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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 a government programme which facilitated the entry of for-profit surgical centres to compete against incumbent National Health Service hospitals in England. We examine the impact of competition from these surgical centres on the efficiency – measured by pre-surgery length of stay for hip and knee replacement patients – and case mix of incumbent public hospitals. We exploit the fact that the government chose the broad locations where these surgical centres (Independent Sector Treatment Centres or ISTCs) would be built based on local patient 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 the government-facilitated entry of surgical centres led to shorter pre-surgery length of stay at nearby public hospitals. However, these new entrants took on healthier patients and left incumbent hospitals treating patients who were sicker. This paper highlights a potential trade-off that policymakers face when they promote competition from private, for-profit firms in markets for the provision of public services.

1. Introduction

In the 2000s, there was a widespread push in Europe and the United States to increase the role of user choice and provider competition in public services. In general, these pro-market reforms were designed to increase the quality and efficiency of public services like health care and education, which had previously been run through non-market means like performance management (Gaynor and Town, 2011; Propper et al., 2007, 2010). Often, as part of these market-based reforms, policymakers encouraged the entry of private, for-profit firms to compete against public sector providers. These efforts are exemplified by the growing use of charter schools in the United States and private health care providers in publicly funded health systems in Western Europe (Jost et al., 2006; Fryer Jr, 2012). This paper explores how competition generated by the government-facilitated entry of private, for-profit firms affects the performance of incumbent public providers. In particular, we estimate the impact of the entry of a series of private, for-profit surgical centres in the English National Health Service (NHS). Policymakers steered the entry of these surgical centres to areas with high patient waiting times, with the aims of increasing surgical capacity and stimulating competition. We estimate the impact of this private provider entry on the efficiency of incumbent public hospitals, and examine whether it left incumbents with a riskier and more costly mix of patients.

Advocates of diversifying the supply of public services providers argue that private, for-profit entrants will innovate and offer higher quality than incumbents, and that entry of private providers will create competitive pressure on public providers to raise their own

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performance (Le Grand, 2009; Seddon, 2007). We are particularly focused on testing this latter claim: can the entry of private, for-profit surgical centres improve the performance of incumbent public hospitals?

Critics of market-based reforms generally cite the many ways that public services, and health care in particular, differ from highly stylised, perfectly competitive markets, and argue that competition will not improve performance (Jones and Mays, 2009; Fotaki et al., 2008). Moreover, it is sometimes argued that, because new entrants are often much smaller than incumbents (in our case, we analyse surgical centres competing against hospitals), they may not have sufficient scale to affect the behaviour of existing providers (Goddard, 2015). A third criticism is that private, for-profit firms may select customers with desirable characteristics (e.g. better students or less risky patients), leaving public providers treating a riskier or costlier group of users (Los Angeles Times Editorial Board, 2016; Bardsley and Dixon, 2011). More generally, it is not clear that governments are well equipped to determine where to locate entrants in such a way as to engineer effective competition.

The English NHS provides a unique environment in which to test the effect of private, for-profit provider entry on public service providers' performance, and in so doing to analyse the extent to which governments can ‘create’ competition. In the 2000s, the British government facilitated the entry of Independent Sector Treatment Centres (ISTCs). ISTCs are private, for-profit surgical centres focused on provision of routine, high volume elective (i.e. medically necessary, non-emergency, scheduled in advance) surgical procedures to public (NHS) patients. This policy was part of a wider policy package designed to tackle waiting times within the English NHS, the centrepiece of which was an ambitious set of targets to reduce waiting times for surgery. ISTCs were established to rapidly expand capacity in regions deemed at risk of not meeting these targets (Naylor and Gregory, 2009). As we demonstrate, while the placement of these specialty surgical centres was correlated with local public hospital waiting times during the pre-policy period, their placement was uncorrelated with measures of the efficiency and clinical quality of these incumbents over the same period. This implies that treatment assignment was unrelated to the pre-policy levels of the outcome variables we study. In addition, 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 and highlight that our control group serves a good counterfactual for what would have occurred to the treatment group after 2004/5 in the absence of the entry of ISTCs.

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 and Davies, 1990; Martin and 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 affected 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 the entry of private, for-profit specialty surgical centres led to a 16% reduction in pre-surgery LOS at nearby public hospitals – which translates to a 24 percentage point increase in the proportion of patients treated on the day of admission. However, we also find evidence that these entrants engaged in risk selection, 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 an 11.6% deterioration in average patient health status as captured by the Charlson score (defined in Section 4). This increase in patient severity likely led to an increase in post-surgery LOS at incumbent NHS hospitals. 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 driven by changes in volume caused by ISTC entry.

This paper adds to several literatures. First, it builds on previous work assessing how the entry of private, for-profit firms impacts the performance of incumbent public service providers (Hoxby, 1994; Barro et al., 2006; Cutler et al., 2010; Sass, 2006). In general, researchers have struggled to assess the causal impact of competition from new market entrants (e.g. surgical centres and charter schools) into markets for public services because the entry location of private firms is usually endogenous. We exploit the fact that siting of surgical centres in England was driven by government policy tied to waiting times, not our efficiency measure, and show that the entry of ISTCs raised incumbent hospitals' productivity. Second, it adds to the broader literature assessing the impact of hospital competition (Kessler and McClellan, 2000; Gaynor et al., 2015; Cooper et al., 2011). We illustrate that, in markets where payments are regulated, competition can raise hospitals' efficiency. Moreover, we find that smaller entrants can affect the behaviour of larger incumbents. Third, it adds to the literature analysing whether private, for-profit surgical centres offering public services risk-select against public incumbents (Barro et al., 2006; Winter, 2003; Cram et al., 2005; Street et al., 2010; Zimmer and Guarino, 2013; Bifulco and Reback, 2014). We find that the entry of ISTCs left public hospitals with a riskier mix of patients. To some extent, this was by design: ISTCs in England were focused on treating uncomplicated cases. While the entry of specialist surgical centres focused on routine procedures could in theory represent efficient patient sorting, such an arrangement is likely to leave existing providers treating a sicker patient mix and worse off financially, unless it is accompanied by a reimbursement system that adequately adjusts payments to reflect patient severity. The consensus is that NHS payments were not adequately risk adjusted during the period we investigate (Mason et al., 2008), meaning that NHS hospitals that had an ISTC enter nearby were likely left worse off as a result of being left with a sicker mix of patients.

More generally, this paper highlights the trade-offs that policymakers face when considering policies to encourage the entry of for-profit firms to compete with public service providers. Facilitating entry can lead to competition, which can prompt incumbent providers to raise their performance. However, these for-profit entrants may have very different objectives than incumbent providers, and may have a higher propensity to risk-select in order to draw a more advantageous mix of patients. Our work highlights the need for policy-makers to take risk-adjustment of payments seriously when considering policies to promote competition between firms with different objectives and differing abilities to treat complicated cases.

The remainder of 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 ISTC entry on incumbents' performance. Section 4 presents the data and empirical strategy. Section 5 reports the 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 (GP) 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.

The NHS long struggled with waiting times for elective surgery, which, in some cases, could exceed a year. In 1997, a new Labour government was elected promising quick action to reduce waiting times. However, 1 year later, waiting times had increased (Klein, 2013, p.200).[1] Concerns over waiting times became the catalyst for a series of reforms from 2000 onwards, which included rigorous performance management of public hospitals; introduction of patient choice and hospital competition underpinned by prospective reimbursement; and facilitated entry of specialist private surgical centres to compete with larger public hospital incumbents. The new prospective reimbursement system (known as Payment by Results or PbR) was modelled on the Diagnosis-Related Group (DRG) system used in Medicare in the United States (US). Under PbR, hospital reimbursement is tied to activity rather than to annual budgets or block contracts as was previously the case (DH, 2011).

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 reduced to 18 weeks, by 2008) using a series of 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., 2008, 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 run surgical centres (ISTCs) to deliver routine, high-volume diagnostic and elective surgical procedures to English NHS patients.[2] 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 three 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 and Gregory, 2009). Third, whereas NHS physicians are in general permitted to also work in private settings, the first wave of ISTCs (which are the focus of this paper) were not allowed to use NHS doctors. This restriction ensured that ISTCs represented genuine new additions to capacity, rather than drawing away physician labour from nearby public hospitals.

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.

There were 27 Wave 1 ISTCs, all of which operated on a for-profit basis.[3] Of these, 23 opened in 2005 or 2006 (see Fig. 1), and most operated from a single site, often in newly built premises that were often co-located with an existing NHS hospital. In March 2005, a second Phase of ISTCs was announced, of which nine were eventually implemented, with most opening between 2007 and 2008. These Phase 2 ISTCs were smaller, provided a wider range of services including diagnosis, and were often on the same site as existing private hospitals. Unlike Wave 1 ISTCs, Phase 2 ISTCs were permitted to recruit NHS staff and employ NHS consultants privately. Phase 2 ISTCs were also required to provide NHS training placements (Naylor and Gregory, 2009). Given these very different characteristics of the Phase 2 programme, in this paper we focus exclusively on analysing the impact of Wave 1 ISTCs by excluding NHS hospitals that were potentially exposed to competition from Wave 2 entrants.[4]

The ISTC programme had a major impact on the market for some elective surgical procedures (Naylor and Gregory, 2009). From 2006, ISTCs accounted for between 5 and 10% of orthopaedic volume nationally. As the ISTC programme's impact was highly geographically differentiated, the share of patients attending ISTCs was much higher in some areas than in others. 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” (McLeod et al., 2014, p.15). The hope amongst the policymakers responsible for the ISTC programme was that these entrants would be less inclined to cooperate with NHS providers, and more inclined to compete (Stevens, 2004). The prohibition on ISTCs employing NHS physicians, and the fact that they were privately owned, may also have contributed to an institutional culture at ISTCs that was more receptive to competition than that of public hospitals (Le Grand, 1999).

ISTC contracts specified a range of ‘exclusion criteria’ – 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.[5] National Joint Registry data from 2010 indicates that, at NHS hospitals, 20% of hip replacement patients and 19% of knee replacement patients were given ASA scores of 3 or 4. The corresponding figures for ISTCs were only 6 and 8%, respectively (NJR, 2011). Critics of the ISTC programme saw these exclusion criteria as particularly problematic because they allowed the new entrants to dump

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[1] 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.”

[2] DH, 2002. This section also draws on Naylor and Gregory, 2009; Allen and 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.

[3] Two of the 27 Wave 1 ISTCs had parent companies with more mixed ownership structures. Circle Health, which co-ran Nottingham ISTC with the for-profit Nations Healthcare, was to a limited extent a doctors' mutual. The New York Presbyterian Healthcare System, a charity, owned Specialist Hospitals Ltd., a for-profit company that ran Shepton Mallet ISTC. All other Wave 1 ISTCs were run on an exclusively for-profit basis, and even these two exceptional cases were run with a significant for-profit component.

