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Death by Market Power: Reform, Competition, and Patient Outcomes in the National Health Service[†]

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The effect of competition on the quality of health care remains a contested issue. Most empirical estimates rely on inference from nonexperimental 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. 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. We find that the effect of competition is to save lives without raising costs. (*JEL* H51, I11, I18, L32, L33)

Health care is one of the most important industries in developed countries, both because of its size and impact on well-being. Historically, health care has been provided through centralized, nonmarket 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 United Kingdom, the Netherlands, Belgium, Israel, and Australia, despite a lack of strong evidence on the effects of market reforms in health care. In the United States, 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.[1] These developments raise questions as to whether pro-market reforms are an appropriate way of improving outcomes in health care and in particular, because of the importance

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[1] For example, Haas-Wilson (2003), Sage, Hyman, and Greenberg (2003), Federal Trade Commission and US Department of Justice (2004), Cuellar and Gertler (2005), and Vogt and Town (2006). Critics of the use of competition include Schlesinger (2006), Rosenbaum (2006), and Jost, Dawson, and den Exter (2006).

of quality in health care, whether competition will deliver the socially optimal quality of care (Sage, Hyman, and Greenberg 2003; Federal Trade Commission and US Department of Justice 2004).

Analyzing the relationship between competition and quality has been hampered by the endogeneity of market structure. Most existing studies come from the US Medicare program and use changes in cross-sectional variation in levels of market structure over time to identify the impact of competition. But market structure may be determined by quality—for example, the existence of a high quality provider may deter entry by potential rivals.

In this paper we exploit an exogenous policy change to examine the effect of competition on health care quality. Our identification comes from a major policy shift designed to promote competition in the English National Health Service (NHS). In 2006 the NHS mandated that all patients requiring treatment be given the choice of five different hospitals and adopted a payment system in which hospitals were paid fixed, regulated prices for treating patients. 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, referring primarily to local hospitals and providing hospitals with income that was based primarily on past activity. The reform gave patients more choice (via the mandated five alternatives and the end of selective contracting), increased the incentive for hospitals to win business and moved hospitals from a market determined price environment to a regulated price environment.

However, while the pro-competitive policy was national, the intensity of the competition induced by the reforms will vary according to prereform 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, postpolicy a hospital in an unconcentrated market faces more exposure to the policy change than does a hospital in a highly concentrated market. We exploit this variation in potential competition to identify the impact of competition.

We begin by discussing how hospital managers might respond to competition in the setting of the reform we study. We are careful to show that the policy change did lead to changes in care seeking patterns that support a picture of greater responsiveness to quality postreform. We examine, as most of the rest of the literature, quality as measured by deaths following emergency admissions of acute myocardial infarction (AMI) patients, but also examine a range of other quality measures and other outcomes, including hospital utilization and expenditure. We undertake a large number of robustness tests to ensure that our identification strategy is not compromised by other factors such as changes in local demand, costs, or other policy interventions. Our results strongly indicate that the introduction of pro-competitive reforms 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. Almost all of these use US data. 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 AMI 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 1990. 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, Zwanziger, and Tomaszewski (2001) find no effect of market concentration on mortality from all causes for Medicare patients. Cutler, Huckman, and Kolstad (2010) examine the impact of entry into the market for heart bypass surgery. They find that entry led to improved quality, but that the welfare gains from increased quality are offset by the fixed costs of entry.

There are a handful of papers which examine the impact of health care competition in the United Kingdom. Propper, Burgess, and Gossage (2008) use a similar identification strategy to the present paper but study an earlier policy regime in which competition in the UK health care market was introduced in 1991 and abolished in 1997. In this regime, prices were negotiated and measures of quality very limited and not publicly available. The study found that competition reduced quality, which was not measured, but reduced waiting times, which were measured. Bloom et al. (2010) examine the impact of competition on management quality in a single year, 2006. While they have only cross-sectional data on management quality, they exploit variation in hospital closures that is driven by the political process to identify the impact of competition on management quality. They find that management quality is higher in more competitive markets and that higher management quality is associated with better outputs, including lower AMI mortality. The paper closest to ours is Cooper et al. (2011). They adopt the same identification strategy to examine the effect of the current pro-market reforms in the United Kingdom, but their focus is considerably narrower. They analyze only one outcome measure: in-hospital deaths within 30 days of admission after a heart attack. While in-hospital mortality is a very commonly used measure of the quality of care, it suffers from a potentially serious measurement problem. By construction it excludes any deaths which occur once the patient is discharged into the community or which occur in another hospital. These accounted for 19 percent of AMI deaths within 30 days of admission in England in 2007 (National Centre for Health Outcomes 2012). Regulated prices give incentives to hospitals to discharge patients "quicker and sicker" (Kosecoff et al. 1990). Thus, focusing only on in-hospital deaths misses this potentially damaging response. In contrast to Cooper et al.(2011), we use data collected by the central government to examine all deaths within 30 days of admission for heart attacks, regardless of where they occur. In addition, we examine several outcome measures on top of the single AMI measure examined in Cooper et al. (2011) to move away from a focus simply on AMI deaths. These include another measure of quality (deaths from all causes) and, as crude measures of productivity, length of stay and total expenditure per patient. We also control for a wide set of potential confounders including the important policy change of the cardiac networks of care established during this time period.

In what follows, we provide background on the NHS and the market-oriented reforms of 2006 (Section I), examine potential managerial responses to these reforms (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).

I. The NHS Reform Program

In the United Kingdom, health care is tax financed and free at the point of use. Almost all care is provided by the NHS. Primary care is provided in the community by publicly funded physicians known as general practitioners (GPs), who also act as the "gatekeeper" for hospital-based care. Secondary care is provided in publicly funded (NHS) hospitals.

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. Local health authorities (publicly funded bodies covering a geographic area) were given the task of buying hospital-based health care for their population. Hospitals were turned into free standing public organizations, known as NHS Trusts, who competed for contracts from the buyers. Both price and quality were negotiable, though information on quality was extremely limited (Propper, Burgess, and Gossage 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 United States, buyers and sellers negotiated over price, and to some degree over quality (mainly waiting times, not clinical outcomes), and volume on an annual basis, with the majority of contracts taking the form of annual bulk-purchasing contracts.

In late 2002, the government signaled a shift in policy and initiated a reform package with a set of phased-in changes leading to the reintroduction of competition from 2006 onwards.[2] There were several elements to this policy (Farrar et al. 2007; Cooper et al. 2011), the most important of which were a policy designed to increase 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 generally offered any choice over the location of their care. The hospitals to which patients were sent were determined by the selective contracting arrangements made by the local NHS body responsible for purchasing health care (the Primary

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

Care Trust, PCT). Patients had little choice of GPs and GPs in this system had no incentives to refer outside the PCT contract. After 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. Patient choice therefore signaled the end of selective contracting by encouraging movement of patients away from the local hospitals GPs had previously used. 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. From 2007 the government also introduced a website designed to provide additional quality information to help patients' choices. This included information collected by the national hospital accreditation bodies, such as 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 (http://www.chooseandbook.nhs.uk/). So patients, and more importantly their GPs, had greater information on which to make these choices.

The ex ante fixed prices are a case-based payment system known as "Payment by Results" (PbR) (Department of Health 2002a). PbR is modeled on the diagnosis-related group (DRG) payment system used by the Medicare program and many private insurers in the United States (Department of Health 2002b). A fixed price is set by the government for every procedure, with adjustments for whether a hospital is an academic medical center, patient severity, and local wage rates (Department of Health 2002b). The price is exogenous to both the seller and buyer. 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 and nonelective 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, nonelective and outpatient care (Farrar et al. 2007). 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 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 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 were also 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 standing and the reduction in waiting times for elective care. Granting of FT status is undertaken by an independent regulator.

Finally, alongside the extension of choice, the government also ought to increase the role for the private sector in delivery of care. It was required that one of the five choices was a private provider. To facilitate this, the government placed contracts with a small number—around 15 initially, rising to around 30 by 2008—of private sector providers of NHS care (known as Independent Sector Treatment Centres, ISTCs) that specialized in a limited range of elective procedures for which NHS waiting times were long (e.g., hip replacements and cataract removal). However, while initially it was believed that their private provision would be important, in practice this policy turned out to be very limited. Neither the existing small private sector nor the new capacity were heavily used and even by 2008 ISTCs accounted for less than 1 percent of all hospital activity.

