This is a repository copy of *Hospital competition in a national health service:evidence from a patient choice reform*.

White Rose Research Online URL for this paper:
https://eprints.whiterose.ac.uk/id/eprint/176913/

Version: Published Version

Article:

Brekke, Kurt R., Canta, Chiara, Siciliani, Luigi orcid.org/0000-0003-1739-7289 et al. (1 more author) (2021) Hospital competition in a national health service:evidence from a patient choice reform. Journal of health economics. 102509. ISSN: 0167-6296

https://doi.org/10.1016/j.jhealeco.2021.102509

Reuse
This article is distributed under the terms of the Creative Commons Attribution (CC BY) licence. This licence allows you to distribute, remix, tweak, and build upon the work, even commercially, as long as you credit the authors for the original work. More information and the full terms of the licence here: https://creativecommons.org/licenses/

Takedown
If you consider content in White Rose Research Online to be in breach of UK law, please notify us by emailing eprints@whiterose.ac.uk including the URL of the record and the reason for the withdrawal request.

eprints@whiterose.ac.uk
https://eprints.whiterose.ac.uk/

Journal of Health Economics 79 (2021) 102509

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/jhealeco

Hospital competition in a national health service: Evidence from a patient choice reform[☆]

Kurt R. Brekke[a,b,*], Chiara Canta[c], Luigi Siciliani[d,e], Odd Rune Straume[f,g]

[a] Norwegian School of Economics (NHH), Department of Economics, Helleveien 30, 5045 Bergen, Norway
[b] Centre for Applied Research at NHH(SNF)
[c] TBS Business School, 1M Place Alphonse Jourdain, 31068 Toulouse, France
[d] University of York, Department of Economics and Related Studies, York YO10 5DD, UK
[e] C.E.P.R., 90–98 Goswell Street, London EC1V 7DB, UK
[f] University of Minho, Department of Economics/NIPE, Campus de Gualtar, 4710-057 Braga, Portugal
[g] University of Bergen, Department of Economics

A R T I C L E I N F O

JEL classification:
I11
I18
L13

Keywords:
Patient choice
Hospital competition
Quality
Cost-efficiency

A B S T R A C T

We study the impact of exposing hospitals in a National Health Service (NHS) to non-price competition by exploiting a patient choice reform in Norway in 2001. The reform facilitates a difference-in-difference approach due to plausibly exogenous (geographical) variation in pre-reform market structure. Employing rich, administrative data, covering the universe of hospital admissions from 1998 to 2005, we estimate models with hospital and treatment (DRG) fixed-effects and use only emergency admissions to limit patient selection issues. The results show that hospitals in more competitive areas have a sharper reduction in AMI mortality but no effect on stroke mortality. We also find that exposure to competition reduces all-cause mortality, shortens length of stay, but increases readmissions, though the effects are small in magnitude. In years with high (DRG) prices, the negative effect on readmissions almost vanishes. Finally, exposure to competition tends to reduce waiting times and increase admissions, but the effects must be interpreted with care as the outcomes include elective treatments.

1. Introduction

The health care sector has traditionally been heavily regulated in most countries with the exception of the US and a few other countries. However, in the last two decades many countries have introduced pro-competitive reforms, especially in the delivery of health care. Examples are prospective payment schemes, patient choice of provider, performance pay, entry of private providers, and corporatisation of public providers. These reforms introduce scope, and possibly also incentives, for competition among health care providers, usually along non-price dimensions such as quality of care. Despite these pro-competitive trends, the policy debate

☆ We thank the Editor and two anonymous referees for very helpful comments and suggestions. This paper is written as a part of the SNF project *Competition in Hospital Markets* (no. 9040) funded by *Prisreguleringsfondet* administrated by the Norwegian Competition Authority. Odd Rune Straume also acknowledges funding from National Funds of the FCT –Portuguese Foundation for Science and Technology within the projectUID/ECO/03182/2019.

* Corresponding author.
E-mail addresses: kurt.brekke@nhh.no (K.R. Brekke), c.canta@tbs-education.fr (C. Canta), luigi.siciliani@york.ac.uk (L. Siciliani), o.r.straume@eeg.uminho.pt (O.R. Straume).

https://doi.org/10.1016/j.jhealeco.2021.102509
Received 25 March 2020; Received in revised form 15 July 2021; Accepted 16 July 2021
Available online 23 July 2021
0167-6296/© 2021 The Author(s). Published by Elsevier B.V. This is an open access article under the CC BY license (http://creativecommons.org/licenses/by/4.0/)

on market-based delivery and competition in health care is still ongoing.[1] There is also a lack of rigorous empirical evidence on the causal effects of exposing health care providers to competition, especially from outside the US and the UK, and the existing evidence on the impact of competition is generally mixed.[2]

This paper contributes to the literature on competition in health care by studying the competitive effects of introducing nationwide patient choice of hospitals in the Norwegian National Health Service (NHS) in 2001. The patient choice reform replaced an administrative scheme with referral of patients to the closest hospital in the catchment area, and thus implied a switch from a situation where hospitals were local monopolists to a situation with potential for (non-price) competition. A hospital offering inferior quality of care would, after the reform, risk losing patients to hospitals offering better quality of care, as patients, or their General Practitioner (GP), could demand care from a more distant hospital outside the catchment area. Thus, the patient choice reform is likely to make demand for hospital care more elastic with respect to quality and thus expose the NHS hospitals to competition.

While patient choice introduces scope for competition, the incentives for the NHS hospitals to compete will depend on several factors, including the hospitals' objectives and their financial gains (losses) from attracting (losing) patients.[3] Thus, to guide our empirical study, we construct a theoretical model with semi-altruistic hospitals that set quality and patients (or GPs) that choose hospital depending on relative distance and quality of care (when choice is allowed). The hospitals are paid a fixed price per patient treated and can also expend cost-reducing effort to improve the profit (price-cost) margin. In this set up, we show that if hospitals' profit margin is positive, then competition has a positive impact on both quality and cost efficiency. The altruistic motive for hospitals to offer high quality treatments is *reinforced* by the profit motive to attract more patients and thus higher revenues. However, if hospitals' profit margin is negative, the effect of competition is generally ambiguous, as hospitals face financial losses on each extra patient they treat. In this case, the profit motive *counteracts* the altruistic motive of offering high quality of care, and the net effect depends on the weight that hospitals put on profits relative to patient benefits. Thus, the impact of hospital competition is essentially an empirical question.

Our empirical analysis exploits a reform introducing nationwide patient choice in the Norwegian NHS in 2001. To identify causal effects of exposing NHS hospitals to patient choice and thus (non-price) competition, we adopt a *difference-in-difference (DiD) approach* similar to Cooper et al. (2011) and Gaynor et al. (2013).[4] The idea behind this approach is simply that, while the reform applied to all NHS hospitals at the same date, the competitive effects of the reform vary substantially across hospitals depending on their geographical location.[5] In areas with a high density of hospitals, the intensity of competition induced by the patient choice reform is likely to be higher than in areas with a low density of hospitals, where the exercise of choice is more restricted. This plausibly exogenous variation in competition (treatment) intensity of the reform, is captured by a hospital-specific Hirschman-Herfindahl Index (HHI) based on the pre-reform allocation of patients across hospitals.[6] This competition intensity measure is then interacted with a post-reform dummy, implying that hospitals in highly concentrated (monopoly-like) areas depict the counterfactual in the post-reform period. Thus, the competitive effects of patient choice are captured by changes in the performance of hospitals in more competitive areas relative to hospitals in less competitive areas before and after the reform.

The empirical analysis is based on rich administrative register data that cover virtually the whole population of patients and hospitals in Norway over an eight-year period from 1998 to 2005.[7] The data contain detailed information about patients and hospitals, which is aggregated to the quarterly hospital-DRG level. Our key dependent variables are (clinical) quality (mortality and readmission rates) and cost efficiency (length of stay). Our main outcome variables are based only on emergency (acute) treatments to avoid that estimates are biased due to patient selection, as common in the literature on hospital competition. Therefore, quality for emergency treatments is used as a marker of overall quality in the hospital (Cooper et al., 2011; Gaynor et al., 2013; Kessler and McClellan, 2000). While hospitals compete for elective treatments where patients excercise choice, competition drives overall quality, which potentially translates into lower mortality and readmission rates.[8] We also control for patient case mix and do various tests to ensure that results are not driven by patient selection but rather by the NHS hospitals' response to competition. To limit endogeneity and selection issues further, we estimate the model using hospital and DRG fixed effects that control for all unobserved (and observed) hospital and DRG heterogeneity that is invariant over time. The model also include time trends common to all hospitals, and we conduct a set of robustness checks to verify that our results are not sensitive to a particular specification.

[1] For instance, the UK government published a white paper titled "Integration and innovation: working together to improve health and social care for all" (11 Feb 2021) proposing less competition and more control in the NHS.

[2] See Gaynor and Town (2011) for an extensive overview of the literature on competition in health care markets. The review by Gaynor et al. (2015) describes how the competitive effects depend on institutional settings.

[3] See for instance the reviews by Gaynor and Town (2011) and Gaynor et al. (2015) or the studies by Brekke et al. (2011); Brekke and Straume (2017).

[4] Cooper et al. (2011) and Gaynor et al. (2013) use a patient choice reform in the UK NHS to study the effects of hospital competition. We discuss the relation to their work in more detail later.

[5] This approach was pioneered by Propper et al. (2008) who study a different type of policy reform in the English NHS.

[6] Since the HHIs are based on market shares which are endogenous in a competitive environment, we use the pre-reform (rather than the instantaneous) allocation of patients to hospitals, as in Gaynor et al. (2013), and use predicted (rather than actual) HHIs, as suggested by Kessler and McClellan (2000).

[7] The data cover, strictly speaking, patients and hospitals in the Norwegian NHS, but private hospital care outside the NHS is negligible during our sample period.

[8] Moreover, quality indicators are usually published for emergency treatments, where mortality rates are not negligible, which potentially gives also a direct incentive to reduce quality for emergency care, such as AMI, stroke and all-cause mortality.

Based on the DiD approach, we obtain the following three key results. First, for *hospital mortality*, we find that the introduction of patient choice and thus competition in the Norwegian NHS is associated with a sharper *decline* in *acute myocardial infarction (AMI) mortality rates* for hospitals in more competitive areas compared to hospitals in less competitive areas. The effect is sizeable: a 10 percent reduction in average HHI (which amounts to an increase from 2.0 to 2.2 hospitals on average) is associated with a 5.42 percent decline in the AMI mortality rate (95% confidence interval: 1.36-9.48 percent).[9] This corresponds to a reduction in the AMI mortality rate from an average 14.0 to 13.2 percent (or 8 deaths per 1000 AMI admissions).[10] The effect on AMI mortality remains positive for alternative competition measures (e.g., the number of hospitals in a fixed radius), but is lower in magnitude and tends to be less significant. We also find a positive effect on all-cause mortality for emergency admissions, but the effect is fairly small (and less significant): a 10 percent reduction in average HHI is associated with a 0.5 percent fall in the all-cause mortality. This corresponds to a reduction in average all-cause mortality from 4.20 to 4.18 percent (or 2 deaths per 10,000 emergency admissions).

Second, for *hospital readmissions,* we find that the introduction of competition in the Norwegian NHS is associated with a sharper *increase* in *readmission rates* for emergency admissions at hospitals in more competitive areas compared to hospitals in less competitive areas after the reform. Higher (emergency) readmission rates are interpreted as being the result of more complications related to the first treatment and thus an indicator of poorer treatment quality. We find that a 10 percent reduction in average HHI is associated with a 1.47 percent increase in (emergency) readmission rates. This corresponds to an increase in average readmission rates from 11.20 to 11.37 percent (or almost 2 readmissions per 1000 emergency admissions).

Third, for *hospital cost efficiency,* we find that the introduction of competition is associated with a sharper *decline* in *length of stay* for emergency admissions at hospitals in more competitive areas compared to hospitals in less competitive areas. As length of stay is a key cost component for inpatient hospital care, this result suggests a pro-competitive effect on hospitals' cost efficiency. However, the effect is fairly small in magnitude. A 10 percent reduction in the average HHI is associated with a 0.37 percent fall in length of stay. This corresponds to a reduction in the average length of stay from 6.51 to 6.49 days per admission (or 2 days per 10,000 emergency admissions). The effect on length of stay is also less robust to different model specifications.

The main analysis is extended in two different directions. In the first extension, we exploit exogenous variations in the treatment (DRG) prices due to governmental changes in the DRG share of total hospital funding, which varies between 40 to 60 percent in the period. In particular, we generate a dummy that captures the years with 60 percent DRG-prices in the post-reform period, and interact this with our DiD coefficient. The results confirm our theoretical prediction related to the regulated price. When the DRG price increases, the pro-competitive effects of introducing patient choice tend to be reinforced. This is true for all-cause mortality and length of stay. For readmissions the negative effect of competition almost vanishes when DRG prices increase to 60 percent of total funding, again confirming our theoretical prediction that a higher DRG price reduces the scope for adverse competition effects.

In the second extension, we investigate the impact of competition on waiting times and admissions. This is because Norway has a publicly funded system where hospital care is mostly free of charge. At the same time, there are capacity constraints leading to long waiting times for elective treatments. This means that waiting times are used to ration excessive demand for hospital care. However, hospitals can (use surplus to) invest in capacity to increase the number of admissions. The results show that hospitals in more competitive areas have a sharper decline in waiting times after the reform than hospitals in less competitive areas: a 10 percent reduction in the average HHI is associated with a 1.05 percent reduction in waiting times (from 125.3 days to 123.9 days on average). We also find that hospitals in more competitive areas have a larger increase in the number of admissions: a 10 percent reduction in the average HHI is associated with a 0.4 percent increase in admissions (from an average of 9.78 to 9.82 per DRG per quarter). However, waiting time applies only to elective (non-emergency) treatments, which implies issues with patient selection that may bias the estimates. The same applies for total admissions, as this covers both emergency and elective treatments. Thus, these results should be interpreted with some caution.

The rest of the paper is organised as follows. Section 2 relates our study to the existing literature. Section 3 provides a theoretical model that derives predictions on the effects of non-price hospital competition. Section 4 describes the Norwegian NHS and the patient choice reform. Section 5 presents our empirical strategy. Section 6 describes data and descriptive statistics, including also a test for the parallel trends assumption. Section 7 presents the results and Section 8 a wide set of robustness checks. Section 9 concludes the paper. Appendix A contains supplementary calculations for the theoretical analysis, Appendix B presents the estimations and description of the hospital competition measures used in the empirical analysis, Appendix C contains the results of the test for the parallel trends assumption, and Appendix D contains the tables of the results from a wide set of robustness checks.

