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Can governments do it better? Merger mania and hospital outcomes in the English NHS

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Abstract
The literature on mergers between private hospitals suggests that such mergers often produce little benefit. Despite this, the UK government has pursued an active policy of hospital mergers, arguing that such consolidations will bring improvements for patients. We examine whether this promise is met. We exploit the fact that between 1997 and 2006 in England around half the short term general hospitals were involved in a merger, but that politics means that selection for a merger may be random with respect to future performance. We examine the impact of mergers on a large set of outcomes including financial performance, productivity, waiting times and clinical quality and find little evidence that mergers achieved gains other than a reduction in activity. Given that mergers reduce the scope for competition between hospitals the findings suggest that further merger activity may not be the appropriate way of dealing with poorly performing hospitals.

Keywords: Hospital mergers, event study, quality, political influence

JEL Classification: I18, I11, L13, L32

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Public intervention in health care markets is pervasive throughout the world. In many countries the state not only finances health care through taxation but also manages provision, directly owning hospitals and employing staff. Examples include the National Health Services of the Nordic Countries, the UK, and Southern Europe. In countries with less centralized social insurance models of health care, many hospitals are also publicly run (for example, France). Even in the United States, which relies heavily on private markets in health care, there is an extensive government role, including publicly run hospitals and state control over entry or capacity.

In this paper we seek to provide evidence on whether public management of hospital markets enhances hospital performance. We focus on one particular issue: the impact of government instigated public hospital reconfigurations on subsequent performance of these hospitals. Analysis of private hospital mergers in the USA has generally concluded that these mergers bring little benefits in terms of prices and costs (e.g. Dranove and Lindrooth, 2003; Harrison, 2010; Vogt and Town 2006). Our paper asks whether governments do better.

We study a wave of hospital mergers in England between 1997 and 2006. In 1997 the Labour Party, led by Tony Blair, won a landslide election victory. One of the major platforms of the election campaign was to reverse the policy of competition between public hospitals for publicly funded contracts for health care that had been instigated by the previous Conservative administration.[1] Following their election victory, the administration undertook a radical programme of hospital closure that merged together many hospitals that were co-located geographically. The scale was such that out of 223 short term general hospitals in England in 1997, 112 had merged between 1997 and 2006. The median hospital market went from 7 to 5 hospitals.

We exploit two features of the English hospital market to try to identify the causal impact of merger on English hospital performance. First, we exploit the large scale nature of the merger activity. The fact that mergers are a common event means that participation in a merger will be less likely to be affected by selection on unobservables than in cases where mergers are rare. Second, we appeal to the fact that whether a hospital in the UK is merged or not extent depends not just on financial or clinical performance, as in a private market, but also on national politics. The NHS is a major political issue and hospital closures are not popular with the UK public. As a result, national politicians get involved in campaigns against local closures. In the UK's first past the post system, where the politician is in a safe seat (i.e. they have a large majority) campaigning against a closure will not alter the chances of their being re-elected. Where their seat is marginal and they could lose it if voters

[1] All individuals in the UK are entitled to tax funded public health care. There is a small private sector which provides services for which there have been historically long waiting lists in the public sector.

swing away from them, campaigning against closure may bring them electoral advantage.[2] But such campaigns are not associated with post election hospital funding.. As support for this, Bloom et al (2010) show that political marginality in a local area affects the number of hospitals in that area but is not associated with their post election funding.

We use these two stylised facts to justify a matching approach to identify the impact of mergers. Hospitals which are similar on observables will have different probabilities of closure, not because they differ in terms of unobservables, but because of political marginality. Using matching, we compare the change in performance of those hospitals that merged with those that did not over a 6 year window, looking at performance from two years before to four years after the merger date. We examine activity, staffing and financial performance and a large set of measures of clinical quality. [3] We find that a merger results in a fall in the scale of a hospital in terms of total activity and total staffing. But that other than this removal of capacity, we find little evidence that performance improves due to merger. Post merger, financial performance declines, labour productivity does not change, waiting times for patients rise and there is no indication of an increase in clinical quality.

The paper contributes to a number of different literatures. First, we contribute to research on hospital mergers. Almost all the extant evidence is from the USA. A number of these studies are "merger retrospectives" (Haas-Wilson and Garmon, 2011; Tenn, 2011; Thompson, 2011; Sacher and Vita, 2001). They study the impact of a particular merger.[4] The policy we exploit led to around 1/4 of hospitals disappearing. Our study is therefore more similar to studies which have examined the impacts of large numbers of hospital mergers in the US during the 1990s (e.g., Dafny, 2009; Ho and Hamilton, 2000; Krishnan, 2001; Spang et al., 2001; Town et al., 2006). These studies find, in general, little benefit from merger and consolidation. These mergers are the result of private decisions, as opposed to central planning, and hospitals are mostly private firms. As a consequence, the evidence may not be directly applicable to public systems.

Second, we contribute to the literature on health reform. Our work is particularly pertinent to the debate over the use of competition as a means to improving hospital productivity. This literature suggests that under certain conditions (regulated prices and observable quality) competition can improve quality (e.g. Gaynor 2006). In the UK recent pro-competitive hospital reforms appear to

[2] A vivid example was the 2010 election where the (then) Labour health minister reversed a decision to close a London hospital that was located near a very marginal Labour constituency just five days prior to the election. See http://www.guardian.co.uk/politics/2010/apr/29/whittington-hospital-closure-halted

[3] Ho and Hamilton (2000) was the first study to examine the impact of mergers on hospital outcomes.

[4] There are a small number of case studies of hospital mergers in the UK post 1997. Fulop et al (2002) and Hutchings et al (2003) undertake a case study of three (Fulop et al) and eight (Hutchings et al) mergers in one city (London). Their focus is on saving s in management costs post merger. They find little evidence of this and highlight the fact that pre-merger forecasts of savings are over-optimistic ex-post.

have increased quality (e.g. Gaynor et al 2010, Cooper et al 2011) and competition has been shown to result in better management of hospitals, which in turn improves a range of outcomes (Bloom et al 2010). Large scale merger activity reduces the opportunity for competition, threatening these gains.

Third, we contribute to research on whether planned systems in welfare provision achieve better outcomes than the private market. There has been a great deal of interest in recent years in competition in education, both theoretically and empirically (e.g., Epple and Romano 1998; Hoxby 2000; Epple, Figlio, and Romano 2004). Initial positive findings on the impact of competition in education (e.g. Hoxby 2000) gave impetus to attempts to promote competition. These findings, however, have been challenged by later research which suggests that the benefits from competition are less easy to achieve (e.g. Rothstein 2007; Bayer and McMillan 2005). Our findings suggest that, in the case of UK hospitals, configuration of the market by government does not result in the promised gains either. Our results thus add to the evidence on the conditions under which gains from competition in the provision of public services may be realized.

The paper is organised as follows. In section 2 we discuss the background to reconfigurations of hospitals in England during the period we study. In section 3 we discuss our methodology. In Section 4 we present the data and in Section 5 the results. Section 6 concludes.

2. Hospital reconfiguration in the UK

The election of the Labour government in the UK in 1997 led to a wave of hospital reconfigurations as the government sought to roll back the pro-competitive reforms of its Conservative predecessor and to manage local service configuration. The Conservative administration had established UK public hospitals as free standing entities, known as hospital trusts, from 1991 onwards (Propper et al 2008).[5] These hospitals were wholly publicly owned. Each was led by a single management board responsible for meeting financial and quality objectives set by the government. Hospitals were supposed to break even each year. Large scale capital expansion was funded by loans from the private sector. Hospitals competed with each other to secure contracts from public sector buyers of health care, known as purchasers, who selectively contracted with hospitals on behalf of a geographically defined population. Individual patients had no choice of purchaser and funding of purchasers was on a capitation basis, adjusted for population need.

[5] An NHS "hospital trust" is a financial, managerial and administrative unit and may cover more than one physical hospital, all of which are located closely in geographical space. We use the term "hospital" rather than "hospital trust" for expositional convenience.

Post-1997 the Labour administration maintained the separation of provision and purchasing and the contracting arrangements, but emphasised cooperation over competition. What this meant is they sought to deal with the problems of hospitals which were failing to meet their financial or quality targets (the quality targets being defined in terms of waiting time rather than clinical quality) through reconfiguration and merger. These involved bringing two (or occasionally more) independent hospital trusts into one, with the removal of one management board and senior management team.[6] This required an official act of dissolution, which was put in place in the year immediately prior to the two trusts operating as one legal unit.

In our analysis we focus on merger activity between short term general (known as acute in the UK) hospitals. The extent of merger activity was high and took place predominantly in the six years after 1997. Of around 223 acute hospitals in 1997, 112 merged sometime between 1997 and 2006. Figure 1 shows the distribution of mergers by year for the sample of 102 acute mergers used in this paper.[7] There were around 20 mergers per year, rising to a peak in 2001 and falling to zero after 2002.[8]

The decision to reconfigure was (and is) made by the Department of Health acting through their regional offices. Hospitals that are subject to merger are located in the same geographical market (there are no public hospital chains or groups in the UK). The purchasers of hospital care (who cover all the population within a geographical area and are known as Primary Care Trusts, PCTs) are typically involved in the merger discussions as well as the hospitals that might be merged. Drivers for mergers have included facilitating hospital or service closures as a response to a belief there is spare capacity in the short term general (acute) sector, securing financial viability of smaller organisations and increasing their negotiating power with buyers and enlarging the hospital to provide better services for the buyers of services (Garside 1999). While a reduction in average costs by better use of resources (Ferguson and Goddard 1997) is used to justify mergers, in the UK the focus has tended to be on better use of management costs rather than hospital wide economies of scale (McClenahan 1999). This is perhaps because UK hospitals are relatively large (in comparison to the average US hospital). A study of NHS hospital mergers in London in 1999-2000 (the earlier of the mergers we examine here) identified financial pressures as the most important driver of mergers

[6] Hospital trusts could also reconfigure services within a trust but this was their sole responsibility and did not need to involve the regulator or any other trust.

