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Does Competition from Private Surgical Centres Improve Public Hospitals' Performance? Evidence from the English National Health Service

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Abstract
This paper examines the impact of competition from government-facilitated entry of private, specialty surgical centres on the efficiency and case mix of incumbent public hospitals within the English NHS. We exploit the fact that the government chose the location of these surgical centres (Independent Sector Treatment Centres or ISTCs) based on nearby public hospitals' waiting times – not length of stay or clinical quality – to construct treatment and control groups that are comparable with respect to key outcome variables of interest. Using a difference-in-difference estimation strategy, we find that ISTC entry led to greater efficiency – measured by pre-surgery length of stay for hip and knee replacements – at nearby public hospitals. However, these new entrants took on healthier patients and left incumbent hospitals treating patients who were sicker, and who stayed in hospital longer after surgery.

Keywords: Hospital Competition, Public-Private Competition, Market Entry, Market Structure, Outsourcing, Hospital Efficiency, Risk Selection, Cherry Picking
JEL Classifications: C23; H57; I11; L1; L33; R12

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1. Introduction

Across wealthy nations, health care spending is one of the largest sectors of the overall economy and can be the biggest single area of government spending. Within the health care sector, hospital spending generally accounts for the largest share of total spending. As a result, significant effort has been devoted to finding policies that make publicly funded hospitals more efficient. This paper asks whether one such policy – government-facilitated entry of private surgical centres into the market – can be an effective means of improving the performance of nearby public hospitals. To this end, we study the entry of a series of private surgical centres during the 2000s on the efficiency, case mix and case load of incumbent public hospitals in the English NHS.

Outside of the United States (US), hospitals tend to be publicly owned and/or run on a not-for-profit basis (Cutler 2002). Reforms designed to improve hospitals' efficiency have often centred on non-market interventions like performance management and command and control (Saltman et al. 2011). However, increasingly, policymakers have introduced market-based reforms designed to expose public hospital systems to competition and financial incentives.

More recently, echoing similar trends in education and other public services, policymakers in government-run health systems have gone beyond introducing competition within the public system, and have begun exposing public hospitals to competition from private providers. Proponents of these reforms have argued, firstly, that private providers may provide care more cost-effectively than large incumbents and, secondly, that they can provide competitive pressure that might prompt incumbents to improve their performance (Le Grand 2007; Seddon 2007). However, there are concerns that new, private entrants 'cherry pick' low-risk patients, leaving incumbents with a costlier case mix (Street et al. 2010). More generally, it is also argued that competition is an ineffective means of improving hospital performance because patients do not act like classical consumers, either because they are not interested in making active choices, or because they are unable to do so given the poor observability of many dimensions of health care quality (Jones & Mays 2009; Fotaki et al. 2008).

The English NHS provides a unique environment for estimating the causal effect of surgical centre entry on incumbent hospitals. In the 2000s, the British government facilitated the entry of Independent Sector Treatment Centres (ISTCs) – private surgical centres focused on provision of routine, high volume elective surgical procedures to public patients – as part of a wider policy package designed to tackle waiting times within the English NHS.[1] The centrepiece of this package was an ambitious set of targets to reduce waiting times for surgery. ISTCs were established to

[1] Elective surgery is medically necessary surgery that is not an emergency, and is therefore scheduled in advance.

rapidly expand capacity in regions deemed at risk of not meeting these targets (Naylor & Gregory 2009).

As we demonstrate, while the placement of these specialty surgical centres was correlated with local public hospital waiting times, their placement was uncorrelated with the efficiency or clinical quality of these incumbents. In line with this finding, we demonstrate that public hospitals close to ISTC entrants had nearly identical pre-entry trends to public hospitals unexposed to ISTC entry across a range of performance measures (other than waiting times). We use this observation to motivate a difference-in-difference (DiD) strategy to estimate the causal effect of ISTC entry on outcomes at nearby public hospitals.

Measuring efficiency of health care provision is a long-standing challenge because of the absence or poor standard of data on costs and quality. Faced with these problems, researchers have frequently used patient length of stay (LOS) as a proxy for efficiency (Fenn & Davies 1990; Martin & Smith 1996; Gaynor et al. 2013) on the grounds that, provided clinical quality can be maintained, shorter LOS implies lower costs for the same outcomes. However, a key difficulty with using LOS to capture efficiency is that it is heavily influenced by patient characteristics – patients in poorer health before surgery will tend to have longer lengths of stay for reasons unrelated to hospital efficiency. In this study, we use an innovative approach to address the influence of patient characteristics on LOS-based efficiency measures by disaggregating LOS into two components: time from admission until surgery ('pre-surgery LOS'), and time from surgery until discharge ('post-surgery LOS'). We show that pre-surgery LOS is less biased by patient characteristics than other components of LOS, and use it – or alternatively, the percentage of patients treated on the day of admission – as a proxy for hospital efficiency.

In what follows, we show that public hospitals exposed to the entry of private specialty surgical centres improved their efficiency (as measured by pre-surgery LOS) by 16 per cent, which translates to a 24 percentage point increase in the proportion of patients treated on the day of admission. Secondly, however, we also find evidence that these private entrants did engage in risk selection – albeit that they were permitted to do so as a result of government policy – thus leaving nearby public hospitals with a sicker (and therefore costlier) mix of patients. In particular, public hospitals exposed to the entry of private specialty surgical centres experienced a 12 per cent deterioration in average patient health status as captured by the Charlson score (defined in Section 4), which translates to a 6.5 percentage point increase in the proportion of patients admitted with a Charlson score of three or more. We also find that public hospitals exposed to private surgical centre entry received a less affluent mix of patients as captured by the IMD income deprivation score, and experienced higher overall costs per patient as captured by post-surgery LOS. Finally, while ISTC entry may have led to reduced case loads at some public hospitals with which they shared a market, we show that our estimated treatment effects are not biased by volume effects.

This paper, which constitutes the first rigorous econometric evaluation of the impact of the British government's ISTC programme on nearby public hospitals, adds to the more general literature on the effects of new market entry on hospital performance. This literature finds that private specialist surgical centre entrants can improve efficiency at large incumbent hospitals, but can also leave incumbents with a sicker and costlier mix of patients due to risk selection practices by entrants (Kessler & McClellan 2002; Barro et al. 2006; Cutler et al. 2010; Winter 2003; Cram et al. 2005). Our findings are consistent with these results from other countries.

This paper is structured as follows. Section 2 presents background information on recent NHS reforms, with particular focus on the ISTC programme. Section 3 explores the potential impact of ISTCs on incumbents' performance. Section 4 sets out our data and empirical strategy. Section 5 reports our results, while Section 6 discusses and concludes.

2. Recent NHS Reforms and the ISTC Programme

The English NHS, founded in 1948, is funded through general taxation and, with few exceptions, offers health care that is free at the point of use. Patients must register with a single general practice clinic for the provision of primary care, and GPs act as 'gatekeepers' to the secondary care system. For the most part, secondary care in England is organised around large public hospitals, which comprise one or more hospital sites.

The NHS long struggled with waiting times for elective surgery that could exceed a year. In 1997, a new Labour government was elected promising quick action to reduce waiting times. However, one year later, waiting times had increased (Klein 2013, p.200).[2] Ultimately, concern over waiting times became the catalyst for a series of reforms from 2000 onwards, which instituted rigorous performance management of public hospitals; introduced patient choice and provider competition underpinned by prospective reimbursement; and facilitated the entry of specialist private surgical centres to compete with larger public hospital incumbents.

In 2000, The Secretary of State released *The NHS Plan* (Secretary of State for Health 2000), in which the government committed to cutting maximum waiting times for elective surgery from 18 months to 6 months by the end of 2005 (later revised down to 18 weeks, by 2008) using a series of

[2] During this period, newspaper stories about excessive waiting times appeared regularly in the popular press. As Klein (2013, p.202) writes, "No matter that the lengths of the [waiting] lists were an ambiguous indicator of performance. No matter that they were, if anything, a misleading measure of the NHS's ability to meet demands. Waiting lists were confirmed as the symbol of the NHS's inability to meet public expectations of quick and ready access to treatment."

targets tied to rewards and punishments. There is substantial evidence that the targets and performance management regime was extremely effective at reducing waiting times (Propper et al. 2008b; 2010; Besley et al. 2009).

As part of its reform programme, in April 2002 the government announced that it was facilitating the entry of a series of privately owned, privately run surgical centres (ISTCs) to deliver routine, high-volume diagnostic and elective surgical procedures to English NHS patients.[3] Like other NHS services, NHS-funded patients could use ISTCs free of charge.

Although the NHS had long made use of private providers in England, ISTCs were distinctive in two ways. First, they were created as a deliberate policy of government, as opposed to being a result of decisions by local commissioners of care. Second, they provided services exclusively to NHS patients, as opposed to earlier arrangements in which NHS patients were treated in settings mainly focused on treatment of private patients (Naylor & Gregory 2009).

In addition to introducing private surgical centres, the government's reform programme also included the 2006 introduction of patient choice of hospital for elective surgery, and, gradually from 2003, a new prospective reimbursement system (known as Payment by Results), modelled on the DRG system of Medicare in the US, in which reimbursement was tied to activity (DH 2011).

More than any other factor, it was local waiting times that influenced where the government sought to locate the new private surgical centres (HCHC 2006). According to government officials, "In October 2002, the Department [of Health] conducted an extensive forward planning exercise, during which all Strategic Health Authorities were asked to identify, in conjunction with their respective Primary Care Trusts, any anticipated gaps in their capacity needed to meet the 2005 waiting times targets. The results of this exercise led to the identification of capacity gaps across the country, particularly in specialties such as cataract removal and orthopaedic procedures, where additional capacity was needed" (Anderson 2006). Following this planning exercise, in December 2002 the Department of Health invited expressions of interest to run the first Wave of ISTCs. These invitations indicated the broad geographical regions within which ISTCs were to be placed, but left securing a specific site to bidders. Preferred bidders for these schemes were announced from September 2003.

In all, there were 27 Wave 1 ISTCs; the overwhelming majority opened in 2005 or 2006, and most operated from a single site, often in newly built premises, and often co-located with an existing NHS hospital. In March 2005, a second Phase of ISTCs was announced, of which nine

[3] DH 2002. This section also draws on Naylor & Gregory 2009; Allen & Jones 2011; Anderson 2006; BSG 2005; and HCHC 2006. ISTCs were also established in Wales and Scotland, but are outside the scope of this paper, given the devolution of the NHS to the constituent countries of the United Kingdom during this period.

were eventually implemented. Most Phase 2 ISTCs opened in 2007 or 2008. Unlike Wave 1 ISTCs, Phase 2 ISTCs generally operated over numerous sites, and were frequently co-located with existing private hospitals. Given these very different characteristics of the Phase 2 programme, in this paper we focus exclusively on analysing the impact of Wave 1 ISTCs.

The ISTC programme had a major impact on the market for some elective surgical procedures. From 2006, ISTCs accounted for between five and ten per cent of orthopaedic volume nationally (see Table 1). As the ISTC programme's impact was highly geographically differentiated, the share of patients attending ISTCs was much higher in some areas. In some markets where ISTCs entered, they became the only alternative to large incumbents. As one local NHS official noted when a large ISTC opened next to a dominant NHS hospital, "that's the first time… we've ever had any competition [here]" (McLeod et al. 2014, p.15).

ISTC contracts specified a range of 'exclusion criteria', or acceptable grounds for refusing to treat NHS patients, on the basis that ISTCs did not possess the emergency or intensive care units required to treat sicker and more complex patients. Each ISTC had its own list of exclusion criteria, which typically included demographic factors such as age, social factors such as availability of a carer at discharge, and clinical factors such as health status (Mason et al. 2008). In relation to the latter, a particularly important criterion for rejection was the patient's American Society of Anaesthesiologist's (ASA) score – ISTCs were typically able to refuse to treat patients with a score of 3 or more.[4] National Joint Registry data from 2010 indicates that, at NHS hospitals, 20 per cent of hip replacement patients and 19 per cent of knee replacement patients were given ASA scores of 3 or 4; the corresponding figures for ISTCs were only 6 and 8 per cent respectively (NJR 2011). Critics of the ISTC programme saw these exclusion criteria as particularly problematic because of the way in which they were perceived to dump responsibility for costlier, more complex patients onto the public hospital system (Wallace 2006; Kmietowicz 2006).[5]

[4] ASA 1: Healthy patient with localized surgical pathology and no systemic disturbance; ASA 2: Patient with mild to moderate systemic disturbance (i.e. surgical pathology or other disease process); ASA 3: Patient with severe systemic disturbance from any cause; ASA 4: Patient with life threatening systemic disorder which severely limits activity; ASA 5: Gravely ill patient with little chance of survival.

[5] In addition, in an effort to facilitate the entry of private providers, NHS policymakers negotiated 'take or pay' contracts guaranteeing that Wave 1 ISTCs would be paid for the number of procedures specified in the contract, irrespective of the number of patients actually treated. Also, to encourage entry and cover initial capital costs, these ISTCs were paid, on average, 11 per cent more per procedure than NHS providers (AC & HC 2008, p.51). Many observers (e.g. HCHC 2006; Squires 2007; Player & Leys 2008; Pollock & Godden 2008; see also Moore 2008 and McLeod et al. 2014) argued that these two provisions meant that ISTCs offered poor value for money.

3. Hypotheses on the Impact of ISTC Entry on Incumbent Public Hospitals

In what follows, we examine the likely response of public (NHS) hospitals to the government-facilitated entry of private surgical centres (ISTCs). In understanding the impact of the ISTC programme, it is important to note that, although public NHS hospitals are run on a not-for-profit basis, they are financially and managerially independent of central government, and during this period had strong incentives to generate a financial surplus, or at least not to make losses.

In the early 2000s, the government introduced a system of 'star rating' of NHS hospitals, in which financial performance was a major factor (Bevan & Hood 2006a; 2006b; DH 2002). Hospitals given a zero-star rating were 'named and shamed', and their chief executives were at risk of losing their jobs. Later, high-performing hospitals (those with Foundation Trust status) were given additional freedoms to retain financial surpluses across financial years, and thus had an incentive to generate such surpluses in order to pursue whatever objectives managers considered important. Other hospitals were eventually able to achieve Foundation Trust status in part through good financial performance. These factors meant that, during this period, public hospitals had a strong incentive to generate operating surpluses – that is, profits. It has therefore been argued that it is reasonable to think of public hospitals during this period as maximising profits, or perhaps profits plus some additional term reflecting altruistic valuation of quality and/or quantity (Gaynor et al. 2013).

3.1. Efficiency

The primary channel through which we envisage that ISTC entry increases efficiency and reduces length of stay in incumbent hospitals is by increasing incentives for fast patient turnaround, given the increase in local competition and the fixed-price prospective reimbursement system, Payment by Results (PbR).

The introduction of prospective reimbursement should lead to improvements in hospital efficiency, as hospitals are being paid on the basis of outputs rather than inputs (Cutler 1995). In particular, prospective reimbursement should, *ceteris paribus,* lead to shorter patient LOS, as a hospital's net revenue declines with each additional day of care provided.[6]

While prospective reimbursement provides incentives to all hospitals to reduce patient LOS, it is likely that hospitals in more competitive markets will respond more aggressively to its

[6] Empirical studies of England (Farrar et al. 2009), the United States (Feder et al. 1987; Guterman & Dobson 1986; Feinglass & Holloway 1991; Kahn et al. 1990), Israel (Shmueli et al. 2002) and Italy (Louis et al. 1999) provide evidence in support of this prediction.

introduction. PbR pays hospitals for their activity. However, hospitals located in more concentrated markets likely have limited scope to expand their activity because they are constrained by the relative inelasticity of clinical demand within their catchment areas. By contrast, hospitals located in more competitive markets have the opportunity to expand their activity by poaching other hospitals' patients. To create capacity for such expansion, hospitals in more competitive markets will likely respond to the introduction of PbR by taking stronger action to reduce patient LOS, so that they can treat additional patients.

Consistent with this hypothesis, studies of the 2006 patient choice reforms in the English NHS find that hospitals located in more competitive markets decreased their LOS by larger amounts than hospitals in less competitive markets (Cooper et al. 2012; Gaynor et al. 2013). As a result, we hypothesise that incumbent hospitals exposed to entry by an ISTC will have reduced patient LOS over and above any secular decreases in LOS resulting from the introduction of PbR. We therefore identify the effect of ISTC entry on the efficiency of nearby public hospitals using a DiD estimator in which the treatment effect equals the change in efficiency at ISTC-exposed public hospitals minus the change in efficiency at unexposed public hospitals.

During the 2000s, the government saw same-day surgery as a key measure of performance. The NHS Institute for Innovation and Improvement (2006; 2008a; 2008b) identified surgery on day of admission as one of the six characteristics of high-performing orthopaedic surgical centres, and argued (2006, p.20) that public hospitals would have to respond to competition from private entrants by streamlining their production: "Same-day admissions are seen as imperative by independent [private] providers. Acute [public] trusts will need to reflect this as an integral element of any market strategy when seeking to demonstrate competitive advantage." This explicit focus on same-day admissions means that, in addition to the more general incentives to increase efficiency brought about by private surgical centre entry, we expect that public hospitals facing increased pressure from private surgical centres will have had particularly strong incentives to improve performance in relation to this dimension of efficiency.

