Does Price Deregulation in a Competitive Hospital Market Damage Quality?

Anne-Fleur Roos, Owen O'Donnell, Frederik T. Schut, Eddy Van Doorslaer, Raf Van Gestel & Marco Varkevisser*

January 2020

Prepared for the 18th Annual International Industrial Organization Conference

Abstract

Regulators are hesitant to permit price competition in healthcare markets because of its potential to damage quality. We assess whether this fear is well founded by examining a reform that permitted Dutch health insurers to freely negotiate prices with hospitals. Unlike previous research on hospital competition that has relied on quality indicators for urgent treatments, we take advantage of a plausible absence of selection bias to identify the effect on quality of elective procedures that should be more price responsive. Using data on all admissions to Dutch hospitals and a difference-in-differences comparison between more and less concentrated markets, we find no evidence that price deregulation in a competitive environment reduces quality measured by hip replacement readmission rates, despite the lack of information on this outcome.

JEL: I11, L14, L15

Keywords: Healthcare; hospital; competition; quality; contracting

Acknowledgements: The authors would like to thank Dutch Hospital Data and Statistics Netherlands for providing access to the data. We are grateful for comments from Michael Chernew (editor), two reviewers, Pieter Bakx, Werner Brouwer, Carol Propper, Ellen van Rooijen, Newel Salet, Ben Scharp, Anthony Scott, Matthew Skellern, Wynand van de Ven, and participants at various seminars and conferences.

* Roos (corresponding author): Netherlands Bureau of Economic Policy Analysis (CPB) & Erasmus School of Health Policy & Management (ESHPM), Erasmus University Rotterdam (EUR), a.f.roos@cpb.nl; O'Donnell: Erasmus School of Economics (ESE) & ESHPM, EUR, Tinbergen Institute (TI), odonnell@ese.eur.nl;

Schut: ESHPM, EUR, schut@eshpm.eur.nl; Van Doorslaer: ESHPM & ESE, EUR, TI, vandoorslaer@ese.eur.nl; Van Gestel: ESHPM & ESE, EUR, vangestel@ese.eur.nl; Varkevisser: ESHPM, EUR, varkevisser@eshpm.eur.nl.

1. Introduction

Competition between healthcare providers is increasingly encouraged with the aim of improving quality of care while slowing the growth of health spending. When prices are regulated, providers are forced to compete on quality to attract patients or contracts with insurers. When prices are unregulated, the effect of competition on quality is less clear. If demand is more responsive to price than to quality, then the optimal competitive strategy will involve driving down the price and sacrificing quality (Gaynor, 2006). This is a plausible scenario when information on quality is poor. Fear that competition with unregulated prices will be damaging to quality has caused regulators to be wary about allowing healthcare providers to compete on price. However, it is not clear whether this fear is well founded. Providers may not adopt the profit maximizing competitive strategies. Not-for-profit goals, a social mission and intrinsic motivation may lead them to maintain quality even if this means forgoing opportunities to gain a competitive advantage by cutting prices at the expense of quality. Whether quality suffers in competitive healthcare markets with unregulated prices is an empirical question. Evidence to answer it is sparse.

This paper examines the impact of price deregulation on the quality of hospital care delivered in the Dutch healthcare market in which insurers compete for customers and hospitals compete for contracts with insurers. We estimate the effect of moving from financing hospitals through prospective global budgets to allowing insurers and hospitals to freely negotiate procedure-specific prices in contracts for the delivery of DRG-type products. We identify the effect of this price deregulation by exploiting variation in its consequences across hospitals differentiated by the concentration of the market in which they operate. Assuming that free insurer-hospital negotiation of prices creates greater competitive pressure where the market is less concentrated, the difference-in-differences (DID) between more and less concentrated markets can identify a lower bound on the effect of deregulating prices in a more competitive environment.

We estimate the effect of permitting price competition on an elective procedure quality indicator – unplanned readmission after non-acute hip replacement. Higher readmission rates following hip replacement have been shown to be related to suboptimal quality (Rosen et al., 2013; Mokhtar et al., 2012). The institutional context and our empirical strategy facilitate identification without running much risk of the selection bias that most other studies of competition in healthcare markets have avoided only by estimating effects on indicators of the quality of urgent treatments, such as mortality after acute myocardial infarction (AMI) (e.g. Kessler & Geppert, 2005; Propper et al., 2008; Cooper et al., 2011; Mutter et al., 2011; Romano

& Balan, 2011; Gaynor et al., 2013).[1] This restriction of attention to urgent treatments leaves a dearth of evidence on the effect of competition on treatments, such as elective surgeries, that hospitals directly compete for and the demand for which potentially exhibits much greater responsiveness to price and quality (Bevan & Skellern, 2011; Gravelle et al., 2014; Colla et al., 2016; Skellern, 2019). The reform we exploit permitted price competition but left patient choice of provider effectively unconstrained. There was no available information on hip replacement readmission rates, and so patients could not select a hospital on the basis of this outcome. To identify the effect, we separate hospitals into two broad (treatment/comparison) groups according to the concentration of the market in which they operate. If there were any selection correlated with the outcome, it would most likely involve switching between neighboring hospitals that belong to the same group. This would not induce selection bias. Baseline patient (casemix) characteristics are similar across the treatment and comparison groups, changes in these characteristics do not differ between the groups and conditioning on these characteristics has little impact on the estimates.

We find no effect on quality despite examining a situation in which price deregulation had the greatest scope to damage quality – an elective procedure with little information on its quality, potentially leaving the demand of insurer-purchasers more responsive to price than to quality. Over a five-year period after price deregulation, the change in the 90-day hip replacement readmission rates of hospitals in less concentrated markets did not differ significantly from that of hospitals in more concentrated markets that were exposed to less competitive pressure. The insignificant point estimate is small in magnitude – less than 1 percent of the pre-reform readmission rate – and reasonably precisely estimated. Failure to reject the null of no effect is robust to alternative definitions of the market, to comparing hospitals at the extremes of market concentration, to using the 30-day (instead of 90-day) readmission rate, to dropping the most rural hospitals and to using readmission after knee replacement as the quality measure. In the year immediately after price deregulation, we find a marginally significant negative effect on the readmission rate, which is followed by insignificant positive point estimates in later years. This hints at a positive immediate impact on quality that is not sustained when hospitals continue to be exposed to competitive pressures

[1] The urgency of AMI treatment greatly reduces the risk of selection bias since patients are simply taken to the nearest hospital. There is little or no opportunity for difficult-to-treat patients selecting hospitals that deviate from the average in both quality and exposure to competition. And there is little scope for those hospitals to cherry pick the easier cases. However, this empirical strategy identifies the impact of competition on quality only in so far as the pressure to compete in the delivery of treatments that are price and/or quality responsive affects the general management of a hospital and this feeds through to treatments, such as AMI, that are largely shielded from competition.

arising from price deregulation. Overall, this study finds no evidence that price deregulation in a more competitive healthcare market damages quality, even when information on quality is poor.

2. Competition and healthcare quality with unregulated prices: theory and evidence

When prices are unregulated, the impact of competition on quality depends on how it affects the responsiveness of demand to quality relative to its responsiveness to price. If consumers, or insurers purchasing on their behalf, observe prices but have only imperfect information on quality, then competition might be expected to raise the price sensitivity relative to the quality sensitivity of demand, and so reduce quality (Kranton, 2003). Gaynor (2006) makes this argument using an amended version of the Dorfman-Steiner condition (Dorfman and Steiner, 1954): $z = \frac{p}{d} \cdot \frac{\varepsilon_z}{\varepsilon_p}$, where z is quality, p is price, d is the marginal cost of quality, ε_z is the elasticity of demand with respect to quality and ε_p is the elasticity with respect to price.[2] If competition exerts downward pressure on the price relative to the marginal cost and/or raises the magnitude of the price elasticity relative to the quality elasticity, then it will reduce quality (Gaynor et al., 2015).[3] However, if quality is sufficiently observable, then competition could conceivably raise the quality elasticity relative to the price elasticity. Quality would increase, provided price does not fall relative to the marginal cost of quality. The effect of competition on quality with an unregulated price is therefore ambiguous. It depends on characteristics of the market, the observability of quality and the objective functions of the demand-side and supply-side agents – insurers and hospitals respectively (Gaynor et al., 2015).

Evidence on the effect of competition on healthcare quality when prices are unregulated is scarce.[4] This is mainly because only a few countries permit free price negotiation in healthcare markets and data on the performance of private healthcare providers are typically not accessible.

[2] Although Dorfman and Steiner (1954) model a monopolist's behaviour, Dranove and Satterthwaite (2000) show that the model provides an approximation to the behaviour of an oligopolistic or monopolistically competitive firm if we think of the demand function as a reduced form. Hence, the model has relevance for imperfectly competitive healthcare markets (Gaynor, 2006; Gaynor et al., 2015).

[3] It is unlikely that hospitals deliberately set out to lower quality of care. Studies that investigate the competition-quality relationship often argue that in response to competitive pressure hospitals may cut services that affect quality outcomes (Propper et al., 2008; Bloom et al., 2015). Gaynor and Town (2012) show that, for the purpose of modeling, it does not matter whether hospitals are assumed to choose quality directly or indirectly through effort exerted.

[4] There is more evidence on the impact of competition on healthcare quality when prices are regulated. Findings are mixed. Some studies find that competition improves quality in this context (Kessler and McClellan, 2000; Kessler and Geppert, 2005; Cooper et al., 2011; Gaynor et al., 2013; Gaynor et al., 2016; Gobillon and Milcent, 2017) . Others find evidence of the contrary (Moscelli et al., 2019; Skellern, 2019), while one study finds no effect at all (Berta et al., 2016).

Using data from Southern California, Gowrisankaran and Town (2003) find that increased competition for Health Maintenance Organization (HMO) patients is correlated with reduced risk-adjusted hospital mortality for both pneumonia and AMI. Consistent with this, Sari (2002) finds that lower hospital market concentration in 16 US states is associated with fewer hospital complications. However, the internal validity of these studies can be doubted because of endogeneity problems (Gaynor and Town, 2012), and their external validity is limited because the HMO markets studied are very particular to the US hospital market in the 1990s.