II. Expected Hospital Responses to the NHS Reforms

In this section, we explore how NHS hospitals might have responded to these changes. Managers of NHS hospitals had incentives to respond to increased competition. While NHS hospitals are public organizations, the regime they operate under gives 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.[3] 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 can be (and are) replaced, while hospitals which perform well can get the greater autonomy from FT status.[4] This is valuable to managers as it gives them greater freedoms: for example, hospitals with FT status can retain surpluses. Managers therefore had strong reasons to care about the gap between the revenues of their hospital and their costs.

This gap was affected by PbR and by competition. PbR is a very highly geared reimbursement system. In 2006, over 60 percent of hospital income came from PbR payments (Department of Health 2007) and was projected to rise to about 90 percent 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. The high level of unionization in the NHS means that both staff and nonstaff costs are relatively fixed within a year. But the reforms changed the certainty of annual revenue streams. Between 1997 and 2005, the use of annual contracts meant that annual revenues were known at the beginning of the year. Post-PbR revenues were more uncertain, as hospitals were no longer guaranteed volume at the start of each year. Supply was also increased, as Choose and Book opened hospitals up to competition from outside their local catchment area and potentially also to

[3] See, for 2004, http://ratings2004.healthcarecommission.org.uk/.

[4] Removal of managers by mergers of acute trusts (short-term general hospitals) was common. By 2005 around half the acute trusts which existed in 1997 had been merged (Gaynor, Laudicella, and Propper 2012). At the end of 2005, 32 acute trusts (of 174) had FT status (http://www.monitor-nhsft.gov.uk/home/news-and-events/media-centre/latest-press-releases/new-applicants-nhs-foundation-trust-status-w).

competition from the private sector. Hospitals could therefore no longer simply rely on capturing the whole of local demand, but had to compete to attempt to retain it, as well as having the opportunity to attract nonlocal patients.

Moreover, Choose and Book, by providing patients with greater choice and information, should have increased the elasticity of demand facing hospitals thereby leading to tougher competition. While increasing choice for patients might have little impact where patients have to make choices unassisted, the program was implemented by GPs. In addition to being mandatory, these physicians received financial payments for the extra costs of implementing the system. Thus they had no reason not to offer their patients choice other than their professional judgment. 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 percent of patients recalled being offered a choice of hospital (Dixon 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.

How might NHS hospital managers have responded to these changes? Standard models of hospital nonprice competition with administered prices (see Gaynor 2006; Gaynor and Town 2012) predict that competition is tougher with more hospitals, conditional on price being set above marginal cost, and that leads to higher quality. These results apply to hospitals regardless of their ownership status (not-for-profit, public, for-profit). In the NHS system, managers are not profit maximizers. But the managerial labor market within the NHS means they do care about retaining their jobs and also about achieving FT status. To meet either of these goals, they must perform well financially. Therefore attracting patients becomes very important. Given that price is fixed, the only way managers can do this is by undertaking effort to increase quality. The reforms made attracting patients tougher in less concentrated markets and so managers had greater incentives to improve quality in these markets. Although the set up is not one of profit maximization, the intuition is similar to a standard competition model—with higher competition the stakes are greater from changes in relative quality. A small change in managerial effort is likely to lead to a greater change of demand when there are many hospitals relative to when there is monopoly. This increases managerial incentives to improve quality/effort as competition grows stronger.[5]

In one respect, this may differ from a standard set up where hospitals compete for patients. In that setting managers will compete to attract profitable patients, those whose costs are below the regulated price. In the NHS, where managers have less knowledge about costs (because services have not been subject to regulated prices in the past) their first response might be simply to attract all patients by increasing quality across the board, subject to not making a loss at total hospital level. Getting more patients is a way of assuring demand and avoiding financial loss in an environment which has become more uncertain. Worrying about avoiding unprofitable patients may be a secondary response, which will only develop later as the data to do so becomes available. While our focus here is on the managerial response to the reforms of the

[5] We present a model which formally derives this result in an Appendix available online.

NHS, we note that in general risk is important for managers of not-for-profits and demand assurance is probably something that all managers try to do, although their precise behavior will depend on the information and tools at their disposal.

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 introduction of competition. This is a difference-in-differences (DiD) approach. The simplest DiD strategy compares two groups over two time periods, where a treatment group is exposed 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 geographic 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, postpolicy 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 treatment intensity variable, the degree of market concentration prepolicy, with a dummy for the postreform year.[6] To avoid contamination from earlier policy changes introduced by the Labour administration such as waiting times targets (Propper et al. 2010), we focus on a short window around the implementation of the policy. We use data from 2003 to capture the period before the policy change and data from 2007 for the period after the policy change. This gives the DiD regression specification:

$$(1) \quad z_{it} = \beta_0 + \beta_1 I(t=2007) + \beta_2 I(t = 2007) \times HHI_{i,2003} + \beta_3 \mathbf{X_{it}} + \mu_i + \xi_{it},$$

where z_{it} is the outcome variable, quality of care at hospital i at time t. $I(\cdot)$ is an indicator function for the postreform period, which takes the value 1 for financial year 2007 and 0 otherwise. $HHI_{i,2003}$ is the Herfindahl-Hirschman index prepolicy, our measure of market structure. $\mathbf{X_{it}}$ is a vector of observed hospital characteristics which vary over time,[7] μ_i is an unobserved hospital fixed effect (which includes the level of the prepolicy market structure), ξ_{it} is random noise and t takes two values, financial year 2003 and financial year 2007.

[6] See Card (1992) for an early use of a continuous treatment variable to estimate the impact of a policy and Angrist and Pischke (2008).

[7] In principle, $\mathbf{X}_i$ would contain the hospital's regulated price, marginal costs, etc. However, we do not have data on prices or marginal costs. Further, in practice we have only one year of data under the regulated price regime, which means we cannot estimate a parameter for price in the DiD framework. However, we do have a cost shifter, the "Market Forces Factor," which we employ in regressions that augment the main specification. See Section VC.

The DiD coefficient is β_2, which measures the change in the effect of market structure prereform and postreform. Any common macro changes are picked up by the time dummy. 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. We explicitly test this assumption in robustness tests in Section V.

Equation (1) uses the HHI as a measure of market structure. In practice we do not use the actual HHI for two reasons. The first is that endogeneity is a common concern in estimating regression models 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 prepolicy 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. In addition, we include in the $\mathbf{X_{it}}$ vector controls for observable time-varying measures of the health of the patients admitted to the hospital and, in robustness tests, the health of the population in the catchment area of the hospital, as well as measures of local income, to control for patient health or income effects on demand. Nevertheless, endogeneity concerns may remain.

Second, in our context, a measure of actual market shares may be less appropriate than a measure that accounts for the number of options that a patient has. The impact of Choose and Book will depend on how easy or difficult it is for patients to go to alternative hospitals. It is well known that travel distance is a major determinant of where patients go to the hospital (e.g., Capps, Dranove, and Satterthwaite 2003; Gaynor and Vogt 2003; Ho 2006, 2009). The more appropriate measure therefore should account for the costs (the travel distance) of choosing among alternative hospitals.

For these two reasons, we instrument actual 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 2003 HHI in (1) with the predicted HHI. The latter 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 Section IVB and in the online Appendix. Finally, in robustness tests in Section V we replace the predicted HHI with a very simple indicator of choice, splitting the sample into those hospitals which are basically monopolies versus the rest.

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. 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. We focus here on a (large) subset of these hospitals—short term general hospitals (called acute hospitals in the United Kingdom). These are the dominant suppliers of hospital-based services. Our data are derived from a large number of administrative data sources that we discuss briefly here and are presented in detail in the online Appendix Table A1. Our sample selection criteria and the impact of selection on sample size are laid out in Table A2 in the online 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 selection rule is to select hospitals with at least 5,000 total admissions to ensure we are examining nonspecialist hospitals. 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, totaling 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.

A. *Measures of Hospital Quality*

We use mortality rates both within the hospital and including deaths posthospital discharge as indicators of quality. These 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 use deaths following AMI and following all admissions. For AMI, we use hospital-level annual deaths within 30 days in any location for patients aged 35–74 years old. These data are constructed by the organization which develops NHS quality indicators (National Centre for Health Outcomes Development, NCHOD). For all-cause mortality, which is not published by NCHOD, we construct annual data at the hospital level for 28-day in-hospital mortality rates for all admissions from HES.