3.2. Casemix

The entry of private surgical centres can also change the case mix at nearby incumbents due to risk selection practices by entrants. Whereas in classical private goods markets the profitability of selling to a particular customer is determined solely by their willingness to pay, in health care markets the profitability of treating a patient will be influenced by characteristics of the patient that are often imperfectly observed. The influence of patient characteristics on profitability provides hospitals with an incentive to refuse to treat the sickest patients – and these incentives will be sharper when providers operate on a for-profit basis, as most ISTCs did. The literature on specialty hospitals in the US, for example, has found evidence that these providers cherry-pick the most profitable patients, leaving the sickest patients to nearby general hospitals (Barro et al. 2006; Winter 2003; Cram et al. 2005). The ISTC programme is likely to have had such an effect on nearby public hospitals because, while public hospitals were unable to reject patients based on clinical criteria, ISTCs could decline to treat complicated cases. Moreover, ISTCs can redistribute any profits made to shareholders, whereas NHS hospitals can only reinvest profits into the organisation.

Previous studies have confirmed that ISTCs treated healthier and less complex patients than nearby public hospitals (Street et al. 2010; Mason et al. 2008; 2010; Browne et al. 2008; Chard et al. 2011; Fagg et al. 2012). However, no one has yet compared the evolution of average patient severity at ISTC-exposed public hospitals with that at public hospitals unaffected by the ISTC programme, and shown that casemix deteriorated more at the former than at the latter. Providing evidence of such an effect of ISTC entry is important because the casemix differences between ISTCs and nearby public hospitals documented by the existing literature may simply reflect the fact that ISTCs attracted patients who would not otherwise have undergone surgery.[7]

Prospective reimbursement encourages cherry-picking, as it provides incentives for hospitals to avoid admitting patients whose cost of treatment is likely to exceed the regulated price (Allen & Gertler 1991; Ellis & McGuire 1986; Ellis 1998; Newhouse 1989). We use DiD methods to estimate the extent to which ISTCs left incumbent NHS hospitals with a sicker, costlier mix of patients, over and above any secular changes in casemix due to the introduction of PbR.[8]

Ellis (1998) and Meltzer et al. (2002) suggest that prospective reimbursement may have differential effects on risk selection practices in low-competition and high-competition markets, with hospitals in more competitive markets facing greater pressure to engage in cherry picking. To account for this possibility, we include in our regressions a control for the overall level of competition intensity faced by a hospital, to differentiate between effects of ISTC entry, and effects of the competitive environment more generally.

[7] Kelly and Stoye (2015) show that, during the 2000s, the number of NHS-funded hip replacements increased more in areas where ISTCs were established than elsewhere. They explain this relative increase by arguing that the expansion in NHS-funded capacity brought about by the ISTC programme led patients who would not otherwise have undergone a hip replacement, or who would have had the procedure performed privately, to instead have their operation performed at an ISTC and funded by the NHS. These patients newly drawn to treatment via the public system as a result of ISTC entry may have had different characteristics to those patients that were already being treated in the public system. This possibility highlights that the mere existence of differences in average patient health status between an NHS hospital and a nearby ISTC should not, in itself, be taken as evidence that the ISTC imposed negative externalities on the NHS hospital's casemix via patient selection. Rather, the presence of such a negative externality can only be demonstrated by comparing, as we do in this paper, the evolution of average patient health status at NHS hospitals that had an ISTC placed nearby, with the evolution of average patient health status at comparable NHS hospitals that did not have an ISTC placed nearby.

[8] While NHS hospitals have no formal scope to reject sicker and costlier patients, it has long been recognised that they may respond to the incentives environment by finding ways to risk select their patients (Propper et al. 2004; 2008a; Le Grand 1999; Appleby et al. 2012).

4. Empirical Strategy

Our aim is to estimate the causal effect of government-facilitated entry of private surgical centres on the efficiency, case mix, case load, and overall cost per patient of nearby incumbent public hospitals. We use difference-in-difference (DiD) regressions in which the impact of ISTC exposure is estimated from the mean change in outcomes for public hospitals in a treatment group (those that had a private surgical centre placed nearby) minus the mean change in outcomes for public hospitals in a control group (those that did not have a private surgical centre placed nearby). In this section, we outline our identification strategy, treatment assignment methodology, definition of pre/post intervention periods, and data and outcome variables.

4.1. Treatment assignment

We assign public hospitals to treatment or control groups based on their geographical proximity to the new market entrants. In particular, we assign treatments by comparing the distance from an NHS hospital to its nearest ISTC with the percentiles of distance travelled by that hospital's hip and knee replacement patients.[9] Our treatment assignment strategy is driven by the assumption that public hospitals that have a private surgical centre established in their market are 'exposed to' the ISTC programme, while hospitals that do not have a private surgical centre established in their market are not.

To capture this idea, we begin by measuring patient flows in the NHS from 2002/3 to 2004/5 – that is, prior to private surgical centre entry, and prior to patients having the opportunity to choose their provider. We identify the radius that captures 25 per cent of patient flows around each hospital (average radius 4.45 kilometres) with the patient's Lower Super Output Area (LSOA) of residence used as a proxy for home address.[10] We allocate public hospitals to a High Treatment group if they have an ISTC placed within this radius. This High Treatment group therefore encompasses public hospitals that were co-located with an ISTC, or which had an ISTC placed very close by. In addition, we allocate public hospitals to a Low Treatment group if they have an ISTC placed within

[9] Our metric is the straight-line distance between the hospital and the patient's Lower Super Output Area of residence. What matters for patients is not straight-line distance but travel time. However, cross-checking with Google Maps using Stata's traveltime command confirmed a very high correlation between our straight line distances and both distance by road (0.99) and travel time (0.93).

[10] Competition indices based on percentiles of patient distance travelled can be endogenous to hospital quality – for example, a high-quality hospital may attract patients from further afield, thus making it appear more competitive. To ameliorate concerns about potential endogeneity of our treatment assignment methodology, we use percentiles of patient distance travelled based on averages from 2002/3 to 2004/5 – that is, before the implementation of either the ISTC programme or patient choice of hospital for elective surgery. In our robustness checks, we further address this potential source of endogeneity by assigning treatments using hospital markets centred on GP surgeries rather than hospitals themselves; our results are qualitatively unchanged.

a radius that captures 95 per cent of the incumbent's referrals but not within the 25 per cent radius. Hospitals that do not have an ISTC within their 95 per cent market radius (average radius 28.05 kilometres) are allocated to our control group.

We allocate NHS hospitals to treatment categories based on percentiles of patient distance travelled, rather than on raw distance, in order to control for rural-urban differences. Treatment assignment based on fixed distances will over-estimate the level of competition in urban areas relative to rural areas, given the impact of urban congestion on travel speeds. In our robustness checks, we examine whether our results change if we use a treatment assignment strategy based on fixed distances from public hospital to ISTC.

4.2. Treatment start and end dates

The bulk of Wave 1 ISTCs were established between April 2005 and December 2006 (see Figure 1). We focus on ISTCs that opened during this period.

There is some ambiguity as to the appropriate way to define the policy-on and policy-off dates for a given public hospital exposed to ISTC entry. Some ISTCs began treating patients before their official contract start date, while others did not begin operations until six months to a year after their contracted start date. Moreover, when the initial ISTC contracts (generally around five years in length) were completed, some managed to secure further contracts, but others were shut down or absorbed into neighbouring NHS trusts. The fate of an ISTC was generally announced in the last year of the contract. Thus, if contract end date were used as treatment end date, estimates of treatment effects could be confounded by changes in behaviour due to anticipated contract completion. In response to these ambiguities, we employ a long differences specification that makes use only of data from the 2004/5 and 2008/9 financial years. We choose 2004/5 as our pre-treatment period because it is the last year before the introduction of the ISTC programme, and thus most likely to capture the effect of ISTC exposure as distinct from the effect of other elements of the government's reform programme. We choose 2008/9 as our post-treatment period to allow time for treatment effects to be realised, while avoiding contamination from responses to announcements concerning extension or non-extension of ISTC contracts.

4.3. Regression specification

We identify the impact of hospital market entry using a DiD regression framework where we interact dummy variables indicating treatment group membership with a post-policy dummy. We run regressions at the patient level and log transform any non-binary dependent variables such that the treatment effects are interpretable as percentage changes.[11]

We first run the most basic possible DiD regression. With t denoting time period (financial year), $post_t \in \{0,1\}$ denoting whether an observation occurs in the post-reform period, y_{ijt} denoting the log of the outcome variable under consideration for patient i attending hospital j at time t, and $high_j$ and low_j denoting dummies for the High and Low Treatment groups, the regression is:

$$y_{ijt} = \beta_0 + \beta_1 post_t + \beta_2 high_j + \beta_3 low_j + \beta_4 (high_j \cdot post_t) + \beta_5 (low_j \cdot post_t) + \varepsilon_{ijt} \quad (1)$$

Treatment effects are given by the coefficients on the interaction terms, β_4 and β_5. We estimate the equation using OLS, with standard errors clustered at the hospital (site) level to account for correlation in unobservables within hospitals.

In our second specification, we include hospital (site) fixed effects (μ_j) in place of the treatment group indicators to capture all time-invariant hospital and spatial characteristics, and time-period-specific (month-year) fixed effects (η_t) in place of the post-policy period dummy. Our third specification, in addition, controls for an extensive set of patient and hospital characteristics ($\mathbf{x}_{ijt}$):[12]

$$y_{ijt} = \beta_0 + \beta_1 (high_j \cdot post_t) + \beta_2 (low_j \cdot post_t) + \beta_3 (-logHHI_j \cdot post_t) + \mathbf{x'}_{\mathrm{jit}} \boldsymbol{\beta}_4 + \eta_t + \mu_j + \varepsilon_{ijt} \quad (2)$$

In our second and third specifications, we allow for market-structure-specific time trends, captured by an interaction between a measure of market competition intensity and a post-policy dummy. Controlling for market structure in this way is potentially important given that there were policy reforms that expanded patient choice of hospital for elective surgery concurrently with the rollout of the ISTC programme, and ISTCs may have entered into markets that were already more (or less) competitive. We measure competition intensity by the negative log of a hospital-specific, hospital-centred Herfindahl-Hirschman Index or HHI (sum of squared market shares), where each hospital's market is defined as the circle corresponding to the radius formed by the distance travelled for treatment by the hospital's 95th percentile orthopaedic surgery patient in the 2002/3 to 2004/5 financial years (i.e. prior to the ISTC programme).

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[11] When a variable takes a minimum value of 0, we add 1 before taking logs.

[12] Thus, while our second specification estimates the effect of ISTC exposure on public hospital performance inclusive of any effects via changing patient characteristics (e.g. due to risk selection of patients by ISTCs), our third specification seeks to estimate the effect of ISTC exposure on public hospital performance *conditional on* patient characteristics.

4.4. Data and outcome variables

Our dataset is derived from the NHS Hospital Episode Statistics (HES) (HSCIC 2016), which contains the universe of hospital admissions funded by the government in England.[13] Our data extract consists of all admissions of patients aged 55 and older for elective hip and knee replacement between the 2002/3 and 2012/13 financial years. We focus on these surgical procedures for two reasons. First, orthopaedic surgery was a major focus of the ISTC programme, as it was recognised in the early 2000s that achieving the government's waiting time targets was going to be more challenging in this surgical specialty than in any other area (Harrison & Appleby 2005). Second, clinical practice in relation to hip and knee replacements did not change significantly during this period in ways that could affect LOS.[14] As a result, any observed changes in LOS will likely be driven by NHS reforms, not by differential uptake of new medical technologies.

Measuring efficiency is a core challenge facing the literature on hospital performance. In the absence of hospital cost data, many studies use proxy measures of efficiency such as LOS (Fenn & Davies 1990; Martin & Smith 1996; Gaynor et al. 2013). The logic underlying this measure is that, if a hospital can treat patients more quickly without any deterioration in clinical quality, then it must have become more efficient.

However, a critical shortcoming of overall LOS as an efficiency measure is that recovery time after surgery is also heavily dependent on patient characteristics and health status. A hospital's average LOS may, moreover, reflect undesirable hospital behaviour such as cherry picking (prioritising treatment of less costly patients); dumping (avoiding treatment of costlier patients); and quality skimping (discharging patients 'sicker and quicker') (Epstein et al. 1990; Martin & Smith, 1996; Sudell et al. 1991).

In this study, we use an innovative method to obtain a cleaner proxy for hospital efficiency, by decomposing LOS into two parts: time from admission to surgery (pre-surgery LOS), and time from surgery until discharge (post-surgery LOS).[15] We hypothesise that, for elective orthopaedic surgery,

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[13] As such, HES should contain data on publicly funded patients treated by private providers, but data from these providers is incomplete during the period we examine (AC & HC 2008). However, this does not pose a problem for the present study, as our aim is not to compare ISTC performance with performance at public hospitals, but rather to use ISTCs as sources of variation in the competitive environment, in order to estimate the impact of private surgical centre entry on the performance of nearby public hospital incumbents.

[14] By contrast, over the period we study, there was a large increase in use, within the English NHS, of percutaneous coronary intervention (angioplasty) for treatment of myocardial infarction. As this treatment has shorter LOS than previous approaches, early adopters of this technology likely had larger decreases in LOS. We have no reason to believe that similar changes in clinical practice occurred during this period for hip and knee replacement surgery.

[15] Total LOS is calculated as day of discharge minus day of admission, pre-surgery LOS as day of surgery minus day of admission, and post-surgery LOS as day of discharge minus day of surgery. To reduce the impact of outliers and coding

pre-surgery LOS is not significantly influenced by patient characteristics, as there is rarely a clinical rationale for admitting an orthopaedic surgery patient before the day of their operation. The extent to which hospitals are able to schedule patient admissions to ensure that they line up with the availability of surgeons, support staff, and operating theatres will therefore be a direct function of the efficiency with which the hospital is run.[16]

By contrast, we view post-surgery LOS as a joint product of underlying hospital efficiency and patient characteristics. We therefore measure the effect of ISTC placement on post-surgery LOS, and interpret our estimates as the combined outcome of (i) competitive pressure brought about by ISTC entry, leading to efficiency improvements by nearby public incumbents, and (ii) ISTC risk selection, leaving nearby public hospitals with a sicker mix of patients.

To test our hypothesis that patient characteristics influence LOS by affecting post-surgery recovery time rather than time from admission to surgery, we regressed pre- and post-surgery LOS for hip and knee replacement on patient controls including age, gender, ethnicity, socioeconomic status, Charlson comorbidity score, diagnosis, and urban residence status. In line with our hypothesis, we found that patient characteristics explain only 0.8 per cent of the variation in pre-surgery LOS, but 14.2 per cent of the variation in post-surgery LOS (see Appendix for coefficients).

We test for the presence of patient risk selection by private surgical centre entrants by examining one direct measure of patient health status, and two demographic variables correlated with health status. Firstly, we calculate a measure of patient severity known as the Charlson score, which predicts a patient's 10-year survival probability based on their health status in relation to 17 conditions likely to lead to death. The score varies from 0 to 6, with 0 denoting the absence of any predictors of mortality (HSCIC 2013). As proxies for health status and clinical risk, we also use the patient's age, as well as the Index of Multiple Deprivation (Noble et al. 2004) income domain, which reports the percentage of households in the patient's LSOA of residence that are income deprived (in our dataset this variable ranges from 0 to 83).

NHS hospital trusts often consist of multiple treatment sites that can be located up to 100 kilometres away from each other. We therefore conduct our analysis at site level rather than trust level, and assign hospitals (sites) to treatment and control groups based on the site's proximity to

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errors, we restrict our sample to observations with pre-surgery LOS between 0 and 14, post-surgery LOS between 1 and 30, and total LOS between 1 and 44. These restrictions led to loss of 2.3 per cent of observations.

[16] We also estimate the effect of ISTC entry on clinical quality at nearby incumbent public hospitals, using mortality from acute myocardial infarction as the outcome variable. The findings are reported in the Appendix. They indicate that the efficiency improvements at public hospitals reported in the main body of the paper were achieved without any concomitant deterioration in clinical quality.

the nearest ISTC. After cleaning (and imputing missing values for)[17] the site code field, allocating a single site code to substantively identical sites,[18] and dropping small sites,[19] we were left with 161 public hospital sites treating orthopaedic patients in the 2004/5 and 2008/9 financial years.[20] 11 sites were assigned to the High Treatment group (with an average distance to nearest ISTC of 0.92 kilometres), 49 to the Low Treatment group (20.42 kilometres), and 101 to the Untreated group (34.52 kilometres).

Table 2 reports summary statistics for the key outcome variables examined in this paper, for the pre-policy (2004/5) and post policy (2008/9) periods used in the main regression analyses.

