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DEATH BY MARKET POWER:
REFORM, COMPETITION AND PATIENT OUTCOMES IN THE NATIONAL HEALTH SERVICE

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ABSTRACT

The effect of competition on the quality of health care remains a contested issue. Most empirical estimates rely on inference from non experimental data. In contrast, this paper exploits a pro-competitive policy reform to provide estimates of the impact of competition on hospital outcomes. The English government introduced a policy in 2006 to promote competition between hospitals. Patients were given choice of location for hospital care and provided information on the quality and timeliness of care. Prices, previously negotiated between buyer and seller, were set centrally under a DRG type system. Using this policy to implement a difference-in-differences research design we estimate the impact of the introduction of competition on not only clinical outcomes but also productivity and expenditure. Our data set is large, containing information on approximately 68,000 discharges per year per hospital from 162 hospitals. We find that the effect of competition is to save lives without raising costs. Patients discharged from hospitals located in markets where competition was more feasible were less likely to die, had shorter length of stay and were treated at the same cost.

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I. Introduction

Health care is one of the most important industries in developed countries, both because of its size and impact on well-being.[1] Historically, health care has been provided through centralized, non-market means in most countries outside of the United States. However, recently market oriented reforms have been adopted or are being considered in many countries, including the UK, Netherlands, Belgium, Israel, and Australia, despite a lack of strong evidence on the effects of market reforms in health care. In the US markets have long been used for the delivery of health care. However, massive consolidation among hospitals has led to concerns about the functioning of these markets.[2] These developments raise questions as to whether pro-market reforms are an appropriate way of improving outcomes in health care.

A central concern about the functioning of markets in health care is whether competition will deliver the socially optimal quality of care (Sage et al. 2003; Federal Trade Commission and U.S. Department of Justice 2004). Quality is a major issue in health care, first because the effect of quality on an individual's well-being can be very great and second, due to the widespread presence of insurance against health care expenditures (the US), or provision of care free of charge (UK, Europe), health care consumers are not exposed to the full expense associated with their health care decisions so that quality looms larger in consumer choice than price.

In the last few years a relatively small, but important, literature has emerged on competition and quality in health care markets (see Gaynor 2006, for a review). The results are mixed. Economic theory suggests that competition will increase quality in markets with regulated prices (provided price is above marginal cost). The models largely derive from analyses of industries subject to price regulation up until the 1970s and 1980s, e.g., airlines and taxis, but there are also some models specific to

[1] Health care accounts for 16% of GDP in the US, 8.4% in the UK, and 9.5% on average across Western Europe and Canada.

[2] For example, Haas-Wilson (2003), Sage et al. (2003), Federal Trade Commission and U.S. Department of Justice (2004), Cuellar and Gertler (2005), Vogt and Town (2006). Critics of the use of competition include Schlesinger (2006), Rosenbaum (2006), Jost et al. (2006).

health care.[3] The intuition of all these models is as follows. Price is regulated, so firms compete for consumers on non-price dimensions, i.e. "quality." If the regulated price is set above marginal cost at some baseline level of quality, then firms will increase quality to try to gain market share. This will continue until profits are zero.

The empirical evidence is almost entirely from the US Medicare program, and mostly from treatment of heart attack patients. Most of this evidence (discussed later in this section) finds that competition with fixed prices improves quality.[4] However, with one exception (Cooper et al. 2010), the existing studies exploit changes in cross sectional variation in levels of market structure over time to identify the impact of competition.

In this paper we exploit a policy change in the UK National Health Service (NHS) to identify the effect of competition on health care quality. Our identification comes both from variation in market structure, as in previous studies, but also from a large policy change designed to promote competition. In 2006 the NHS adopted a payment system in which hospitals were paid fixed, regulated, prices for treating patients (similar to the Medicare hospital payment system in the US) and mandated that all patients requiring treatment be given the choice of five different hospitals. Prior to this reform, the local public agencies responsible for purchasing health care on behalf of the population in their area engaged in selective contracting with hospitals, bargaining over price and quantity. The reform therefore provided patients with more choice, both via the mandated five alternatives and the end of selective contracting, and moved hospitals from a market determined price environment to a regulated price environment. Greater choice should increase the (quality) elasticity of demand faced by hospitals, which should intensify competition. In addition, the shift from selective contracting to fixed prices should focus competition on quality.

[3] See for example, Douglas and Miller (1974), Schmalensee (1977), Vander Weide and Zalkind (1981) and White (1972) on airlines, and Frankena and Pautler (1984) on taxicabs. On health care, see for example, Allen and Gertler (1991), Held and Pauly (1983) and Pope (1989).

[4] When prices are market determined, however, there is no general theoretical prediction of the effect of competition on quality. The empirical literature mirrors that indeterminacy. There are studies that find that competition decreases quality (e.g., Propper et al. 2004), and those that find the opposite effect (e.g., Sari 2002). All of these studies use variation in market structure for identification.

We use the introduction of the policy in 2006, interacted with market structure, to identify the impact of market concentration on quality. Figures 1a and 1b illustrate our approach. They show the unconditional relationship between hospital quality (as measured by mortality) and market structure (measured by the Herfindahl-Hirschman concentration index, HHI) before and after the reform.[5] Figure 1a presents a commonly used measure of hospital quality – the mortality rates for acute myocardial infarction (AMI, commonly known as a heart attack).[6] The red lines show the smoothed nonparametric relationship between mortality and market structure. The left hand panel shows that mortality is clearly higher in less concentrated markets before the reform. But post-reform the direction of the association has been reversed and mortality is lower in unconcentrated markets. The same pattern is observed in Figure 1b, which plots all-cause hospital mortality rates against the HHI.

These figures strongly suggest that the reform intensified competition and that intensified competition post-reform led to increased quality. The paper subjects this hypothesis to rigorous testing and examines, in contrast with most of the prior literature, not only deaths from AMI as the measure of quality but a range of other quality measures and at other outcomes, including hospital utilization and expenditure. We find that the introduction of competition led to an increase in quality without a commensurate increase in expenditure.

Our research contributes to the empirical literature on competition and quality in health care. The most prominent study of markets with fixed prices is Kessler and McClellan (2000), who examine the impact of market concentration on mortality for Medicare heart attack patients. They find that mortality is substantially and significantly higher for patients in more concentrated markets. Kessler and Geppert (2005) find that high risk Medicare patients' heart attack mortality is higher in highly concentrated markets, while there is no such effect for low risk patients. Tay (2003) estimates a model of hospital choice for Medicare patients and finds that demand is responsive to quality, again measured by heart attack mortality, implying the potential for quality competition. Shen (2003) finds that the number of hospitals interacted with the Medicare payment leads to reduced Medicare patient heart attack mortality after

[5] These estimates are adjusted for differences in severity between hospital populations.
[6] We discuss the use of mortality as an indicator of quality in section IV.A.

1990. In the one paper from outside the US, Cooper et al. (2010) examine the effect of the current pro-market reforms in the UK on heart attack mortality and find the reforms reduced AMI mortality in less concentrated markets relative to more concentrated markets. In contrast, Gowrisankaran and Town (2003), using similar methods to Kessler and McClellan (2000), find that mortality is higher for Medicare heart attack and pneumonia patients receiving care in less concentrated markets in the Los Angeles area and Mukamel et al. (2001) find no effect of market concentration on mortality from all causes for Medicare patients.

Our research also contributes to the growing evidence on the impact of competition in public services. There has been a great deal of interest in recent years in competition in education, both theoretically and empirically (e.g., Epple and Romano 1998; Hoxby 2000; Epple, Figlio, and Romano 2004). In this literature, as in health, the predictions from theoretical models are often ambiguous and the empirical evidence quite contested (e.g., Hoxby 2000; Rothstein 2007; Bayer and McMillan 2005; for a review see Burgess, Propper and Wilson 2005). Our results thus add to the evidence on the conditions under which gains from competition in the provision of public services may be realized.

In what follows, we provide background on the NHS and the market oriented reforms of 2006 (Section II), present our empirical strategy (Section III), describe the data (Section IV), present our results and report on an extensive set of robustness tests (Section V), and provide a summary and conclusions (Section VI).

II. The reform program

A. The NHS reforms

In the UK health care is tax financed and free at the point of use. All primary care and almost all hospital care is funded and provided in the National Health Service (NHS). Primary care is provided in the community by publicly funded physicians known as

General Practitioners (GPs), who also act as the "gate keeper" for hospital based care. Secondary care is provided in publicly funded public (NHS) hospitals.[7]

Prior to 1991 funding was allocated to public bodies at the local level (local health authorities), who were responsible for running hospitals. From 1991 the roles of buyer and seller of hospital based health care were separated, with the intention of promoting competition between public hospitals. The local health authorities were given the task of buying hospital based health care for their population.[8] Hospitals were turned into free standing public organisations, known as NHS Trusts, who competed for contracts from the buyers.[9] Both price and quality were negotiable, though information on quality was extremely limited (Propper et al. 2008). In 1997 the newly elected Labour administration retained the architecture of the buyer and seller split but changed policy to reduce competition and to implement instead longer term cooperative relationships between buyers and sellers. In this regime, which was similar to selective contracting in the US,[10] buyers and sellers negotiated over price, quality (mainly waiting times), and volume on an annual basis, with the majority of contracts taking the form of annual bulk-purchasing contracts.

In late 2002 the government signalled a shift in policy and initiated a reform package with a set of phased-in changes leading to the re-introduction of competition from 2006 onwards.[11] There were several elements to this policy (Farrar et al. 2007; Cooper et al. 2010), the most important of which were a policy designed to increase

[7] There is a small private sector. Around 15 percent of the population have private health insurance which is a complement to, rather than a substitute for NHS care. Private medical insurance is not tax deductible and covers care in the small private sector, which specializes in the hospital based provision of non-acute services for which there are long NHS waiting lists. There are also some private sector providers of NHS care (known as Independent Sector Treatment Centres) but they account for less than 1% of all hospital activity.

[8] These buyers were known as District Health Authorities (DHAs). Health authorities covered an administratively defined geographical area containing around 100,000 patients.

[9] Although purchasers were given the right to buy from whichever supplier of health care they wished, in practice, almost all care purchased by NHS purchasers was bought from NHS Trusts. Relatively little business went to the very small private sector.

[10] Health insurance plans in the US typically form provider networks for their enrollees. Enrollees are covered for care obtained from providers in the network, and either have no coverage outside of the network or have to pay substantially more. Insurers selectively contract with hospitals and doctors to be in their network. This includes negotiation over prices (and possibly some other factors), with the understanding that the provider will receive a substantial volume of patients from the insurer.

[11] The NHS uses financial years which run April 1st to 31st March. In the description of the reforms we give precise dates of policy announcements. In the rest of the paper we refer to full years by the first calendar year which the financial year spans.

patient choice and a change in hospital payments from negotiated to ex ante fixed prices.

Patient choice was introduced in January 2006. Prior to this date, patients were referred by their GPs to the local hospital that provided the service they required and were not offered generally offered any choice over the location of their care.[12] Post January 2006 patients had to be offered a choice of five providers for their hospital care (Department of Health 2004) and GPs were required (and paid) to ensure that patients were made aware of, and offered, choice.[13] Along with giving patients a formal choice of where they could receive secondary care, the government also introduced a new information system that enabled paperless referrals and appointment bookings and provided information on quality to help patients make more informed choices. This system, known as 'Choose and Book', allows patients to book hospital appointments online, with their GP, or by telephone. The booking interface gives the person booking the appointment the ability to search for hospitals based on geographic distance and see estimates of each hospital's waiting time.[14] Patient choice therefore signalled the end of selective contracting by encouraging movement of patients away from the local hospitals GPs had previously used.

The ex ante fixed prices are a case-based payment system known as 'Payment by Results' (PbR) (Department of Health 2002b). PbR is modelled on the diagnosis-related group (DRG) payment system used by the Medicare program and many private insurers in the US (Department of Health 2002a). A fixed price is set by the government for every procedure, with adjustments for whether a hospital was an academic centre, patient severity and local wage rates (Department of Health

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[12] The hospitals to which patients were sent were determined by the selective contracting arrangements made by the local NHS body responsible for purchasing healthcare (the Primary Care Trust, PCT).

[13] In April 2008, choice was extended so that patients could choose any hospital in England, as long as the hospital met NHS standards and were paid using the NHS ex ante fixed tariff (Department of Health 2007; Department of Health 2009).

[14] From 2007 the government also introduced a website designed to provide additional quality information to inform patients' choices. This included information collected by the national hospital accreditation bodies, including risk-adjusted mortality rates and detailed information on waiting times, infection rates and hospital activity rates for particular procedures as well as information on hospital accessibility, general visiting hours and parking arrangements (Choose and Book website 2010, http://www.chooseandbook.nhs.uk/patients).

2002a).[15] The price is exogenous to both seller and the buyer. The aims were that hospitals would only receive payment if they attracted patients (Le Grand 2007; Dixon 2004) and that fixed prices would mean that choice would depend on quality, and not price as in the previous system (Department of Health 2003).

