change x Post (SR)		0.46***		0.22***
		(0.06)		(0.06)
Negative rate change x Post (LR)		0.38***		0.17**
		(0.07)		(0.07)
Post (SR)	0.00	-0.36***	0.05***	-0.13**
	(0.02)	(0.06)	(0.01)	(0.06)
Post (LR)	0.21***	-0.16**	0.34***	0.16**
	(0.02)	(0.06)	(0.02)	(0.06)
Cluster FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
Observations	48308	48308	65715	65715
Adjusted R^2	0.331	0.334	0.333	0.334
Pre-reform mean	15.66	15.66	11.86	11.86
F-test on rate change x post	0.00	0.00	0.04	0.00

Observations at hospital cluster month level. In Columns 1-2 the sample is restricted to the panel of hospitals that filed claims both pre- and post-reform; Columns 3-4 include all hospitals. Standard errors at hospital-cluster level in parentheses. The cluster level rate change is calculated using the change in rates across all packages within a cluster weighted by their share of all claims within the cluster in Phase 1, as discussed in the Appendix. Hospital reimbursements are the payment for each transaction received by the hospital from the government. Log claims are the volume of hospital monthly claims per cluster.

Table 6: Cluster-level Changes in Out-of-Pocket Charges

Panel A: Private Panel Hospitals

	(1) Hospital Reimbursement	(2) OOP Payment Amount	(3) Hospital Revenue
Cluster rate change x Post (SR)	1.23***	-0.65***	0.64
	(0.31)	(0.15)	(0.40)
Cluster rate change x Post (LR)	1.40***	-0.48**	0.98**
	(0.30)	(0.20)	(0.41)
Observations	20760	13783	13783
Pre-reform mean	11231.34	1975.28	13589.27
F-test	0.00	0.00	0.02

Panel B: All Private Hospitals

	(1) Hospital Reimbursement	(2) OOP Payment Amount	(3) Hospital Revenue
Cluster rate change x Post (SR)	1.35***	-0.69***	0.60
	(0.29)	(0.17)	(0.37)
Cluster rate change x Post (LR)	1.54***	-0.52**	0.97**
	(0.27)	(0.21)	(0.37)
Observations	22443	15058	15058
Pre-reform mean	11126.35	2041.16	13503.46
F-test	0.00	0.00	0.00

Observations are at the individual transaction level. Standard errors clustered at the hospital-cluster level in parentheses. The cluster level rate change is calculated using the change in rates across all packages within a cluster weighted by their share of all claims within the cluster in Phase 1, as discussed in the Appendix. Hospital reimbursements are the payment for each transaction received by the hospital from the government. Out-of-pocket charges are patient reported payments to the hospital for the same transaction. Hospital revenue is the sum of the two. Tobit regressions used to allow for bottom censoring, as hospitals that were not charging pre-reform cannot reduce their patient charges further.

Table 7: Cluster Level Changes in Out-of-Pocket Payments by Pre-Reform Market Concentration

Panel A: Private Panel Hospitals

	(1) Below Median HHI	(2) Above Median HHI	(3) Below Median Hospital Density	(4) Above Median Hospital Density
Cluster rate change x Post	-0.46**	-0.11	-0.13	-0.47**
(SR)	(0.20)	(0.12)	(0.12)	(0.19)
Cluster rate change x Post	-0.41**	-0.15	-0.13	-0.43**
(LR)	(0.15)	(0.11)	(0.11)	(0.15)
Cluster FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
Observations	7814	6875	7226	7462
Pre-reform mean	1981.32	1859.74	1999.51	1869.74
F-test	0.03	0.41	0.47	0.01

Panel B: All Private Hospitals

	(1) Below Median HHI	(2) Above Median HHI	(3) Below Median Hospital Density	(4) Above Median Hospital Density
Cluster rate change x Post	-0.43**	-0.08	-0.11	-0.42**
(SR)	(0.18)	(0.11)	(0.11)	(0.18)
Cluster rate change x Post	-0.39**	-0.12	-0.11	-0.41**
(LR)	(0.17)	(0.11)	(0.11)	(0.16)
Cluster FE	Yes	Yes	Yes	Yes
Observations	8629	7387	7823	8193
Pre-reform mean	2029.25	1945.32	2094.67	1908.89
F-test	0.06	0.53	0.57	0.05

Observations at the individual transaction level. The Herfindahl Index (HHI) is calculated at the district level (where the hospital is located) using pre-reform hospital claims for each package, to generate a cluster-specific measure of district-level market concentration. The HHI takes a value between 0 and 1, where 1 represents a single monopolistic hospital, or complete concentration. Claims by public hospitals are included in the calculation of competition. We also present the pre-reform number of hospitals filing claims for a cluster in the district as a measure of hospital density, or competition. Controls for hospital urban location and district population, along with recall period included. Standard errors clustered at the hospital-cluster level in parentheses.