2. Related literature

Our paper relates primarily to the empirical literature on the impact of competition in hospital markets.[11] The evidence on the effect of competition is mostly from the US and the UK, and has mixed findings. In the US, Kessler and McClellan (2000) find that

[9] The average (predicted) pre-reform HHI is 4946, as shown in Table 1. In a symmetric oligopoly, this corresponds to an average of 2.1 hospitals. A 10 percent reduction in the average HHI is then equivalent to increasing the number of hospitals from 2.0 to 2.2 (or from 5 to 6).

[10] The point estimate for stroke mortality has the same sign and magnitude as AMI mortality, but is not statistically significant.

[11] There is also a large theoretical literature on hospital competition. Gaynor (2006) shows that profit-maximising hospitals respond to competition by improving quality of care when prices are fixed, while this effect is generally ambiguous with endogenous prices. Brekke et al. (2011) show that the positive effect of hospital competition on quality with fixed prices holds also for semi-altruistic hospitals unless the degree of altruism is sufficiently high. See also Brekke et al. (2010) for a study on quality competition with endogenous prices.

AMI mortality is higher for Medicare patients in more concentrated (less competitive) markets.[12] They also find that hospitals in more concentrated areas have higher expenditures when Medicare introduced fixed (DRG) prices, and conclude that (non-price) competition among US hospitals is welfare improving. Shen (2003) finds that competition (measured by the number of hospitals) interacted with the Medicare payment leads to lower AMI mortality for Medicare patients after 1990. In contrast, Gowrisankaran and Town (2003) find that AMI and pneumonia mortality rates are higher for Medicare patients in less concentrated markets in the Los Angeles area. Mukamel et al. (2001) find no effect of competition (measured by concentration) on overall hospital mortality for Medicare patients. Colla et al. (2016) find that competition reduces AMI mortality, has no effect on emergency readmissions for hip and knee replacement, and reduces quality for dementia patients in Medicare.[13]

In England, Propper et al. (2004) and Propper et al. (2008) find that more competition increased AMI mortality in the 1990s when the internal market was introduced and hospital prices negotiated (not regulated) with local health authorities. Propper et al. (2008), however, find that competition did significantly reduce waiting times, which may be explained by purchasers negotiating mainly on waiting times rather than on clinical quality. The most recent empirical studies from England find that competition reduces AMI mortality after the price negotiation regime was abandoned and replaced by a fixed price regime. In more detail, Bloom et al. (2015) use cross-sectional data, instrument for competition with the marginality of local Parliamentary seats and find that hospitals in more competitive areas had lower AMI mortality. Cooper et al. (2011) and Gaynor et al. (2013) find, as mentioned previously, that hospitals in more competitive areas had sharper reductions in AMI mortality following the patient choice reform of 2006. Moscelli et al. (2018) also find that competition reduces hip fracture mortality, but not stroke mortality. In relation to efficiency, Gaynor et al. (2013) find that competition reduces length of stay but does not affect expenditure or volume of admissions. Cooper et al. (2018) find that the entry of a private hospital in the NHS market reduced pre-operative length of stay for hip and knee replacement patients. Some studies investigate quality for non-emergency treatments. Feng et al. (2015) find that competition is positively associated with health gains for hip replacement patients, where the health gain is measured by the difference in patient-reported health outcomes (PROMs) before and after the surgery. However, this is not confirmed by Skellern (2019), which uses an instrumental variable approach and finds that competition reduces elective quality as measured by PROMs in 2009-13 (after patient choice was introduced). Moscelli et al. (2021) find that patient choice led to an increase in emergency readmissions for hip and knee replacement, but not for coronary bypass, using an empirical strategy in line with Cooper et al. (2011) and Gaynor et al. (2013). For Italy, Berta et al. (2016) find no association between hospital competition and quality (measured by an index of mortality and readmissions), which they explain by lack of quality information for patients.

Our paper contributes to the literature on hospital competition along several dimensions. First, many of the existing studies use cross-sectional variation in market structure over time to estimate the effects of competition. A key issue, though, is that market structure is endogenous. Most of the previous literature aims at accounting for this by computing HHIs based on predicted, rather than actual, patient choices, as proposed by (Kessler and McClellan, 2000). While we also compute HHIs based on predicted patient choices, we follow Gaynor et al. (2013) and Cooper et al. (2011) to identify the causal effects of competition by employing a DiD research design that exploits a reform that introduces competition and makes use of competition measures based only on pre-reform market structures, which are plausibly exogenous due to administrative referrals of patient.

Second, the vast majority of existing studies are from the US or the UK. Thus, our study contributes by providing evidence from a country with different market characteristics and institutional settings.[14] The Norwegian NHS is obviously very different than the more market-based health care system in the US, but it also differs from the English NHS along a few important dimensions that may influence the impact of competition. These differences include hospital ownership (which are mainly public in Norway and not independent trusts as in England), hospital payment scheme (which is a mixture of block grant and fixed prices in Norway, while more of a pure fixed price scheme in England), and geography (the distribution of patients and hospitals is much more dispersed in Norway than in England).

The closest papers to ours are Cooper et al. (2011, 2012) and Gaynor et al. (2013). While we adopt a similar approach to identify the impact of hospital competition, our study differs along several dimensions. First, in the English NHS there was a gradual roll-out of the fixed price (Payment by Result) funding scheme in the pre-reform period, with the full scale implementation coinciding with the patient choice reform, which potentially could affect the estimates of competition, as pointed out by Gaynor et al. (2013). This was not the case in the Norwegian NHS, where the hospital payment scheme was introduced several years before the patient choice reform, which implies that we have a cleaner identification of the competitive effects.

Second, Cooper et al. (2011, 2012) and Gaynor et al. (2013) use only hospital fixed effects to account for endogeneity issues when considering outcomes that covers several DRGs (such as all-cause mortality, length of stay and total admissions). Our study takes additional steps by including also treatment (DRG) fixed effects, which allow us to control for all unobserved (and observed) treatment-specific heterogeneity that is time invariant. This means that the effects of competition are identified using only within hospital and treatment (DRG) variation, which essentially means that we estimate the competitive effects at product (or service) level rather than at aggregate firm level. We believe this approach is more appropriate when considering supply-side (hospital) responses

[12] In a related paper, Kessler and Geppert (2005) find that the AMI mortality rate for high-risk Medicare patients is higher in concentrated markets, while there is no such effect for low-risk patients.

[13] There is also a related strand of literature on the interaction between information and competition in hospital markets. For example, Chou et al. (2014) find that report cards on the quality of providers reduced CABG mortality for more severely ill patients in more competitive areas.

[14] There exists a few studies of the patient choice reform in Norway, but none of these focus on the competitive effects of this reform. For a review of this literature, see Brekke and Straume (2017).

to competition on outcomes that cover many DRGs (services or disease groups), as it eliminates compositional effects across DRGs that may influence the outcome variables if considered only at the aggregate hospital level.

Third, we extend the mentioned studies by considering more outcomes, such as readmissions and waiting times, but also by investigating how changes in the fixed prices interact with the competition effect. We do so by exploiting changes in the relative share of block grant and fixed (DRG) price funding, which effectively implies exogenous changes in the DRG price levels. We interact the DiD coefficient with the years with a high DRG share, and estimate the differential effects of competition on quality and cost efficiency. We thus provide new evidence on how the incentives to compete for patients are affected by the regulated prices.

3. Theoretical framework

Suppose that there are two hospitals, denoted by subscripts i and j, in a given market for secondary health care.[15] Patients are fully insured against health expenses, so the demand for Hospital i, measured by number of treatments, is given by $x_i(q_i, q_j, \theta)$, where $q_k \geq \underline{q}$ is the quality of Hospital $k = i, j$.[16] The lower bound on hospital quality represents the minimum treatment quality that hospitals are allowed to offer, implying that $q < \underline{q}$ can be interpreted as malpractice. We assume that x_i is increasing in q_i and decreasing in q_j. The effect of competition is captured by the parameter θ, which measures the degree of patient choice, implying that a higher value of θ represents a market with more competition. We assume that $\partial x_i/\partial\theta > (<)0$ if $q_i > (<)q_j$, and that $\partial x_i/\partial\theta = 0$ if $q_i = q_j$. Thus, for a given distribution of qualities across hospitals, patient choice increases (reduces) demand for hospitals with higher (lower) quality. Furthermore, we assume that $\partial^2 x_i/\partial\theta\partial q_i > 0$, implying that patient choice makes demand for each hospital more responsive to quality changes. This is, intuitively, the key effect of competition in markets where the providers compete on quality.

The objective function of Hospital i is assumed to be given by

$$\pi_i = (1-\mu)\left[T + p x_i(q_i, q_j, \theta) - (1-\eta)c\left(x_i(q_i, q_j, \theta), q_i, e_i\right)\right] + \alpha B_i\left(x_i(q_i, q_j, \theta), q_i\right) - g(e_i). \tag{1}$$

The hospital payment system is characterised by the contract (p, T), where each hospital receives a fixed price p per treatment and a lump-sum payment T. Total treatment costs are given by a cost function $c(\cdot)$ which depends on the total number of treatments (x_i), quality (q_i) and the amount of cost-containment effort (e_i) exerted by the hospital.[17]

We assume that, by spending more effort on cost containment, the hospital can (i) reduce the total costs of a given treatment volume and quality provision ($\partial c/\partial e_i < 0$), (ii) reduce the marginal cost of treatments ($\partial^2 c/\partial e_i \partial x_i < 0$) for a given quality level and (iii) possibly also reduce the marginal cost of quality provision for a given treatment volume ($\partial^2 c/\partial e_i \partial q_i \leq 0$). The disutility of exerting cost-containment effort is given by a strictly convex function $g(e_i)$.

We also assume that the providers are semi-altruistic in the sense that patient utility is part of the hospitals' objectives. More specifically, we assume that the decision-makers at Hospital i to some extent take into account the total utility of patients treated at the hospital, given by $B_i(\cdot)$, which is increasing in x_i and q_i. The degree of altruism is captured by the parameter α, implying that a purely profit-oriented hospital is characterised by $\alpha = 0$.

Finally, we include two parameters that are intended to capture two particular, and closely related, features of public (or non-profit) hospitals, namely *profit constraints* and *soft budgets*. Profit constraints, in the sense that profits cannot be distributed, are captured by the parameter $\mu \in (0, 1)$, effectively working as a tax on profits. This is a standard way of modelling profit constraints in the literature and rests on the underlying assumption that distributing profits in kind (e.g., in the form or perquisites) is less valuable than distributing profits in cash.[18] Budget softness, on the other hand, is modelled, somewhat crudely, as being equivalent to cost sharing and captured by the parameter $\eta \in (0, 1)$. The underlying assumption is that softer budgets imply that hospital deficits might be partly covered ex post, which makes the hospital management relatively less concerned about costs (i.e., softer budgets make hospitals less cost-disciplined).[19]

Suppose that the two hospitals play a non-cooperative game where they simultaneously choose quality and cost-containment effort. The first-order conditions for Hospital i are given by

$$\frac{\partial \pi_i}{\partial q_i} = (1-\mu)\left(p\frac{\partial x_i}{\partial q_i} - (1-\eta)\left(\frac{\partial c}{\partial x_i}\frac{\partial x_i}{\partial q_i} + \frac{\partial c}{\partial q_i}\right)\right) + \alpha\left(\frac{\partial B_i}{\partial x_i}\frac{\partial x_i}{\partial q_i} + \frac{\partial B_i}{\partial q_i}\right) = 0, \tag{2}$$

$$\frac{\partial \pi_i}{\partial e_i} = -(1-\mu)(1-\eta)\frac{\partial c}{\partial e_i} - \frac{\partial g}{\partial e_i} = 0. \tag{3}$$

---

[15] As long as the market is symmetric, the analysis can easily be extended to n hospitals. However, only two hospitals are needed in order to illustrate all the potential mechanisms at play.

[16] Hospital quality is multi-dimensional and should be interpreted broadly, covering all quality aspects that affect patient choice, including clinical quality, service quality, waiting time, etc. Lower mortality, better service, and shorter waiting time are quality indicators that are likely to increase a hospital's demand, everything else equal.

[17] In reality, a hospital's quality and cost efficiency are likely to result from choices made by different decision makers (e.g., managers and doctors) whose objectives might not be perfectly aligned. We take a standard black-box approach and assume that the objective function (1) is an aggregation of the objectives of all relevant decision makers within the hospital.

[18] See, e.g., Glaeser and Shleifer (2001), Ghatak and Mueller (2011), and Brekke et al. (2012).

[19] Soft budgets might not only imply that any deficit is (partly) covered, but also that any surplus is (partly) confiscated. If this is symmetric, soft budgets would be equivalent to a profit constraint in our model. For a more explicit and comprehensive modelling of hospital competition with soft budgets in a context of demand uncertainty, see Brekke et al. (2015).

We consider a symmetric equilibrium with interior solutions, where $q_j = q_i > \underline{q}$ and $e_j = e_i > 0$. The Nash equilibrium is then characterised by the following 2-equation system:

$$F_q := \left.\frac{\partial \pi_i}{\partial q_i}\right|_{q_j=q_i, e_j=e_i} = 0, \tag{4}$$

$$F_e := \left.\frac{\partial \pi_i}{\partial e_i}\right|_{q_j=q_i, e_j=e_i} = 0. \tag{5}$$

3.1. Competition and quality provision

From (4)-(5), we derive the following relationship between patient choice and equilibrium quality provision (see Appendix A for details):

$$\frac{\partial q_i}{\partial \theta} > (<) 0 \quad if \quad (1-\mu)\left(p-(1-\eta)\frac{\partial c}{\partial x_i}\right) + \alpha\frac{\partial B_i}{\partial x_i} > (<) 0. \tag{6}$$

An effect (positive or negative) of competition on equilibrium quality provision requires that the equilibrium is an interior solution with $q_i > \underline{q}$. Thus, the condition in (6) needs to be seen in conjunction with the first-order condition for optimal quality provision, given by (2).[20] For this purpose, it is useful to re-write (2) as follows:

$$\left[\frac{p}{1-\eta} - \frac{\partial c}{\partial x_i} + \frac{\alpha}{(1-\mu)(1-\eta)}\frac{\partial B_i}{\partial x_i}\right]\frac{\partial x_i}{\partial q_i} + \frac{\alpha}{(1-\mu)(1-\eta)}\frac{\partial B_i}{\partial q_i} = \frac{\partial c}{\partial q_i}, \tag{7}$$

Comparing (6) and (7), we see that the sign of $\partial q_i/\partial\theta$ is given by the sign of the sum of the terms in square brackets on the left-hand side of (7).