[7] No mergers occurred between 2003 and 2004. From 2005 onwards the mergers are of a different nature and are to form Academic Health Science Centres, which are alliances of hospital trusts. To avoid contamination of our sample with these complex alliances, we exclude 3 merging hospitals in 2005 and 2 merging hospitals in 2006. We also excluded 5 other cases involving hybrid mergers between hospitals and primary care trusts and mergers involving the reconfigurations of hospital mental health service only.

[8] The unit of analysis are the hospital merging partners observed during the financial year when the dissolution act is approved. In our subsequent analysis we refer to this as year 0.

during this period. The need to make savings featured in all consultation documents. More specifically, most of the merging parties had financial deficits and it was argued that mergers would reduce these. The main costs identified as being potentially savable were management and back office costs, rather than a reduction in clinical costs. Economies of scale were rarely mentioned (Hutchings et al 2003).

Figure 2 shows the location of merged (pre-merger) and never merged hospitals in England between 1997 and 2006. The map shows the large scale of the merger activity and that mergers, as expected given the focus on removal of spare capacity, happened more in urban areas than in rural ones. However, there were mergers in rural areas as well as urban ones.

As noted in the Introduction, while the case for a merger is made by the Department of Health on the grounds of performance, whether a merger actually takes place or not can be affected by national politics. Figure 3 plots the number of hospitals per person in a political constituency against the 1997 winning margin of the governing party (the Labour Party in our sample period).[9] When Labour's winning margin is small (under 5 percentage points) there are about 10% more hospitals than when it or the Opposition parties (Conservatives and Liberal Democrats) have a large majority. More formally, Bloom et al (2010) showed that the number of hospitals in a local market is a function of the political marginality of the constituencies around a hospital. Hospitals whose catchment area contains a higher share of politically marginal constituencies were more likely to have rivals in their area than those where there are few marginal constituencies. In other words, closures (mergers) are more likely to be carried out in areas which are not politically marginal. However, while political marginality affects the probability of a merger, it does not directly affect the resources of hospitals. Resources are allocated to the public purchasers of hospital services using a formula based on demography and need. This formula does change from time to time, but not in step with national elections. As support for this, Bloom et al (2010) showed that the financial resources of hospitals (their annual expenditure and surpluses) were not related to marginality. Given this, political marginality should not directly affect post merger performance.

3. Methodological approach

The nature of the merger activity has implications for any attempt to identify the impact of the merger. First, as in the analysis of private sector mergers, those hospitals which merge are likely to be different from other hospitals on aspects that are likely to influence their pre- and post-merger

[9] Figure taken from Bloom et al (2010). The picture is very similar for the 2001 election.

performance. Second, the regulator is likely to implement the most potentially beneficial mergers sooner than the less beneficial (in the private sector the best matches may also be made first). Third, there may be anticipation effects, as in contrast to the secrecy that often accompanies mergers and acquisitions in the private sector, the decision to merge is discussed in public before the merger is realised. Fourth, merger decisions may be subject to political influence. We take all these into consideration in our empirical design.

Our aim is to estimate the causal effect of mergers on performance. Given the possible anticipation effects, we need to allow for changes to happen in the one or two years immediately before the merger. Given there is no period in which the merger policy is 'switched off' (i.e., mergers occur in most years of our sample) we use an 'event study' design with matching.

A. Event study design

The event study design takes merged hospitals (the treated group) and examines within hospital variation in outcomes for this group before and after the merger. Examining changes within hospitals that merge controls for time invariant characteristics that may affect performance post merger. We match merged hospitals with controls (via a procedure described in the next section) and use the matched controls to detrend the outcomes for the merged hospitals. This provides non-parametric control for selection and other confounder effects, conditional on the validity of the matching procedure. By looking at performance before the merger we allow for anticipation effects.

This is akin to an extended DiD approach, where we use matching to select the appropriate control group (e.g. Blundell and Costa Dias, 2000) and the treatment date is the date of merger, which differs across merging hospitals.[10] The identifying assumption is that the outcomes in the treated and control groups follow parallel trends before the treatment occurs.

To implement this approach we need to compare performance before and after the merger. Since there is only one (merged) entity after the merger, for the "before" measure we construct an aggregate measure of the outcomes of the partners prior to the merger, following, for example, Dranove and Lindrooth (2003). Hospitals in the control group are used to estimate the time trend for the outcomes examined. The time trend is estimated for the period corresponding to the before and after years of any merger in the sample and is subtracted from the (log of the) outcome variables of the merged hospitals to give the detrended outcomes.

Using the detrended data for the ever-merged hospitals we estimate:

(1) $y^k_{it} = \alpha + \Sigma \beta^k T_s^k + \mu_i^k + e_{it}^k, \qquad s= -1, ...,4$

[10] See Girma and Gorg (1997) and Stevenson and Wolfers (2006) for examples of applications of this technique.

where y_{it}^{k} = the log of the *kth* detrended outcome for hospital *i* in year *t*, β^{κ} is a vector of time effects relative to time *t = -2 (*the baseline), μ_i^k is a fixed hospital effect for the *kth* outcome and e_{it}^{k} is random noise. Time varying confounders are not in (1) as they are dealt with by matching (see section 3B below). The coefficients of interest are β^{κ}.

We examine the outcomes of the merged hospitals from 2 years prior to the announcement of the merger to four years post merger. The announcement date is denoted *t* = 0 and is the last date at which the hospitals operated separately. This gives us a baseline three years prior to the first year of operation as the merged entity. As this is well before the actual merger date it should reduce contamination from potential anticipation effects.

B. Matching

We now describe the matching procedure we use to select control hospitals for the purpose of detrending the data for the hospitals involved in mergers. As described in the preceding section, our aim is to estimate the effect of treatment on the treated, as in the treatment effect literature. Let $m_{it} \in \{0,1\}$ be an indicator of whether hospital *i* is merged in period t and let y_{it+s}^{1} be the hospital outcome s years after the merger and y_{it+s}^{0} as the outcome had the hospital not merged. The average causal effect of merger on the merged hospitals is:

$$(2) \qquad ATT = E\{y_{it+s}^{1} - y_{it+s}^{0} | m_{it} = 1\} = E\{y_{it+s}^{1} | m_{it} = 1\} - E\{y_{it+s}^{0} | m_{it} = 1\}$$

Identification of the causal effect relies on the construction of a valid counterfactual for the last term in equation 2.

As is common, our challenge is that the last term is unobservable. One potential candidate is the average outcomes of the hospitals that are not merged at time *t*. However, since those hospitals that are subject to merger are not randomly selected by the regulator, the use of all hospitals that do not merge as a counterfactual is not likely to be appropriate. To get round this we match each of the merged hospitals with (a set of) hospitals that never merge that are similar in terms of observables. We match on pre-merger hospital and local market observable characteristics that determine the decision of the regulator to merge (the exact variables are discussed in Section 4B). The key assumption here is that selection on observables removes unobserved heterogeneity between those hospitals which merged and those which did not.

We reduce the dimensionality problem by using propensity score matching (Rosenbaum and Rubin, 1983). We estimate a separate propensity score for each year using, for year *t*, hospitals which merged in year *t* and any never-merged hospitals observed in that year. Merged hospitals are used only in the year in which they merge and never-merged hospitals are re-sampled every year.

Stratification by year allows us to relax the assumption that the characteristics that drive the selection of hospitals into mergers are independent across time which, given the scale of mergers in the English hospital market, seems likely. The idea is similar to the design of a clinical trial where the same treatment (i.e. a merger) is provided to 6 different groups: hospitals selected in 6 different years. Treated units can only be selected into the program once, whereas untreated units can be selected more than once as controls. We use the three closest matches for each merged hospital that are located outside their local markets. Imposing this latter restriction avoids bias from any spillover effects of the merger on the local market. The three matches are weighted by the difference between their propensity score and the propensity score of the merged hospital.[11]

From this matching procedure we obtain 6 groups of controls (with repetitions), one for each year of mergers. We use each control for the full six year window to estimate the time trends.

This approach deals with the identification issues discussed above. First, potential selection into merger is dealt with by matching on observables. Second, the fact that the more beneficial mergers may occur first is dealt with by estimating propensity scores for the matching year-by-year. Third, the anticipation effect is dealt with by taking the baseline year to be three years prior to the first year of operation as a merged hospital (this is 2 years prior to the merger announcement).[12] Fourth, political influence in the merger decision is used to answer the question of why control hospitals that are similar in terms of observables to those that merge do not merge. As the cost for the regulator of enforcing a merger between two otherwise identical hospitals differs by electoral marginality, variation in electoral marginality allows for similar hospitals to be randomly selected in or out of the merger program.