In 2003 PbR was used for a very limited number of procedures (15 elective procedures) and only for purchases from a small group of hospitals (known as Foundation Trusts, FTs). In the following year it was extended to a wider set of elective spells and non elective spells in FTs. In 2005 it was also applied to elective care (which accounts for approximately half of all hospital admissions) in non-FT NHS hospitals. In 2006 PbR was applied to almost all elective, non elective and outpatient care (Farrar et al. 2007).[16]

In addition to Choose and Book and PbR, the government sought to give additional fiscal, clinical and managerial autonomy to NHS hospitals in order to further foster a competitive environment for hospitals. From April 2004 onwards high performing NHS hospitals could apply for Foundation Trust (FT) status. This gave hospitals greater financial autonomy, allowing them to keep and reinvest surpluses across financial years. This represents considerable freedom over financial matters as non-FT NHS hospitals were required to break even on an annual basis and were heavily constrained in their access to capital. FTs also were given easier access to (primarily) private sources of capital. Hospitals earned this additional autonomy by performing well against key performance targets, the most important of which were good financial performance and the reduction in waiting times for elective care.[17]

B. *Expected hospital responses to the reforms*

[15] The payment unit is the HRG (Healthcare Resource Group), a group of diagnoses which utilize similar levels of resources and are very similar to DRGs used in the US.

[16] http://www.dh.gov.uk/en/Managingyourorganisation/Financeandplanning/NHSFinancialReforms/DH_077259 (accessed February 27, 2010).

[17] Trusts apply to become an FT. Granting of FT status is undertaken by an independent regulator, Monitor, who pay particular attention to financial performance. In 2004 only 20 hospitals out of a population of over 270 were granted FT status. By the end of 2007 there were 83 FTs out of a population of around 240 NHS hospitals, with the intention that eventually all trusts will get FT status. http://www.monitor-nhsft.gov.uk/home/about-nhs-foundation-trusts/nhs-foundation-trust-directory (accessed February 27, 2010).

The established literature (reviewed in Gaynor 2006) suggests that an increase in the elasticity of demand combined with a fixed price regime should lead to an improvement in hospital quality where hospitals face competition, and a larger increase in quality where hospitals face greater competition. The question is whether the incentives facing the market participants in the English NHS are such that the reforms have this effect. There are reasons to expect this response.

First, NHS hospitals have incentives to respond to increased competition. While NHS hospitals are public organisations, the regime they operate under gives hospital managers strong incentives not to make losses. The government monitors the performance of hospitals on an annual basis and publishes summary assessments of their performance based on a range of indicators. These include measures of quality of care, access to care and financial performance. The weight given to financial performance in the summary assessments is high. Managers of hospitals which perform poorly in terms of the summary assessments may be replaced (and this does happen), while hospitals which perform well can get the greater autonomy awarded to Foundation Trust status. Further, hospitals with FT status can retain surpluses, and non-FT hospitals that perform well have the opportunity to earn FT status.

Second, PbR is a very highly geared case based reimbursement system. In 2006 over 60% of hospital income came from PbR payments (Department of Health 2007) and was projected to rise to closer to 90% in the following years (Street and Maynard 2007). The effect of PbR is to tighten the annual budget constraints for hospitals and increase the amount of uncertainty for hospital managers. Between 1997 and 2005, the use of annual contracts meant that annual revenues were known at the beginning of the year and costs were reasonably certain. Post-PbR revenues are more uncertain, as hospitals are no longer guaranteed volume at the start of each year. Supply has also been increased, as "Choose and Book" has opened hospitals up to competition from outside their local catchment area. Hospitals obtain revenues from patient volume (as prices are fixed), and, where rivals are present, have to compete for patients based on quality. Hospitals facing more competitors should have to compete more intensively for patient volume, and vice versa.

Third, Choose and Book, by providing patients with greater choice and information, should increase the elasticity of demand facing hospitals. While increasing choice for patients might have little impact where patients have to make choices unassisted, the program is implemented by GPs. In addition to being mandatory, these physicians receive financial payments for the extra costs of implementing the system. Thus they have no reason not to offer their patients choice, other than their professional judgement. And while there is some evidence that not all primary care physicians thought that patients were able or wanted to make choices, a survey commissioned by the Department of Health found that 45% of patients recalled being offered a choice of hospital (Department of Health 2009). In addition, Dixon et al. (2010) found that the most important dimensions in patient choice of hospital were primarily measures of quality of care, such as hospital 'superbugs' (acquired infection rates) and cleanliness.

III. Empirical strategy

Our goal is to test the hypothesis that the pro-competition policy improved hospital quality. To do this we exploit the variation in market structure across hospitals and examine whether quality is higher for hospitals in less concentrated markets after the reform than before. This is a difference-in-differences (DiD) approach to estimating the effect of a policy change. The simplest DiD strategy compares two groups over two time periods, where a treatment group is exposed to a policy change in the second period and a control group is not exposed to the policy in either period. The NHS market based reforms do not fit neatly within this simple DiD framework, as the reforms apply to all hospitals in England at the same time. However, the intensity of the competition induced by the reforms will vary according to market structure, which is a function of the geographical configuration of patient location and hospital sites. In some places population density results in a market structure which permits a high degree of choice. In others, population density is low and hospitals are located in highly concentrated markets with few competitors. As a consequence, post-policy a hospital in an unconcentrated market faces more exposure to the policy change than does a hospital in a highly concentrated market.

We therefore identify the impact of competition from the interaction of a continuous treatment intensity variable (the degree of concentration) with a dummy for the post-reform year.[18] We use data from 2003 to capture the period before the policy change and data from 2007 for the period after the policy change.[19] This gives the DiD regression specification:

$$q_{it} = \beta_0 + \beta_1 I(t=2007) + \beta_2 I(t=2007) * HHI_{it} + \beta_3 HHI_{it} + \beta_4 X_{it} + \mu_i + \xi_{it} \qquad (1)$$

where q_{it} is the outcome variable, quality of care at hospital i at time t. $I(.)$ is an indicator function for the post-reform period, which takes the value 1 for financial year 2007 and 0 otherwise. HHI_{it} is the Herfindahl-Hirschman index, our measure of market structure, X_{it} is a vector of observed hospital characteristics which vary over time, μ_i is an unobserved fixed hospital effect, ξ_{jt} is random noise and t takes two values, financial year 2003 and financial year 2007. The DiD coefficient is β_2 which measures the effect of market structure post-reform. Any common macro changes are picked up by the time dummy.

Endogeneity is a common concern in estimating regression models like (1) with the HHI on the right hand side (see, e.g., Bresnahan 1989). For example, if unobservably sicker patients go to better hospitals, which are in urban areas (and hence less concentrated markets), this would result in a negative correlation between the HHI and (poor) quality as measured, for instance, by mortality rates. On the other hand, if better hospitals have higher HHIs because of their higher quality, this would result in a positive correlation between the HHI and mortality.

Our use of a short time series minimizes changes in populations or labor markets that may result in demand or supply changes. The use of a fixed effects estimator controls for the impact of any time invariant hospital-specific factors associated with quality, so that the levels of the outcomes may differ freely across hospitals in the pre-policy world. These hospital-specific factors include location, so we control for features that may be spatially associated with market concentration but are related to competition.

[18] Card (1992) was one of the first applications in economics to use a continuous treatment variable to estimate the impact of a policy. See also Angrist and Pischke (2008).

[19] The 2007 data are the most recent comprehensive data available. Aggregating the data into two periods, pre and post, is an approach recommended by Bertrand et al. (2004) to solve the problem of serial correlation in DiD applications.

In addition, we include in the X_{it} vector controls for observable time varying measures of the health of both the patients admitted to the hospital and the population in the catchment area of the hospital, as well as measures of local income to control for patient health or other effects of income on demand.

However, there may remain concerns about unobserved heterogeneity. To deal with this, we instrument our measure of market structure with a measure of market structure based on factors unrelated to quality or unobserved patient heterogeneity. Following Kessler and McClellan (2000) and Gowrisankaran and Town (2003) we predict market structure on the basis of patient and hospital characteristics (patient distance from each hospital, patient demographics, patient illness severity, and size and teaching status of hospitals) and replace the actual HHI in (1) with a predicted HHI. This predicted HHI will depend only on these patient and hospital observables (in large part, patient distances from hospitals) and thereby eliminate possible correlation with the error in the quality equation. We discuss the construction of our predicted HHI in detail in Section IV and in Appendix B.[20]

IV. Data

We have assembled a rich database with hospital-level panel information on a variety of hospital quality and access to care indicators, financial performance, patient case mix and local area conditions. This was compiled from a large number of administrative data sources that we discuss briefly here and are presented in detail in Appendix Table A1. We use data on the universe of inpatient discharges from every hospital in the NHS in England for the financial years 2003 to 2007, comprising over 13 million admissions in around 240 hospitals per year. [21] We focus here on a (large) subset of these hospitals - short term general hospitals (called acute hospitals in the UK). These are the dominant suppliers of hospital-based services.

[20] In robustness checks (Section V.C) we estimate (1) using actual market structure.

[21] Hospitals are called "trusts" in the UK. An NHS "trust" is a financial, managerial and administrative unit and may cover more than one physical hospital, all of which are located closely in geographical space as there are no hospitals chains in the NHS. We use the term "hospital" rather than "hospital trust" for expositional convenience.

Our sample selection criteria and impacts on sample size are laid out in panel (a) of Table A2 in the Appendix. The population of acute hospitals falls slightly from 180 in 2003 to 175 in 2007 (due to hospital reorganization by the government to deal with longer term changes in population density). Our first, and main, selection rule is to select hospitals with at least 5,000 total admissions. We do this to drop hospitals with low numbers of admissions as these hospitals will be specialist hospitals without emergency rooms (around 50% of patients enter hospitals via the emergency rooms). This eliminates 10 hospitals from the analysis in 2003 and 8 hospitals in 2007. Second, we drop those hospitals for which mortality data or the data necessary to calculate HHIs are not available for both years. Our final sample contains 162 hospitals for each year of the analysis, totalling 324 hospital-year observations for the main analyses. For our analysis of AMI mortality we also exclude hospitals with fewer than 150 AMI admissions to avoid the problem of variability of rates from small denominators (see e.g., Kessler and McClellan 2000). This reduces the number of hospitals to 130 in 2003 and 121 in 2007, giving 251 hospital-year observations for the emergency AMI analyses.

The fall in the number of hospitals from the pre- to the post-policy period may raise concerns as to whether we have a selected sample. To test whether sample exit is correlated with hospital market structure we examine the association between the probability of a hospital exiting our sample and, first, the market structure it faced in 2003 and, second, the change in the market structure experienced by the hospital between 2003 and 2007. Using all acute hospitals as the initial sample, Table A2, panel (b), shows that exit is associated with the level of HHI in 2003. Exit is not, however, associated with the change in the HHI. We take from this that we have a sample which contains a few less hospitals in unconcentrated markets than the population of all acute hospitals, but that the selection made necessary by reconfigurations, small numbers and missing data does not create a problem for our DiD identification strategy, which uses changes in the HHI for identification.

A. *Measures of hospital quality*

We use both mortality rates within the hospital and in all locations (i.e. including deaths post-hospital discharge). Both are derived from Hospital Episode Statistics

(HES) data, which are administrative data on every NHS health episode such as an operation or physician consultation. We construct yearly data at the hospital level for 28 day in-hospital mortality rates for all admissions and for emergency AMI for patients aged 55 or over. Deaths following emergency admission for AMI have been published by both the US and UK governments as indicators of hospital quality. This indicator is also the most widely used measure of hospital quality in the economics literature for a number of reasons.

First, AMI admissions are reasonably high volume and mortality is a fairly common outcome, so variability in the rates is less of an issue than for other treatments. Second, the infrastructure used to treat AMI is common to other hospital services, making it a good general marker of hospital quality (Gaynor 2006).[22] Third, all patients with a recognized AMI are admitted and, in the UK, patients are taken to their closest hospital, so there is little scope for selection bias to affect the decision of who gets admitted. We also examine deaths following AMI admissions in all locations within 30 days of admission (on or after discharge). These data allow us to examine whether hospitals respond to competition by discharging patients in a poorer health state.[23] As another indicator of a hospital's quality of care we use the all-cause in-hospital mortality rate, as studies have found falls in overall hospital mortality linked to clinical and managerial quality improvement programmes and to variability in hospitals' performance across a number of conditions (Wright and Shojania 2009; Jha et al. 2005).[24]

[22] Many of the actions to reduce deaths from emergency admissions for AMI need to be taken soon after an attack, and so the performance of a hospital in terms of AMI reflects the performance of its A&E department (accident and emergency department, or emergency room).