The few studies that exploit a policy change to identify the quality effect of some form of price deregulation are stronger with respect to internal validity but also difficult to generalize because the findings are obtained in specific settings with a particular design of price competition. Subject to this caveat, these studies generally find that permitting greater price competition does damage hospital quality. Volpp et al. (2003) compare AMI mortality rates of New Jersey hospitals before and after the deregulation of prices in 1992 with those of New York hospitals where there was no deregulation. The mortality rate of uninsured AMI patients increased in New Jersey relative to New York. However, coincident to the reform in New Jersey and potentially confounding its effect, hospital prices were also pressured through rapid growth of large-volume buyers, such as HMOs, and there were large reductions in subsidies for hospital care of uninsured patients.

The switch from fixed budgets that hospitals received directly from the national government to contracts hospitals negotiated with purchasing organizations in the British National Health Service in 1991 has been used to estimate the quality effect of a highly regulated form of price competition (Propper et al., 2004; Propper et al., 2008). Contracts were written for blocks of services, including accident and emergency procedures and not for defined products, such as DRGs. Hospitals were mandated to set price equal to average cost, had to publish these prices and were not permitted to carry surpluses or losses across financial years. A limited degree of price competition was possible at the specialty level because of the difficulty of checking adherence to the pricing rule at that level (Propper et al., 2008). The evidence suggests that even this regulated form of price competition had a negative impact on quality (measured by AMI mortality rates), which is attributed to hospitals' incentives to compete on price rather than quality when the available information on the latter is poor (Propper et al., 2004; Propper et al., 2008).

If the highly regulated form of price competition permitted in the UK could damage quality, then one might anticipate that allowing hospitals to freely negotiate prices with purchasers would be seriously detrimental to the quality of care delivered in more competitive markets. The 2005 reform of the Dutch hospital market provides an opportunity to test this conjecture. Since there were no published outcome indicators of quality available to patients and health insurers before and after the reform, the risk of a negative impact on quality was substantial. Particularly in more competitive hospital markets, price deregulation may have raised the magnitude of the price elasticity relative to the quality elasticity and so may have reduced quality.

3. Price deregulation in the Dutch hospital care market

Comprehensive health insurance in the Netherlands with very limited cost sharing leaves patients insensitive to price and plausibly more concerned about quality.[5] However, as Dutch health insurers compete on the prices of the packages they offer, they are likely to be more sensitive to the prices of healthcare products than patients.[6] After price deregulation in 2005, insurers had an incentive to push prices lower in negotiations with hospitals and were possibly more concerned about price than quality. This was strengthened by the lack of outcome-based quality indicators – only a limited set of structure- and process-based indicators was available at the time. If better information on quality had been available, then insurers, presumably, would have been more responsive to it.

All Dutch hospitals are private nonprofit foundations facing a legally binding non-distribution constraint prohibiting them from distributing any net earnings. Before 2005, the hospitals were financed by a prospective budgeting system with regulated per diem rates that produced relatively stable revenue flows known at the beginning of each year. From 2005, hospitals' revenues became contingent on contracts secured with individual health insurers. At that time, there were five health insurance companies, plus a joint purchasing cooperative of smaller insurers.[7] Contracts are written for products defined by a purposively developed product

[5] In the specific context of our study, prices of hip replacements exceed the deductible, which is the only form of cost-sharing. For this procedure, out-of-pocket costs to patients are, therefore, invariant to the prices charged by hospitals.
[6] The scenario we examine is similar to that captured by a two-stage model in which insurers (Managed Care Organizations) and hospitals first negotiate prices, and then patients, who are exposed to little cost sharing, select a hospital (Capps et al., 2003; Town and Vistness. 2001; Gowrisankaran et al., 2015). The estimates Gowrisankaran et al. (2015) obtain from such a model imply that the insurers are much more price sensitive than the insured patients, but the insurers are less price sensitive than patients would be if they were uninsured.
[7] The four largest companies account for 90 percent of the market. Market concentration by region is often even higher, which is due to the fact that these companies typically evolved from former regional sickness funds (Halbersma et al., 2010).

classification system – Diagnosis and Treatment Combinations (DTCs) – akin to DRGs.[8] These products bear no relation to the output parameters of the pre-reform hospital budgets (e.g. number of admissions and hospital days) and the product-specific prices are not related to the regulated per diem rates that were used to set these budgets. For all products, hospitals and insurers are permitted to negotiate over volume while taking account of quality, although in the period we study the attention that could be paid to quality was hampered by the lack of outcome indicators.[9] For some products, free insurer-hospital negotiation of prices was allowed. In 2005, this was permitted for a subset of products that accounted for about 10% of hospital revenues. This subset included non-acute hip replacements – the procedure we focus on. Over time, the number of products for which free price setting is permitted has increased. An extension in 2008 took the share of hospital revenues obtained from products with negotiated prices to 20%. The fraction was further increased to 34% in 2009 and 70% in 2012.

After permitting price competition, the price elasticity of the insurers' demand is greater (in magnitude) in less concentrated, more competitive markets, and prices could be pushed down further in these markets. According to the Dorfman-Steiner condition, quality would then suffer in more competitive markets unless there was a sufficient countervailing increase in the quality elasticity. This increase would occur only if quality was sufficiently observable such that insurers could monitor it and the new contracting arrangements gave them greater motivation and scope to pressure hospitals for quality improvements.[10]

Because of the high overall number of products (DTCs), insurers and hospitals often negotiate over clusters of products. This was not the case for high-volume products like non-emergency hip replacements during the period covered by this study.[11] At that time, contracts, including those agreed after negotiation over prices, were written for a period of one year. The goal of the contracting reform was to make insurers, acting as purchasing agents for their

---

[8] The DTC system is more comprehensive than DRGs. It includes outpatient consultations and the remuneration of medical specialists. There were about 29,000 DTCs in the period we examine.

[9] During the period covered by our analysis, almost all contractual agreements that included quality improvement initiatives were framed in terms of structure and process indicators rather than outcome indicators (Schut and Van de Ven, 2011).

[10] Note that, at the time of the contracting reform, medical specialists were paid fee-for-service at regulated product-specific remuneration rates determined by the predicted time required for each procedure and a fixed payment per hour. While specialists, like hospitals, had a financial interest in attracting more patients, their compensation was, therefore, not directly affected by the outcome of any price negotiation. The quality of treatment delivered by the specialists could be affected indirectly through the volume of procedures – negotiated simultaneous to price – they were called upon to perform. Quality – specifically, the readmission rates we examine – could also have been affected by price negotiation through consequent pressures on non-specialist medical staff, attention paid to after care and hospital purchasing of non-manpower inputs.

[11] This has been confirmed by the authors through interviews with representatives of insurers and hospitals who were involved in contract negotiations during the period covered by this study.

customers, more responsive to price, volume and quality. Insurers were allowed to contract hospitals selectively, giving them leverage to negotiate lower prices and, possibly, also to obtain better quality to the extent that this could be specified by the limited quality indicators available at the time. In a competitive insurance market, lower prices would feed through to lower premiums (Ho, 2009). In this respect, an important complement to the 2005 insurer-hospital contracting reform was a 2006 reform of the health insurance market intended to increase price competition among insurers by mandating citizens to purchase a basic health insurance package from private insurers (Schut and Van de Ven, 2011). The logic of these twin reforms is that – as in the two-stage model of managed competition (Capps et al., 2003; Town and Vistness. 2001; Gowrisankaran et al., 2015) – insurers would compete for customers on premiums, as well as the scope and quality of their provider networks, while hospitals would compete on price and quality for inclusion in those networks. In fact, prior to 2010 – the last year covered by our analysis – there was very little implementation of selective contracting. By that year, only one insurer had entered into selective contracts and these did not cover hip replacements (Schut and Van de Ven 2011). Since 2010, the number of insurers contracting with restricted provider networks has increased.[12] But during the post-reform period we study (2006-2010), insurers relied on the threat of selective contracting, rather than its implementation, to give them bargaining power in negotiations with hospitals. Some insurers tried to steer their customers to hospitals identified on their websites as 'preferred providers' that were claimed to offer good quality care, although choice was not restricted to these hospitals. Some insurers waived the deductible if the insured sought treatment at a preferred provider. The available evidence suggests that these soft channeling policies had little influence on patient choice during the period we study (Van der Geest and Varkevisser, 2016; 2019), and we are not aware of any attempt to use them to specifically target hip replacement patients. Additionally, a survey of patients who had non-emergency hospital care in the period 2008-10 revealed that only 7 percent had searched for quality information (Van der Geest and Varkevisser, 2012). Hence, the reform left patients' choice of hospital (for hip replacements and other procedures) effectively unfettered during the period studied.

The complexity of healthcare and its stochastic relationship with health outcomes makes measurement of its quality inherently difficult. In 2008, the Dutch Health Care Inspectorate launched an initiative with the ambition of developing a comprehensive set of quality indicators

[12] This is often done by stipulating minimum volume standards, but only for a limited set of treatments, such as complex cancer surgery, that does not include hip replacements.

for 10 procedures, including hip replacement. But this was confined to a minority of 33 hospitals and plans to extend it were not implemented because of lack of cooperation from hospitals and failure to agree on a standardized set of quality indicators (Schut and Van de Ven, 2011). After this failed attempt at self regulation, only in 2013 did the government establish a Quality Institute to disseminate uniform quality indicators and define legally enforced standards. Hence, up to the end of our period of analysis in 2010, no meaningful, comparable information about the quality of hip replacements, such as hospital-specific readmission rates, was available. This would be expected to result in hospitals exposed to greater competitive pressure shifting effort from maintaining poorly observed quality to cutting costs in order to become more price competitive once prices were deregulated (Propper et al., 2008). On the other hand, the new contracts involved hospitals and insurers negotiating for the first time over the delivery of specific procedures akin to DRGs. Hospitals exposed to more competition might have expended greater effort on ensuring that they were not penalized for poor performance on the available proxies for quality taken into account in future contract negotiations. If this motivation was sufficiently strong, then exposure to greater competitive pressure after the contracting reform could even have raised quality.

Existing evidence on the market response to the 2005 contracting reform is limited. Qualitative analyses conclude that price rather than quality has been the primary focus of contract negotiations (Meijer et al., 2010; Ruwaard et al., 2014; Schut and Van de Ven, 2011). This is unsurprising given the dearth of information available on quality. The Dutch Healthcare Authority reports that prices of products (DTCs) that were in the free-pricing segment from 2005 declined in real terms and relative to the regulated prices of other products up to 2008 (NZa, 2009). Between 2006 and 2009, nominal prices increased by 2.7%, on average, in the regulated segment of products compared with an increase of only 1.2% in the free-pricing segment (Schut and Van de Ven, 2011). There is no evidence that hospitals offset lower price increases by increasing service volume in the free-pricing segment (Krabbe-Alkemade et al., 2017; Schut and Van de Ven, 2011). Krabbe-Alkemade et al. (2017) found that permitting price competition led to lower total hospital costs.