Consider first the special case of purely profit-oriented hospitals (i.e., $\alpha = 0$). It is evident from (7) that, if an interior solution exists, each hospital will choose a quality level that implies a positive 'perceived' price-cost margin (i.e., $p/(1-\eta) - \partial c/\partial x_i > 0$). This implies, from (6), that $\partial q_i/\partial\theta > 0$. Because of continuity, this result holds also for sufficiently small values of α. Thus, if the degree of altruism is sufficiently low, competition leads to higher quality.[21] The intuition behind this result is straightforward. If the marginal patient is profitable to treat, a sufficiently profit-oriented hospital will react to competition (which implies a more quality-elastic demand) by increasing quality in order to attract more patients.[22]

However, a sufficiently high degree of altruism might introduce a counteracting incentive. All else equal, altruism stimulates incentives for quality provision. This creates not only a larger scope for the existence of an interior-solution equilibrium, but it also creates a scope for an interior solution with a negative perceived price-cost margin (i.e., $p/(1-\eta) - \partial c/\partial x_i < 0$). In this case, competition has two counteracting effects on the incentives for quality provision. On the one hand, hospitals have an incentive to 'compete' to avoid treating unprofitable patients (since $p/(1-\eta) - \partial c/\partial x_i < 0$), implying lower quality. On the other hand, the presence of semi-altruistic preferences creates an incentive for 'altruistic competition' to treat more patients, implying higher quality. Overall, competition will lead to lower quality provision in equilibrium if the former effect is stronger than the latter. From (6)-(7) we see that the scope for a negative relationship between competition and quality (i.e., $\partial q_i/\partial\theta < 0$) is larger if p is relatively low, and if $\partial B_i/\partial q_i$ is large relative to $\partial B_i/\partial x_i$ (i.e., if the hospitals care more about the quality offered to patients than about the number of patients treated).

In qualitative terms, the above described results hold with and without profit constraints, and for any degree of budget softness. The effect of profit constraints (i.e., $\mu > 0$) is equivalent to an increase in α, as we can see from (7), which increases the scope for a negative relationship between competition and quality. Nevertheless, semi-altruistic preferences (i.e., $\alpha > 0$) is a necessary (but not sufficient) condition for such a relationship even in the presence of profit constraints. On the other hand, soft budgets (i.e., $\eta > 0$) is equivalent to an increase in α *and* p, which implies that the effect of budget softness on the nature of the relationship between competition and quality is ambiguous.

3.2. Competition and cost efficiency

The effect of increased patient choice on equilibrium cost efficiency is given by (once more, see Appendix A for details):

$$\frac{\partial e_i}{\partial \theta} > (<) 0 \quad if \quad \frac{\partial F_e}{\partial q_i}\frac{\partial q_i}{\partial \theta} > (<) 0 \tag{8}$$

[20] These conditions represent a generalised version of the main result derived in Brekke et al. (2011), where competition is explicitly modelled as a switch from local monopolies to localised competition in a spatial framework.

[21] Alternatively, if an interior solution does not exist, i.e., if

$$\left[\frac{p}{1-\eta} - \frac{\partial c}{\partial x_i} + \frac{\alpha}{(1-\mu)(1-\eta)}\frac{\partial B_i}{\partial x_i}\right]\frac{\partial x_i}{\partial q_i} + \frac{\alpha}{(1-\mu)(1-\eta)}\frac{\partial B_i}{\partial q_i} < \frac{\partial c}{\partial q_i}$$

for all $q_i \geq \underline{q}$, each hospital will choose quality at the minimum level and (a marginal increase in) competition has no effect on equilibrium quality provision.

[22] Notice that, since $\mu \in (0,1)$, a positive price-cost margin always implies a positive 'perceived' price-cost margin. Thus, if hospitals are sufficiently profit-oriented, a positive price-cost margin always implies a positive relationship between competition and quality provision.

Thus, under the condition $\partial F_e/\partial q_i > 0$, competition leads to higher (lower) cost efficiency if it also leads to higher (lower) quality provision. The condition $\partial F_e/\partial q_i > 0$ requires that either (i) higher quality provision leads to higher total demand for hospital treatment, i.e., $\partial(x_i + x_j)/\partial q_i > 0$, or (ii) higher quality provision reduces the marginal cost of cost-containment effort for a given treatment volume, i.e., $\partial^2 c/\partial e_i \partial q_i < 0$.

If (i) holds, a positive relationship between competition and quality provision implies that competition also leads to a higher treatment volume at each hospital, which gives each hospital a stronger incentive to increase the price-cost margin (or reduce a negative price-cost margin) by reducing marginal treatment costs.[23] If (ii) holds, a positive relationship between competition and quality provision implies that competition also increases the return to cost-containment effort (i.e., if competition leads to higher quality it increases the absolute value of $\partial c/\partial e_i$), thereby leading to increased cost efficiency. Obviously, the logic is reversed for the case of a negative relationship between competition and quality provision. Thus, if (i) or (ii) holds, quality and cost-containment efforts are complementary strategies for each hospital. On the other hand, if total demand for hospital treatment is fixed and if it is not possible to reduce the marginal cost of quality provision through cost-containment effort, then competition has no effect on hospital cost efficiency. These results hold regardless of whether the hospitals are profit constrained or face soft budgets.

The theoretical analysis in this section can be summarised by the following set of results.

1. If the *price-cost margin* is *positive*, exposing hospitals to competition results in better quality and cost efficiency irrespective of whether the hospitals are profit- or patient-oriented.
2. If the *price-cost margin* is *negative* and hospitals are *semi-altruistic,* the effects of competition on quality and cost efficiency are generally ambiguous.
3. All else equal, a higher (lower) treatment price p increases (reduces) the scope for a positive relationship between competition and quality.
4. If competition has an effect on cost efficiency, it always goes in the same direction as the effect on quality.

The question of how semi-altruistic hospitals respond to (increased) competition is thus ultimately an empirical question, which we will address in greater detail in the subsequent sections.

4. The Norwegian NHS and the patient choice reform

Norway has a National Health Service (NHS) with mandatory national health insurance that is primarily financed through general taxation. The NHS covers basically the whole Norwegian population and offers a comprehensive set of health services to the citizens. There exists a private health care sector alongside the NHS, but this is quite small and amounts to less than five percent in terms of total health expenditures.

The municipalities are responsible for primary care and the services are provided by General Practitioners (GPs). The large majority of the GPs (around 90 percent) have a contract with the municipality with a payment scheme defined by a mixture of capitation and fee-for-service, which on average amount to 30 and 70 percent, respectively, of the GPs' total payment. There are also a few GPs (around 10 percent) that are employed by the municipality and receive a fixed salary. Patients can choose which GP to register with, and the GPs act as gatekeepers to specialist care, so patients can only access an NHS hospital after receiving a referral from their GP.

The central government is responsible for the delivery of secondary care, and has divided the country into four health regions (north, south/east, west and middle). The large majority of NHS hospitals have public ownership, but there are also a few private non-profit NHS hospitals.[24] During our sample period, there were 64 NHS hospitals in Norway; 58 public and 6 private non-profit. All NHS hospitals face the same regulation and funding schemes, so the only difference between the public hospitals and private non-profit hospitals is ownership and governance.[25] The (public and private) NHS hospitals are general hospitals that offer a wide set of emergency and elective treatments. Fig. 1 displays the geographical distribution of the NHS hospitals in Norway for our sample period from 1998 to 2005, and shows that there are large variations across regions in the density of hospitals. The pattern of NHS hospital locations closely follows the population distribution in Norway.

The hospital payment scheme in the Norwegian NHS is a mixture of block grant and volume-based funding. The block grant is an annual lump-sum payment based on the number of inhabitants in the hospital catchment area with some risk adjustments. The volume-based funding is based on the diagnosis related groups (DRG) scheme, where each hospital is paid a fixed price per treatment and the price is set equal to the average costs across (a representative sample of) hospitals within each DRG. This mixed payment scheme was introduced in 1997 and replaced a pure block grant scheme. The relative shares of block grant and DRG-based funding is decided by the central government. For the period we study the relative shares vary between 40 to 60 percent.[26] The implication

[23] This incentive does not depend on the sign of the price-cost margin, since the profit gain associated with a marginal cost reduction does not depend on the price. Thus, as long as the hospitals care about profits to some extent, a higher volume implies stronger incentives for cost reductions, also for semi-altruistic hospitals that operate at a negative price-cost margin.

[24] There are no private for-profit NHS hospitals, but the NHS may contract with for-profit providers for specific outpatient treatment (e.g., day surgery) through competitive tenders. Since we focus on inpatient treatments, our analysis does not involve private for-profit providers. The share of total hospital care they provide is also negligible.

[25] The public NHS hospitals are organised as state-owned health enterprises (that by legislation cannot go bankrupt). The private NHS hospitals are non-profit organisations, with the usual restrictions on distribution of profits.

[26] The fixed (DRG) price share of total hospital funding was 45% in 1998, 50% in 1999 to 2001, 55% in 2002, 60% in 2003, 40% in 2004 and 60% in 2005.

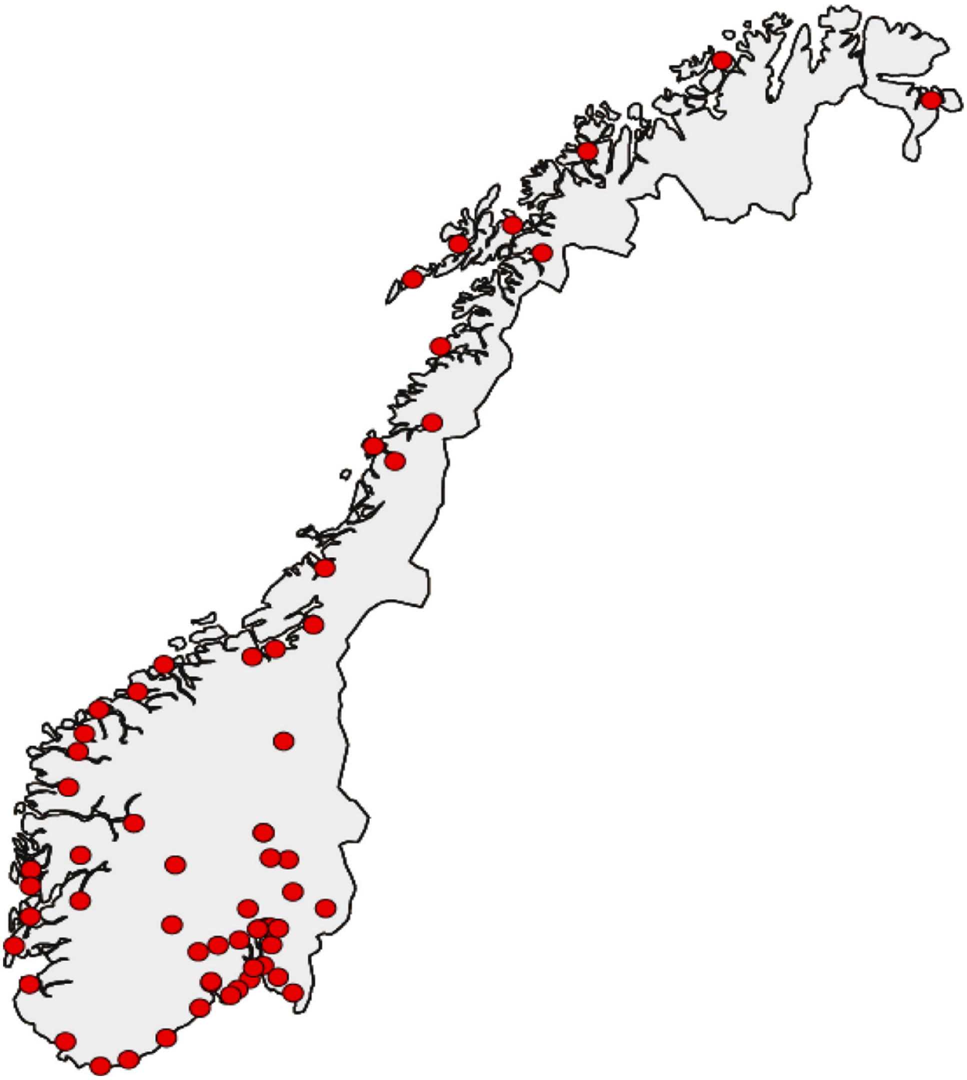


Fig. 1. Distribution of NHS hospitals in Norway, 1998 - 2005.

of such a mixed payment scheme is that the NHS hospitals do not receive the full DRG price when treating a patient. If, for example, the block grant funding is set at 60 percent, then each hospital receive only 40 percent of the full DRG price for each patient they treat. Thus, the fixed DRG price is reduced proportionally to the share of the block grant funding. The DRG price is paid for both elective and emergency admissions (and readmissions).[27]

While patients need a referral from their GP to obtain care from an NHS hospitals, there is virtually no out-of-pocket expenses related to receiving hospital care. For inpatient care, which is what we study in this paper, the insurance coverage is 100 percent. There could be some copayments for outpatient hospital care or specialist treatment outside the hospitals, but the out-of-pocket expenses are generally very low due to an annual expenditure cap (at around € 200) for each patient covering all sorts of copayments. The only monetary costs for the patients when demanding inpatient hospital care are travelling expenditures.

The patient choice reform in the Norwegian NHS, which we study in this paper, was introduced in 2001, and replaced an administrative system where patients were referred to the closest hospital within their county of residence by their GP. Thus, hospitals were basically local monopolists in their catchment area prior to the reform. After the reform, patient were allowed to select any NHS hospital across the whole of Norway for elective (non-acute) treatments. To stimulate patient choice, the government also allowed for reimbursement of travel expenses (net of a deductible), which is likely to be important given that Norway is geographically large with a widely dispersed population, implying long distances between hospitals. Moreover, the government also set up a ‘patient choice’

[27] The DRG price for a readmission is identical to a standard admission. There is no financial penalty related to a readmission in the hospital funding scheme in Norway.

website with fairly detailed hospital information at treatment level, including a set of quality indicators and expected waiting times.[28] This information was published by the government to enable patients (and the GPs) to make informed choices of hospital.