[4] To prevent our estimates being contaminated by effects arising from the Phase 2 ISTC programme, we drop from our estimation sample any NHS hospital whose closest ISTC was a Phase 2 ISTC, provided that this ISTC was close enough to act as a competitor (using a criterion set out in Section 4).

[5] 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.

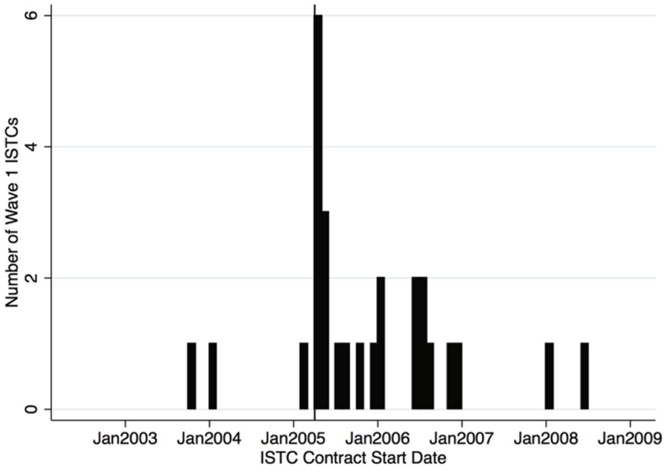

Fig. 1. Timeline of Wave I ISTC openings. Notes: Figure shows the distribution of Wave 1 ISTCs contract start dates, with our treatment-on date (April 2005) marked as a vertical line. Three Wave 1 ISTCs had contract start dates before April 2005 but are not included in our analysis, either because they did not have (sufficient) orthopaedic capacity (the Birkdale Clinic and the Cataract Initiative) or because of ambiguity over location and differentiation from a nearby NHS hospital (Kiddeminister ISTC). Additionally, only two Wave 1 ISTCs had contract start dates after December 2006: the Havant Diagnostic Centre (January 2008), which only performed diagnostic procedures and which is therefore not used for treatment assignment; and the Nottingham NHS Treatment Centre (July 2008).

costlier, more complex patients onto the public hospital system (Wallace, 2006; Kmietowicz, 2006).[6]

3. Hypotheses on the impact of ISTC entry on incumbent public hospitals

This section examines the likely response of public (NHS) hospitals to the entry of private, for-profit 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 substantial 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 and 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. 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. It has therefore been argued that it is reasonable to think of public hospitals during this period as maximising profits plus some additional term reflecting altruistic valuation of quality and/or quantity (Gaynor et al., 2013).

Ultimately, Wave 1 ISTCs differed from public hospitals in three key dimensions. First, they were explicitly for-profit ventures. Second, they were narrowly focused on offering a small range of elective surgical procedures. Third, given their inability to hire NHS staff, their institutional cultures may have differed sharply from those at NHS hospitals. In what follows, keeping in mind these three differences, we present hypotheses about the response of NHS hospitals to the ISTC programme.

3.1. Efficiency

We expect ISTC entry to lead to efficiency improvements at nearby incumbents. As mentioned in Section 1, we measure hospital efficiency using pre-surgery LOS. Prospective reimbursement systems (like PbR in England) pay hospitals on the basis of outputs rather than inputs. This creates incentives for hospitals to reduce marginal costs by shortening patient LOS (Cutler, 1995). Empirical studies of England (Farrar et al., 2009), the United States (Feder et al., 1987; Guterman and Dobson, 1986; Feinglass and Holloway, 1991; Kahn et al., 1990), Israel (Shmueli et al., 2002) and Italy (Louis et al., 1999) provide evidence that prospective reimbursement leads to shorter LOS.

While prospective reimbursement systems provide incentives for all hospitals to reduce patient LOS, these incentives will likely be particularly sharp in more competitive markets. Hospitals located in less competitive 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 in more competitive markets have greater opportunity to expand activity by capturing market share from other hospitals. To create capacity for such expansion, in health care systems with prospective reimbursement, hospitals in more competitive markets are likely to take 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 found 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). In light of this theoretical prediction and empirical evidence, 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 prospective reimbursement. 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 announced that performing elective surgery on the day of a patient's admission was a key measure of hospital performance, and highlighted that ISTCs would be particularly effective at this. 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 facilities and argued (2006, p.20) that public hospitals would have to respond to competition from private entrants by streamlining their production: "Same-day admissions [i.e. admission on day of surgery] 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 admission on day of surgery means that, in addition to the more general incentives to increase efficiency brought about by surgical centre entry, we expect public hospitals facing increased pressure from private surgical centres to have improved their performance in this dimension in particular.

3.2. Case mix

The entry of private, for-profit surgical centres could also change the case mix at nearby incumbents due to risk selection 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 – as in many other markets for the provision

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[6] 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% more per procedure than NHS providers (AC & HC 2008, p.51). Many observers (e.g. HCHC, 2006; Squires, 2007; Player and Leys, 2008; Pollock and Godden, 2008; see also Moore, 2008 and McLeod et al., 2014) argue that these two provisions meant that ISTCs offered poor value for money.

of public services, such as social care and education – the profitability of treating a given customer will be influenced by characteristics of the customer that are often imperfectly observed.

The influence of patient characteristics on profitability provides all hospitals with an incentive to refuse to treat the sickest patients. However, private, for-profit entrants like ISTCs are likely to be more willing than public hospital incumbents to actively select against costly patients, as for-profit firms are able to redistribute profits to shareholders, whereas public hospitals are, at most, only allowed to reinvest profits into the organisation. The literature on specialty hospitals in the US, for example, has found evidence that these providers select low-risk patients, leaving the sickest patients to nearby general hospitals (Barro et al., 2006; Winter, 2003; Cram et al., 2005).

Two further factors add weight to the hypothesis that ISTCs had stronger incentives to risk-select than public hospital incumbents. First, ISTCs could legally decline to treat complicated cases, whereas public hospitals were formally prohibited from doing so. Second, as mentioned previously, ISTCs were prohibited from using NHS doctors, so their workplace culture likely differed sharply from that at incumbents. As Rose-Ackerman (1996) notes, the culture of staff plays a key role in dictating firm behaviour – thus these cultural differences may have led ISTCs to be more willing than NHS providers to engage in profit-driven risk-selection.

Prospective reimbursement encourages cream-skimming, since it provides incentives for hospitals to avoid admitting patients whose cost of treatment is likely to exceed the regulated payment (Allen and Gertler, 1991; Ellis and 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 case mix over this period (either as a result of the introduction of prospective reimbursement, or for other reasons).

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 ISTC-exposed public hospitals experienced a larger reduction in average patient health status (measured using a Charlson Index) than public hospitals not exposed to the entry of an ISTC. Providing evidence of such an effect of ISTC entry is important because the case mix 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]

4. Data, definition of treatment group, and estimation strategy

Our aim is to estimate the causal effect of the entry of private surgical centres on the efficiency, case mix, and case load 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) before and after entry occurred. This section describes our outcome measures, construction of treatment groups, and identification strategy.

4.1. Data and outcome variables

Our dataset is derived from the NHS Hospital Episode Statistics (HES) (HSCIC, 2016), which contains the universe of government-funded hospital admissions in England.[8] Our data extract consists of all elective hip and knee replacements on patients aged 55–100 performed between financial years 2002/3 and 2008/9 (see Table 1). We focus on hip and knee replacements 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 and 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. As a result, any observed changes in LOS will likely be driven by NHS reforms, not by differential uptake of new medical technologies.

We focus on hip and knee replacements performed in NHS hospitals. NHS hospital trusts (firms) often consist of multiple hospitals (individual sites) that can be located up to 100 km away from each other. We therefore analyse the data at site (hospital) level rather than trust (firm) level, and assign hospitals (sites) to treatment and control groups based on the site's proximity to the nearest ISTC. All references to ‘hospitals’ in this paper are therefore to hospital sites, not to trusts (firms). After cleaning and imputing missing values for the site code field, and applying exclusion criteria detailed below, there are 166 public hospitals treating hip and knee replacement patients from 2002/3 to 2008/9.

Researchers have generally struggled to quantify hospital efficiency. In the absence of hospital cost data, many studies use proxy measures of efficiency such as LOS (Fenn and Davies, 1990; Martin and 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. Moreover, a hospital's average LOS may reflect undesirable hospital behaviour such as cream skimming (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 and Smith, 1996; Sudell et al., 1991).

In this study, we use an innovative method to obtain a cleaner proxy for hospital efficiency. We decompose LOS for hip and knee replacements into two parts: the time from admission to surgery (pre-surgery LOS), and the time from surgery until discharge (post-surgery LOS). We hypothesise that, for elective orthopaedic surgery, pre-surgery LOS is not significantly influenced by patient characteristics, as there is rarely a clinical rationale for admitting an elective orthopaedic surgery patient before the scheduled day of their operation. In the early 2000s, fewer than 20% of hip and knee replacement patients had surgery on the day they were admitted to the hospital. Patients were often kept overnight before elective surgery not for clinical reasons, but because operating

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[7] Kelly and Stoye (2016) 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 ISTC entry imposed negative externalities on the NHS hospital's case mix via patient selection. In this paper, we therefore measure risk selection by instead comparing the evolution of average patient health status at NHS hospitals that had an ISTC enter nearby with the evolution of average patient health status at comparable NHS hospitals that did not have an ISTC enter nearby.

[8] 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 and 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.

Table 1
Number of hip and knee replacements in estimation sample and means of key dependent variables.

Year	Observations	Mean pre-surgery LOS	% treated on day of admission	Mean post-surgery LOS	Mean total LOS	Mean Charlson score
2002/3	57,559	0.934	0.157	8.91	9.84	0.701
2003/4	65,116	0.911	0.174	8.26	9.18	0.755
2004/5	65,785	0.846	0.228	7.84	8.69	0.885
2005/6	68,688	0.772	0.297	7.36	8.13	0.96
2006/7	71,043	0.629	0.422	6.82	7.45	1.03
2007/8	74,159	0.433	0.604	6.43	6.86	1.11
2008/9	75,876	0.335	0.700	6.27	6.60	1.20
Total	478,226	0.677	0.384	7.34	8.02	0.964

Notes: Table reports the total number of NHS-funded hip and knee replacements that meet our sample restrictions and are performed in NHS facilities that are used in our analytic sample. The means of key outcomes variables are estimated from our analytic sample of data. Table B.1 (Supplementary Material) presents separate versions of this table for hip replacement and knee replacement. Length of stay is measured in days.