5. Results

5.1. Descriptive graphical evidence

Figures 2, 3 and 4 present graphical evidence demonstrating the evolution of outcome variables in our treatment and control groups. In each graph, the x-axis denotes time while the y-axis denotes the outcome variable. The solid line denotes the High Treatment group, the dashed line the Low Treatment group, and the dotted line the control group. The shaded area represents the range of treatment start dates for the Wave 1 ISTCs. We expect that any treatment effects will arise either within the shaded region or, if behavioural responses take place with a lag, some time after the shaded region. Each data point represents a month, but the plots are smoothed using a moving average of the month and the three previous quarters. The graphs plot the evolution of outcomes until the end of March 2009, the first treatment end date in our dataset.

Figure 2 shows the evolution of our key efficiency indicator – pre-surgery length of stay (LOS) for hip and knee replacement. Panels (a) and (b) show the raw levels of pre-surgery LOS. The treatment and control groups evidently differ in terms of the levels of pre-surgery LOS, indicating

[17] Unlike the HES trust code field, which is always complete, the site code field is missing in approximately 10 per cent of cases, and contains invalid data in approximately 10 per cent more. In the vast majority of such cases, however, it is possible to impute the correct site codes with certainty – for example, because only one hospital site within a trust performs a given surgical procedure. In the small number of remaining cases – around 4.4 per cent – we randomise our imputation of site codes amongst all sites in a trust that perform the procedure in question.

[18] As it is vital for our analysis to establish continuity between sites, we allocate a single site code to identical sites (when two NHS trusts merge, component sites of the trusts are generally given a new site code) or substantively identical sites (for example, two sites of an NHS trust with the same postcode). This allocation was performed manually, but was informed by the spreadsheet of predecessors of current sites that is published by the Organisation Data Service of the HSCIC (ODS 2014).

[19] We drop any site that did not treat at least 50 patients for at least one orthopaedic surgical procedure in at least one year between 2002/3 and 2012/13.

[20] We also drop from our dataset any NHS hospital site that had an ISTC within its 95 per cent market radius, and whose closest ISTC was a Phase 2 ISTC. We do this to prevent our estimates from being contaminated by the impact of this later phase of ISTC expansion.

that ISTCs entered in areas where hospitals were already getting patients into surgery quicker. However, the crucial identifying assumption for our DiD analysis is that the counterfactual *trends* in outcomes for treatment and control groups are similar. To facilitate a comparison of pre- and post-reform trends, Panels (c) and (d) present pre-surgery LOS again, after normalising each group's period-specific value by subtracting the pre-treatment average. Pre-reform trends appear parallel across all treated and control groups, with no evidence of effects before or after the announcement of ISTC locations in September 2003. There is no evidence that ISTCs were targeted to areas where hospitals were already experiencing improvements in pre-surgery LOS. There is a general downward trend for both groups, reflecting general improvements in turnaround time. Over and above this secular downward trend, however, there is evidence of a treatment effect from ISTC placement. Around the middle of the 'treatment on' period, trends diverge, and by the end of the treatment period the reduction in pre-surgery LOS is notably larger for the High Treatment group than for the control group. There also appears to be a smaller effect for the Low Treatment group. Figure 2 provides visual evidence that the entry of private specialty surgical centres in the English NHS made nearby public hospitals more efficient, by reducing pre-surgery delays.

The picture for post-surgery and total LOS in Figure 3 is markedly different. As discussed in Section 4.4, post-surgery LOS will be influenced both by changes in hospital efficiency due to increased competitive pressure from the entry of private surgical centres, and by changes in patient characteristics due to risk selection by entrants. As panels (a) and (b) illustrate, for both procedures, pre-reform levels are similar, trends appear parallel, and post-surgery LOS decreases in the High Treatment group *less* than for the control group after ISTC entry. As illustrated in panels (c) and (d), total LOS follows a similar pattern. Figure 3 provides suggestive evidence that the negative impact of ISTC risk selection on nearby public hospitals' LOS may have outweighed any efficiency improvements with respect to LOS arising from competitive pressure from these new market entrants.

Figure 4 looks more directly at the impacts of ISTC entry on public hospitals' case mix by plotting the evolution of outcomes for the Charlson score.[21] Both panels suggest that hospitals in the High Treatment group started receiving sicker patients from near the beginning of the shaded area. Pre-policy levels of the Charlson score are similar across treatment and control groups, and pre-reform trends are parallel for hip patients. For knee patients, the pre-reform trend is, if anything, slightly flatter in High Treatment group hospitals relative to the controls. After ISTC entry, there is a large increase in Charlson scores in High Treatment group hospitals relative to other groups. This

[21] Graphical evidence for other casemix variables is provided in the Appendix.

observation is consistent with our hypothesis that selection of less risky patients by ISTCs left a residual pool of higher risk patients to be treated by public hospitals.

Overall, the similar pre-policy trends in treatment and control groups for all outcome variables provides strong support for our argument that DiD estimates are likely to provide an unbiased estimate of treatment effects from ISTC entry. The fact that pre-policy trends (and in many cases levels) of our outcome variables are similar across treated and untreated groups is consistent with our argument that the principal target of ISTC placement, and health policy generally in England at the time, was to reduce waiting times for admission to hospital – not to reduce time spent in hospital, or to improve clinical quality. As Table 3 illustrates, average waiting times in 2002/3, when ISTC placement decisions were being made, were substantially longer at High Treatment group hospitals than elsewhere. By contrast, Table 3 shows that there is no systematic relationship between total lengths of stay in 2002/3 at High Treatment group, Low Treatment group, and control group hospitals.[22]

5.2. Regression-based difference-in-difference estimates

Table 4 and Table 5 present estimates from our key DiD regressions of the effect of ISTC entry on LOS and case mix. Both tables show regression coefficients and standard errors (clustered at hospital site level). These empirical estimates mirror results presented in Figures 2, 3 and 4. The key coefficients are in the first two rows and show the estimated impact of surgical centre entry in the High Treatment group (ISTC within the hospital's 25th percentile patient travel radius) and Low Treatment group (ISTC between the 25th percentile and 95th percentile patient travel radius) relative to untreated (outside the 95th percentile patient travel radius). Given the similarities in the patterns

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[22] To further investigate the determinants of ISTC placement decisions, we use a logit model to predict ISTC placement (assignment to the High Treatment group) using a procedure-hospital-level dataset derived from 2002/3 data, and omitting hospitals with extreme average waits to mitigate the effect of outliers (see Appendix for full description and regression outputs). The results show that average waiting times in 2002/3 significantly predict ISTC placement (*z*-statistic = 2.44, pseudo R-squared from univariate regression = 0.036), while average total length of stay has no similar predictive power (*z*-statistic = -0.76, pseudo R-squared from univariate regression = 0.004). We take this as evidence that ISTC placement decisions were driven by nearby hospitals' waiting times, and that nearby hospitals' efficiency as captured by total LOS played no role in these decisions.

The fact that there are pre-treatment differences in average waits between treatment and control groups might cause concerns about the comparability of these groups. However, so long as hospital performance in relation to waiting times (and any other determinants of ISTC placement) is not correlated with performance in relation to LOS, allocation to treatments will (conditional on the control variables included in our regressions) be as good as random as far as our LOS measures are concerned. The correlation between log of waiting time and log of total LOS for orthopaedic surgery in 2002/3 is 0.0047. Simple bivariate regressions of the log of waiting time on the log of total LOS during this period yield a tiny and statistically insignificant coefficient (0.0113, *t*-statistic 1.12) (see Appendix). We therefore conclude that there was a weak or non-existent relationship between these two dimensions of hospital performance during the period when ISTC placement decisions were being made. We take this as evidence that selection for ISTC placement on the basis of the average waiting times of nearby NHS hospitals does not imply selection, via correlation, on the basis of nearby hospitals' average LOS.

for hip and knee replacement surgery in Figures 2, 3 and 4, we pool the data on both orthopaedic procedures in the regression analysis presented in this section, and include a hip replacement dummy to account for level differences between the two procedures. The Appendix presents estimates when regressions are run separately for hip and knee patients.

We look first at the impact of ISTC entry on measures of LOS, in Table 4. Columns (1) to (6) investigate the effects on pre-surgery LOS – our preferred measure of hospital efficiency – either in log days (Columns (1)-(3)) or in terms of an indicator of surgery on day of admission (pre-surgery LOS equal to zero) (Columns (4)-(6)). Columns (7) to (9) present results relating to log of post-surgery LOS. The first of each set of three columns is the basic DiD regression of Equation (1) with no additional control variables. The second column of each set introduces hospital fixed effects in place of the treatment group dummies, a full set of month-year dummies, plus a control for overall market competitiveness (Equation (2) with no patient controls). The third column of each set also includes controls for patient characteristics.

Columns (1) and (4) of Table 4 show that, for the High Treatment group, ISTC entry led to a 14 per cent (= $100(e^{-0.153}-1)$) reduction in pre-surgery LOS, or a 21 percentage point increase in the proportion of patients treated on day of admission, significant at the 5 per cent level. Controlling for site fixed effects and market structure trends (and, optionally, for a wide range of patient characteristics, listed in the table notes) increases this estimate slightly, giving a 16 per cent reduction in pre-surgery LOS or a 24 percentage point increase in the proportion treated on day of admission. Importantly, controlling for patient characteristics barely shifts the estimated treatment effects for the High Treatment group, suggesting that there is little selection into treatment on the basis of these observable demographic characteristics; this, in turn, implies that there is likely to be little selection into treatment on the basis of unobservable patient characteristics (Altonji et al. 2008). The impact on the Low Treatment group is of the same sign and around one third of the High Treatment group effect, but is imprecisely measured and never significant at conventional levels. The most likely interpretation is that there were moderate impacts of ISTC entry on the Low Treatment group, but that our research design does not have sufficient power to detect them.

Column (7) presents DiD estimates of the impact of ISTC entry on post-surgery LOS. In line with Figure 3, the estimate for the High Treatment group is positive, indicating an 8.5 per cent increase in post-surgery LOS (around half a day on average) significant at the 10 per cent level. The effect of ISTC entry on hospitals in the Low Treatment group is smaller and insignificant. However, when we control for hospital fixed effects and market-structure-related trends in Column (8), the effect for the High Treatment group diminishes and becomes statistically insignificant, although it is still substantial in magnitude, implying an increase in post-surgery LOS of 5 per cent (or around a third of a day). The effect in the Low Treatment group increases in size and becomes significant at the 10 per cent level. Controlling in addition for patient characteristics in Column (9) renders all the treatment effects statistically insignificant.

As set out in Section 4.4, we interpret changes in post-surgery LOS resulting from ISTC entry as a joint product of (i) compositional changes in the patients being treated by public hospitals, due to risk selection by neighbouring ISTCs, and (ii) behavioural responses by public hospital managers and clinicians to competition from new private entrants. Although only significant at the 10 per cent level, our estimates in Column (7) – ISTC exposure leads to longer post-surgery LOS – suggest that the first effect (which implies longer post-surgery LOS) dominated the second effect (which implies shorter post-surgery LOS). The fact that controlling for patient characteristics wipes out any evidence of changes in post-surgery LOS is consistent with this interpretation that the estimated effect of ISTC entry on this outcome variable is driven by ISTC cherry picking.

Unlike our estimates for pre-surgery LOS, which barely change when patient characteristics are included, our High Treatment group estimates for post-surgery LOS halve in magnitude when patient controls are added. We take this as further evidence that patient characteristics are a major driver of post-surgery LOS, but have a vastly smaller influence on pre-surgery LOS.

We address the question of casemix directly in Table 5, which shows the effect of ISTC entry on indicators of patient risk: log of the Charlson score (as depicted in Figure 4); an indicator for patients with a Charlson score equal to three or more; log of IMD income deprivation score; and log of patient age. Columns (1) to (4) show that ISTC entry left High Treatment group hospitals with a more severely ill mix of patients. ISTC exposure led High Treatment group hospitals to experience a 12.3 per cent deterioration in patient health status, or a 6.56 percentage point increase in the proportion of patients with a Charlson score of three or more. Both effects are significant at the 5 per cent level in both the basic DiD and fixed effects specifications (since we are estimating the impact of ISTC entry on patient casemix, we do not control for other patient characteristics in these regressions).

We also find an effect of ISTC exposure on patient deprivation, with Column (5) showing a 5.56 per cent increase in the IMD income deprivation score in the High Treatment group, significant at the 5 per cent level. This finding is not, however, robust to inclusion of site fixed effects and market-structure-related time trends in Column (6). Columns (7) and (8) report negligible and insignificant effects of ISTC entry on patient age.

5.3. Robustness checks

In this section, we show that our main results are robust to a range of alternative specifications. Table 6 reports the coefficient estimates of interest from our robustness checks, all of which are performed, except where explicitly noted otherwise, using our headline specification (Equation (2) without patient controls), with hospital fixed effects, month-year dummies, and a control for overall competition intensity, with 2004/5 as the pre-reform year and 2008/9 as the post-reform year.

Row (1) reports estimates from a placebo DiD regression with 2002/3 as the pre-reform year and 2004/5 (the last year before entry of the first ISTCs in our dataset) as the post-reform year. The results confirm the plausibility of the parallel trends assumption: none of our placebo treatment effects are statistically significant.

Row (2) reports estimates when we adjust for differences in pre-reform trends between treated and control groups, by subtracting the pre-reform trend between 2002/3 and 2004/5 (as captured by Row (1)) from the headline DiD estimates reported in Table 7.[23] If pre-reform trends are exactly parallel across treated and control groups, this specification should yield identical estimates to our headline DiD specification. In line with this intuition, the estimates are similar to our main results.

Row (3) reports estimates when our regressions are run on four years of data rather than two – 2003/4-2004/5 are used as pre-treatment periods, and 2007/8-2008/9 are used as post-treatment periods.[24] Our estimates for pre-surgery LOS (and percentage of patients treated on day of admission) remain significant at the 5 per cent level, and are of similar magnitude, but our estimate of the effect of ISTC entry on the Charlson score is no longer significant. This is most likely a result of the flatter pre-reform trend for this outcome variable at High Treatment group hospitals in relation to knee replacement patients; using pre-reform data from 2003/4 as well as from 2004/5 therefore imparts a downward bias to our estimates.

Row (4) reports estimates using a continuous measure of treatment intensity based directly on percentiles of patient distance travelled. Instead of dividing ISTC-exposed hospitals into two discrete treatment groups, we define treatment intensity as 100 minus the percentile patient equivalent. That is, if an ISTC is located as far away as the 20th percentile of patient distance travelled, the hospital is assigned a treatment intensity of 80.[25] Using this specification, our treatment effects for pre-surgery LOS (and percentage treated on day of admission) are no longer statistically significant. By contrast, treatment effects for the Charlson score remain significant at

[23] If $\Delta T_{x\text{-}y}$ and $\Delta C_{x\text{-}y}$ denote the change in outcomes between years x and y at the treatment and control groups respectively, our headline specification, Equation (2), is essentially estimated as $(\Delta T_{2004/5\text{-}2008/9} - \Delta C_{2004/5\text{-}2008/9})$. By contrast, Row (2) is essentially estimated as $(\Delta T_{2004/5\text{-}2008/9} - \Delta C_{2004/5\text{-}2008/9}) - (\Delta T_{2002/3\text{-}2004/5} - \Delta C_{2002/3\text{-}2004/5})$.

[24] This specification can be understood as having an opposite effect to adjusting for differences in pre-reform trends as in Row (2), in the sense that, if the treatment effect is positive and pre-reform trends are flatter for the treatment group than for the control group, the specification in Row (3) will yield less statistically significant estimates than a standard DiD specification using only data from the last pre-reform year, while the specification in Row (2) will yield more significant estimates.

[25] To incorporate our assumption that hospitals with no ISTC in their 95 per cent market radius are completely untreated (which implies that all such hospitals should have the same treatment intensity), we subtract 3 from this continuous measure of treatment intensity, and set a minimum value of 1, so that a treatment intensity of 1 corresponds to 'Untreated', while a treatment intensity of 2 corresponds to a hospital with an ISTC in its 95 per cent market radius but not its 94 per cent market radius, and so on.

the 10 per cent level, while treatment effects for post-surgery LOS are now significant at the 5 per cent level: a ten per cent increase in treatment intensity leads to a 0.22 per cent increase in post-surgery LOS. These findings suggest that the negative impacts of ISTC risk selection (affecting incumbents' case mix and overall costs per patient) may have been relatively widely felt, whereas the positive competitive effects of ISTC entry on incumbents' efficiency may have been restricted to a smaller geographical area.

Row (5) reports estimates using a treatment assignment strategy that centres hospital markets on GP surgeries rather than hospitals. Hospital-centred measures of market size based on percentiles of patient distance travelled are endogenous to hospital performance. While we address this concern by basing our treatment assignment and competition indices on percentiles of patient distance travelled between 2002/3 and 2004/5 – before the introduction of patient choice of hospital or the ISTC programme – concerns may remain. To address these concerns, in this check we assign treatments by constructing a list of all the NHS hospitals and ISTCs that fall within each GP surgery's market (95th percentile of distance from GP surgery to treatment site). If an ISTC is in 95 per cent of the GP surgery markets that an NHS hospital falls within, that NHS hospital is assigned to the High Treatment group. If an ISTC is in 75 per cent of the GP surgery markets that an NHS hospital falls within, but not 95 per cent, that NHS hospital is assigned to the Low Treatment group. All other NHS hospitals are assigned to the control group. For this check, GP-centred competition indices based on Cooper et al. (2011) are also used. The estimates reported in Row (5) are consistent with our headline results, providing evidence that our findings are not driven by our assignment of treatments based on hospital-centred market definitions.

