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ORIGINAL ARTICLE

Do mergers really increase output? Evidence from English hospitals

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ABSTRACT

The English hospital sector underwent a major restructuring program between 2000 and 2008 to centralize activity in fewer and larger hospitals. The aim of this paper is to evaluate the effects of such consolidations on hospital outputs. As mergers occurred in a staggered way, treatment could start and end at every time and treatment duration varied over the years. As every time is a mix of hospital pre-treatment, treatment and post-treatment phases, the canonical difference-in-differences assumption of homogeneous policy effects is not only meaningless but also misleading, raising doubts about the appropriateness of the methods previously used in this literature and consequently the accuracy of its results. We instead adopt a new matching and difference-in-differences approach, the flexible conditional difference-in-differences approach, developed by Dettmann et al. in 2020, more appropriate for causal analysis of treatments characterized by varying start dates and varying treatment duration. Our results suggest that mergers downsize hospital activities, especially the most expensive ones. If the goal of hospital mergers is to gain efficiency by centralization of activity, our findings suggest this restructuring programme is not the most successful policy to pursue. Mergers reduce the scope for competition between hospitals and do not create any incentive for poorly performing hospitals.

KEYWORDS
Hospital mergers, organizational changes, organizational processes

1 INTRODUCTION

Over the past fifty years, there have been marked changes in organizational structures and budgetary arrangements in the English National Health Service (NHS) causing, among other things, widespread merger activity in the hospital sector that has significantly reshaped the health system. Several waves of hospital consolidation have dramatically reduced the number of providers operating in England from about 400 in 1960, serving an average population of 100,000 people, to about 145 in 2020, serving an average population of 450,000 people. Such reshaping of course has posed questions of quality of services provided, performance of hospital providers, efficiency in terms of economies of scale and scope.

In the context of these re-organizational changes, we want to investigate whether merging activity has had any significant effect on hospital output. In particular, we refer to horizontal mergers between neighboring providers carrying out similar services to overlapping or contiguous population in order to rationalise the offered services (Collins, 2015), as "horizontal merger is one route through which a firm can acquire dominance over the supply of goods or services in a market" (Goddard & Ferguson, 1997, p. 15).

According to the economic theory of industrial organization, horizontal mergers decrease competition, tend to create a monopoly and raise prices, but may also offer an opportunity to rationalize production, to shift production from high-cost facilities to low-cost ones and to improve productive efficiency if firms are capable of generating technological synergies (Farrell & Shapiro, 1990). As efficiency of a firm depends not only on the degree of market competition (Hay & Liu, 1997), but also on the degree of monopoly power within the firm (Coase, 1937; Williamson, 1968), it is thus fair to ask to what extent horizontal mergers, as a way to acquiring monopoly power, are a potential problem in a healthcare system.

The industrial organization literature applied to the healthcare market offers several explanations about the driving forces behind merger activity and the impact of mergers on the performance of hospitals. First, as implementing cost-saving mechanisms is costly in terms of both time and effort (Goddard & Ferguson, 1997), the existence of monopoly power is likely to discourage hospital management from taking action to operate efficiently (Schmidt, 1997). Moreover, the pricing regime in the English NHS, which allows hospitals to cover costs plus an allowance for the rate of return on capital (Goddard & Ferguson, 1997), provides further incentives for those in monopoly positions to put less effort into restricting costs (Propper, 1995), as monopolistic competition allows to transfer cost increases from providers onto purchasers in the form of higher prices (Brooks, 1961). Finally, the reconfiguration of hospital services due to hospital mergers and closures will have implications for patients in terms of ease and cost of access (Fulop et al., 2012).

Therefore, "whether mergers can be expected to deliver benefits overall to patients depends largely on the incentives generated for improving efficiency" (Goddard & Ferguson, 1997, p. 19).

In effect, hospitals may have used mergers as a strategic tool to improve their financial performance through price increases (made possible by increased market power) and/or cost reductions (made possible by either economies of scale and scope or monopsony power or favorable adjustments in the product mix), with important policy implications in terms of both services provided to patients and use of available resources (Goddard & Ferguson, 1997). In theory, then, hospital mergers should enhance efficiency and quality: cost reduction, output increase, quality improvement, operating and managerial efficiency enhancement, reinforcement of financial sustainability, simplification of staff recruitment. Nevertheless, these benefits have not been always achieved (Gale, 2015). Indeed, for instance, the management savings from NHS mergers have been highly shifting and sometimes much lower than expected or they have been more likely to injury finances of hospitals than improve them (Gaynor et al., 2012); or the process of staff recruitment has not been made easier after the merging process (Fulop et al., 2005).

