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Hospital reimbursement and capacity constraints: Evidence from orthopedic surgeries*

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A B S T R A C T

Health care providers' response to payment incentives may have consequences for both fiscal spending and patient health. This paper studies the effects of a change in the payment scheme for hospitals in Norway. In 2010, payments for patients discharged on the day of admission were substantially decreased, while payments for stays lasting longer than one day were increased. This gave hospitals incentives to shift patients from one-day stays to two-day stays, or to decrease the admission of one-day stays. I study hospital responses using two separate difference-in-differences estimation strategies, exploiting, first, the difference in price changes across diagnoses, and secondly, the difference in bed capacity across hospitals. Focusing on orthopedic patients, I find no evidence that hospitals respond to price changes, and capacity constraints do not appear to explain this finding. Results imply that the current payment policy yields little scope for policymakers to affect the health care spending and treatment choices.

1. Introduction

Escalating health care spending constitutes one of the largest fiscal challenges facing governments in developed countries, and supply side factors have gained increased attention as the main driver (see, e.g. [13,11,6,27,3]). Health care systems around the world, from Medicare in the U.S. to national health services in England and Norway, often employ payment schedules in which prices are set as the average costs across all patients admitted for a certain diagnosis (i.e. diagnosis-related group (DRG) prices). Such contracts may stimulate both efficiency and quality, but distortion effects may arise from the wedge between hospital payment and actual costs.

This paper examines how hospitals respond to price changes. An important challenge when studying price responses is that prices are typically adjusted to reflect changes in treatment costs. Hence, any observed changes in prices likely reflect a reverse causality between care intensity and prices. To address such endogeneity concerns, I rely on two features of the reimbursement scheme for hospitals in Norway. First, as payments are calculated based on costs from 2–3 years back, price changes are unlikely to reflect concurrent changes in treatment costs. Second, I exploit variation from a policy change in 2010 which substantially affected the payments, but did not reflect cost changes. Before the policy change, hospitals received the same amount per patient regardless of her length of stay. After the policy change, hospitals are paid a considerably lower amount for stays wherein the patient is discharged on the day of admission, while receiving substantially more for stays lasting two or more days. The new payment scheme thus creates incentives for hospitals to decrease the admission of one-day stays, or to shift patients from one-day stays to two-day stays, provided that the marginal revenue of the second day exceeds the marginal cost of the additional day. Additionally, the size of the marginal payment for the second day varies significantly across diagnosis groups. This may induce hospitals to prioritize patients in the more profitable diagnoses.

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In this paper, I focus patients admitted for any orthopedic surgery. These admissions are subject to sharp variation in provider incentives, and they account for about one third of all surgeries in Norway. The focus on one specialty at the hospital allows me to study admissions that are likely subject to the same personnel and bed constraints. In addition, orthopedic patients are often relatively healthy and have fewer multiconditions, which make them more likely to be on the length of stay margin affected by the payment scheme changes. Finally, because most procedures are fairly standard, they will be provided by most local hospitals.

I start out by examining whether these patients are, on average, more likely to stay longer than one day following the policy change. This approach has the intuitive appeal that hospitals may respond to the new payment scheme without considering the magnitude of the marginal payments. However, since all hospitals and patient groups are subject to new payments, this specification amounts to a comparison of before versus after the policy change. A further challenge with this approach is that the average response may mask differential responses across patient groups. For example, patients in less profitable diagnosis groups may be discharged, or not admitted, to free up beds or personnel for patients in more profitable diagnosis groups, leaving a zero net effect.

I therefore proceed to investigate whether hospitals respond differently by the size of the marginal payments. The price variation across diagnosis groups allows me to specify a difference-in-differences model where I compare changes in admissions within diagnoses subject to large price changes to changes in admissions within diagnoses subject to small price changes. This model is useful to study whether prices affect the prioritization across patient groups. The interpretation of these price effects is, however, complicated by the potential heterogeneity in marginal costs across groups. In presence of heterogeneous marginal costs, changes in prices may not necessarily reflect changes in profit, i.e., they may not capture the true monetary incentives.

To remediate this concern, I propose an alternative difference-in-differences model which does not require information on marginal costs, nor equivalence of prices and incentives. All patient groups are, albeit to a various extent, more profitable to retain for a second day after the policy change compared to before, but hospitals can only act on this increased profitability if there are available beds. The final model therefore studies whether hospitals with a high share of available beds respond differently to the policy change compared to hospitals with fewer available beds.