Row (6) reports estimates when we assign treatments based on fixed distances from NHS hospital to ISTC – a hospital is assigned to the High Treatment group if it has an ISTC enter within 5km, to the Low Treatment group if it has an ISTC enter within 30km but not within 5km, and to the Untreated group if it does not have an ISTC enter within 30km. Our results are qualitatively unchanged.

Studies of hospital competition in the English NHS have been criticised on the grounds that they simply pick up systematic differences between hospitals in London and elsewhere. Our inclusion of hospital fixed effects should control for level effects due to location in London, but one might also be concerned about possible bias due to differential trends between London and elsewhere. Row (7) shows that our results are robust to the inclusion of a London differential trend term. Row (8) shows that our results are also robust to dropping all London hospitals from our sample.

Row (9) reports estimates when outcome variables are measured in levels rather than logs. We continue to find that entry of an ISTC leads nearby public hospitals to become more efficient, but also leaves them with sicker patients.

The Appendix reports on a number of other robustness checks, which provide further confirmation that our results are robust to a wide range of specifications.

5.4. Patient volumes

Increases in local capacity could potentially impact on public hospital efficiency by reducing congestion and overcrowding, independently of any competitive pressure exerted by entrants. Moreover, public hospitals located near new private entrants may have experienced a reduction in demand. Any resulting reduction in case loads at nearby public hospitals could affect average pre-surgery LOS at these incumbents, given the important influence of volume on efficiency.

We therefore look at the impact of ISTC exposure on (log of) case load in Table 7. As before, the first column is the pure DiD specification, while the second column controls for site fixed effects and market-structure-related trends. We find no strong evidence of caseload reductions in our High Treatment group, which had the biggest reductions in pre-surgery LOS in Table 4. This suggests that ISTC exposure led to shorter pre-surgery LOS in close-neighbouring hospitals without any reduction in the volumes being treated. There are, however, significant reductions in the volume of patients being treated at Low Treatment group hospitals. This finding seems to suggest that ISTC entry did not simply add to overall clinical capacity, but, at least to some extent, reduced patient volume at public hospitals with which they shared a market – although these patients seem not to have been drawn from the closest hospitals (i.e. those in the High Treatment group) in which we observe statistically significant reductions in pre-surgery LOS. Although these findings do not completely rule out the possibility that efficiency improvements arose partly through local increases in capacity, they are broadly supportive or our conjecture that the reductions in pre-surgery LOS arose primarily through competitive incentives.

6. Discussion and Conclusions

In this paper, we examine the effect of increased competition from private specialty surgical centres on incumbent public hospitals' efficiency, case mix and case load by examining the government-facilitated entry of a series of Independent Sector Treatment Centres within the English NHS during the 2000s. We exploit the fact that ISTC location decisions were driven by local waiting times, not by other hospital characteristics such as LOS or clinical performance, to construct treatment and control groups that are comparable with respect to the outcome variables we study. Indeed, we demonstrate that trends of key outcome variables – including pre-surgery LOS, post-surgery LOS, and various measures of patient casemix – were the same for public hospitals that had an ISTC enter nearby as for those that did not.

We find that public hospitals that had a Wave 1 ISTC enter in their immediate vicinity experienced substantial reductions in pre-surgery length of stay for hip and knee replacement surgery. The addition of an ISTC to a public hospital's immediate neighbourhood leads to a decrease in pre-surgery LOS of around 16 per cent – or a 24 percentage point increase in the proportion of patients treated on the day of admission. Given that these faster turnaround times were achieved without additional expenditure (as they occurred in an environment with fixed payments per procedure), they suggest that hospitals exposed to the ISTC programme became more efficient.

As well as investigating possible positive effects of private surgical centre entry on the efficiency of incumbent public hospitals, we looked for evidence of possible negative effects in the form of worsened casemix and longer post-surgery length of stay resulting from ISTC cherry picking. We find that ISTC entry led to increases in post-surgery LOS at exposed public hospitals. We take this as suggestive evidence that any cost reductions due to increased efficiency at High Treatment group hospitals as a result of competitive pressure from private surgical centre entrants was outweighed, at least as far as length of stay is concerned, by cost increases due to worsened casemix. Indeed, we find that ISTC entry led nearby public hospitals to experience a 12.3 per cent increase in average patient severity as captured by the Charlson score – or a 6.56 percentage point increase in the proportion of patients with a Charlson score of three or more – and a 5.56 per cent increase in income deprivation in the patient's area of residence.

In principle, this sorting of patients between private surgical centres and public hospitals could represent an efficiency-improving division of responsibility if reimbursement rates were appropriately adjusted for patient severity to compensate public hospitals for their sicker casemix. However, NHS reimbursement rates during the period we study (HRG versions 3.1 and 3.5) did not adequately adjust for patient severity. This situation not only provided private surgical centre entrants with an added impetus to risk select – but it also meant that nearby incumbent public hospitals were left treating a costlier mix of patients without adequate financial compensation. While the prospective reimbursement regime (Payment by Results) was updated in April 2009 to include a more dramatic adjustment for patient severity (HRG version 4.0), Mason et al. (2008) note that providers were still likely underpaid for treating riskier patients. Indeed, Mason et al. (2008, p.34) articulate a widespread scepticism that prospective reimbursement regimes will ever be able to completely adjust for patient casemix when they say that the "HRG system is unable (and probably never will be able) to finely differentiate between the types of patient treated in each setting."

Our work suggests that facilitating the entry of for-profit surgical centres can stimulate improvements in incumbent hospitals' performance. However, these benefits must be weighted against potential risk selection. Public and private hospitals can have very different objective functions. Absent suitably risk-adjusted payments, for-profit firms may take steps to attract patients that are less costly to treat, leaving incumbents with a riskier casemix. We believe that these findings offer insights into many situations involving provision of public services where profitability is influenced by characteristics of the recipient: namely that the case for increased private sector involvement should depend, in part, on comparison of the benefits of increased competitive pressure, with the costs arising from risk selection by private entrants.

Our findings raise a key question: is it possible to reap the positive effects of increased competition resulting from expanded independent sector provision within the NHS, without the negative effects? It is likely that the answer to this question depends on whether it is possible to risk-adjust payments and outcome measures sufficiently to ensure that independent sector providers have an incentive to make profits by raising efficiency and clinical quality, not by selecting against certain patients.

Tables

Table 1: ISTC activity by financial year and orthopaedic procedure

Financial year	Number of procedures: Hip replacement	Percentage of all NHS procedures: Hip replacement	Number of procedures: Knee replacement	Percentage of all NHS procedures: Knee replacement	Number of procedures: Combined	Percentage of all NHS procedures: Combined
2005/6	397	0.75	492	0.84	889	0.80
2006/7	1771	3.11	2009	3.20	3780	3.15
2007/8	4038	6.58	4956	7.01	8994	6.81
2008/9	4954	7.52	5970	7.83	10924	7.68
2009/10	3321	5.19	4051	5.77	7372	5.50
2010/11	5088	7.48	5756	7.86	10844	7.68
2011/12	4981	7.00	5567	7.32	10548	7.17
2012/13	4731	6.64	5164	6.87	9895	6.76
Total	29281	5.72	33965	6.03	63246	5.88

Table reports ISTC patient numbers as a share of total NHS patients for orthopaedic surgery in the years 2005/6 to 2012/13. The actual share of patients attending ISTCs will be somewhat higher due to incomplete submission of HES data by ISTCs before 2010/11 (HC 2008).

Table 2: Summary statistics

Variable	Procedure	Year	Mean	Standard Deviation	Min	Max	Number of Observations
Pre-surgery LOS (Days)	Hip Replacement	2004/5	0.87	0.76	0	14	31,113
Pre-surgery LOS (Days)	Hip Replacement	2008/9	0.36	0.71	0	14	34,871
Pre-surgery LOS (Days)	Knee Replacement	2004/5	0.84	0.70	0	14	35,616
Pre-surgery LOS (Days)	Knee Replacement	2008/9	0.32	0.63	0	14	41,668
Indicator: Treated on Day of Admission	Hip Replacement	2004/5	0.22	0.41	0	1	31,208
Indicator: Treated on Day of Admission	Hip Replacement	2008/9	0.68	0.46	0	1	34,916
Indicator: Treated on Day of Admission	Knee Replacement	2004/5	0.23	0.42	0	1	35,684
Indicator: Treated on Day of Admission	Knee Replacement	2008/9	0.71	0.45	0	1	41,722
Post-surgery LOS (Days)	Hip Replacement	2004/5	8.01	4.23	1	30	30,610
Post-surgery LOS (Days)	Hip Replacement	2008/9	6.42	3.92	1	30	34,513
Post-surgery LOS (Days)	Knee Replacement	2004/5	7.71	4.12	1	30	35,245
Post-surgery LOS (Days)	Knee Replacement	2008/9	6.15	3.67	1	30	41,399
Total LOS (Days)	Hip Replacement	2004/5	9.12	5.10	1	44	30,915
Total LOS (Days)	Hip Replacement	2008/9	6.96	4.68	1	44	34,736
Total LOS (Days)	Knee Replacement	2004/5	8.72	4.75	1	44	35,492
Total LOS (Days)	Knee Replacement	2008/9	6.58	4.20	1	44	41,575
Charlson Score	Hip Replacement	2004/5	0.86	1.81	0	6	31,208
Charlson Score	Hip Replacement	2008/9	1.16	2.05	0	6	34,916
Charlson Score	Knee Replacement	2004/5	0.94	1.85	0	6	35,684
Charlson Score	Knee Replacement	2008/9	1.26	2.08	0	6	41,722
IMD Income Deprivation Score	Hip Replacement	2004/5	11.07	8.88	0	70	31,165
IMD Income Deprivation Score	Hip Replacement	2008/9	12.53	9.44	0	83	34,861
IMD Income Deprivation Score	Knee Replacement	2004/5	12.10	9.65	0	79	35,635
IMD Income Deprivation Score	Knee Replacement	2008/9	13.95	10.45	1	83	41,646
Age	Hip Replacement	2004/5	71.35	8.24	55	97	31,208
Age	Hip Replacement	2008/9	71.60	8.36	55	100	34,916
Age	Knee Replacement	2004/5	71.47	7.93	55	97	35,684
Age	Knee Replacement	2008/9	71.05	8.07	55	99	41,722
Case Load (Episodes)	Hip Replacement	2004/5	353.06	217.15	1	950	31,208
Case Load (Episodes)	Hip Replacement	2008/9	429.18	268.41	1	1212	34,916
Case Load (Episodes)	Knee Replacement	2004/5	372.92	225.10	1	993	35,684
Case Load (Episodes)	Knee Replacement	2008/9	470.01	281.56	2	1442	41,722
Negative Log HHI	Hip/Knee Replacement	2008/9	0.983	0.882	0	4.20	76,638

Table reports summary statistics for all outcome variables plus Negative Log HHI. Negative Log HHI captures overall competition intensity after the introduction of patient choice of hospital (i.e. in our post-reform year, 2008/9), and is calculated as the negative log of the Herfindahl-Hirschman Index or HHI (sum of squared market shares) with market size defined by the 95^{th} percentile of patient distance travelled, averaged across all hip and knee replacement patients in financial years 2002/3 through to 2004/5 (the pre-reform period). Case load measures number of patients per hospital site and year, and is measured by finished consultant episodes. We restrict our sample to hospitals that had at least 50 hip replacement cases or at least 50 knee replacement cases in at least one financial year between 2002/3 and 2012/13. Within this sample, hospital case loads are lower than 50 in some financial years.

Table 3: Policy targeting: average waiting times and length of stay for treated and control groups, 2002/3

	High Treatment group	Low Treatment group + Untreated	Low Treatment group	Untreated
Average Waiting Time, Hip Replacement	253.5	238.0	232.0	240.9
Average Waiting Time, Knee Replacement	288.7	271.5	261.6	276.4
Total Length of Stay, Hip Replacement	10.52	10.53	10.26	10.65
Total Length of Stay, Knee Replacement	9.91	10.02	9.85	10.10

Table reports average values of waiting times and length of stay in 2002/3 for treatment and control groups.

Table 4: Impact of ISTC entry on orthopaedic surgery length of stay at nearby public hospitals. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1) Log of pre-surgery LOS	(2) Log of pre-surgery LOS	(3) Log of pre-surgery LOS	(4) % treated on day of admission	(5) % treated on day of admission	(6) % treated on day of admission	(7) Log of post-surgery LOS	(8) Log of post-surgery LOS	(9) Log of post-surgery LOS
Post × High treatment	-0.153** (0.0652)	-0.172** (0.0679)	-0.178*** (0.0682)	0.213** (0.0904)	0.239** (0.0939)	0.244** (0.0941)	0.0812* (0.0485)	0.0495 (0.0498)	0.0255 (0.0444)
Post × Low treatment	-0.0447 (0.0479)	-0.0579 (0.0545)	-0.0591 (0.0551)	0.0641 (0.0680)	0.0857 (0.0781)	0.0868 (0.0787)	0.0493 (0.0311)	0.0565* (0.0303)	0.0498 (0.0316)
High treatment	0.0287 (0.0447)	-	-	-0.0369 (0.0596)	-	-	0.0494 (0.0576)	-	-
Low treatment	0.0136 (0.0409)	-	-	-0.0204 (0.0572)	-	-	7.0e-3 (0.0255)	-	-
Post	-0.314*** (0.0269)	-	-	0.436*** (0.0378)	-	-	-0.263*** (0.0190)	-	-
Post×Neg log HHI	-	-6.9e-3 (0.0243)	-6.4e-3 (0.0246)	-	6.5e-3 (0.0344)	5.8e-3 (0.0347)	-	-5.5e-3 (0.0176)	-6.4e-3 (0.0171)
Hip	0.0167*** (4.1e-3)	0.0118*** (2.9e-3)	0.0636*** (0.0177)	-0.0192*** (5.7e-3)	-0.0121*** (3.9e-3)	-0.0720*** (0.0236)	0.0405*** (7.6e-3)	0.0468*** (7.4e-3)	0.273*** (0.0231)
Site fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Month-year dummies	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Patient controls	No	No	Yes	No	No	Yes	No	No	Yes
Patient obs	143,268	143,268	143,046	143,530	143,530	143,307	141,767	141,767	141,549
R-squared	0.201	0.388	0.396	0.227	0.441	0.447	0.064	0.121	0.260

Table reports regression coefficients and standard errors clustered at the hospital level. The coefficients on (Post × High treatment) and (Post × Low treatment) give treatment effects for the High Treatment group and Low Treatment group respectively. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. Patient control variables in Columns (3), (6) and (9) include Charlson score; number of diagnoses; IMD income deprivation score; IMD health and disability deprivation score; dummy variables indicating HRG codes F41, H72, H80; dummy variables indicating self-discharge, revision to hip replacement, urban residence, mixed ethnicity, Asian ethnicity, black ethnicity, other ethnicity, and unknown ethnicity; and a full set of casemix dummies with gender interacted with five-year age bins. The full set of coefficients is reported in the Appendix.

Table 5: Impact of ISTC entry on orthopaedic surgery case mix at nearby public hospitals. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1) Log Charlson score	(2) Log Charlson score	(3) Charlson score of 3 or more	(4) Charlson score of 3 or more	(5) Log IMD income deprivation index	(6) Log IMD income deprivation index	(7) Log age (coefficients x 100)	(8) Log age (coefficients x 100)
Post × High treatment	0.116** (0.0501)	0.116** (0.0500)	0.0656** (0.0287)	0.0653** (0.0287)	0.0541** (0.0254)	0.00689 (0.0252)	0.232 (0.272)	0.154 (0.226)
Post × Low treatment	-0.00445 (0.0242)	0.0161 (0.0252)	-0.00102 (0.0143)	0.0109 (0.0148)	-0.0272 (0.0262)	-0.0242 (0.0163)	0.165 (0.181)	0.125 (0.201)
High treatment	-0.0101 (0.0465)	-	-0.00679 (0.0275)	-	-0.158* (0.0844)	-	-0.172 (0.305)	-
Low treatment	-0.0112 (0.0271)	-	-0.00775 (0.0162)	-	-0.0139 (0.0601)	-	-0.498* (0.253)	-
Post	0.106*** (0.0173)	-	0.0618*** (0.0102)	-	0.153*** (0.0115)	-	-0.257** (0.111)	-
Post × Neg log HHI	-	-0.0154 (0.0129)	-	-0.00863 (0.00755)	-	0.00755 (0.00790)	-	7.84e-04 (0.0924)
Hip	-0.0406*** (0.00535)	-0.0409*** (0.00499)	-0.0276*** (0.00310)	-0.0277*** (0.00288)	-0.0818*** (0.00717)	-0.0524*** (0.00483)	0.293*** (0.0855)	0.212** (0.0848)
Site fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Month-year dummies	No	Yes	No	Yes	No	Yes	No	Yes
Patient controls	No	No	No	No	No	No	No	No
Patient obs	143,530	143,530	143,530	143,530	143,307	143,307	143,530	143,530
R-squared	0.008	0.035	0.008	0.034	0.019	0.174	0.001	0.011

Table reports regression coefficients and standard errors clustered at hospital level. For further information, see Table 4 notes.