4. Data

We have assembled a rich database with hospital-level panel information, derived from a large number of administrative data sources, on a variety of measures at hospital level. Sources and the detailed variable definitions are in the Appendix. We examine mergers between short term general hospitals as these are the dominant suppliers of hospital-based services. As most of our data are available from 1997 onwards, we examine all mergers which occurred in 1999 or later as this is first

[11] The results are robust to using more or less matches.

[12] As Figure 1 shows, around half the mergers occurred in by 2000. Given that we allow for anticipation effects for two years prior to the merger date (i.e. back to 1997), we do not have sufficient data to test explicitly to test the common trends assumption of similar performance of the treated and controls.

merger wave for which we can observe the premerger outcomes two years prior to the legal Dissolution Act.

A. Outcome variables

We focus on two sets of outcome variables. These first comprises measures of activity and financial performance, and include admissions, staffing, financial performance and a simple measure of labour productivity, the ratio of activity to staff. The second comprises measures of quality, broadly defined to include waiting times and a set of clinical outcomes used to measure the performance of hospitals in England. Table 1 presents these variables for the ever-merged and the never-merged hospitals, the former prior to merger. The final column of the table presents a t-test of the difference in means.

The table shows that hospitals that merge are, prior to merger, smaller than never-merged hospitals. They have fewer admissions, staff, and total beds and (significantly) lower operating expenditure. Their staff composition is slightly different in that they have a significantly smaller share of expenditure on agency and qualified staff. They also have lower productivity, defined as admissions per staff member. In terms of clinical and quality indicators, there is no strong indication that merged hospitals were performing less well in terms of clinical quality prior to merger. Hospitals which merged are more likely to be in areas with lower wages and poorer health, reflecting the fact that capacity in inner city areas is high for historic reasons.

The final row of the table examines the relationship between political marginality and the merger. We define a catchment area for each hospital of a 30 km radius round each hospital.[13] The table shows merged hospitals are less likely to have catchments areas which contain political constituencies that were marginal in the 1997 election.[14]

Figures 4 and 5 show the distribution of average size (as measured by beds) and financial deficits of merged hospitals, averaged over the three years pre-merger. Figure 4 shows the size of the parties to each merger sorted by increasing difference in pre-merger bed size between the hospitals. This shows that at one end of the distribution there are some hospitals in which there is a large partner and a smaller one. But it also shows that large hospitals are also quite likely to merge with other

[13] This definition has been used widely in analyses of hospital markets in the UK. See for example, Propper et al (2008), Bloom et al (2010).

[14] To construct this variable we use data on outcomes of the national elections at the constituency level from the British Election Study. We observe the vote shares for all parties and use these to compute the winning margin. We define a constituency to be marginal if the winning margin is below 5%. As hospitals usually have a catchment area that comprises several constituencies we use the share of marginal constituencies in a 30 km radius of the hospital.

large hospitals. Figure 5 shows the surpluses of the hospitals in the merger sorted by increasing pre-merger joint deficits. The figure shows that around half the mergers take place between hospitals at least one of which has a large deficit. In some cases both parties have large deficits.

B. Matching

To define our controls we employ propensity score matching, as discussed in Section 3. Matching estimators typically assume that there exist a set of observed characteristics, Z, such that outcomes are independent of program participation conditional on Z. But a DiD matching estimator allows selection into the program to be based on anticipated gains from the program, provided that participation does not help predict changes for the controls conditional on Z used in estimating the propensity score. We therefore select variables which are likely to determine the probability of mergers, which may include some variables which we expect will change as a results of the merger. We use size (as measured by the number of beds) and the financial surplus/deficit. Both of these are averaged over the 2 years pre-merger. We also use two non-time varying characteristics (teaching and specialist status) and two variables which shift demand for hospital care in the hospital's catchment area (defined as a 30 km radius round the hospital). These are the mean male wage, as a measure of income in the area, and the standardised mortality rate, as a measure of the need for care in the area. These variables are intended to measure the issues considered by the regulator in making the merger decision – the issue of size, financial performance and demand.

We estimate the propensity score separately for each year to allow for the fact that the pool of potential mergers changes over time. Table A1 presents the probit estimates. This confirms the differences between hospitals which merge and those which do not shown in Table 1. Table A1 shows that hospitals which merge tend to be located in poorer areas, are smaller in terms of bed size, and have larger deficits. It also shows that as the pool of merged hospitals changes so do the factors which are associated with merger. So, for example, post 1997 there are no mergers involving teaching or specialist hospitals, and difference in bed size is no longer a determinant of the probability of merger.

Under the assumption of independence conditional on observables, the pre-merger variable should be balanced between the merged and non merged groups. To test this we carry out a balancing test as suggested by Smith and Todd (2005). This test examines the standardised difference (or bias) for all the covariates used in the propensity score. [15] The lower the standardised difference, the more balanced or similar the treatment and control groups will be in terms of the variables under

[15] The standardised bias of variable X is the difference in means between the merged hospitals and the appropriately matched never merged hospitals scaled by the average variance of the variable X in the two groups.

consideration. Although there is not a formal criterion for how large a standardised bias should be, we follow Rosenbaum and Rubin (1995) and assume that a value of 20 is large. For each variable entering the propensity score model we perform a formal paired t-test between the merged and matched control observations.

Table A2 shows the tests for each of the variables in the propensity score based on our matching procedure. For each variable, the first row presents the tests for the unmatched sample (all the non merging hospitals) and the second row the test for the matched controls. Without matching the standardised difference between the treated and the controls is over 20 for all variables except surpluses, and the t-tests indicate that these differences are significant at 10 percent or greater for all variables except surpluses. However, once we match the standardised differences between the merged and the matched controls are all less than 10% except for surpluses. The substantial reduction as a result of the matching is also apparent in the column reporting the percentage bias reduction. All of the t-test values on the differences between the merged and the matched controls are not significant at the 5 percent level. This suggests that our matching procedure gives us a control group that on observables is similar, even though we impose the restriction that hospitals cannot be matched with hospitals located in the same local market.

C. The effect of political marginality

As a test of our assumption that marginality affects the probability of merger, we estimated the probability that a hospital will be involved in a merger between 1997 and 2003 as a function of the all the variables in the propensity score regressions (those in Table A1) plus a variable measuring the share of constituencies in the hospital's catchment area that which were politically marginal in 1997. The results indicate that political marginality has a significant and negative sign.[16] This supports our assertion that marginality results in a different probability of merger. For marginality to explain why the control group does not merge even though the hospitals in the group are the same in terms of observables, the control and treatment group should differ in terms of political marginality. To test this we carry out the balancing test on the political marginality variable. This is shown in the last row of Table A2 and shows that, after matching, the treated and the controls still differ significantly. From this we infer that our control group contains hospitals which are similar in terms of observables that might determine performance post merger but differ in terms of their chances of being merged for reasons that are exogenous to the success of the merger.

[16] The coefficient on marginality is -0.037 (t-stat = 3.51). The other controls remain robust to inclusion of this variable. Similar results are found using marginality in 2001, as this variable is highly correlated with 1997 marginality. Full results available on request.

5. Results

We begin by presenting the changes in activity following a merger and then discuss changes in quality.

A. Activity, Staffing and Financial Performance

The first four columns of Table 2 show measures of activity – total admissions, total staff, beds and total operating expenditure. These show a general fall in hospital activity post merger. Columns (1) – (3) show that post merger, admissions, staff and beds have fallen by around 11-12 percent each year. Column (4) shows the fall in activity is not matched by a fall of the same size in total operating expenditure. In the first year post merger the fall in the growth rate of expenditure was similar to that of admissions, but thereafter was less than the fall in admission, staff or beds.

Column (5) examines the number of staff and shows little change in the share of staff that is medically qualified, implying that mergers lead to little change in hospital spending on staff who might bring about higher clinical quality. Columns (6) and (7) examine expenditure on managers and agency staff as a share of total hospital expenditure. The share of expenditure on managers rises a little in the year of merger: the point estimate of 0.35 percentage points represents around an 8 percent increase on a mean of around 4.4 percent. What is more dramatic is the increase in the share of staff who are not permanent employees of the hospital. The share of expenditure on agency staff rises significantly post merger and by year 4 post merger is about 1.15 percentage points higher. At the sample mean of around 3.5 percent this is an increase of around 33 percent. This provides a possible answer to how merger can lead to a larger decrease in (permanent) staff than in hospital spending. Merging hospitals appear to be offsetting decreases in permanent staff with temporary hires.

The last two columns of Table 2 shows a key measure of performance for the government – the surplus of the hospital (in levels) - and a crude measure of labour productivity: the (log of the) volume of admissions per NHS employee in the hospital. The surplus is shown in column (8). It is clear that mergers are costly: any surplus falls in the year immediately before operation as a merged unit and falls thereafter, such that by four years after the year of first operation as a merged entity, the deficit is nearly £3m. This result suggests that mergers are expensive to carry out and result in

the increasing deterioration of the financial position of the hospitals involved both in the short and in the long run. Column (9) shows no significant productivity gain following the merger.[17]

B. Waiting times, length of stay and quality indicators

Table 3 presents results for a large set of measures that have been used as indicators of quality of patient care. We begin by examining waiting time and length of stay. We then examine measures of quality of clinical care published by the national agency which constructs measures of clinical quality of care (NCHOD). [18]

In the main, none of these measures show an improvement and there are some signs of a decrease in quality of care. Column (1) shows no effect of merger on length of stay. Columns (2) and (3) present some evidence of an increase in both mean waiting times and of the share of patients waiting more than 180 days for an elective admission four years post merger. [19] In terms of the clinical measures, we examine death rates from emergency heart attack (AMI) admissions, a widely used measure in the literature on the impact of market configuration on outcomes (e.g. Kessler and McClellan, 2000), measures of care for patients with stroke and measures of care for patients with fractured proximal femur. For AMI (column 4) and fractured proximal femur (columns 8 - 10) the quality indicators remain relatively stable post merger. However, columns (5) and (6) show poorer outcomes for patients admitted following a stroke. Column (5) shows higher death rates post discharge after merger. Column (6) shows higher readmission rates to hospital within 28 days of discharge, both immediately before the merger and post-merger. Column (7) shows an improvement in one measure – the 56 day return rate to usual place of residence - but this is for only one of the years post-merger and is only significant at 10%.