Table 8: Rate Change and Care Quality

Panel A: Private Panel Hospitals

	(1) Post-visit complications Index	(2) Technical quality Index	(3) Luxury Index	(4) Perceived quality Index
Cluster rate change x Post (SR)	-0.02**	0.01	0.02	0.01
	(0.01)	(0.01)	(0.02)	(0.02)
Cluster rate change x Post (LR)	-0.01	0.03**	0.02	0.01
	(0.01)	(0.02)	(0.02)	(0.02)
Cluster FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
Observations	21395	15446	15240	14093
Pre-reform mean	0.01	-0.01	0.01	-0.01
F-test on rate change x post	0.12	0.07	0.41	0.73

Panel B: All Private Hospitals

	(1) Post-visit complications Index	(2) Technical quality Index	(3) Luxury Index	(4) Perceived quality Index
Cluster rate change x Post (SR)	-0.02*	-0.00	0.03**	0.01
	(0.01)	(0.02)	(0.02)	(0.02)
Cluster rate change x Post (LR)	-0.01	0.02	0.00	0.02
	(0.01)	(0.02)	(0.02)	(0.02)
Cluster FE	Yes	Yes	Yes	Yes
Observations	23250	16988	16768	15539
Pre-reform mean	0.01	-0.01	0.01	-0.01
F-test on rate change x post	0.24	0.23	0.04	0.57

Observations at the individual level. Standard errors clustered at the hospital-cluster level in parentheses. Indices are computed separately for each package by demeaning, normalizing by the pre-reform standard deviation and weighted by the inverse of the covariance matrix. Post-visit complications include a series of complications such as infection, bleeding, fever, or death for the patient and, in the case of deliveries, also the child after leaving the hospital. Technical quality includes whether seen by a doctor, called back for a checkup, and warned of dangerous symptoms for all packages; for deliveries it also includes whether a labor companion was allowed and skin to skin care encouraged. Luxury includes having own bed, private room, and airconditioning. Perceived quality includes whether staff were very respectful, facility very clean, patient satisfied with care, and would recommend the facility to others.

Table 9: Rate Change and Patient Demographic and Socioeconomic Status

Panel A: Private Panel Hospitals

	(1) Female	(2) Age	(3) Asset Index	(4) Schooling	(5) Low Caste	(6) Awareness Index
Cluster rate change	0.00	-0.69**	0.03*	0.00	-0.01	0.00
x Post (SR)	(0.01)	(0.27)	(0.02)	(0.02)	(0.01)	(0.02)
Cluster rate change	0.00	-0.49*	0.03*	-0.01	-0.00	0.07***
x Post (LR)	(0.01)	(0.27)	(0.02)	(0.02)	(0.01)	(0.02)
Cluster FE	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15920	15881	15077	15876	13880	15172
Pre-reform mean	0.72	32.84	0.01	0.00	0.31	0.03
F-test	0.89	0.04	0.14	0.48	0.73	0.00

Panel B: All Private Hospitals

	(1) Female	(2) Age	(3) Asset Index	(4) Schooling	(5) Low Caste	(6) Awareness Index
Cluster rate change	0.01	-0.66**	0.04**	0.00	-0.01	0.01
x Post (SR)	(0.01)	(0.26)	(0.02)	(0.02)	(0.01)	(0.01)
Cluster rate change	0.00	-0.31	0.04**	-0.02	-0.01	0.08***
x Post (LR)	(0.01)	(0.27)	(0.02)	(0.02)	(0.01)	(0.02)
Cluster FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17500	17456	16591	17455	15231	16696
Pre-reform mean	0.72	32.84	0.01	0.00	0.31	0.03
F-test	0.67	0.03	0.03	0.21	0.63	0.00

Observations at the individual level. Standard errors clustered at the hospital-cluster level in parentheses. Indices are computed separately for each package by demeaning, normalizing by the pre-reform standard deviation and weighted by the inverse of the covariance matrix. Schooling is standardized to the pre-reform package-specific mean. Low caste is a dummy for Scheduled Tribe or Caste. Awareness includes knowing of BSBY and knowing that the hospital was covered prior to the visit, and knowing that all costs are supposed to be covered. Assets include a list of 11 assets.