Deaths following emergency admission for AMI have been published by both the US and UK governments as indicators of hospital quality. There is little or no scope for competition for emergency admissions of AMI patients, who in the United Kingdom are taken to the nearest hospital with capacity. But AMI mortality is treated as an indicator of overall quality in the hospital for a number of reasons. First, AMI patients are among the sickest and most influenced by quality of care. Thus, if hospital managers respond to weaker competition by exerting less effort, and this harms the quality of care, the reduced quality of care is likely to manifest itself as increased mortality among AMI patients as well. Second, 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. Third, as all patients with a recognized AMI are admitted, there is little scope for selection bias with regard to admission. Fourth, the use of within-30-day mortality in any location allows us to examine whether hospitals respond to competition by discharging patients in a poorer health state. These data are constructed by linking information on deaths following discharge to the admitting hospital (Health and Social Care Information Centre 2010). We also use the all-cause in-hospital mortality rate, as studies have found declines in overall hospital mortality linked to clinical and managerial quality improvement programs and to variability in hospitals' performance across a number of conditions (Wright and Shojania 2009; Jha et al. 2005). In addition to these measures of quality, we also examine the impact of competition on other aspects of performance. These are the length of stay, the total number and mix of admissions, total expenditure and a simple measure of (lower) productivity, expenditure per admission.

Table 1 presents the means, standard deviations, minima and maxima for all the variables used in our main regressions and our robustness tests (Table A3 in the online Appendix presents the within and between variation).

The average hospital in our estimation sample admits just under 68,000 patients and has 412 emergency AMI admissions a year. 6.9 percent of AMI patients aged 35–74 die within 30 days in either the hospital or the community. 1.6 percent 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 30–40 percent of the variation in the within-30-day AMI death rate and the all-cause mortality rate is within hospitals.

B. *Measures of Hospital Market Structure*

We measure market structure at the hospital level using an HHI based on patient flows to each hospital. The HHI is built up from patient flows at the 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. The neighborhood definition we use (the MSOA) contains on average around 7,000 people and so is similar, or smaller, in population, to a US zipcode.[8] 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 admissions and patients' locations in the HES dataset. We calculate these for both 2003 and 2007. We refer to these as *actual* HHIs.

[8] MSOAs (Middle Layer Super Output Areas) are defined to ensure maximum homogeneity of population type. In England, each of the 6,780 MSOAs has a minimum population of 5,000 residents and had an average population of 7,200 residents in 2010. For more information see http://neighborhood.statistics.gov.uk/dissemination/Info.do?page=userguide/moreaboutareas/furtherareas/further-areas.htm.

Table 1—Descriptive Statistics

Variable	Mean	Standard deviation	Minimum	Maximum	Observations
Panel A. Death rates					
30 day AMI mortality rate (on or after discharge, ages 35–74, percent)	6.9	2.6	1.8	22.8	$N = 251$ $n = 133$
28 day all-cause mortality rate (in-hospital, all ages, percent)	1.6	0.6	0.0	3.3	$N = 324$ $n = 162$
28 day all-cause mortality rate (in-hospital, excluding AMI all ages, percent)	1.6	0.6	0.0	3.2	$N = 324$ $n = 162$
Panel B. Market concentration					
Herfindahl-Hirschman index (actual patient flows)	5,543	1,410	2,674	9,050	$N = 324$ $n = 162$
Herfindahl-Hirschman index (predicted patient flows)	4,308	1,931	1,878	9,550	$N = 324$ $n = 162$
Panel C. Length of stay and admissions					
Mean length of stay (days)	1.2	0.8	0.3	7.1	$N = 324$ $n = 162$
Total admissions	67,896	35,929	8,792	206,633	$N = 324$ $n = 162$
Total AMI admissions (all ages)	412	198	153	1,275	$N = 262$ $n = 135$
Elective admissions (number)	35,135	20,109	3,882	116,471	$N = 324$ $n = 162$
Elective admissions (percent of total)	52.4	12.2	24.4	98.4	$N = 324$ $n = 162$
Panel D. Finances and prices					
Operating expenditure (£1,000)	197,082	125,368	18,881	766,137	$N = 319$ $n = 162$
Total expenditure per admission (£1,000)	3.0	1.3	0.2	9.9	$N = 319$ $n = 162$
Market forces factor	1.00	0.07	0.89	1.28	$N = 324$ $n = 162$
Retained surplus (£1,000)	0.2	6.5	−40.3	56.0	$N = 303$ $n = 162$
Average male full time wage in area (£)	24,955	3,774	18,985	34,551	$N = 320$ $n = 160$
Panel E. Area health, case mix and economic conditions					
Standardized mortality rate (per 100,000, normalized)	100.0	10.0	77.6	129.5	$N = 324$ $n = 162$
Charlson index (average for all admissions)	0.48	0.23	0.03	1.85	$N = 324$ $n = 162$
Urgent ambulance calls responded within eight minutes (percent)	76.4	3.3	55.7	86.6	$N = 306$ $n = 162$
Panel F. Controls for other policy changes					
Number of ISTCs within 30 kilometers	0.9	1.9	0.0	11.0	$N = 324$ $n = 162$
AMI patients having thrombolytic treatment within 30 minutes of arrival at hospital (percent)	83.5	9.3	47.0	100.0	$N = 218$ $n = 126$
AMI patients having thrombolytic treatment within 60 minutes of calling for help (percent)	58.9	19.3	8.0	95.0	$N = 223$ $n = 127$
AMI patients having primary angioplasty (PCI) (percent)	5.1	18.4	0.0	100.0	$N = 232$ $n = 128$

(*Continued*)

Table 1—Descriptive Statistics (*Continued*)

Variable	Mean	Standard deviation	Minimum	Maximum	Observations
AMI patients discharged on aspirin (percent)	97.8	2.6	84.0	100.0	$N = 240$ $n = 129$
AMI patients discharged on beta blockers (percent)	92.8	6.5	68.0	100.0	$N = 240$ $n = 129$
AMI patients discharged on statins (percent)	95.9	3.5	81.0	100.0	$N = 240$ $n = 129$

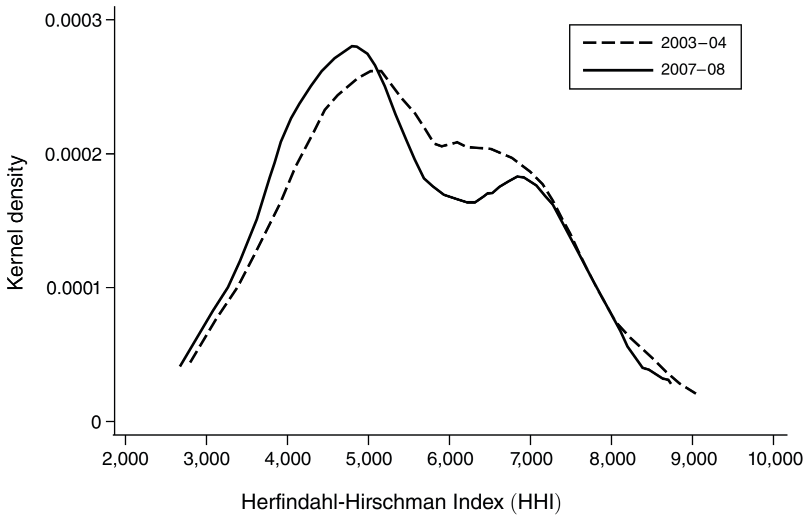
Notes: Summary statistics refer to fiscal years 2003 and 2007. N = Total number of hospital-year observations for the whole sample; n = Total number of hospitals in the sample. The samples for the AMI mortality rates include only hospitals with at least 150 AMI admissions. Herfindahl-Hirschman indices computed using all elective services. Market forces factor used by the Department of Health to adjust hospital reimbursement tariffs. 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 km from the hospital. Age-gender area standardized mortality rate (normalized) is an inverse distance weighted average rate specific to the hospital. Average Charlson index refers to all 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 five year age-gender bands.

In our analysis, as noted above, we use 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. 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 flows are used instead of actual patient flows.[9] 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. *Other Controls*

As many potential control variables may be endogenous (for example, admissions, financial position, or staffing), in our primary estimates we use a very limited number of time-varying controls. In all specifications, to allow for differences in the health of hospitals' patient mix (often referred to as a hospital's "case mix"), we include hospital fixed effects, which will pick up observed and unobserved time-invariant differences between hospitals, and 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

[9]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 percent level).



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

Figure 1. Kernel Density Estimates for the Distribution of HHI (All Elective Services)

this has been shown to do a good job of controlling for case mix (Propper and Van Reenen 2010). It is possible that there are omitted variables which account for cross-hospital heterogeneity and could be correlated with our included measure of market structure. To check for this we perform a large number of robustness tests in which we employ additional variables to control for any further heterogeneity across hospitals. The variables used in our robustness tests are described in Section V.

D. *Patterns in the Data*

We now examine some simple patterns in the data in order to see if they are consistent with responses implied if the reforms induced competition.