It is possible that the changes in stroke care are due to a change in the severity of patients attending the hospital. [20] As a check we examine changes in case mix over time, using the same model as used to estimate the impact of merger on the outcome variables. We examine two indicators of hospital case mix. The first is the share of patients aged 75 and over. The second is the Charlson index for patients with AMI. The Charlson index is a commonly used measure of AMI patient severity. We find a statistically significant *reduction* in the proportion of patients aged 75 and older from year -1 to 4

[17] The pattern in labour productivity is similar if we examine admissions per medical staff rather than all NHS staff or if we use admissions/total expenditure.

[18] The clinical outcomes data in Table 3, columns (4) - (11) are available from only 1998/9 only. This shorter time series means that we lose observations both in the controls and the merger samples. All regressions are estimated using weights to reflect the population of patients at risk of a negative outcome in each hospital. Only hospitals having a population of patients at risk (of the particular outcome) of 100 or more are included in the analysis. The sum of the population of the merger partners is used to calculate this threshold.

[19] Similar results are also found for the share of patients waiting over 90 days and over 240 days.

[20] NCHOD indicators are standardised by age and sex of the population at risk of a negative outcome. Heterogeneity in the health status of the patients within age and sex bands may exist.

and we find no effect of merger on the Charlson index.[21] Thus it does not appear that merged hospitals are attracting sicker patients either immediately before or post merger. We therefore conclude that the poorer outcomes for stroke are not due to a worsening in the case-mix.

In summary, we find that whilst the effect of merged was to shrink the combined size of the merged hospitals, other than this reduction in size and associated fall in activity, the merger does not appear to have brought benefits. Labour productivity does not appear to have risen, the merger has not stemmed the increases in size of deficits and there are no indications of an increase in quality (in fact there is one indicator of a fall in measures of clinical care).

C. Robustness to the control group

Our methodology involves indentifying a control group based on matching on observed characteristics. It is possible that results are sensitive to this choice. To test this we used all hospitals that never merged as the controls and replicated the analyses of Tables 3 and 4. The results are presented in Appendix Table A3 and A4.[22] These tables show, in the main, that our results are unaffected by our choice of control group, though there are some small differences. First, while the general reduction in hospital activity (as measured by falls in admissions, beds and staffing) is also apparent when all never merged hospitals are used as a control group, the results for staff composition is sensitive to the control group. In our matched analyses, the share of qualified staff is unchanged post merger. However, compared to all never merged hospitals, the share in the merged hospitals falls significantly from year t-1. This may indicate that smaller never merged hospitals, which are more highly represented in the matched control group than in all the full control group of never merged hospital, reduced their share of clinical staff relative to larger hospitals. Second, the growth rate in productivity of the merged hospitals is positive when compared to all never merged hospitals but not when compared to the matched controls. This is because the fall in admissions in merged hospitals is larger relative to all never merged hospitals than to the control group. Third, the mortality rate following emergency AMI admissions is significantly higher for merged hospitals when the control group is all never merged hospitals. However, all these changes are relatively small in magnitude and primarily driven by the fact that non matched controls are larger than the matched controls.

[21] For example, the coefficient on the impact of mergers at time t+4 for patients aged 75 and over is 0.017 (standard error = 0.043) and the coefficient at time t+4 for the Charlson index for emergency AMI patients is -0.01 (s.e.=0.049)

[22] These specifications include 8 merged hospitals that were off common support for the results in Tables 3 and 4.

D. Heterogeneity in the impact of mergers

Mergers may have more of an impact when they occur in certain markets or to certain types of hospital than others. We examine heterogeneity along three dimensions – the extent of concentration in the market pre-merger, the pre-merger size of the merging parties and the pre-merger deficits of the merging parties.

i) Mergers and pre-merger market concentration

One obvious concern may be that mergers which occur in already concentrated markets may have a more negative effect on welfare than those where there are a large number of competitors. The mergers we examine took place in an era of selective contracting. Given that localness is an important dimension of health care, buyers in more concentrated pre-merger markets would have had a larger drop in the number of sellers post merger. This possibly would have given the post-merger hospitals more power. It is possible that this could have been offset by countervailing power from the smaller number of buyers in more rural areas, though as buyers tend to be the weaker group in the NHS (Hutchings et al 2003), this countervailing power may be relatively weak.

Which effect dominates is however an empirical matter and to investigate whether mergers in concentrated markets had more negative effects, we divided the sample of merged hospitals on the basis of pre-merger concentration. We measure market concentration by counting the number of hospital competitors within a 30km fixed radius two years before the merger occurs. We examine whether there were different effects for mergers in the most concentrated markets by interacting the time dummies with an indicator for the pre-merger market being in the top third of the distribution of market concentration. We examined all the indicators of activity, staffing, financial performance, productivity and quality examined in Tables 2 and 3.

Table 4 shows that there is less reduction in capacity post-merger in markets which were more concentrated pre-merger and that this had a knock on measures of throughput. Column (1) shows total admissions. These fell less in concentrated markets post-merger. The interactions with time post-merger for those hospitals located in more concentrated markets are all positive and in terms of magnitude are around two-thirds of the size of the time dummies for the full sample. This lower fall in admission is due to a lower fall in elective admissions. Column (2) shows there is no difference in the two groups for emergency admission. But the interaction terms for elective admissions in column (3) all rise post merger and are of a similar size to the fall in elective admissions for the full sample. This lower fall in capacity fed into waiting times. For the full sample, column (4) shows that the percentage of patients waiting for very long periods (180 days) rose. But the interaction terms indicate that hospitals in more concentrated markets had less increase in the proportion of patients

waiting over 180 days. Similar results are seen in all the waiting times measures (the others are not shown here).

However, this smaller decrease in capacity in more concentrated markets did not translate into higher labour productivity (column 5) or less of a fall in the financial surplus (column 6). In fact, there is some indication that the average growth in deficits for the full sample (Table 2, column 8) comes particularly from mergers in more concentrated markets. The coefficients on the interaction terms in column (6) are all negative and, the for the last year post-merger, significant at the 10 percent level. However, the size of our sample means this difference between the two groups is only weakly estimated. There is also no pattern of difference in clinical quality between the two groups (results not shown, available from authors).

It therefore appears that mergers in more concentrated markets led to less reduction in capacity and elective activity, with fewer negative effects on waiting times for elective care. As reduction in capacity is generally one of the desired outcomes of hospital mergers in England, this suggests that reconfiguration in already concentrated markets will be less effective in bringing about this outcome

(ii) Heterogeneity of merging parties in terms of size and financial position.

If there are economies of scale or scope, mergers between small hospitals may bring about more benefits than those between already large hospitals. If nothing else, a merger between two small hospitals may be easier to bring about than one between two large hospitals. In terms of financial position, mergers where both parties had large deficits may be are harder to recover from that one where one partner is relatively solvent. To examine this we identified hospitals where the combined size of the merged parties was large pre-merger (as measured by average bed size in the three years prior to merger) or where both merging parties have large deficits pre-merger (as measured by the average deficit in the three years prior to merger).[23] We did not find robust evidence of differences in performance post merger by pre-merger size or financial performance of the merging parties. We also examined heterogeneity by differences in size and deficits between the merging parties. Again these show no systematic differences. [24]

6. Conclusions

The literature on mergers between private hospitals suggests that such mergers often produce little benefit. Despite this, the UK government has pursued an active policy of hospital merger. These

[23] Note no NHS hospitals had large surpluses in this period and that the average hospital in our sample has a deficit of just under £1m.

[24] Results available from the authors.

consolidations are initiated by a regulator, acting on behalf of the public, and justified on the grounds that they will improve financial performance, productivity or patient care. We examine whether this promise is met by exploiting the fact that between 1997 and 2004 in England around half the acute general hospitals were involved in a merger.

We examine the impact of mergers on a large set of outcomes including financial performance, productivity, waiting times and clinical quality and find little evidence that mergers achieved gains. While admissions and staff numbers fell relative to the pre-merger position, which is desirable if the regulator wanted to remove spare capacity, labour productivity did not rise and financial deficits increased. And while most measures of quality were unchanged, there is no indication of an improvement in quality to offset this poorer financial performance. Further, in already concentrated markets, mergers brought about lower reductions in capacity. This suggests smaller gains in these markets.

We therefore conclude that there seems to be little hard evidence that this attempt at government planning of hospital care has achieved much more than simply reducing hospital admissions. This removal of capacity may reduce patient welfare. We show that waiting times rose post merger; travel distances may also rise when hospitals are closed. Consolidation also downstream reduces potential competition, which has been shown in the UK market to have some beneficial effects on patient outcomes and length of stay (Gaynor et al 2010, Cooper et al 2011, Bloom et al 2010). Given this, it seems the English government should carefully consider potential losses before allowing more mergers between short term general hospitals.