[23] These data are constructed by linking information on deaths following discharge (from the UK Office for National Statistics, ONS) to the admitting hospital (from HES data, The NHS Information Centre). They are for emergency AMI admissions of patients aged 35-74. Our measure of in-hospital AMI deaths is for patients aged 55 and over. The in-hospital AMI death rate for the 35-74 age group in our sample in 2007 is 4.6%, which is comparable to but lower (as expected given the ONS-HES rate includes deaths in all locations, on or after discharge) than the 5.8% in the ONS-HES linked data in the same year. Patients aged 75 and over account for 43% of all AMI admissions in our sample, while patients aged 35-54 account for only 14%. Patients aged 75 and over are three times more likely to die than those aged 55-74. Patients aged 35-54 are four times less likely to die than those aged 55-74. This explains the difference between the in-hospital and all-location AMI mortality rates in Table 1.

[24] We also examine other markers of hospital quality used by governments, including the English, to monitor performance. These are the MRSA bacteraemia rate (per 10,000 bed days), which is a measure of health care associated infection, and two measures of waiting times, the proportion of attendances spending less than four hours in the Emergency Room (Accident and Emergency department) and the proportion of patients in the waiting list waiting three months or more. All measures of hospital quality are positively and significantly correlated at the 1% level, except for waiting times in Accident and

Mortality is the most widely used measure of hospital quality. However, it is not clear that hospitals compete directly over mortality rates, in the sense of maximizing profits (or some other objective function). But we can think of mortality as an indicator of overall quality in the hospital. It seems unlikely that hospitals deliberately choose lower quality in the form of an increased probability of death. However, hospitals that face less competitive pressure may choose to exert less effort or supply less quality in ways that indirectly affect mortality. Patients who are at serious risk of death are the sickest and most sensitive to the quality of care. As a consequence, if the overall quality of care suffers when hospitals are not pushed hard by competitive pressure, then we would expect to see mortality rates rise.

B. *Measures of hospital market structure*

We measure market structure at the hospital level in both the pre- and post-policy years using an HHI based on patient flows to each hospital. This results in a hospital-specific time varying index. The HHI is built up from patient flows at neighborhood level and is calculated in two steps. In the first, the HHI in each geographically defined neighborhood in England is calculated as the sum of squared patient shares across all hospitals the neighborhood sends its residents to for all elective care.[25] The neighborhood definition we use (the MSOA) contains on average around 7,000 persons and so is similar, or smaller, in population, to a US zip-code.[26] We allow the market to be the whole of England (i.e. we include all hospitals used by each MSOA in the calculation of patient shares). In the second step, the HHI for each hospital is calculated as a weighted average of the HHIs for the neighborhoods it serves, where the weights are the shares of the hospital's patients that live in each neighborhood. Thus each hospital has its own market. Patient flows are from the information on

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Emergency which is negatively and significantly correlated with the other measures. The two measures of AMI deaths have a correlation of around 0.6. The correlation between AMI death rates and all causes in-hospital mortality is around 0.2. The correlations between the non mortality outcomes and the mortality indicators range from 0.3 - 0.4.

[25] Elective admissions make up around half of all hospital admissions and these are those subject to "Choose and Book".

[26] MSOAs (Middle Layer Super Output Areas) are defined to ensure maximum within MSOA homogeneity of population type. In England each of the 6,780 MSOAs has a minimum population of 5,000 residents and an average population of 7,200 residents. Data retrieved from http://neighborhood.statistics.gov.uk/dissemination/Info.do?page=userguide/moreaboutareas/furtherareas/further-areas.htm on 26/04/2010.

admissions and patients' locations in the HES dataset. In what follows, we refer to HHIs based on actual patient flows to the hospital as *actual* HHIs.

As noted above, there is concern in the industrial organization literature in general about the potential endogeneity of concentration indices when used as regressors for the purpose of making inference about competition. In the health care context, Kessler and McClellan (2000) have argued that measures of hospital competition based on actual patient flows are potentially endogenous in regressions with hospital quality as the outcome of interest, due to potential correlation with the unobserved characteristics of patients, hospitals or geographic markets. To avoid this problem, we follow Kessler and McClellan (2000) and use as our main measure of market structure hospital HHIs based on patient flows that are predicted using only exogenous patient and hospital level characteristics. To generate predicted patient flows we first estimate multinomial logit patient level hospital choice models and then derive the predicted probabilities that a given patient attends each hospital in their choice set (for details, see Appendix B). These predicted probabilities are used to calculate *predicted* HHIs for each hospital using the same method described above.

The summary statistics in Table 1 show that predicted HHIs tend to be lower than actual HHIs, i.e. markets are less concentrated when HHIs based on predicted compared to actual patient flows.[27] This suggests that patient flows are likely to be influenced by potentially endogenous factors - such as unobserved hospital quality - leading hospital markets to appear to be more concentrated than they would otherwise be. So the use of predicted HHIs based on exogenous hospital and patient characteristics means our estimates of the impact of market structure on hospital quality are less likely to suffer bias arising from endogeneity between hospital quality and actual patient flows.

C. Covariates

We employ a large set of covariates to control for heterogeneity across hospitals. To allow for differences in the health of hospitals' patient mix (often referred to as a

[27] The correlation coefficients between predicted and actual HHIs in our estimation sample are 0.73 and 0.70 for the years 2003 and 2007 respectively (both statistically significant at the 1% level).

hospital's "case mix") we include three main sets of controls. First, we control for hospital fixed effects, which will pick up observed and unobserved non-time varying differences between hospitals.[28] As our panel is short this should pick up a considerable amount of heterogeneity. Second, we control for the health of population in the hospital's catchment area by means of the all-cause time varying mortality of the neighborhood of the hospital.[29] Third, we control for the age-gender distribution of total admissions (cause-specific admissions for emergency AMI) through the proportions of admissions in five year age bands for men and women (36 variables). In the UK context this has been shown to do a good job of controlling for case mix (Propper and Van Reenen 2010). In robustness tests in Section V we employ additional controls for the severity of patients admitted to hospitals, using a commonly used measure of patient severity (the Charlson index), and for measures of overall deprivation of the local population in the hospital's catchment area and the income of the area.[30] There may remain some time varying, within area, unobservable that affects hospital quality and is not captured by area mortality rates or the other observables. However, changes in this unobservable would have to be systematically correlated with changes in the HHI in order to bias our results.

Our baseline models also control for the total number of admissions (cause-specific admissions for AMI mortality) to account for any effects due to hospital size, and for differences in staff skill mix between hospitals by including the proportions of total clinical staff that are doctors and qualified nurses.[31]

Table 1 presents the mean, standard deviation, minimum and maximum, and the within and between variation for all the variables used in our main regressions. The

[28] We experimented with including FT status in our regressions, but it was never remotely close to statistical significance, so we omitted it.

[29] This is constructed from data on 353 Local Authorities (LA) and standardized for age and gender. The hospital-specific area standardized mortality rate is an inverse distance-weighted average of the figures for all LAs. Data sources for this and all other covariates are listed in the Appendix (Table A1).

[30] The Charlson index is an index of severity of illness based upon a patient's diagnoses and procedures. 19 co-morbidities are constructed based on the patient's diagnoses and procedures. These are aggregated using weights derived from estimates of the co-morbidities' contribution to predicting mortality. The additional measures of population health are discussed in Section V.C.

[31] There is a well established relationship between hospital volume and patient outcomes (see, e.g., Gaynor, Seider, and Vogt 2005; Gowrisankaran, Ho, and Town 2006). While in principle the number of admissions may be endogenous, the recent studies by Gaynor et al (2005) and Gowrisankaran et al (2006) can not reject exogeneity. Further, we estimate our models both including and excluding the total number of admissions (see the Appendix).

average hospital in our estimation sample admits just under 68,000 patients and has 361 emergency AMI admissions a year. About 13.2% of AMI patients aged 55 and older die in the hospital within 28 days. 6.9% of those aged 35-74 die within 30 days in either the hospital or the community. 1.6% of all patients admitted die in the hospital within the first 28 days after admission. However, there is wide variation in these rates between, and within, hospitals. Around half of the variation in the AMI mortality rate and around 30% of the variation in the all-cause mortality rate is within hospitals.

D. Did the reforms result in less concentration?

Our estimates use differences in market concentration within hospitals (as well as between variation) to identify the impact of the policy. Before presenting our estimates, we therefore examine whether the policy had an impact on levels of market concentration.

Figure 2 presents the kernel density estimate of the distribution of the actual HHI at the hospital level for 2003 and 2007. The figure shows a clear leftward shift in the distribution of HHI levels over the time period so that in 2007 the level of concentration faced by hospitals had fallen at virtually all HHI levels, with the bulk of the change in the middle of the distribution.

To show the spatial distribution of market concentration, the left hand panel of Figure 3 plots the location of hospitals and their concentration levels in 2003, divided into quartiles of the actual HHI. Darker blue dots represent more concentrated markets. As expected, hospitals in the least concentrated markets pre-policy (those in the bottom quartile of the HHI distribution, light blue dots) were largely located in the more densely populated urban localities, particularly in the Greater London and Manchester areas, while hospitals in the most concentrated markets (those in the top quartile of the HHI distribution) tended to be located outside urban centres. However, *changes* in concentration were not confined to the cities. The second panel of Figure 3 shows that some hospitals located in the largest urban areas experienced the largest decrease in concentration (dark red dots indicate largest change, yellow indicates least change), but many of the hospitals which experienced the largest decrease after the implementation of the pro-competition policies of the 2000s are actually located around, rather than in, urban areas. Large decreases in the level of concentration faced by hospitals have therefore occurred both in the more densely populated areas where the market structure was already relatively unconcentrated in the pre-policy period and in more rural areas where market structure was more concentrated.[32]

E. Did patients respond to the reforms?

One of the intentions of the reforms was to change the patterns of care-seeking by patients.[33] If the reforms were successful we would expect to see this reflected in the data. We examine these patterns in Table 2, which shows the change in patient care seeking post-reform by hospital quality, as measured by 2003 hospital mortality rates. We use 2003 rates as mortality is observed with a lag and to reduce the likelihood of simultaneous determination of mortality and patient volume. If patients became more responsive to quality post policy we should see better hospitals (those in the bottom quartile of the mortality distribution) attracting more patients relative to worse hospitals (those in the top quartile). That is exactly what the data show. The total number of patients increased overall, but better hospitals experienced a larger increase in patients than did worse hospitals. The distances patients travelled for care also increased more for better hospitals, as did the share of patients bypassing their nearest hospital. This provides reassurance that there is a patient response to quality and that it increased during the reform.

F. Test of the difference-in-differences assumptions

Our objective is to determine whether the differences in the evolution of average outcomes across groups of hospitals in England are due to changes in market structure brought about by the pro-competition policy changes or whether they mainly reflect pre-existing (observable and unobservable) differences between hospitals located in

[32] The correlation between levels of HHI in 2003 and changes in HHI between 2003-2007 is -0.09 (p-value = 0.250) showing that changes in competition levels after the reforms occurred for hospitals in both more and less concentrated markets pre-policy.

[33] We note that decisions about where to seek care are likely to be the product of patient and family preferences and doctor advice. The identity of the decision maker is not critical here. What matters is whether decisions about where to go respond to quality.

markets with different structures.[34] To test whether our DiD assumptions are satisfied we examine the bivariate relationship between the observed baseline conditions and the subsequent four year change in the HHI. Differences in HHI change associated with the baseline conditions may indicate that hospitals that differ in terms of HHI growth also differ in terms of unobserved factors. The bivariate associations between the baseline conditions, as measured by the controls used in the main analyses and in the robustness checks, and the subsequent change in market structure are presented in columns (1)-(8) of Table 3. In no cases were any of the baseline conditions significantly associated with the subsequent HHI change. Columns (9)-(11) present the bivariate associations between the initial levels of mortality and the subsequent changes in market structure. Again, none of these associations are significantly different from zero. We conclude that our DiD assumptions are likely to be satisfied.

V. Results

Figures 1a and 1b presented in Section I suggested that the introduction of competition reduced the AMI and all-cause death rates in markets where the policy could have more effect – for those hospitals operating in markets with lower levels of concentration. In this section we formally test this using equation (1) to estimate the effect of the policy. We begin by looking at the impact of competition on quality measured by our four sets of death rates. We then examine the impact of the policy on the volume and composition of patients treated and on simple measures of productivity, subject our results to a wide set of robustness checks and present estimates of the financial magnitude of the effects.

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[34] It is unlikely that hospitals in less concentrated markets are similar in terms of observables to hospitals in more concentrated markets as hospital density is positively associated with population density. An examination of the relationship between observables and market structure in 2003 (available from the authors) shows hospitals in less concentrated markets had lower total admissions, larger shares of doctors and qualified nurses, larger retained surpluses, lower average Charlson index, had more deprived catchment areas but had higher area average wages.