Did the Reforms Result in Less Concentration?—The policy introduced the potential for competition. If the policy did lead to competition, then patient admission patterns likely should have changed, leading to changed market concentration. To establish whether patient flows changed postpolicy we examine measures of market concentration prepolicy and postpolicy. Figure 1 presents the kernel density estimates 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 2 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 prepolicy (those in the bottom quartile of the HHI

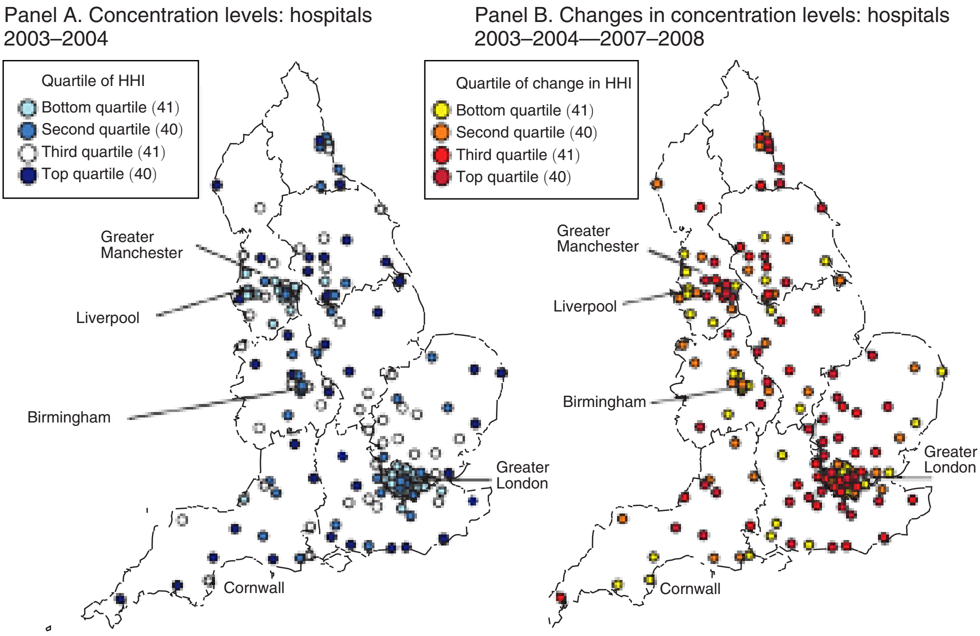


Figure 2. The Location of, and Changes in, Market Concentration (2003–2004—2007–2008)

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 2 shows that some hospitals located in the largest urban areas experienced large decreases 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 prepolicy period, and in more rural areas where market structure was more concentrated.[10]

Did Patients Respond to the Reforms?—One of the intentions of the reforms was to change the patterns of care seeking by patients.[11] If the reforms were successful we would expect to see this reflected in the data. We examine this in Table 2.

Panel A shows the change in patient care seeking postreform by hospital quality. For our measure of quality we use 2003 AMI mortality rates to reduce the likelihood

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

[11] 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.

Table 2—Descriptive Changes in Patient Care Seeking by Hospital Mortality Rate and Market Concentration

	Bottom quartile			Top quartile		
	2003	2007	Percent change (2003–2007)	2003	2007	Percent change (2003–2007)
Panel A. AMI mortality rate (2003)						
Number of elective admissions	27,787	31,882	14.7%	37,212	42,417	14.0%
Average distance traveled by patients	12.9	13.1	1.6%	10.9	11.1	1.8%
Share of patients bypassing nearest hospital	0.43	0.44	2.3%	0.43	0.40	−7.0%
Number of hospitals	38	38		37	37	
	Bottom quartile			**Top quartile**		
	2003	2007	Percent change (2003–2007)	2003	2007	Percent change (2003–2007)
Panel B. Share of patients waiting 3+ months (2003)						
Number of elective admissions	24,135	29,029	20.3%	35,284	38,028	7.8%
Average distance traveled by patients	13.5	13.8	2.2%	13.8	14.0	1.4%
Share of patients bypassing nearest hospital	0.47	0.48	2.1%	0.46	0.44	−4.3%
Number of hospitals	41	41		40	40	
	Bottom quartile			**Top quartile**		
	2003	2007	Percent change (2003–2007)	2003	2007	Percent change (2003–2007)
Panel C. HHI level (2003)						
Number of elective admissions	21,757	26,924	23.8%	55,253	61,049	10.5%
Average distance traveled by patients	8.1	8.3	2.3%	15.5	15.5	0.5%
Share of patients bypassing nearest hospital	0.45	0.46	2.2%	0.47	0.47	0.0%
Number of hospitals	41	41		40	40	

Notes: Time period is years 2003 and 2007. Elective admissions only. 30 day AMI mortality rate on or after discharge (ages 35–74) measured in 2003 for hospitals with at least 150 AMI admissions. HHI for all elective services calculated using actual patient flows measured in 2003. Sample means of variables in the rows for quartiles of AMI mortality and HHI (bottom 25 percent hospitals and top 25 percent hospitals). Average distance traveled by patients who attended the hospital in kilometers.

of simultaneous determination of mortality and patient volume. If patients became more responsive to quality postpolicy 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 elective patients than did worse hospitals. Although the distances patients traveled for care rose similarly for worse and better hospitals, the share of patients bypassing their nearest hospital increased for better hospitals while it clearly *decreased* for worse hospitals. Panel B undertakes the same exercise using waiting times rather than measures of clinical quality. We cut hospitals into those with the lowest waiting times and those with the highest (as measured by the share of patients waiting over three months in 2003). The tables show that the hospitals with the better waiting times (those in the bottom quartile) experienced a 20 percent increase in the number of elective admissions, while those with the poorest waiting times saw a rise of only 8 percent. The distances patients traveled for care rose more for better hospitals and the share of patients bypassing their nearest hospital increased for better hospitals and *decreased* for worse hospitals.

Thus, hospitals which provided better quality and had shorter waiting times saw a larger increase in patients. This is not, of course, an estimate of a demand model but it does provide reassurance that there is a patient response to quality and that it increased during the reform.[12]

Panel C of Table 2 shows changes in patient care seeking after the reform by levels of prereform market concentration. Our estimation strategy is based on potential exposure to policy change, as measured by market concentration. If the policy had an effect, we would expect hospitals with greater potential exposure—those located in markets which prepolicy were less concentrated—to experience more changes in the patterns of patient care seeking. Table 2 shows this is the case. Hospitals in markets in the bottom quartile of the HHI distribution had a greater increase in elective admissions, a greater increase in the distance traveled by their patients and a greater change in the share of patients bypassing their nearest hospital.

V. Results

The data above suggest that the introduction of pro-competition reforms gave hospitals operating in markets with lower levels of concentration incentives to improve quality. In this section we formally test the effect of the policy using equation (1). We begin by looking at the impact of market structure on quality, as measured by 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.

A. *The Impact of Market Structure on Quality*

Table 3 reports our DiD estimates of the impact of market structure on hospital quality.[13] The estimates control for year effects (a 2007 year dummy), the age-gender composition of admissions, and hospital fixed effects.

Column 1 presents estimates for the 30-day mortality rate following emergency AMI admission. Concentration has a statistically significant positive effect on mortality, i.e., higher market concentration (a larger HHI) leads to lower quality. A 10 percent increase in the HHI leads to an increase of 2.91 percent in the AMI death rate.[14]

Column 2 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 smaller than that for AMI but precisely estimated. To test whether

[12] Gaynor, Propper, and Seiler (2011) estimate a structural demand model for one elective treatment (coronary artery bypass graft). They find that the reforms increased patient sensitivity to distance, waiting times and mortality (see also Beckert, Christensen, and Collyer 2012). They calculate that the reform reduced the mortality rate from CABG surgery by 3 percent. While the estimates of structural parameters from that model are difficult to compare to the DiD estimates in this paper, both papers show significant impacts of the reform.

[13] Full estimation results are available upon request.

[14] In principle, we should take account of the fact that the predicted HHI is estimated for the standard errors. 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. As a consequence, there is little sampling variation to account for. 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. The intra-hospital correlations between the bootstrapped predicted HHIs was very high (0.998 for 2003).