The Norwegian health care system provides an attractive context for this study for several reasons. A first advantage of the Norwegian context is the publicly financed health care system, which is comparable to the systems in place throughout Europe, as well as in Canada. In light of marked differences across institutional settings, in terms of hospital ownership and physician remuneration, one might expect to see different effects of similar changes in financial incentives. Reliable causal estimates of price changes are, however, somewhat sparse, and among the notable exceptions, many are set in the U.S. Nonetheless, this literature finds mixed evidence for effects on the number of admissions and medical care intensity, including the length of a hospital stay, but generally finds small or no effects on patient outcomes (see, e.g. [1,10,28,4,20,17,2,7,8]).

A second advantage of the Norwegian context is the availability of high quality data. The paper draws on data from administrative registers which include all visits financed by the Norwegian public health care system. The unusually rich data comprise complete patient level observations of diagnoses, procedures, admission and discharge times. This allows me to construct detailed patient level outcomes, as well as measures of hospitals' capacity constraints. The measure of capacity leverages variation across hospitals in the bed occupancy rate, emerging from the combination of bed capacity and patient congestion.

An important contribution of this paper is to account for the marginal costs arising from capacity constraints. Studies of hospital behavior under capacity constraints are rarely seen within the literature despite the possibility that capacity may be binding and hence affect the price response. One notable exception is [28] who study price changes in Italy, and find smaller price effects for hospitals with lower excess capacity. However, they study effects of changes in reimbursements across diagnoses, whereas I study effects of incentives to change the length of stay within diagnoses – a setting where capacity constraints may be a particularly important channel to examine.

The main findings of this paper may be summarized along the following lines. I do not find any evidence that the 2010-policy change increased the overall probability of patients staying longer than one day. Capacity constraints may be flagged as one potential explanation for the absence of any price response, as a substantial share of the hospital beds in Norway are filled at any given day [23]. However, the two difference-in-differences models lend no support to this explanation.

First, when comparing diagnosis groups subject to large price changes to groups subject to low price changes, I find no evidence that hospitals are more likely to shift patients from one-day stays to two-day stays within diagnosis groups for which the marginal revenue of the second day is the highest. I also find no discernible differences in the admission rates.

Second, when comparing hospitals with high pre reform bed occupancy rates to hospitals with lower bed occupancy rates, the results lend again no support to the hypothesis that capacity constraints impede hospitals' shifting of patients into longer stays.

Finally, given the absence of response on the length of stay, it is unsurprising that I also find no evidence that price changes affect patient health. I conclude that, within the context studied, hospitals are notably insensitive to prices.

One explanation for the absence of effects is that the hospital level incentives do not necessarily trickle down to the actual decision makers, i.e. the doctors, whose salary is fixed. This institutional feature is, however, not specific to the Norwegian health care system. For example, NHS hospital doctors in the UK are also salaried and do not share hospitals' profits or losses [12]. Nonetheless, similar studies from the UK [12,2], as well as one paper form Norway [20], find that the share of same-day stays increases when prices are changed in favor of such admissions relative to overnight stays. The apparent willingness to shift towards same-day stays but not towards overnight stays may have several potential explanations. For example, overnight stays may require more resources besides bed capacity, e.g. staff availability, constraining the possibility to respond to increased prices of overnight stays. Moreover, same-day stays may be considered more medically defensible than overnight stays, which could justify responses to incentives pushing towards same-day stays.

It is further important to keep in mind that my results are based on a sample of orthopedic surgeries, and may not necessarily be generalizable to other specialties. However, the findings confirm those of [17], who also find no price response when studying a broader set of surgical procedures at Norwegian hospitals. In their paper, however, price responses were estimated off of relatively small price changes, which could potentially imply that the absence of effects is explained by the incentives being too weak. Nonetheless, as is shown in the present paper, there does not appear to be any effect even when incentives are substantially stronger.

2. Institution

The reimbursement scheme from the Norwegian government to regional health authorities entails a fixed part and an activity-based part (40%). There are no clear guidelines for distribution of funding to lower levels [22], but in practice activity-based financing trickles down to the hospital level, and to the departmental level within hospitals [14,24]. Physicians at hospitals are paid by a fixed salary, which is not directly connected to the hospital reimbursement rates. A salaried physician may nevertheless have a motivation to internalize hospital incentives if it pays back indirectly, for instance by increasing the physician's bargaining power, by improving future job prospects, or by allowing for more comfortable working conditions. Several physicians have expressed concern over the perverse incentives implied by the hospital payment scheme, as illustrated in an op-ed by [19] published in the Journal of the Norwegian Medical Association. This suggests that hospital incentives are at least partially embedded in the decision-making process of the clinicians.