The effect of the price deregulation brought about by the 2005 reform on hospital quality has not previously been estimated. A few studies look at the relationship between price and quality variation or between hospital concentration and quality after prices were liberalized. Heijink et al. (2013) find only limited variation in hospital quality and no relationship between contract prices and quality for cataract treatment. Croes et al. (2017) find a negative relationship between hospital market share and quality scores for two of the three diagnostic groups studied. Bijlsma et al. (2013) find that hospital concentration is associated with various process indicators of quality, but both positive and negative relationships are found and there is no relationship between hospital concentration and any of a number of outcome indicators examined. None of these studies have a design capable of identifying the causal effect on quality of price deregulation.

4. Data and measures

4.1 *Sources*

We use comprehensive, hospital-level data from the National Medical Registry on patient discharges from all Dutch hospitals for the years 2001-2010. For each discharge, we observe the patient's gender, age, zip code, primary/secondary/tertiary diagnoses (ICD-9CM), admission period, the admission hospital code (but not its name) and procedures carried out. Procedures are classified according to a method that is based on, and for the procedures examined equivalent to, the International Classification of Procedures in Medicine (WHO-FIC, 2017). For most of the analyses, we restrict attention to patients discharged after a non-acute hip replacement (see below for details of the selection criteria).[13] As a robustness test, we examine patients discharged after non-acute knee replacement. We construct a hospital-level panel which includes information on quality of care and patient case mix, and supplement this with an index of socioeconomic status that is averaged over all the non-acute hip replacement patients of a hospital in a given year. This index is constructed by the Netherlands Institute for Social Research from the education, income and labor market status of residents of a zip code area (SCP, 2017).

4.2 *Quality measures*

We use the unplanned 90-day readmission rate following non-acute hip replacement as our main quality indicator. This is preferred to the post hip replacement mortality rate because the latter was very low in the period studied.[14] We restrict attention to all unplanned readmissions, including emergency readmissions, because planned readmissions (e.g. for a scheduled procedure) are not generally a signal of quality of care. Higher unplanned readmission rates

[13] Also sometimes referred to as planned hip replacements or elective hip replacements. Hence, these do not include hip fractures or acute hip replacements.
[14] Using the Causes of Death Register provided by Dutch Hospital Data and Statistics Netherlands, we calculate a within hospital mortality rate of 0.25 percent and a 30-day mortality rate of 0.30 percent following non-acute hip replacement in the period 2001-2010.

have been shown to be related to suboptimal quality of treatment generally (e.g. Mokhtar et al., 2012; Rosen et al., 2013) and specifically for hip replacements (e.g. Clement et al., 2013; Avram et al., 2014; Saucedo et al., 2014; Kurtz et al., 2016). All unplanned readmissions are attributed to the original treatment hospital using the anonymized hospital codes.

Unplanned readmissions following joint replacement are determined, in part, by the quality and safety of the initial hospital stay, transitional care services and post discharge support (Friebel et al., 2017). Widespread belief that readmissions are indicative of poor quality treatment is reflected in the fact that financial penalties for excess readmissions (including for hip replacements) have been imposed on hospitals in both the US and the UK since 2012 (Joynt and Jha, 2012). Consistent with this, in our data, four of the top five reasons (identified from diagnostic codes) for hip replacement patients to be readmitted within 90-days are related to complications, infections or inflammatory reactions due to prosthetic implants.[15] There is no consensus on whether a 90-day or 30-day follow-up window to define orthopedic readmissions provides the better indicator of quality (Ramkumar et al., 2015). Since the two are highly correlated for hip replacements in our data (r=0.85, p<0.01 at the start of our study period in 2001), it should make little difference which is used. Complications are also the main reasons to be readmitted within 30-days. We examine robustness to using 30-day readmissions. Information on hip replacement readmission rates was not in the public domain or available to health insurers during the period of analysis, and so this indicator is unlikely to have been subject to manipulation by hospitals.

4.3 *Sample selection*

We have a balanced panel of 103 hospitals observed from 2001 to 2010, yielding 1,030 hospital-year observations. Patient level sample inclusion and exclusion criteria are based on those defined in the technical specifications of the US Agency for Healthcare Research and Quality (AHRQ) Inpatient Quality Indicator #14 (AHRQ QI Version 5.0; IQI #14), which measures the hip replacement mortality rate. The total population includes all discharged patients aged 18 or older in any year between 2001 and 2010 with any procedure code that indicates partial or full hip replacement *and* any principal diagnosis code that indicates osteoarthrosis of the pelvic region or thigh, and for which all necessary information was present (n=153,208).[16] For our analysis, we first drop those with any listed diagnosis codes indicating hip fracture and those

[15] Consequently, it is unlikely that restricting attention to unplanned readmissions judged (by some criteria) to be related to the index hip replacement admission would have much affect on our estimates.
[16] See Appendix 1 for the relevant procedure codes and ICD-9CM diagnosis codes.

with codes indicating pregnancy, childbirth or puerperium (n=793). We then also exclude those who transfer to another hospital (n=264) because it is impossible to determine whether readmission in such cases indicates sub-optimal quality of the treatment received in the first or the second hospital. Patients who died in the hospital (n=85) are also dropped.[17] After imposing all these exclusion restrictions, we are left with 152,066 (99.3% of) discharges following non-acute hip replacement during our period of analysis. Of these, 8.0 percent were readmitted to a hospital within 90 days for any reason that was not planned.[18]

4.4 *Measures of hospital market structure*

We measure concentration at the hospital level using the Herfindahl-Hirschman Index (HHI) based on the number of hospital beds: $HHI_h=\sum_{i=1}^{N_h} m_i^2$, where m_i is the percent market share of hospital i that lies within a fixed radius of hospital h and N_h is the total number of hospitals in that market. Some hospitals have multiple locations that do not all lie within the same market defined by distance.[19] Appendix 2 explains how we calculate the HHI in these cases. For our baseline analysis, we use a 30 kilometers (by road) fixed radius because patients travel, on average, for 20 minutes to get to the hospital of their choice (Varkevisser et al., 2010; Varkevisser et al., 2012; Beukers et al., 2014) and most Dutch hospitals do hip replacements (Roos et al., 2019). But since variation around the mean travel time is high (Varkevisser et al., 2010; Varkevisser et al., 2012; Beukers et al., 2014), we examine sensitivity to fixing the radius at 20, 40 and 50 kilometers to define the market.

To protect privacy, hospitals are anonymized in the patient-level data and we are not allowed to attach a continuous measure of HHI to a patient record since this could reveal the hospital used. The HHI of each hospital was therefore constructed in a database not containing patient-level data. Next, an indicator of whether the HHI of each hospital is under 2500 was derived – to determine location in a less concentrated market – and this was then linked to the patient-level dataset using the hospital identifier code by Statistics Netherlands. There is no objective HHI threshold for defining a competitive market. Our choice of 2500, which corresponds to a market comprising four equally sized hospitals, is based on the US Federal Trade Commission

[17] Given the very low within hospital mortality rate following non-acute hip replacement, any selection bias arising from excluding those who die is likely to have a negligible impact on the estimates (Laudicella et al., 2013; Fischer et al., 2014).
[18] As shown by Figure A2 in the Appendix, total volumes aggregated over the two groups dropped in 2006 and then slightly increased again. The difference in volumes between the treatment and comparison groups is relatively stable over time.
[19] Of the 103 hospitals, 4 had more than one location within the period that we study.

(2010) horizontal merger guidelines.[20] Given the greater competitive pressure on hospitals operating in less concentrated markets approximated by HHI<2500, it is hypothesized that they would be affected more by the 2005 reform that permitted competition through free negotiation of prices than were hospitals operating in more concentrated markets (i.e. with HHI≥2500).[21] Over the period 2001-2010, there is a nearly even split between hospitals with HHI≥2500 (n=52) and hospitals with HHI<2500 (n=51).

5. Empirical strategy

Identification of the quality effect of permitting competition through free negotiation of price is difficult given that all hospitals are exposed to price deregulation after 2005. This problem of identifying from an across-the-board policy change often arises in empirical research on competition in healthcare markets. The standard solution is to compare before-and-after (policy) changes in outcomes that occur in less concentrated markets with the respective changes in more concentrated markets (e.g. Propper et al., 2008; Cooper et al., 2011; Gaynor et al., 2013). While this difference-in-differences (DID) strategy does not identify the effect of the policy *per se*, it may be thought capable of identifying the differential effect of the greater competition generated by the policy in less concentrated markets compared with the weaker competition induced in more concentrated markets, provided the outcomes of hospitals operating in more and in less concentrated markets would have followed common trends if the policy had not been implemented. Since we are ultimately interested in the effect of different degrees of competition, this differential effect is of substantive interest.

Fricke (2017) points out that the stated common trends assumption is insufficient to identify the differential effect of treatment intensities. If the assumption does hold, then the differential effect is identified only under the further assumption of homogeneity in the effect of marginal changes in treatment across, in this context, more and less concentrated markets. This homogeneity assumption is likely to be implausible given that hospitals are not randomly assigned to markets. Applying Fricke's logic to the situation studied here, what can plausibly be identified is a lower bound (in magnitude) on the effect of price deregulation in less

[20] The European Commission (2004) guidelines on the assessement of horizontal mergers do not include an HHI-based market classification. They mention a slightly lower threshold (HHI>2000) as an initial indicator of a lack of competition.
[21] As a check for further heterogeneity in hospitals' responses to price deregulation, a finer categorization of hospitals by HHI is also used.

concentrated markets compared with the continuation of price regulation in those markets.[22] Two assumptions are necessary to obtain this partial identification. First, there must be common trends across more and less concentrated markets under the counterfactual of no policy change. Second, the policy effect (relative to no change) in less concentrated markets must be the same sign but of greater magnitude than the effect in more concentrated markets.[23] Intuitively, if price deregulation has any effect in more concentrated markets, then taking the DID between more and less concentrated markets cannot point identify the effect in the latter. But it will give a lower bound on the effect in the less concentrated markets provided that the effect is greater in magnitude but has the same sign as the effect in more concentrated markets (and common trends hold).

Hospitals with an HHI below 2500 form our treatment group, while those with an HHI of at least 2500 belong to the comparison group. Only hospitals with an HHI either always below 2500 or always above 2500 during the 2001-2010 period are used in the analysis. Hence, no hospital can switch from the treatment group to the comparison group or vice versa, and the composition of each group is held constant by construction. Twenty hospitals out of a total of 103 are excluded because they fail to meet this criterion. This is mainly because of merger activity between 2001 and 2010. Of the remaining 82 hospitals, 43 have HHI<2500 and are in the treatment group.