APPENDIX

A1. Claims Data and Package Matching

Package Level Matching and Rate Change: 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 A1. An INR1 increase in the calculated package rate change results in an INR1 increase in the hospital reimbursement for a package. This gives us confidence that the rate change calculations are correct.

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.

Cluster Level Rate Change: Our analysis of changes in claims volumes filed at the package level suggests hospitals engage in substantial coding manipulation – i.e. they frequently file claims for a higher-reimbursed service than they actually provide – and that this changes substantially with the policy reform. Difference-in-differences specifications at the package level may thus bias estimates of service volumes. To the extent that the marginal cost of care provision is not uniform for patient visits that have been upcoded or not, they will also bias estimates of pass-through into out-of-pocket payments. However, coding manipulation is restricted to packages for closely related services, but not across totally different clusters of services. For example, a basic vaginal delivery may be upcoded as a vaginal delivery with episiotomy, but not as a c-section delivery or as an ear surgery. This is because the threat of detection of fraud is substantially higher when coding manipulation is egregious. The Insurer requires hospitals to submit supporting documentation for all claims filed and reviews a random selection of these. It would also be much harder for hospitals to coordinate coding across departments. However, we found during field visits that all providers within departments have the list of packages for the services they provide and the corresponding reimbursement rates, making coding changes relatively easy. Our survey evidence corroborates the assumption that there is no upcoding across clusters.

For our analysis, we identified 18 service clusters, identified all packages within those services, and ensured we were able to match them between Phase 1 and 2 to ensure each cluster is complete. Claims were stratified by these clusters before being sampled for surveys. In order to run the DID specification at the service cluster level rather than the package level, we create a cluster-level rate change treatment variable. To ensure this is orthogonal to the coding decisions or package composition of any particular hospital, we calculated the cluster rate change as the average rate change across all packages in a cluster, where each package was weighted by its Phase 1 volume. This effectively creates a cluster level predicted rate change based on the overall Phase 1 composition of each cluster. We note, however, that because the predicted rate is not hospital-specific but is based on the rate change across all hospitals and because the package composition of clusters changes in Phase 2, the predicted rate will not translate directly into a one-for-one change in reimbursement rate at the hospital-cluster level. This methodology does, however, ensure the rate is orthogonal to hospital outcomes pre- and post-reform.

Table A2 presents descriptive information on the reimbursement rates and changes for all packages in our sample. Figure 7 presents the full variation in rate change across all packages and clusters.

Table A2: Package and Cluster Rate Changes

Cluster	Number of Packages	Mean Phase1 Rate	Mean Phase2 Rate	Mean Package Rate Change	Mean Package Rate Change (%)	Cluster Predicted Rate Change	Cluster Predicted Rate Change (%)
1 Ward days	2	1,125.00	1,125.00	-	0%	0	0%
2 Csection delivery	4	8,147.54	11,750.00	3,602.46	47%	1598.712	18%
3 Tooth restoration	3	283.33	308.33	25.00	5%	11.91109	5%
4 Blood transfusions	4	1,050.00	1,087.50	37.50	3%	35.50306	3%
5 Vaginal delivery	6	5,920.21	7,583.33	1,663.13	27%	988.6535	18%
6 Tympanoplasty	5	15,025.61	9,400.00	(5,625.61)	-38%	-2419.709	-20%
7 Neonatal care	3	7,141.38	12,000.00	4,858.62	59%	4451.466	65%
8 Bladder Stones	22	8,736.04	12,980.68	4,244.64	54%	-776.6789	-7%
9 Hemodialysis	4	1,777.50	1,687.50	(90.00)	-5%	-250	-13%
10 Cholecystectomy	1	12,000.00	13,000.00	1,000.00	8%	1000	8%
11 Pterigeum removal	1	7,003.68	6,000.00	(1,003.68)	-14%	-1003.678	-14%
12 Chemotherapy	4	3,464.12	4,050.00	585.88	15%	216.2801	8%
13 Renal Stones	4	10,396.93	14,625.00	4,228.07	45%	2854.545	22%
14 Appendicectomy	1	9,500.00	10,000.00	500.00	5%	500	5%
15 Kidney Stones	11	23,563.48	27,818.18	4,254.70	22%	5405.016	21%
16 PTCA	9	47,373.33	54,166.67	6,793.33	14%	4215.164	7%
17 Catheter	6	4,044.00	5,500.00	1,456.00	37%	1167.272	28%
18 Myringotomy	2	5,116.14	5,750.00	633.86	13%	651.7857	14%
	92						

A2. 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, 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.

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 A4.** 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.

[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 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.