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Appendix

Definition and source of variables used in the analysis

Variable	Source	Definition	Years
Activity & staffing			
Total admissions	Hospital Episode Statistics (HES)	Patients admitted to the hospitals for elective and emergency treatments	1997-2006
Total staff	NHS Hospital and Community Health Services (HCHS)	Total medical and non medical qualified and non qualified staff employed in the hospital on 30 September of each year	1997-2006
Share of total staff that are med. qualified (%)	Department of Health: Trust financial returns; Published Income and Expenditure Accounts for Foundation Trusts	Total qualified medical and non medical staff over total staff	1997-2006
Share of exp. on management (%)	Department of Health: Trust financial returns; Published Income and Expenditure Accounts for Foundation Trusts	Total expenditure on (TFR3A5) NHS Senior Managers and Managers and (TFR3B180) non NHS Chairman & Non-Executive Directors Remuneration, divided by (TFR3B200) total revenue expenditure on salaries and wages.	1997-2006
Share of exp. on agency staff	Department of Health: Trust financial returns; Published Income and Expenditure Accounts for Foundation Trusts	(TFR3B170) Total non NHS staff salaries and wages divided by (TFR3B200) total revenue	1997-2006
Total beds	Department of Health KH03		1995-2006
Financial performance & productivity			
Surplus (£K)	Chartered Institute of Public Finance and Accountancy (CIPFA) and Monitor	Hospital surplus or deficit at the end of the financial year	1995-2006
Total expenditure (£K)	Department of Health: Trust financial returns; Published Income and Expenditure Accounts for Foundation Trusts		1995-2006
Admissions/staff (productivity)		Total admissions divided by total staff	1997-2006

cont

Clinical outcomes and quality indicators			
Mean length of stay (days)	Hospital Episode Statistics (HES)	Number of days from patient in hospital spell from admission to discharge	1997-2006
Mean waiting time (days)	Hospital Episode Statistics (HES)	Number of days from patient referral to admission to hospital for elective care	1997-2006
Share of list that waited >180 days (%)	Hospital Episode Statistics (HES)		1997-2006
AMI death rate within 30 days of discharge (%)	NHS: National Centre for Health Outcomes Development (NCHOD)	Patients aged 35 to 75	1998-2006
Stroke death rate within 30 days of discharge (%)	NCHOD	Patients aged 35 to 75	1998-2006
28 day readmission rate: stroke (%)	NCHOD		1998-2006
56 day return rate, stroke (%)	NCHOD		1998-2006
FPF Death rate within 30 days of discharge (%)	NCHOD	Patients aged 35 to 75	1998-2006
28 day readmission rate: fractured proximal femur (%)	NCHOD		1998-2006
28 day return rate, fractured proximal femur (%)	NCHOD		1998-2006
Area characteristics			
Median male wage (£K)	Office for National Statistics: Annual Survey of Hours and Earnings (ASHE)		1997-2006
Area SMR (per 100,000)	Office for National Statistics (ONS)	Inverse distance weighted average local authority age-standardised SMR	1997-2006
Share of marginal constituencies in 1997 (%)	British Election Study	Share of marginal constituencies falling within a 30 km radius of each hospital (location defined by postcode) in 1997	1997

Table A1: Probability of merger by year of merger

	1997	1998	1999	2000	20001	2002
Teaching	0.279**	-	-	-	-	-
	(2.27)					
Specialist	-0.0547	-	-	-	-	-
	(-1.18)					
SMR in the hospital catchment	0.000113	-0.000182	-0.000202	0.00101***	0.00119***	0.000185
area (per 100K)	(0.47)	(-0.40)	(-0.64)	(2.97)	(3.07)	(0.56)
Mean male wages in the	-0.0185***	-0.00163	-0.0210***	-0.0155	-0.0532***	-0.00397
hospital catchment area (1K)	(-3.01)	(-0.14)	(-2.74)	(-1.52)	(-4.43)	(-0.46)
Surplus or deficit (£100K)	-0.00319**	-0.00301	-0.00246	0.00550*	-0.00434	0.000197
(average over 2 years before	(-2.09)	(-1.30)	(-0.74)	(1.77)	(-1.38)	(0.29)
the year of merger)						
Total beds (100s)	-0.0312***	-0.0188**	-0.0191**	-0.00520	-0.00138	-0.00639
(average over 2 years before	(-2.99)	(-2.00)	(-2.46)	(-0.63)	(-0.14)	(-1.12)
the year of merger)						
Chi2	18.20	3.52	8.28	15.61	19.69	2.99
Observations	128	126	124	125	128	113
Merged hospitals	20	18	17	18	22	6
Never merged hospitals	108	108	107	107	106	107

Notes: *p<0.1, **p<0.005, ***p<0.01. Average marginal effects; t statistics in parentheses. Probit model with robust SE. Sample: Merged hospitals in their merger year only, never merged in the same year. For variable definitions see Appendix.

Table A2: Balancing tests from matching

Variable	Sample	Mean		%bias	% bias	t-test (p value)	
		Treated	Controls		reduction		
Teaching	Unmatched	0.059	0.140	-27.3		-2.27	(0.023)
	Matched	0.054	0.055	-0.6	98	-0.06	(0.951)
Specialist	Unmatched	0.0196	0.084	-29.3		-2.29	(0.022)
	Matched	0.0215	0.030	-3.8	86.9	-0.43	(0.668)
SMR in the hospital catchment area	Unmatched	718.92	698.06	33.1		2.91	(0.004)
	Matched	718.75	722.6	-6.1	81.5	-0.52	(0.602)
Mean log male wages in the hospital catchment area	Unmatched	19.16	20.46	-48.2		-4.17	(0.001)
	Matched	19.20	19.14	2.4	94.9	0.24	(0.807)
Surplus (100k) (3 year pre-merger average)	Unmatched	-2.661	-1.561	-9.6		-0.97	(0.331)
	Matched	-0.959	-2.843	16.5	-71.1	1.62	(0.106)
Total beds (100s) (3 year pre-merger average)	Unmatched	6.106	6.639	-19.2		-1.73	(0.084)
	Matched	6.133	6.387	-9.2	52.4	-0.84	(0.401)
Share of marginal constituencies in 1997 (%)	Unmatched	7.647	16.35	-50.3		-3.83	(0.001)
	Matched	7.944	14.89	-40.1	20.2	-2.69	(0.007)

Notes: For variable definitions see Appendix

Table A3: Activity, staffing, financial performance, productivity with all never merged hospitals as control group

	(1) Total admissions (log)	(2) Total staff (log)	(3) Total available beds (log)	(4) Total operating expenditure (log)	(5) Share of qualified staff (%)	(6) Share of total expend on managers (%)	(7) Share of total expend on agency staff (%)	(8) Financial surplus or deficit (£K)	(9) Productivity: admissions /staff (log)
Year -1	-0.012	-0.007	-0.023*	-0.012	-0.615*	0.127	0.056	-140.583	0.002
	(0.020)	(0.015)	(0.012)	(0.014)	(0.307)	(0.085)	(0.150)	(332.936)	(0.016)
Year of merger	-0.015	-0.039*	-0.030*	-0.032*	-0.665**	0.332**	0.031	-1167.538***	0.039*
	(0.022)	(0.020)	(0.018)	(0.017)	(0.324)	(0.138)	(0.198)	(349.113)	(0.021)
Year 1	-0.024	-0.089***	-0.069***	-0.102***	-0.683*	-0.125	0.135	-410.534	0.087**
	(0.021)	(0.031)	(0.020)	(0.023)	(0.346)	(0.143)	(0.250)	(355.119)	(0.034)
Year 2	-0.070***	-0.091***	-0.056**	-0.099***	-1.469***	-0.114	0.418	-1227.068**	0.049
	(0.020)	(0.028)	(0.024)	(0.019)	(0.548)	(0.136)	(0.280)	(571.254)	(0.030)
Year 3	-0.098***	-0.105***	-0.077***	-0.114***	-1.398***	-0.036	0.585**	-1206.427	0.040
	(0.025)	(0.029)	(0.023)	(0.037)	(0.435)	(0.145)	(0.281)	(790.125)	(0.035)
Year 4	-0.065***	-0.109***	-0.066**	-0.128***	-1.333***	-0.061	0.811***	-2170.008**	0.081**
	(0.020)	(0.029)	(0.025)	(0.031)	(0.478)	(0.149)	(0.274)	(1066.497)	(0.031)
F test	5.47	3.42	3.61	7.84	2.24	2.02	1.87	5.41	1.95
R^2	0.006	0.011	0.010	0.008	0.004	0.019	0.021	0.028	0.011
N	51	51	51	51	51	51	51	51	51
Observations	313	309	313	311	309	310	310	312	309

Notes: * Significant at 10%; ** Significant at 5%; *** Significant at 1%. Robust standard errors in parentheses. Estimates from linear panel models with hospital fixed effects. Sample: population of hospitals merged in 1997-2002. Dependent variable de-trended by using closest 3 never merged hospitals outside the market area of a merged hospital and weighted by their difference in the propensity score. Baseline: 2 years before the decision to merge is approved by the regulator. F-test of joint significance of the time dummies.