A wide empirical literature has investigated the possible consequences of hospital mergers. Some authors have recognized hospital reorganization (either through mergers or acquisitions) as possible trigger to change the mix of services offered (Krishnan et al., 2004) or as a tool to gain efficiency (Dranove, 1998; Preyra & Pink, 2006; Kjekhus & Hagen, 2007; Spang et al., 2009; Kristensen et al., 2010), while others have focused on the impact of hospital mergers on prices (Spang et al., 2009) or costs (Azevedo & Mateus, 2014; Schmidt, 2017). Ambiguous results have been gathered from the analysis of potential effects on welfare (Calem et al., 1999; Town et al., 2006) or quality of the services provided (Ho & Hamilton, 2000; Propper et al., 2004), although Bloom et al. (2015) suggest that higher management quality and improvement of performance have derived from higher competition results. Additionally, several papers have highlighted how the impact of competition in markets with fixed prices has led to improvements in hospital performance (Gaynor, 2004) or in hospital quality and efficiency (Propper et al., 2008; Propper, 2012; Bloom et al., 2015). However, other studies report mixed results. Propper et al. (2004) show that in the UK competition reduces quality (due to increased death rates), although it has a positive effect on waiting times (Propper et al., 2008), Gaynor et al. (2013) find that competition saves lives without raising costs, while Cooper et al. (2011) show that increased competition between private and public hospitals decreased productivity among the public sector while it increased in the private sector. A more recent analysis of the impact of mergers between NHS hospitals on financial performance, productivity, waiting times and measures of clinical quality found little evidence of improvement in any of these areas and, on some measures, performance even declined, producing little benefits in terms of patient welfare (Gaynor et al., 2012).

Despite its relevance in the literature, evidence of the success of hospital mergers in terms of delivering benefits it promised, or indeed, any benefits at all, remains a contested issue. The ambiguity of the results casts doubt both on the selection of an appropriate control group and on the methodological strategy applied. Regarding the first issue, matching models or instrumental variable approaches should lead the choice, especially when the treatment is voluntary as in the case of hospital mergers. Regarding the second issue, most of the cited studies adopted a standard difference-in-differences (DID) approach to evaluate the average treatment effect for the treated (merged hospitals) conditional on observable characteristics of the untreated (non-merged hospitals). As voluntary mergers are de facto planned decisions that can be easily anticipated, standard DID models should be at least modified to account for possible anticipation effects, for example by event study design, modification feasible if the announcement date of the merger is known, as in Gaynor et al. (2012). However, when the decision to merge is voluntary, it takes place in different years, and its duration is variable, the adoption of event studies is not effective as it requires generalized forms of parallel trends assumption, no anticipation effect of treatment and no variation in treatment effects across groups, at the same time. Some recent papers (among others, Athey & Imbens, 2018; Imai et al., 2020; Callaway & Sant'Anna, 2020; Abraham & Sun, 2020; Dettmann et al., 2020) have overcome these difficulties with an alternative DID approach based on the assumption of heterogeneous treatment.

Our paper extends the existing literature in several directions. From a methodological perspective, we implement the innovative empirical strategy proposed by Dettmann et al. (2020) to estimate the average treatment effect for the treated with time-varying treatment and variable duration, combined with Cerulli and Ventura (2019) to test the conditional parallel trend assumption, selecting a certain number of pre- and post-intervention periods (i.e., time leads and time trends). Second, unlike the empirical literature above discussed, we expand the empirical analysis of the effects of hospital mergers with the study of potential substitution effects between elective and emergency admissions, inpatients and outpatients, daycases and elective admissions following hospital mergers aimed at improving efficiency. Third, in line with some of the empirical papers above mentioned (Propper et al., 2008; Gaynor et al., 2012; Bloom et al., 2015), we focus on the consequences of the reforms in favor of competition. Nonetheless, we depart from those papers by focusing on the effects of hospital mergers on hospital outputs and efficiency, not on quality and performance. Finally, in line with Gaynor et al. (2012), our empirical context of hospitals offers a unique advantage: all English public hospitals operate in a homogeneous market—that is, offer services to the same customers and are subject to the same regulations. This allows us to extend the prior literature and more clearly identify the impact of hospital mergers on hospital outputs, modelling hospital heterogeneity through hospital-specific characteristics and controlling for market competition.