The available evidence suggest that the policy reform had moderate effects on actual patient choices. Ringard et al. (2006) report a small increase in patient mobility across health regions in the first two years after the reform (from 8–9% to 9–10%). They also show results from a survey where 48% of GPs reported that the reform had made them change their referral pattern to some extent. According to the results reported by Godager and Iversen (2004), choice of hospital is more likely to be made by higher-educated patients, with the most important choice criterion being "reputation for high-quality treatment". Indirect evidence on patient choice effects is also provided by Bjorvatn and Ma (2011). Using data for the period 1999–2005, they present evidence from regression analysis showing that patient mobility responds positively to differences in waiting times, particularly for younger patients. These results do not necessarily imply that hospitals are competing for patients, but indicate that the patient choice reform has created conditions for hospitals to compete.

There was a second policy reform the year after the patient choice reform that changed the ownership and organisation of the *public* NHS hospitals. The motivation behind this organisational reform was to reduce political influence on the governance of the public NHS hospitals, but also to enforce harder budgets and better cost discipline. The reform transferred ownership from the counties to the central government, and reorganised the public hospitals into state-owned health enterprises that, by legislation, could not go bankrupt. During the period we study, there were loss-making hospitals but no hospital closures. This organisational reform gave the public NHS hospitals also more financial autonomy, as surpluses and deficits could be transferred across years.

5. Empirical strategy

To identify the causal effects of exposing NHS hospitals to competition, we adopt a DiD research design, similar to Cooper et al. (2011) and Gaynor et al. (2013), that exploits the patient choice reform introduced in the Norwegian NHS in 2001. The basic idea is that, while the reform applied to all NHS hospitals at the same time, the treatment (competition) effect of the reform varies substantially across geographic locations.[29] In particular, hospitals located in rural areas with long distances to other hospitals are likely to be affected less by the patient choice reform than hospitals located in urban areas with many hospitals close by. This variation in treatment (competition) intensity across the NHS hospitals facilitates the use of a DiD approach, where the effect of exposing hospitals to competition is identified by comparing the relative change in outcomes for hospital in less competitive areas with hospitals in more competitive areas before and after the reform.

The treatment intensity of the patient choice reform is captured by a hospital-specific competition index. As common in the literature, we compute an HHI for each hospital based on market shares, which are given by the share of patients in the catchment areas. To compute the hospital-specific HHIs, we follow a standard two-step procedure. First, we compute the hospital's market share in a given municipality as its share of the total number of hospitalised patients in that municipality.[30] Second, we compute the hospital's HHI weighted with the share of patients coming from a given municipality relative to the total number of patients at the given hospital.

A key issue, though, is that market (patient) shares are endogenous. We take two measures to account for this. First, we use predicted rather than actual market (patient) shares when computing the hospital-specific HHIs. This is done by estimating a logit choice model, which controls for patient and hospital characteristics, as originally suggested by Kessler and McClellan (2000). Second, we use only market shares in the pre-reform period (with no patient choice) as these are arguably more exogenous than market shares in the post-reform period (with patient choice). This approach is consistent with Gaynor et al. (2013), but not with Cooper et al. (2011) who use contemporaneous HHIs in the estimation.[31] The use of pre-reform market shares is a key advantage, as patient flows are likely to be affected by hospital quality after the patient choice reform, and thus likely to give rise to reverse causality bias between hospital competition and quality of care. For further details, see Appendix B.

To identify the effects of exposing NHS hospitals to competition (induced by patient choice), we estimate the following DiD regression model,

$$Y_{hdt} = \gamma_{hd} + \lambda_t + \delta\left(D_t * HHI_h\right) + X'_{hdt}\beta + \varepsilon_{hdt}, \tag{9}$$

where h denotes the hospital, d the DRG and t the quarter (with $t = 1, ., 32$). Y_{hdt} is the dependent variable of interest, which is either a mortality rate (AMI, stroke, or all-cause mortality), readmission rate, length of stay, waiting time or admissions in a given hospital,

[28] At the patient choice website (operated by the Health Directorate) you entered your diagnosis and then you would get a list of NHS hospitals that offered relevant treatment. This list would also report expected waiting time for the specific treatment, the monthly/yearly number of treatments, patient satisfaction, and also a few clinical quality indicators. The Health Directorate also publish quality indicators at hospital level such as risk-adjusted mortality rates, hospital infections, etc.

[29] This idea was initially presented in Propper et al. (2008) who study the impact of competition in the English NHS in the 90s, using discouragement of competition by new (Labour) administration in 1997 as policy change.

[30] Kessler and McClellan (2000) compute patient shares at zip-code level, Gaynor et al. (2013) at neighbourhood level, and Cooper et al. (2011) at GP level. We have only information of which municipality the patient lives in, but for the period we study there are more than 450 municipalities of a population of around 4 million inhabitants, so municipalities are generally small in Norway.

[31] We have estimated the DiD model using also post-reform variation in the treatment intensity variable, as Cooper et al. (2011). The results are quite similar in qualitative terms, and available upon request.

DRG and quarter.[32] All dependent variables are log-transformed. D_t is a post-reform dummy taking the value 1 for all periods after the patient choice policy was implemented in January 2001 and 0 otherwise. HHI_h is the logarithm of the predicted pre-reform HHIs specific for each hospital. We interact this concentration measure with the post-choice dummy. Thus, δ is our key coefficient of interest, which captures how hospitals facing more competition responded differentially to the patient choice policy in 2001 relative to hospitals facing less competition (as measured by HHI_h). Given that both the dependent variable and HHI_h are in logs, this DiD coefficient can be interpreted as an elasticity.

In the regression, we include hospital and DRG fixed effects (γ_{hd}) that control for hospital and treatment specific unobserved (and observed) time-invariant heterogeneity. We enter the fixed effects multiplicatively, rather than additively, leading to ($h \times d$) fixed effects.[33] This flexible fixed-effects specification does not impose that differences in mortality (or other outcomes) between DRGs are constant across hospitals, but instead allows mortality for each hospital-DRG pair to vary, and therefore captures any time-invariant unobserved differences in casemix or other factors for each hospital and DRG. In more detail, in addition to patient casemix the hospital and DRG fixed effects control for time-invariant factors such as hospital type (local, regional, university), ownership (public or private non-profit), specialisation (mix of DRGs), etc., that may influence quality and cost efficiency. They also control for time-invariant locational factors, such as population size, health status, morbidity, etc. This specification implies that the effects of competition are estimated using only within hospital and DRG variation over time in our outcome variables.

In addition to controlling for time-invariant casemix through the hospital and DRG fixed effects, we also control for differences in time-varying casemix through a vector of patient characteristics (X_{hdt}), which are also entered as squared and cubic terms to allow for non-linear effects. These include the average age, the proportion of male patients and the Charlson index measured for each quarter, DRG and hospital.[34] The regression model also includes 31 time dummies (λ_t), one for each quarter in each year (with the first quarter used as reference category), to control for time trends in our dependent variables flexibly. Finally, ε_{hdt} is the error term.

6. Data and descriptive statistics

To analyse the effects of exposing NHS hospitals to competition, we have assembled a rich database with panel information at the hospital and DRG level on a range of variables, including hospital quality indicators, cost efficiency indicators, access to care indicators and patient case mix. The primary data source is the Norwegian Patient Registry (NPR), which covers the universe of hospital episodes in the Norwegian NHS.[35] From this registry we have obtained detailed patient level information for a period of eight years from 1998 to 2005. For each hospital episode, we observe a set of patient characteristics (age, gender, comorbidities, municipality of residence, etc.), treatment characteristics (date of admission, diagnosis, DRG, emergency or elective, regular admission or readmission, etc.), and hospital characteristics (university, regional or local hospital, address, ownership status, etc.).

6.1. Dependent variables

We use four indicators of hospital quality, including three measures of in-hospital mortality rates: (i) AMI mortality, (ii) stroke mortality, (iii) all-cause mortality of emergency patients, i.e., patients who were admitted to the hospital through the Accidents and Emergency department.[36] Additionally, we also include hospital readmission rates, which refer to patients who had an emergency hospital admission (in the same or any other hospital) within 30 days of discharge after their initial treatment. As for all-cause mortality, we compute readmission rates only for patients whose initial treatment was an emergency one. We therefore follow the literature and measure hospital quality only for emergency patients. This is to avoid possible selection bias due to unobserved severity, with more unobservably severe patients more likely to choose high-quality hospitals that would appear with higher relative mortality despite the higher quality.

While patient choice applies to elective treatments, quality indicators are usually published for emergency treatments as these are considered good markers of clinical quality. This means that demand is likely to be affected by emergency quality indicators, like AMI, stroke and all-cause mortality. Moreover, AMI and stroke patients are among the sickest and most likely to be influence by the quality of care. Thus, if hospitals respond to stronger competition by improving quality of care, this is likely to result in lower mortality among AMI and stroke patients. All-cause mortality and emergency readmissions are included to allow for broader competitive effects along more dimensions and across several DRGs.

[32] For AMI and stroke mortality, there is only one DRG and therefore the model reduces to

$$Y_{ht} = \gamma_h + \lambda_t + \delta\left(D_t * HHI_h\right) + X'_{ht}\beta + \varepsilon_{ht}.$$

[33] If entered additively this would instead lead to only ($h + d$) fixed effects.

[34] In order to better control for potential changes over time in each hospital's age-gender distribution, we have also included an interaction between average age and gender. However, all of our results are unaffected by the inclusion of this control variable and we have therefore dropped it from our main empirical model. Further details are available upon request.

[35] More information is available on the webpage of the Norwegian Directorate of Health: https://helsedirektoratet.no/english/norwegian-patient-registry

[36] Unfortunately, information on post-hospital discharge mortality rates (e.g., 30 day AMI mortality rate) was not available during our sample period. However, most studies do find similar effects of competition for both in-hospital and after-discharge mortality rates. See, for instance, Kessler and McClellan (2000) and Gaynor et al. (2013).

We also include an indicator of efficiency as proxied by length of stay, which is defined as the number of days from admission to discharge from hospital. Again, we measure length of stay only for emergency patients to avoid possible selection bias due to unobserved severity. As secondary outcomes, we also measure waiting times, as a proxy of patient responsiveness, and total admissions. We measure waiting times for elective (non-emergency) patients. They are defined by the number of days from a patient being placed on the waiting list (by a specialist following a referral) to being admitted to hospital. Instead, we measure total hospital admissions for all emergency and elective patients.

6.2. Sample

The unit of observation for each dependent variable is at quarter and hospital level (for AMI and stroke), and at quarter, hospital and DRG level for all other indicators with multiple DRGs (such as all-cause mortality and readmissions). All hospitals in our sample provide emergency treatments, leaving us with 64 hospitals of which 58 are public hospitals and 6 are private non-profit hospitals. However, for the analysis of AMI and stroke mortality, we exclude hospitals with very few patients (less than 3 patient per quarter) in order to avoid the problem of variability of rates from small denominators, reducing the number of hospitals to 61. For AMI and stroke mortality rates, we have respectively 1752 and 1765 (quarter × hospital) observations.

In order to exclude very infrequent DRGs, we exclude from our elective and emergency sample those observations that, in any given year, have less than 10 patients in the whole country. As a result, out of 462,563 observations measured at (quarter × hospital × DRG) level, we drop 13,112 observations, or 2,8% of the sample, leaving us with 449,451 observations. This sample is used to compute total admissions.

For all-cause mortality rates, readmission rates and length of stay, the sample includes only emergency patients. We further exclude 28 outliers exhibiting a length of stay higher than 180 days. Our final sample of emergency patients includes 382,176 observations, and 24,256 hospital × DRG cells. Waiting times are by definition measured for the sample of elective patients. After excluding (quarter × hospital × DRG) observations with waiting times longer than 730 days (3,345 observations out of 199,850, or 1.7%), we are left with 196,505 observations and 18,861 hospital × DRG cells.

To control for variations in patient casemix, we measure for each observation the average patient age, the proportion of patients which are male, and the Charlson index. Since the data include information on patient municipality of residence and hospital location, we can compute a matrix of travel distances between each patient residence and each hospital location, with distance measured in kilometers, which is used to compute the predicted HHIs for each hospital (see Appendix B).

6.3. Descriptive statistics

Table 1 presents the descriptive statistics for our five different samples (AMI, stroke, emergency patients, elective patients, and ‘emergency plus elective’ patients). The table shows that at the sample mean the AMI mortality is 14 percent and stroke mortality is 13.1 percent. For emergency patients, the overall in-hospital mortality is 4.2 percent, the readmission rate is 11.2 percent, and length of stay is 6.5 days. The average waiting time for elective patients is 125 days, and the total number of admissions for emergency and elective patients in the average quarter, hospital and DRG is 9.8.

Emergency (elective) patients are on average 54.5 (54.9) years old, and 50.9 (49.2) percent of them are male. They have a Charlson index of 0.8 (0.8). Thus, emergency patients do not appear to be more severe than elective patients. By contrast, AMI and stroke patients appear to be older and display more comorbidities than the average patient.

The degree of hospital competition (i.e., the treatment intensity of the patient choice reform) is captured by the HHI. The HHI of the average hospital in our sample indicates a fairly high degree of market concentration with actual and predicted HHIs at the levels of 4532 and 4946, respectively. As explained above, the HHIs are based on patient flows and the predicted HHIs are computed using the approach by Kessler and McClellan (2000). For more details, see Appendix B. Fig. 2 also displays the change in the average (predicted) HHI over time, and shows that HHI reduced and therefore competition increased following the reform. Fig. 3 compares the HHI distribution pre- and post-reform, and shows that although the HHI has shifted to the left, the distribution of HHI is very similar pre- and post-reform. However, note that in the diff-in-diff analysis we use the (predicted) HHI measured in 1998 (pre-policy) to avoid potential endogeneity of the HHI measure.

To get a first glimpse of the possible effects of exposing NHS hospitals in Norway to competition, we compare the percentage change[37] in the means of our dependent variables before and after the patient choice reform for hospitals with (predicted) HHI below or above the median, respectively. The descriptive statistics of these relative changes by market concentration over time are reported in Table 2. Regarding relative changes in our outcome variables, Table 2 shows that hospitals with an HHI below the median (more competition) tend to have a sharper reduction in mortality rates for AMI and stroke than hospitals with an HHI above the median (less competition). In more competitive areas, AMI and stroke mortality went down by more than 20% whereas in less competitive areas by less than 20%. All-cause mortality decreased by 20% in both areas. Readmission rates instead displayed a larger increase in more competitive areas. Similarly, there was a smaller decrease in length of stay in more competitive areas (9.1% versus 11.1%). Waiting times instead reduced more, though by a small amount, in areas with a higher degree of competition, whereas the increase in total admissions was larger in more competitive areas.

An underlying key assumption of our analysis is that patients exercise choice if allowed, and that their choices are partly based on the quality offered by the hospitals in the patients’ choice set. Indeed, Table 2 shows that patients travelled longer distances for

[37] We focus on the percentage change to be consistent with our log-log specification.