Table A4: Clinical outcomes and quality indicators with all never merged hospitals as control group

	(1) Mean length of stay per admission (log)	(2) Mean time waited per admission (log)	(3) Share of patients waited 180 days or more (%)	(4) 30 day mortality on or after discharge: AMI (log)	(5) 30 day mortality on or after discharge: stroke (log)	(6) 28 day readmission rate: stroke (log)	(7) 56 day return rate stroke (log)	(8) 30 day mortality on or after discharge: fractured proximal femur (log)	(9) 28 day readmission rate: fractured proximal femur (log)	(10) 28 day return rate: fractured proximal femur (log)
Year -1	-0.007	0.027	0.655	0.052	0.030	0.083	0.011	0.021	0.059	-0.022
	(0.015)	(0.023)	(0.652)	(0.046)	(0.032)	(0.060)	(0.015)	(0.037)	(0.056)	(0.020)
Year of merger	0.007	0.032	0.611	0.095**	0.035	0.122**	0.017	0.006	0.083	-0.003
	(0.015)	(0.030)	(0.855)	(0.045)	(0.029)	(0.055)	(0.014)	(0.037)	(0.054)	(0.021)
Year 1	-0.020	0.005	0.136	0.126**	0.050*	0.115**	-0.014	-0.064	0.035	-0.000
	(0.017)	(0.033)	(0.988)	(0.061)	(0.028)	(0.053)	(0.016)	(0.054)	(0.044)	(0.020)
Year 2	0.024	0.032	1.040	0.090*	0.076***	0.114*	-0.013	0.005	0.019	-0.013
	(0.024)	(0.035)	(1.054)	(0.049)	(0.027)	(0.066)	(0.016)	(0.050)	(0.053)	(0.022)
Year 3	0.016	0.066*	1.994*	0.101*	0.068**	0.062	-0.004	-0.009	0.025	-0.015
	(0.020)	(0.036)	(1.104)	(0.054)	(0.028)	(0.058)	(0.016)	(0.043)	(0.055)	(0.019)
Year 4	0.014	0.082**	2.464**	0.062	0.068*	0.080	-0.021	-0.047	0.075	-0.029
	(0.019)	(0.039)	(1.206)	(0.053)	(0.037)	(0.057)	(0.016)	(0.046)	(0.050)	(0.021)
F test	1.99	2.39	2.47	1.19	1.89	1.54	3.3`	1.27	1.00	0.84
R^2	0.004	0.007	0.018	0.001	0.005	0.007	0.015	0.016	0.006	0.002
N	51	50	50	48	48	50	48	48	48	48
Observations	313	308	308	279	279	282	279	277	277	277

Notes: * Significant at 10%; ** Significant at 5%; *** Significant at 1%. Robust standard errors in parentheses. Estimates from linear panel models with hospital fixed effects. Sample: population of hospitals merged in 1997-2002. Dependent variable de-trended by using closest 3 never merged hospitals outside the market area of a merged hospital and weighted by their difference in the propensity score. Baseline: 2 years before the decision to merge is approved by the regulator. F-test of joint significance of the time dummies.

Table 1: Difference between merged and non merged hospitals

Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Difference	t-test of difference
		Merged			Never merged			
Activity & staffing								
Total admissions	61	46,204.41	22,818.64	107	52,050.45	25,534.91	-5,846.04	-1.53
Total staff	60	2,253.00	864.67	107	2,400.99	1,203.64	-148.00	-0.92
Share of total staff that are med. qualified (%)	60	54.42	4.61	107	55.62	4.53	-1.20	-1.63
Share of exp. on management (%)	61	4.33	1.15	107	4.34	1.26	-0.01	-0.08
Share of exp. on agency staff	61	2.68	2.31	107	4.04	3.06	-1.36	-3.26
Total beds	61	618.69	214.05	107	653.09	285.19	-34.41	-0.89
Financial performance & productivity								
Surplus (£K)	61	-401.02	1,096.21	107	-557.19	1,511.68	156.17	0.77
Total expenditure (£K)	61	62,873.12	25,255.50	107	74,161.21	37,451.94	-11,288.09	-2.33
Admissions/staff (productivity)	60	20.07	5.47	107	22.22	7.71	-2.14	-2.09
Clinical outcomes and quality indicators								
Mean length of stay (days)	61	4.36	2.73	107	3.88	0.93	0.48	1.34
Mean waiting time (days)	60	90.41	28.60	105	91.94	30.42	-1.52	-0.32
Share of list that waited >180 days (%)	60	15.80	6.97	105	16.24	7.43	-0.44	-0.38
AMI death rate within 30 days of discharge (%)	52	9.87	2.20	93	10.03	2.71	-0.16	-0.38
Stroke death rate within 30 days of discharge (%)	58	28.29	6.41	97	29.30	5.10	-1.01	-1.02
28 day readmission rate: stroke	56	8.28	2.79	93	8.84	4.75	-0.56	-0.90
56 day return rate, stroke	57	49.97	5.77	97	48.08	4.89	1.89	2.07
FPF Death rate within 30 days of discharge	52	9.90	2.65	96	9.85	2.74	0.05	0.11
28 day readmission rate: fractured proximal femur	53	8.78	2.79	96	8.94	3.13	-0.16	-0.31
28 day return rate, fractured proximal femur	52	52.08	7.43	96	51.40	8.57	0.68	0.50

Table 1 cont

Area characteristics								
Median male wage (£K)	61	18.46	1.81	107	19.84	2.59	-1.37	-4.03
Area SMR (per 100K)	61	736.42	56.41	107	712.57	67.27	23.85	2.45
Share of marginal constituencies in 1997 (%)	61	7.84	10.37	107	16.37	22.77	-8.53	-3.32

Notes: Data for merged hospitals in for the year immediately prior to operation as one hospital. For variable definition see Appendix.

Table 2: Activity, staffing, financial performance and productivity

	(1) Total admissions (log)	(2) Total staff (log)	(3) Total available beds (log)	(4) Total operating expenditure (£K) (log)	(5) Share of qualified staff	(6) Share of total Expend on managers	(7) Share of total expend on agency staff	(8) Financial surplus or deficit(£k)	(9) Productivity: admissions /staff (log)
Year -1	-0.027	-0.011	-0.031**	0.002	-0.411	0.159	0.201	87.05	-0.014
	(0.023)	(0.018)	(0.014)	(0.016)	(0.343)	(0.095)	(0.176)	(347.665)	(0.016)
Year of merger (t = 0)	-0.041	-0.047**	-0.048**	-0.006	-0.354	0.346**	0.202	-1092.064**	0.016
	(0.027)	(0.023)	(0.022)	(0.020)	(0.373)	(0.160)	(0.226)	(407.53)	(0.023)
Year 1	-0.062**	-0.100***	-0.102***	-0.063**	-0.058	0.000	0.433	-516.780	0.052
	(0.027)	(0.034)	(0.023)	(0.025)	(0.410)	(0.151)	(0.283)	(414.51)	(0.039)
Year 2	-0.109***	-0.102***	-0.101***	-0.037*	-0.605	0.050	0.816***	-1523.508**	0.015
	(0.023)	(0.031)	(0.028)	(0.021)	(0.651)	(0.146)	(0.302)	(669.99)	(0.033)
Year 3	-0.136***	-0.125***	-0.133***	-0.052	-0.262	0.214	0.849***	-1549.415*	0.013
	(0.029)	(0.032)	(0.026)	(0.044)	(0.497)	(0.152)	(0.306)	(887.35)	(0.040)
Year 4	-0.111***	-0.117***	-0.120***	-0.041	0.089	0.137	1.149***	-2888.167**	0.037
	(0.027)	(0.035)	(0.030)	(0.037)	(0.531)	(0.166)	(0.311)	(1237.941)	(0.037)
F test	8.25	4.13	6.01	1.36	1.20	1.92	3.54	5.24	0.84
R^2	0.010	0.018	0.024	0.003	0.007	0.011	0.034	0.044	0.009
N	43	42	43	43	42	43	43	43	42
Observations	263	254	263	261	254	260	260	262	254

Notes: * Significant at 10%; ** Significant at 5%; *** Significant at 1%. Standard errors in parentheses Estimates from linear panel models with hospital fixed effects. Sample: population of hospitals merged in 1997-2002. Dependent variable de-trended by using closest 3 never merged hospitals outside the market area of a merged hospitals. Baseline: 2 years before the decision to merge is approved by the regulator. F-test of joint significance of the time dummies.