A. The impact of competition on quality

Table 4 reports our DiD estimates of the impact of market structure on hospital quality.[35] All estimates control for the HHI and a 2007 year dummy, case mix, cause-specific admissions, staffing and the health of the population in the hospital's catchment area. Column (1) presents estimates for the in-hospital death rate after emergency AMI admission. Concentration has a statistically significant positive effect on mortality, i.e. higher market concentration (a larger HHI) leads to lower quality.[36] A 10% increase in the HHI leads to an increase of 2.46% in the AMI death rate.[37]

It is possible that the in-hospital mortality rate may not adequately measure mortality post-admission. Some patients will die outside the hospital after being discharged. Further, it is possible that hospitals facing more competition may discharge patients sooner, with adverse health effects (the "quicker and sicker" response). We therefore also use AMI mortality occurring anywhere (in-hospital or community) within 30 days after admission to address these concerns. Column (2) presents the results for the AMI mortality rate in any location 30 days after admission.[38] The estimates again show that higher market concentration significantly leads to poorer outcomes.[39]

[35] All dependent variables and covariates (except retained surplus where used as this may be negative) are in logs. To ensure that this functional form does not drive our results we estimate in levels in robustness tests in Table 6.

[36] In principle, one should take account of the fact that the predicted HHI is estimated in calculating the standard errors. However, the predicted HHI is constructed from a patient choice model estimated on the population of hospital elective admissions, which number 6.5 million in 2003 and 7.8 million in 2007. As a consequence, there is little sampling variation to account for. To be conservative, we examined this empirically. We generated ten bootstrap samples of hospital elective admissions for each year, estimated the patient choice model on each sample, then constructed predicted hospital HHIs (as described in Section IV). The intra-hospital correlations between the bootstrapped predicted HHIs were 0.9977 for 2003 and 0.9999 for 2007. This indicates that there is in fact very little sampling variation empirically.

[37] Full estimation results for the in-hospital AMI mortality rate are presented in Table A3 in the Appendix.

[38] The data linking deaths post discharge to the hospital of discharge are only available for persons aged 35-74.

[39] While the point estimate is about 25% higher than for in-hospital mortality the difference is not statistically significant.

Column (3) presents the DiD estimate for the all-cause mortality rate. The estimate again shows a significant relationship between quality and market concentration. The magnitude is considerably smaller than that for AMI. To test whether the estimated effect for all-cause mortality is driven only by AMI, column (4) presents the DiD estimate for all-cause in-hospital mortality excluding deaths after AMI admissions. The coefficient is essentially the same as when AMI deaths are included, indicating that there is an effect from the policy on both the AMI death rate and the death rate following all other admissions.[40, 41]

The larger effect we find when quality of care is measured by the AMI mortality rate rather than all-cause mortality rate is likely to be due, at least in part, to the fact that many conditions that result in death in the hospital are not responsive to better quality health care. Nolte and McKee (2008) report that "amenable mortality" - deaths from causes that should not occur in the presence of timely and effective health care - accounted for around 27% of total mortality for males aged under 75 years in the UK in 2002-03, and 33% for females. On the other hand, this study (and other studies based on systematic reviews of published clinical evidence, see Nolte and McKee 2004 for a review) includes ischemic heart disease - and its AMI component - among the causes of death which are amenable to better health care, with around half of such premature deaths considered to be avoidable by factors such as better management of the condition within the hospital. Therefore, the effect of improvements in hospital care quality (driven by the pro-competition reforms) is likely to be larger when quality is measured by AMI mortality rates than when quality is measured by the overall death rate, as the latter includes several conditions for which mortality is less (or not at all) affected by health care quality.

[40] It is worth reporting the DiD estimates with no controls except hospital fixed effects, as they represent the unconditional effect of the policy. The DiD estimates (standard errors) for the regressions without controls are as follows: in-hospital AMI mortality, 0.113 (0.080), AMI mortality in any location, 0.204 (0.090), all-cause in-hospital mortality, 0.095 (0.028), and all-cause in-hospital mortality excluding AMI, 0.096 (0.028).

[41] It is worth noting that the estimates are robust to whether the number of admissions or the area mortality rate are included. One might be concerned about the possible endogeneity of these measures, but the point estimates and their significance are virtually unchanged by excluding them.

In summary, we find that hospitals operating in less concentrated markets had significantly lower mortality rates post-reform than those in more concentrated markets.[42] We infer from this that the policy "worked" – it increased competition in less concentrated markets and the increased competitive pressure led to improvements in quality.

Although the estimates are statistically significant, the estimated magnitude of the response, while not trivial, is relatively modest. A 10% fall in the HHI is associated with a fall in the in-hospital death rate following AMI admissions by 2.46%. This amounts to 1/3rd of a percentage point at the mean AMI death rate of 13.2%. Our estimated magnitudes are also fairly similar to those from some other relevant studies. Kessler and McClellan (2000) estimate that a move from the top quartile to the bottom quartile of the HHI in their sample will lead to a 3.37 percentage point fall in the AMI death rate. The equivalent figure using our estimates and data is 3.61 percentage points.[43] Cooper et al. (2010, p.27) find that a one standard deviation increase in their measure of competition for English hospitals is associated with a 0.3 percentage point reduction in the 30 day in-hospital AMI mortality rate (per year) following the NHS pro-competition reforms. Our results imply a very similar 0.33 percentage point reduction for each year post-policy (2004 to 2007).[44] We discuss the economic significance of these estimates at the end of this Section.

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[42] We also examined another measure of quality - the MRSA rate - and two access measures - the share of patients waiting more than three months and share of attendances spending more than four hours waiting for care in the emergency room (A&E department). Using the same model as presented in Table 4, the coefficients (standard errors) on these estimates are -0.110 (0.118), 0.078 (0.167) and -0.005 (0.011). Thus none of these outcomes are associated with the policy change. MRSA rates are highly influenced by changes in behavior in the community as well as hospital policy (see e.g., Ferry and Etienne 2007 and references therein) and so may not respond strongly to hospital level attempts to reduce them. Waiting times had been the target of a major policy campaign between 2000 and 2005 and had fallen substantially by 2005 (Propper et al. 2010), perhaps leaving relatively little scope for further reductions in response to the competition policy.

[43] See Appendix 3 in their paper, which presents the results for the specification in which HHI enters linearly. Their measure of mortality is the 1 year AMI mortality rate for US Medicare beneficiaries. The comparable estimated effect for our sample is derived as follows. A 43 unit decrease in HHI (=1% in our sample) leads to a 0.032 percentage point decrease in AMI deaths at the mean (= 0.246% *13.2%). So a unit decrease in HHI leads to 0.032/43 = 0.000744 percentage point decrease in AMI deaths. The difference in mean HHI between the top and bottom quartiles in our sample is 4,854.6. This equals a 3.61 percentage points (= 4854.6*0.000744) fall in the death rate.

[44] The effect in our sample is derived as follows. Using our estimated AMI coefficient, a one standard deviation reduction in HHI (=1,921 units, a 41% reduction) from the mean implies a 10.1% reduction in AMI deaths for the post-policy period, or 1.33 percentage point (our mean AMI death rate is 13.2%). This is equivalent to a 0.33 percentage point decrease per year in AMI mortality for the period 2004-2007 (=1.33/4).

B. The impact of competition on other aspects of performance

We also examine whether the reform had an impact on resource use. In Table 5 we examine the mean length-of-stay of admitted patients (LOS), the total number and the mix of admissions (the shares that are elective and non-elective), expenditure and a simple measure of (lower) productivity, expenditure per admission.

Column (1) indicates that increases in concentration are significantly associated with a rise in the length-of-stay. The estimated coefficient implies that a 10% fall in a hospital's HHI on average is associated with a 2.5% fall in length-of-stay. At the mean length-of-stay in the sample of 1.2 days this is just under an hour.[45] However, the policy change does not seem to have affected either the total number of admissions or their composition (columns (2)-(4)). Nor did the policy result in any change in either total hospital operating expenditure or expenditure per admission.[46] Overall, we do not find evidence that resource utilization increased in less concentrated markets following the reforms.

Taken together, the findings for quality (Table 4) and resource utilization (Table 5) indicate that hospitals facing more competitive pressure were able to find ways to marshal resources more efficiently to produce better patient outcomes.

C. Robustness checks

Table 6 reports a large number of robustness tests for AMI, all-cause mortality and length-of-stay. All cells report the DiD estimates from separate regressions using model (1). The first row presents the baseline results from Table 4, columns (1) and (3), and Table 5, column (1).

<u>Placebo tests</u>

If our results are being driven by pre-existing observable or unobservable differences between hospitals facing different levels of market concentration, we would expect to find significant estimates if we compared hospitals facing more and less concentrated

[45] Farrar et al. (2007) find that the application of PbR was associated with reductions in the average length-of-stay ranging between 1.2%-2.3%, depending on the comparison group used..

[46] We also find no policy effect on a simple measure of labor productivity (number of admissions per clinical staff). The DiD coefficient (standard error) is -0.020 (0.026).

market structure *before* the reforms were introduced. To test this we undertake placebo DiD tests by estimating the same models as in Tables 4 and 5 using data from before the reforms.[47] Row 2 in Table 6 presents the results using 2001 as the placebo "policy-off" year and 2003 as the placebo "policy-on" year. None of the estimated coefficients in row 2 are even close to being statistically significant at conventional levels and their magnitudes are very small compared to the baseline estimates. This suggests that our results are driven by the reforms rather than due to pre-existing differences between hospitals.

<u>Estimation using only variation in pre-policy market structure</u>

In our model, identification comes from the within hospital change in (predicted) market structure. Figure 2 shows that while market concentration does change, this change is relatively small due to the short time period we study. We therefore estimate a different specification of the DiD in which we estimate the impact of the policy from only cross-sectional variation in exposure to the policy, where exposure is defined as the market structure pre-policy. The idea behind this estimator is that hospitals located in less concentrated markets before the policy will face more competitive pressure after the policy.[48] We use the predicted pre-reform HHI ($HHI^{p}_{i,2003}$) as the measure of policy exposure and estimate:

$$q_{it} = \gamma_0 + \gamma_1 I(t=2007) + \gamma_2 I(t=2007) * HHI^{p}_{i,2003} + \gamma_3 X_{it} + \mu_i + \xi_{it} \qquad (2)$$

The impact of market structure differs pre- and post-reform through the interaction of the fixed $HHI^{p}_{i,2003}$ with the post-reform year indicator. This approach identifies a change in conduct due to the reform, the key identifying assumption being that without the policy intervention the trend in the outcome would have been the same whatever the market structure. Treatment induces a deviation from this parallel trend.

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[47] A few hospitals used in our main regressions were not active before 2003 and this slightly changes the sample size compared to the baseline estimates.

[48] Similar ideas have been used in other contexts (Angrist and Pischke 2008), for example in evaluation of the employment effects of the minimum wage (Card 1992). In the health care context this approach was used by Propper et al. (2008) to study the 1990s NHS internal market reforms and adopted by Cooper et al. (2010) to study the current NHS reforms.

Row 3 presents the DiD estimates from equation (2). It is clear that these are statistically significant and virtually identical to those from our preferred specification, indicating that our results are robust to the exact specification of the DiD model.[49]

Further controls for patient heterogeneity

In our main regressions we control for differences in case mix between hospitals with 36 demographic variables (the shares of admissions within 5 year age-gender bands), the local area mortality rates and a full set of hospital dummies. This goes some way to alleviating the concern that our estimates are biased by different patterns of case severity across hospitals facing different market structure (for instance, by more severely ill patients systematically choosing high quality urban hospitals post-reform). We test the robustness of our results by including a further measure of the severity of patients treated in the hospital, the Charlson index. We do not use the index in our main regressions because of concerns that it may pick up-coding responses to the PbR system. However, provided up-coding post-policy is not correlated with market structure, the Charlson index is a good further control for case mix.[50] The results in row 4 of Table 6 show that the DiD coefficients change little after inclusion of this measure. In addition, the estimated coefficients on the Charlson index (not shown) are themselves never close to statistical significance.

As a further test of patient heterogeneity issues, we test the sensitivity of our results to inclusion of controls for changes in the health of the population in the catchment area of the hospital. Our main results already include a direct measure of population health (the age-gender standardized mortality rate of the population in the catchment area). We augment this with a measure of social deprivation (which includes a health dimension) of the patients admitted to the hospital: this is the government constructed

[49] We also estimated equation (1) using actual HHIs rather than predicted. The coefficients are significant and similar for AMI deaths (coefficient = 0.256, se=0.135), but larger for all-cause mortality (coefficient = 0.093, se=0.043) and length-of-stay (coefficient = 0.488, se=0.091) indicating that our use of predicted HHIs provides more conservative estimates.

[50] DRG systems have been argued to give incentives for greater coding of patient severity in order to move patients into categories which have higher reimbursement (see e.g., Ellis and McGuire 1986, Dafny 2005).

Index of Multiple Deprivation.[51] The results in row 5 show an increase in the coefficient for AMI mortality and length-of-stay and a decrease in the coefficient for all-cause mortality after inclusion of these controls but the results remain statistically significant. We conclude that our estimates are robust to these extra case mix controls, reducing concerns that unobserved heterogeneity in patient severity may be driving our results.