Table 1
Descriptive Statistics.

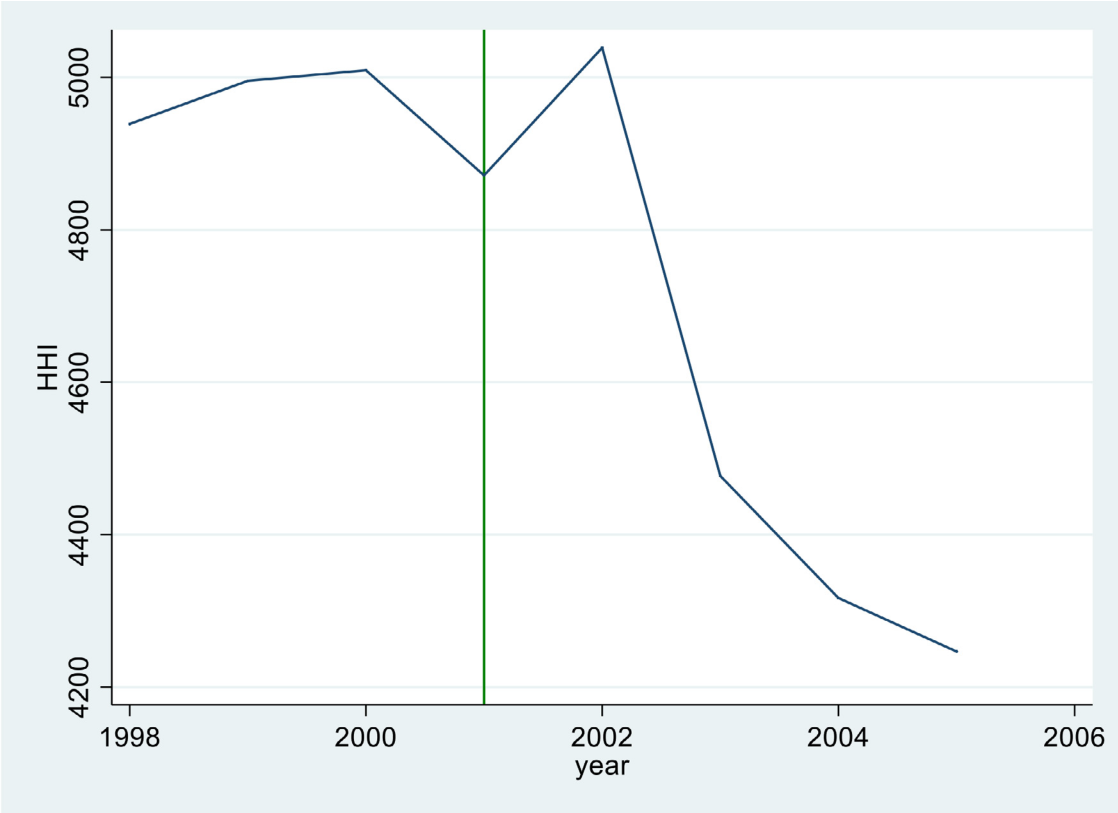
	Mean	Std. Dev	Obs.
Acute myocardial infarction (AMI) sample			
AMI mortality rate	0.140	0.074	1,752
Average age	72.744	3.294	1,752
Proportion male	0.608	0.100	1,752
Charlson index	2.350	0.527	1,752
Stroke sample			
Stroke mortality rate	0.131	0.074	1,765
Average age	74.603	3.631	1,765
Proportion male	0.501	0.108	1,765
Charlson index	1.490	0.243	1,765
Emergency patients sample			
All-cause mortality rate	0.042	0.152	382,176
Readmission rate	0.112	0.211	382,176
Length of stay (days)	6.510	7.842	382,176
Average age	54.504	23.376	382,176
Proportion male	0.509	0.360	382,176
Charlson index	0.802	1.191	382,176
Elective patients sample			
Waiting Times (days)	125.315	131.770	196,505
Average age	54.900	20.780	196,505
Proportion male	0.492	0.397	196,505
Charlson index	0.848	1.268	196,505
Emergency and elective patients sample			
Total admissions	9.775	19.545	449,451
Average age	54.607	22.302	449,451
Proportion male	0.502	0.352	449,451
Charlson index	0.801	1.162	449,451
Herfindahl-Hirschman Index (HHI)			
HHI (actual market shares)	4526.252	1139.975	1,910
HHI (predicted market shares, 1998)	4945.534	1425.053	1,910

Notes: Summary statistics refer to the period from 1998 to 2005. AMI and stroke sample is an unbalanced panel of 61 hospitals over 32 quarters. Hospitals with less than 3 AMI or stroke patients per quarter are excluded. The emergency and elective patients sample is an unbalanced panel including 64 hospitals over 32 quarters. The elective patients sample provides 168 unique DRGs, the emergency sample 313, and the emergency and elective sample 384 unique DRGs. Hospital-DRG cells are excluded if in any given quarter the hospital does not provide any treatment in the DRG. For emergency patients outcomes, in any given quarter we drop hospital-DRG cells if the length of stay for emergency patients is higher than 180 days. For elective patients outcomes, in any given quarter we drop hospital-DRG cells if the waiting time exceeds 730 days. HHIs are at hospital-quarter level. Summary statistics for HHIs are provided at the hospital-quarter level.

Table 2
Means of outcomes and travel distance by market concentration before and after reform.

	Predicted HHI below median				Predicted HHI above median			
	1998-2000 A	2001–2005 B	Δ = B − A, percentage points	(B-A)/A % change	1998–2000 C	2001–2005 D	Δ = D − C, percentage points	(D-C)/C % change
AMI mortality rate	0.17	0.12	-0.04***	-23.53%	0.16	0.13	-0.03***	-18.75%
Stroke mortality rate	0.16	0.12	-0.04***	-25.00%	0.14	0.12	-0.02***	-14.29%
All-cause mortality rate (emergency)	0.05	0.04	-0.01***	-20.00%	0.05	0.04	0.01***	-20.00%
Readmissi on rate (emergency)	0.10	0.12	0.02***	20.00%	0.10	0.11	0.01***	10.00%
Length of stay, days(emergency)	6.68	6.07	-0.61***	-9.13%	7.14	6.35	-0.79***	-11.06%
Waiting times, days (elective)	117.32	111.74	-5.58***	-4.76%	135.31	129.82	-5.49***	-4.06%
Total admissions(emergency and elective)	7.93	8.77	0.84***	10.59%	10.32	10.85	0.53***	5.14%
Patienthospital distance (km)	48.51	63.66	15.15***	31.23%	91.40	97.80	6.40***	7.00%

Notes: t-test level of significance: *** p<0.01, ** p<0.05, * p<0.1. Time period is 1998 to 2005. Predicted HHI is measured in 1998 for all admissions, calculated using predicted patient flows. Patient-hospital distance is the distance (in kilometres) from patient residence to chosen hospital, calculated using all admissions across the 64 hospitals included in the sample.



Notes: HHI averaged across 64 hospitals based on the 25,652 hospital-DRG observations in 1998-2005. The reform is implemented after the 12th quarter (vertical green line).

Fig. 2. Average HHI over time. *Notes:* HHI averaged across 64 hospitals based on the 25,652 hospital-DRG observations in 1998–2005. The reform is implemented after the 12th quarter (vertical green line). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

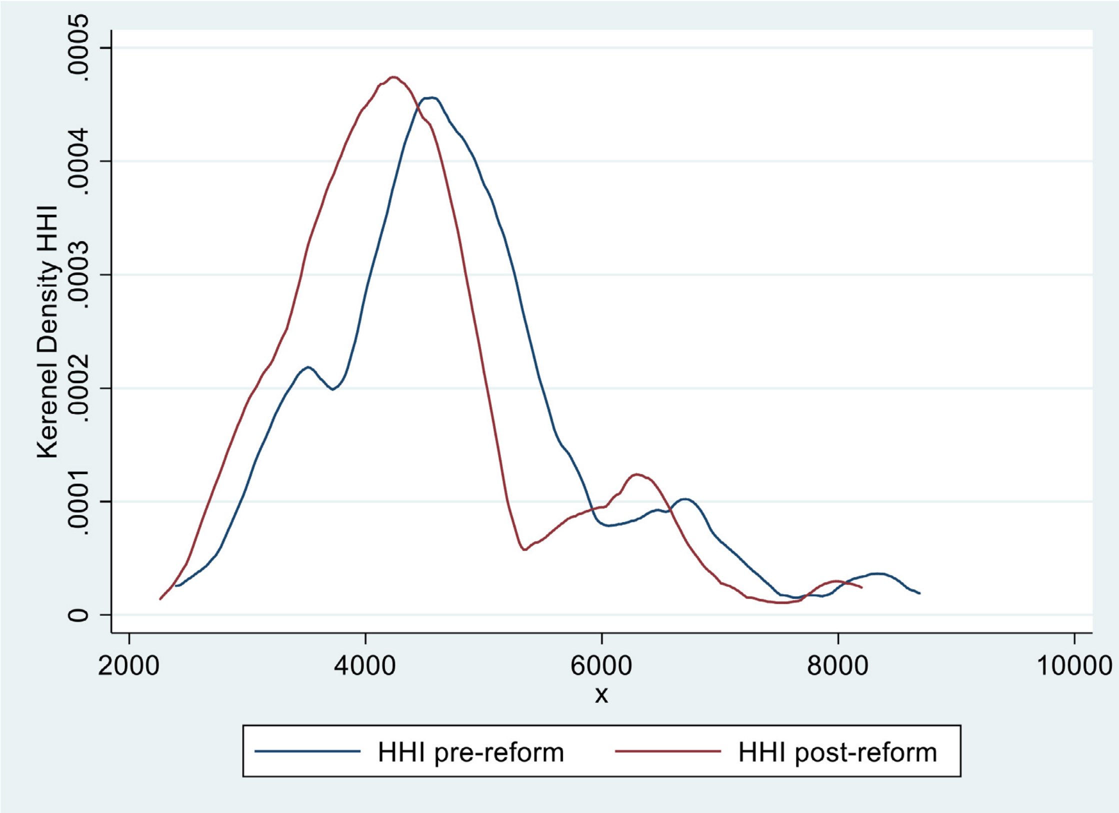
hospital care after the reform, and that the increase in average travel distance to the chosen hospital was substantially larger in more competitive areas (31%) than in less competitive areas (7%). This is consistent with the treatment intensity (i.e., exposure to competition due to the patient choice reform) being stronger (weaker) for hospitals with a lower (higher) HHI.

6.4. *Test for parallel trends assumption*

The validity of the DID identification strategy relies on the parallel trends assumption. More specifically, our empirical strategy relies on the assumption that the trend in outcomes for hospitals facing less competition represent a valid counterfactual for the hospitals that are exposed to a higher degree of competition, such that the effect of higher competition can be measured by this difference in differences. We can assess the plausibility of this assumption by evaluating whether hospitals facing different scope for competition, as measured by the pre-reform (predicted) HHI, have similar time trends in outcomes before the implementation of the patient choice reform.

In Fig. 4 we plot the trends in outcomes for the years 1998 to 2005. The four lines depict the (logged) average of the relevant outcome for hospitals in each predicted pre-policy HHI quartile. The blue line (numbered 1) is the lowest HHI quartile (most competitive markets) and the orange line (numbered 4) is the highest HHI quartile (least competitive markets). The vertical line indicates the introduction of patient choice (i.e., first quarter of 2001).

For the mortality outcomes the figures display, broadly speaking, a downward trend from poorer to better outcomes over the period across all HHI quartiles. For all-cause mortality, the pre-policy trends appear to be fairly parallel for all HHI quartiles, while the post-policy trends indicate a sharper reduction for the less competitive markets. For AMI and stroke mortality, the trends across the HHI quartiles are quite noisy and display no clear pre-policy or post-policy trends. Readmission rates have an upward sloping trend across all HHI quartiles over the period. The pre-policy trends are fairly parallel across the HHI quartiles, but the post-policy trends display no clear picture on the effects of competition. Length of stay has a downward trend for all HHIs over the whole period. Pre-policy the trends are fairly parallel, and the post-policy trends indicate a slightly sharper decline for hospitals in more competitive



Notes: Distribution of HHIs for 64 hospitals based on the 25,652 hospital-DRG observations split by pre-reform period 1998-2000 (blue line) and post-policy period 2001-2005 (red line).

Fig. 3. Kernel density of HHIs before and after the reform. Notes: Distribution of HHIs for 64 hospitals based on the 25,652 hospital-DRG observations split by pre-reform period 1998–2000 (blue line) and post-policy period 2001–2005 (red line). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

areas, though this trend flattens by end of period. Admissions and waiting times display fairly parallel pre-policy trends across the HHI quartiles. The post-policy trends for admissions display no sharp changes across hospitals in different HHI quartiles, whereas the post-policy trends for waiting time show a sharp decline but with some lag after the policy reform.

To further assess the plausibility of the parallel trends assumption, we use data from the pre-policy period (from 1998 to 2000) and regress our outcomes against period dummies and period dummies interacted with the treatment (competition) intensity variable, in addition to hospital-DRG fixed effects and the full set of covariates. More precisely, we estimate the following model:

$$Y_{hdt} = \gamma_{hd} + \lambda_t + (HHI_h * \lambda_t)' \boldsymbol{\delta}_t + X'_{hdt}\beta + \varepsilon_{hdt}, \tag{10}$$

where $t = 1, \ldots, 12$, and $\boldsymbol{\delta}_t$ is a vector of coefficients showing whether hospitals with different HHIs had a different time trend relative to the reference quarter given by the last quarter before the choice policy was introduced, i.e. quarter 4 in 2000. For the parallel trend assumption to be plausible, we expect the coefficients $\boldsymbol{\delta}_t$ not to be statistically significant from zero, both individually and jointly.

The results of the parallel trends test, which are reported in Table C1 in the Appendix, suggest that the parallel trends assumption is plausible for AMI mortality, stroke mortality, readmissions and length of stay. For each of these outcomes, none of the 11 coefficients associated with the HHI interacted with each quarter in the pre-choice period are statistically significant. The F-tests of joint significance are also rejected. For all-cause mortality, total admissions and waiting times, the results indicate that the parallel trends assumption is reasonably plausible. Few of the interactions are significant, and those that are tend to be only weakly significant, and the F-tests for joint significance are rejected. Thus, the results for our key outcome variables, which focus on emergency treatments only, do not appear to be driven by pre-policy trends.