Table 6: Impact of ISTC entry on orthopaedic outcomes at nearby public hospitals: robustness tests.

		(1) Log of pre-surgery LOS	(2) % treated on day of admission	(3) Log of post-surgery LOS	(4) Log of Charlson Score
(1) Pre year 2002/3, post year 2004/5	Post × High treatment	0.0296 (0.0269)	-0.0414 (0.0388)	0.000657 (0.0358)	0.00124 (0.0280)
	Post × Low treatment	0.00582 (0.0222)	-0.0105 (0.0300)	0.00461 (0.0187)	-0.0185 (0.0191)
(2) Adjust for differences in pre-reform trend (2002/3 to 2004/5)	Post × High treatment	-0.224** (0.0896)	0.312** (0.124)	0.0514 (0.0763)	0.136** (0.0633)
	Post × Low treatment	-0.0634 (0.0586)	0.0991 (0.0835)	0.0177 (0.0386)	0.0219 (0.0365)
(3) Pre years 2003/4 & 2004/5, post years 2007/8 & 2008/9	Post × High treatment	-0.151** (0.0695)	0.208** (0.0953)	0.0685 (0.0547)	0.0880 (0.0534)
	Post × Low treatment	-0.0691 (0.0544)	0.0994 (0.0777)	0.0585** (0.0278)	0.00595 (0.0198)
(4) Continuous treatment intensity	Post × Treatment intensity	-0.0253 (0.0165)	0.0356 (0.0232)	0.0217** (0.00889)	0.0141* (0.00811)
(5) GP-centred treatment assignment	Post × High treatment	-0.167** (0.0763)	0.230** (0.103)	0.0455 (0.0462)	0.179*** (0.0333)
	Post × Low treatment	-0.0187 (0.0523)	0.0288 (0.0740)	-0.00200 (0.0343)	-0.0260 (0.0252)
(6) Treatment assignment & competition index using fixed distances (5km-30km)	Post × High treatment	-0.157** (0.0702)	0.216** (0.0972)	0.0372 (0.0515)	0.111** (0.0513)
	Post × Low treatment	-0.0102 (0.0445)	0.0174 (0.0635)	0.0222 (0.0304)	0.00690 (0.0247)
(7) London differential trend	Post × High treatment	-0.166** (0.0674)	0.229** (0.0929)	0.0480 (0.0498)	0.112** (0.0500)
	Post × Low treatment	-0.0489 (0.0543)	0.0724 (0.0779)	0.0553* (0.0310)	0.0105 (0.0248)
(8) No London	Post × High treatment	-0.168** (0.0708)	0.234** (0.0977)	0.0470 (0.0530)	0.115** (0.0527)
	Post × Low treatment	-0.0439 (0.0582)	0.0672 (0.0836)	0.0670** (0.0329)	0.0146 (0.0265)
(9) Outcomes in levels not logs	Post × High treatment	-0.259** (0.104)	-	0.200 (0.400)	0.327** (0.139)
	Post × Low treatment	-0.0828 (0.0796)	-	0.431** (0.205)	0.0353 (0.0681)

Table reports robustness tests based on the 'headline' regression specification (Equation (2) without patient controls), with hospital (site) fixed effects, a full set of month-year dummies, and controls for overall market competitiveness. The coefficients on (Post × High treatment) and (Post × Low treatment) give treatment effects for the High Treatment group and Low Treatment group respectively. Standard errors clustered at the hospital level are reported in parentheses. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. Unless otherwise noted, all tests reported in this table use 2004/5 as the pre-reform period and 2008/9 as the post-reform period. Row (1) is a placebo regression with 2002/3 designated as the pre-reform year and 2004/5 as the post-reform year. Row (2) adjusts for differences in pre-reform (2002/3 to 2004/5) trends between treated and control groups. Row (3) uses four years of data rather than two – 2003/4-2004/5 for the pre-reform period, and 2007/8-2008/9 for the post-reform period. Row (4) uses a continuous measure of treatment intensity instead of dividing up ISTC-exposed hospitals into two discrete treatment groups. Row (5) uses a GP-centred treatment assignment strategy and GP-centred competition index. Row (6) assigns treatments and constructs a competition index using fixed distances from public hospital to ISTC (High Treatment group = ISTC within 5km; Low Treatment group = nearest ISTC is between 5km and 30km; Untreated group = no ISTC within 30km). Row (7) includes a London differential trend term. Row (8) drops all London hospitals from the sample. Row (9) runs the regression on levels of the outcome variable rather than logs.

Table 7: Impact of ISTC entry on patient volumes at nearby public hospitals. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1) Log of orthopaedic case-load	(2) Log of orthopaedic case-load
Post × High treatment	-0.00516	-0.0772
	(0.0981)	(0.0898)
Post × Low treatment	-0.158**	-0.161***
	(0.0704)	(0.0580)
High treatment	-0.0362	-
	(0.121)	
Low treatment	0.166	-
	(0.141)	
Post	0.273***	-
	(0.0353)	
Post × Negative log HHI	-	0.0263
		(0.0292)
Hip	-0.0913***	-0.121***
	(0.0210)	(0.0160)
Site fixed effects	No	Yes
Month-year dummies	No	Yes
Patient controls	No	No
Patient observations	143,530	143,530
R-squared	0.043	0.896

Table reports regression coefficients and standard errors clustered at the hospital level. The coefficients on (Post × High treatment) and (Post × Low treatment) give treatment effects for the High Treatment group and Low Treatment group respectively. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1.

Figures

Figure 1: Contract start dates of ‘treating’ Wave 1 ISTCs

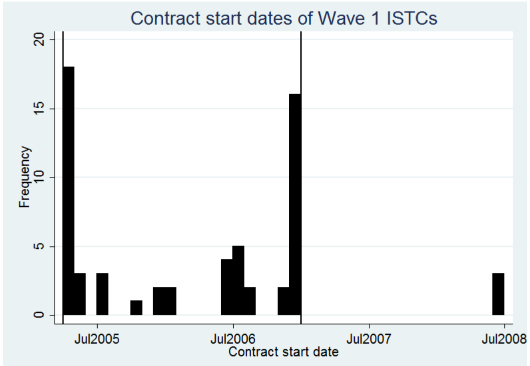
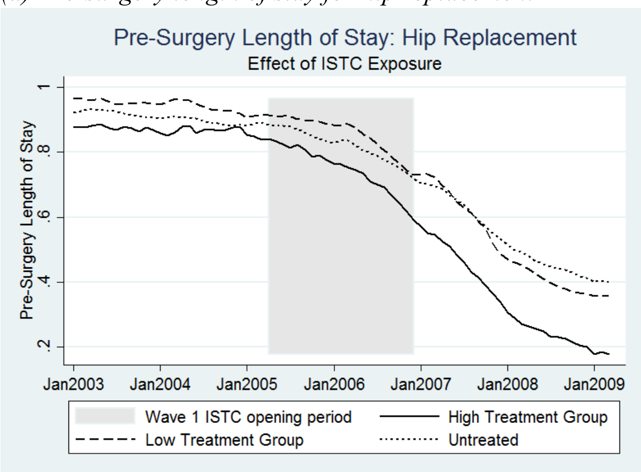


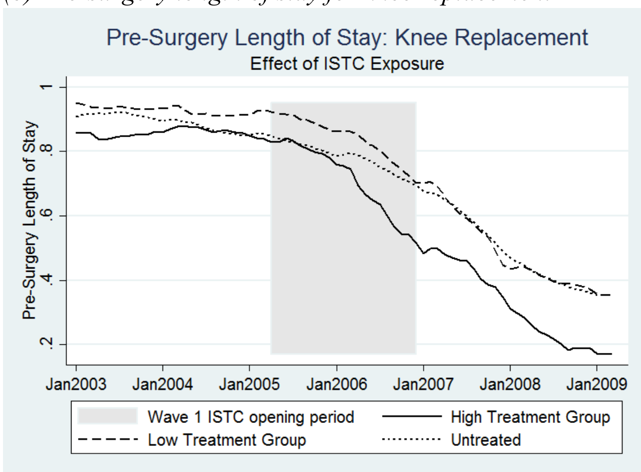
Figure shows the distribution of contract start dates for ‘treating’ Wave 1 ISTCs in our dataset. There are 60 data points, one for each NHS hospital that is ‘treated’ by a Wave 1 ISTC. If a Wave 1 ISTC is the closest ISTC to more than one NHS hospital in our dataset, it is represented more than once. This graph shows that the bulk of Wave 1 ISTCs relevant to our analysis opened between April 2005 and December 2006 (between the two vertical lines). We focus on Wave 1 ISTCs that opened during this period.

Figure 2: Trends in pre-surgery length of stay

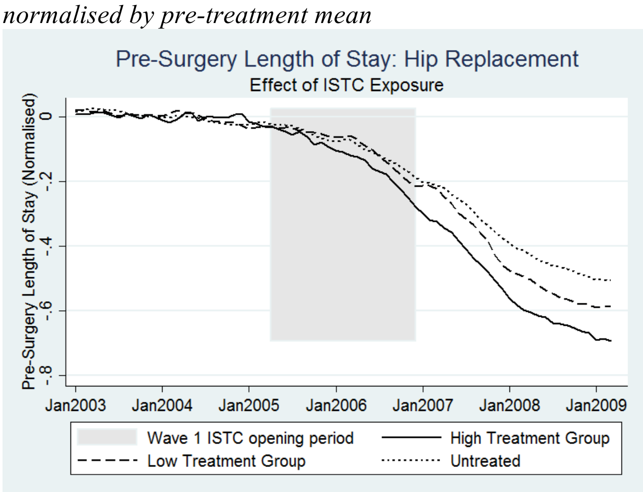
(a) Pre-surgery length of stay for hip replacement



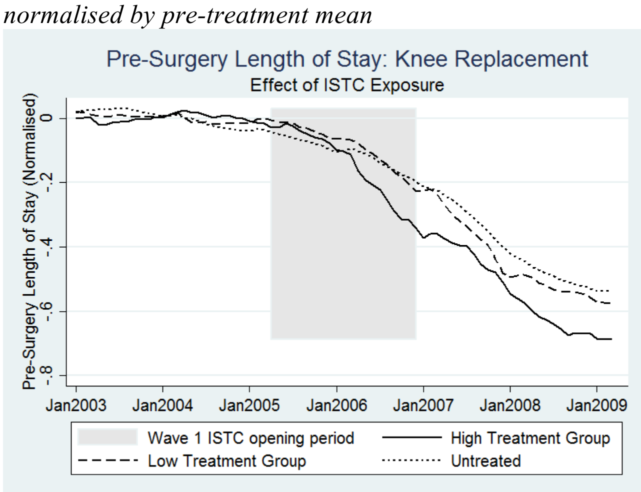
(b) Pre-surgery length of stay for knee replacement



(c) Pre-surgery length of stay for hip replacement, normalised by pre-treatment mean



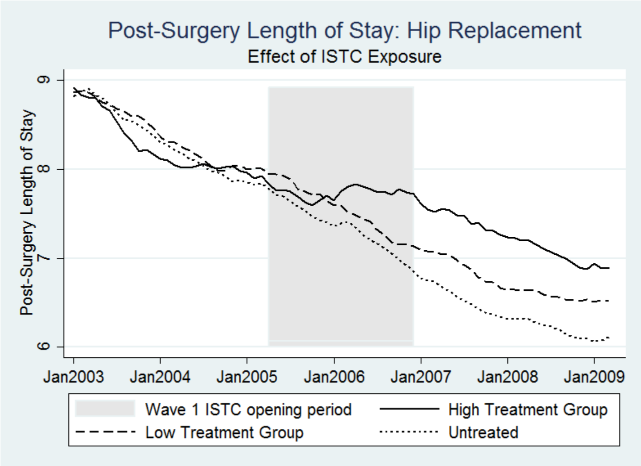
(d) Pre-surgery length of stay for knee replacement, normalised by pre-treatment mean



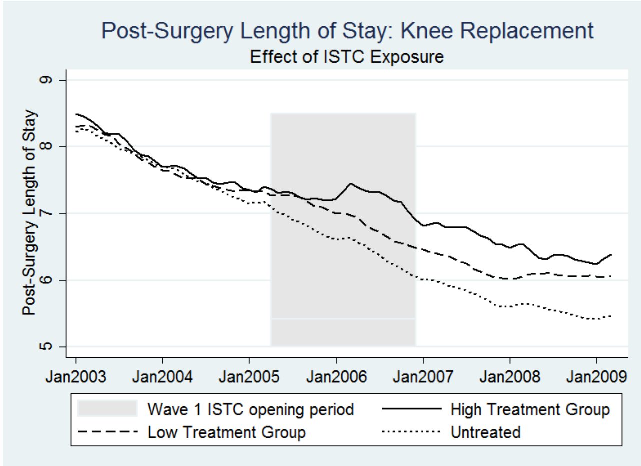
Treatment groups are: High – hospitals with Wave 1 ISTC within their 25th percentile patient travel distance; Low – hospitals with a Wave 1 ISTC between their 25th and 95th percentile patient travel distance; Untreated – hospitals without an ISTC in their 95th percentile patient travel distance. Sample includes hospitals whose closest ISTC (within 95th percentile patient travel distance) opened between April 2005 and December 2006. Graphs show moving averages of hospital means calculated over a month and the previous three quarters. Shaded area marks range of Wave 1 ISTC opening dates. Pre-surgery length of stay (LOS) is number of days between admission and surgery (zero implies surgery on day of admission). In panels (c) and (d), the outcome variable is normalised by subtracting the pre-treatment (2002/3-2004/5) average for each group.

Figure 3: Trends in post-surgery and total length of stay

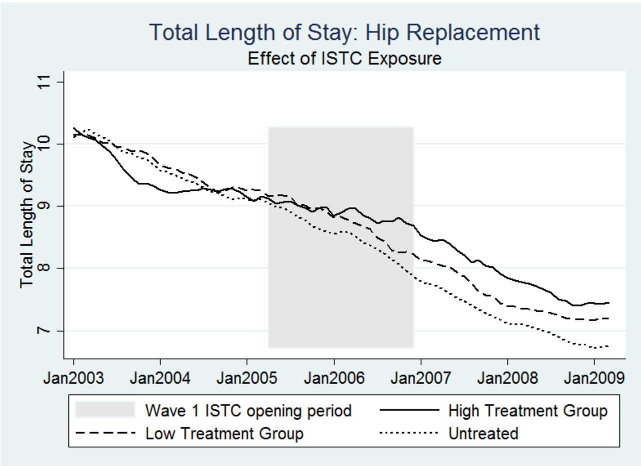
(a) Post-surgery length of stay for hip replacement



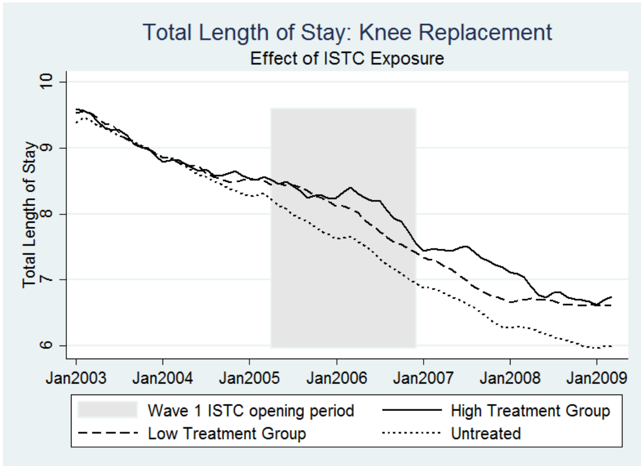
(b) Post-surgery length of stay for knee replacement



(c) Total length of stay for hip replacement



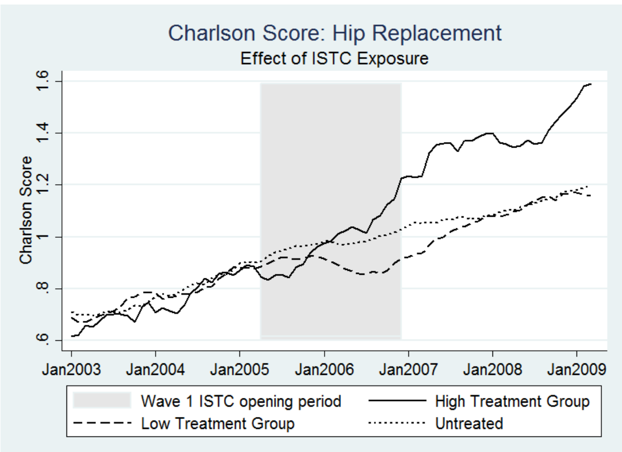
(d) Total length of stay for knee replacement



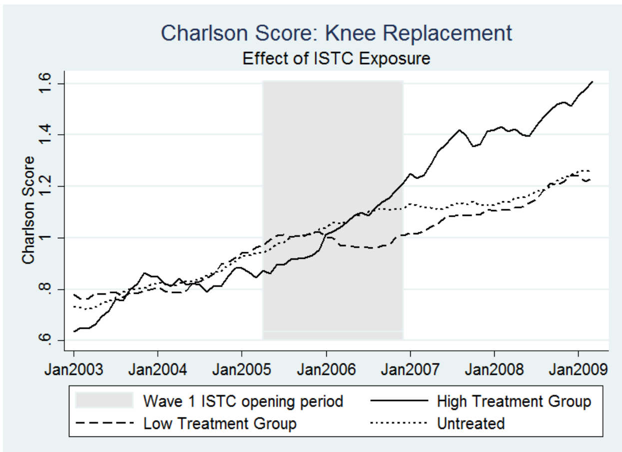
Post-surgery length of stay (LOS) measures the number of days between surgery and discharge. Total LOS measures the number of days between admission and discharge. See Figure 2 notes for further explanation.