Financial position of the hospital

We do not control in our main models for hospitals' financial surpluses or deficits, since this may be endogenous. Hospitals with higher quality may admit higher volumes of patients and this could result in higher surpluses, and vice versa. However, the switch to PbR may give hospitals a large income shock. Under PbR hospitals get a price per procedure equal to the average cost of all hospitals. This price may be far from the hospital's own cost. As PbR payment accounted for a large proportion of hospital revenue, some hospitals may have experienced large income shocks (positive and negative) when full PbR was rolled out in 2006. Our results may be driven by this income shock rather than changes in market concentration. To test this, we include an additional control for the hospital's financial position as measured by the end of year surplus/deficit. Row 6 presents the estimates including this control. The DiD point estimates change very slightly but remain statistically significant at conventional levels and we therefore conclude that the response is the result of competitive pressure rather than the result of any associated income shock.

Weighting and functional form

We exclude hospitals with low volumes of admissions to ensure our results are not driven by the variability in rates induced by low denominators. To test that our results are robust to this exclusion we re-estimated the model including all observations, but weighted our regressions by either the total number of admissions (for the all-cause mortality rate and length-of-stay) or the cause-specific admissions (for the AMI mortality rate). Row 7 presents these estimates. The coefficients for all-cause mortality and length-of-stay are virtually unchanged, while that for AMI falls by

[51] The Index of Multiple Deprivation is an average of the rankings for the patients' local areas of residence (where ranking=1 for the most deprived area in the year). The overall index is constructed as a weighted area level aggregation of specific dimensions of deprivation including income, health and education.

around 45% but remains statistically significant. The results suggest that hospitals with small volumes of AMI admissions are different from those with higher volumes, but we still find a statistically significant effect.

In row 8 we test the robustness of our results to our functional form assumptions and estimate the models with both the dependent variable and HHI in levels instead of logs. We report the implied elasticity estimates from this specification to enable comparison with the baseline results. The table shows that our results are robust to estimation in levels. The estimates are statistically significant, are somewhat lower than the log specification for AMI and length-of-stay but are almost identical for all-cause mortality. As a further robustness check of our functional form assumptions, we investigated the policy effects at quartiles of the HHI distribution. A DiD specification using an indicator for whether the hospital is in the top quartile of the HHI distribution rather than the continuous variable shows that hospitals operating in more concentrated markets post-policy had higher death rates and length-of-stay than their counterparts in less concentrated markets.[52]

Local area economic conditions

Between 2003 and 2007 areas with higher and lower hospital concentration may have experienced different economic growth rates. Recent research has suggested that economic growth can adversely affect AMI outcomes, with a greater number of heart attacks being observed during upturns in the business cycle (Ruhm 2006). It could therefore be argued that the impact of market concentration that we find is not due to the result of poor hospital quality, but to smaller falls in fatalities due to higher rates of economic growth in more concentrated markets. To ensure our results are not driven by differential business cycle effects we add a control for a time varying measure of economic activity at the level of the hospital's catchment area. This is the average male full-time wage.[53] Row 9 presents the results and shows that our DiD

[52] The coefficients (standard errors) on the top quartile of HHI are 0.137 (0.061) for the AMI death rate, 0.074 (0.024) for all-cause mortality, and 0.103 (0.062) for length-of-stay.

[53] Male full-time wage is the average of the median full-time gross wages for male workers (all occupations) in the local area districts within a radius of 30 kilometers from the hospital. In the context of our study, area male wages do not serve exclusively as a proxy for changes in local income. Since male wages are highly correlated with female wages, controlling for the former should also account for the impacts of local wages on hospital staff quality that in turn affects health outcomes, as discussed in Propper and Van Reenen (2010).

effect of market concentration remains virtually unchanged for the AMI death rate (and the two other hospital outcomes).

In addition to the potential effect of the economic cycle on health status, economic growth might also directly affect hospital emergency outcomes. This is particularly relevant for deaths following AMI admissions. Stronger economic activity is likely to generate increased traffic flows and road congestion, thus potentially increasing the time elapsed between the heart attack and hospital arrival ("floor to door" time), so decreasing the chances of patient survival. To address this we add a control for ambulance speeds to our estimates for AMI. The government monitors the proportion of urgent and life-threatening ambulance calls which arrive at the scene of the incident within eight minutes and we use this independent assessment. The results in row 10 show that our market concentration impacts on AMI mortality are robust to this additional control.[54] These tests provide reassurance that our market concentration effects are not attributable to differential economic growth rates across localities.

Overall, we conclude our results for the impact of the reforms are robust to a wide range of checks.

D. *Did the policy matter?*

To provide a better sense of the economic significance of the reforms we undertake some simple back of the envelope calculations. The first is to calculate the benefits in monetary terms from the observed change in market structure following the reforms. We calculate the value of the gain in life years that would arise from a change in the HHI equal to the observed (average) decrease in HHI between 2003 and 2007 (this equals 118). Using the estimated coefficient from Table 4, column (3), the average hospital would experience a 0.2% fall in its overall mortality rate from this decrease

[54] We also find a strongly significant DiD estimate using AMI mortality rate on or after discharge (coefficient (standard error) = 0.400 (0.140)). The corresponding estimates for all-cause mortality are 0.058 (0.035), significant at 10%. Due to missing data on ambulance response times for some hospitals, our sample size is slightly reduced for this robustness test (233 observations compared with 250 in the baseline in-hospital AMI model, and 305 compared to 323 for all-cause mortality). Our baseline results of the effect of market concentration on in-hospital AMI death rates are almost unchanged if we estimate the model using the same sample used for the ambulance response robustness check: the estimated DiD coefficient (standard error) is 0.249 (0.099). For all-cause deaths, the corresponding estimates are 0.060 (0.032).

in the HHI.[55] The average age of death of patients in hospital is 77 years. A 77 year old male in Britain has an additional life expectancy of 9.5 years and a female has an additional life expectancy of 11 years. Using our estimate that 0.2% percent of these deaths are averted and combining it with these extra years of life leads to an estimated 3,354 life years saved.[56] If we adopt the $100,000 benchmark of Cutler and McClellan (2001) for the value of a year of life, the beneficial effects of the pro-competition reforms are on the order of $335.4 million, or approximately £227 million.[57]

This calculation gives the average benefit associated with the policy change. A second calculation is derive the cost of being in a concentrated compared to an unconcentrated market. We therefore compare the difference in life years saved for a hospital located in a market at the average concentration versus one with low concentration, defining low concentration to be an HHI one standard deviation below the mean. This is a difference in HHI of just under 2,000. Using the estimated coefficient from Table 4, column (3), a hospital in the lower HHI market would have 3.1% fewer deaths per year. Using the same methods and numbers as above this translates into 54,771 more life years saved, with a monetary value of $5.5 billion, or £3.7 billion.[58]

As a basis for comparison, the annual NHS budget is of the order of £100 billion. The estimate of the immediate impact of the policy (£227 million) is approximately 2-10ths of one percent of the NHS budget. While this is small, it is not trivial, as it represents the short run impact of the policy immediately after implementation and we

[55] A one percentage change in HHI is equal to 43.5 units, so a fall of 118 would lead to a change of 0.069*118/43.5 = 0.2%.

[56] The calculations of lives saved were made separately for each hospital and then aggregated up, weighting by the hospital's number of admissions in 2007. The average age of death of 77 years is a weighted average for our sample of hospitals using as weights the observed death rates within six age bands (under 15, 15-34, 35-54, 55-64, 65-74 and 75+). Male and female life expectancy is from "Interim Life Tables, England, 2006-2008", Office for National Statistics.

[57] A US dollar exchanged for a pound sterling at a rate of 0.676 on May 7, 2010 (http//:www.xe.com).

[58] We find no change in either operating expenditure or operating expenditure per admission following the implementation of the policy. So these life year gains did not increase the cost paid by the tax payer. However, the policy did lead to a fall in length-of-stay, which should have reduced costs. A conservative assumption is that the cost of achieving the extra quality is equal to the value of the reduction in length-of-stay. Using the coefficient from Table 5, column (1), and assuming a cost per day in hospital of £250 (based on personal communication with the UK Department of Health), this gives values of £24 million ($35.5 million) for the average change in HHI 2003-2007, and £0.4 billion ($0.6 billion) for the difference between the mean HHI and one standard deviation below the mean for the value of the reduction in length-of-stay. In both cases this is considerably less than the value of the lives saved.

only enumerate the gains in quality arising from decreases in death rates and not any other aspects of quality that are important, but not readily measured (e.g., quality of life).

It is possible that we are recovering short run effects of the policy, since we only have one year of data following implementation. If so, the long run effects could be larger once hospitals and patients adjust fully to the new system. In addition, the estimate of the gains from a change in market structure from high to low concentration (£3.7 billion) are substantially greater than the short run effects. This suggests that there could be large positive effects of policies that result in substantial decreases in concentration.[59]

VI. Summary and conclusions

We have examined the impact of the introduction of a pro-competition policy on hospital outcomes in England. Our results constitute some of the first evidence on the impacts of a market-based reform in the health care sector. We find strong evidence that under a regulated price regime that hospitals engage in quality competition and that the 2006 NHS reforms were successful. Within two years of implementation the NHS reforms resulted in significant improvements in mortality and reductions in length-of-stay without changes in total expenditure or increases in expenditure per patient. Our back of the envelope estimates suggest that the immediate net benefit of this policy is around £227 million. While this is small compared to the annual cost of the NHS of £100 billion, we have only calculated the value from decreases in death rates. Allowing for improvements in other less well measured aspects of quality will increase the benefit, as will any further falls in market concentration which may occur as the policy continues in operation. If the UK were to pursue policies that lead to de-concentration of hospital markets, the gains could be substantially larger.

[59] This does not mean that quality was optimal following the reform. Further, this is not a precise welfare calculation, merely a simple illustrative exercise.

These results suggest that competition is an important mechanism for enhancing the quality of care patients receive. Monopoly power is directly harmful to patients, in the worst way possible - it substantially increases their risk of death. The adoption of pro-market policies in European countries, as well as policies directed at increasing or maintaining competition such as antitrust enforcement, appear to have an important role to play in the functioning of the health sector and assuring patients' well being.

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Table 1: Descriptive statistics

Variable		Mean	Standard deviation	Minimum	Maximum	Observations
Death rates						
28 day AMI mortality rate	overall	13.2	3.7	4.1	26.6	N = 251
(in-hospital, ages 55+, %)	between		3.1	7.4	23.4	n = 133
	within		2.4	6.1	20.3	
30 day AMI mortality rate	overall	6.9	2.6	1.8	22.8	N = 251
(on or after discharge, ages 35-74, %)	between		2.4	3.3	22.8	n = 133
	within		1.6	0.8	13.1	
28 day all-cause mortality rate	overall	1.6	0.6	0.0	3.3	N = 324
(in-hospital, all ages, %)	between		0.5	0.1	2.7	n = 162
	within		0.2	1.0	2.2	
28 day all-cause mortality rate	overall	1.6	0.6	0.0	3.2	N = 324
(in-hospital, excluding AMI, %)	between		0.5	0.1	2.7	n = 162
	within		0.2	1.0	2.1	
Market concentration						
Herfindahl-Hirschman index,	overall	5,543	1,410	2,674	9,050	N = 324
(actual patient flows)	between		1,395	2,742	8,896	n = 162
	within		221	4,663	6,423	
Herfindahl-Hirschman index,	overall	4,308	1,931	1,878	9,550	N = 324
(predicted patient flows)	between		1,929	1,966	9,548	n = 162
	within		139	3,218	5,398	
Admissions and length of stay						
Total admissions	overall	67,896	35,929	8,792	206,633	N = 324
	between		35,331	9,079	201,744	n = 162
	within		6,817	25,471	110,321	
Total AMI admissions	overall	361	169	150	1,066	N = 251
(ages 55+)	between		154	150	973	n = 133
	within		69	149	573	
Mean length-of-stay	overall	1.2	0.8	0.3	7.1	N = 324
(days)	between		0.8	0.5	6.8	n = 162
	within		0.3	0.1	2.2	
Elective admissions	overall	52.4	12.2	24.4	98.4	N = 324
(% of total)	between		12.0	26.7	98.2	n = 162
	within		2.3	42.6	62.2	
Non-elective admissions	overall	47.6	12.2	1.6	75.6	N = 324
(% of total)	between		12.0	1.8	73.3	n = 162
	within		2.3	37.8	57.4	
Staffing and finances						
Doctors	overall	13.5	2.2	8.0	20.1	N = 323
(% of total clinical staff)	between		1.8	8.4	18.3	n = 162
	within		1.2	10.5	16.5	
Qualified clinical staff	overall	53.2	3.8	42.2	68.3	N = 323
(% of total clinical staff)	between		3.6	45.0	65.1	n = 162
	within		1.3	49.3	57.1	
Retained surplus	overall	0.2	6.5	-40.3	56.0	N = 303
(£1,000)	between		4.7	-22.2	28.0	n = 162
	within		4.3	-27.7	28.2	
Operating expenditure	overall	197,082	125,368	18,881	766,137	N = 319
(£1,000)	between		120,012	37,764	691,830	n = 162
	within		35,841	85,736	308,428	
Total expenditure per admission	overall	3.0	1.3	0.2	9.9	N = 319
(£1,000)	between		1.2	1.1	9.4	n = 162
	within		0.4	1.4	4.7	

(cont.)