In our main analysis, we use data from 2001 to 2004 to capture the period before price deregulation and data from 2006 to 2010 for the post-reform period. We exclude data from 2005, as the policy was implemented on February 1 of that year.

We estimate the following fixed effects model by least squares:

$$RR_{ht} = \alpha + \delta 1(HHI_h < 2500) \times POST_t + \boldsymbol{X}_{ht}\boldsymbol{\mu} + u_h + \lambda_t + \varepsilon_{ht} \tag{1}$$

where RR_{ht} is the unplanned 90-day readmission rate (percent) for non-acute hip replacements at hospital h in year t, $1()$ is the indicator function, $POST_t$ is a binary indicator equal to 1 for

[22] Fricke (2017) considers identification of the effects of two (or more) distinct treatments using a DID strategy. The treatments can be distinguished by degree of intensity. To apply the setup to this study, one must think of the treatments not as price deregulation, but rather as the intensity of competition arising from this policy. We are interested in the quality effect of more intense competition and identify this by (effectively) interacting a single policy change (price deregulation) with market concentration to obtain variation in competition (treatment) intensity.

[23] That is, monotonicity in treatment intensity, where treatment is the competitive pressure induced by the price deregulation.

the post-reform period (2006 - 2010), $\boldsymbol{X}_{ht}$ is a vector of hospital characteristics that vary over time but are plausibly not affected by price deregulation, u_h is a hospital fixed effect, λ_t is a year effect and ε_{ht} is a random error term. The covariates consist of the Charlson index of comorbidity (Quan et al., 2011; Quan et al., 2005) averaged over a hospital's non-acute hip replacement patients in a year, the percentage of these patients aged 65+, 40-60 and 18-39 years, the percentage female, the percentage discharged to a skilled nursing facility,[24] and the mean zip code-specific socioeconomic score of the patients. These indicators of case mix are included to increase efficiency and to allow for any change in the composition of hip replacement patients that differs between hospitals in less and more concentrated markets without being caused by the differential effect of price deregulation. We have argued that there is little or no reason to expect the reform to have caused hip replacement patients to select different hospitals or hospitals to have selected different patients, and we return to this point at the end of this section.

Table 1 presents means of the covariates before and after the reform for the treatment and comparison groups. Prior to the reform, there are some significant differences in the characteristics of the patients across the two groups. But the differences are rather small. Significance reflects the large sample size. The treatment group has a slightly higher proportion of females, its patients are about 1 year older and they have a higher socioeconomic status as well as a greater propensity to be admitted to a skilled nursing facility after discharge, on average. There are no pre-reform differences in comorbidity measured by the Charlson index. The characteristics of the patients change relatively little between the two periods for both groups. None of the difference-in-differences of these characteristics are significantly different from zero, indicating that there was no differential change in the composition of the groups with respect to these observables.

Conditional on the covariates ($\boldsymbol{X}_{ht}$), if in the absence of price deregulation in 2005 the average readmission rate of hospitals in less concentrated markets would have changed in 2006-10 by as much as the change that would have occurred in hospitals operating in more concentrated markets and if permitting price competition had a larger effect in less concentrated markets, then the parameter δ in (1) is a lower bound (in magnitude) on the average effect of the price deregulation in a more competitive market environment on the readmission rate among the hospitals in the less concentrated markets.

[24] We assigned patients reported to be discharged to 'other healthcare organization' to a nursing home discharge since a recent (unpublished) Statistics Netherlands study reports that around 70% of these patients are transferred to a nursing home facility.

Figure 1 supports the plausibility of the common trends assumption. Between 2001 and 2005, the trend in the readmission rate, and indeed its level, is very similar for hospitals operating in more and less concentrated markets. Estimation of a model similar to (1) using data from 2001 to 2004 only and allowing the year effects to differ between hospitals located in more (HHI≥2500) and less concentrated markets reveals no evidence of differential trends in the period immediately preceding the reform (Appendix 3, Table A1 column (i)).

A gap opens up in the readmission rates in 2006 immediately after the contracting reform. The readmission rate falls in the less concentrated markets, while it continues to rise in the more concentrated markets. Taken at face value, this would suggest that hospitals that experienced price deregulation in a more competitive environment raised the quality of the care they delivered. However, the divergence is not sustained. From 2007, the trends return to being similar in more and in less concentrated markets. Over the full five-year post-reform period, the figure suggests that price deregulation in a more competitive environment did not consistently lower or raise the quality of hip replacements.

Motivated by Figure 1, and because hospitals and insurers may not have fully adjusted to the new contracting conditions immediately after prices became freely negotiable, we estimate a second model that allows the treatment effect to vary in the post-reform period:

$$RR_{ht} = \alpha + \sum_{k=06,10} \delta_k \, 1(HHI_h < 2500) \times YEARk_t + \boldsymbol{X}_{ht}\boldsymbol{\mu} + u_h + \lambda_t + \varepsilon_{ht} \quad (2)$$

where $YEAR06_t$=1 ($YEARk_t$=1) if the year is 2006 (20*k*). Under the same assumptions about common trends and differential effects in more and less concentrated markets, δ_{06} gives a lower bound on the average effect of price deregulation in a more competitive market environment in 2006, etc.

Market concentration is generally considered to be potentially endogenous because performance may feed back into structure and unobservable attributes may influence both quality and patient choice of hospital (Evans et al., 1993). The empirical strategy adopted and the institutional context in which this study is conducted minimize the threats to identification from these two potential sources of endogeneity. Hospital fixed effects deal with any time invariant correlated unobservables. We avoid using time varying information on market concentration by categorizing each hospital into one of two groups according to whether its HHI is always below 2500 or always above 2500. Hospitals that cross this threshold over the period of analysis are dropped.[25] The HHI are calculated from bed numbers, rather than patient flows, and so endogeneity of this measure of market structure to performance is not a major concern.

We deliberately choose an elective procedure to measure quality in order to obtain evidence on the effect of competition on a treatment that is likely to exhibit much greater demand elasticity with respect to price and quality than is the case with acute treatments (e.g. for AMI) that have been the focus of previous reseach (e.g. Cooper et al., 2011; Gaynor et al., 2013). If greater competition does potentially reduce quality because demand is more responsive to price than to quality, then we would expect to observe this for an elective procedure. There are three reasons why this study is not particularly vulnerable to selection bias arising from patient choice despite its focus on an elective procedure. First, we eliminate correlated time invariant unobservable differences in patient composition across hospitals with fixed effects. Only if the reform were to change unobservable patient characteristics differentially across the treatment and comparison groups would there be any potential bias. The lack of any substantial or significant difference-in-differences in observable covariates (Table 1) suggests that there may be little reason to worry about potentially correlated time varying unobservables. Second, as previously mentioned, patients and insurers lacked information on hospital quality, including readmission rates for hip replacements, before and after the reform. There was limited scope for selection on quality. Third, in contrast to the UK healthcare market reforms that have been the subject of many previous studies,[26] the reform we examine did not change opportunities for patient choice. As explained above, patients had *de facto* free choice of provider before and after the reform.

Hospital-initiated selection of patients is potentially of greater concern. After prices were deregulated, hospitals operating in more competitive markets could possibly have had the incentive to drive down costs; e.g. by cherry picking more straightforward cases so that tighter budgets would not impinge on quality. However, because we identify from comparison across hospitals categorized by broad ranges of HHI, any cherry picking would only bias our estimates in the highly unlikely situation that patients were shunted long distances. More likely is that a hospital would refer a patient who is at greater risk of readmission to a neighboring hospital,

[25] As noted earlier, horizontal mergers among hospitals are mainly responsible for threshold crossings.

[26] These studies either use rich data or intruments to deal with time varying patient selection. Skellern (2019) controls for risk-adjusted Patient Reported Outcome Measures (PROMs), while Gaynor et al. (2013), Cooper et al. (2011) and Moscelli et al. (2016) instrument hospital choice using GP/patient-hospital distances. Cooper et al. (2011) do not reject exogeneity of market structure and Moscelli et al. (2016) find that instrumenting has very little impact on the estimates, relative to controlling for a rich set of patient covariates.

which will most probably be in the same treatment or comparison group. So, while the case mix of individual hospitals may change due to patient selection in response to the reform, it is rather unlikely that this would change the composition of the groups, and the comparisons in Table 1 again support this.

6. Results

6.1 Main estimates

Prior to the reform, there was no difference in the 90-day readmission rate between the treatment (low market concentration, HHI<2500) group and the comparison (high market concentration) group (Table 2, top panel). The groups are balanced on the outcome at baseline. Post reform, the readmission rate increased (10% significance) by 0.59 percentage points (pp) or 7.8% in the comparison group and by only slightly less (and not significantly) in the treatment group. Consequently, the simple (non-parametric) DID estimate is negative, which would indicate that price deregulation in a competitive market environment led to lower readmission rates (i.e. higher quality). But the estimate is very small in magnitude – 1.6% of the treatment group pre-reform rate – and not at all close to being significantly different from zero.

The conditional DID estimate obtained from model (1) and given in the first column of the top panel of Table 3 is positive, but it is even smaller in magnitude than the simple DID estimate, and it also lacks any significance. The fact that conditioning on observables does not markedly change the estimate further indicates that there is likely to be little bias from correlated time varying unobservables. The insignificant conditional DID point estimate is only 0.7% of the readmission rate in the treatment group hospitals prior to the reform. We can rule out an effect greater than 11% of the pre-reform readmission rate with 95% confidence. Subject to the usual caveat that failure to reject the null hypothesis does not necessarily imply that there is no effect, the magnitude and precision of the estimate do not give cause to believe that price deregulation in a competitive market led to substantial, or even any, deterioration in the quality of treatment. This inference is subject to the further caveat that our empirical strategy delivers only a lower bound (in magnitude) estimate of the effect of price deregulation in a competive setting.

Consistent with what is observed in Figure 1, the conditional DID estimates in the first column of the bottom panel of Table 3 suggest that price deregulation in less concentrated markets may have reduced the 90-day readmission rate by at least 1.5 percentage points (19%) in the first year (2006) after the reform but had no effect in the years thereafter (2007-2010). As is apparent from the figure, the negative effect in 2006, which is significant only at the 10%

level, is driven by a fall in the readmission rate of the hospitals operating in less concentrated markets and a rise in the readmission rate of hospitals in more concentrated markets. It would be difficult to attribute these divergent movements to a positive effect of price deregulation on quality.