Figure 4: Trends in case-mix – Charlson co-morbidity scores

(a) Charlson score for hip replacement patients



(b) Charlson score for knee replacement patients



The Charlson co-morbidity score gives a score between 0 and 6 which captures the patient's 10-year survival probability. It is based on the presence of 17 medical conditions that are likely to lead to death (HSCIC 2013). A score of 0 denotes the absence of any symptoms indicating death, while a score of 6 denotes a high likelihood of death. It is calculated at spell (not episode) level for all observations in our sample. See Figure 2 notes for further explanation.

Appendix

Predicting ISTC entry

Table A1 reports estimates predicting ISTC entry using nearby public hospital waiting times and length of stay in 2002/3. The regressions are run on a procedure-hospital-level dataset; hospitals treating both hip and knee replacement patients in 2002/3 have two observations. The dependent variable is assignment to the High Treatment Group (ISTC within 25 per cent market radius), while the predictor variables are log of average hospital waiting time, log of average total length of stay, and 30-day in-hospital mortality rate for hip or knee replacement patients in 2002/3. To mitigate the impact of outliers, observations in the top 10 per cent and bottom 10 per cent of waiting times are omitted. Standard errors are clustered at the hospital level.

The results show that average waiting times in 2002/3 strongly predict ISTC placement, while average total length of stay has no similar predictive power. Indeed, the (statistically insignificant) coefficients on length of stay are always negative, implying that public hospitals with *shorter* lengths of stay were more likely to have an ISTC placed nearby. Including the 30-day in hospital mortality rate alongside waiting times and length of stay does not substantially increase our ability to predict ISTC placement.

Table A1: Predicting ISTC entry using public hospital waiting times and length of stay in 2002/3

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS estimates predicting assignment to High Treatment group				Logit estimates predicting assignment to High Treatment group			
Log of Average Waiting Time	0.156**	-	0.152**	0.149**	2.749**	-	2.708**	2.663**
	(0.0683)		(0.0666)	(0.0659)	(1.128)		(1.128)	(1.123)
Log of Average Length of Stay	-	-0.0792	-0.0554	-0.0706	-	-1.186	-0.905	-1.101
		(0.109)	(0.106)	(0.113)		(1.560)	(1.582)	(1.649)
30-Day In-Hospital Mortality Rate	-	-	-	2.585	-	-	-	37.39
				(4.191)				(55.13)
Hospital observations	252	252	252	252	252	252	252	252
R-squared (or pseudo R-squared)	0.017	0.002	0.018	0.021	0.036	0.004	0.038	0.043

Table reports regression coefficients and standard errors clustered at the hospital level. The outcome variable is a binary indicator of assignment to the High Treatment group. Observations in the top 10 per cent and bottom 10 per cent of waiting times are omitted. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1.

Relationship between waiting time and length of stay

Table A2 reports estimates of the relationship between log of waiting time and log of length of stay using a patient-level dataset that includes all publicly funded hip and knee replacement patients in 2002/3. It confirms that there was no apparent relationship between these variables during the period when ISTC placement decisions were being made. We take this as evidence that selection for ISTC placement on the basis of local hospital waiting times does not imply selection, via correlation, on the basis of these hospitals' average LOS.

Table A2: Bivariate regression of log of waiting time on log of length of stay in 2002/3

	(1) Log of Waiting Time
Log of Total Length of Stay	0.0113
	(0.0101)
Hospital observations	55,658
R-squared	0.000

Table reports regression coefficients and standard errors. Statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

Graphical evidence – other outcome variables

Figure A1 presents graphical evidence for additional hip and knee replacement casemix variables discussed in the paper. Panels (a) and (b) present the evolution of the average percentage of patients with a Charlson score of three or more. They show a treatment effect in which High Treatment group hospitals receive a sicker mix of patients as a result of ISTC entry.

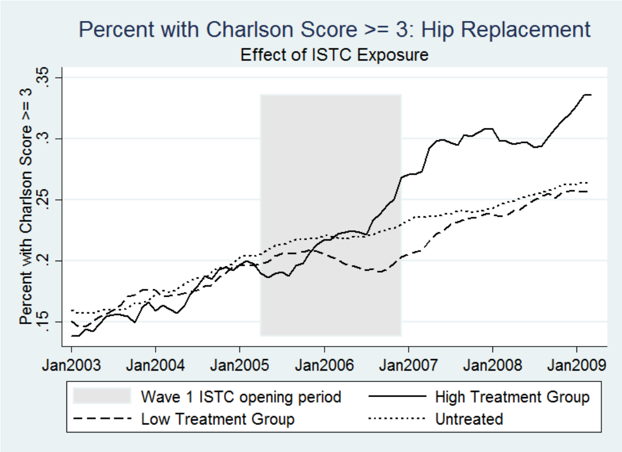
Panels (c) and (d) present the evolution of average IMD income deprivation score. The outcome variable is normalised by subtracting the pre-treatment average in order to facilitate a comparison of pre-treatment and post-treatment trends, as well as being demeaned (by subtracting the annual mean value across the whole sample) to control for a recalculation of the score in April 2007. Panel (c) indicates a treatment effect for hip replacement patients, in which High Treatment group hospitals received a more deprived mix of patients as a result of ISTC entry. It is not clear whether panel (d) indicates an equivalent treatment effect for knee replacement patients. Panels (e) and (f) present the evolution of average patient age. Panel (e) may suggest a treatment effect for hip replacement patients in which High Treatment group hospitals receive older patients as a result of ISTC entry, but it is hard to be sure because pre-reform trends are not very parallel. Panel (f), for knee replacement patients, provides little evidence of a treatment effect.

Figure A2 presents graphical evidence for other hip and knee replacement outcome variables discussed in the paper. Panels (a) and (b) present the evolution of the percentage of patients treated on day of admission (i.e. an indicator of pre-surgery LOS = 0), normalised by subtracting the pre-reform average value from the period-specific value. Hospitals in the High Treatment group experience a larger increase in percentage treated on day of admission than hospitals in the Low Treatment group, which in turn experience a larger increase than hospitals in the control group.

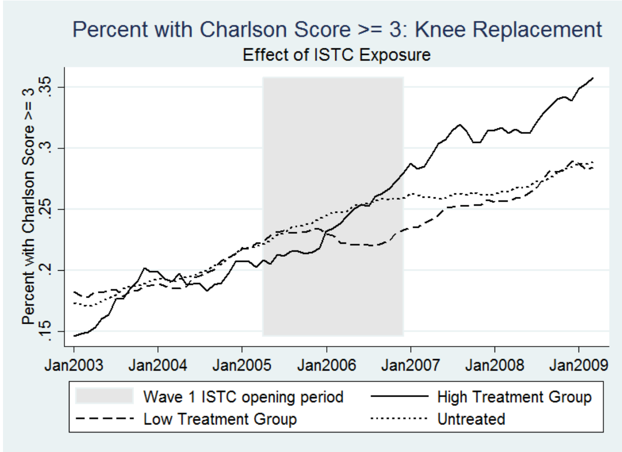
Panels (c) and (d) present evolution of hospital case loads. They show little evidence of an effect of ISTC exposure on case load at High Treatment group hospitals, but a possible treatment effect for Low Treatment group hospitals. Panels (e) and (f) present the evolution of average waiting times. They show that average waiting times at High Treatment group hospitals were initially substantially higher than for other groups; this is consistent with our argument that the government facilitated ISTC entry in areas where average waiting times were particularly high. The rapid convergence and reduction in waiting times for all groups is a product of the increasingly stringent waiting time targets regime introduced over the course of the 2000s.

Figure A1: Trends in other orthopaedic surgery casemix variables

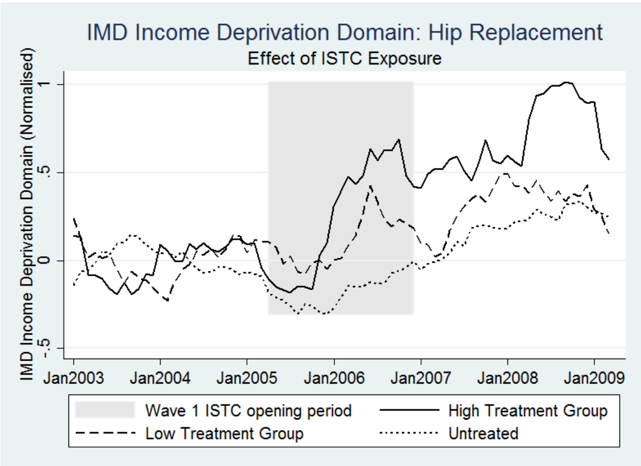
(a) Percentage of patients with Charlson score of three or more for hip replacement



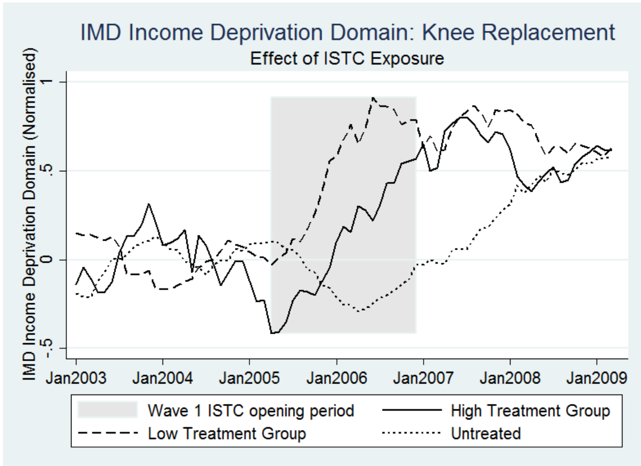
(b) Percentage of patients with Charlson score of three or more for knee replacement



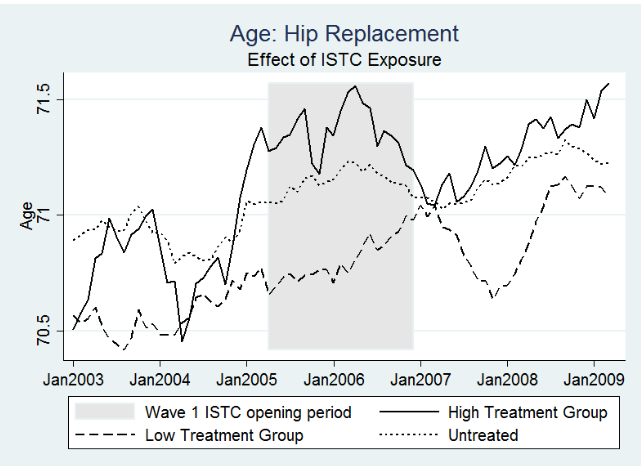
(c) Average IMD income deprivation score for hip replacement, normalised by pre-treatment average



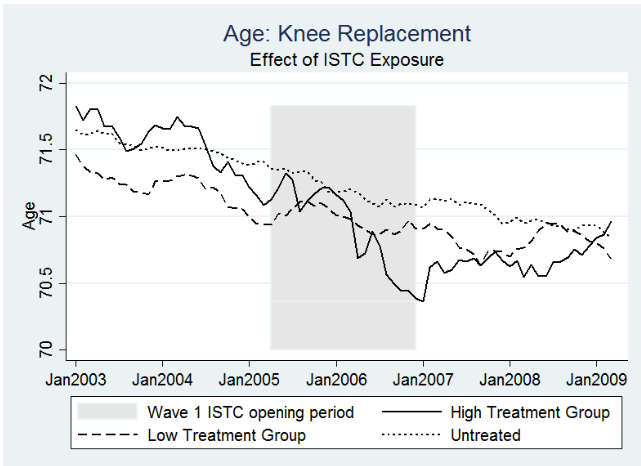
(d) Average IMD income deprivation score for knee replacement, normalised by pre-treatment average



(e) Average patient age for hip replacement



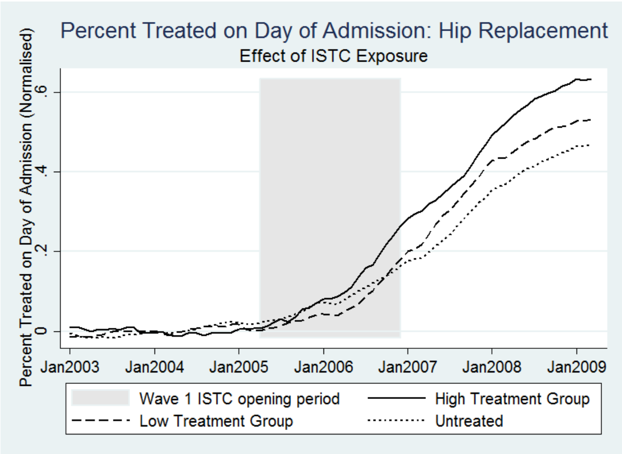
(f) Average patient age for knee replacement



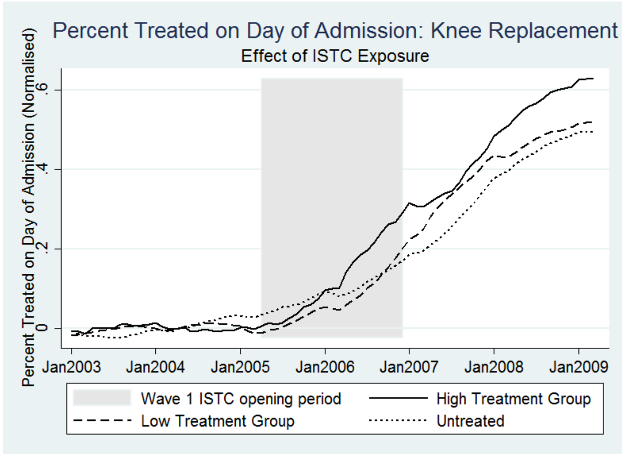
The Charlson index gives a score between 0 and 6 which captures the patient's 10-year survival probability. See Table 4 notes for further information. The IMD Income Deprivation score measures the percentage of households in the patient's Lower Super Output Area of residence that are income-deprived. In panels (c) and (d), the outcome variable is normalised by subtracting the pre-treatment (2002/3-2004/5) average for each group, and the annual mean value is subtracted from the variable to address a rescaling that occurred in April 2007. See Figure 2 notes for further explanation.