Table 3: Clinical outcomes and quality indicators

	(1) Mean length of stay per admission (log)	(2) Mean time waited per admission (log)	(3) Share of patients waited 180 days or more	(4) 30 day mortality on or after discharge: AMI (ages 35-74) (std) (log)	(5) 30 day mortality on or after discharge: stroke (std) (log)	(6) 28 day readmission rate: stroke (std) (log)	(7) 56 day return rate stroke (std) (log)	(8) 30 day mortality on or after discharge: fractured proximal femur (std) (log)	(9) 28 day readmission rate: fractured proximal femur (std) (log)	(10) 28 day return rate fractured proximal femur (std) (log)
Year -1	0.004	0.037	1.000	0.046	0.014	0.122*	0.024	0.024	0.027	-0.000
	(0.015)	(0.024)	(0.680)	(0.063)	(0.033)	(0.065)	(0.014)	(0.036)	(0.062)	(0.022)
Year of merger	0.009	0.047	1.264	0.081	0.020	0.162**	0.029*	0.039	0.058	0.011
	(0.019)	(0.032)	(0.908)	(0.061)	(0.030)	(0.060)	(0.014)	(0.038)	(0.056)	(0.023)
Year 1	-0.006	0.019	0.864	0.115*	0.043	0.142**	-0.008	-0.047	-0.005	0.009
	(0.019)	(0.033)	(1.027)	(0.058)	(0.030)	(0.061)	(0.016)	(0.048)	(0.045)	(0.021)
Year 2	0.047*	0.045	1.857	0.085	0.072**	0.159**	-0.006	0.013	-0.003	-0.001
	(0.028)	(0.036)	(1.135)	(0.058)	(0.030)	(0.070)	(0.016)	(0.043)	(0.058)	(0.024)
Year 3	0.030	0.069*	2.475**	0.056	0.061**	0.109*	-0.007	-0.006	-0.042	-0.006
	(0.024)	(0.036)	(1.173)	(0.065)	(0.030)	(0.057)	(0.014)	(0.040)	(0.059)	(0.020)
Year 4	0.030	0.096**	3.223**	-0.004	0.055	0.097	-0.020	-0.072	0.024	-0.007
	(0.023)	(0.040)	(1.293)	(0.063)	(0.039)	(0.063)	(0.015)	(0.044)	(0.053)	(0.022)
F test	1.88	2.14	2.51	1.36	1.68	1.64	4.01	2.24	1.40	0.28
R^2	0.005	0.005	0.019	0.009	0.008	0.013	0.026	0.018	0.006	0.001
N	43	42	42	43	42	44	42	42	42	42
Observations	263	258	258	248	243	245	243	243	243	243

Notes: * Significant at 10%; ** Significant at 5%; *** Significant at 1%. Standard errors in parentheses Estimates from linear panel models with hospital fixed effects. Sample: population of hospitals merged in 1997-2002. Dependent variable de-trended by using closest 3 never merged hospitals outside the market area of a merged hospitals. Baseline: 2 years before the decision to merge is approved by the regulator. F-test of joint significance of the time dummies.

Table 4: Heterogeneity by market concentration

	(1) Total admissions (log)	(2) Emergency admissions (log)	(3) Elective admissions (log)	(4) % patients waiting over 180 days	(5) Productivity: admissions /staff (log)	(6) Financial surplus (£K)
Year -1	-0.051	-0.037	-0.062**	1.952***	-0.014	-271.068
	(0.034)	(0.036)	(0.03)	(0.639)	(0.016)	(237.104)
Year of merger	-0.057	-0.059	-0.059*	2.630***	0.016	-1270.371***
	(0.036)	(0.045)	(0.034)	(0.927)	(0.023)	(468.983)
Year 1	-0.087**	-0.091*	-0.081**	2.572***	0.052	-653.777
	(0.038)	(0.048)	(0.034)	(0.949)	(0.039)	(485.261)
Year 2	-0.133***	-0.160***	-0.124***	3.581***	0.015	-1514.526*
	(0.032)	(0.04)	(0.034)	(1.048)	(0.033)	(820.495)
Year 3	-0.176***	-0.229***	-0.182***	4.076***	0.013	-779.226
	(0.039)	(0.046)	(0.044)	(1.115)	(0.040)	(1149.02)
Year 4	0.145***	-0.189***	-0.142***	5.026***	0.037	-948.149
	(0.034)	(0.044)	(0.042)	(1.192)	(0.037)	(781.948)
Interactions with top quartile of pre-merger concentration						
Year -1	0.067	-0.032	0.067	-2.675*	-0.014	1001.43
	(0.041)	(0.059)	(0.05)	(1.55)	(0.016)	(835.389)
Year of merger	0.043	0.039	0.093*	-3.719*	0.016	835.884
	(0.045)	(0.064)	(0.05)	(1.889)	(0.023)	(957.243)
Year 1	0.070*	-0.026	0.120***	-4.646**	0.052	793.315
	(0.041)	(0.052)	(0.042)	(2.266)	(0.039)	(1063.37)
Year 2	0.069*	0.005	0.107**	-4.497*	0.015	282.321
	(0.038)	(0.046)	(0.045)	(2.529)	(0.033)	(1469.1)
Year 3	0.124**	0.059	0.209***	-4.159	0.013	-2381.91
	(0.048)	(0.051)	(0.054)	(2.612)	(0.040)	(1635.77)
Year 4	0.101**	0.019	0.194***	-4.769	0.037	-6172.275*
	(0.048)	(0.053)	(0.057)	(2.912)	(0.037)	(3495.89)
F test	8.90	23.77	8.28	3.18	0.84	2.94
R^2	0.02	0.024	0.025	0.011	0.009	0.14
N	43	43	43	42	42	43
Observations	263	263	263	258	254	262

Notes: * Significant at 10%; ** Significant at 5%; *** Significant at 1%. Standard errors in parentheses Estimates from linear panel models with hospital fixed effects. Sample: population of hospitals merged in 1997-2002. Dependent variable de-trended by using closest 3 never merged hospitals outside the market area of a merged hospitals. Baseline: 2 years before the decision to merge is approved by the regulator. F-test of joint significance of the time dummies.

Figure 1: Number of merging hospitals by year

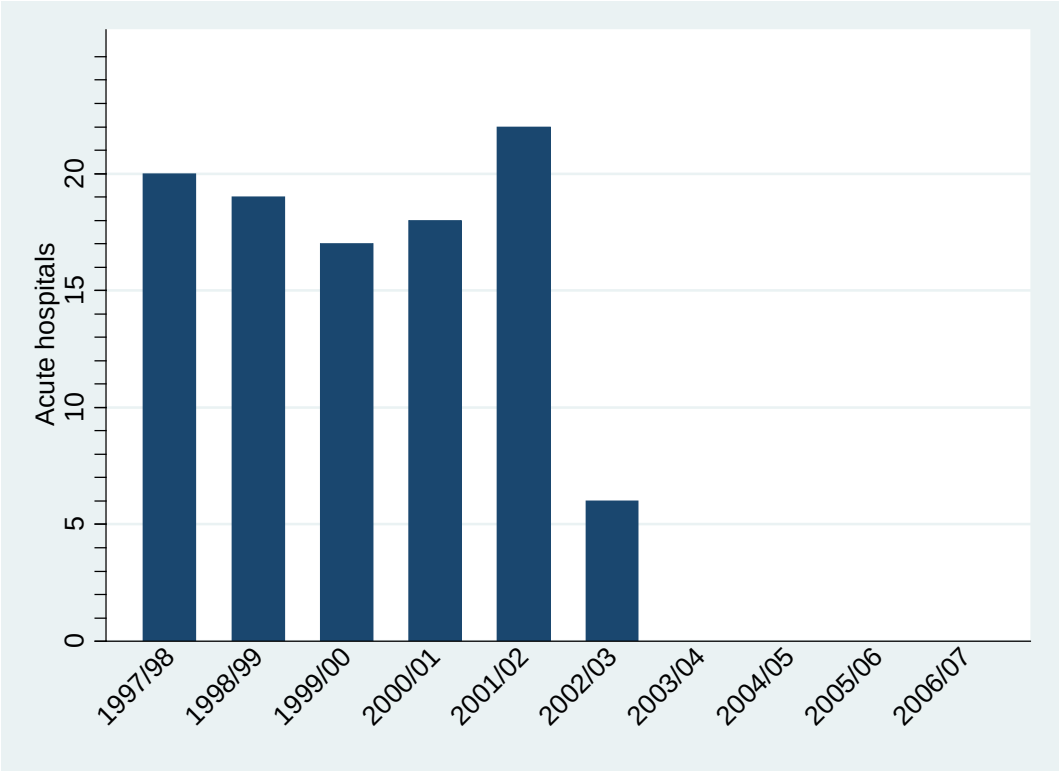


Figure 2: Location of merged and never merged acute hospitals in England 1997-2006