Table 3—Difference-in-Differences Estimates of the Impact of Prereform Market Structure on Outcomes

Dependent variable	30-day AMI mortality rate (on or after discharge, ages 35–74) (1)	28-day all-cause mortality rate (in-hospital) (2)	28-day all-cause mortality rate (in-hospital, excluding AMI all ages) (3)
DiD coefficient	0.291**	0.099***	0.098***
	(0.115)	(0.031)	(0.031)
Case mix controls (36) (*p*-value)	0.000	0.000	0.000
Adjusted R^2	0.483	0.977	0.977
Number of hospitals	133	162	162
Observations	251	324	324

Notes: Time period is 2003 and 2007. Models estimated by OLS with standard errors (in parentheses under coefficients) robust to arbitrary heteroskedasticity. HHI measured in year 2003 for all elective services, calculated using predicted patient flows. In addition to HHI in 2003 and the year 2007 dummy, controls are 36 case mix variables corresponding to shares of cause-specific admissions within five year age-gender bands. The estimation sample for the AMI mortality rate includes only hospitals with at least 150 AMI admissions. Due to data limitations, in column 3 AMI admissions for all ages (not only 35–74) are excluded. Dependent and independent variables (except case mix) are in logs. All models also include a constant and a full set of hospital dummies. *p*-values refer to two-tailed joint Wald tests of significance of the group of variables.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

the estimated effect for all-cause mortality is driven only by AMI deaths, column 3 presents the DiD estimate for all-cause in-hospital mortality excluding deaths after AMI admissions. The coefficient is almost 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. 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.[15] 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.[16]

The coefficients are statistically significant but the estimated magnitude of the response is relatively modest. A 10 percent fall in the HHI is associated with a fall

[15] Nolte and McKee (2008) state that "amenable mortality"—deaths from causes that should not occur in the presence of timely and effective health care—accounted for around 27 percent of total mortality for males aged under 75 years in the United Kingdom in 2002–2003 and 33 percent for females. Nolte and McKee (2004) includes ischemic heart disease 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 like better management of the condition within the hospital.

[16] 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). None of these outcomes are affected by the policy change. Using the same specifications as Table 3, the coefficients (standard errors) on these estimates are −0.027 (0.108), 0.028 (0.174) and −0.002 (0.011). 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 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 little scope for further reductions.

in the 30-day death rate following AMI admissions by 2.91 percent. This amounts to one-fifth of a percentage point at the mean AMI death rate of 6.9 percent, implying a little over eight fewer AMI deaths annually per hospital, or approximately 1,000 fewer total deaths per year over all 133 hospitals in our sample.

Our estimated magnitudes are 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 2.26 percentage points. Cooper et al. (2011) 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 similar 0.21 percentage point reduction for each year postpolicy (2004 to 2007). We discuss the economic significance of our estimates at the end of this section.

B. *The Impact of Market Structure on Other Aspects of Performance*

We also examine whether the reform had an impact on resource use. In Table 4 we examine the mean length of stay of admitted patients (LOS), the total number of admissions, the number and share of elective admissions, expenditure and a simple measure of (lower) productivity, expenditure per admission.

Column 1 indicates that increases in concentration are significantly linked to a rise in the length of stay. The estimated coefficient implies that a 10 percent fall in a hospital's HHI on average results in a 2.3 percent fall in length of stay. At the mean length of stay in the sample of 1.2 days this is just under an hour. The policy does not seem to have affected 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, so we do not find evidence that resource utilization increased in less concentrated markets following the reforms. We also find no policy effect on a simple measure of labor productivity, the number of admissions per clinical staff (the coefficient (standard error) is −0.014 (0.026)).

Taken together, the findings for quality (Table 3) and resource utilization (Table 4) suggest that hospitals facing more competitive pressure were able to find ways to marshal resources more efficiently to produce better patient outcomes.

C. *Robustness Checks*

To avoid inclusion of potentially endogenous variables our estimates above control only for time-invariant factors at the hospital level and a simple measure of case mix. It is possible that there are omitted variables potentially associated with market concentration that are driving our results. To examine this we undertake a wide set of robustness tests for our AMI, all-cause mortality and length of stay results. These are presented in Table 5. All cells report the DiD estimates from separate regressions. The first row presents the baseline results from Table 3, columns 1 and 2, and Table 4, column 1. Further tests are in the online Appendix, Table A4.

Table 4—Difference-in-Differences Estimates of the Impact of Prereform Market Structure on Length of Stay, Admissions and Expenditure

	Length of stay and admissions				Expenditure and spending per admission	
	Mean length of stay (days)	Total admissions (number)	Elective admissions (number)	Elective admissions (share of total)	Operating expenditure (£1,000)	Operating expenditure (£1,000) per admission
	(1)	(2)	(3)	(4)	(5)	(6)
DiD coefficient	0.230***	−0.019	−0.012	0.006	−0.043	−0.029
	(0.057)	(0.030)	(0.036)	(0.016)	(0.068)	(0.071)
Case mix controls (36) (p-value)	0.000	0.000	0.000	0.000	0.879	0.011
Adjusted R^2	0.871	0.976	0.966	0.942	0.898	0.715
Number of hospitals	162	162	162	162	162	162
Observations	324	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 measured in year 2003 for all elective services, calculated using predicted patient flows. In addition to HHI in 2003 and the year 2007 dummy, controls are 36 case mix variables corresponding to shares of admissions within five year age-gender bands. Expenditure in columns 5–6 excludes capital expenditure. Dependent and independent variables (except case mix) are in logs. All models also include a constant and a full set of hospital dummies. p-values refer to two-tailed joint Wald tests of significance of the group of variables.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Specification of the DiD Estimator and Placebo Tests.—One important concern is that there may be prepolicy trends which are driving the results. To avoid such contamination, up to this point we have focused on a narrow window around the introduction of the competition reforms. We now include additional years in order to examine the sensitivity of the estimated relationship. Figure 3 presents the raw relationship over a longer time period between prereform market structure and the three outcomes for which we find significant effects.

Four time series lines are presented for the median of each outcome, one for hospitals in each quartile of the (predicted) prepolicy concentration distribution. The pro-competition policy was fully rolled out in January 2006, which is three-quarters of the way through financial year 2005–2006 (labeled 2005 on the figure). All three panels show that outcomes were poorer and improved before the policy was announced (late 2002). AMI mortality, all-cause mortality, and length of stay all decline (although with fluctuations) over the entire time period. AMI deaths (panel A) show some evidence of a steeper relative drop post 2005 than between 2003 and 2005 in hospitals in the lower two quartiles of the HHI distribution, although with quite a bit of noise. Panel B shows a drop in all-cause death rates in the lowest quartile (relative to the top quartile) and panel C shows a sharper fall in length of stay in the least concentrated markets post 2005. But, as can be seen, all three series are rather noisy and it is difficult to draw any strong conclusions from these patterns in the raw data. We therefore turn to formal statistical inference.

We begin by modifying equation (1) to include all observations for all years between 2003 and 2007, and include a linear time trend and an HHI-time trend interaction. This last variable picks up the reform effect after allowing for a linear

Table 5—Robustness Tests: Difference-in-Differences Estimates

Robustness test	30-day AMI mortality rate (on or after discharge, ages 35–74) (1)	28-day all-cause mortality rate (in-hospital) (2)	Mean length of stay (days) (3)
1. Baseline	0.291**	0.099***	0.230***
	(0.115)	(0.031)	(0.057)
Observations	251	324	324
Specification of the DiD estimator and placebo test			
2. Linear trend and linear trend × HHI 2003	0.052**	0.021***	0.051***
	(0.020)	(0.005)	(0.010)
Observations	619	810	810
3. Year dummies × HHI 2003			
2004 × HHI	0.014	0.025	0.024
	(0.079)	(0.019)	(0.040)
2005 × HHI	0.049	0.081***	0.052
	(0.084)	(0.026)	(0.040)
2006 × HHI	0.152*	0.077***	0.123***
	(0.088)	(0.025)	(0.042)
2007 × HHI	0.196**	0.080***	0.190***
	(0.089)	(0.023)	(0.044)
Observations	619	810	810
4. Placebo DiD test for 1999–2003	0.029	0.025	−0.158
	(0.150)	(0.030)	(0.099)
Observations	238	310	295
5. Indicator for near monopoly (top quartile of predicted HHI in 2003)	0.258***	0.079***	0.103
	(0.091)	(0.028)	(0.063)
Observations	251	324	324
6. Excluding London	0.284**	0.065*	0.206**
	(0.117)	(0.037)	(0.082)
Observations	230	268	268
Differences in costs, wages, and financial position of the hospital			
7. Controlling for the market forces factor	0.282**	0.104***	0.227***
	(0.116)	(0.030)	(0.056)
Observations	251	324	324
8. Controlling for average male wage in area	0.285**	0.075**	0.233***
	(0.123)	(0.031)	(0.060)
Observations	249	320	320
9. Controlling for surpluses/deficits	0.335***	0.103***	0.205***
	(0.121)	(0.034)	(0.060)
Observations	237	303	303