Activity-based financing using diagnosis-related groups (DRGs) is a central feature of the reimbursement scheme. Patients discharged at a somatic hospital are assigned a diagnosis group, comprising patients who are homogeneous in medical criteria and costs of treatment. Each diagnosis group is assigned a cost weight which reflects the average costs of treating a patient within that group, relative to all other patients. Hospitals are reimbursed 40% of the average cost, regardless of the actual costs incurred in treating the patient. Cost estimations are based on average costs of a sample of hospitals. National average treatment costs are revised regularly, and there is a time lag of two to three years for changes in costs to be reflected in price changes.

DRG-specific reimbursement rates were initially independent of the length of a patient stay; hospitals would gain the same reimbursement per patient regardless of how long the patient stayed at the hospital. Effective from January 1 2010, the reimbursement rates for each surgical diagnosis group were split, yielding one rate for stays lasting one day only, and one rate for stays lasting longer than one day. The intention was to make the diagnosis groups more homogeneous in costs, and to remove financial incentives to discharge patients too early [26]. In the calculation of new rates, the total set of diagnosis-specific weights was recalibrated so that the total sum of diagnosis group weights produced in 2010 would correspond to that of 2009, given the activity level (and behavior) in 2009 [15,16]. Hence, the reform was budget neutral at the national level.

Before the policy change, hospitals gained on average about \$3300 per orthopedic patient. After the policy change, the hospital receives on average \$1600 for patients staying one day, and \$3700 for patients staying at least two days or longer. Fig. 1 plots the difference in reimbursement for two-days stays and one-days stays by diagnosis group. The figure also plots the change in mean reimbursement per patient. As can be seen, there is substantial variation between diagnosis groups: some diagnosis groups are subject to large marginal payments at the second day, while others are subject to minor changes only. However, the change in *average* payments per patient is minimal.

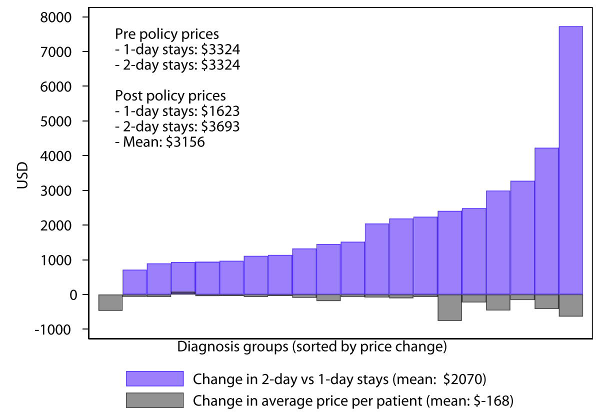

Fig. 1. Policy-induced changes in reimbursement rates.

3. Empirical strategy

I start out by exploring the policy induced change in the overall probability of staying longer than one day. This has the intuitive appeal that hospitals may respond to the new payment scheme without considering the size of the marginal payment.

I next test whether the decision makers consider the size of the marginal payment in a difference-in-differences model which compare patients in diagnosis groups subject to large price changes with groups subject to smaller price changes.

As a more direct way to test whether capacity constraints affect the price response, I finally estimate an alternative difference-in-differences model and compare patients at hospitals with low capacity to patients at hospitals with high capacity.

3.1. Before vs after: average response to policy change

Studying average responses to the policy reform is challenging since everyone is affected by the price change, rendering no single control group. Instead, I compare the pre and post policy outcomes in a simple model:

$$y_{ijkt} = \alpha post_t + \beta year_t + \gamma_{jk} + \varepsilon_{ijkt}, \tag{1}$$

where y_{ijkt} is either a binary variable equal to one if patient i in diagnosis group j at hospital k stays longer than one day at the hospital. The regression is estimated at the individual level to allow for more weight on the largest patient groups, and hence accounting for the capacity constraints at the hospitals. However, for completeness, I show results where cells are *not* weighted by patient volumes in Appendix Table A2.4 – this yields similar results.