Table 2
Share of variation in pre-surgery and post-surgery LOS explained by patient characteristics.

	(1)	(2)
	Pre-surgery LOS	Post-surgery LOS
R-squared	0.004	0.128

Notes: Table reports R-squareds from regression of pre-surgery and post-surgery length of stay on patient characteristics in years 2002/3 to 2008/9. The patient characteristics included are: Charlson score; number of diagnoses; IMD income deprivation score; IMD health and disability deprivation score; dummy variables indicating self-discharge, urban residence, mixed ethnicity, Asian ethnicity, black ethnicity, other ethnicity, and unknown ethnicity; and a full set of case mix dummies with gender interacted with five-year age bins. Table C.1 (Supplementary Material) reports the complete set of coefficients from these regressions.

rooms were not available on their scheduled surgery date.[9] 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. By contrast, we view post-surgery LOS as a joint product of underlying hospital efficiency and patient characteristics. Therefore, when we estimate the effect of ISTC entry on post-surgery LOS, we interpret the results as a combined outcome of (i) competitive pressure brought about by ISTC entry, leading to efficiency improvements by nearby public incumbents, and (ii) ISTC cream skimming, leaving nearby public hospitals with a sicker mix of patients.

To test our hypothesis that pre-surgery LOS is less influenced by patient characteristics than post-surgery LOS, we regress pre- and post-surgery LOS for hip and knee replacements against a range of patient characteristics. Patient characteristics included in the regression are: Charlson score; number of diagnoses; Index of Multiple Deprivation (IMD) income deprivation score; IMD health and disability deprivation score; dummy variables indicating self-discharge, urban residence, mixed ethnicity, Asian ethnicity, black ethnicity, other ethnicity, and unknown ethnicity; and a full set of case mix dummies with gender interacted with five-year age bins. The results, reported in Table 2, are consistent with our hypothesis – patient characteristics explain less than half a per cent of the variation in pre-surgery LOS, but 12.8% of the variation in post-surgery LOS.

We observe changes in patient health status at NHS hospitals before and after ISTCs enter in order to directly measure whether risk selection occurred. To measure patient health status, we calculate a Charlson score for each patient. The Charlson score 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).[10] As proxies for health status and clinical risk, we also use the patient's age, as well as the IMD income domain (Noble et al., 2004), which reports the percentage of households in the patient's residential Lower Super Output Area (LSOA, a statistical geographical areas containing around 1500 residents) that are income deprived (in our dataset this variable ranges from 0 to 83).

The data includes 478,226 hip and knee replacements performed from 2002/3 to 2008/9 that met the sample restrictions. As Table 1 illustrates, during the analysis period pre-surgery LOS, post-surgery LOS, and total LOS fell considerably.[11]

4.2. Treatment assignment

We assign public hospitals to treatment or control groups based on their geographical proximity to the new market entrants, on the assumption that exposure to competition from these entrants is a product of proximity. 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.

We measure the distance travelled by each hip and knee replacement patient to hospital using the centroids of a patient's residential LSOA to define home location. We then calculate, for each NHS hospital, percentiles of patient distance travelled (e.g. the distance that captures 25% of a hospital's hip and knee replacement patients). Percentiles of patient distance travelled can be endogenous to hospital quality – for example, a high-quality hospital may attract patients from further afield. To ameliorate this concern, we use percentiles of patient distance travelled based on patient flows from 2002/3 to 2004/5 (i.e. before implementation of either the ISTC programme or patient choice of hospital for elective surgery).

Table 3 presents descriptive statistics for quantiles of patient distance travelled and the exposure of NHS hospitals to Wave 1 ISTCs within each of these quantile bands. Panel A presents the mean kilometre distances (averaging over the NHS hospitals in our estimation sample) corresponding to these patient percentile travel distances. The mean value of the 25th percentile of travel distance for hip and knee replacement patients is 4.25 km, while for the 95th percentile it is

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[9] Personal communication with NHS physicians supported this claim. We recognise too that not all pre-surgery LOS is wasted time and there may, in rare cases, be clinical reasons to admit patients before the day of surgery. However, unless this clinical need is correlated with ISTC treatment status, this issue will not threaten our strategy for identification of the effects of ISTC treatment on efficiency.

[10] The Charlson score ranges from 0 to 130. However, only around 8% of patients have a score above 6. We therefore cap the score at 6, to ensure that our results for this outcome variable are not driven by outliers.

[11] Appendix A provides further information about how hip and knee replacement patients are identified, how the LOS-related variables are constructed, and how missing site codes are imputed. Separate versions of Table 1 for hip and knee replacements are reported in Table B.1. Descriptive statistics for all variables used in this paper are reported in Table B.2. Because our results for hip replacements and knee replacements were very similar, we pooled together these surgical procedures in the estimates reported in the main body of this paper and included a dummy variable indicating hip replacement, to capture level differences in the outcome variable across the two surgical procedures. Tables F.1 to F.4 show that our results are qualitatively unchanged when separate treatment effects are estimated for hip replacements and knee replacements.

Table 3
Patient travel distance percentile groups and kilometre distances.

A. Average kilometre distances corresponding to percentiles in the distribution of patient distance travelled from home to hospital, 2002/3 to 2004/5

Percentile of patient flows	5th	10th	25th	50th	75th	90th	95th
Kilometres	1.46	2.19	4.25	8.24	13.24	21.10	26.34

B. Number of NHS Hospitals with a Wave 1 ISTC within percentiles of patient distance travelled

Percentile of patient flows	5th	10th	25th	50th	75th	90th	95th
NHS hospitals (total number = 166)	8	9	11	16	25	48	62

Notes: Panel A reports the mean distances corresponding to percentiles in the distribution of patient distance travelled from home to hospital, averaging over the 166 NHS hospitals in our estimation sample. These mean distances are calculated based on percentiles of patient distance travelled in the 3 years before the ISTC programme began (2002/3 to 2004/5). Panel B reports the number of NHS hospitals with a Wave 1 ISTC within each of these patient distance percentile groups. In our main specification, the 25th percentile and 95th percentile are used to define the High and Low Treatment groups respectively.

26.34 km. Panel B shows how many NHS hospitals had a Wave 1 ISTC enter within each of these distance bands.

To define Treatment and Control groups, we start by assuming that, if there is no ISTC entrant within an NHS hospital's 95th percentile of patient distance travelled – a widely-used definition of market size – then the NHS hospital is not exposed to the ISTC programme. These NHS hospitals are assigned to the Control group. We assign the remaining NHS hospitals to discrete treatment groups based on natural breaks in the distribution of distances from NHS hospital to the nearest ISTC in our dataset, measured in terms of patient travel percentiles (see Fig. 2). This assignment yields two discrete treatment groups – a Low Treatment group (ISTC within 95th percentile of patient distance travelled but not within 25th percentile) and a High Treatment group (ISTC within 25th percentile of patient distance travelled).[12] There are 11 NHS hospitals in the High Treatment group, 51 in the Low Treatment group and 104 in the Control group. Fig. 3 maps the ISTCs and public hospitals in our data. In the robustness section, we show the effect of changing the threshold used to define the treatment groups on our estimates.

One obvious concern is that treatment assignment might be endogenous to our primary outcome (LOS) because ISTCs may have opened near inefficient NHS providers. However, as noted earlier, ISTC placement decisions were driven by local NHS hospital waiting times, not by local hospital LOS or other performance measures. Waiting times are dependent on a wide range of demand and supply side capacity-related factors beyond simply hospitals' LOS. As a result, there is little reason to expect hospitals with high waiting times to necessarily have high LOS. Indeed, as we observe, hospitals' LOS is uncorrelated with their waiting times.[13]

Table 4 illustrates this point by comparing waiting times, total LOS, pre-surgery LOS, and post-surgery LOS for hip and knee replacement at High Treatment group, Low Treatment group and Control group hospitals in 2002/3 (the year that ISTC placement decisions were being made). Average waiting times in 2002/3 were around 6% higher at High Treatment group hospitals than at hospitals in our other groups. By contrast, there are no systematic differences between total LOS or post-surgery LOS at High Treatment group hospitals and others. Pre-surgery LOS is slightly lower at High Treatment group hospitals – i.e. ISTCs tended to enter near NHS hospitals that were already slightly more efficient, although there is no evidence to suggest that these small efficiency differences were a factor in ISTC location decisions. As we show in later analysis, these differences in pre-surgery LOS are not associated with any statistically significant differences in trends of pre-surgery between 2002/3 and 2004/5, prior to the entry of ISTCs – which is the key assumption of our DiD identification strategy. The fact that ISTCs entered where nearby public hospitals were already more efficient might be of concern if we were to find that ISTC exposure led nearby public hospitals to become less efficient, as it might suggest that mean reversion is driving our results. However, as we find the opposite (i.e. ISTCs entered near public hospitals that were already more efficient, and that became even more efficient in relative terms as a result of ISTC exposure), we have no reason to be concerned that these small differences in pre-ISTC levels of pre-surgery LOS across treated and control groups will confound our DiD estimates.

We allocate NHS hospitals to treatment categories by comparing distance to ISTC with percentiles of patient distance travelled, not with kilometre distances, to control for rural-urban differences – treatment assignment based on fixed distances will over-estimate the size of markets in urban areas relative to rural areas, given the impact of urban congestion on travel speeds. In the robustness tests, we examine whether our results change if we use a treatment assignment strategy based on fixed distances from public hospital to ISTC.

4.3. Treatment start and end dates

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. As the first ISTCs in our analysis opened in April 2005 (financial year 2005/6), we define 2004/5 as the last pre-treatment (pre-ISTC-programme) financial year.[14] However, some ISTCs did not begin operations until 6 months to a year after their contracted start date. Moreover, when the initial ISTC contracts (generally around 4 or 5 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 using data from the 2004/5 and 2008/9 financial years. We choose 2004/5 as the pre-treatment period in our main specification because it is the last year before the first contract start date amongst the ISTCs we use for treatment assignment, 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 the 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. For

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[12] We arrived at these percentile splits using a Jencks Natural Breaks based analysis, implemented with Stata's group1d command. See Appendix A for more details.

[13] The correlation between hospitals' average total LOS and average waiting time for hip and knee replacement surgery in 2002/3 is 0.06, *p*-value 0.46). Simple bivariate regressions of the log of average total LOS on log of average waiting time during this period yield a tiny and statistically insignificant coefficient (0.03, *p*-value 0.275). 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.

[14] All references to years in this paper are to financial years. Three Wave 1 ISTCs had contract start dates before April 2005 but are not included in our analysis, either because they did not have (sufficient) orthopaedic capacity or because of ambiguity over location and differentiation from a nearby NHS hospital. See Appendix A for further details.