(*Continued*)

Table 5—Robustness Tests: Difference-in-Differences Estimates (*Continued*)

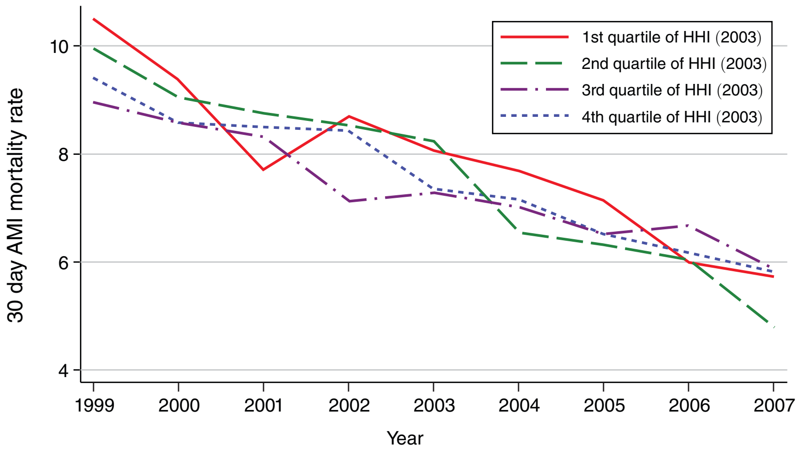
Robustness test	30-day AMI mortality rate (on or after discharge, ages 35–74) (1)	28-day all-cause mortality rate (in-hospital) (2)	Mean length of stay (days) (3)
Patient heterogeneity and number of admissions			
10. Controlling for the age-gender standardized mortality rate in area	0.309** (0.122)	0.074** (0.030)	0.238*** (0.061)
Observations	251	324	324
11. Controlling for the Charlson index	0.314*** (0.114)	0.098*** (0.031)	0.219*** (0.057)
Observations	251	324	324
Possible contamination from other policy changes			
12. Controlling for the number of ISTCs within 30km of the hospital	0.333*** (0.124)	0.096*** (0.031)	0.215*** (0.062)
Observations	251	324	324
13. Controlling for cardiac treatment measures	0.472** (0.188)	—	—
Observations	210		
14. Including all additional controls in rows 7–12 (rows 7–13 for AMI)	0.623*** (0.221)	0.065* (0.033)	0.161** (0.076)
Observations	199	299	299

Notes: Models estimated by OLS with standard errors (in parentheses under coefficients) robust to arbitrary heteroskedasticity. Time period is 2003 and 2007, except for rows 2, 3, and the placebo DiD row 4 (1999 and 2003). HHI measured in the year 2003 for all elective services, calculated using predicted patient flows. Controls are for the year 2007 dummy (except in row 2; row 3 where dummies for all years between 2004–2007 are included; and row 4 which uses a year 2003 dummy), and 36 case mix variables corresponding to shares of cause-specific admissions within 5 year age-gender bands. In row 4, the dependent variable in column 2 is the all-cause in-hospital mortality rate at any point during a hospital stay and the test uses the shorter period 2001–2003. Row 13 adds controls for the shares of patients (i) having thrombolytic treatment within 30 minutes of arrival at hospital, (ii) having thrombolytic treatment within 60 minutes of calling for help, (iii) having primary angioplasty (PCI), (iv) discharged on aspirin, (v) discharged on beta blockers, and (vi) discharged on statins. For the variables in (iv)–(vi), we use data for year 2004 as this is the earliest available year. In all rows, the dependent and independent variables (except age-gender controls, surplus, number of ISTCs, share of patients having primary angioplasty, and the indicator for HHI top quartile) are in logs. All models also include a constant and a full set of hospital dummies.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

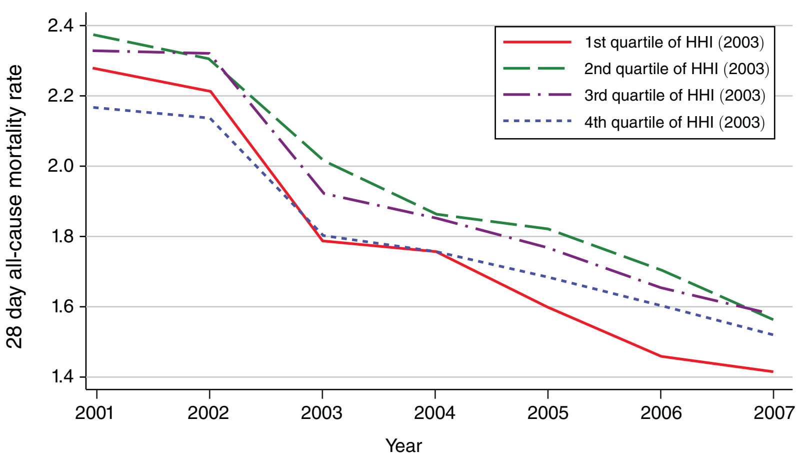
time trend. The estimates of the interaction term are presented in row 2 of Table 5. The coefficients for all three outcomes are significant at the 5 percent level. In magnitude they are approximately one quarter of the size of the relevant baseline estimates in row 1, so that the estimate for 2007 (four years post 2003) is very similar to our baseline estimates for 2007 (row 1) for all three outcomes. We then replace the linear trend with dummies for each year and the time trend-HHI interaction with a full set of HHI-year interactions to examine exactly when the break occurred. The results are in row 3 of Table 5. For both AMI and length of stay the

Panel A. 30-day AMI mortality rates (ages 35–74)



Market definition method: predicted patient flows.

Panel B. 28-day all-cause mortality rates



Market definition method: predicted patient flows.

Figure 3. Outcomes Below and Above Median HHI (1999–2007) (*Continued*)

estimates show no significant rise pre-2006, and an increase in the impact of market concentration in 2006 (the first full year of the policy) and a further increase in 2007. For all-cause mortality the increase first occurs in 2005. While this increase predates the full implementation of the pro-choice policy, it is three years after the policy was announced, suggesting little anticipation effect.[17] We repeated this analysis replacing the continuous HHI measure with a dummy indicating whether the hospital is in the top quartile of the HHI distribution. This parameterization, presented in Table A4, row 2, shows similar patterns: significantly poorer outcomes in

[17] Below we control for measures of health of the local area that may affect all-cause mortality. These controls do explain some of the difference between hospitals in more or less concentrated markets.

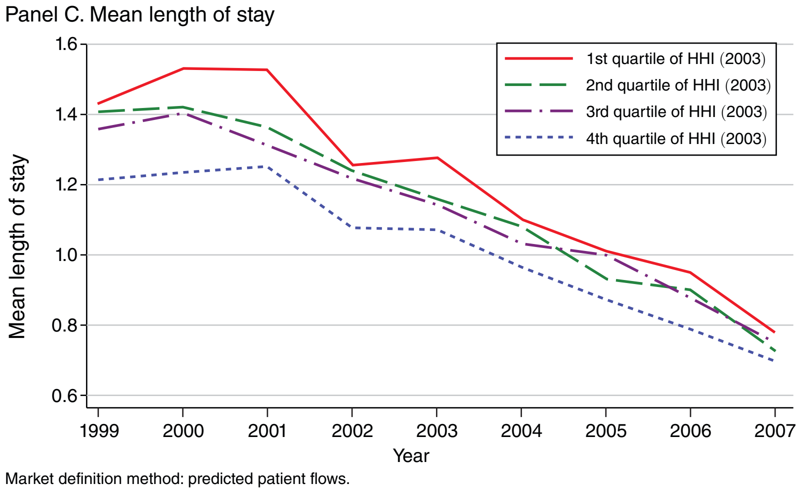


FIGURE 3. OUTCOMES BELOW AND ABOVE MEDIAN HHI (1999–2007) (*Continued*)

Notes: HHI is predicted. Due to data limitations, Panel B is for 2001–2007. AMI and all-cause mortality rates are percentages, while mean length of stay is number of days.

concentrated markets that arise after the full implementation of the policy for AMI deaths and length of stay, and slightly earlier for all-cause deaths.

If our results are being driven by preexisting 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 market structure *before* the reforms were introduced. To test this we undertake placebo DiD tests by estimating the baseline specification using data from prior to the announcement of the reforms. Row 4 in Table 5 presents the results using 1999 as the placebo "policy-off" year and 2003 as the placebo "policy-on" year.[18] None of the estimated coefficients are even close to being statistically significant at conventional levels and their magnitudes are very small compared to the baseline estimates. This provides further reassurance that, even though there are changes prepolicy, the estimated differences between hospitals in more or less concentrated markets between 2003 and 2007 are driven by the pro-competitive reforms.