Figure A2: Trends in other orthopaedic surgery outcome variables

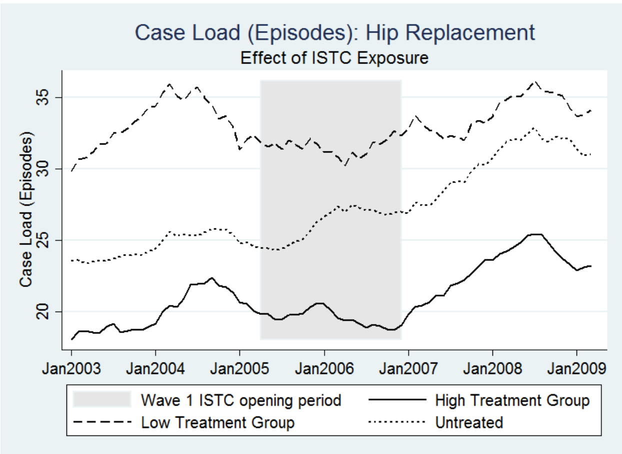
(a) Percentage treated on day of admission for hip replacement, normalised by pre-treatment average



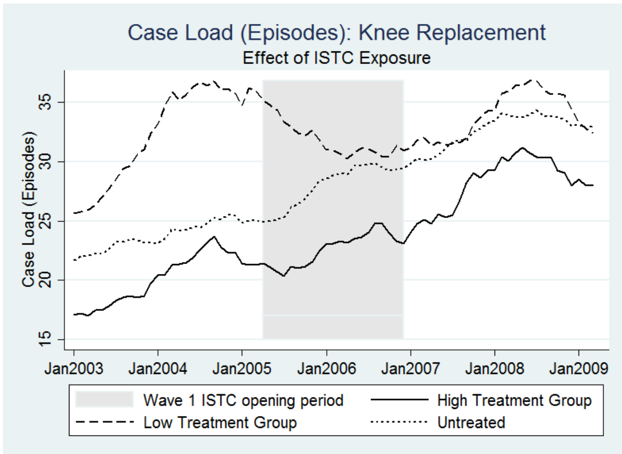
(b) Percentage treated on day of admission for knee replacement, normalised by pre-treatment average



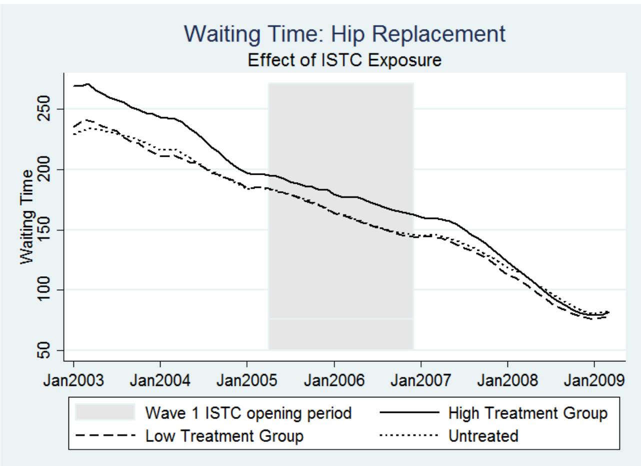
(c) Case load for hip replacement



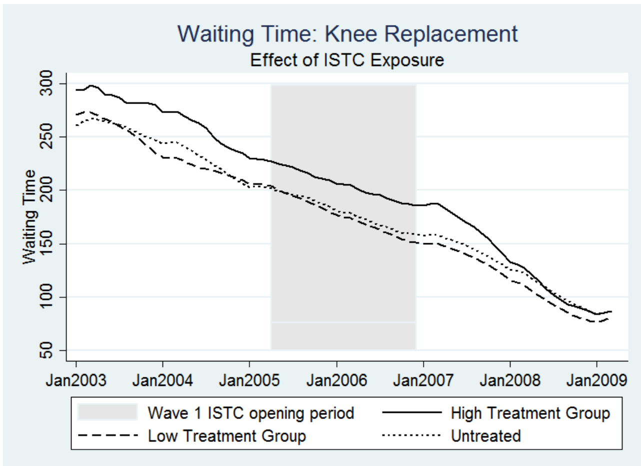
(d) Case load for knee replacement



(e) Average waiting time for hip replacement



(f) Average waiting time for knee replacement



Percentage treated on day of admission reports the percentage of patients with a pre-surgery LOS of zero. In panels (a) and (b), the outcome variable is normalised by subtracting the pre-treatment (2002/3-2004/5) average for each group. Case load is measured in finished consultant episodes. Waiting time is measured by HES's elecdur field. See Figure 2 notes for further explanation.

Effects of ISTC entry on clinical quality

In addition to affecting incumbents' efficiency and case mix, private surgical centre entry may also affect incumbents' clinical quality, either because competition can stimulate more general changes in quality, or because the reduction in pre-surgery LOS already documented may have come at the expense of clinical quality. Standard economic models of hospital competition (Gaynor 2006; Gaynor & Town 2012), in which hospitals offer a single type of good and set a single (vertical) quality level for that good in markets with prices set by a central authority, offer a clear prediction: increased hospital competition leads to higher care quality so long as the regulated price is set above the marginal cost with respect to quantity. While some questions have been raised about the generality of this basic theoretical prediction,[26] empirical evidence from the US (Gaynor & Town 2012; Kessler & McClellan 2000; Kessler & Geppert 2005) and UK (Cooper et al. 2011; Gaynor et al. 2013; Bloom et al. 2015) does, on balance, provide support for this view.[27]

Ideally, we would estimate the effect of private surgical centre entry on clinical quality at nearby public hospitals using quality indicators that relate to orthopaedic surgery. Unfortunately, there are no suitable measures of orthopaedic surgery quality available in our data. Instead, we follow previous research (e.g. Cooper et al. 2011) by looking a general barometer of hospital quality – survival rates for acute myocardial infarction (AMI). As this quality indicator pertains to an area of hospital activity (emergency cardiac care) not exposed to competition from private surgical centre entry, it is not a central focus of our study. It nonetheless allows us, at minimum, to examine whether ISTC entry had a negative impact on clinical quality in areas that were not exposed to competition from new entrants (for example, via substitution of managerial attention).

Figure A3 presents the evolution of 30-day in-hospital mortality for AMI.[28] It shows a possible treatment effect, in which AMI mortality falls faster at High Treatment group hospitals than elsewhere. However, the High Treatment group trend does not diverge until well after the last ISTC enters the market, raising the question as to whether the accelerated reduction of AMI mortality rates at High Treatment group hospitals is really being driven by ISTC exposure.

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[26] Brekke, Siciliani & Straume (2011) show that, if hospitals are semi-altruistic and costs are more convex than altruistic valuation of quality, the marginal patient may be loss-making, thus violating the requirement that prices exceed marginal costs and implying that increased competition may lead to lower care quality. Bevan & Skellern (2011) discuss the multi-product nature of the hospital, and, following Holmstrom & Milgrom (1991), note that incentives to improve quality for one output may have a positive or negative impact on quality of other, unincentivised outputs, depending on whether there are cost complementarities or substitutabilities between outputs.

[27] This finding is not universally replicated, however: see Gowrisankaran & Town 2003; Mukamel et al. 2001; Colla et al. 2014; Skellern 2015.

[28] See Appendix for diagnosis definitions. Following Cooper et al. (2011), we restrict attention to AMI patients aged 40 to 100, and exclude patients that were discharged alive with a total length of stay of less than three days in order ameliorate concerns about possible upcoding of diagnoses.

Table A3 reports regression-based estimates of the impact of ISTC entry on clinical quality, as captured by AMI mortality, at incumbent public hospitals. We report estimates from all three specifications used in this paper: a simple DiD estimator, and a DiD estimator with hospital fixed effects, both with and without patient controls. The first two specifications yield a negative and statistically significant coefficient, suggesting that ISTC entry led to higher clinical quality at nearby incumbents. However, in the fullest specification with patient controls, the coefficient is no longer significant. When set alongside our findings in relation to pre-surgery LOS, we take this to indicate that ISTC entry led to efficiency improvements at nearby public hospitals without any evidence of concomitant deterioration in clinical quality.

Figure A3: Trends in mortality rate for acute myocardial infarction

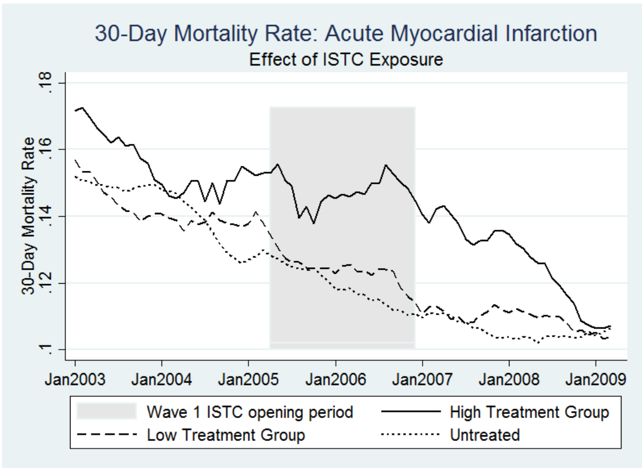

Figure reports evolution of 30-day in-hospital mortality rate for acute myocardial infarction. See Figure 2 for further information.

Table A3: Impact of ISTC entry on mortality for acute myocardial infarction at nearby public hospitals. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1) AMI mortality	(2) AMI mortality	(3) AMI mortality
Post × High treatment	-0.0249* (0.0147)	-0.0255** (0.0120)	-0.0193 (0.0158)
Post × High treatment	-0.0115 (0.00727)	-0.0164** (0.00760)	-0.00935 (0.00794)
High treatment	0.0187 (0.0168)	-	-
Low treatment	0.00838 (0.00815)	-	-
Post	-0.0252*** (0.00548)	-	-
Post × Negative log HHI	-	0.00716 (0.00560)	0.00534 (0.00576)
Site fixed effects	No	Yes	Yes
Month-year dummies	No	Yes	Yes
Patient controls	No	No	Yes
Patient observations	96,110	96,110	95,796
R-squared	0.002	0.015	0.067

Table reports regression coefficients and standard errors clustered at the hospital level. The coefficients on (Post × High treatment) and (Post × Low treatment) give treatment effects for the High Treatment group and Low Treatment group respectively. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1.

Additional robustness checks

Table A4 presents estimates from a number of robustness checks not presented in the main body of the paper. Row (1) includes a simple dummy indicating whether an observation is from a post-reform period in place of a full set of month-year dummies. The results are virtually unchanged. Row (2) reports estimates when our control for overall competition intensity is omitted. The estimates are again nearly unchanged, confirming that our headline findings are not driven by our control for overall competition intensity.

Row (3) reports estimates when treatment assignment is performed based on percentiles of patient distance travelled across five elective surgical procedures instead of the two procedures that are the focus of this paper. In addition to patients undergoing hip and knee replacement, we also make use of percentiles of distance travelled by patients undertaking three other surgical procedures that were a major focus of ISTC activity: knee arthroscopy, hernia repair, and varicose vein stripping (see Appendix for definitions). In this check, we also use an index of competition intensity based on patients undergoing all five of these surgical procedures. The results are very similar to those from the headline specification, except that the effect of ISTC entry on post-surgery LOS at Low Treatment group hospitals is now significant at the 5 per cent level rather than the 10 per cent level.

Table A4: Impact of ISTC entry on outcomes at nearby public hospitals: additional robustness tests. Difference in difference estimates using 2008/9 – 2004/5 differences.

		(1) Log of pre-surgery LOS	(2) % treated on day of admission	(3) Log of post-surgery LOS	(4) Log of Charlson Score
(1) Post-reform dummy in place of month-time dummies	Post × High treatment	-0.173** (0.0679)	0.239** (0.0940)	0.0490 (0.0498)	0.116** (0.0501)
	Post × Low treatment	-0.0580 (0.0546)	0.0858 (0.0783)	0.0572* (0.0304)	0.0163 (0.0251)
(2) No control for overall competition intensity	Post × High treatment	-0.173** (0.0675)	0.240** (0.0936)	0.0487 (0.0499)	0.114** (0.0511)
	Post × Low treatment	-0.0633 (0.0458)	0.0909 (0.0655)	0.0527* (0.0297)	0.00282 (0.0246)
(3) Treatment assignment & comp index based on five elective procedures	Post × High treatment	-0.175** (0.0674)	0.242** (0.0932)	0.0486 (0.0494)	0.113** (0.0501)
	Post × Low treatment	-0.0697 (0.0534)	0.106 (0.0767)	0.0676** (0.0296)	0.0146 (0.0251)

Table reports robustness tests based on the 'headline' regression specification (Equation (2) without patient controls), with hospital (site) fixed effects, a full set of month-year dummies, and controls for overall market competitiveness. The coefficients on (Post × High treatment) and (Post × Low treatment) give treatment effects for the High Treatment group and Low Treatment group respectively. Standard errors clustered at the hospital level are reported in parentheses. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. All tests reported in this table use 2004/5 as the pre-reform period and 2008/9 as the post-reform period. Row (1) includes a pre/post dummy in place of month-year dummies. Row (2) omits the control for overall competition intensity. Row (3) assigns treatments, and constructs an index of competition intensity, based on five elective surgical procedures: hip and knee replacement, knee arthroscopy, hernia repair, and varicose vein stripping.

Effect of patient characteristics on pre-surgery LOS and post-surgery LOS

Table A5 reports estimates from regression of pre-surgery LOS and post-surgery LOS on a large set of patient characteristics using a dataset containing all publicly funded hip and knee replacement patients from 2002/3 to 2012/13. The R-squareds indicate that observable patient characteristics explain 14.2 per cent of the variation in post-surgery LOS, but only 0.8 per cent of the variation in pre-surgery LOS. The fact that observable patient characteristics explain so little of pre-surgery LOS implies, in turn, that little of the variation is likely to be driven by unobservable patient characteristics (Altonji et al. 2008). It is also striking that, even when the coefficients in the pre-surgery LOS regression are statistically significant, they are almost always tiny in magnitude. These findings provide support for our argument that, while post-surgery LOS is significantly influenced by patient characteristics and health status, pre-surgery LOS is largely free of such influence.

Table A5: Effect of patient characteristics on pre-surgery and post-surgery LOS

	(1) Pre-Surgery LOS	(2) Post-Surgery LOS
Charlson Score	0.000905	-0.00964
	(0.00233)	(0.0137)
IMD Income Score	0.0215	1.405***
	(0.0858)	(0.262)
Number of Diagnoses	0.00933	0.525***
	(0.00666)	(0.0359)
Dummy: 1 diagnosis	0.0540***	0.501***
	(0.0141)	(0.0542)
Dummy: 3 or more diagnoses	-0.0252**	-0.313***
	(0.00987)	(0.0432)
Dummy: From Poor Neighbourhood	0.0168*	0.0229
	(0.00944)	(0.0428)
Dummy: Lives in Urban Area	-0.0286	0.258***
	(0.0260)	(0.0898)
Dummy: Mixed Ethnicity	-0.0353	0.676***
	(0.0532)	(0.189)
Dummy: Asian Ethnicity	-0.108***	0.794***
	(0.0284)	(0.129)
Dummy: Black Ethnicity	-0.0177	1.008***
	(0.0421)	(0.141)
Dummy: Other Ethnicity	-0.0885*	0.490***
	(0.0488)	(0.135)
Dummy: Unknown Ethnicity	0.0227	0.230**
	(0.0350)	(0.113)
Dummy: Hip Replacement	0.0174***	0.111***
	(0.00436)	(0.0377)
Age	-0.00938	-0.477***
	(0.00582)	(0.0360)
Age Squared	9.46e-05**	0.00415***
	(4.12e-05)	(0.000264)
Dummy: Revision to Hip Replacement	0.168***	2.185***
	(0.0163)	(0.0788)
Dummy: Self-discharge	0.0309	-0.509***
	(0.0302)	(0.150)
Female 56-60 Dummy	-0.00271	0.159***
	(0.0126)	(0.0532)
Female 61-65 Dummy	-0.0143	0.177**
	(0.0157)	(0.0696)

Female 66-70 Dummy	0.000845	0.178**
	(0.0175)	(0.0833)
Female 71-75 Dummy	-0.00257	0.176*
	(0.0196)	(0.0901)
Female 76-80 Dummy	-0.0117	0.149
	(0.0209)	(0.0952)
Female 81-85 Dummy	0.000422	0.152
	(0.0213)	(0.104)
Female 86 Plus Dummy	-0.0296	0.0892
	(0.0240)	(0.134)
Male 51-55 Dummy	0.00411	-0.278***
	(0.0160)	(0.0742)
Male 56-60 Dummy	0.0144	-0.249***
	(0.0143)	(0.0581)
Male 61-65 Dummy	-0.00272	-0.283***
	(0.0165)	(0.0754)
Male 66-70 Dummy	0.0179	-0.274***
	(0.0184)	(0.0877)
Male 71-75 Dummy	0.00992	-0.357***
	(0.0193)	(0.0981)
Male 76-80 Dummy	0.00414	-0.304***
	(0.0210)	(0.104)
Male 81-85 Dummy	-0.0139	-0.181
	(0.0224)	(0.114)
Male 86 Plus Dummy	-0.0321	-0.159
	(0.0266)	(0.139)
Dummy: Acute Myocardial Infarction	0.00108	-0.0583
	(0.0163)	(0.0860)
Dummy: Cerebrovascular Disease	0.0528*	1.715***
	(0.0292)	(0.241)
Dummy: Congestive Heart Failure	0.185***	1.482***
	(0.0258)	(0.138)
Dummy: Connective Tissue Disease	0.0258**	0.164**
	(0.0125)	(0.0684)
Dummy: Dementia	0.0304	2.294***
	(0.0396)	(0.269)
Dummy: Diabetes	0.0236**	-0.0798
	(0.0118)	(0.0523)
Dummy: Liver Disease	0.144**	0.623**
	(0.0567)	(0.250)
Dummy: Peptic Ulcer Disease	0.0745**	3.173***
	(0.0346)	(0.316)
Dummy: Peripheral Vascular Disease	-0.0119	-0.377***
	(0.0205)	(0.110)
Dummy: Chronic Obstructive Pulmonary Disease	-0.0281**	-0.144**
	(0.0120)	(0.0651)
Dummy: Cancer	0.0207	0.107
	(0.0246)	(0.115)
Dummy: Complications of Diabetes	0.0820**	0.283
	(0.0379)	(0.243)
Dummy: Paralysis	0.0872**	0.745***
	(0.0349)	(0.273)
Dummy: Renal Disease	-0.0562**	0.461***
	(0.0216)	(0.115)
Dummy: Metastatic Cancer	0.221***	0.673***
	(0.0585)	(0.235)
Dummy: Severe Liver Disease	0.102	-0.0263
	(0.0810)	(0.850)
Dummy: HIV	0.0500	1.638
	(0.276)	(1.481)
Observations	558,650	552,642
R-squared	0.008	0.142

Table reports coefficients and standard errors. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1.