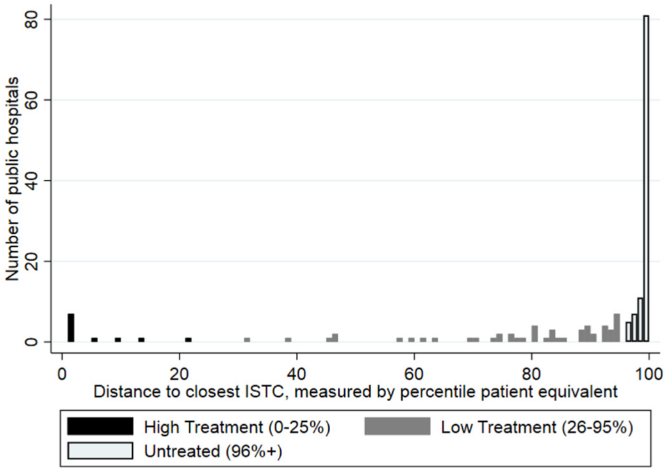

Fig. 2. Proximity of public hospitals to Wave 1 ISTCs, measured in terms of percentiles of patient distance travelled. Notes: Figure shows the distribution of public hospitals' proximity to private specialty surgical centre entrants (ISTCs), measured in terms of equivalents to percentiles of patient distance travelled to hospital, with the patient's Lower Super Output Area (LSOA) of residence used as a proxy for home address. Public hospitals were grouped into discrete treatment groups based on natural breaks in the distribution of treatment intensities – a High Treatment group for public hospitals whose nearest ISTC was closer than the 25th percentile of patient distance travelled, and a Low Treatment group for public hospitals whose nearest ISTC was closer than the 95th percentile of patient distance travelled, but further away than the 25th percentile.

robustness, we show how treatment effects change annually in the post-reform years from 2005/6 to 2008/9, and report estimates that define a public hospital's treatment start date as the contract start date of its nearest ISTC.

4.4. Regression specification

We identify the impact of hospital market entry using a DiD regression framework where dummy variables indicating treatment group membership are interacted with a post-policy dummy, which is switched on for the 2008/9 financial year. Regressions are run at the patient level and any non-binary dependent variables are log-transformed (after adding 1 to any variables that have a minimum value of zero) such that the treatment effects are interpretable as percentage changes.

Our basic DiD specification is:

$$y_{ijt} = \beta_0 + \beta_1 \mathrm{post}_t + \beta_2 \mathrm{high}_j + \beta_3 \mathrm{low}_j + \beta_4 \left(\mathrm{high}_j * \mathrm{post}_t\right) + \beta_5 \left(\mathrm{low}_j * \mathrm{post}_t\right) + \varepsilon_{ijt} \tag{1}$$

In this specification, t denotes the time period (financial year), $post_t \in \{0,1\}$ denotes whether an observation occurs in the post-reform period, y_{ijt} denotes the outcome variable under consideration for patient i attending hospital j at time t, and $high_j$ and low_j denote dummies for the High and Low Treatment groups respectively. Treatment effects are given by the coefficients on the interaction terms, β_4 and β_5. We also include a dummy in our regressions to control for the type of procedure a patient undergoes (hip or knee surgery) but suppress this in the notation for simplicity.

Our second specification includes hospital 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:

$$y_{ijt} = \beta_0 + \beta_1 \left(\mathrm{high}_j * \mathrm{post}_t\right) + \beta_2 \left(\mathrm{low}_j * \mathrm{post}_t\right) + \eta_t + \mu_j + \varepsilon_{ijt} \tag{2}$$

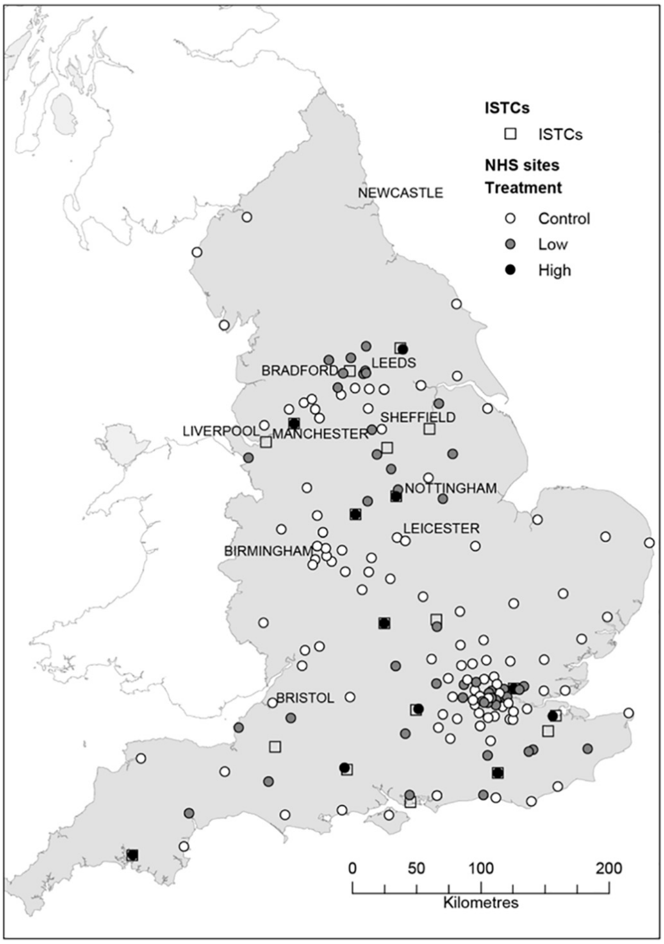

Fig. 3. Map of ISTCs and NHS hospitals. Notes: Figure shows the geographic location of the NHS hospitals included in our regression sample, and the 19 ISTCs used to define treatment exposure. NHS hospitals not included in our analysis (for example, because their nearest ISTC had no orthopaedic capacity or was a Phase 2 ISTC) are not depicted on the map. In some cases, Low Treatment Group hospitals are further from an ISTC than some Control group hospitals, because treatment assignment is performed using the percentiles of patient distance travelled at NHS hospitals, not at ISTCs themselves. NHS hospitals located very close to an ISTC and not assigned to the High Treatment group are generally located in urban areas, where the 25th percentile of patient distance travelled (the threshold used to define the High Treatment group) can be a very short distance.

Our third specification is identical to (2), but includes an extensive set of controls for patient and hospital characteristics.[15] All specifications are estimated using ordinary least squares (OLS), with standard errors clustered at the hospital level to account for correlation in unobservables within hospitals (between patients and over time).

There are two core threats to our identification strategy. The first is that trends in outcome variables may not have been parallel between treatment and control groups prior to ISTC entry. The second is that there may have been other time-varying policy changes that also affected outcomes concurrently with the ISTC programme. To address the first possibility, we demonstrate that treated and control hospitals had parallel trends for our key dependent variables before the ISTC programme was launched by showing trends in the data graphically, and formally testing for statistically significant differences in trends. To address the risk that concurrent and correlated policy shocks drive our results, Section 5.4 discusses the two most prominent policy

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[15] 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 estimates the effect of ISTC exposure on public hospital performance *conditional on* observable patient characteristics.

Table 4
Comparison of performance indicators at high treatment group, low treatment group and control group hospitals in 2002/3.

	(1) High treatment group	(2) Low treatment group	(3) Control group	(4) Low treatment group + control group
Waiting times	271.4	248.5	259.8	256
Length of stay	9.985	9.843	10.14	10.04
Pre-surgery length of stay	0.889	0.923	0.925	0.925
Post-surgery length of stay	9.096	8.920	9.214	9.119

Notes: Table reports average waiting times, total length of stay (LOS), pre-surgery LOS, and post-surgery LOS for hip and knee replacement patients at treatment and control group hospitals in 2002/3 (i.e. before commencement of the ISTC programme).

changes that could have affected outcomes contemporaneously with ISTC entry – the introduction of hospital competition via patient choice of hospital for elective surgery, and the enactment of differential health policies by Strategic Health Authorities (SHAs) – and illustrates that controlling for these policy changes does not materially influence our main estimates.

5. Results

5.1. Descriptive evidence

Fig. 4 presents the evolution of key outcome variables – pre-surgery LOS, percentage treated on day of admission, post-surgery LOS, total LOS and Charlson score – between 2002/3 and 2008/9, for treatment and control groups. 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 time period captured by shaded region or, if behavioural responses took place with a lag, some time thereafter. Each data point represents a month, but the plots are smoothed using a moving average of the month and the previous quarter. These graphs allow visual examination of whether treated and control groups followed similar trends prior to the entry of ISTCs.

Panel A shows changes in pre-surgery LOS and illustrates that High Treatment group, Low Treatment group, and Control group hospitals follow similar trends before ISTC entry. Over and above a secular downward trend, reflecting general improvements in turnaround time, there is evidence of a treatment effect from ISTC entry. After ISTC entry, 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. Panel B shows trends in the percentage of patients treated on the day of admission. All three groups have similar pre-entry trends, but after ISTC entry the percentage of patients treated on the day of admission increases more quickly for High Treatment group hospitals. Overall, Panels A and B provide 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.

Panel C shows trends in post-surgery LOS, which have a markedly different pattern. The High Treatment group, Low Treatment group, and Control group hospitals have similar pre-entry trends. However, there is a sharp increase in post-surgery LOS for the High Treatment group from the middle of the ISTC entry period onwards. Overall, after ISTC entry post-surgery LOS decreases in the High Treatment group less than in the Control group. Panel D presents trends in total LOS, which follow a similar pattern to post-surgery LOS. As discussed in Section 4.1, post-surgery LOS (and therefore total 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 cream skimming by entrants. Panels C and D therefore provide suggestive evidence that the negative impact of ISTC cream skimming on nearby public hospitals' LOS may have outweighed any efficiency improvements with respect to LOS arising from competitive pressure from these new market entrants.

Panel E looks more directly at the impacts of ISTC entry on public hospitals' case mix by plotting the evolution of average Charlson scores. The pre-policy levels and trends of the Charlson score are similar across treatment and control groups. However, the High Treatment group starts receiving sicker patients from early in the ISTC entry period. This evidence 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. Graphical evidence for other case mix variables is presented Appendix D.

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 was to reduce waiting times for admission to hospital, not to reduce time spent in hospital or to improve clinical quality.[16]

5.2. Regression-based difference-in-difference estimates

Table 5 presents our main difference-in-difference estimates of the effect of ISTC entry on log of pre-surgery LOS, percentage of patients treated on day of admission, and log of post-surgery LOS. The sample includes hip and knee replacements, with a hip replacement dummy included to account for level differences in outcomes between the two procedures.