Table 1: Descriptive statistics *(continued)*

Variable		Mean	Standard deviation	Minimum	Maximum	Observations
Area health, case mix and economic conditions						
Standardized mortality rate	overall	100.0	10.0	77.6	129.5	N = 324
(per 100,000, normalized)	between		8.4	83.5	123.0	n = 162
	within		5.4	91.5	108.5	
Charlson index	overall	0.48	0.23	0.03	1.85	N = 324
(average for all admissions)	between		0.22	0.04	1.83	n = 162
	within		0.05	0.28	0.69	
Index of Multiple Deprivation	overall	15,411	4,940	3,849	26,985	N = 324
(average over patients' rankings)	between		4,929	3,902	26,676	n = 162
	within		423	14,120	16,701	
Male full time wage in area	overall	24,955	3,774	18,985	34,551	N = 320
(£)	between		3,391	19,691	32,362	n = 160
	within		1,668	22,111	27,799	
Urgent ambulance calls responded within eight	overall	76.4	3.3	55.7	86.6	N = 306
minutes (%)	between		2.6	63.9	83.8	n = 162
	within		2.0	68.1	84.6	

Notes: Summary statistics refer to fiscal years 2003 and 2007. N = Total number of observations for the whole sample; n = Total number of hospitals in the sample. The samples for the AMI mortality rates include only hospitals with at least 150 AMI admissions. Herfindahl-Hirschman indices computed using all elective services. Staffing variables refer to shares of whole time equivalent clinical staff. Age-gender area standardized mortality rate (SMR, normalized) is an inverse distance weighted average rate specific to the hospital. Index of Multiple Deprivation is an average of the rankings for the patients' local areas of residence (where ranking=1 for the most deprived area in the year). Male full time wage is the average of the median full-time gross wages for male workers (all occupations) in the local area districts within a radius of 30 kilometers from the hospital. Average Charlson index for admissions at the hospital. Share of urgent and life-threatening (category A) ambulance calls receiving an emergency response at the scene of the incident within eight minutes. We also use, as measures of case mix, 36 variables corresponding to shares of cause-specific admissions within 5 year age-gender bands.

Table 2: Descriptive Changes in Patient Care Seeking by Hospital Mortality Rate

	AMI mortality rate (2003)					
	Bottom quartile			Top quartile		
	2003	2007	% change (2003-07)	2003	2007	% change (2003-07)
Number of elective admissions	33,985	38,274	12.6%	41,398	45,132	9.0%
Average distance travelled by patients	11.4	11.7	2.4%	10.0	10.1	1.1%
Share of patients bypassing nearest hospital	0.37	0.39	5.4%	0.45	0.43	-4.4%
Number of hospitals	33	33		32	32	

Notes: Time period is years 2003 and 2007. Elective admissions only. In-hospital 28 day AMI mortality rate (ages 55+) measured in 2003 for hospitals with at least 150 AMI admissions. Sample means of variables in the rows for quartiles of AMI mortality (bottom 25% hospitals and top 25% hospitals). Average distance travelled by patients who attended the hospital in kilometers.

Table 3: Tests of association between changes in HHI and baseline covariates and outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Covariate	Total admissions	AMI admissions (ages 55+)	Doctors (share of clinical staff)	Qualified clinical staff (share of clinical staff)	Area standardized mortality rate	Case mix	Index of Multiple Deprivation (average for patients' areas of residence)	Charlson index (average for admissions at the hospital)	28 day AMI mortality rate (in-hospital, ages 55+)	30 day AMI mortality rate (on or after discharge, ages 35-74)	28 day all-cause mortality rate (in-hospital)
Coefficient	-0.624	-0.051	5.329	-10.233	-1.695		-0.001	81.732	-1.080	-1.808	47.847
	(0.642)	(0.086)	(8.792)	(7.947)	(1.936)		(0.003)	(72.197)	(4.751)	(6.623)	(42.840)
P-value for Wald test						0.129					
Observations	162	151	161	161	162	162	162	162	130	130	162

Notes: Time period is years 2003 and 2007. Herfindahl-Hirschman index (HHI) for all elective services calculated using predicted patient flows. The coefficients reported are from separate OLS regressions where the dependent variable is the change in the hospital's HHI between 2007-03 and the regressor is the variable in the column measured in year 2003 (with a constant and no other regressors). Standard errors in parentheses under coefficients are robust to arbitrary heteroskedasticity. Column (1) is total admissions in 1,000s. Columns (3)-(4) are shares of whole time equivalent clinical staff. Column (5) is the area age-gender standardized mortality rate, an inverse distance-weighted average rate specific to the hospital (normalized with mean 100 and standard deviation 10). The case mix in column (6) reports the p-value for the joint Wald test of significance of 36 variables corresponding to shares of cause-specific admissions within 5 year age-gender bands. The Index of Multiple deprivation in column (7) is the average for the patients' areas of residence, where rank = 1 is least deprived. The estimation samples for the AMI variables include only hospitals with at least 150 AMI admissions in 2003. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Difference-in-differences estimates of the impact of market structure on outcomes

Dependent variable	(1) 28 day AMI mortality rate (in-hospital, ages 55+)	(2) 30 day AMI mortality rate (on or after discharge, ages 35-74)	(3) 28 day all-cause mortality rate (in-hospital)	(4) 28 day mortality rate (in-hospital, excluding AMI)
DiD coefficient	0.246***	0.313***	0.069**	0.066**
	(0.084)	(0.116)	(0.027)	(0.028)
Case mix controls (36) (p-value)	0.007	0.106	0.000	0.000
Cause-specific admissions (p-value)	0.008	0.079	0.001	0.001
Staff controls (p-value)	0.020	0.125	0.091	0.084
Area SMR (p-value)	0.709	0.593	0.045	0.049
Adjusted R-squared	0.502	0.503	0.980	0.979
Number of hospitals	133	133	162	162
Observations	250	250	323	323

Notes: Time period is 2003 and 2007. Models estimated by OLS with standard errors (in parentheses under coefficients) robust to arbitrary heteroskedasticity. HHI is for all elective services calculated using predicted patient flows. In addition to HHI in the respective year and the year 2007 dummy, controls are 36 case mix variables corresponding to shares of cause-specific admissions within 5 year age-gender bands, number of cause-specific admissions, doctors and qualified clinical staff as shares of whole time equivalent clinical staff (staff controls), and the area age-gender standardized mortality rate (SMR, an inverse distance-weighted average rate specific to the hospital). Dependent and independent variables (except case mix) are in logs. All models also include a constant and a full set of hospital dummies. The estimation samples for the AMI mortality rates include only hospitals with at least 150 AMI admissions. P-values refer to two-tailed t-tests of significance of the corresponding variable or joint Wald tests of significance of the group of variables. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Difference-in-differences estimates of the impact of market structure on length-of-stay, admissions and expenditure

	(1)	(2)	(3)	(4)	(5)	(6)
	Length-of-stay and admissions				Expenditure and spending per admission	
	Mean length-of-stay (days)	Total admissions (number)	Elective admissions (share of total)	Non-elective admissions (share of total)	Operating expenditure (£1,000)	Operating expenditure (£1,000) per admission
DiD coefficient	0.254***	-0.012	-0.005	-0.001	0.007	0.014
	(0.059)	(0.031)	(0.017)	(0.024)	(0.072)	(0.074)
Hospitals	162	162	162	162	162	162
Observations	323	324	324	324	319	319

Notes: Time period is 2003 and 2007. Models estimated by OLS with standard errors (in parentheses under coefficients) robust to arbitrary heteroskedasticity. HHI is for all elective services calculated using predicted patient flows. In addition to HHI in the respective year and the year 2007 dummy, controls are 36 case mix variables corresponding to shares of cause-specific admissions within 5 year age-gender bands and the area age-gender standardized mortality rate (SMR, an inverse distance-weighted average rate specific to the hospital). Dependent and independent variables (except case mix) are in logs. For mean length-of-stay we also control for number of admissions and doctors and qualified clinical staff as shares of whole time equivalent clinical staff. Expenditure in columns (5)-(6) excludes capital expenditure. All models also include a constant and a full set of hospital dummies. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Robustness tests: main coefficients from difference-in-differences models

Robustness test	(1) 28 day AMI mortality rate (in-hospital, ages 55+)	(2) 28 day all-cause mortality rate (in-hospital)	(3) Mean length-of-stay (days)
1. Baseline	0.246***	0.069**	0.254***
	(0.084)	(0.027)	(0.059)
Observations	250	323	323
2. Placebo DiD test for 2001-2003	-0.047	0.005	-0.036
	(0.077)	(0.027)	(0.047)
Observations	250	309	309
3. Using time invariant pre-reform HHI level (2003) as market structure measure	0.216***	0.066**	0.245***
	(0.079)	(0.028)	(0.059)
Observations	250	323	323
4. Controlling for the Charlson index	0.246***	0.067**	0.239***
	(0.084)	(0.027)	(0.060)
Observations	250	323	323
5. Controlling for the Index of Multiple Deprivation	0.278***	0.067**	0.263***
	(0.085)	(0.029)	(0.061)
Observations	250	323	323
6. Controlling for surpluses/deficits	0.242**	0.076**	0.229***
	(0.093)	(0.030)	(0.066)
Observations	236	302	302
7. All hospitals (weighted by number of admissions)	0.138**	0.069***	0.261***
	(0.069)	(0.024)	(0.061)
Observations	299	323	323
8. Using levels of the dependent variable and HHI (implied elasticity)	0.170**	0.069***	0.197***
Observations	250	323	323
9. Controlling for income (male wage in area)	0.247***	0.061**	0.258***
	(0.086)	(0.029)	(0.062)
Observations	248	319	319
10. Controlling for the share of urgent ambulance calls responded within eight minutes	0.238**		
	(0.100)		
Observations	233		

Notes: Models estimated by OLS with standard errors (in parentheses under coefficients) robust to arbitrary heteroskedasticity. Time period is years 2003 and 2007, except for the placebo DiD models in test 2 (2001 and 2003). HHI for all elective services calculated using predicted patient flows. Controls are year 2007 dummy (or year 2003 dummy in test 2), 36 case mix variables corresponding to shares of cause-specific admissions within 5 year age-gender bands, number of cause-specific admissions, doctors and qualified clinical staff as shares of whole time equivalent clinical staff, and the area age-gender standardized mortality rate (SMR, an inverse distance-weighted average rate specific to the hospital) normalized with mean 100 and standard deviation 10. In row 2 the dependent variable in column (1) is the AMI mortality rate at any point during a hospital stay for all ages, and the dependent variable in column (2) is the all-cause in-hospital mortality rate at any point during a hospital stay. Row 3 uses the time invariant HHI at the hospital level in 2003 ($HHI^{p}_{i,2003}$). Row 4 adds the average Charlson index for admissions to the hospital as a control. Row 5 adds the Index of Multiple Deprivation ranking for the patients' areas of residence (average over patients' rankings, where ranking = 1 for the most deprived area in the year). Row 6 adds the hospital retained surplus or deficit as a control. In row 7 regressions are weighted by AMI admissions (column (1)) or total admissions (columns (2) and (3)), and use all hospitals regardless of the number of AMI admissions (full sample). Row 8 displays the elasticity implied by the estimated coefficient (calculated at mean values) and its significance level. Row 9 adds as a control the average of the median full-time gross wages for male workers (all occupations) in the local area districts within a radius of 30 kilometers from the hospital. Row 10 adds to the AMI mortality model in column (1) the share of category A ambulance calls (defined as urgent and life-threatening) receiving an emergency response at the scene of the incident within eight minutes. In all rows except 8 the dependent and independent variables (except age-gender controls and surplus) are in logs. All models also include a constant and a full set of hospital dummies. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1a: AMI mortality vs. market structure pre- and post-reform