Our goal is to assess the impact of re-organizational changes on hospital efficiency. We do so by identifying the impact of hospital mergers on hospital outputs and by examining whether hospital mergers occurring at different years have had any significant effect on hospital outputs estimating the average treatment effect for the treated with time-varying treatment and variable duration. In order to capture some evidence of merger hospitals in terms of efficiency, we address a policy evaluation of merger effects on several measures of hospital activities, such as inpatient spells, elective admissions, emergency admissions, outpatients, daycases and various combinations of them. We analyze hospital mergers occurring in the English NHS at different years over the period 2000–2008.

The rest of the paper is organized as follows. We describes the institutional background and explain organizational changes among hospitals in England in Section 2. In Section 3 we discuss our empirical methodology. In Section 4 we describe our data. In Section 5 we present and discuss our empirical results. Section 6 concludes.

2 | INSTITUTIONAL FRAMEWORK

Since its establishment in 1948, the English NHS has provided healthcare services free at the point of use, funded out of general taxation.

During the 1980s the Conservative Party, which won the national election in 1979, started a process of transformation in each public sector introducing some private-sector economic mechanisms. The healthcare sector goal of the Thatcher's governments (from 1979 to 1990), was to improve the quality of the healthcare services by reducing the monopolistic power of the public healthcare sector and introducing some forms of competition. In particular, several market-oriented changes were set out, from the imposition of a managerial organization to the introduction of audits, inspections and other monitoring practices to measure the performance of the public healthcare sector and their staff (Dorey, 2015).

In order to improve primary healthcare services, in 1987 the government published the "Promoting better health" white paper and set out its intentions to raise standards of care, promote health and prevent illness, increase competition and give the public a greater choice, and improve value for money (Wilson, 1987). In order to give patients better healthcare and greater choice and generate greater satisfaction, in 1989 the government published the white paper "Working for patients" which introduced a split between the bodies who provided care (hospitals) and those who purchased it (health authorities), creating an internal market in the NHS. The internal market, established by the National Health Service and Community Care Act 1990, instituted several types of NHS trusts: hospital trusts, mental health trusts, ambulance services trusts and community health trusts. Hospital trusts, also known as acute trusts, provide secondary healthcare services.[1]

The separation between providers and purchasers should have allowed the latter to freely invest their budget to obtain the best services offered by the vast audience of providers, expected to compete on the supply of services. "Competition was therefore expected to provide the incentives for efficiency and responsiveness through decentralized decision-making, rather than relying on central control and planning" (Goddard & Ferguson, 1997, p. 8). Competition between providers failed instead because they reacted unexpectedly to the provider–purchaser split. Despite the recognized importance of preserving a certain level of supply-side competition, providers began to offer services jointly, leading first to a concentration of services and later to formal mergers. The number of hospitals fell dramatically (and continues to fall nowadays) with fewer and larger hospitals becoming the norm. In some areas the increasing concentration of hospitals lessened the competition principle advocated by the internal market reform.

The Labour Party, which won the national election in 1997, had campaigned against the internal market, but none of Blair's governments (from 1997 to 2007) abolished it. Instead, whilst leaving services free at the point of use, the Labour government encouraged outsourcing of medical services and support to the private sector, and pursued measures to strengthen the internal market as part of its plan to "modernize" the NHS. Specifically, the NHS Plan, published in 2000, promoted closer relationships between the private sector and the NHS, encouraging collaborative working in elective, critical and intermediate care and developing diagnostic and treatment centres in partnership with the private sector, aimed at reducing waiting times and waiting lists.

In practice, the competition introduced by the Conservative governments as leverage of enhancing efficiency and improving quality in the NHS was de facto replaced by the idea of collaboration between the private sector and the NHS supported by the Labour governments. The new government also declared its intention to consider hospital mergers as a way to achieve financial savings and to manage costs, and ultimately to gain efficiency and improve quality (Goddard & Ferguson, 1997). However, while during the 1980s and the 1990s hospital mergers were a "spontaneous" reaction to the provider–purchaser split, in the 2000s mergers were induced by the government to cut healthcare costs. Regardless of whether hospital mergers are primarily due to the government or the hospitals themselves, the institutional context allows us to test whether there are any

[1] From here onwards, we use "hospital" and "trust" interchangeably, especially when required for the precision of institutional discussion.

differences between merged and non-merged trusts and whether these arise due to the organizational changes brought by merger policy.