Coefficient estimates with patient controls

Table A6 reports the full set of coefficients from estimation of treatment effects when, in addition to the inclusion of hospital fixed effects, controls for patient characteristics are included. Column (1) provides the full set of coefficients for the regression on pre-surgery LOS, as reported in Column (3) of Table 4. Column (2) provides the full set of coefficients for the regression on percentage treated on day of admission, as reported in Column (6) of Table 4. Column (3) provides the full set of coefficients for the regression on post-surgery LOS, as reported in Column (9) of Table 4. Column (4) provides the full set of coefficients for the regression on AMI mortality, as reported in Column (3) of Table A4.

Table A6: Impact of ISTC entry on outcomes at nearby public hospitals: coefficients on patient characteristics. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1)	(2)	(3)	(4)
Procedure	Orthopaedic Surgery			Acute Myocardial Infarction
Outcome	Log of pre-surgery LOS	% treated on day of admission	Log of post-surgery LOS	Mortality
Post × High treatment	-0.178*** (0.0682)	0.244** (0.0941)	0.0255 (0.0444)	-0.0193 (0.0158)
Post × Low treatment	-0.0591 (0.0551)	0.0868 (0.0787)	0.0498 (0.0316)	-0.00935 (0.00794)
Post × Negative Log HHI	-0.00637 (0.0246)	0.00584 (0.0347)	-0.00640 (0.0171)	0.00534 (0.00576)
Log of Charlson Score	0.0107*** (0.00231)	-0.0158*** (0.00305)	0.0170*** (0.00254)	0.171*** (0.0193)
Log Number of Diagnoses	0.0283*** (0.00833)	-0.0241** (0.0110)	0.151*** (0.00643)	0.0357*** (0.00392)
Log of IMD Income Score	0.00387 (0.00425)	-0.00507 (0.00581)	0.0163*** (0.00381)	0.00501* (0.00300)
Log IMD Health/ Disability Score	0.00871 (0.0183)	-0.00897 (0.0255)	0.0398** (0.0176)	0.00473 (0.0123)
HRG F41	-	-	-	0.184*** (0.0534)
HRG H72	0.0363*** (0.00858)	-0.0321*** (0.0107)	0.0236 (0.0192)	0.273 (0.202)
HRG H80	-0.0551*** (0.0172)	0.0645*** (0.0228)	-0.242*** (0.0227)	0.421*** (0.103)
HRG H81	-0.0629*** (0.0192)	0.0727*** (0.0258)	-0.264*** (0.0247)	0.163 (0.129)
Self-Discharge Dummy	0.0151 (0.0251)	-0.0163 (0.0293)	-0.115*** (0.0298)	-0.0807*** (0.00499)
Urban Dummy	-0.000534 (0.00296)	-0.00165 (0.00376)	0.00648** (0.00325)	-0.00447* (0.00256)
Mixed Ethnicity Dummy	0.00689 (0.0193)	-0.00954 (0.0221)	0.0908*** (0.0281)	0.0112 (0.0194)
Asian Ethnicity Dummy	-0.0134** (0.00673)	0.0187** (0.00819)	0.110*** (0.0129)	-0.0135*** (0.00507)
Black Ethnicity Dummy	-0.000972 (0.0125)	-0.000881 (0.0163)	0.0903*** (0.0152)	-0.00167 (0.0121)

Other Ethnicity Dummy	-0.0318** (0.0125)	0.0299* (0.0175)	0.0236 (0.0211)	-0.00849 (0.00924)
Unknown Ethnicity Dummy	-0.0148 (0.00899)	0.0193 (0.0127)	-0.0181** (0.00719)	0.0353*** (0.00547)
Female 41-45 Dummy	-	-	-	-0.0230 (0.0331)
Female 46-50 Dummy	-	-	-	-0.0231 (0.0314)
Female 51-55 Dummy	-	-	-	-0.0259 (0.0325)
Female 56-60 Dummy	0.00753 (0.00780)	-0.00591 (0.0106)	0.0183 (0.0127)	-0.0226 (0.0314)
Female 61-65 Dummy	0.00303 (0.00810)	0.000959 (0.0109)	0.0477*** (0.0129)	-0.00423 (0.0314)
Female 66-70 Dummy	0.0130 (0.00794)	-0.00868 (0.0111)	0.0899*** (0.0132)	0.00868 (0.0309)
Female 71-75 Dummy	0.0191** (0.00803)	-0.0169 (0.0109)	0.156*** (0.0127)	0.0354 (0.0306)
Female 76-80 Dummy	0.0252*** (0.00828)	-0.0278** (0.0113)	0.254*** (0.0134)	0.0586* (0.0307)
Female 81-85 Dummy	0.0500*** (0.00889)	-0.0562*** (0.0113)	0.352*** (0.0147)	0.0875*** (0.0308)
Female 86 Plus Dummy	0.0655*** (0.0106)	-0.0792*** (0.0138)	0.504*** (0.0150)	0.135*** (0.0317)
Male 40 Dummy	-	-	-	-0.0481 (0.0317)
Male 41-45 Dummy	-	-	-	-0.0360 (0.0316)
Male 46-50 Dummy	-	-	-	-0.0419 (0.0312)
Male 51-55 Dummy	-0.00293 (0.0130)	0.0106 (0.0167)	-0.0860*** (0.0216)	-0.0326 (0.0315)
Male 56-60 Dummy	0.0146 (0.00900)	-0.0139 (0.0124)	-0.0713*** (0.0141)	-0.0246 (0.0312)
Male 61-65 Dummy	0.00436 (0.00822)	0.00238 (0.0113)	-0.0505*** (0.0140)	-0.0201 (0.0312)
Male 66-70 Dummy	0.0158* (0.00845)	-0.0143 (0.0116)	-0.000281 (0.0134)	0.000710 (0.0312)
Male 71-75 Dummy	0.0222*** (0.00822)	-0.0186* (0.0109)	0.0679*** (0.0139)	0.0232 (0.0313)
Male 76-80 Dummy	0.0306*** (0.00876)	-0.0323*** (0.0121)	0.189*** (0.0145)	0.0551* (0.0312)
Male 81-85 Dummy	0.0440*** (0.00954)	-0.0481*** (0.0125)	0.313*** (0.0150)	0.0886*** (0.0305)
Male 86 Plus Dummy	0.0698*** (0.0109)	-0.0830*** (0.0145)	0.459*** (0.0212)	0.125*** (0.0315)
Hip Revision Dummy	0.00949 (0.0186)	-0.00761 (0.0245)	-0.0530** (0.0257)	-
Hip Dummy	0.0636*** (0.0177)	-0.0720*** (0.0236)	0.273*** (0.0231)	-
Site Fixed Effects	Yes	Yes	Yes	Yes
Month-Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	143,046	143,307	141,549	95,796
R-squared	0.396	0.447	0.260	0.067

Table reports regression coefficients and standard errors clustered at the hospital level. The coefficients on (Post × High treatment) and (Post × Low treatment) give treatment effects for the High Treatment group and Low Treatment group respectively. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1.

Main estimates for hip replacement patients separately

Tables A7 and A8 report estimates for our main outcome variables of interest when regressions are run on a dataset consisting only of hip replacement patients.

Table A7: Impact of ISTC entry on hip replacement surgery length of stay at nearby public hospitals. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1) Log of pre-surgery LOS	(2) Log of pre-surgery LOS	(3) Log of pre-surgery LOS	(4) % treated on day of admission	(5) % treated on day of admission	(6) % treated on day of admission	(7) Log of post-surgery LOS	(8) Log of post-surgery LOS	(9) Log of post-surgery LOS
Post × High treatment	-0.154** (0.0640)	-0.172*** (0.0658)	-0.178*** (0.0662)	0.213** (0.0900)	0.238** (0.0929)	0.243*** (0.0931)	0.0885 (0.0568)	0.0649 (0.0587)	0.0419 (0.0488)
Post × Low treatment	-0.0554 (0.0465)	-0.0548 (0.0556)	-0.0570 (0.0560)	0.0815 (0.0658)	0.0836 (0.0793)	0.0856 (0.0797)	0.0427 (0.0312)	0.0469 (0.0314)	0.0370 (0.0319)
High treatment	0.0203 (0.0471)	-	-	-0.0237 (0.0620)	-	-	0.0374 (0.0618)	-	-
Low treatment	0.00938 (0.0388)	-	-	-0.0153 (0.0542)	-	-	0.00543 (0.0269)	-	-
Post	-0.307*** (0.0261)	-	-	0.425*** (0.0365)	-	-	-0.264*** (0.0196)	-	-
Post × Neg log HHI	-	-0.00998 (0.0249)	-0.00930 (0.0250)	-	0.0106 (0.0349)	0.00986 (0.0350)	-	-0.00652 (0.0177)	-0.00391 (0.0169)
Site fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Month-year dummies	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Patient controls	No	No	Yes	No	No	Yes	No	No	Yes
Patient obs	65,984	65,984	65,886	66,124	66,124	66,026	65,123	65,123	65,027
R-squared	0.194	0.377	0.388	0.223	0.436	0.444	0.063	0.131	0.302

Table reports coefficients and standard errors clustered at hospital level. The coefficients on (Post × High treatment) and (Post × Low treatment) give treatment effects for High Treatment group and Low Treatment group respectively. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. See Table A6 for list of patient characteristics controls used in Columns (3), (6) and (9).

Table A8: Impact of ISTC entry on hip replacement surgery case mix at nearby public hospitals. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1) Log Charlson score	(2) Log Charlson score	(3) Charlson score of 3 or more	(4) Charlson score of 3 or more	(5) Log IMD income deprivation index	(6) Log IMD income deprivation index	(7) Log age (coefficients x 100)	(8) Log age (coefficients x 100)
Post × High treatment	0.111** (0.0529)	0.119** (0.0559)	0.0609** (0.0301)	0.0659** (0.0318)	0.0488* (0.0260)	0.000433 (0.0263)	0.0646 (0.439)	0.0685 (0.444)
Post × Low treatment	0.000964 (0.0242)	0.0202 (0.0260)	0.00140 (0.0141)	0.0126 (0.0153)	-0.0346 (0.0245)	-0.0379* (0.0211)	0.102 (0.259)	0.247 (0.265)
High treatment	0.0106 (0.0496)	-	0.00472 (0.0289)	-	-0.137 (0.0883)	-	0.116 (0.289)	-
Low treatment	-0.0130 (0.0273)	-	-0.00863 (0.0163)	-	-0.0184 (0.0578)	-	-0.390 (0.286)	-
Post	0.0996*** (0.0161)	-	0.0574*** (0.00945)	-	0.146*** (0.0124)	-	0.299** (0.145)	-
Post × Neg log HHI	-	-0.0133 (0.0132)	-	-0.00751 (0.00766)	-	0.00945 (0.00914)	-	-0.174 (0.132)
Site fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Month-year dummies	No	Yes	No	Yes	No	Yes	No	Yes
Patient controls	No	No	No	No	No	No	No	No
Patient obs	66,124	66,124	66,124	66,124	66,026	66,026	66,124	66,124
R-squared	0.007	0.033	0.007	0.032	0.014	0.161	0.000	0.013

Table reports coefficients and standard errors clustered at the hospital level. See Table A7 notes for further information.

Main estimates for knee replacement patients separately

Tables A9 and A10 report estimates for our main outcome variables of interest when regressions are run on a dataset consisting only of knee replacement patients.

Table A9: Impact of ISTC entry on knee replacement surgery length of stay at nearby public hospitals. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1) Log of pre-surgery LOS	(2) Log of pre-surgery LOS	(3) Log of pre-surgery LOS	(4) % treated on day of admission	(5) % treated on day of admission	(6) % treated on day of admission	(7) Log of post-surgery LOS	(8) Log of post-surgery LOS	(9) Log of post-surgery LOS
Post × High treatment	-0.152** (0.0675)	-0.172** (0.0708)	-0.177** (0.0710)	0.213** (0.0935)	0.239** (0.0977)	0.245** (0.0977)	0.0747 (0.0472)	0.0351 (0.0469)	0.00879 (0.0456)
Post × Low treatment	-0.0357 (0.0503)	-0.0628 (0.0548)	-0.0632 (0.0556)	0.0493 (0.0716)	0.0904 (0.0788)	0.0907 (0.0797)	0.0548 (0.0343)	0.0635** (0.0320)	0.0603* (0.0337)
High treatment	0.0360 (0.0438)	-	-	-0.0482 (0.0597)	-	-	0.0597 (0.0571)	-	-
Low treatment	0.0171 (0.0435)	-	-	-0.0247 (0.0609)	-	-	0.00843 (0.0284)	-	-
Post	-0.320*** (0.0283)	-	-	0.445*** (0.0400)	-	-	-0.262*** (0.0198)	-	-
Post × Neg log HHI	-	-0.00361 (0.0244)	-0.00347 (0.0247)	-	0.00235 (0.0348)	0.00195 (0.0350)	-	-0.00728 (0.0181)	-0.0107 (0.0177)
Site fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Month-year dummies	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Patient controls	No	No	Yes	No	No	Yes	No	No	Yes
Patient obs	77,284	77,284	77,160	77,406	77,406	77,281	76,644	76,644	76,522
R-squared	0.207	0.401	0.407	0.230	0.449	0.453	0.061	0.120	0.233

Table reports coefficients and standard errors clustered at hospital level. The coefficients on (Post × High treatment) and (Post × Low treatment) give treatment effects for High Treatment group and Low Treatment group respectively. Statistical significance is reported as follows: *** p<0.01, ** p<0.05, * p<0.1. See Table A6 for list of patient characteristics controls used in Columns (3), (6) and (9).

Table A10: Impact of ISTC entry on knee replacement surgery case mix at nearby public hospitals. Difference in difference estimates using 2008/9 – 2004/5 differences.

	(1) Log Charlson score	(2) Log Charlson score	(3) Charlson score of 3 or more	(4) Charlson score of 3 or more	(5) Log IMD income deprivation index	(6) Log IMD income deprivation index	(7) Log age (coefficients x 100)	(8) Log age (coefficients x 100)
Post × High treatment	0.122** (0.0505)	0.116** (0.0495)	0.0700** (0.0290)	0.0663** (0.0285)	0.0596** (0.0298)	0.0139 (0.0312)	0.402 (0.495)	0.258 (0.432)
Post × Low treatment	-0.00901 (0.0274)	0.0115 (0.0284)	-0.00305 (0.0163)	0.00867 (0.0167)	-0.0209 (0.0321)	-0.0124 (0.0180)	0.222 (0.241)	-0.0156 (0.258)
High treatment	-0.0279 (0.0452)	-	-0.0167 (0.0271)	-	-0.176** (0.0820)	-	-0.429 (0.428)	-
Low treatment	-0.00962 (0.0284)	-	-0.00695 (0.0170)	-	-0.0100 (0.0631)	-	-0.600** (0.286)	-
Post	0.112*** (0.0195)	-	0.0655*** (0.0115)	-	0.159*** (0.0127)	-	-0.737*** (0.125)	-
Post × Neg log HHI	-	-0.0183 (0.0141)	-	-0.0102 (0.00832)	-	0.00543 (0.00896)	-	0.183 (0.122)
Site fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Month-year dummies	No	Yes	No	Yes	No	Yes	No	Yes
Patient controls	No	No	No	No	No	No	No	No
Patient obs	77,406	77,406	77,406	77,406	77,281	77,281	77,406	77,406
R-squared	0.007	0.038	0.007	0.038	0.017	0.183	0.001	0.014

Table reports coefficients and standard errors clustered at the hospital level. See Table A9 notes for further information.

Procedure and diagnosis definitions

We use the following OPCS4 procedure codes to identify the procedures included in our dataset.

- Hip replacement: Any patient with a procedure field beginning with W37, W38, W39, W93, W94 or W95.
- Knee replacement: Any patient with a procedure field beginning with W40, W41, or W42.

We restrict attention to patients aged 55 to 100. HES allows up to 24 surgical procedures to be listed in an episode. When creating our initial dataset, which is used for treatment assignment and construction of competition indices, we match on all 24 procedure fields. In order to compare like with like, when running regressions we restrict attention to patients whose primary procedure field (opertn_1) is one of the above.

The regressions reported in the Appendix with mortality from acute myocardial infarction (AMI) as the outcome variable include patients with ICD10 diagnosis codes beginning with I21 and I22. Following Cooper et al. (2011), we restrict attention to AMI patients aged 40 to 100, and exclude patients that were discharged alive with a total length of stay of less than three days in order ameliorate concerns about possible upcoding of diagnoses.

For the robustness test in the Appendix that assigns hospitals to treatment and control groups based on percentiles of patient distance travelled for five surgical procedures rather than the two that are the focus of this paper, we employ the following procedure definitions:

- Varicose vein stripping: Any patient with a procedure field beginning with L84 through to L88, or L93.
- Hernia repair: Any patient with a procedure field beginning with T19 through to T27.
- Knee arthroscopy: Any patient with a procedure field beginning with W82 through to W89.

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