The typical hospital in our sample is in a highly concentrated market, and because the HHI is nonlinear, variation in the HHI might reflect small changes in concentration. To examine this, we replace the continuous measures of market concentration in 2003 in (i) with a dummy for a hospital being in the top quartile of the HHI distribution in 2003. This test is very close to comparing monopoly versus nonmonopoly markets.[19] Row 5 indicates that the results for AMI and all-cause mortality are driven by the poorer performance of hospitals operating in more concentrated markets.

[18] Due to data limitations, the all-cause mortality variable for the placebo test is the in-hospital mortality rate at any point during a hospital stay and the corresponding test for that variable uses the shorter period 2001–2003.
[19] The mean HHI in the top quartile is 7,155. The number of equal size firms implied by a value of the HHI can be calculated using the formula 1-log(HHI/10,000). This gives an implied mean value of 1.3 hospitals for the top quartile.

However, it is possible that the competition effects are mostly driven by the large London market. London is both unconcentrated, due to the large number of hospitals in close proximity to one another, and high quality, due to the presence of top hospitals and physicians. To test this, in row 6 of Table 5 we omit all London hospitals. The results are robust to this. The coefficients for AMI mortality rates and length of stay are little changed and the estimate for all-cause mortality drops but is still significant at 10 percent. We conclude that our results are robust to the exact specification of the concentration measure and to omission of the largest hospital market.[20]

Differences in Costs, *Wages*, *and the Financial Position of the Hospital*.—In our main specification, the hospital fixed effects control for any constant differences across hospitals in costs and financial position (or anything else). They do not, however, control for any time-varying cost differences. If those differences are uncorrelated with the HHI, this is unproblematic. However, it is possible that hospitals in large urban areas tend to have higher costs and are also in less concentrated markets. Higher costs will lead hospitals to choose lower quality, so omitting costs could lead to a downward bias in the estimated effect of the HHI on quality. To test whether omitted hospital cost differences have biased our results we control for a measure of cost differences: the Market Forces Factor (MFF). This is an adjustment factor calculated by the NHS to capture geographic differences in hospital costs (specifically input prices: land values, staffing costs, etc.). After the reform the MFF was applied to regulated prices, so that hospitals in high cost areas have their HRG prices adjusted upwards and vice versa. Row 7 in Table 5 reports the estimates. There is very little impact on the estimated magnitudes of the DiD coefficients or their standard errors. Propper and Van Reenen (2010) show that higher outside wages reduce the quality of labor a hospital can attract and thus reduce the quality of care. Therefore, as a further control we introduce the level of wages in the outside labor market, as measured by the average male wage in the area.[21] The results including the outside wage are reported in row 8 of Table 5. The results show our main estimates are robust to including the outside wage, although the all-cause death rate estimate is slightly reduced.[22]

Our main models do not control for hospitals' financial surpluses or deficits, since these may be endogenous. Hospitals with higher quality may admit higher volumes of patients, resulting in higher surpluses, and vice versa. However, the switch to

[20] As further checks, online Table A4, row 3, presents estimates of the policy using both cross-sectional and time series variation in exposure to the policy. These estimates are very close to those from our base specification. Row 4 in Table A4 uses actual HHIs rather than predicted. This shows the use of predicted HHIs provides more conservative estimates for all-cause mortality and LOS. The magnitude of the impact of HHI on AMI mortality is similar using actual and predicted HHIs, but imprecisely estimated using actual HHI.

[21] We use the average of the median full-time gross wages for male workers (all occupations) in local authorities (units of local government) within a 30 kilometer radius of the hospital. Male wages are highly correlated with female wages over time within local authorities.

[22] A related issue is differential economic growth in more and less concentrated markets. We deal with this in two ways. Controlling for male wages in the local area is also a test that a relationship between the business cycle and AMI outcomes (Ruhm 2006) does not drive our results. Stronger economic activity is likely to generate increased traffic flows and road congestion, potentially increasing the time 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. Online Table A4, row 7, shows our results are robust to this.

PbR may give hospitals an exogenous large income shock. Under PbR, hospitals get a price per procedure equal to the average cost of all hospitals (adjusted for local cost conditions, as described above). 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. It is possible that our results may be driven by this income shock rather than changes in market concentration if the income shock is positively correlated with concentration (that is, hospitals in concentrated markets received positive income shocks, and those shocks were larger than those received by hospitals in unconcentrated markets). To test this, we include an additional control for the hospital's financial position as measured by the end of year surplus/deficit. Row 9 of Table 5 presents the estimates including this control. The DiD point estimates change very little. We therefore conclude that the response we see is not due to differential price-cost mark ups or gearing associated with PbR.[23]

Controls for Patient Heterogeneity.—In our main regressions we control for differences in case mix between hospitals using the shares of admissions within five year age-gender bands and a full set of hospital dummies. We first seek to alleviate the concern that our estimates are driven by *unobserved* differential changes in patient severity by market structure postreform by adding further controls for observed patient composition. The first control is the health of the local population (as measured by the standardized mortality rates of the population in the catchment area of the hospital).[24] This is a measure of the potential rather than actual patient mix so should suffer from less endogeneity bias than actual case mix. The results in Table 5, row 10, show that again our estimates are little changed by this control, though it does bring down the estimates on within hospital all-cause mortality. This is perhaps to be expected as the mortality rate of the population in a hospital's catchment area will be affected by the all-cause in-hospital mortality rate. The second control is a direct measure of the severity of patients treated in the hospital, the Charlson index, widely used as a marker of patient severity.[25] While we do not use the index in our main regressions because of concerns over endogeneity and that it may contain upcoding responses to the PbR system (e.g., Ellis and McGuire 1986; Dafny 2005), the results in row 11 of Table 5 show that the DiD coefficients change little after inclusion of this measure.

[23] Ideally, we would also like to distinguish between procedures which are likely to be profitable and those which are not in order to see if hospital responses differed across these services. In a world in which managers are motivated by profits, we would expect managerial effort to be focused on the former conditions and not the latter. In the NHS world, in which marginal costs are not well estimated, managers might spread effort across all procedures, profitable or not, subject to the break-even condition at hospital level. We therefore also examined mortality for all elective and all nonelective (emergency) surgery separately, on the grounds that the former are more likely to be profitable. However, mortality rates for elective surgery are very low and DiD results (available from the authors) were not different from zero for both elective and nonelective surgery.

[24] 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 online Appendix (Table A1).

[25] The Charlson index is an index of severity of illness based upon a patient's diagnoses and procedures, including 19 co-morbidities. These are aggregated using weights derived from estimates of the co-morbidities' contribution to predicting mortality.

Second, it may be the case that the reforms led to welfare reducing selection against sicker patients or welfare improving matching of patients to hospitals. To examine this we looked at whether measures of patient severity changed post reform differentially by market concentration. We estimated various measures of patient severity as *outcome* variables in our DiD specification. We found no evidence that severity, as measured by the Charlson index, changed differentially by market concentration post reform and while there was some change in patient composition as measured by the age-gender mix of patients, this was small.[26] We also estimated models with no case mix controls. These estimates are given in online Table A4, row 5, and show that the coefficients for all three outcomes in Table 5 remain significant.

These results indicate that our estimates are robust to observed changes in case mix controls, and so reduce concerns that changes in unobserved heterogeneity in patient severity may be driving our results. In addition, the lack of differential change in observed patient severity across markets suggest that there is no patient selection against sicker patients. Given this, our results thus suggest that the gain to quality is from greater effort on the same type of patients prereform and postreform (or to better matching on unobservables).[27]

Possible Contamination from Other Policy Changes.—Our estimation strategy exploits a policy change. We therefore need to check that the change we observe is due to the pro-competition policy rather than other policies that might have been running at the same time. One potential policy candidate, which was part of the pro-competition reforms, was the attempt to increase the supply of non-NHS facilities through the offer of NHS guaranteed payments (regardless of actual volume) to non-NHS entrants of specific types of (elective) care for which there were long NHS waiting lists. These entrants were known as Independent Sector Treatment Centres (ISTCs). This policy began in 2003 but did not achieve the entry that was initially hoped for. Even by 2008 the volume of admissions in ISTCs was only equal to 1 percent of NHS admissions. However, it is possible that these centres might have sharpened the competitive pressure on NHS hospitals. To test this we control for the number of ISTCs in each NHS hospital's markets.[28] The results are presented in row 12 of Table 5 and show that this also has little effect on the estimates.