3 | METHODOLOGY

We use a DID methodology to test whether there are any significant differences between merged and non-merged trusts following the restructuring program, whether the policy has made any difference at all or whether indeed there are long-standing pre-treatment differences in hospital outputs between trusts, which have made some of them more likely to merge than others.

One of the main challenges in evaluating hospital mergers is the ability to draw firm conclusions based on the comparison between merged and non-merged trusts, when the decision to merge is voluntary, it takes place in different years, and its duration is variable. Allowing for potential selection bias associated both with the voluntary decision and to a variable treatment effect over time is, therefore, a key component of our research, and we describe below our approach to this.

We use the flexible conditional DID approach (Dettmann et al., 2020), a modification of the matching and DID approach of Heckman et al. (1998) for the staggered treatment adoption design (as in Callaway & Sant'Anna, 2020), where units that are treated once in the observation time are regarded as treated units from that date onwards and where time is defined in relation to the treatment start. Flexibility is gained in three ways: including individual treatment time information from the panel into the matching process; introducing a combined statistical distance function for matching; and incorporating flexible observation durations into the DID estimation (Dettmann et al., 2020, p. 1). Flexibility ensures that variation in treatment timing and variable treatment effects can be properly accounted for in an appropriate way and that the point in time an individual (treated) is compared to his matched counterpart (untreated) can be exactly determined, even when treatment is administered in a staggered way.

As the flexible conditional DID approach is a combination of propensity score matching (PSM) and DID methodology, the conditional independence assumption for matching and the common trend assumption for DID are replaced by the conditional parallel trend assumption (as proposed by Callaway & Sant'Anna, 2020): unobservable individual characteristics must be invariant over time for units with the same observed characteristics (Dettmann et al., 2020, p. 9). Also, as for PSM, the common support condition must be satisfied (as suggested by Callaway & Sant'Anna, 2020). Additionally, the approach assumes no spillover effects (due to the assumption of constant value of unit treatment for matching), and that potential carryover effects do not influence the matching variables at the matching time (as suggested by Imai et al., 2020). The last assumption, usual for the staggered adoption design, is also referred to as Irreversibility of Treatment (as proposed by Callaway & Sant'Anna, 2020): if a unit receives a treatment, it is regarded as treated unit for all the following time periods.

The flexible conditional DID approach is a two-step process. In the first step (pre-processing) the original data set is rearranged in individual selection groups for every treated unit. Potential controls for every treated unit are limited to those observed just at the individual matching date, for example the treatment start. The matching algorithm selects one or more matched counterparts among these pre-selected units. For example, if a trust is the result of a merger in 2001, we consider its characteristics in this year and assign a trust which has similar characteristics in 2001. In this first step, the observation time of both the matching variables and the outcomes is normalized such that they are measured with respect to the individual treatment start. Also in this first step, the individual identifier (trust id), the treatment variable (merger policy dummy), the time variable (year) and all the variables used for exact matching and matching must be specified, where exact matching creates matched sets for the treated units and matching refines the matched sets based on pre-treatment outcome and additional covariates. Finally, a relative time specification (in relation to the treatment start) that defines the time of matching must also be defined in the pre-processing. This time specification identifies when the matching process is conducted. The result of the first step is a temporary data set with essential information for the second step.

In the second step (estimation), based on a matching process that allows to eliminate systematic differences between treated and untreated, the average treatment effect for the treated (ATT) is estimated conditional on observable characteristics. Within the framework of the conditional DID model, usually the mean outcome developments in the treated and the control group are compared. Unlike the standard DID model, the flexible conditional DID model compares individual differences in outcome development between treated (merged trusts i) and untreated (non-merged trusts j). In this step, the individual identifier (trust id), the treatment variable (policy dummy), the time variable (year) and all the variables used for the estimate must be specified. Also the distance function used for matching and the period of outcome development, defined in relation to the treatment start or end, must both be selected.[2]