For all post-reform years after 2006, the point estimates are positive. Overlooking the facts that none of the estimates are remotely significant and they are small in magnitude, one might venture to explain this pattern as arising from an initial post-reform shock (in 2006) followed by a gradual deterioration in quality the longer hospitals that are exposed to more competitive markets are operating with unregulated prices. Inconsistent with this hypothesis, the point estimates do not monotonically increase in magnitude as time since the reform lengthens. Still, the largest (but highly insignificant) point estimate is in the last of the post-reform years and it is 10% of the pre-reform readmission rate of the treatment group. This places a final caveat on the conclusion that there was no negative impact on quality.

6.2 Robustness

6.2.1 Market definition

The main estimates are generated on the basis of HHIs calculated with a radius of 30km used to define the boundary of the market around a hospital. To check robustness, we recalculate the HHI using a radius of 20 and 40km to define a market, recategorize hospitals into the treatment and comparisons groups on the basis of the revised index and then re-estimate models (1) and (2) in each case. Estimates are given in the appropriately labelled columns of Table 3. With a radius of 30km, hospitals are evenly split between the treatment and comparison groups. As the radius is widened, more hospitals get put into the treatment group because the HHI decreases as the area that defines the market increases.

Irrespective of the radius used, the treatment effect averaged over the five years of the post-reform period is insignificant. When the radius is increased, the point estimate of the effect in 2006 (from model (2)) continues to be negative but significance is lost.[27] When the radius is reduced, this point estimate turns positive and again it loses significance. The marginally significant negative point estimate obtained for 2006 using the baseline radius of 30km is clearly not robust. There is no significant effect after 2007 irrespective of the geographic radius used to define the market. Overall, irrespective of the radius used to define a hospital market,

[27] Extending the radius further to 50km produces the same general pattern: no significant effect average over all post-reform years and a negative but not at all significant point estimate for 2006. See Appendix 3 Table A2 and A3, column (iv).

there is no clear evidence that price deregulation in a more competitive market environment consistently impacted on the readmission rate.

One might be concerned that exposure to different degrees of market concentration is confounded by differences (in reponse to the reform) between urban and rural locations. This is unlikely to be a well-founded concern in the context of the Netherlands, which is the second most densely populated country in the OECD and one of the most urbanized.[28] Highly developed and integrated transport networks further limit the scope for any marked and consequential urban-rural division in the country. Only three hospitals in our sample are not within a 30 km radius of a city with at least 50,000 inhabitants, and only one of these three hospitals remains in our sample after selecting hospitals that are persistently in the same group defined by HHI above or below 2500.[29] The estimates are robust to excluding this hospital (see Appendix 3, Table A2 and A3, column (v).

6.2.2 Curtailed post-reform period

In 2008, there were a number of policy changes in the Dutch hospital industry. As mentioned above, the Health Care Inspectorate made an unsuccessful attempt to develop and implement a comprehensive set of uniform quality indicators across hospitals. Free price negotiation was extended to more procedures that brought the share of hospital revenues derived from products with unregulated prices to 20%. The method of paying specialists also changed in 2008, such that their annual income became completely activity-based, which generated strong incentives to increase production (Schut and Varkevisser, 2013). Their remuneration rates, however, were still regulated and, therefore, not affected by the outcome of the insurer-hospital price negotiations. Entry to the hospital market became easier from that year because government approval for the construction of new hospital buildings (or additional capacity) was no longer required. This was accompanied by a gradual increase in the financial risk for hospitals because reimbursement of capital costs was no longer (fully) assured.

Although the year-specific estimates in the bottom panel of Table 3 give no indication of any substantial change from 2008 that may be due to confounding, we examine the robustness of our findings to curtailing the post-reform period to 2006-2007. Doing so gives an insignificant negative point estimate of the effect averaged over these two years that is small in

[28] Dutch population density is almost twice that of the UK and more than 14 times greater than that of the US (IndexMundi, 2020). More than 75 percent of the population lives in predominantly urban regions (OECD, 2018).
[29] All three hospitals are in the high HHI group.

magnitude.[30] This gives no reason to suspect that an effect of price deregulation on the hip replacement readmission rate is being confounded by other policy changes that occurred within our period of analysis.

6.2.3 30-day readmission rate

Since theoretical and empirical grounds for unambiguously preferring the 90-day to the 30-day readmission rate as an indicator of the quality of care are lacking, we check robustness to using the shorter period. Pre-reform trends in 30-day readmission rates are reasonably parallel between the treatment and comparison groups, although there is some divergence in 2004 (see Figure 2). The hypothesis that year effects in the 30-day readmission rate are equal for the treatment and comparison group hospitals in the pre-treatment period is not rejected (Appendix 3, Table A1; column (ii)), which lends plausibility to the common trends identification assumption for this outcome also.[31]

The estimated effect on the 30-day readmission rate over the full post-reform period given in the top panel of column (4) of Table 3 is about half the magnitude of the baseline estimated effect on the 90-day rate in column (1), which is due to the lower mean rate of readmissions over the shorter period. The estimated effect on the 30-day rate is also not at all significant, and so there continues to be no evidence that price deregulation in more competitive markets affected the quality of care. The year-specific estimates are negative in 3 of the 5 post-reform years, but they are always small in magnitude and never close to significance.

6.2.4 Knee replacements

To further assess the credibility of our finding of a null effect, we replicate the analysis for an indicator of the quality of a second elective procedure – readmission after knee replacement. Within the subset of products for which free price negotiation was allowed from 2005, knee replacement is the only other procedure with sufficiently high volume and for which a reliable quality indicator can be constructed. Knee replacements and hip replacements are commonly performed by the same specialty, which makes this a local test of robustness. The upside is that consistency across the two procedures would lend a lot of credibility to the evaluation of the

[30] These results are available from the authors upon request.
[31] For both groups, the trends in the 30-day rate display greater volatility than those for the 90-day rate, which is due to the substantially lower rate of readmissions over the shorter period. This greater noise in the 30-day rate is one good reason for relying more on the 90-day rate.

effect on the quality of the treatment performed by a particular speciality. We use the 90-day readmission rate as the quality indicator.

The readmission rate after knee replacement displays greater variation from year to year than the rate for hip replacements (see Appendix Figure A1), which reflects the much smaller number of patients undergoing knee replacement. We cannot reject that the time effects are common between the treatment and comparison groups in the pre-reform period (see Appendix Table A1, column (iii)). The estimates of the effect on the knee replacement readmission rate are highly consistent with those for the effect on readmission after hip replacement (Table 3. Column (5)). The estimated effect over the full post-reform period (top panel) is very close in (small) magnitude to the respective estimate for hip replacements, only it is less precise reflecting the smaller sample. As with hip replacements, the estimated effect is negative in 2006. Thereafter, it is positive, except for 2009. Over all the years and for each year, the estimated effect is never close to significance. Irrespective of whether readmission rate after hip replacement or after knee replacement is used to indicate quality, there is no evidence that price deregulation in a more competitive market affected the quality of care.

6.3 Heterogeneity

While the HHI threshold of 2500 is based on US antitrust guidelines, it is somewhat arbitrary, and even more so in a European context. Further, a binary classification will miss any variation in the response to the intensity of competitive pressure induced by price deregulation across finer degrees of market concentration. The null effect we find could possibly arise from differential responses at the extremes of the market concentration distribution being diluted by similar responses either side, but closer to, the 2500 threshold. To test for this, we categorize hospitals into four groups: i) HHI<1500, ii) 1500≤HHI<2500, iii) 2500≤HHI<5000, and iv) HHI≥5000.[32] Category i) is sometimes used in antitrust regulation to identify an unconcentrated market, while ii) is taken to correspond to moderate concentration. Although HHI≥2500 is a conventional definition of a highly concentrated market, we use HHI≥5000 to distinguish the most concentrated markets. We use this as the reference category and estimate regression model (1) with dummies for the other three categories replacing the dummy for HHI<2500. Provided that in the absence of the reform readmission rates in each of categories i), ii) and iii) would

[32] In our analytical sample, the number of hospitals in each category is i) 32, ii) 11, iii) 24, and iv) 15.

have evolved over the 2006-2010 period as they would have in category iv) without the reform, and assuming that the effect is monotonically decreasing with the degree of market concentration (but of the same sign), then the regression gives lower bound estimates of the effect of price deregulation at each of the three lower levels of market concentration.

The estimates given in Table 4[33] indicate no significant effect on the readmission rate at any level of market concentration. The estimated effect of price deregulation does not differ significantly even between hospitals operating in the most competitive (HHI<1500) and the least competitive (HHI≥5000) markets. The point estimates for the two categories on either side of the 2500 threshold are particularly small in magnitude. They are also negative, which contributes to the fact that the positive point estimate for the HHI<1500 category is almost five times greater than the baseline estimate given in the first column of Table 3, which is obtained by averaging over hospitals with HHI<2500 in comparison with the average over those with HHI≥2500. Comparing more broadly defined treatment and comparison groups reduces the estimate of the effect of price deregulation relative to the estimate obtained from comparison of the extremes of the market concentration distribution. Whether this corresponds to dilution of a true effect in the most competitive market environment is more difficult to judge. The estimate for the HHI<1500 category is not remotely significant.

Estimates of an extended version of model (2) that includes a dummy for each market concentration category interacted with the year indicators to get the time varying effect of price deregulation at each level of market concentration are given in Appendix Table A5. The significant negative effect on the readmission rate in the year immediately after the reform is observed at all levels of concentration. Otherwise, there is only one significant effect, which is positive and for the HHI<1500 group in 2008.[34]

7. Conclusion

This is the first paper to credibly test for an effect of price deregulation in a more competitive market on the quality of elective healthcare. When producers are free to compete on price as well as quality and information on the latter is lacking or poor, it may be profitable for providers to cut prices and lower quality. This is one of the reasons regulators have been leery of permitting price competition in healthcare markets.

[33] A table with all results can be found in Appendix 3, Table A4.

[34] As with the estimates given in Table 3, the point estimate of the effect for the HHI<1500 group does not increase monotonically in magnitude as the post-reform period lengthens. The significant estimate obtained for 2008 could therefore be an anomaly and due to the lack of correction for multiple testing.