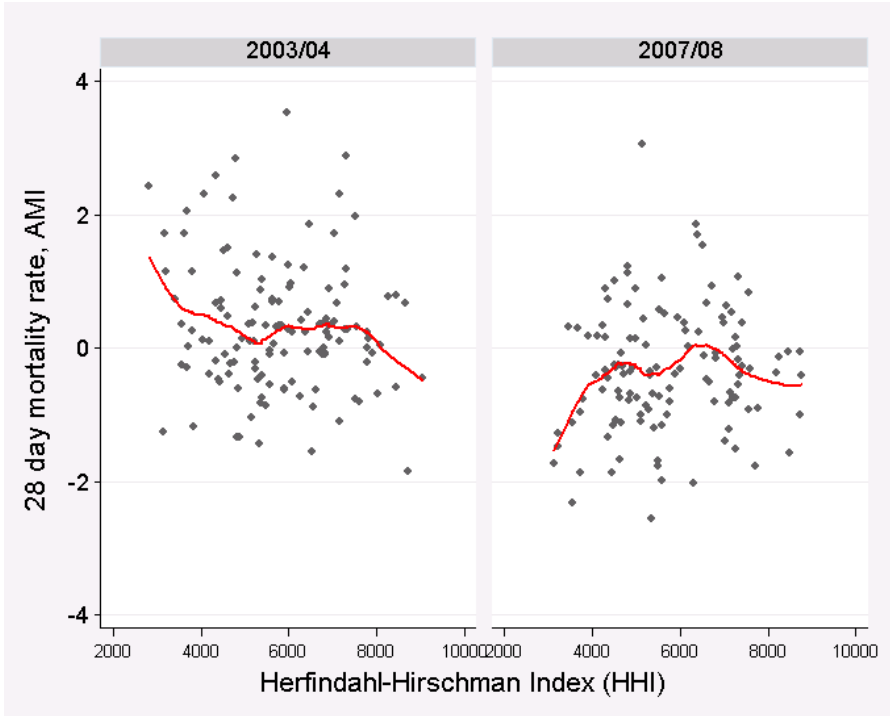
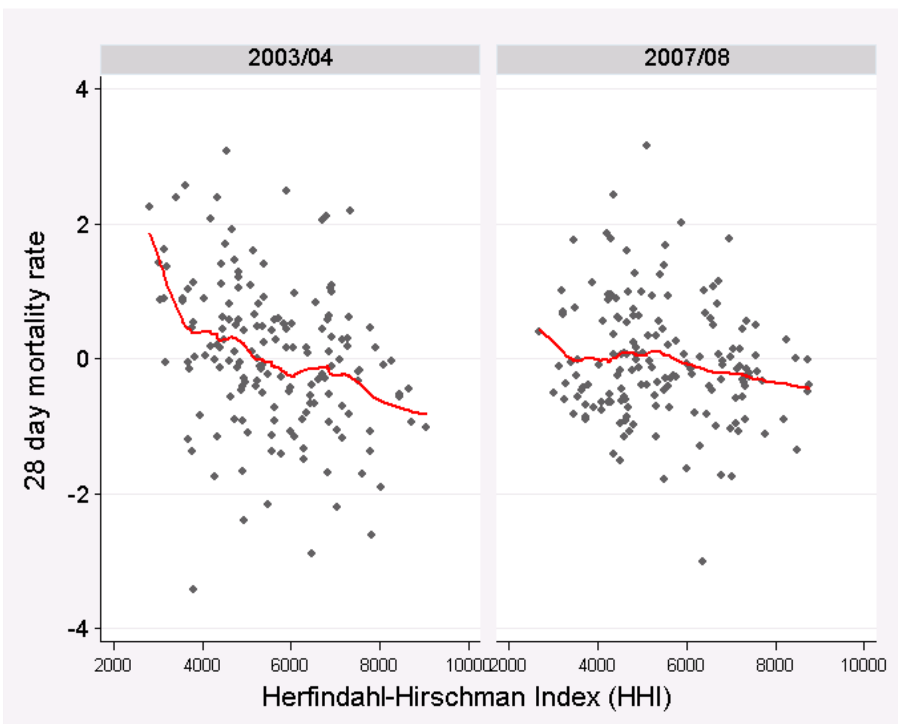
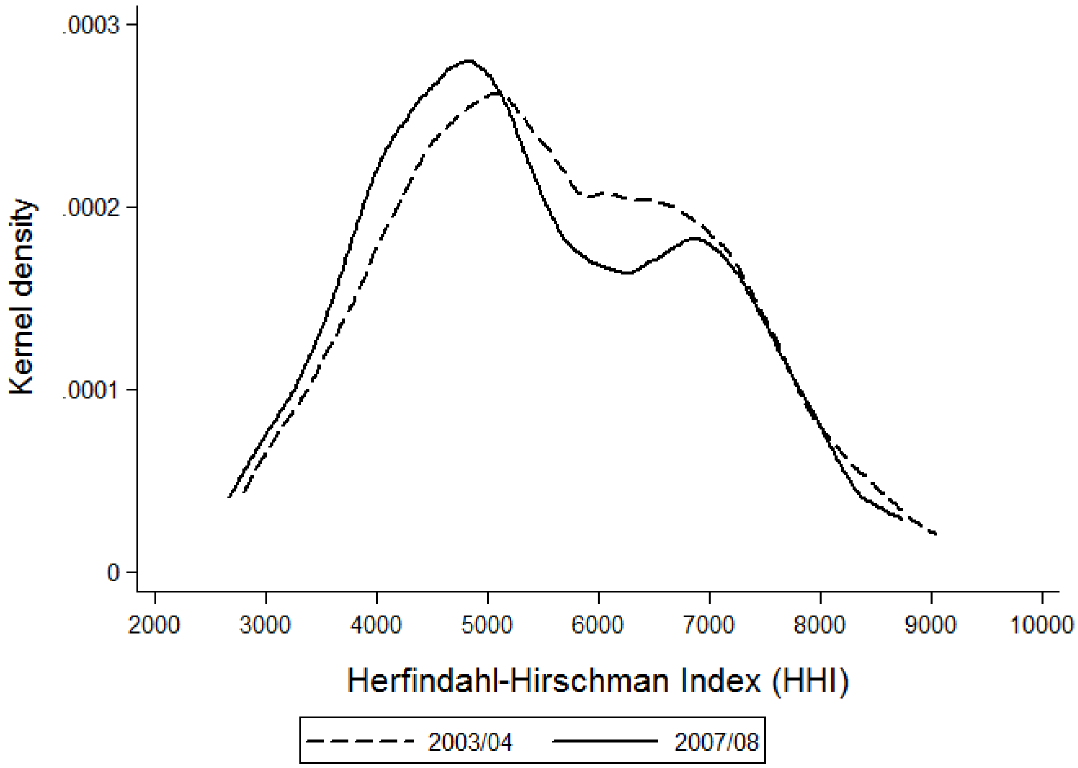


Figure 1b: All-cause mortality vs. market structure pre- and post-reform



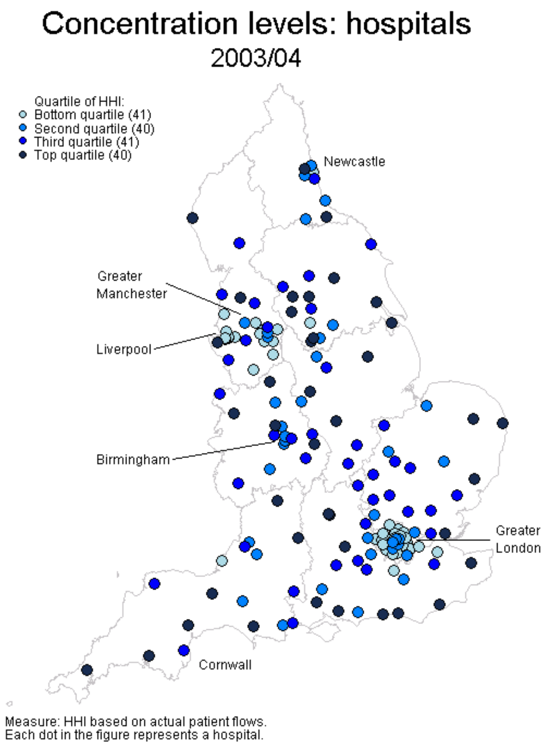
Notes: Each point in the figure represents a hospital. HHI is for all elective services calculated using actual patient flows. AMI mortality rate is in-hospital deaths within 28 days of emergency admission for over 55 year olds. The line is the prediction from a locally weighted regression of the mortality rate adjusted for case mix (using the shares of admissions within 5 year age-gender bands) on HHI.

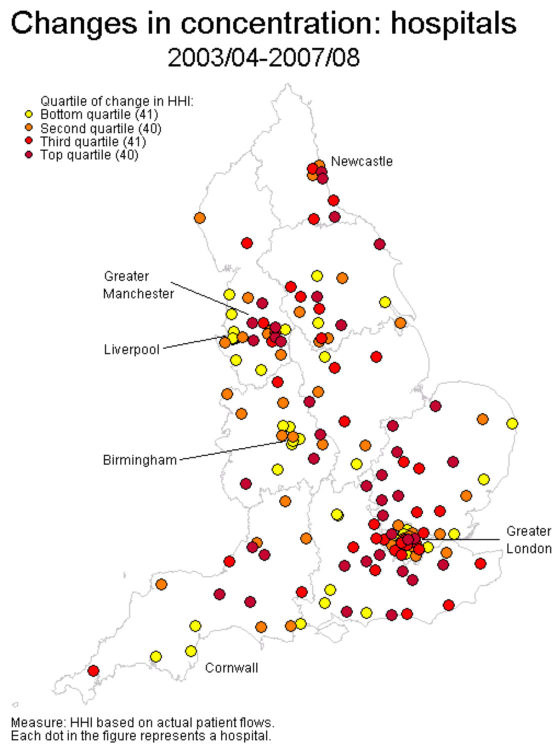
Figure 2: Kernel density estimates for the distribution of HHI (all elective services)



Number of hospitals: 162 (2003/04); 162 (2007/08).
Market definition method: actual patient flows.

Figure 3: The location of, and changes in, market concentration (2003/04-2007/08)





Appendix A

Table A1: Data sources

Variable	Source
28 day AMI mortality rate (in-hospital, ages 55+)	Hospital Episode Statistics (HES)
30 day AMI mortality rate (on or after discharge, ages 35-74)	Office for National Statistics (ONS) and Hospital Episode Statistics (HES)
28 day all-cause mortality rate (in-hospital, all ages)	Hospital Episode Statistics (HES)
28 day all-cause mortality rate excluding AMI (in-hospital, all ages)	Hospital Episode Statistics (HES)
MRSA bacteraemia rate	Health Protection Agency
Patients waiting 3 months or more	Department of Health: Performance Data and Statistics
Attendances spending less than 4 hours in A&E	Department of Health: Performance Data and Statistics
Herfindahl-Hirschman index (all elective services)	Authors' own calculations using admissions data from Hospital Episode Statistics (HES)
Total, elective and non-elective admissions	Hospital Episode Statistics (HES)
Total AMI admissions (ages 55+)	Hospital Episode Statistics (HES)
Mean length-of-stay	Hospital Episode Statistics (HES)
Clinical staff (whole time equivalents)	NHS Information Centre; Department of Health: NHS and Social Services Workforce Statistics
Retained surplus or deficit	Department of Health: Trust financial returns; Published Income and Expenditure Accounts for Foundation Trusts
Operating expenditure	Department of Health: Trust financial returns; Published Income and Expenditure Accounts for Foundation Trusts
Age-gender distribution of admissions within 5 year age-gender bands	Hospital Episode Statistics (HES)
Age-gender standardized mortality rate at the local authority level	NHS: National Centre for Health Outcomes Development (NCHOD)
Charlson index	Authors' own calculations based on data from Hospital Episode Statistics (HES)
Index of Multiple Deprivation (average over patients' rankings)	Hospital Episode Statistics (HES)
Full time male wages at the local authority level	Office for National Statistics: Annual Survey of Hours and Earnings (ASHE)
Urgent and life-threatening (category A) ambulance calls responded within eight minutes	Care Quality Commission (CQC)

Table A2: Sample selection and exit probabilities

(a) Sample selection

Year	(1) Active acute hospitals	(2) Hospitals with at least 5,000 total admissions	(3) Hospitals with non-missing HHI and mortality data	(4) Hospitals with at least 150 AMI admissions per year
2003	180	170	162	130
2007	175	167	162	121

Notes: The table reports the number of hospitals in our sample in each year (2003 and 2007) under different restrictions on the set of all English NHS hospitals. Each column puts a further restriction on the sample compared to the column before it, so column (4) is a strict sub-sample of (3) and so on. Column (1) presents the total number of active acute hospitals in each year. Column (2) refers to the number of hospitals with at least 5,000 admissions in each year. Column (3) reports the number of hospitals for which the Herfindahl-Hirschman (HHI) concentration indices could be calculated, and for which mortality data (all causes) were available. Thus column (3) corresponds to the full sample used in our main difference-in-differences model estimations. Column (4) presents the sub-sample used in the AMI mortality rate regression models.