In Columns (1) to (3), the dependent variable is log of pre-surgery LOS. Column (1) presents estimates of Eq. (1), without hospital fixed effects or patient controls. The estimate implies that ISTC entry led to a 14.4% ($=100(e^{-0.156}-1)$) reduction in pre-surgery LOS. In Column (2), we estimate Eq. (2), adding hospital and month × year fixed effects. The results are qualitatively unchanged and imply that ISTC entry led to a 16.1% reduction in pre-surgery LOS. In Column (3), we add patient controls and the results again remain qualitatively unchanged (we find a 16.6% reduction). That controlling for patient characteristics barely shifts the estimated treatment effects for the High Treatment group suggests 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., 2005). The impact on the Low Treatment group is of the same sign and around one third the magnitude 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

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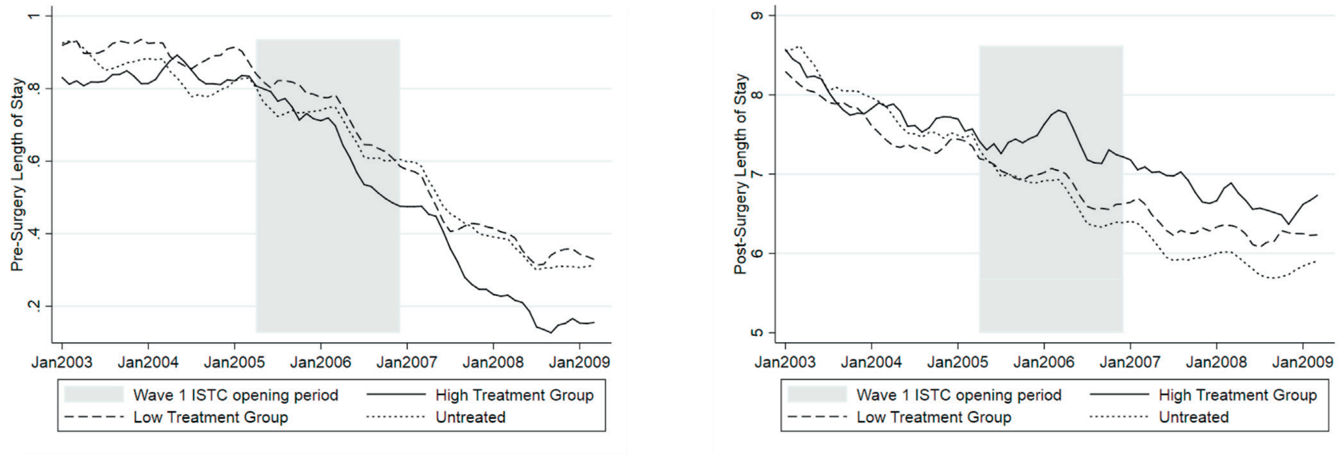
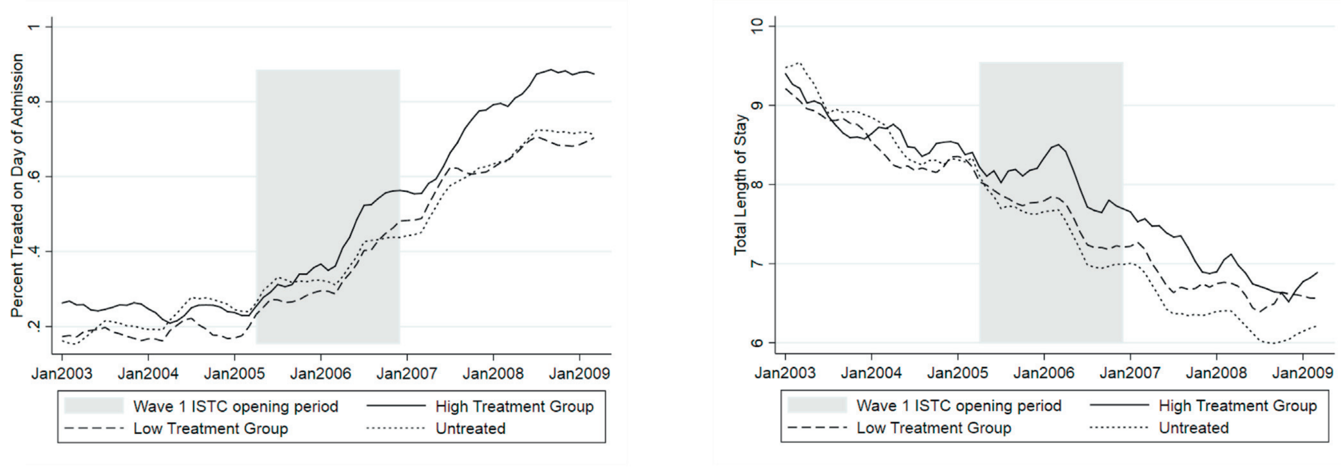
[16] We deliberately choose not to study the effect of ISTC entry on nearby public hospital waiting times in the main body of this paper because ISTCs were strategically located near hospitals with high waiting times, implying that treatment is endogenous with respect to this outcome variable. Nevertheless, for interested readers, the Appendix I presents graphical evidence concerning the evolution of waiting times at treatment and control group hospitals, and DiD estimates corresponding to this graphical evidence. The accompanying text explains in further detail why our DiD estimates cannot be interpreted as causal effects of ISTC entry on waiting times.


A: Pre-Surgery Length of Stay

C: Post-Surgery Length of Stay


B: Percentage Treated on Day of Admission

D: Total Length of Stay

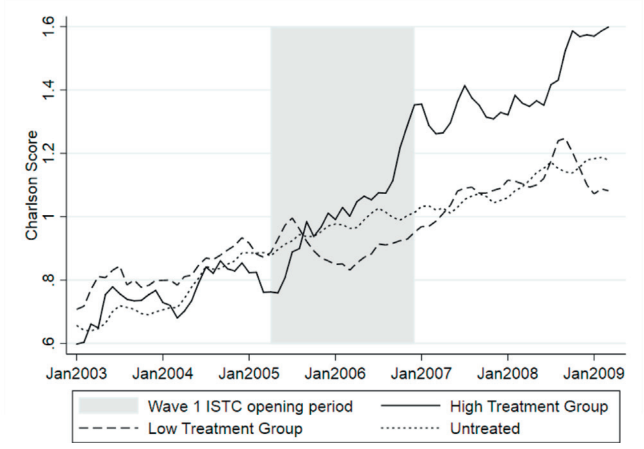

E: Charlson Score

Fig. 4. Trends in key outcomes variables. Notes: Sample includes hip and knee replacement patients. Treatment groups are as defined in Table 5 notes. Graphs show moving averages of hospital means calculated over a month and the previous quarter. Shaded area marks main period of Wave 1 ISTC entry. Figs. F.1 and F.2 provide versions of figures for hip replacement patients and knee replacement patients separately.

impacts of ISTC entry on the Low Treatment group, but that our research design does not have sufficient power to detect them.

Columns (4) to (6) examine the effect of ISTC entry on the proportion of patients who were treated on the day of admission. The results are similar with and without hospital fixed effects, and with and without patient controls. The results in Column (5), which estimates Eq. (2), shows that ISTC entry led to a 24.3 percentage point reduction in the proportion of patients treated on the day of admission at High

Table 5
Impact of ISTC entry on length of stay at nearby public hospitals (treatment defined using patient flows).

	Log of pre-surgery length of stay			% treated on day of admission			Log of post-surgery length of stay		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High treatment × post	−0.156**	−0.176***	−0.181***	0.217**	0.243***	0.248***	0.0813*	0.0482	0.0242
	(0.0636)	(0.0659)	(0.0663)	(0.0886)	(0.0917)	(0.0919)	(0.0481)	(0.0498)	(0.0445)
Low treatment × post	−0.0455	−0.0642	−0.0649	0.0648	0.0912	0.0916	0.0478	0.0505*	0.0434
	(0.0480)	(0.0459)	(0.0465)	(0.0682)	(0.0656)	(0.0662)	(0.0312)	(0.0296)	(0.0311)
Hospital fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Month × year controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Patient controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	141,661	141,661	141,443	141,661	141,661	141,443	141,661	141,661	141,443
R-squared	0.204	0.394	0.401	0.228	0.445	0.450	0.064	0.121	0.260
Mean dependent var	0.846	0.846	0.846	0.228	0.228	0.228	7.84	7.84	7.84

Notes: Sample includes hip and knee replacement patients. High and Low Treatment groups are defined using percentiles of patient distance travelled in 2002/3 to 2004/5. 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. Our pre-reform period is 2004/5; our post-reform period is 2008/9. Mean Dependent Variable reports the average value of the outcome variable (not logged) in our pre-reform period, 2004/5. Standard errors are clustered at the hospital level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All regressions include a dummy variable indicating hip replacement. The regressions in Columns (1), (4) and (7) include dummy variables indicating the High Treatment group, Low Treatment group and post-reform period. Coefficients on these variables are always near-zero and statistically insignificant. Table E.1 reports coefficient estimates for these variables. The specifications reported 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 procedure type (hip replacement cemented, hip replacement uncemented, revision to hip replacement, knee procedure revisions); indicators for self-discharge, urban residence, mixed ethnicity, Asian ethnicity, black ethnicity, other ethnicity, and unknown ethnicity; and a full set of case mix dummies with gender interacted with five-year age bins. Table E.3 reports coefficient estimates for these variables. Tables F.1 and F.2 report separate estimates for hip replacements and knee replacements respectively.

Treatment group hospitals, from a baseline of 22.8% in 2004/5. As with pre-surgery LOS, the impact on the Low Treatment group is same-signed, smaller, and never significant at conventional levels.

Columns (7) to (9) present estimates of the effect of ISTC entry on post-surgery LOS. In Column (7), we estimate that the entry of ISTCs led to an 8.47% increase in post-surgery LOS at High Treatment group hospitals. However, this effect is only significant at the 10% level. The precision and magnitude of our estimates is reduced when hospital and month × year fixed effects are included, in Column (8). Adding patient controls further reduces the size of the point estimate and decreases precision, as we would expect if patient characteristics affect post-surgery LOS and it is selection of less riskier patients into ISTCs and out of NHS hospitals which drives the treatment effects on post-surgery LOS. That patient controls reduce the magnitude of the post-surgery LOS estimates, but have little impact on the pre-surgery LOS estimates, provides further evidence that patient characteristics are a major driver of post-surgery LOS, but have little influence on pre-surgery LOS.

We interpret changes in post-surgery LOS resulting from ISTC entry as a joint product of (i) changes in the mix of patients being treated by public hospitals, due to cream skimming 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% level, the estimates in Column (7) suggest that the increases in post-surgery LOS generated by cream-skimming were larger than the reduction in LOS generated from any efficiency gains in terms of the total time patients spent in the hospital.