As pointed out by Roos et al. (2020), who study the effects of price deregulation in the Dutch health care market on hospital quality, the parallel trends assumption is not necessarily sufficient when identifying differential effects of treatment intensities (more or less competitive markets). Referring to Fricke (2017), there is an additional requirement of homogeneity in outcomes across the different treatment groups. In particular, the policy effect (relative to no change) in less concentrated markets must be of the same sign but of greater magnitude than the effect in more concentrated markets. This additional requirement appears to be satisfied when inspecting Table 2 and Fig. 4. In Table 2 the relative changes in outcomes before and after the policy reform have the same sign

Fig. 4. Dependent variables (logged) by quartiles of HHI *Notes*. The sample is split according to quartiles of HHI, with 1 corresponding to less concentrated markets and 4 corresponding to more concentrated markets. The reform is implemented after the 12th quarter (vertical green line). The stroke and AMI mortality samples include 61 hospitals in 1998–2005. In each quarter, a hospital is included in the stroke mortality sample only if it has at least 3 stroke patients. In each quarter, a hospital is included in the AMI mortality sample only if it has at least 3 AMI patients. In the emergency samples, only patients admitted as an emergency are included. DRGs with less than 10 patients in a given year are dropped from the analysis. In each quarter, a hospital-DRG cell is dropped if the average length of stay is larger than 180 days. In the total admissions sample, both elective and emergency patients are included. DRGs with less than 10 patients in a given year are dropped from the analysis. In the waiting time sample only elective patients are included. DRGs with less than 10 patients in a given year are dropped from the analysis. In each quarter, a hospital-DRG cell is dropped if the average waiting time is larger than 730 days. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

for hospitals above and below the median HHI. The same appears to be the case across HHI quartiles in Fig. 4, though these raw descriptives are more noisy.

7. Results

This section reports the results from our empirical DiD analysis on the effects of exposing NHS hospitals to patient choice and thus (non-price) competition. The section is structured as follows. First, we present our main results, which are the effects on hospital quality (mortality and readmission) and cost efficiency (length of stay) for emergency patients. Second, we present an extension of our analysis to waiting times and total admissions. Third, we present a placebo test to validate the parallel trend assumption that the DiD analysis relies on. Finally, we present results on how the competitive effects of the choice reform is affected by exogenous variation in the regulated (DRG) prices. All the dependent variables and the (reform × HHI) variable are in logs, and therefore the key DiD estimate can be interpreted as an elasticity. The casemix variables are instead in natural units.

7.1. Hospital quality I: Mortality rates

Table 3 reports our DiD estimates of the impact of exposing NHS hospitals to competition on hospital quality as measured by (in-hospital) AMI, stroke, and all-cause mortality rates for emergency patients. The DiD variable is given by the interaction of the hospital-specific (predicted) HHIs and the post-policy period following the introduction of patient choice. The regression includes

Table 3
Baseline results.

AMI mortality rate	Stroke mortality rate (1)	All-cause mortality rate (emergency) (2)	Readmission rate (emergency) (3)	Length of stay (emergency) (4)	Total admissions (emergency & elective) (5)	(6)
Reform*HHI	0.542***	0.471	0.050*	-0.147***	0.037***	-0.041***
	(0.203)	(0.387)	(0.030)	(0.045)	(0.012)	(0.013)
Age	-5.048	1.767	-0.014***	-0.031***	0.006***	0.005***
	(10.500)	(6.408)	(0.002)	(0.004)	(0.002)	(0.001)
Age2	0.072	-0.027	0.001***	0.001***	0.001***	0.001***
	(0.146)	(0.088)	(0.001)	(0.001)	(0.001)	(0.001)
Age3	-0.001	0.001	-0.001***	-0.001***	-0.001***	-0.001***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Male	0.114	0.794	0.061***	0.033*	-0.088***	-0.023***
	(0.624)	(0.625)	(0.011)	(0.018)	(0.007)	(0.003)
Charlson index	0.599	-8.317	0.297***	0.814***	0.223***	0.246***
	(1.190)	(9.028)	(0.021)	(0.030)	(0.009)	(0.007)
Charlson index^2	-0.014	5.760	0.028**	-0.175***	-0.045***	-0.085***
	(0.347)	(5.122)	(0.011)	(0.015)	(0.004)	(0.003)
Charlson index^3	-0.012	-1.213	-0.006***	0.011	-0.003***	-0.006***
	(0.030)	(0.937)	(0.001)	(0.002)	(0.001)	(0.001)
Observations	1,752	1,765	382,176	382,176	382,176	449,451
R^2	0.088	0.053	0.017	0.015	0.050	0.060
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
N. of hospitals	61	61	-	-	-	-
Hospital dummies	Yes	Yes	-	-	-	-
Hospitals × DRGs	-	-	24,256	24,256	24,256	25,652
Hospital × DRG dummies	-	-	Yes	Yes	Yes	Yes

Notes: Time period is 1998 to 2005. Dependent variables and HHI are in logs. Reform is a dummy equal to 1 if year is 2001-05. The sample is an unbalanced panel of 64 hospitals (61 hospitals for AMI and stroke) over 32 quarters. In columns (1) and (2), hospitals with less than 3 AMI or stroke treatments per quarter are excluded. In columns (3)-(5), hospital-DRG cells are excluded if in a given quarter the hospital does not provide any emergency treatment in the DRG and if the average length of stay in the DRG is greater than 180 days. In column (6), hospital-DRG cells are excluded if in a given quarter the hospital does not provide any treatment in the DRG. In column (7), hospital-DRG cells are excluded if in a given quarter the hospital does not provide any elective treatment in the DRG and if the average waiting time in the DRG is greater than 730 days. Standard errors clustered at hospital level in parenthesis. *** p<0.01, ** p<0.5, * p<0.1.

controls for time trends (through 31 quarterly dummies), patient casemix, and hospital fixed effects. For all-cause mortality, hospital fixed effects vary also by DRG in line with the econometric specification described in (9) therefore controlling flexibly for (time-invariant) differences in patient casemix across different DRGs and allowing for the effect of casemix on quality to vary differentially across hospitals. We also control for time-varying casemix variables, through age, gender and a measure of patient severity (the Charlson index), and we allow for non-linear effects of age and severity with square and cube terms.

The first three columns in Table 3 present the estimates for AMI, stroke and all-cause mortality for emergency admissions. While the point estimates of the DiD coefficients are positive for all three mortality measures, suggesting that higher hospital concentration (less competition) increases mortality, the effect is statistically significant at 1% level only for AMI mortality, and at 10% level for all-cause mortality, while not significant for stroke mortality.[38]

The DiD coefficient for AMI mortality implies that a 10 percent reduction (i.e., a 495 points reduction from the mean) in the predicted HHI (more competition) is associated with a 5.42 percent reduction in the AMI mortality rate**.** The 95% confidence interval for our point estimate has a lower bound of 1.36 percent and an upper bound of 9.48 percent. A 5.42 percent decline in AMI mortality amounts to a reduction of 0.76 percentage points at the mean AMI mortality rate of 14 percent in the sample, which corresponds to almost 8 lives saved per 1000 AMI admissions. The DiD coefficient for all-cause mortality is much smaller. A 10 percent fall in the predicted HHI increases all-cause mortality by 0.5 percent, which implies a 0.02 percentage point reduction in the average all-cause mortality of 4.2 percent.[39]

The results are consistent with the findings from the English NHS. Both (Cooper et al., 2011) and (Gaynor et al., 2013) find a positive effect of hospital competition on AMI mortality. Gaynor et al. (2013) also report a small, positive effect on all-cause mortality, and Moscelli et al. (2018) find no effect on stroke mortality.

In terms of magnitude, the pro-competitive effect on AMI mortality appears to be higher in our study than in the studies from England. Gaynor et al. (2013) report a 2.91 percent drop in AMI mortality due to a 10 percent reduction in the HHI, which is about

[38] We cannot rule out that the non-significant result on stroke mortality is due to insufficient power. Compared to Gaynor et al. (2013), we have a smaller sample of hospitals but exploit more time variation, so the total number of observations is higher. An alternative would have been to use patient level data, as in Cooper et al. (2011) and Moscelli et al. (2018), but the latter find a non-significant effect of the English patient choice reform on stroke mortality, despite having a much higher number of observations.

[39] The results for AMI and stroke mortality show that age, gender and the Charlson index are not significant, suggesting that most of the variation in casemix is between hospitals (as captured by hospital fixed effects) rather than within hospitals in our sample for these two conditions.

half of the effect in our study. However, the market concentration is higher in the Norwegian NHS than in the English NHS, implying that a 10 percent change in the HHI yields a larger change in the competition intensity in our study than in Gaynor et al. (2013).[40] Adjusting for this brings the DiD coefficients closer in magnitude.[41] Moreover, Cooper et al. (2011) report a stronger effect on AMI mortality than Gaynor et al. (2013). They find that one standard deviation reduction in the (inverse of) the HHI results in 0.31 percentage point reduction in AMI mortality, while Gaynor et al. (2013) report a comparable measure of 0.21 percentage points. Thus, our pro-competitive effect on AMI mortality is comparable to the English studies, though stronger in magnitude.

Since we have a smaller sample of (61) hospitals than Gaynor et al. (2013) and Cooper et al. (2011), that have respectively 133 hospitals and 227 hospital sites, our point estimate for AMI (and stroke) mortality could potentially be less precise. However, we have a longer (five year) post-policy period than the English studies (respectively one year and two years) and make use of more time variation than Gaynor et al. (2013).[42] Thus, we believe that our study has sufficient power to obtain a credible estimate of the AMI mortality effect. In any case, the effects reported in both Cooper et al. (2011) and Gaynor et al. (2013) are within the 95% confidence interval [1.36, 9.48] of our point estimate.

According to our theoretical analysis, a positive relationship between hospital competition and quality suggest either that the NHS hospitals have a positive profit margin, i.e., that the regulated (DRG) price is higher than the marginal cost of treating an extra patient, or that the profit margin is negative but the hospitals' altruistic motivation to attract and treat patients is sufficiently strong to outweigh their financial incentives to avoid treating unprofitable patients.

7.2. Hospital quality II: Readmission rates

Column (4) in Table 3 reports the results on hospital readmission rates for emergency patients, using the same specification as for all-cause mortality. The DiD coefficient indicates a negative and statistically significant effect of market concentration on readmission rates, but the effect is relatively small in magnitude. The estimated coefficient imply that a 10 percent reduction in a hospital's predicted HHI (more competition) increases readmission rates for emergency patients by 1.47 percent, which corresponds to a 0.16 percentage point increase in the average readmission rate of 11.2 percent, or 16 additional emergency readmissions per 10,000 admitted patients.

Thus, we report mixed findings on the effect of competition on quality indicators. Exposing NHS hospitals to competition reduces mortality but increases the risk for an emergency readmission. One possible explanation for this result is that competition improves the survival rate and therefore increases the (average) severity of surviving patients, which, if not fully controlled for by patient casemix variables, may lead to higher readmission rates (as empirically shown by Laudicella et al., 2013; see Lisi et al., 2020 for a theoretical analysis). However, we have shown above that the effect on all-cause mortality is only marginally significant. Moreover, as a robustness check (presented in Table 11), we restrict the sample to DRGs which have a mortality rate which is less than 1 percent, and show that the effect of competition on readmission is qualitatively very similar with an elasticity of -0.141, which is statistically significant at 1% level.

7.3. Hospital cost efficiency: Length of stay

We also examine whether the pro-competition reform had any impact on hospital cost efficiency measured by length of stay of emergency patients. While cost efficiency is not directly affected by competition, hospitals' incentives to invest in cost reductions are indirectly affected through the effects on quality, as explained in the theoretical analysis in Section 3. In particular, if competition induces hospitals to improve quality, then this increases demand, which in turn reinforces the incentives to expend effort on cost reductions. Thus, quality and cost-containment incentives are complementary strategies. However, since we report mixed findings on quality, the corresponding effect on length of stay is not obvious.

Column (5) in Table 3 reports our DiD estimates of the effect of competition on length of stay, using the same specification and sample used for all-cause mortality and readmissions. The DiD coefficient is positive and significant. The estimate implies that a 10 percent reduction in the average HHI (more competition) reduces length of stay for emergency patients by 0.37 percent, which corresponds to a reduction in the average length of stay from 6.51 to 6.48 days. Thus, the magnitude of the effect is very modest as it is less than an hour in the duration of the patient stay.

For England, Gaynor et al. (2013) also find a positive effect of competition on length of stay, with a 10 percent fall in a hospital's HHI on average resulting in a 2.3 percent reduction in length of stay, which is a considerably larger reduction than the one we obtain for Norway. Cooper et al. (2012) also find a positive effect of competition on length of stay in public hospitals using a DiD strategy with private hospitals as a comparison group. Cooper et al. (2018) report mixed findings on length of stay in public hospitals that experience entry of private for-profit surgical centres in their catchment area. All these studies use length of stay for elective (non-acute) treatments, which potentially gives rise to a concern about patient selection issues. Thus, the added value of the our

[40] The mean (predicted) HHI is 4945 in our study and 4308 in Gaynor et al. (2013).

[41] Assuming a similar change in the HHI as in Gaynor et al. (2013), i.e., 430 point reduction, downscales our DiD coefficient for AMI mortality from 0.542 to 0.472.

[42] Gaynor et al. (2013) use a sample of 133 hospitals for the AMI mortality regression, but observe each hospital only twice (2003 and 2007), yielding a total of 251 observations. We have a sample of 61 hospitals that are observed in 12 quarters before and 20 quarters after the policy reform, yielding a total of 1752 observations.

study is that we focus on length of stay for emergency admissions and include DRG fixed effects to account for unobserved patient heterogeneity at treatment level.

One possible concern with a pro-competitive effect on length of stay is that patients are discharged 'quicker but sicker'. Shorter stays indicate improved cost efficiency but potentially also poorer quality. Taken together with the negative effect on readmissions, this claim may have some substance, in the sense that the effect of competition on readmissions might partly be explained by some patients being discharged too early. On the other hand, we also find that (AMI and all-cause) mortalities tend to be lower, which runs contrary to the 'quicker but sicker' hypothesis.

Another possible concern, similarly to the readmission variable, is selection through mortality, i.e., if the patient dies in hospital the length of stay is likely to be truncated and shorter. Again, we conduct a robustness check where we restrict the sample to DRGs which have an overall mortality which is less than 1 percent. In Table 11 in the next section, we show that the effect of competition on length of stay is qualitatively similar with an elasticity of 0.051, which is statistically significant at 1% level.