I also estimate the effect on the number of admissions, in which case the data are collapsed to cells of hospital-diagnosis-year.

The indicator $post_t$ is one in all years $t \geq 2010$, and $year_t$ is either a linear or quadratic time trend. Hospital-by-diagnosis fixed effects (γ_{jk}) control for time invariant characteristics in diagnosis groups within hospitals, and flexibly allow for heterogeneity in the case-mix offered at different hospitals.

If the probability of staying longer than one day increases following the policy change, I expect $\alpha > 0$. If hospitals respond by decreasing the number of one-day stays, this would yield a negative impact on the total number of admitted patients.

The error term ε_{ijkt} is assumed uncorrelated with the explanatory variables. Standard errors are clustered at the hospital level.

3.2. Dfference-in-differences (DiD) 1: response by magnitude of marginal payments

I next study whether the payment change differentially affected the probability of staying longer than one day in diagnosis groups that were subject to large price changes compared to diagnosis groups subject to smaller price changes. Since this outcome may also be affected by the patient volume, I additionally estimate the effect on the number of admitted patients.

To this end, I formulate the following model:

$$y_{ijkt} = \alpha HighPrice_j \times post_t + \delta_t + \gamma_{jk} + \varepsilon_{ijkt}, \tag{2}$$

where $HighPrice_j = 1$ when the change in the marginal revenue at the second day is above the median, zero otherwise. As before, $post_t = 1$ for $t \geq 2010$, and γ_{jk} are hospital-diagnosis fixed effects.

While time entered linearly (or quadratically) in Eq. (1), the specification in Eq. (2) allows me to include year fixed effects (δ_t) to purge out any average impact of the payment change. Hence, the model offers insights into whether providers respond differently according to the amount to be gained. If providers respond to the incentive but do so independently of size of the marginal payments, this will not be picked up in this model. However, such overall responses will be captured in the aforementioned model of Eq. (1).

To examine the robustness of the results, estimates are reported with and without hospital-diagnosis-specific time trends. These account for variation in the size of price changes that might coincide with pre-existing trends in the outcome. I also compare only the top and bottom quartile of price changes, as well as including price changes linearly.

If patients admitted in high price groups are more likely to spend longer than one day at the hospital, we would expect $\alpha > 0$.

Since the specification in Eq. (2) leverages price variation across diagnosis groups, standard errors are now clustered at the diagnosis group level. To avoid overstating the significance of the findings due to few clusters (20 in total), I calculate p-values and 95% confidence intervals using the wild bootstrap [5,25].

The main assumption in any difference-in-differences model is that, absent the policy change, trends in outcomes would evolve similarly across groups (parallel trends). That is, the trend in the probability to stay an additional night should be similar for admissions to groups subject to large price changes compared to groups subject small price changes. This can be investigated directly in an event-study model where the $post_t$ indicator is replaced by separate year dummies and the effect of high price changes is estimated relative to low price changes for each year relative to pre-reform year 2009. For the parallel trend assumption to hold, the estimates for years prior to the reform should not be significantly different from zero.

3.3. Difference-in-differences (DiD) 2: capacity constraints

The final model is motivated by the notion that hospitals often operate at high capacity. Hospitals can only induce longer stays if they have spare capacity to accommodate the patient. To test the hypothesis that capacity constraints may be binding, I group hospitals by their pre policy change bed occupancy rate and formulate an alternative difference-in-differences model:

$$y_{ijkt} = \alpha ExcessCapacity_k \times post_t + \delta_t + \gamma_{jk} + \varepsilon_{ijkt}, \tag{3}$$

where $ExcessCapacity_k$ is one if the orthopedic unit at hospital k operated below the median bed occupancy rate prior to the payment change, zero otherwise.

If hospitals with spare capacity are more likely to respond to the policy change, we expect $\alpha > 0$. The identifying assumption is that outcomes evolve similarly at hospitals subject to high bed capacity compared to hospitals subject to low bed capacity. I test the validity of this assumption in an event-study specification.