Our results do not lend support to this cautious approach. We find no evidence that Dutch hospitals operating in more competitive markets reduced the quality of care – measured by rates of readmission after hip (knee) replacements – they delivered after prices were deregulated by permitting the hospitals to negotiate prices with insurers. One possible explanation of this null effect is that hospitals, which in the Netherlands, as in most other countries, are not-for-profit organizations, were not prepared to grasp a competitive advantage obtainable by cutting prices if this required skimping on quality. Another potential explanation is that, in common with other papers on the effects of competition in healthcare markets, our empirical strategy delivers a lower bound on the effect. If the opportunity for free price negotiation were to have damaged quality to a lesser degree in less competitive markets, then we will underestimate the effect in more competitive markets. The trend in the readmission rate in our comparison group of hospitals operating in more concentrated markets does not offer much support for this explanation. Nor does the fact that the estimated effect remains insignificant when we compare hospitals at the extremes of the distribution of market concentration.

We find a reasonably consistent estimate of a reduction in the readmission rates of hospitals in the most competitive markets in the first year after prices were deregulated, although the significance of this estimate is not robust to all specifications. Notwithstanding this lack of robustness, such an effect could possibly result from initial misapprehension on one side of the market (i.e. the hospital administrators) of how the other side (i.e. the health insurers) would behave in the new contracting space. For the first time, hospitals and insurers were negotiating contracts for hospital products (DRG equivalents) rather than agreeing on prospective budgets and related parameters, like hospital days. Hospitals may have expected that the insurers would be sensitive at contract renewal to both price and quality. Hospitals in more competitive markets might therefore have been afraid that they would lose out if they did not improve quality. When hospitals came to realize that bargaining primarily focused on price and comparative quality information would not be readily available, they may have scaled down their initial efforts aimed at quality improvements. Overall, our results lend provisional support for the conclusion that permitting price competition among Dutch hospitals did not negatively impact on quality.

REFERENCES

AHRQ. 2015. *AHRQ QI™ Version 5.0, Inpatient Quality Indicators #14, Technical Specifications, Hip Replacement Mortality Rate*. Rockville: Agency of Healthcare Research and Quality.

Avram. V., D. Petruccelli, M. Winemaker & J. de Beer. 2014. 'Total joint arthroplasty readmission rates and reasons for 30-day hospital readmission'. *The Journal of Arthroplasty*. 29:465-468.

Berta. P., G. Martini, F. Moscone & G. Vittadini. 2016. 'The association between asymmetric information, hospital competition and quality of healthcare: evidence from Italy'. *Journal of the Royal Statistical Society: Series A*. 179(4):907-926.

Beukers. P.D.C., R.G.M. Kemp & M. Varkevisser. 2014. 'Patient hospital choice for hip replacement: empirical evidence from the Netherlands'. *European Journal of Health Economics*. 15:927-936.

Bevan. G. & M. Skellern. 2011. 'Does competition between hospitals improve clinical quality? A review of evidence from two eras of competition in the English NHS'. *British Medical Journal*. 343(7):6470-6470.

Bijlsma. M.J., P.W.C. Koning & V. Shestalova. 2013. 'The Effect of Competition on Process and Outcome Quality of Hospital Care in the Netherlands'. *De Economist*. 161(2):121-155.

Bloom. N., C. Propper, S. Seiler & J. van Reenen. 2015. 'The Impact of Competition on Management Quality: Evidence from Public Hospitals'. *Review of Economic Studies.* 82:457-489.

Capps. C, D. Dranove & M. Satterthwaite. 2003. 'Competition and market power in option demand markets'. *RAND Journal of Economics.* 34(4):737-763.

Clement. R.C., P.B. Derman, D.S. Graham, R.M. Speck, D.N. Flynn., L.S. Levin, L.A Fleisher. 2013. 'Risk factors, causes and the economic implications of unplanned readmissions following total hip arthroplasty'. *The Journal of Arthroplasty*. 28: 7-10.

Colla. C.H., J. Bynum, A. Austin & J. Skinner. 2016. 'Hospital competition, quality and expenditures in the US Medicare population'. *NBER Working papers No. 22826*. Cambridge: National Bureau of Economic Research.

Cooper. Z., S. Gibbons, S. Jones & A. McGuire. 2011. 'Does hospital competition save lives? Evidence from the English NHS patient choice reforms'. *The Economic Journal.* 121:F228-F260.

Croes. R.R., Y.J.F.M. Krabbe-Alkemade & M.C. Mikkers. 2017. 'Competition and quality indicators in the health care sector: empirical evidence from the Dutch hospital sector'. *The European Journal of Health Economics*. 19(1)1-15.

Dorfman. R. & P.O. Steiner. 1954. 'Optimal Advertising and Optimal Quality'. *The American Economic Review*. 44(5):826-836.

Dranove. D. & M.A. Satterthwaite. 2000. *The Industrial Organization of Health Care Markets*. In: 'Handbook of Health Economics, Volume 1B'. (eds: A.J. Culyer & J.P. Newhouse). Amsterdam & Boston: Elsevier. 1093-1139.

European Commission. 2004. *Guidelines on the assessment of horizontal mergers under the Council Regulation on the control of concentrations between undertakings.* 2004/C 31/03.

Evans. W.N., L.M. Froeb & G.J. Werden. 1993. 'Endogeneity in the concentration-price relationships: causes, consequences and cures'. *The Journal of Industrial Economics*. 4:431-438.

Federal Trade Commission. 2010. *Horizontal Merger Guidelines*. Washington: Federal Trade Commission.

Fischer. C., H.F. Lingsma, P.J. Marang-van de Mheen, D.S. Kringos, N.S. Klazinga & E.W. Steyerberg. 2014. 'Is the readmission rate a valid quality indicator? A review of the evidence'. *PLOS one*. 9(11):1-9.

Fricke, H. 2017. Identification Based on Difference-in-Differrences Approaches with Multiple Treatments. *Oxford Bulletin of Economics and Statistics*, 79(3):426-433,

Friebel. R., K. Dharmarajan, H. Krumholz & A. Steventon. 2017. 'Reductions in Readmission Rates are Associated with Modest Improvements in Patient-reported Health Gains following Hip and Knee Replacement in England'. *Medical Care*. 55(9):834-840.

Gaynor. M. 2006. 'What Do We Know About Competition and Quality in Health Care Markets?'. *Foundations and Trends in Microeconomics*. 2(6):1-40.

Gaynor. M., K. Ho & R.J. Town. 2015. 'The industrial organization of health-care markets'. *Journal of Economic Literature*. 53(2):235-284.

Gaynor. M., R. Moreno-Serra & C. Propper. 2013. 'Death by Market Power: Reform, Competition and Patient Outcomes in the National Health Service'. *American Economic Journal: Economic Policy*. 5(4):134-166.

Gaynor. M., C. Propper & S. Seiler. 2016. 'Free to choose? Reform, Choice and Consideration Sets in the English National Health Service'. *American Economic Review*. 106(11):3521-3557.

Gaynor. M. & R.J. Town. 2012. *Competition in Health Care Markets*. In: 'Handbook of Health Economics Volume 2'. (eds: McGuire. T., M.V. Pauly & P. Pita Barros). Amsterdam: Elsevier North-Holland. 499-637.

Gobillon. L. & C. Milcent. 2017. *Competition and hospital quality: evidence from a French natural experiment*. 'CEPR Discussion Papers 11773'.

Gowrisankaran. G., A. Nevo & R.J. Town. 2015. 'Mergers when prices are negotiated: evidence from the hospital industry'. *American Economic Review*. 105(1):172-203.

Gowrisankaran. G. & R.J. Town. 2003. 'Competition, Payers and Hospital Quality'. *Health Services Research*. 38(6):1403-1422.

Gravelle. H., G. Moscelli, R. Santos & L. Siciliani. 2014. *Patient choice and the effects of hospital market structure on mortality for AMI, hip fracture and stroke patients*. 'CHE Research Paper 106'. York: Centre for Health Economics.

Halbersma. R.S., M.C. Mikkers., E. Motchenkova & I. Seinen. 2010. 'Market structure and hospital-insurer bargaining in the Netherlands'. *European Journal of Health Economics*. 12(6):589-603.

Heijink. R., I. Mosca & G. Westert. 2013. 'Effects of regulated competition on key outcomes of care: Cataract surgeries in the Netherlands'. *Health Policy*. 113(1-2):142-150.

Ho. K. 2009. 'Insurer-Provider Networks in the Medical Care Market'. *American Economic Review*. 99(1):393-430.

IndexMundi. 2020. *Population density (people per sq. km of land area)*. Retrieved from: https://www.indexmundi.com/facts/indicators/EN.POP.DNST/ (on January 1, 2020).

Joynt. K.E. & A.K. Jha. 2012. ‘Thirty-day readmissions: truth and consequences’. *New England Journal of Medicine*. 366:1366-1369.

Kessler. D. & J.J. Geppert. 2005. ‘The effects of competition on variation in the quality and cost of medical care’. *Journal of Economics and Management Strategy*. 14(3):575-589.

Kessler. D. & M. McClellan. 2000. ‘Is hospital competition socially wasteful?’. *Quarterly Journal of Economics.* 115 (2): 577-615.

Krabbe-Alkemade. Y., T. Groot & M. Lindeboom. 2017. ‘Competition in the Dutch hospital sector: an analysis of health care volume and cost’. *European Journal of Health Economics*. 18(2):139-153.

Kranton. R.E. 2003. Competition and the incentive to produce high quality. *Economica*, 70(279):385– 404.

Kurtz. S.M., E.C. Lau, K.L. Ong, E.M. Adler, F.R. Kolisek & M.T. Manley. 2016. ‘Hospital, patient, and clinical factors influence 30- and 90-day readmission after primary total hip arthroplasty’. *Journal of Arthroplasty*. 31(10):2130-2138.

Laudicella. M., P. Li Donni & P.C. Smith. 2013. ‘Hospital readmission rates: Signal of failure or success?’. *Journal of Health Economics*. 32:909-921.

Meijer. S., R. Douven. & B. van den Berg. 2010. ‘Hoe beinvloedt gereguleerde concurrentie de Nederlandse ziekenhuismarkt?’ [In Dutch]. *TPEdigitaal.* 4(1):39-49.

Mokhtar. S.A., A.A. El-Mahalli, S. Al-Mulla & R. Al-Hussaini. 2012. ‘Study of the relation between quality of inpatient care and early readmission for diabetic patients at a hospital in the eastern province of Saudi Arabia’. *Eastern Mediterranean Health Journal*. 18(5):474-479.

Moscelli. G., L. Siciliani, N. Gutacker & H. Gravelle. 2016. ‘Location, quality and choice of hospital: evidence from England 2002-2013’. *Regional Science and Urban Economics*. 60:112-124.