An important point to note, in relation to the results presented in Table 5, is that the unreported coefficients on the High Treatment and Low Treatment indicators in Columns (1), (4) and (7) are always near-zero and statistically insignificant. For example, in Column (1) the coefficient on the High Treatment variable is 0.0308 (0.0442). These results imply that the treatment and control groups are balanced in terms of the pre-treatment, 2004/5 levels of the outcomes under investigation, consistent with the graphical evidence in Fig. 4 discussed above.[17]

Table 6 tests for cream skimming directly by assessing whether the entry of an ISTC left nearby public hospitals with a riskier mix of patients. We estimate Eq. (1) and Eq. (2) with hospital and month × year fixed effects; no specifications include patient controls. Columns (1) to (4) indicate that ISTC entry led to an 11.6% increase in the average Charlson score of patients at High Treatment group hospitals – or a 6.2 percentage point increase in the proportion of patients with a Charlson score of three or more – significant at the 5% level. Column (5) indicates that ISTC entry led to a 5.54% increase in the IMD income deprivation score at High Treatment group hospitals, although this point estimate reduces in magnitude and becomes imprecise when hospital and month × year fixed effects are added. We do not find that ISTC entry led to a precisely estimated increase in patient age at nearby NHS hospitals.

Table 7 presents event study estimates of year-by-year effects of ISTC entry on log of pre-surgery LOS, percentage of patients treated on day of admission, and log of Charlson score at exposed NHS hospitals, from 2004/5 to 2008/9. The estimates mirror our graphical evidence in Fig. 4 and show that ISTC entry had a statistically significant effect on these outcomes at exposed NHS hospitals from 2007/8 onwards.

Appendix J explores the impact of ISTC entry on clinical quality at NHS hospitals by analysing changes in 30-day in-hospital mortality from acute myocardial infarction (AMI) at nearby public hospitals. We find that, after controlling for patient characteristics, ISTC entry did not have a statistically significant effect on AMI mortality at nearby public hospitals. The results suggest that the efficiency improvements reported in Columns (1) to (6) of Table 5 were achieved without any evidence of concomitant deterioration in clinical quality.

5.3. Treatment assignment using fixed distances

Table 8 presents estimates of the effect of ISTC entry when treatment assignment is based not on patient flows but on fixed kilometre distances between ISTCs and NHS hospitals. Specifically, the High Treatment group comprises NHS hospitals that had an ISTC enter within 5 km, the Low Treatment group comprises NHS hospitals that had an ISTC enter within 30 km (but not within 5 km), and the Control group comprises NHS hospitals that did not have an ISTC enter within 30 km. Using this definition, the High Treatment group contains 14 hospitals, the Low Treatment group 78 hospitals, and the Control group 77 hospitals. Table 8 indicates that ISTC entry within 5 km of an NHS

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[17] The coefficients on the High Treatment and Low Treatment indicators are reported in Table E.1 of Appendix E.

Table 6
Impact of ISTC entry on case mix at nearby public hospitals (treatment defined using patient flows).

	Log of Charlson score		Charlson score of 3 or more		Log of IMD income deprivation score		Log of age (coefficients × 100)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High treatment × post	0.111**	0.110**	0.0628**	0.0618**	0.0539**	0.00803	0.281	0.199
	(0.0522)	(0.0528)	(0.0297)	(0.0301)	(0.0254)	(0.0259)	(0.294)	(0.259)
Low treatment × post	−0.00387	0.00597	−0.000865	0.00499	−0.0266	−0.0150	0.164	0.116
	(0.0244)	(0.0246)	(0.0144)	(0.0144)	(0.0262)	(0.0154)	(0.182)	(0.182)
Hospital fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Month × year fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	141,661	141,661	141,661	141,661	141,443	141,443	141,661	141,661
R-squared	0.008	0.035	0.008	0.035	0.019	0.174	0.000	0.011
Mean dependent variable	0.885	0.885	0.205	0.205	11.6	11.6	71.3	71.3

Notes: Sample includes hip and knee replacement patients. Treatment groups, pre-reform and post-reform period, mean dependent variable, standard errors and statistical significance are defined as in the Table 5 notes. All regressions include a dummy variable indicating hip replacement. In addition, the regressions in Columns (1), (3) and (5) and (7) include dummy variables indicating the High Treatment group, Low Treatment group and post-reform period. Coefficients for these variables are reported in Table E.2. Tables F.3 and F.4 report separate estimates for hip replacements and knee replacements respectively.

hospital led to a 14.7% reduction in pre-surgery LOS, a 21.9 percentage point increase in the share of patients treated on the day of admission, and an 11.2% increase in the average Charlson score.

5.4. Controlling for contemporaneous NHS policy changes

One concern with our DiD identification strategy is that the resulting estimates may be biased by other policies, implemented concurrently with the ISTC programme, that had a differential effect on our outcome variables across treated and control groups. The most prominent such policy is the 2006 introduction of hospital competition within the NHS via patient choice of hospital for elective surgery. In addition, during this era much NHS policy was dictated by ten regional Strategic Health Authorities (SHAs) – differences in SHA policies implemented during the ISTC period may bias our results, to the extent that ISTC entry was differentiated across regions of England. We investigate these possibilities in Table 9; all reported estimates use as their baseline Eq. (2).

Columns (1) to (3) test whether the estimates reported in Tables 5 and 6 are robust to controlling for the 2006 patient choice reforms. We control for overall competition intensity by including a measure of market concentration (a time-invariant negative log of hospital HHI) interacted with a post-ISTC-entry dummy variable.[18] If the patient choice reforms were driving the results, inclusion of this interaction term would severely attenuate our estimates. However, including this interaction term does not materially change the estimates – they remain precisely estimated and similar in magnitude.

Columns (4) to (6) test for other region-specific policy changes that could be driving our results. To do so, we interact dummies for each of the ten English SHAs with a post-ISTC-entry dummy. These additional controls do not materially impact our results. The results are also robust to controlling more flexibly for differential SHA policies and regional trends via separate SHA × year or SHA × year × month interaction terms (see Appendix G). Overall, the estimates reported in Table 9 provide assurance that our main estimates are not driven by the most worrisome potential sources of bias from contemporaneous policy changes.

5.5. Altering the threshold used to define treatment exposure

So far, we have defined the High Treatment group to include any public hospital that had an ISTC enter within the 25th percentile of patient distance travelled between 2002/3 and 2004/5, or, in the alternative specification reported in Table 8, within 5 km. Figs. 5 and 6 show how the estimates of Eq. (2) change as we vary the treatment group definition thresholds. Fig. 5 (Panel A for log of pre-surgery LOS and Panel B for log of Charlson score) shows how the estimates change when the threshold used to define the treatment group changes from one that captures 15% of a hospital's hip and knee replacement patients, to one that captures 95%. Panels A and B illustrate that the estimated treatment effects decrease as the treated group is defined more widely.

Table 7
Event study analysis of the impact of ISTC entry on length of stay and case mix.

	(1) Log of pre-surgery LOS	(2) % treated on day of admission	(3) Log of Charlson score
High treatment × 2005/6	−0.00843	0.0114	0.0127
	(0.0405)	(0.0566)	(0.0284)
High treatment × 2006/7	−0.0904	0.127	0.0647
	(0.0776)	(0.107)	(0.0426)
High treatment × 2007/8	−0.149*	0.205*	0.0868
	(0.0798)	(0.111)	(0.0597)
High treatment × 2008/9	−0.171**	0.236**	0.105**
	(0.0661)	(0.0921)	(0.0520)
Low treatment × 2005/6	−0.0129	0.0214	−0.0108
	(0.0203)	(0.0291)	(0.0149)
Low treatment × 2006/7	−0.0369	0.0552	−0.0103
	(0.0358)	(0.0510)	(0.0204)
Low treatment × 2007/8	−0.0658	0.0961	0.0248
	(0.0444)	(0.0633)	(0.0228)
Low treatment × 2008/9	−0.0554	0.0791	0.00440
	(0.0450)	(0.0642)	(0.0240)
Hospital fixed effects	Yes	Yes	Yes
Month × year fixed effects	Yes	Yes	Yes
Patient controls	No	No	No
Observations	355,551	355,551	355,551
R-squared	0.353	0.398	0.030
Mean dependent variable	0.846	0.228	0.885

Notes: Sample includes hip and knee replacement patients. Table reports treatment effects in the 4 years after commencement of the ISTC programme, with 2004/5 used as the base year. Treatment groups, mean dependent variable, standard errors and statistical significance are defined as in the Table 5 notes. The reported treatment effects are not cumulative (e.g. the coefficient on High Treatment × 2005/6 is not turned on for observations in years after 2005/6). The coefficients on High Treatment × 2008/9 and Low Treatment × 2008/9 estimate the same effect as our main results reported in Tables 5 and 6, but differ slightly because the estimated hospital fixed effects are different due to the inclusion of additional years of data. All regressions include a dummy variable indicating hip replacement.

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[18] 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 hip and knee replacement patient from 2002/3 to 2004/5 (i.e. prior to ISTC entry). We capture competition intensity using the average pre-reform level of market structure, rather than a time-varying measure, because measures of market concentration after the introduction of choice and competition will be an endogenous function of the responses of market participants to the policies whose effects we wish to control for. We show in Table G.1 that our results are robust to using a time-varying measure of market concentration.

Table 8
Impact of ISTC entry on length of stay and case mix (treatment defined using straight-line distances between public hospitals and ISTCs).

	(1)	(2)	(3)
	Log of pre-surgery LOS	% treated on day of admission	Log of Charlson score
High treatment × post	-0.159^{**}	0.219^{**}	0.106^{**}
	(0.0676)	(0.0942)	(0.0534)
Low treatment × post	−0.0180	0.0263	0.00169
	(0.0437)	(0.0621)	(0.0256)
Hospital fixed effects	Yes	Yes	Yes
Month × year fixed effects	Yes	Yes	Yes
Patient controls	No	No	No
Observations	141,453	141,453	141,453
R-squared	0.396	0.446	0.036
Mean dependent variable	0.846	0.228	0.885

Notes: Sample includes hip and knee replacement patients. The High and Low Treatment groups are defined using straight-line distances from public hospital to ISTC. The High Treatment group consists of NHS Hospitals that had a Wave 1 ISTC enter within the circle defined by a 5 km radius. The Low Treatment group consists of NHS hospitals that had a Wave 1 ISTC enter within the circle defined by a 30 km radius, but not within the circle defined by a 5 km radius. The Control group consists of NHS hospitals that did not have an ISTC enter within the circle defined by a 30 km radius. Pre-reform and post-reform period, mean dependent variable, standard errors and statistical significance are defined as in the Table 5 notes. All regressions include a dummy variable indicating hip replacement.