4. Data, sample and descriptives

The estimation sample includes all surgical orthopedic admissions in the Norwegian Patient Registry over the period 2008 to 2012. Orthopedics is by far the largest branch of surgery, comprising about one third of all surgical admissions, both in terms of the number of patients and revenue. The average share of patients staying longer than one day has decreased over the sample period; starting at 0.59 in 2009 and reaching 0.55 in 2012. Though we would expect

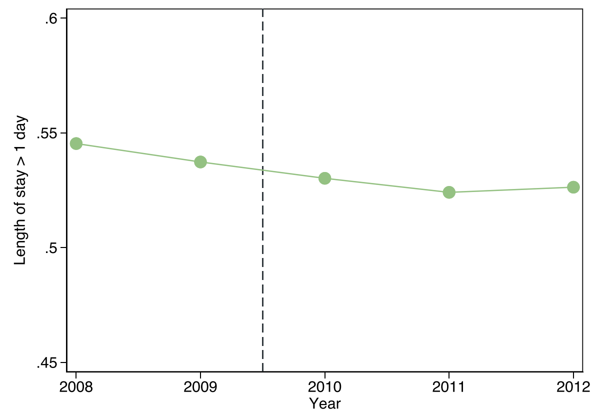

Fig. 2. Overall probability of staying longer than one day. Figure plots the probability of staying longer than one day, purged of hospital-diagnosis fixed effects. Sample means are added back in to facilitate interpretation of the axes.

an increase in the length of stay if hospitals responded to the policy change, it is also worth noting that technological progress works in the opposite direction, towards shorter hospital stays. The patient background indicators age, female and number of secondary diagnoses are fairly stable over the period; so are the patient health indicators: 30-days and 90-days emergency readmission rate, 1-year re-surgery rates. The patient safety indicators, operative and post-operative complications; hospital-acquired infections; and sentinel events, are extremely rare. The number of hospitals is lower in 2008 compared to the following years due to an increase in private hospitals. However, private institutions are small, such that this increase amounts to less than 1% of the total patient volume. There are in total 20 diagnosis groups, 11 in high price change groups and 9 in low price change groups.

The pre-reform bed occupancy rate is calculated as the average number of hospitalized patients per beds per day, where the bed stock is approximated by the yearly maximum number of patients hospitalized from one day to the next at the hospital's orthopedic surgery unit. The variation across hospitals is presented in Online Figure A1.1, showing a mean of about 0.53, while the median is 0.55 and the standard deviation 0.26.

See Online Appendix for more information about the data, sample and descriptives.

5. Results

I begin the presentation of results by discussing the overall effect on the probability of patients staying longer than one day. In Fig. 2, I plot the probability of staying longer than one day purged of hospital by diagnosis fixed effects. More precisely, the figure plots the residuals after a regression of a binary indicator for staying longer than one day on hospital-diagnosis fixed effects. Although there might be indications that the trend is flattening out, there is no sign of any abrupt increase in 2010, despite the large financial incentives.

In Table 1, model 1, I show that the estimate of the post indicator from Eq. (1) is almost zero, in fact marginally negative with a point estimate of −0.007. This corresponds to a 1.3% reduction relative to the mean. The estimate is quite precise, and effect sizes larger than 0.08 percentage points can be rejected at a 95% significance level. The estimate increases somewhat when allowing for a linear time trend. Now, estimates higher than 0.7 percentage points can be rejected at a 95% significance level.

The effect on the log number of patients is likewise minimal and non-significant, with point estimates ranging from 4.2% to 4.7%.

Table 1
Results.

Outcome	(1)	(2)	(3)	(4)
	Length of stay > 1 day		*Log number of patients*	
Model 1 (Before vs after): Average response to policy change				
Post	−0.007	−0.002	0.042	0.047
95% CI	[−0.015,0.001]	[−0.011,0.007]	[−0.027,0.111]	[−0.035,0.130]
Lin. trend	–	–	–	–
Quad. trend		–		–
Model 2 (DiD 1): Response by magnitude of marginal payments				
HighPrice × post	0.012	0.003	−0.031	−0.021
95% CI	[−0.007,0.032]	[−0.014,0.021]	[−0.141,0.096]	[−0.097,0.059]
Group trend		–		–
Model 3 (DiD 2): Response by hospitals' bed capacity				
ExcessCap × post	−0.008	−0.029*	−0.078	−0.063
95% CI	[−0.024,0.007]	[−0.052, −0.005]	[−0.193,0.036]	[−0.196,0.070]
Group trend		–		–
Observations	524,802		3,459	
Dep. mean	0.56			

Notes: Group time trends are linear yearly trends at hospital by diagnosis level. All models include fixed effects hospital-diagnosis; model 2 and 3 additionally include year dummies. Cluster robust standard errors: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

Taken together, there is strong evidence of no average response to the new payment schedule.