Moscelli. G., H.S.E. Gravelle & L. Siciliani. 2019. Effects of *Market Structure and Patient Choice on Hospital Quality for Planned Patients*. ‘CHE Research Paper 162’. York: Centre for Health Economics.

Mutter. R., P.S. Romano. & H.S. Wong. 2011. ‘The Effects of US Hospital Consolidations on Hospital Quality’. *International Journal of the Economics of Business*. 18(1):109-126.

NZa (Nederlandse Zorgautoriteit). 2009. *Ziekenhuiszorg 2009 Tijd voor reguleringszekerheid*. [In Dutch] Utrecht: Nederlandse Zorgautoriteit.

Organisation for Economic Co-operation and Development (OECD). 2018. *Regions and Cities at a Glance, 2018 – NETHERLANDS*. Retrieved from: https://www.oecd.org/cfe/NETHERLANDS-Regions-and-Cities-2018.pdf (on January 1, 2020)

Propper. C., S. Burgess & K. Green. 2004. ‘Does competition between hospitals improve the quality of care? Hospital death rates and the NHS internal market’. *Journal of Public Economics.* 88:1247-1272.

Propper. C., S. Burgess. & D. Gossage. 2008. ‘Competition and quality: evidence from the NHS internal market 1991-99’. *Economic Journal.* 118:138-170.

Quan. H., B. Li, C. Couris, K. Fushimi, P. Graham, P. Hider, J-M. Januel & V. Sundararajan. 2011. ‘Updating and Validating the Charlson Comorbidity Index and Score for Risk Adjustment in Hospital Discharge Abstracts Using Data from 6 Countries'. *American Journal of Epidemiology*. 173(6):676-682.

Quan. H., V. Sundararajan, P. Halfon, A. Fong, B. Burnand, J-C. Luthi, D. Saunders, C. Beck, T. Feasby & W. Ghali. 2005. 'Coding Algorithms for Defining Comorbidities in ICD-9-CM and ICD-10 Administrative Data'. *Medical Care*. 43(11):1130-1139.

Ramkumar. P.N., C.T. Chu, J.D. Harris, A. Athiviraham, M.A. Harrington, D.L. White, D.H. Berger, A.D. Naik & L.T. Li. 2015. 'Causes and Rates of Unplanned Readmissions after Elective Primary Total Joint Arthroplasty: A Systematic Review and Meta-Analysis'. *The American Journal of Orthopedics*. 44(9):397-405.

Romano. P.S. & D.J. Balan. 2011. 'A Retrospective Analysis of the Clinical Quality Effects of the Acquisition of Highland Park Hospital by Evanston Northwestern Healthcare'. *International Journal of the Economics of Business*. 18(1):45-64.

Roos. A.F., R.R. Croes, V. Shestalova, M. Varkevisser & F.T. Schut. 2019. 'Price effects of a hospital merger: heterogeneity across health insurers, hospital products, and hospital locations'. *Health Economics*. 28(9):1130-1145.

Rosen. A.K., S. Loveland, M. Shin, M. Shwartz, A. Hanchate, Q. Chen et al. 2013. 'Examining the impact of the AHRQ Patient Safety Indicators (PSIs) on the Veterans Health Administration: the case of readmissions'. *Medical Care*. 51(1):37-44.

Ruwaard. S., R. Douven, J. Struijs & J. Polder. 2014. 'Hoe kopen zorgverzekeraars in bij ziekenhuizen: een analyse van de contracten tussen verzekeraars en ziekenhuizen'. *TPEdigitaal*. 8(2):98-117.

Saucedo. J.M., G.S. Marecek, T.R. Wanke, J. Lee, S.D. Stulberg & L. Puri. 2014. 'Understanding readmission after primary total hip and knee arthroplasty: who's at risk?'. *The Journal of Arthroplasty*. 29:256-260.

Sari. N. 2002. 'Do competition and managed care improve quality?'. *Health Economics*. 11:571-584.

Schut. F.T. & W.P.M.M. van de Ven. 2011. 'Effects of purchaser competition in the Dutch health system: is the glass half full or half empty?'. *Health Economics, Policy & Law*. 6(1):109-123.

Schut, F.T. & M. Varkevisser. 2013. 'Tackling hospital waiting times: the impact of past and current policies in the Netherlands'. *Health Policy*. 113(1-2):127-133.

SCP (Sociaal Cultureel Planbureau). 2017. *Statusscores*. [In Dutch] Available at: https://www.scp.nl/Onderzoek/Lopend_onderzoek/A_Z_alle_lopende_onderzoeken/Statusscores.

Skellern. M. 2019. *The effect of hospital competition on value-added indicators of elective surgery quality*. Working paper. Obtained from: http://personal.lse.ac.uk/skellern/Research_files/PROMs_comp_paper.pdf. An earlier version of the paper has been published as Skellern. M. 2017. *The Hospital as a Multi-Product Firm: The Effect of Hospital Competition on Value-Added Indicators of Clinical Quality*. 'CEP Discussion Paper No. 1484'. London: Centre for Economic Performance.

Town. R. & G. Vistnes. 2001. 'Hospital competition in HMO networks'. *Journal of Health Economics*. 20(5):733-753.

Van der Geest, S.A. & M. Varkevisser. 2012. 'Zorgconsumenten en kwaliteitsinformatie' [In Dutch]. *ESB*. 97(4631): 174-175.

Van der Geest, S.A. & M. Varkevisser. 2016. ‘Using the deductible for patient channeling: did preferred providers gain patient volume?’. *European Journal of Health Economics*. 17(5): 645–652.

Van der Geest, S.A. & Varkevisser, M. 2019. ‘Patient responsiveness to a differential deductible: empirical results from The Netherlands’. *European Journal of Health Economics*. 20(4): 513-524.

Varkevisser. M., S.A. van der Geest & F.T. Schut. 2012. ‘Do patients choose hospitals with high quality ratings? Empirical evidence from the market for angioplasty in the Netherlands’. *Journal of Health Economics*. 31:371-378.

Varkevisser. M., S.A. van der Geest & F.T. Schut. 2010. ‘Assessing hospital competition when prices don’t matter to patients: the use of time-elasticities’. *International Journal of Health Care Finance and Economics*. 10:43-60.

Volpp. K.G.M., S.V. Williams, J. Waldfogel, J.H. Silber, J.S. Schwartz & M.V. Pauly. 2003. ‘Market Reform in New Jersey and the Effect on Mortality from Acute Myocardial Infarction’. *Health Services Research*. 38(2):515-533.

WHO-FIC (World Health Organization (WHO) – Collaborating Centre for the Family of International Classifications (FIC) in Nederland). 2017. *CMSV, Classificatie van Medisch Specialistische Verrichtingen.* Available at: http://www.who-fic.nl/Familie_van_Internationale_Classificaties/Verwante_classificaties/CMSV_Classificatie_van_Medisch_Specialistische_Verrichtingen.

Table 1 — MEANS OF COVARIATES BY PERIOD AND TREATMENT GROUP

Patients' characteristics		**Pre-reform**	**Post-Reform**	**Change (Post - Pre)**
Proportion discharged to skilled nursing facility	*Comparison Group*	0.048	0.059	0.011
		{0.061}	{0.084}	(0.009)
	Treatment Group	0.075	0.087	0.012
		{0.096}	{0.133}	(0.013)
	Difference (T-C)	0.03***		**0.001**
		(0.009)		**(0.016)**
Proportion female	*Comparison Group*	0.69	0.68	-0.01*
		{0.05}	{0.06}	(0.01)
	Treatment Group	0.72	0.69	-0.03***
		{0.06}	{0.07}	(0.01)
	Difference (T-C)	0.03***		**-0.02***
		(0.01)		**(0.01)**
Mean age	*Comparison Group*	69.85	70.36	0.51*
		{19.23}	{22.53}	(0.24)
	Treatment Group	70.60	70.50	-0.11
		{25.09}	{33.50}	(0.34)
	Difference (T-C)	0.75***		**-0.61**
		(0.25)		**(0.42)**
Mean Charlson Score (comorbidity)	*Comparison Group*	0.0018	0.0009	-0.0009*
		{0.01}	{0.00}	(0.0005)
	Treatment Group	0.0039	0.0017	-0.0022
		{0.02}	{0.00}	(0.0016)
	Difference (T-C)	0.0021		**-0.0014**
		(0.0016)		**(0.0016)**
Mean socioeconomic score	*Comparison Group*	6.81	6.88	0.06
		{0.53}	{0.55}	(0.06)
	Treatment Group	7.46	7.49	0.03
		{0.48}	{0.48}	(0.06)
	Difference (T-C)	0.65***		**-0.04**
		(0.06)		**(0.08)**
Number of hospitals	Comparison Group	39	39	39
	Treatment Group	43	43	43
Number of patients	Comparison Group	28613	33096	61709
	Treatment Group	25528	26851	52379

Notes: Pre-/post-reform cell entries are obtained by first computing the mean across all non-acute hip replacement patients discharged from each hospital and then taking the simple average of these means across all hospitals within a group and period. Figures in curly brackets are standard deviations across hospitals. Figures in parentheses are standard errors of the estimated change in the mean. Hospitals and patients are selected using the criteria described in the Data, sample selection section. The socioeconomic score is increasing in socioeconomic status and ranges from 0 to 10. The Charlson score (Quan et al. 2011) ranges from 0 to 9, with higher being more severe. *** Significant at the 1 percent level. ** Significant at the 5 percent level.

Table 2 — UNPLANNED 90-DAY AND 30-DAY HIP REPLACEMENT READMISSION RATES BY PERIOD AND TREATMENT GROUP

Outcome		**Pre-reform**	**Post-Reform**	**Change (Post - Pre)**
90-day readmission rate	*Comparison Group*	0.0768	0.0827	0.0059*
		(0.0316)	(0.0290)	(0.0036)
	Treatment Group	0.0766	0.0813	0.0047
		(0.0321)	(0.0326)	(0.0037)
	Difference (T-C)	-0.0003		**-0.0012**
		(0.0036)		**(0.0051)**
30-day readmission rate	*Comparison Group*	0.0419	0.0482	0.0062**
		(0.0227)	(0.0200)	(0.0025)
	Treatment Group	0.0439	0.0493	0.0054*
		(0.0237)	(0.0239)	0.0027)
	Difference (T-C)	0.0019		**-0.0008**
		(0.0026)		**(0.0037)**
Number of hospitals	Comparison Group	39	39	39
	Treatment Group	43	43	43
Number of patients	Comparison Group	28613	33096	61709
	Treatment Group	25528	26851	52379

Notes: Table gives the simple mean readmission rate averaged over all hospitals in the treatment (HHI<2500) group and the comparison (HHI≥>2500) group. Figures in curly brackets are standard deviations across hospitals. Robust standard errors in parentheses. Hospitals and patients selected by criteria described in the Data, sample selection section. * Significant at the 10 percent level.