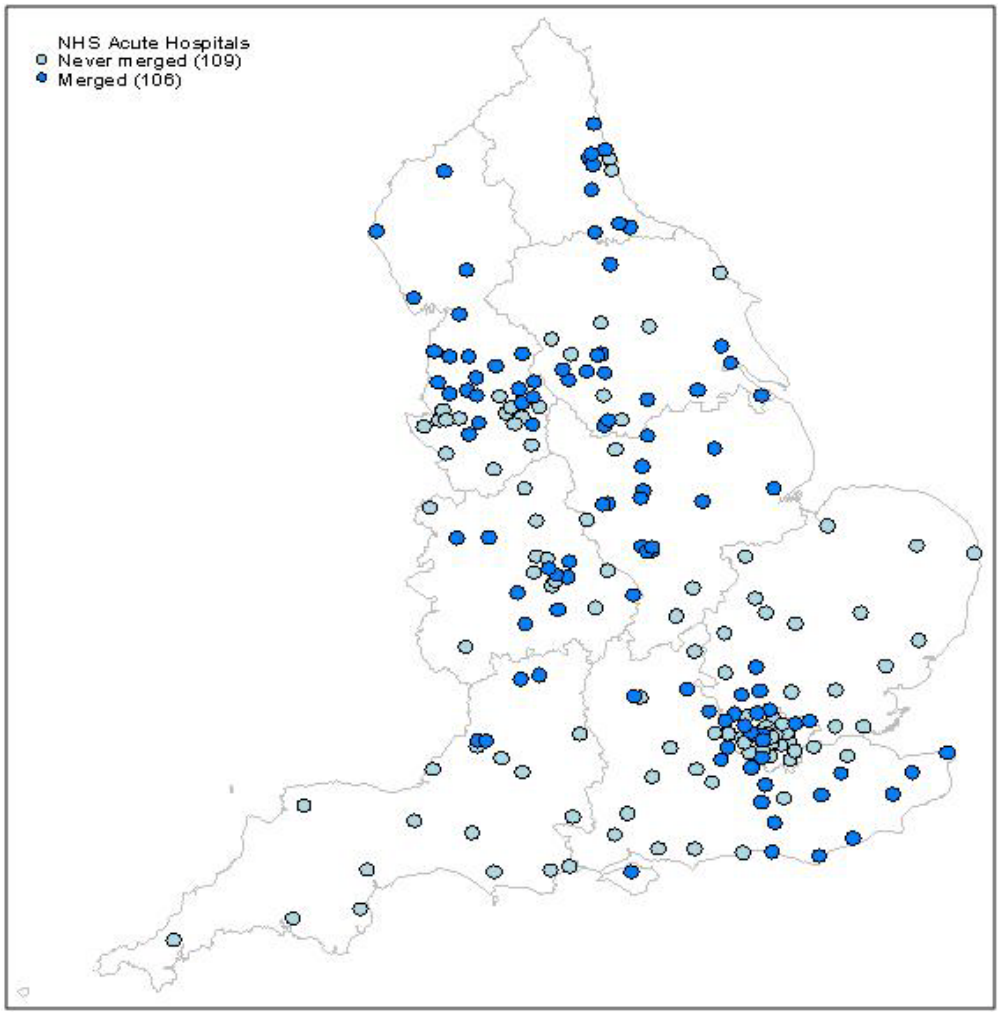
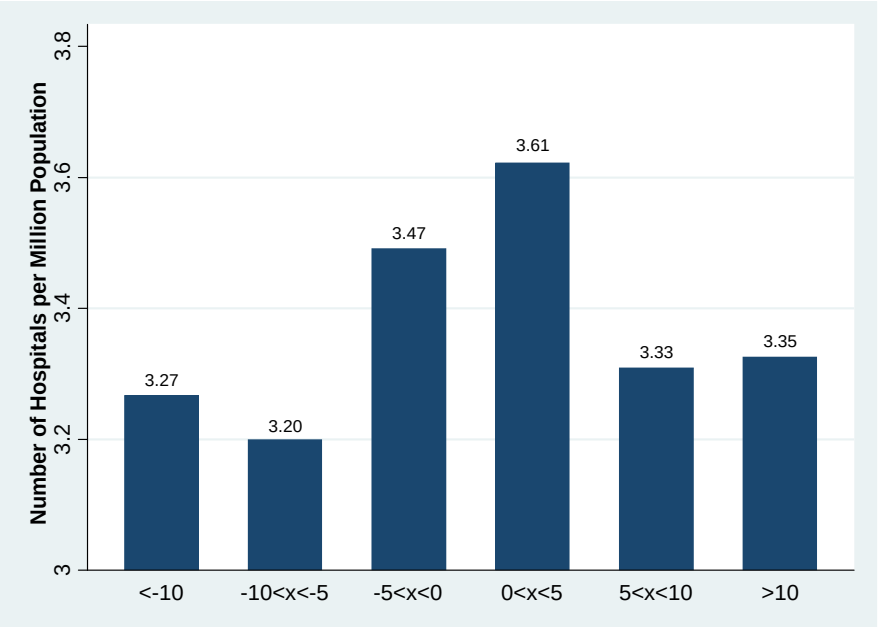
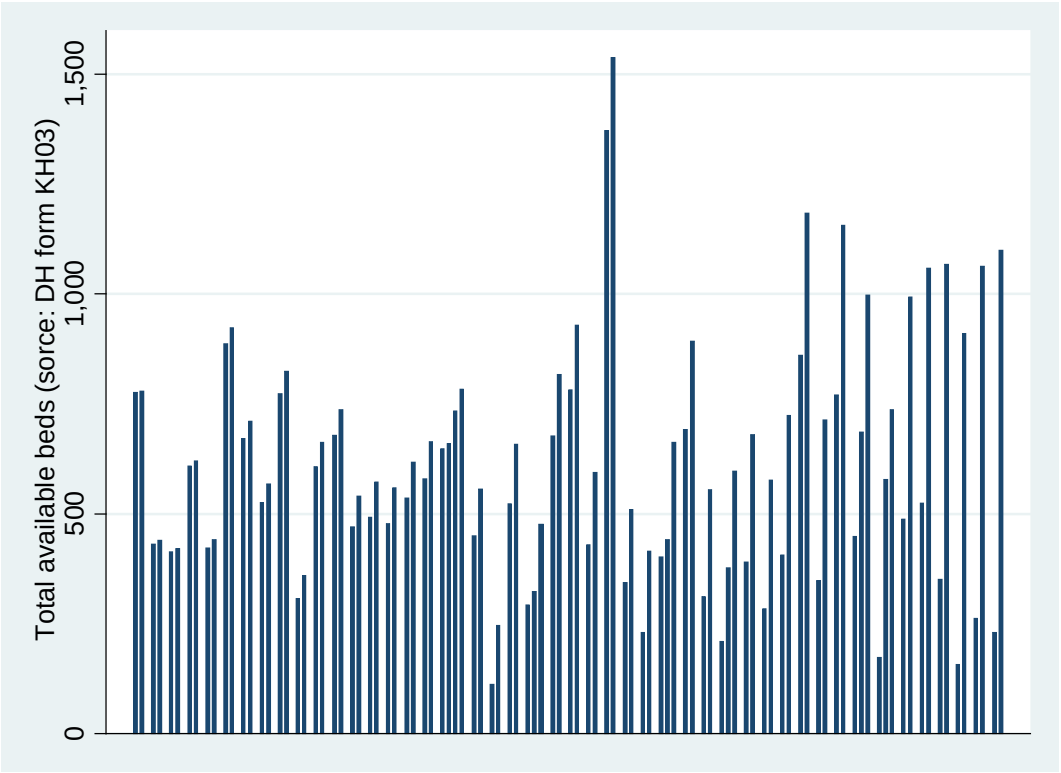


Figure 3: Governing Party's (Labour) Winning Margin and the Number of Hospitals in a Political Constituency



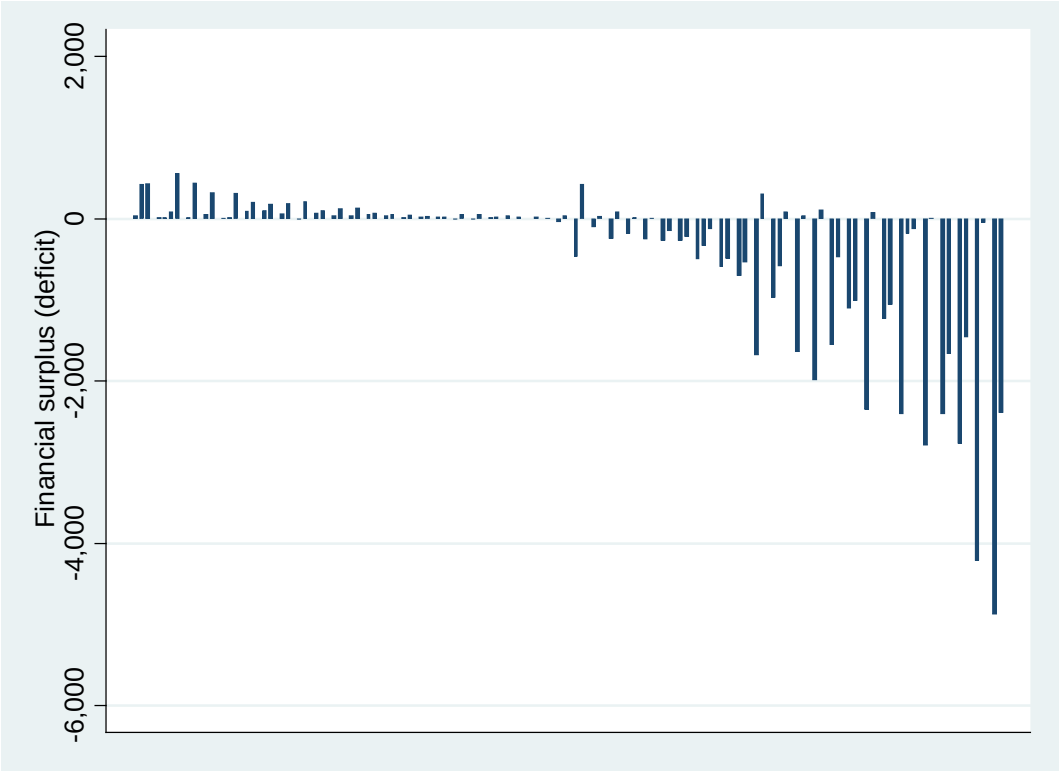
Notes: This figure plots the mean number of hospitals per 1 million people within a 30km radius of the centroid of a political constituency against the "winning margin" in 1997 of the governing party (Labour). When Labour is not the winning party, the margin is the negative of the difference between the winning party (usually Conservative) and the next closest party. The margin is denoted "x". There are 528 political constituencies in England. Source: Bloom et al (2010).

Figure 4: Total beds of each hospitals in a merger sorted by increasing standard deviation in the total beds of the hospitals pre merger



Note: Hospital total beds calculated as the average over year -1 and year -2

Figure 5: Surplus of each hospital in a merger sorted by joint surplus pre-merger.



Note: Hospital surplus calculated as the average over year -1 and year -2.