(b) Exit probabilities: Marginal effects of the level of HHI and changes in HHI on the probability of exiting the sample

Dependent variable	(1) Level of HHI in 2003/04	(2) Change in HHI between 2003/04-2007/08
Indicator for leaving the final estimating sample	-0.105***	-0.003
	(0.038)	(0.015)
Number of hospitals	175	167

Notes: The table reports the marginal effects of probit estimates where the dependent variable is a dummy equal to unity if the hospital is in our estimation sample (column (3) in panel *(a)* above). The sample is the set of all active acute hospitals (column (1) in panel *(a)*) in 2003. Standard errors (in parentheses under marginal effects) are robust to arbitrary heteroskedasticity. The regressors are the log of the hospital's Herfindahl-Hirschman index (HHI) for all elective services in 2003 (column (1)), and the change in the HHI (in 1,000s) calculated as the difference between HHI levels for the hospital in 2007 and 2003 (column (2)). Market structure measured by the HHI based on predicted patient flows as this is the primary measure in the DiD analyses.

Table A3: Full difference-in-differences estimates of the impact of market structure on the 28 day AMI mortality rate (in-hospital, ages 55+)

	(1)	(2)	(3)	(4)	(5)
DiD coefficient	0.113	0.160*	0.196**	0.240***	0.246***
	(0.080)	(0.087)	(0.079)	(0.082)	(0.084)
Year 2007	-1.220*	-1.724**	-2.081***	-2.547***	-2.648***
	(0.688)	(0.746)	(0.673)	(0.720)	(0.778)
HHI	-0.425	-0.610	-0.721	-1.036**	-1.051**
	(0.404)	(0.464)	(0.440)	(0.474)	(0.484)
AMI admissions			-0.367***	-0.342***	-0.342***
			(0.124)	(0.126)	(0.126)
Doctors (share of clinical staff)				0.467	0.468
				(0.343)	(0.345)
Qualified clinical staff (share of clinical staff)				1.562**	1.568**
				(0.615)	(0.617)
Area standardized mortality rate (SMR)					-0.472
					(1.261)
Case mix controls (36) (p-value)		0.014	0.015	0.006	0.007
AMI admissions (p-value)			0.004	0.008	0.008
Staff controls (p-value)				0.020	0.020
Area SMR (p-value)					0.709
Adjusted R-squared	0.391	0.432	0.482	0.507	0.502
Number of hospitals	133	133	133	133	133
Observations	251	251	251	250	250

Notes: Time period is years 2003 and 2007. Coefficients are for difference-in-differences (DiD) models estimated by OLS with standard errors (in parentheses under coefficients) robust to arbitrary heteroskedasticity. The AMI mortality rate refers to in-hospital deaths within 28 days of emergency admission for acute myocardial infarction for over 55 year olds. Herfindahl-Hirschman index (HHI) for all elective services calculated using predicted patient flows. In addition to HHI in the respective year and the year 2007 dummy, the controls are 36 case mix variables corresponding to shares of AMI admissions within 5 year age-gender bands, the total number of AMI admissions (ages 55+), doctors and qualified clinical staff as shares of whole time equivalent clinical staff (staff controls), and the area age-gender standardized mortality rate (SMR, an inverse distance-weighted average rate specific to the hospital) normalized with mean 100 and standard deviation 10. Dependent and independent variables (except case mix) are in logs. All models also include a constant and a full set of hospital dummies. The estimation sample includes only hospitals with at least 150 AMI admissions. P-values refer to two-tailed t-tests of significance of the corresponding variable or joint Wald tests of significance of the group of variables. * Significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix B: Estimation of predicted HHIs

Assigning hospital market competitiveness based on which hospital patients *actually* attended - rather than, for example, their area of residence - can induce a correlation between competitiveness and unobservable determinants of outcomes, because patients' hospital of admission may depend on unobserved determinants of their hospital's quality and their own health status. We therefore follow Kessler and McClellan (2000) and Gowrisankaran and Town (2003) in assigning a level of market competition to a hospital based on predicted patient flows from neighborhoods to hospitals. Hospitals are assigned the predicted level of market competition based on the neighborhoods from which they draw their patients.

To do this, we estimate a logit model for patient choice. Having estimated these models, *predicted HHIs* at the hospital level are then computed as functions of the patient level predicted probabilities. First, neighborhood level predicted HHIs are computed as the sum of squared (predicted) shares of patients from the neighborhood attending each hospital and second, the hospital level predicted HHI is calculated as a weighted average across these neighborhood HHIs, where the weights are the predicted proportions of the hospital's patients from each neighborhood. The neighborhood is defined as an MSOA (middle layer super output area).[60]

The details are as follows.

Estimated HHIs

The probability π_{ij} that patient i chooses hospital j is given by:

$$\pi_{ij} = \Pr\left(y_{ij} = 1\right) = \frac{\exp\left(\beta_1 d_{ij}\right)}{\sum_{j=1}^{J_i} \exp\left(\beta_1 d_{ij}\right)}$$

The log-likelihood function is:

$$\log L = \sum_{i=1}^{n} \sum_{j=1}^{J} \log\left(\pi_{ij}\right)$$

The predicted HHI for patient i is the sum of their squared probabilities:

$$H\hat{H}I_i = \sum_{j=1}^{J} \hat{\pi}_{ij}^2$$

Following Kessler and McClellan (2000) we compute the predicted HHI for hospital j as the weighted average across neighborhood level predicted HHIs where the weights equal the predicted proportions of patients from hospital j that live in neighborhood k.

[60] There are approximately 7,000 MSOAs in England each containing approximately 7,200 people, so they are similar in size if not a little smaller than a US zipcode. MSOAs are constructed to have maximum within MSOA homogeneity of population characteristics.

$$H\hat{H}I_j = \sum_{k=1}^{K} \left(\frac{\hat{n}_{kj}}{\hat{n}_j} \right) H\hat{H}I_k, \qquad H\hat{H}I_k = \sum_{j=1}^{J} \left(\frac{\hat{n}_{jk}}{\hat{n}_k} \right)^2$$

$$\hat{n}_j = \sum_{i=1}^{n} \hat{\pi}_{ij}, \qquad \hat{n}_k = \sum_{i=1}^{n_k} \sum_{j=1}^{J} \hat{\pi}_{ij} = \sum_{i=1}^{n_k} 1 = n_k, \qquad \hat{n}_{kj} = \hat{n}_{jk} = \sum_{i=1}^{n_k} \hat{\pi}_{ij}$$

The predicted HHI for neighborhood k is the sum of the squared shares of patients from neighborhood k who attend each hospital j.[61]

Specification of the utility function

We estimate alternative specific conditional logit models using the following specification of the patient utility function:

$$\begin{aligned} U_{ij} = & \sum_{h=1}^{2} \left\{ \beta_1^h \left(d_{ij} - d_{ij^+}^h \right) \times z_j^h + \beta_2^h \left(d_{ij} - d_{ij^+}^h \right) \times \left(1 - z_j^h \right) \right\} \\ & + \sum_{h=1}^{2} \left\{ \beta_3^h \left(d_{ij} - d_{ij^-}^h \right) \times z_j^h + \beta_4^h \left(d_{ij} - d_{ij^-}^h \right) \times \left(1 - z_j^h \right) \right\} \\ & + \sum_{h=1}^{2} \left\{ \begin{array}{l} \beta_5^h female_i \times z_j^h \\ + \beta_6^h young_i \times z_j^h + \beta_7^h old_i \times z_j^h \\ + \beta_8^h lowseverity_i \times z_j^h + \beta_9^h highseverity_i \times z_j^h \end{array} \right\} + e_{ij} \end{aligned}$$

where z_j^1 is a binary indicator of whether hospital j is a teaching hospital, z_j^2 is a binary indicator of whether hospital j is a big hospital (defined as being in the top 50% of the distribution of admissions), d_{ij} is the distance from the geographic centre of the neighborhood (the MSOA) for patient i to the geographic centre of the neighborhood (the MSOA) for hospital j, $d_{ij} - d_{ij^+}^h$ is the additional distance from patient i to the alternative under examination j over and above the distance to the nearest alternative j^+ which is a good substitute in terms of hospital characteristic h, $female_i$ indicates gender, $young_i$ and old_i are binary indicators of whether patient i is below 60 years old or above 75 years old respectively, and $lowseverity_i$ and $highseverity_i$ are binary indicators of whether patient i has one ICD diagnosis or

[61] The predicted HHI for hospital j can be calculated in different ways. Gowrisankaran and Town (2003) compute the predicted HHI for hospital j as the weighted average across patient level predicted HHIs where the weights are equal to the predicted probability that they attend hospital j, $H\hat{H}I_j = \frac{1}{\hat{n}_j} \sum_{i=1}^{n} \hat{\pi}_{ij} H\hat{H}I_i; \qquad \hat{n}_j = \sum_{i=1}^{n} \hat{\pi}_{ij}$.
When each patient lives in their own neighborhood, our approach will give the same predicted hospital level HHIs as Gowrisankaran and Town (2003). However, the larger the geographic scale of the neighborhoods, the more the HHIs based on this approach will differ from those based on the Gowrisankaran and Town (2003) approach.

three or more ICD diagnosis respectively. All patient level variables are interacted with the variables z_j^1 and z_j^2.[62]

Following Kessler and McClellan (2000), no individual or hospital level variables are entered as main effects and as Kessler and McClellan (2000) and Gowrisankaran and Town (2003), we explicitly omit hospital level fixed effects to prevent predicted choice being based on unobserved attributes of quality. The error term, e_{ij}, is assumed i.i.d, Type I extreme value and captures the effects of unobservable attributes on patient choice.

The model is estimated for years 2003 and 2007, and undertaken separately for each of the nine Government Office Regions of England, thus allowing parameter estimates to be region-specific.[63]

Products
The sample of admissions is all elective admissions.

Sample of hospitals
We restrict our analysis to those hospitals which have 50 or more elective admissions. Hospitals with fewer admissions are dropped from the sample as are the patients who attend these hospitals.[64]

Travel distance
We restrict the distance travelled to be 100km, subject to ensuring that each patient's choice set includes the hospital actually attended and the first and second nearest hospital with each binary characteristic switched on and off.

To see why choice of both the first and second hospital is included, the following alternatives are included in all patients' choice sets, irrespective of distance: the hospital actually chosen, the nearest non teaching hospital ($z^1 = 0$), the nearest teaching hospital ($z^1 = 1$), the nearest small hospital ($z^2 = 0$) and the nearest big hospital ($z^2 = 1$).

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62 For example, consider the teaching hospital dimension $h = 1$ and suppose that the hospital under examination is a non-teaching hospital $z_j^1 = 0$, then the differential distance $d_{ij} - d_{ij^+}^1$ is the distance to the hospital under examination over and above the distance to the nearest hospital which is also a non-teaching hospital.

63 To make the model computation more efficient, we collapse patients who are identical in terms of model characteristics (i.e. who live in the same MSOA and go to the same hospital and have the same patient level characteristics) into groups. All patients within the group have the same choice set. Similarly, all patients within the group also have the same distances to each hospital within the choice set as distances are measured from MSOA centroids to hospital locations. Frequency weights are used in the estimation to reflect the number of patients within each group.

64 It is possible for some alternatives within patients' choice sets to be never chosen. This is likely to happen since hospitals located outside the region under investigation will be included in the choice set of those patients living close to the boundary, even if no patients from the region under investigation go to that hospital. These faraway hospitals should not cause any problems with the statistical identification of the model parameters. This is because, unlike standard alternative-specific conditional logit models, our model does not include any hospital-specific intercepts.

If the hospital under examination is, for example, the nearest hospital for which $z^1 = 0$, then the nearest alternative which is a good substitute will actually be the second nearest hospital where $z^1 = 0$ and so the differential distance is negative. To compute the value of this differential distance, we must also ensure that we include the second nearest hospital for which $z^1 = 0$ in patient's choice sets. The same argument can be made when the hospital under examination is the nearest hospital that has each of the other hospital characteristics (i.e. $z^1 = 1$, $z^2 = 0$, $z^2 = 1$). Thus, the following alternatives must also be included in all patients' choice sets, even if they are beyond the cut-off distance: the second nearest non teaching hospital ($z^1 = 0$), the second nearest teaching hospital ($z^1 = 1$), the second nearest small hospital ($z^2 = 0$), the second nearest big hospital ($z^2 = 1$).

Where patients actually travel further than 100km, we extend their choice set to additionally include the actual hospital attended. Each patient will thus always have at least four to nine alternatives within their choice set.

Model fit
The proportion of correct predictions is around 75%.[65] The results are robust to a range of model specifications including: (1) whether we allow model parameters to be region-specific; (2) the extent to which we expand patients' choice sets beyond the minimum set of hospitals required to estimate the model; and (3) whether we enter distance variables as linear or non-linear variables. Hospital HHIs based on predicted data are lower in value than HHIs based on actual data. The most important coefficient estimates are for distance, so that if patients were allocated to hospitals solely on a distance basis then hospitals would appear more competitive than they actually are. Actual choice of hospital is therefore based on additional factors that we have excluded from the model and these additional factors lead hospitals to become less competitive than they would otherwise be given geographical location.

[65] Parameter estimates available from the authors.