The absence of any increase in the probability of staying longer than one day at the time of the reform may hide heterogeneous responses across groups. For instance, hospitals may retain fewer patients in low price groups in order to free up beds for patients admitted to groups subject to high price changes, rendering a zero average effect.

I continue by presenting results from the event-study equivalent of Eq. (2). Fig. 3(a) serves two purposes: one is to test the identifying assumption of there being no trends in the outcome variable prior to the reform; the second is to give a visual presentation of any potential effect. The effect of high relative to low price changes is estimated for each year relative to 2009. The figure displays no systematic pattern before the policy change. A statistical joint test of the pre-reform point estimates cannot reject that they are significantly different from zero (joint p-value = 0.25), lending support to the parallel trend assumption of the difference-in-differences model described in Eq. (2).

A second takeaway from Fig. 3(a) is that the effect of a high price change on staying longer than one day is not significantly different from a low price change. This result is further quantified in Table 1 where I present results from the difference-in-differences specification in Eq. (2).

Column (1) in model 2 of Table 1 shows that patients subject to higher price changes are slightly more likely to stay longer than one day, but the estimate is small (1.2 percentage points) and non-significant. The effect size decreases when controlling for hospital-diagnosis-specific time trends in column (2). This is easiest explained by Figure A2.1 which plots the residualized outcome over years separately for low and high price change groups. The figure reveals a slight upward pre-trend in the high price group, which will bias the effect estimate from column (1) upwards. When taking account of this small pre-trend, the point estimate falls to 0.4 percentage points, still non-significant. Note that the potential within diagnosis group correlation is adjusted for by wild cluster bootstrapping, which may produce asymmetric confidence intervals.

In Table A2.2 I show that results are robust to alternative treatment indicators. This includes comparing only admissions to the top and bottom price group quartiles, and including the price change variable linearly. None of the models find any significant effects of price changes on the probability of staying longer than one day.

Table 1 presents evidence that price changes do not appear to have affected the number of patients either. The estimated effect of high price changes on the log number of patients is small (3.1%) and non-significant. In the online appendix I also show results for elective procedures only, and when excluding private hospitals. None of these models suggest that hospitals respond to price changes.

Binding capacity constraints may prevent hospitals from retaining any type of patients, regardless of the size of the price incentives. Fig. 3(b) shows the event study equivalent of Eq. (3), where I group hospitals by their pre reform capacity rate. There does not appear to be any differential trends in the probability to stay longer than one day in the years before the policy change, lending support to the difference-in-differences model. A joint test for significance of the pre-trend years cannot reject the null hypothesis of no pre-trend. Moreover, there does not seem to be any indications that hospitals with relatively high spare capacity are more likely to shift patients into longer stays following the policy change. This finding is consistent with the difference-in-differences estimates presented in Model 3 of Table 1, which are modestly negative and non-significant. There is also no indications of any effects on the number of admitted patients, but precision is low.

In an alternative model, I have also examined time-variant measures of capacity, where the capacity measure is allowed to vary within hospitals at a daily basis. This model brings the same conclusion as the one presented: capacity constraints do not seem to explain the absent price response.

Since the policy change did not seem to shift patients into longer stays, health outcomes cannot be impacted through an increase in the length of stay. Health outcomes may, however, be directly affected through other channels, e.g. if hospitals compromise on quality to avoid incurring financial losses from the policy change. To test this, I use the difference-in-differences specification in Eq. (2) to estimate the effect of price changes on patient health indicators and endoscopic surgery (proxying technological changes). The overall finding is that price changes do not carry over to patient health or technology changes, but precision is fairly low across all models. Results are presented in Table A2.1. Logit models yield the same conclusion (not shown).

6. Discussion

I do not find any evidence that hospitals shift patients from one-day stays to two-days stays, despite the financial incentives to do so. Capacity constraints seem unlikely to be the reason for the absence