There is a second possible confounding candidate, the establishment of cardiac networks to improve the treatment of cardiac patients (Myocardial Ischaemia National Audit (Minap) 2010). This policy started in 2001 and was in operation while regulated prices and mandated patient choice were introduced; while having no element of promotion of competition for patients, the policy was geographically defined. These networks sought to improve treatment of patients through the promotion of specialist units that undertook angioplasty (PCI) as the first treatment for AMI patients (generally replacing thrombolytics), faster use of thrombolytics and

[26] Results available from the authors.

[27] To check that a volume-outcome relationship is not driving our results (Gowrisankaran, Ho, and Town 2006; Gaynor, Seider, and Vogt 2005), we added a control for the number of cause-specific admissions. Online Table A4, row 6, indicates that our results are little affected by this control.

[28] We do not have accurate estimates of the actual volumes of patients treated in ISTCs, and therefore measure the competition from ISTCs for NHS hospital i by the number of ISTCs located within 30km of i.

ambulance response times, and greater prescription of beta blockers, aspirins, and statins on discharge. Performance in these dimensions was published at the hospital level annually. The first two activities are geographically differentiated. PCI as the first type of treatment is only possible in urban areas and the use of thrombolytics in ambulances therefore increased more in rural areas. It is therefore possible that the better AMI survival rates in less concentrated markets are due to the operation of these networks rather than the competition policy.

To test this we first examine the correlation of market concentration in 2003 with the change in use of PCI, thrombolytics, speed of ambulance arrival, and use of appropriate drugs on discharge between 2003 and 2007 (at the hospital level).[29] We find that there is indeed a significant association: the increase in use of thrombolytics is positively associated with high market concentration, and in primary PCI with low levels of concentration.[30] However, these associations do not drive our estimates of better outcomes in less concentrated areas. Table 5, row 13 presents our AMI results controlling for all the cardiac treatment measures. The estimate shows that our results are actually stronger when we control for differences in treatment. Analysis of the coefficient estimates on the treatment variables (not shown) indicates that this is mainly driven by response rates for thrombolytic treatment. The share of patients who have thrombolytic treatment within 60 minutes of calling for help increased more between 2003 and 2007 in more concentrated markets (rural areas). Controlling for this, hospitals in more concentrated markets have a higher increase in AMI death rates, i.e., a lower increase in quality.

Finally, we include all covariates above in the regressions (including, for AMI, the cardiology network variables). Row 14 indicates that the results are robust to this (the large rise in the AMI coefficient relative to the baseline is due to inclusion of the cardiology network variables). Overall, we conclude that our results are robust to a wide range of specification checks.[31]

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. Since we find no impact of the reforms on operating expenditures, we simply calculate the value of the life years gained due to the policy (i.e., gross benefits equal net benefits).

On the benefit side, using the estimated coefficient from Table 3, column 2, the average hospital would experience a 0.3 percent fall in its overall mortality rate

[29] We measure the cardiac treatment variables by the annual performance against the standards published by the body, which promoted this policy (MINAP). The treatment variables used in our analyses are the shares of AMI patients: (i) having thrombolytic treatment within 30 minutes of arrival at hospital; (ii) having thrombolytic treatment within 60 minutes of calling for help; (iii) having primary angioplasty (PCI); (iv) discharged on aspirin; (v) discharged on beta blockers; and (vi) discharged on statins. For variables (iv)–(vi) we use data for year 2004 as this is the earliest available year.

[30] The estimated OLS coefficient (standard error) on HHI for the change in the percentage of patients having thrombolytic treatment within 60 minutes of calling for help is 4.770 (0.871). For the change in the percentage of patients having PCI the corresponding estimates are –3.707 (1.401).

[31] In online Appendix Table A4, rows 8 and 9, we investigate the robustness of our results to weighting and functional form.

from the average decrease in the HHI from 2003 to 2007 (118).[32] 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 0.3 percent of these deaths are averted and combining it with these extra years of life leads to an estimated 4,791 life years saved.[33] 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 \$479.1 million, or approximately £302 million for a single year.[34] A second estimate is the benefit of being in an unconcentrated compared to a concentrated 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 same methods and numbers as above, this translates into 4.4 percent fewer deaths per year and 78,318 more life years saved, with a monetary value of \$7.8 billion, or £4.8 billion.[35]

We can compare these benefits with the fixed costs that would be necessary to induce a reduction of one standard deviation in the HHI from its mean. In our sample, this is equivalent to increasing the average number of competitors a given hospital faces by 0.6, from 1.8 to 2.4.[36] The approximate cost of a new hospital in the United Kingdom context is around £120 million (http://www.nhshistory.net/parlymoney.pdf), so the costs associated with a one standard deviation reduction in the HHI are £72 million. In terms of cost-benefit calculation, this once-off fixed cost leads to ongoing annual benefits of approximately £29.7 million per hospital (the total benefit of £4.8 billion divided by the number of hospitals, 162). Assuming that the useful life of a hospital is 20 years, an ongoing level benefit, and using a conservative discount rate of 5 percent, the present discounted value of the benefits in terms of the value of life years saved from reducing the HHI by one standard deviation is approximately £370 million. The benefits outweigh costs by a factor of 5.

These estimates of the impacts of the policy only enumerate the gains in quality arising from decreases in death rates and not from any other aspects of quality such as quality of life. Further, we are recovering short run effects of the policy, since we only have one year of data following implementation. The long run effects could be larger once

[32] A one percentage change in HHI is equal to 43.5 units, so a fall of 118 would lead to a change of $0.099 \times 118/43.5 = 0.3$ percent.

[33] 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.

[34] A US dollar exchanged for a pound sterling at a rate of 0.63 on March 16, 2012 (http://www.xe.com).

[35] We can also calculate the value of the gains in life years specifically for AMI patients. This allows us to apply quality adjusted life years (QALYs) that account for the lower quality of life for heart attack survivors, and thus to have a more finely tuned measure of benefits. The average age of death of AMI patients in the sample is 65 years and these patients have an estimated life expectancy of 7.5 years (quality unadjusted; Ünal et al. 2005) or 6.8 QALYs (Bravo Vergel et al. 2007) after surviving a heart attack. Using the same calculation methods as above, a total of 1,527 life years or 1,384 QALYs would be gained for AMI patients, for a reduction of one standard deviation in the HHI. The monetary value of these gains amounts to \$152.7 million (£96.2 million) and \$138.4 million (£87.2 million), respectively. While these gains are substantial, they are well below the total gains from the reform, indicating that the value of the reform was rather general, and not confined to a specific group of patients.

[36] We calculate the number of equivalent hospitals for a given HHI level using the formula 1-log(HHI/10,000).

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 (£4.8 billion) is substantially greater than the short run effect. This suggests that there could be large positive effects of policies that result in substantial decreases in concentration.[37]

VI. Summary and Conclusions

We have examined the impact of the introduction of a pro-competition policy on hospital outcomes in England. We find strong evidence that under the regulated price regime hospitals within the NHS engaged in activities that increased quality of patient care. 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 $479 million per year. 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. The estimated present discounted value of constant gains over multiple years substantially outweighs estimates of the fixed costs of entry associated with reducing market concentration.

Our results show that the introduction of competition can be an important mechanism for enhancing the quality of care patients receive even in a set up where hospitals are not profit maximizers. A logical next step for future research is to examine whether managers in this set up are less sensitive to the difference between price and cost at a procedure level than managers in a system where profit is more important. This could have important implications both for the distribution of benefits across patients of different types and for the way in which markets with many not for profit health care providers might be regulated. Another extension is to consider the separate impacts of information and choice. If information is released at the same time as choice is introduced, there may be an information effect separate from competition. This may simply increase demand elasticity in both concentrated and unconcentrated markets, which will tend to bias the estimated impact of concentration on quality down. Alternatively, it may only increase demand elasticity in unconcentrated markets where there is choice, which would have the opposite effect. While our data do not enable us to separately identify these effects, this is an important avenue to consider for future research. More generally, while our results come from the United Kingdom, they have implications for the impacts of health care competition in other settings where the price is regulated, such as the US Medicare and Medicaid programs or a number of other countries, such as Denmark, Germany, the Netherlands (where prices for some services are deregulated), and Sweden.

Last, our research also contributes to the growing evidence on the impact of competition in public services generally. There has been a great deal of interest in recent

[37] The gains from the policy were located in less concentrated markets, so there are distributional issues we do not address. However, in general in the United Kingdom, individuals living in these markets are more deprived than those in more concentrated hospital markets.

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.

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