Table 3 — ESTIMATED EFFECTS OF PRICE DEREGULATION ON READMISSION RATES AFTER HIP (KNEE) REPLACEMENT

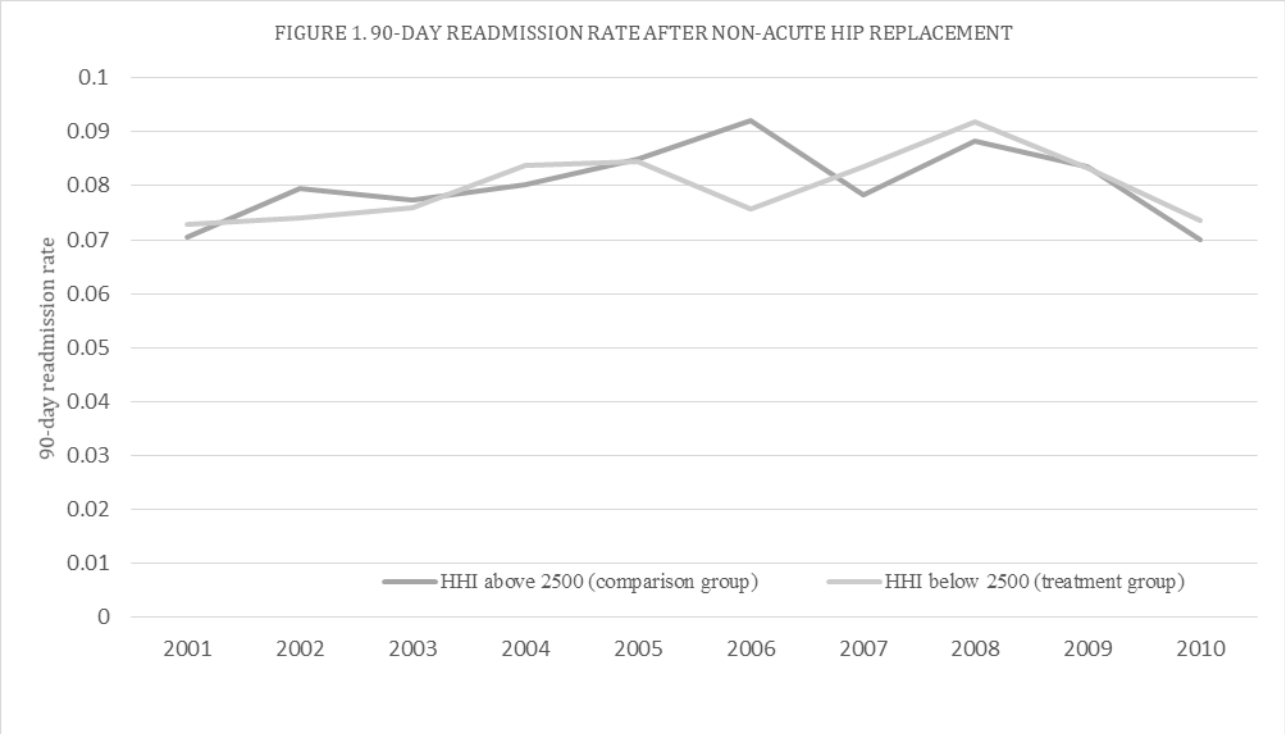
Specification	(1)	(2)	(3)	(4)	(5)
	Hip replacement				Knee Replacement
	90-day readmission	90-day readmission	90-day readmission	30-day readmission	90-day readmission
	radius 30	radius 20	radius 40	radius 30	radius 30
Model (1)					
δ	0.0005	0.0063	0.0016	0.0003	0.0007
	(0.0048)	(0.0055)	(0.0057)	(0.0037)	(0.0153)
R^2	0.49	0.461	0.458	0.437	0.375
Model (2)					
δ_6	-0.0147*	0.0007	-0.0056	-0.0059	-0.0315
	(0.0076)	(0.0097)	(0.0093)	(0.0054)	(0.0199)
δ_7	0.0077	0.0089	0.0062	0.0072	0.0080
	(0.0086)	(0.0099)	(0.0098)	(0.0064)	(0.0249)
δ_8	0.0039	0.0085	0.0012	0.0028	0.0260
	(0.0085)	(0.0101)	(0.0097)	(0.0066)	(0.0215)
δ_9	0.0003	0.0018	0.0026	-0.0011	-0.0147
	(0.0068)	(0.0074)	(0.0077)	(0.0058)	(0.0195)
δ_{10}	0.0081	0.0122	0.0038	-0.0015	0.0220
	(0.0074)	(0.0087)	(0.0088)	(0.0058)	(0.020)
R^2	0.4990	0.4630	0.4600	0.4410	0.3880
N Treatment Hospitals	43	13	29	43	34
N Comparison Hospitals	39	32	15	39	32
N hospitalsxyears	738	495	396	738	5974
N patients	114408	80077	74888	114408	18231
N readmitted patients	9064	6489	6043	5241	1183

Notes: Top panel gives OLS estimates of δ from regression (1). Second panel gives OLS estimates of δ6 until δ10 from regression (2). All estimates obtained from regressions containing hospital and year fixed effects and covariates identified in Table 1. Full estimates in Appendix 3; Tables 2 and 3. Robust standard errors in parentheses. Hospitals and patients selected by criteria described in the Data, sample selection section. Radius X indicates that the estimates are based on treatment/comparison groups formed on the basis of a HHI calculated with a radius of X km defining the boundary of a market. The sample size falls as the radius is reduced because more hospitals cross the HHI threshold of 2500 used to define the treatment/comparison groups during the estimation period.** Significant at the 5 percent level.* Significant at the 10 percent level.

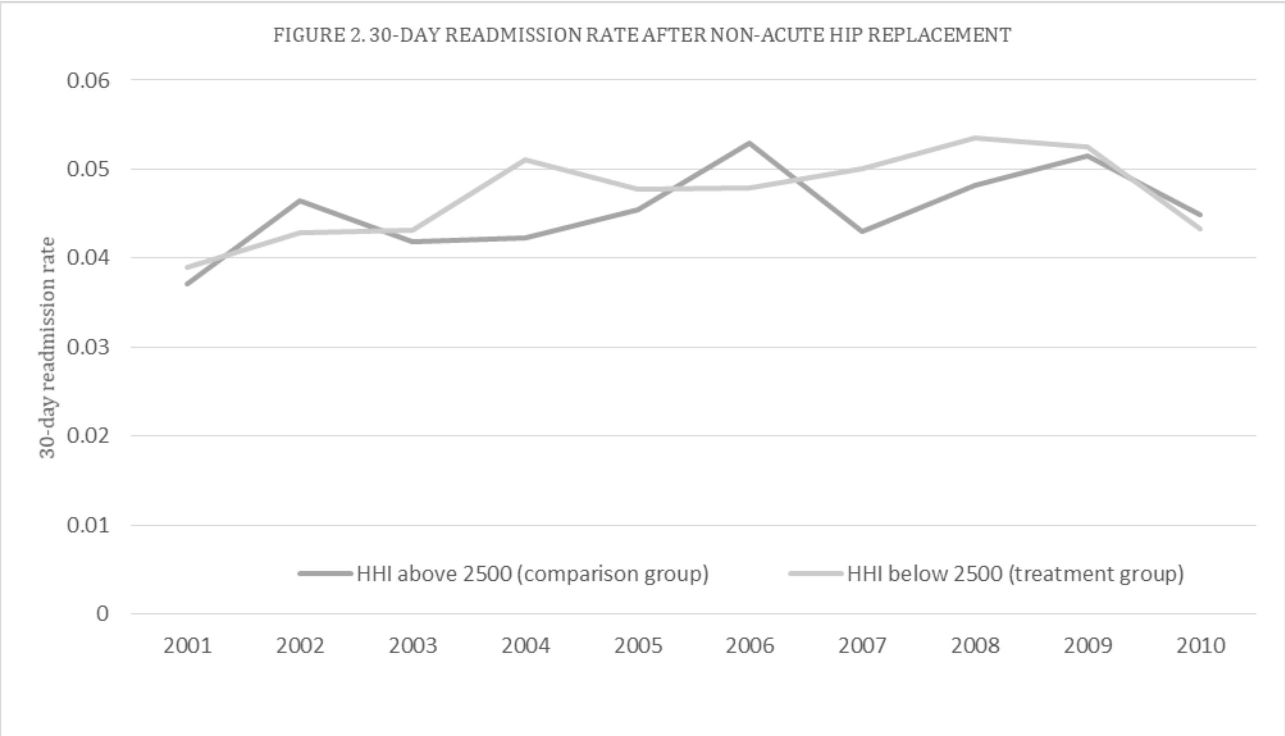
Table 4 – DIFFERENCE-IN-DIFFERENCES ESTIMATES OF EFFECTS OF PRICE DEREGULATION AT DIFFERENT DEGREES OF MARKET CONCENTRATION ON READMISSION RATES AFTER HIP REPLACEMENT

	90-Day readmission
	radius 30
HHI(≤1500)*treatment	0.0024
	(0.0070)
HHI(>1500, ≤2500)*treatment	0.0005
	(0.0162)
HHI(>2500, ≤5000)*treatment	0.0052
	(0.0068)
HHI(>5000)*treatment	-
(reference category)	-
N Treatment Hospitals	43
N Comparison Hospitals	39
N hospitals x years	738
N patients	114,408
N readmitted patients	9,064

Notes: OLS estimates from an extended version of regression (1) in which the single indicator of HHI<2500 is replaced with indicators for HHI<1500, 1500≤HHI<2500 and 2500≤HHI<5000, with HHI≥5000 being the reference category . Robust standard errors in parentheses. Hospitals and patients selected by criteria described in the Data, sample selection section. Full results see Appendix 3 Table A4.



Notes: Hospitals and patients selected by criteria described in the Data, sample selection section. HHI (Herfindahl-Hirschman Index) calculated with a radius of 30 km defining the boundary of a market.



Notes: Hospitals and patients selected by criteria described in the Data, sample selection section. HHI (Herfindahl-Hirschman Index) calculated with a radius of 30 km defining the boundary of a market.