7.4. Total admissions and waiting times

We now extend our main analysis of the effects of hospital competition by considering two additional outcomes, namely waiting times and total admissions. Differently from the results presented so far, the sample includes all elective patients for waiting times, and all elective and emergency patients for total admissions, while the specification remains in line with (9).

Columns (6) and (7) in Table 3 show that hospitals in more competitive areas have a sharper reduction in waiting times and increase in total admissions than hospitals in less competitive areas. A 10 percent reduction in the HHI (more competition) increases admissions by 0.41 percent and reduces waiting times by 1.05 percent. Given that the average waiting time is 125 days, the competition effect implies a reduction of 1.3 days. For total admissions the effect is fairly small: the average number of total admissions is increased from 9.78 to 9.82 (per DRG and quarter). For England, Gaynor et al. (2013) find no effect of competition on total or elective-only admissions, but the DiD coefficients have the same sign. Propper et al. (2008), who study the internal market reform, find that competition reduced waiting times.

More admissions and shorter waiting times suggest a supply-side response to competition. Such a response could be due to hospitals being more effective in treating patients, as indicated by our finding that length of stay dropped, which in turn frees up beds and capacity to treat additional patients therefore increasing the supply.[43] Taken together, these findings suggest that the NHS hospitals facing more competitive pressure were able to utilise their resources in more efficient ways. Except for the case of readmissions, our findings also suggest that these supply-side responses benefited the patients due to shorter waiting times, shorter stays, and a higher chance of survival.

Although the inclusion of total admissions and waiting times as outcome variables allows us to make comparisons with Gaynor et al. (2013), Propper et al. (2008) and other studies of the English NHS, we should, however, stress that these two variables are fully (in case of waiting times) and partly (in case of total admissions) based on elective (non-acute) treatments and may be thus subject to patient selection, especially after the policy reform when choice is exercised. This implies that the results related to these two outcomes should be interpreted with caution. Thus, our key results are primarily related to the outcomes that are based solely on emergency (acute) treatments, i.e., mortality, readmissions, and length of stay.[44]

7.5. Variations in regulated (DRG) prices

In our final extension we exploit exogenous variations in the treatment (DRG) prices due to annual governmental changes in the relative shares of block grand and volume-based funding of the NHS hospitals. As explained in Section 4, the DRG share of total hospital funding is decided by the government on an annual basis and varies between 40 to 60 percent in the period we study. Fig. 5 plots the DRG shares for the years 1998 to 2005.

A reduction in the DRG share implies a proportional cut in the DRG price that is paid to the NHS hospitals. As shown in the theoretical analysis in Section 3, a higher DRG price increases (reduces) the scope for a positive (negative) effect of competition on hospital quality and cost efficiency. To empirically test if hospital responses to competition differ when the DRG price is higher, and in a way predicted by the theory, we interact our DiD coefficient with a dummy taking the value 1 for the years (2003 and 2005) with maximum DRG prices (i.e., 60 percent) in the post-reform period.

The results from this analysis are reported in Table 4. Interestingly, we find that the pro-competition effect on all-cause mortality is higher in the years when the DRG price was higher, which is line with our theoretical predictions, as the coefficient of the interaction variable with a DRG share of 60 percent is positive and statistically significant. A similar pattern is found for the effect of competition on length of stay. Moreover, we find that the adverse effect of competition on readmission rates is significantly attenuated in years with

[43] The results from the theoretical literature on hospital competition and waiting times are mixed. Brekke et al. (2008) study the effect of patient choice on waiting times in a static model and find that the effect is generally ambiguous. Sá et al. (2019) set up a model of dynamic hospital competition in which waiting times are indirectly determined by the hospitals' supply decisions and find that more patient choice leads to higher waiting times in the steady state.

[44] To address issues of multiple hypothesis testing, we can use the (conservative) standard Bonferroni correction of the form α/m, where m is the number of hypotheses. With a 10% threshold for statistical significance ($\alpha = 0.1$) and 7 outcome variables ($m = 7$), the corrected p-value for statistical significance is $\alpha_b = 0.1/7 = 0.014$. Using this more conservative threshold, AMI mortality, readmissions, length of stay, admissions and waiting times remain statistically different from zero.

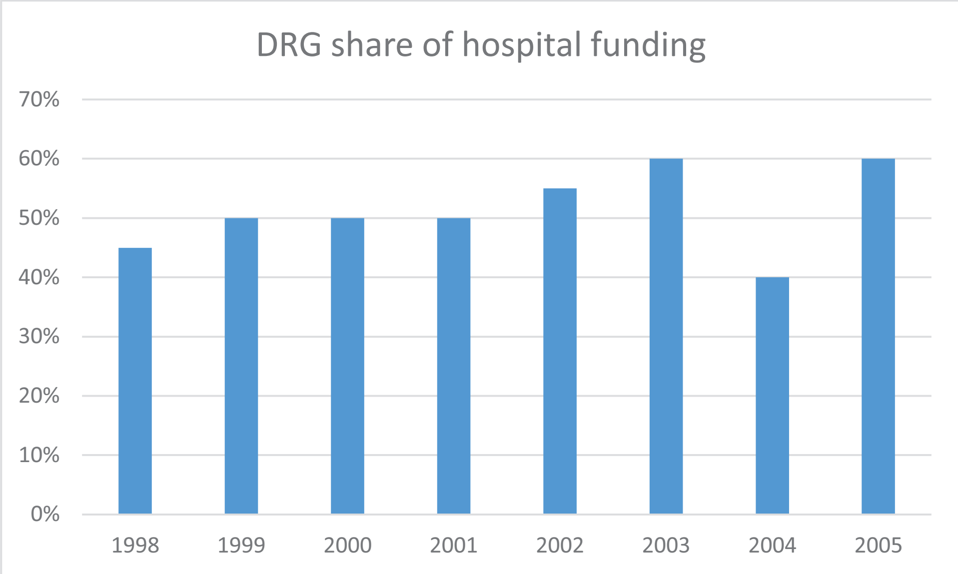


Fig. 5. Share of DRG-based funding relative to block grant funding for NHS hospitals, 1998–2005.

Table 4
Results. Differential effect across years when DRG share payment was 60%.

	AMI mortality (1)	Stroke mortality (2)	Mortality(emergency) (3)	Readmission (emergency) (4)	Length of stay (emer.) (5)	Admissions (6)	Waiting time (7)
Reform*HHI	0.340**	0.375	0.008	-0.210***	0.014	-0.047***	0.127***
	(0.160)	(0.372)	(0.031)	(0.047)	(0.013)	(0.013)	(0.031)
Reform*HHI* DRGshare60%	0.481	0.231	0.104***	0.154***	0.056***	0.014	-0.055*
	(0.340)	(0.356)	(0.028)	(0.046)	(0.013)	(0.009)	(0.028)
Observations	1,752	1,765	382,176	382,176	382,176	449,451	196,505
R2	0.089	0.054	0.017	0.015	0.051	0.060	0.035
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of hospitals	61	61	-	-	-	-	-
Hospital dummies	Yes	Yes	-	-	-	-	-
Hospitals × DRGs	-	-	24,256	24,256	24,256	25,652	18,793
Hospital × DRG dummies	-	-	Yes	Yes	Yes	Yes	Yes

a higher price. For the remaining two outcomes based on emergency admissions (AMI and stroke mortality), we find no significantly differential effects in the high-price years. The coefficients have the right sign, though, and the non-significant results could be due to fewer observations when splitting the post-policy period according to years with high and low DRG shares. The coefficient of the interaction variable with the 60 percent DRG share is positive, as we would expect from the theory, but not statistically significant. For the two secondary outcomes (including elective treatments), the signs of the coefficients are contrary to the predictions, but only weakly significant for waiting times. Thus, for our main outcomes (based on emergency admissions), the results give support to our theoretical predictions that higher DRG prices increase the scope for positive effects of competition on quality and cost efficiency, all else equal.

8. Robustness checks

To avoid inclusion of potentially endogenous variables, our estimates above control only for time-invariant factors at the hospital and treatment (DRG) level and simple measures of case mix. However, there may be omitted variables potentially associated with market concentration that are driving our results. To examine this we undertake a wide set of robustness checks and also test whether our results are sensitive to alternative specifications of our main model.

8.1. Alternative competition measures

In Table 5 we re-estimate our DiD model using different restrictions on the choice set of patients and alternative measures of competition. First, we allow for no restrictions or weaker restrictions (closest 20 hospitals rather than closest six hospitals) in the patient choice set when predicting the HHIs. The results, presented in the first and second rows of Table 5 show that, for AMI

Table 5
Robustness checks. Alternative competition measures.

Reform*C	AMI mortality (1)	Stroke mortality (2)	Mortality (emergency) (3)	Readmission (emergency) (4)	Length of stay (emer.) (5)	Admissions (6)	Waiting time (7)
C=HHI, no restriction on choice set	0.143	0.233	0.035**	-0.085***	0.056***	-0.040***	0.025*
	(0.101)	(0.180)	(0.014)	(0.021)	(0.005)	(0.006)	(0.013)
R^2	0.086	0.053	0.017	0.015	0.051	0.060	0.034
C=HHI, 20 closest hospitals choice set	0.221*	0.233	-0.072***	0.069***	-0.042***		
	(0.131)	(0.228)	(0.019)	(0.029)	(0.008)	(0.008)	(0.018)
R^2	0.086	0.053	0.017	0.015	0.051	0.060	0.035
C=(1/number of hospitals 100km radius)	0.118*	0.101	0.035***	-0.080***	0.034***	-0.034***	-0.001
	(0.067)	(0.135)	(0.010)	(0.016)	(0.004)	(0.004)	(0.010)
R^2	0.086	0.053	0.017	0.015	0.051	0.060	0.034
C=(1/number of hospitals 50km radius)	0.032	0.143	0.037**	-0.140***	0.061***	-0.039***	-0.005
(0.104)	(0.193)	(0.015)	(0.022)	(0.006)	(0.006)	(0.014)	
R^2	0.085	0.053	0.017	0.015	0.051	0.060	0.034
Observations	1,752	1,765	382,176	382,176	382,176	449,451	196,505
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of hospitals	61	61	-	-	-	-	-
Hospital dummies	Yes	Yes	-	-	-	-	-
Hospitals × DRGs	-	-	24,256	24,256	24,256	25,652	18,793
Hospital × DRG dummies	-	-	Yes	Yes	Yes	Yes	Yes

Notes: Time period is 1998 to 2005. Dependent variables and HHI are in logs. The sample is an unbalanced panel of 64 hospitals (61 hospitals for AMI and stroke) over 32 quarters. The reform is interacted with different measures of competition. The first two measures are HHI estimated using actual 1998 patient choices, including all hospitals and the 20 closest hospitals in the patients' choice set, respectively. The third and fourth measures are log(1/n) where n is the number of hospitals within a 100 and 50 km radius (including the hospital itself), respectively. In columns (1) and (2), hospitals with less than 3 AMI or stroke treatments per quarter are excluded. In columns (3)-(5), hospital-DRG cells are excluded if in a given quarter the hospital does not provide any emergency treatment in the DRG and if the average length of stay in the DRG is greater than 180 days. In column (6), hospital-DRG cells are excluded if in a given quarter the hospital does not provide any treatment in the DRG. In column (7), hospital-DRG cells are excluded if in a given quarter the hospital does not provide any elective treatment in the DRG and if the average waiting time in the DRG is greater than 730 days. Standard errors clustered at hospital level in parenthesis. *** p<0.01, ** p<0.5, * p<0.1.

mortality, the DiD coefficient is positive for both assumptions on the choice set, but only weakly significant when the choice set is restricted to the closest 20 hospitals and not significant when no restrictions on the choice set are imposed. The magnitude of the coefficients are also smaller than in our main analysis. The effect on all-cause mortality becomes more significant, but the coefficient is slightly smaller. The other outcomes remain robust and significant to this alternative definition of the choice set. However, the fit of the choice (logit) model is much higher in our preferred model where the choice set is limited to the six closest hospitals, as this covers more than 90 percent of the patient population. Given the large distances between hospitals in Norway, an unconstrained (or close to unconstrained) choice set is likely to yield poorer predictions due to outliers.[45]

Second, we re-estimate our main DiD model using the number of hospitals within a fixed radius (50 and 100 km) as an alternative to the more refined HHI measure.[46] The results, presented in rows 3 and 4 in Table 5, show that the effect on AMI mortality becomes less significant or not significant, but has a positive sign as in the baseline model. The magnitude of the DiD coefficients are also substantially smaller. The other outcomes, except for waiting times, remain robust and significant, as in the baseline model. The magnitude of the effects is also fairly similar to the baseline model. The use of a fixed radius is a poorer measure than the predicted HHIs, due to the fact that the market borders are exogenously and arbitrarily defined, which is likely to result in biased measures of competition. The simple count of hospitals is also a cruder and more imprecise measure of competition intensity, as it ignores asymmetries across hospitals that are reflected in their size or market share.

In summary, we conclude that our results from the baseline model tend to be robust with a few exceptions. AMI mortality is less significant and weaker in magnitude with the alternative competition measures. All-cause mortality tend to be more significant, whereas waiting time is mostly insignificant. However, we argue that these alternative measures of competition and choice sets constitute inferior measures of competition than the one used in our baseline model, as explained above. See Appendix B for details on the computation of the alternative competition measures.

8.2. Casemix test

Another possible concern is that our results are driven by patient selection rather than supply-side responses by hospitals to competition. In the main model we account for this by including hospital and treatment fixed effects and by focusing only on outcomes of emergency treatments. We also include time-varying patient characteristics, such as age, gender and the Charlson index. However,

[45] See Fig. 1 for the geographical distribution of hospitals in Norway and Table B1 for patient flow descriptives.

[46] To make the results comparable with our main results, we specify the competition variable as 1/(number of hospitals in a fixed radius), which corresponds to the HHI if the market shares of hospitals are identical.

there could be casemix effects associated with competition that may affect our results. For example, hospitals facing more competition may respond by avoiding sicker patients. Hospitals may also experience changes in casemix which differ systematically between more and less competitive areas if more severe patients are more likely to choose high-quality hospitals. The latter argument applies only to elective patients and is thus relevant only for waiting times and total admissions.

The results are reported in Table D1, where we estimate a similar specification as in (9) to test if competition is associated with changes in casemix. Reassuringly, we find that competition does not significantly correlate with patient casemix for emergency patients, except for age in the emergency sample where it is significant only at the 10% level (and for emergency and elective sample, which is mostly driven by the effect on the emergency sample). We also find that for elective patients less competition (higher HHI) is associated with more severe patients, as proxied by the Charlson index, which is contrary to our expectations that it is providers in more competitive areas that may have an incentive to avoid sicker patients.