Fig. 6 performs the same exercise when defining treatment exposure using fixed distances from NHS hospital to ISTC, as in Table 8. Panel A shows that ISTC entry within 5 km of an NHS hospital leads to a precisely estimated reduction in log of pre-surgery LOS at incumbents. As the threshold used to define the treatment group increases, the estimated treatment effects lose precision and asymptote to zero. Panel B, for log of Charlson score, shows a similar sensitivity to the threshold used to define treatment exposure. ISTC entry within 5 km of an NHS hospital leads to a large and statistically significant increase in the average Charlson score at incumbents, with estimated treatment effects decreasing as the distance to ISTC used to define the treatment group increases.

Overall, Figs. 5 and 6 demonstrate that our estimated treatment effects are robust to the exact threshold used to define treatment exposure. Furthermore, as the treatment group is expanded, there is a negative gradient to our estimates, which is broadly supportive of the argument that our estimated treatment effects are driven by ISTC exposure.

5.6. Additional tests of robustness

Table 10 reports additional robustness tests for our main estimates of the impact of ISTC entry on log of pre-surgery LOS, percentage of patients treated on day of admission, and log of Charlson score at High Treatment group hospitals. We estimate Eq. (2) unless otherwise noted.

Panel A formally tests for parallel pre-reform trends for the High Treatment group relative to the Control group using a ‘placebo’ DiD regression where 2002/3 is the base year and 2004/5 (the last year before ISTC entry) is the treated year. This regression allows us to explore whether there was a statistically significant difference in the change in key outcomes in our High Treatment group relative to the change in key outcomes for the Control group during our pre-period from 2002/3 to 2004/5. If there were statistically significant differences (i.e. a ‘placebo’ treatment effect), it would illustrate that there was a difference in trends over the pre-reform period that would potentially invalidate our identification strategy. Consistent with the graphical evidence in Fig. 4, none of the 2002/3 to 2004/5 point estimates are statistically significant at conventional levels, confirming that there were no statistically significance differences in trends for key outcomes between our control group and high treatment group from 2002/3 to 2004/5 (our pre-period). Though not reported, we also find no statistically significant treatment effects for the Low Treatment group relative to the Control group over the same period.

To address any residual concerns about comparability of treated and untreated hospitals, Panel B reports inverse propensity score weighted estimates where we weight our treatment and control groups by the inverse of the probability that an observation belongs in its group. To do this, we first calculate the probability of a hospital being assigned to the High and Low Treatment groups based on hospital and average patient characteristics.[19] We then run separate regressions to estimate treatment effects for the High and Low Treatment groups, using as probability weights the inverse of the probability of assignment to the group that the observation was actually assigned to. While it is impossible to reject a null hypothesis of no significant differences between treated and control groups with respect to the above determinants of treatment assignment even before these weights are applied, weighting by the inverse propensity score increases the similarity of the treated and control groups further still, by giving higher weight to control observations that look more like treated observations, and vice versa. The resulting estimates are similar to the headline findings, but more precise. The corresponding (unreported) estimates for the Low Treatment group are not statistically significant.

Our main specification accounts for the fact that different ISTCs commenced operations at different times by using a long differences estimation strategy with 2004/5 as the pre-reform year and 2008/9 as the post-reform year. An alternative approach is to define $t = 0$ (the treatment start date) for each public hospital as the contract start month (or month of ‘full service commencement’) of the nearest ISTC, and to use pre- and post-reform periods defined relative to $t = 0$ rather than using calendar time. We do not use this as our main specification because the contract start date is not always an accurate indicator of when an ISTC started treating patients. Nonetheless, Panel C presents estimates from such a specification, with months −12 to −1 before ISTC entry designated as the pre-reform period, and months 24 to 35 after entry designated as the post-reform period. Hospitals in the Control group are allocated a placebo ‘treatment’ start date equal to the contract start date of the nearest ISTC, even though this ISTC lies outside the 95th percentile of patient distance travelled. The resulting estimates are very similar to our main results.

Panel D 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 potentially endogenous to hospital performance. While we address this concern by basing treatment assignment 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, this check assigns 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 NHS hospital for that GP surgery's hip and knee replacement patients). If an ISTC is in 95% 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% of the GP surgery markets that an NHS hospital falls within, but not 95%, that NHS hospital is assigned to the Low Treatment group. All other NHS hospitals are assigned to the Control group. The estimates reported in Panel D are consistent with our main results, providing assurance that they are not driven by assignment of treatments based on hospital-centred market definitions.

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[19] Hospital-level characteristics used to predict treatment assignment include local hospital waiting times, hip and knee replacement case load, and dummy variables for – located in London, teaching hospital, standard acute hospital, university hospital. Average hip and knee replacement patient characteristics used include mortality rates, total LOS, pre-surgery LOS, post-surgery LOS, Charlson score, IMD income deprivation score, IMD health and disability deprivation score, and IMD overall deprivation ranking.

Table 9
Robustness: impact of ISTC entry on length of stay and case mix – controlling for contemporaneous policy changes.

	Controlling for introduction of choice and competition			Controlling for changes in strategic health authority (SHA) policy & differential regional trends		
	(1)	(2)	(3)	(4)	(5)	(6)
	Log of pre-surgery LOS	% treated on day of admission	Log of Charlson score	Log of pre-surgery LOS	% treated on day of admission	Log of Charlson score
High treatment × post	-0.175^{***}	0.243^{***}	0.112^{**}	-0.132^{**}	0.180^{**}	0.116^{**}
	(0.0663)	(0.0920)	(0.0518)	(0.0618)	(0.0859)	(0.0504)
Low treatment × post	−0.0589	0.0856	0.0184	−0.0261	0.0353	0.0299
	(0.0548)	(0.0784)	(0.0251)	(0.0473)	(0.0680)	(0.0244)
NegLogHHI × post	Yes	Yes	Yes	No	No	No
SHA × post-dummies	No	No	No	Yes	Yes	Yes
Hospital fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Patient controls	No	No	No	No	No	No
Observations	141,661	141,661	141,661	141,661	141,661	141,661
R-squared	0.394	0.445	0.035	0.400	0.453	0.036
Mean dependent variable	0.846	0.228	0.885	0.846	0.228	0.885

Notes: Sample includes hip and knee replacement patients. Columns (1) to (3) control for the 2006 introduction of hospital competition by including a control for overall competition intensity (Negative Log of HHI × Dummy variable indicating period after introduction of competition). Columns (4) to (6) control for changes in policy at the Strategic Health Authority (SHA) level, and for differential regional trends generally, by including controls (the coefficients on which are not included) for SHA (region of England) × Dummy variable indicating period after introduction of ISTC programme. Treatment groups, pre-reform and post-reform period, mean dependent variable, standard errors and statistical significance are defined as in the Table 5 notes. All regressions include a dummy variable indicating hip replacement. Table G.1 provides alternative specifications of these robustness tests.

Panel E reports estimates when we do not take logs of the outcome variables (pre-surgery LOS and Charlson score). We continue to find that ISTC entry made nearby public hospitals more efficient, but also left them with sicker patients; the estimates are nearly equal to the exponent of our main results. A number of other checks are reported in the online Supplementary Material (see Appendices E through G); they provide further confirmation that our results are robust to a wide range of specifications.

5.7. Ruling out the possible confounding effect of changes in patient volumes

Increases in local capacity could potentially affect incumbents' efficiency by reducing congestion and overcrowding, independent 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 investigated the impact of ISTC exposure on case load using similar regressions to those used for our main estimates, but found no statistically significant evidence of case load reductions in our High Treatment group, which had the biggest reductions in pre-surgery LOS (results available in Table H.1 in the online Supplementary Material). This suggests that the increases in local capacity brought about by private surgical centre entry did not lead to any reduction in the volume of patients treated at High Treatment group hospitals, but, instead, served to take people off waiting lists and reduce waiting times. That is, ISTC exposure led to shorter pre-surgery LOS in close-neighbouring hospitals without any concomitant reduction in the volume of patients being treated.

We did find, 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, may have reduced patient volume at public hospitals with which they shared a market – although, crucially, 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. These findings are broadly supportive of our conjecture that the reductions in pre-surgery LOS reported in Table 5 arose primarily through competitive incentives.[20]

6. Discussion and conclusions

This paper examines the effect of a UK government programme designed to increase capacity and competition by facilitating the entry of private, for-profit specialty surgical centres into the English NHS. We test the impact that the entry of these facilities – ISTCs – had on incumbent public hospitals' efficiency, case mix, and case load. 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 examined. Indeed, we demonstrate that trends of key outcome variables – including pre-surgery LOS, post-surgery LOS, and patient case mix – 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 private, for-profit surgical centre enter close by 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 led to a decrease in pre-surgery LOS of around 16% – or a 24 percentage point increase in the proportion of patients treated on the day of

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[20] Increases in local capacity could potentially affect the efficiency of incumbents even in the absence of case load reductions at these incumbents. For example, ISTC entry may have reduced pressure on waiting lists at nearby public hospitals, thus making it easier for incumbents to meet the national waiting time targets. If these incumbents had previously been avoiding target breaches by admitting patients inappropriately early and then making them wait in hospital until a surgery slot became available, then ISTC entry could have ameliorated the need for such premature admissions. We think it is unlikely that our estimated reductions in pre-surgery LOS at ISTC-exposed hospitals could be explained by this channel, for two reasons. First, blocking a bed until a surgery slot becomes available would be a very costly way of avoiding waiting time target breaches. Secondly, there is a substantial literature on NHS waiting time targets, which describes a wide range of gaming activities undertaken by hospitals in order to meet waiting time targets, but never (to the best of our knowledge) refers to hospitals admitting patients prematurely, and making them wait for a surgery slot to become available, in order to ensure that they did not breach the maximum permitted waiting time (National Audit Office, 2001; Audit Commission, 2003; Bevan and Hood, 2006a; Besley et al., 2009; Propper et al., 2010; EWHC 2787 (QB), 2005).