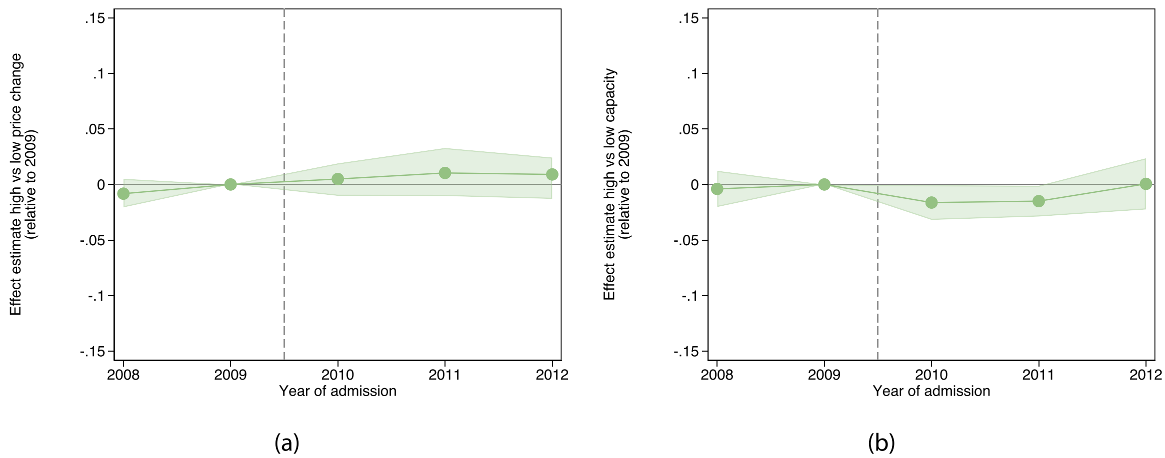

Fig. 3. Event study estimates of staying longer than one day.

of any response. This conclusion is supported through two different difference-in-differences model. First, I show that hospitals do not discharge patients in low profitability diagnosis groups to increase hospitalization in high profitability price groups. Second, I group hospitals by their pre-reform capacity constraints, and find that the response to the new payment scheme does not differ between hospitals with different capacity levels.

One potential explanation for why hospitals do not appear to respond to price changes could be high marginal costs which are not resembled by the bed capacity, e.g. personnel and equipment. Another explanation could be that the actual decision maker – the physician – is unconnected to the incentives of the hospital. Physicians at hospitals are paid by a fixed salary, whereas the price changes studied are at the hospital level. A salaried physician may nevertheless have a motivation to internalize hospital incentives if it pays back indirectly, for instance by increasing the physician's bargaining power, by improving future job prospects, or by allowing for more comfortable working conditions. Several physicians have expressed concern over the perverse incentives implied by the hospital payment scheme (see, e.g. [19]). Moreover, price responses have previously been documented at Norwegian hospitals (see, e.g. [17]). It thus seems likely that hospital incentives, at least to some extent, trickle down to the decision making level. Nevertheless, if physician incentives are only partly related to the hospital payment scheme, it may be difficult to estimate effects of their true incentives.

Physician ethics or clinical motivations are yet alternative explanations for the absent price response. If longer stays worsen patient outcomes, e.g. through the risk of contracting hospital infections, the findings are consistent with hospital objectives that value patient welfare sufficiently more than revenues.

Finally, even if there does not seem to be any response for orthopedic surgeries, effects may not be generalizable to other medical specialties.

7. Conclusion

A thorough understanding of how providers respond to incentives is crucial for policy makers to affect costs and ultimately patient welfare. In this paper, I study the effects of a policy reform which decreased the relative profitability of one-day stays, while increasing the profitability of two-day stays. Results reveal that, within the Norwegian health care system, hospitals are notably insensitive to price incentives for orthopedic surgeries.

The findings have important policy implications. Prospective, activity-based prices are used in many countries to give hospitals incentives to contain costs. Critics argue that hospitals may be inclined to attract profitable patients, and to lower the quality for a given patient. This paper's findings suggest less concern for perverse incentives within systems similar to that of Norway. The results further imply that the current payment policy yields little scope for policymakers to affect the provision of health care. However, evidence is mixed about the generalizability of this finding. More research is needed to understand how and why hospital behavior may differ between medical specialties, across different hospitals, and between countries. For example, [12] find price responses for 8 out of 13 planned conditions. The reasons for differences across medical conditions remain an open question. Finally, more knowledge is also needed to understand how hospital behavior vary by the share of activity-based financing. For example, responses to changes in incentives will likely depend on the percentage share of hospital revenues that are related to DRG. In the period studied, this share is 40% in Norway. In comparison, the shares range from 20% in Spain, around 39% in Estonia, 60% in Poland and England, 80% in Portugal, Germany, France, and Ireland to 96% in Austria [21].

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.healthpol.2021.02.004.

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Further reading

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