8.3. Exclusion of Oslo region

It is possible that the competition effects are mostly driven by the Oslo region, which displays low concentration due to the large number of hospitals in close proximity to one another. It also is a high quality region, due to the presence of cutting edge hospitals. To test this, we omit all hospitals in the Oslo region and re-estimate the DiD model in (9). The results are reported in Table D2. The coefficient for AMI mortality is slightly stronger than in the main model. A 10 percent increase in the HHI results in a 5.93 percent reduction in AMI mortality when excluding hospitals in the Oslo region. However, the coefficient is less significant (but still significant at a 5 percent level). The coefficient for readmissions remains highly significant and slightly more negative, but length of stay, all-cause mortality and waiting time become insignificant (though recall that the effect on all-cause mortality was significant only at 5 percent level, and the effect on length of stay was quantitatively small in the main specification). Total admission is less significant, but remains significant at 5 percent level.

8.4. Other robustness checks

Finally, we perform three additional robustness checks to our main empirical model. First, we re-estimate our main model by weighting hospitals-DRG observations using the number of patients treated in the hospital-DRG in each quarter. This procedure implies that large hospitals and large treatment categories (DRGs) have a larger impact, preventing a large number of small categories to drive the results. The results, presented in Table D3 in the Appendix, show that the competition effects on AMI mortality and readmission rates remain highly significant and almost at the same magnitude as in the main model. All-cause mortality becomes more significant (at 5 percent level) and the coefficient is also larger compared to the main model. However, the effects on length of stay and waiting times lose their statistical significance.

Second, for the outcomes with several DRGs, we use information about the type of treatment and split the sample into medical and surgical treatments, that may differ in the fixed and marginal costs, and run separate regressions for the two groups. The results, presented in Table D4 in the Appendix, show that the competition effects on readmissions and length of stay are concentrated in medical treatments, and the magnitudes of the coefficients are almost identical to the main model. On the other hand, the effect on the number of admissions is concentrated in surgical treatments, whereas the waiting time effect is present for both types of treatment. The effect of competition on all-cause mortality is similar across treatments but not statistically significant most likely due to smaller samples.

Third, we re-estimate our main model using only DRGs with a mortality rate below 1 percent in order to avoid the possibility that length of stay and readmission rates might be biased due to selection effects through mortality. Since we find that competition tends to reduce (AMI and all-cause) mortality, this may in turn affect length of stay and readmission rates, so that the DiD estimates for these outcomes could be biased. To check this, we exclude all DRGs with a mortality rate above 1 percent. Table D5 in the Appendix shows that the results are robust, as both coefficients remain highly significant. The effect is almost exactly the same for readmission and slightly stronger for length of stay.

Finally, we re-estimate our main model and interact a linear time trend with DRG fixed effects to allow for possible relative price changes over the period. The results, presented in Table D6 in the Appendix, are very similar with those presented in Table 3 both in terms of the size of the coefficients and their statistical significance. The main difference is the effect on all-cause mortality, which as for other robustness checks, has a smaller (but still positive) coefficient but is not anymore significant at the 10% level.

9. Concluding remarks

We have studied the impact of exposing hospitals to (non-price) competition. Our empirical analysis exploits a policy reform that allowed for nationwide patient choice in the Norwegian NHS in 2001, replacing an administrative scheme where patients were allocated to the closest hospital within their county of residence. The reform facilitates a DiD research design due to plausibly exogenous variation in the scope for competition based on the geographical distribution of hospitals and patients. To capture this variation in market structure, we compute an HHI for each hospital based on (predicted) patient flows prior to the policy reform. Using a rich administrative data set with quarterly information over eight years from 1998 to 2005, we estimate the effects of competition on a wide set of outcomes, focusing mainly on emergency treatments to avoid patient selection issues.

The results show that hospitals in more competitive areas have a significantly sharper reduction in AMI mortality, all-cause mortality and length of stay than hospitals in less competitive areas after the reform. However, the results also show that competition increases readmissions, which indicate more complications, so the effects of competition on quality are mixed. In an extension, we find that exposing hospitals to competition tend to reduce waiting times and increase the number of admissions. However, these effects need to be interpreted with care due to patient selection issues related to elective (non-emergency) treatments. Finally, our results indicate that higher DRG prices tend to reinforce (weaken) the positive (negative) competition effects, which is in line with our theoretical predictions.

By way of conclusion, we would like to stress some limitations of our study. First, the competition effect on AMI mortality is highly significant and sizeable in magnitude in the main model. It also remains highly significant in a wide set of robustness checks. However, the AMI mortality effect is sensitive to alternative competition measures (i.e., number of hospitals in fixed radius and different choice set restrictions) both in terms of significance and magnitude. While we believe the main model provides the better measure of competition, we cannot rule out that the lack of robustness could be due to relatively small sample size for AMI mortality.[47] The same applies to the insignificant effect on stroke mortality.

Second, while our findings point in the direction that competition is welfare improving, we have not performed a full welfare analysis, which would require detailed information about hospital costs and other factors affecting patient utility, such as health benefits. Related to this point, choice and competition may involve equity issues if, say, patients from more affluent areas are more likely to travel and thus benefit from the reform.[48] While these are important issues, our data do not allow us to investigate these further.

Third, we have only data on in-hospital mortality rates, whereas other studies also have information on mortality rates after hospital discharge (usually 30 days). If hospitals discharge patients in a poorer state when being exposed to competition, improvements in-hospital mortality rates may be artificially driven by the fact that patients die after being discharged. Although our finding that competition leads to a higher rate of readmissions (complications) might suggest that this is a potential concern, it should be stressed that there are no explicit incentives for hospitals to manipulate their in-hospital mortality rates as a response to competition, since monitoring and rankings in Norway are based on post-hospital (30 days) discharge mortality rates. Furthermore, existing studies that have both in-hospital and post-hospital mortality rates tend to find similar (not opposite) effects of competition on the two measures.

Fourth, our distance measures are based on municipality of residence, and thus more crude than the ones used in the studies from England (defined neighbourhoods) and the US (zip-codes). However, the municipality structure in Norway is highly decentralised. During the analysis period, there were more than 450 municipalities of a population of around 4 million inhabitants, which means that municipalities are generally small in Norway. Moreover, the geographical distribution of patients and hospitals is such that the majority of patient flows are across rather than within municipalities.

Finally, the ownership reform, implemented one year after the patient choice reform, implied a corporatisation of the public NHS hospitals and a transfer of ownership from county to state level. While this reform does not in itself imply any competitive effects, the organisational changes of the public NHS hospitals could influence the competitive effects of introducing patient choice. This relationship is studied in the theoretical analysis through the effects of budget softness and profit constraints. For the empirical analysis, we could not identify whether the organisational changes to the public NHS hospitals reinforced or counteracted the competitive effects of patient choice. The two reforms were too close in time, and the organisational changes applied to all public hospitals at the same time, so any potential differential effects could not be examined. Thus, the effects of competition that we report in this paper do also include any additional (reinforcing or counteracting) effects due to the organisational reform of the public NHS hospitals. This is similar to the studies of the English NHS, e.g., Cooper et al. (2011) and Gaynor et al. (2013), which had a gradual roll-out of the fixed price regime and the introduction of the Foundation Trust model in the same period as the patient choice reform was introduced.

Author statement

As corresponding author, I confirm that all co-authors have contributed equally to the paper “Hospital Competition in the National Health Service: Evidence from a Patient Choice Reform” submitted to the Journal of Health Economics. The paper is written jointly with Chiara Canta, Luigi Siciliani and Odd Rune Straume. All authors have taken part in all parts of the paper, including the theoretical and empirical analysis.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jhealeco.2021.102509

References

Berta, P., Martini, G., Moscone, F., Vittadini, G., 2016. The association between asymmetric information, hospital competition and quality of healthcare: evidence from italy. Journal of Royal Statistical Society Series A 179, 907–926.
Bjorvatn, A., Ma, A., 2011. Patient mobility and public hospital waiting time. In Four essays on health care reforms in Norway. University of Bergen Phd dissertation.
Bloom, N., Propper, C., Seiler, S., Van Reenen, J., 2015. The impact of competition on management quality: evidence from public hospitals. Rev Econ Stud 82, 457–489.

[47] We have 61 hospitals in the AMI and stroke mortality estimations, whereas Gaynor et al. (2013) have 133 hospitals and (Cooper et al., 2011) have 227 hospitals. Note however that we have a longer time period than both studies. We also make use of much more time variation than Gaynor et al. (2013) where the effect on AMI mortality is based on 251 observations.

[48] For instance, the paper by Moscelli et al. (2018) shows that patient choice had a limited effect in explaining the social gradient in health care access, as measured by waiting times for coronary bypass.

Brekke, K.R., Siciliani, L., Straume, O.R., 2008. Competition and waiting times in hospital markets. J Public Econ 92, 1607–1628.
Brekke, K.R., Siciliani, L., Straume, O.R., 2010. Price and quality in spatial competition. Regional Science & Urban Economics 40, 471–480.
Brekke, K.R., Siciliani, L., Straume, O.R., 2011. Hospital competition and quality with regulated prices. Scandinavian Journal of Economics 113, 444–469.
Brekke, K.R., Siciliani, L., Straume, O.R., 2012. Quality competition with profit constraints. Journal of Economic Behavior & Organization 84, 642–659.
Brekke, K.R., Siciliani, L., Straume, O.R., 2015. Hospital competition with soft budgets. Scandinavian Journal of Economics 117, 1019–1048.
Brekke, K.R., Straume, O.R., 2017. Competition policy for health care provision in norway. Health Policy 121 (2), 134–140.
Chou, S.-Y., Deily, M.E., Suhui, L., Lu, Y., 2014. Competition and the impact of online hospital report cards. J Health Econ 34, 42–58.
Colla, C., Bynum, J., Austin, A., Skinner, J., 2016. Hospital competition, quality, and expenditures in the u.s. medicare population. Working Paper 22826, National Bureau of Economic Research.
Cooper, Z., Gibbons, S., Jones, S., McGuire, A., 2011. Does hospital competition save lives? evidence from the NHS patient choice reforms. Economic Journal 121, 228–260.
Cooper, Z., Gibbons, S., Jones, S., McGuire, A., 2012. Does competition improve public hospitals' efficiency? evidence from a quasi-experiment in the english national health service. CEP Discussion Papers 1125.
Cooper, Z., Gibbons, S., Skellern, M., 2018. Does competition from private surgical centres improve public hospitals' performance? evidence from the english national health service. J Public Econ 166, 63–80.
Feng, Y., Pistollato, M., Charlesworth, A., Devlin, N., Propper, C., Sussex, J., 2015. Association between market concentration of hospitals and patient health gain following hip replacement surgery. Journal of Health Services Research & Policy 20, 11–17.
Fricke, H., 2017. Identification based on difference-in-differrences approaches with multiple treatments. Oxf Bull Econ Stat 79, 426–433.
Gaynor, M., 2006. What do we know about competition and quality in health care markets? Foundations and Trends in Microeconomics 2 (6).
Gaynor, M., Ho, K., Town, R.J., 2015. The industrial organization of health care markets. J Econ Lit 53, 235–284.
Gaynor, M., Moreno-Serra, R., Propper, C., 2013. Death by market power: reform, competition and patient outcomes in the british national health service. American Economic Journal: Economic Policy 5, 134–166.
Gaynor, M., Town, R.J., 2011. Competition in Health Care Markets. In: Pauly, M., McGuire, T., Barros, P.P. (Eds.), Handbook of health economics, North-Holland chapter, Vol. 9, pp. 499–637.
Ghatak, M., Mueller, H., 2011. Thanks for nothing? not-for-profits and motivated agents. J Public Econ 95, 94–105.
Glaeser, E.L., Shleifer, A., 2001. Not-for-profits entrepreneurs. J Public Econ 81, 99–115.
Godager, G., Iversen, T., 2004. Hvem Bruker Retten Til Fritt Sykehusvalg? Resultater Fra Samordnet LevekåRsundersøKelse 2002. In: Økonomisk forum, 4–5, pp. 49–56. Økonomisk Forum, 4–5 (2004), pp. 49–56
Gowrisankaran, G., Town, R., 2003. Competition, payers, and hospital quality. Health Serv Res 38, 1403–1422.
Kessler, D., McClellan, M., 2000. Is hospital competition socially wasteful? Quarterly Journal of Economics 115, 577–615.
Kessler, D.P., Geppert, J.J., 2005. The effects of competition on variation in the quality and cost of medical care. Journal of Economics and Management Strategy 14, 575–589.
Laudicella, M., Li Donni, P., Smith, P.C., 2013. Hospital readmission rates: signal of failure or success? J Health Econ 32, 909–921.
Lisi, D., Siciliani, L., Straume, O.R., 2020. Hospital competition under pay-for-performance: quality, mortality and readmissions. Journal of Economics and Management Strategy 29, 289–314.
Moscelli, G., Gravelle, H., Siciliani, L., 2021. Hospital competition and quality for non-emergency patients in the english NHS. Rand Journal of Economics 52 (2), 382–414.
Moscelli, G., Gravelle, H., Siciliani, L., Santos, R., 2018. Heterogeneous effects of patient choice and hospital competition on mortality. Social Science and Medicine 216, 50–58.
Mukamel, D., Zwanziger, J., Tomaszewski, K.J., 2001. HMO Penetration, competition and risk-adjusted hospital mortality. Health Serv Res 36, 1019–1035.
Propper, C., Burgess, S., Gossage, D., 2008. Competition and quality: evidence from the NHS internal market 1991–9. Economic Journal 118, 138–170.
Propper, C., Burgess, S., Green, K., 2004. Does competition between hospitals improve the quality of care? hospital death rates and the NHS internal market. J Public Econ 88, 1247–1272.
Ringard, A., Hagen, T. P., Rico, A., 2006. Introducing patient choice introducing patient choice of hospital in national health systems—a comparison of the UK and norway. Working Paper, HORN—Health Organization Research Norway.
Roos, A.-F., O'Donnell, O., Schut, F.T., Van Doorslaer, E., Van Gestele, R., Varkevisser, M., 2020. Does price deregulation in a competitive hospital market damage quality? J Health Econ 72, 102328.
Sá, L., Siciliani, L., Straume, O.R., 2019. Dynamic hospital competition under rationing by waiting times. Journal of Health Economics 66, 260–282.
Shen, Y.S., 2003. The effect of financial pressure on the quality of care in hospitals. J Health Econ 22, 243–269.
Skellern, M., 2019. The effect of hospital competition on value-added indicators of elective surgery quality. LSE Working paper.