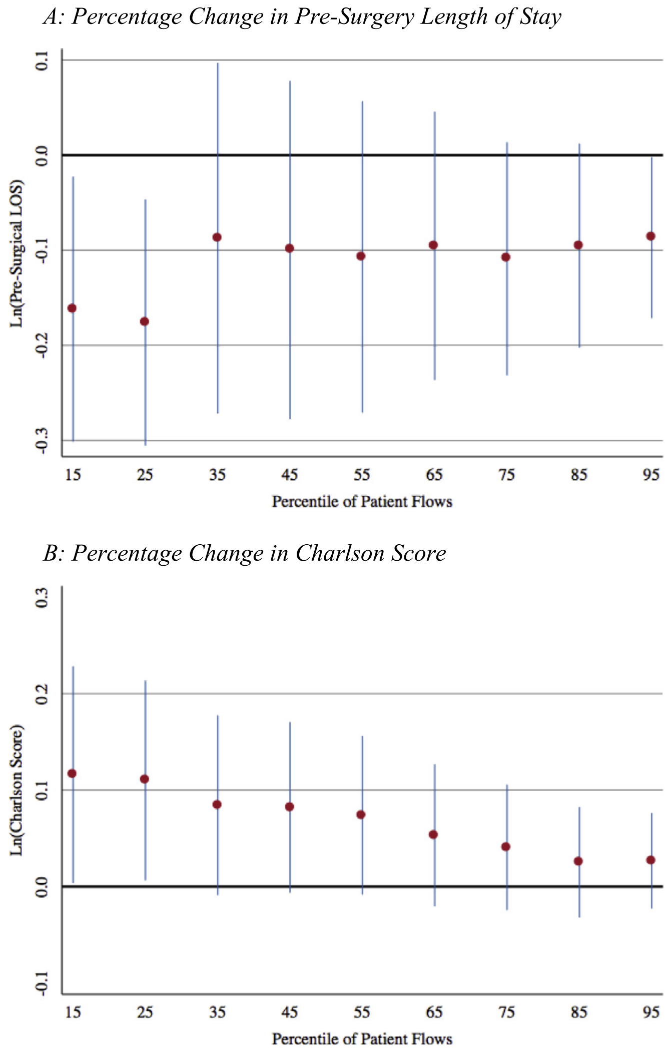

Fig. 5. Treatment effects with alternative treatment group definitions (treatment defined using patient flows). Notes: Figures plot treatment effects (coefficients and 95% confidence intervals) as the cutoff distance from home to hospital used to define the treatment group (measured in percentiles of distance travelled) changes. For example, a percentile cutoff of 25 implies that the treatment group consists of all public hospitals that had an ISTC enter within the circular area around the hospital defined by a radius equal to the 25th percentile of patient distance travelled, defined using patient flows from 2002/3 to 2004/5. In all cases, the comparator (control) group consists of all public hospitals that did not have an ISTC enter within the 95th percentile of patient distance travelled. Patient's home address is proxied by centroid of residential LSOA. Standard errors are clustered around hospitals.

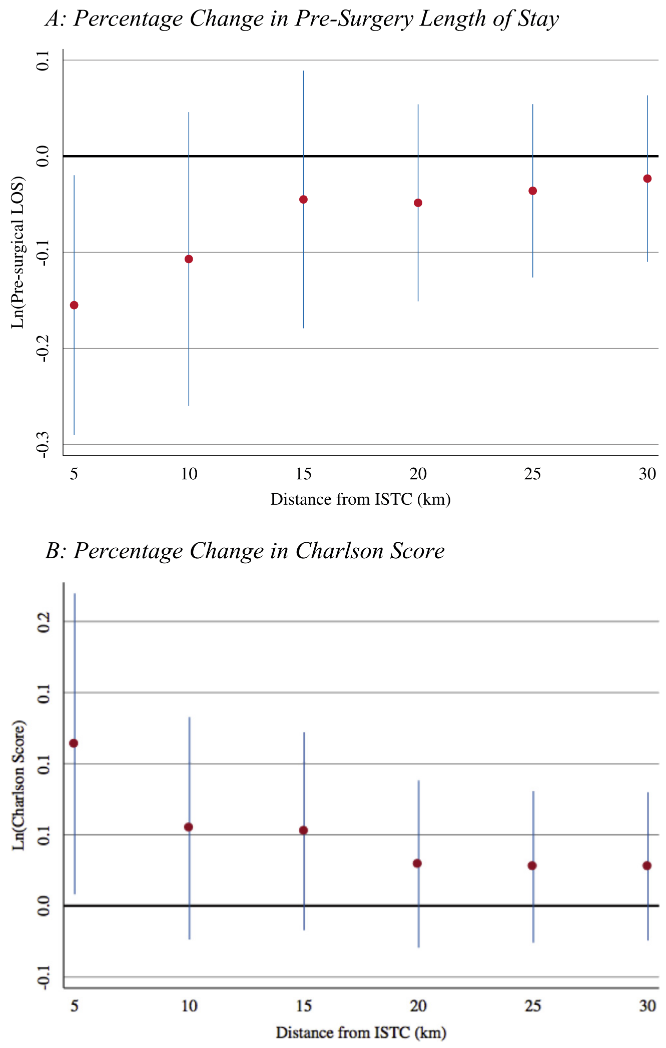

Fig. 6. Treatment effects with alternative treatment group definitions (treatment defined using straight-line distances). Notes: Figures plot treatment effects (coefficients and 95% confidence intervals) as the cutoff distance from home to hospital used to define the treatment group (measured in kilometre distances) changes. For example, a distance of 10 implies that the treatment group consists of all public hospitals that had an ISTC enter within the circular area around the hospital defined by a 10 km radius. In all cases, the comparator (control) group consists of all public hospitals that did not have an ISTC enter within a 30 km radius. Patient's home address is proxied by centroid of residential LSOA. Standard errors are clustered around hospitals.

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 competition from new private entrants became more efficient.

As well as investigating possible positive effects of ISTC entry on the efficiency of incumbent public hospitals, we looked for evidence of possible negative effects in the form of worsened case mix. We find that ISTC entry led nearby public hospitals to experience an 11.6% increase in patients' average illness severity as captured by the Charlson score – or a 6.2 percentage point increase in the proportion of patients with a Charlson score of three or more. We also find suggestive evidence that this increase in the sickness of incumbent hospitals' patients led to an increase in post-surgery LOS. While our identification strategy is unable to pinpoint how much of this increase in patient complexity at incumbent NHS hospitals is due to ISTCs actively applying their exclusion criteria – as opposed, for example, to differential choice behaviour by sick and healthy patients – the fact remains that surgical centre entry appears to have led to worsened case mix at nearby incumbents, irrespective of the exact channel by which this effect arose.

In principle, this sorting of patients between private surgical centres and public hospitals could represent an efficiency-improving division of responsibility between routine and more complex cases. Indeed, this appears to have been the government's rationale for including wide-ranging exclusion criteria in ISTC contracts, which would allow these private entrants to focus on routine cases. Thus, the risk-selection we document was to some extent an intended policy outcome. However, the fact that policymakers explicitly intended such a division of responsibility between private entrants and public incumbents does not automatically imply that the division was devoid of negative consequences. To ensure that a division of responsibility between complex and straightforward cases does not have a negative effect on the financial position of providers that receive the most

Table 10
Additional tests of robustness.

	(1) Log of pre-surgery LOS	(2) % treated on day of admission	(3) Log of Charlson score
A. Test for parallel trends during pre-reform period (2002/3 to 2004/5)			
High treatment × 2004/5 dummy	0.0322	−0.0436	0.00659
	(0.0265)	(0.0387)	(0.0261)
B. Estimation using inverse propensity score weights			
High treatment × post	-0.167^{***}	0.232^{***}	0.109^{***}
	(0.0509)	(0.0740)	(0.0363)
C. Contract start date as $t = 0$			
High treatment × post	-0.175^{**}	0.245^{**}	0.123^{**}
	(0.0741)	(0.103)	(0.0543)
D. GP-centred treatment assignment			
High treatment × post	-0.186^{***}	0.259^{***}	0.125^{**}
	(0.0657)	(0.0903)	(0.0526)
E. Outcomes in levels, not logs			
High treatment × post	-0.264^{***}	–	0.309^{**}
	(0.100)		(0.147)

Notes: Sample includes hip and knee replacement patients. The table reports robustness tests of the main estimates for the High Treatment group based on the 'headline' regression specification (Eq. (2)), with hospital fixed effects and a full set of month-year dummies. With the exception of Panels A and C, this table uses 2004/5 as the pre-reform period and 2008/9 as the post-reform period. With the exception of Panel D, treatment groups are defined as in the Table 5 notes. Standard errors and statistical significance are defined as in the Table 5 notes. Panel A is a placebo regression with 2002/3 as the pre-reform year and 2004/5 as the post-reform year. Panel B weights observations by the inverse of the probability of assignment to the treatment or control group that they were actually assigned to. Panel C defines the post-reform period as the period after the contract start date of the nearest ISTC and defines time relative to this date instead of using calendar time; months −12 to −1 are designated as the pre-reform period, while months 24 to 35 are designated as the post-reform period. Panel D assigns treatments by centring hospital markets on GP surgeries rather than hospitals themselves. Panel E runs the regression on levels of the outcome variable rather than logs. All regressions include a dummy variable indicating hip replacement.

complex patients, hospitals that treat sicker patients must be appropriately compensated.[21]

Unfortunately, NHS reimbursement rates during the period we study did not adequately adjust for patient severity (Mason et al., 2008). 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, Mason et al. (2008) state that providers were still likely underpaid for treating sicker patients, and note that is unlikely that a prospective reimbursement system can ever be designed to fully compensate hospitals for a more costly case mix.

Our work highlights one trade-off that arises from the entry of for-profit surgical centres. We show that entry can stimulate improvements in the performance of incumbents, but that entrants may engage in risk selection at the expense of incumbents. 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 cream skimming by private entrants.

Our work is part of the broader task of evaluating the overall social welfare implications of the entry of private firms into the market for delivery of public services. Such an overall evaluation would not only consider the impacts of private provider entry on the performance of public incumbents, but would also compare the performance of private and public providers, as well as taking into account any increases in overall capacity resulting from private surgical centre entry.

An interesting thought experiment is to consider whether our selection results were a function of the for-profit status of entrants, or of the fact that entrants were specialist surgical centres who competed against full service hospitals. Ultimately, theory and the broader evidence suggest that both distinctions – competition from for-profit ventures and competition from specialist surgical centres – likely drove our selection results (Barro et al., 2006; Dafny, 2005; Cram et al., 2005; Street et al., 2010).

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. Absent suitably risk-adjusted payments, new entrants may take steps to attract patients that are less costly to treat, leaving incumbents with a riskier mix of patients.

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Supplementary material

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[21] When certain types of care are removed from public facilities and shifted to the private sector, it can also have negative dynamic effects through missed learning opportunities for public sector clinicians and care professionals. Although not part of our formal analysis, personal conversations with clinicians indicate that an additional negative effect of ISTC entry was that it reduced the number of straightforward cases at nearby NHS teaching hospitals, which are essential for the training of surgical registrars.

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