The flexible conditional DID estimator developed by Dettmann et al. (2020) is built on the estimate of group-time average treatment effects with the number of groups equal to the number of treated observations and respective group sizes of one. Single group-time estimators are then summarized in a simple weighted average with respective group weights of one. Control observations are individually selected for every treated unit and outcomes are individually compared. As the matching procedure proposed by Dettmann et al. (2020) gives equal weights to each included covariate, the statistical distance function returns a straight description of the similarities and disparities regarding the individual covariates, and the overall estimator reflects the unbiased comparability of unweighted observations.[3]

The estimator, defined as the mean of the individual comparisons, is given by the following equation:

$$ATT = \frac{1}{N}\sum_{i=1}^{N}[(Y_{i,t_{0i}+\beta_i} - Y_{i,t_{0i}}) - (Y_{j,t_{0i}+\beta_i} - Y_{j,t_{0i}})], \tag{1}$$

where i is the ith treated unit, with $i = 1, \dots, N$, j is the jth untreated unit matched with the ith treated unit, t is the tth year, with $t = 1, \dots, T$, and Y is the outcome. The estimator (1) includes individual treatment start dates, t_{0i}, and a flexible number of years, $t_{0i} + \beta_i$, reflecting the unit-specific duration from treatment start to outcome observation. Due to heterogeneous treatment durations, the observed periods may be heterogeneous among the treated individuals. The average treatment effect for the treated is thus a weighted average of different observation periods.

As a robustness check, we present a standard fixed effect DID model in which we compare the change in output for merged trusts before and after the restructuring program with the change in output for trusts in the comparator group that is not undergoing the intervention.

[2] Further technical details on outcome development in relation to treatment start or end are provided by Dettmann et al. (2020, pp. 15, 16).

[3] Further details on matching and distance function are available in Dettmann et al. (2020, pp. 10, 15).

To identify the average effect of the restructuring program on hospital output, we estimate the following model:

$$y_{it} = \beta_0 + \beta_1 M_i + \sum_{t=1}^{9} \beta_{2t} D_{it} + \sum_{t=1}^{9} \delta_t M_i D_{it} + \sum_{t=1}^{9} \sum_{k=1}^{13} \beta_{3k} X_{kit} + \mu_i + \epsilon_{it}, \tag{2}$$

where y_{it} is the output measure for trust i in year t where t covers nine years from 2000 to 2008; M_i is a dummy variable for the merger where $M_i = 1$ is the trust is the result of a merger and 0 otherwise; D_{it} is count dummy variable with relative difference to treatment start. X_{kit} is the kth observable time-variant factor (inputs, controls, hospital characteristics) affecting our dependent variable for trust i in year t.

The merger main effect M_i controls for all time-invariant differences between the treated and the control group. The count dummy D_{it} controls for all other unobserved temporal factors affecting the dependent variable. The interaction between D_{it} and M_i dummies identifies the change in trust output for merged trusts relative to untreated trusts (i.e., the ATT). The effect of the policy intervention on merged trusts is tested by checking whether the DID coefficient δ_t is significantly different from zero.

The DID methodology assumes that all other temporal factors affecting the dependent variable have the same effects for treated and untreated. Due to possible heterogeneity in output at trust level, we use trust-specific dummies to control for trust fixed effects. We also include year dummies to control for time fixed effects and all other unobserved temporal factors affecting our dependent variable.

3.1 | Conditional parallel trend assumption

According to the parallel trend assumption (PTA), unobservable individual characteristics must be invariant over time for units with the same observed characteristics (Callaway and Sant'Anna, 2020; Dettmann et al., 2020). Thus, any possible anticipation of the treatment (i.e., the anticipation effect) is only related to "eventually treated" groups.[4]

Conditioning on covariates X, the average outcomes for the group first treated in a period t and for the "never-treated" group would thus follow parallel paths in the absence of treatment (i.e., conditional PTA based on a "never-treated" group; Callaway and Sant'Anna, 2020, p. 8). This assumption is particularly important where there are covariate-specific trends in outcomes over time and when the distribution of covariates is different across groups.

To test the conditional PTA, we estimate a modified version of (2) (as in Cerulli & Ventura, 2019, p. 556):

$$y_{it} = \beta_0 + \left\{ \sum_{t=1}^{9} \delta_{t-l} D_{it-l} \right\}_{l=F}^{L} + \sum_{t=1}^{9} \sum_{k=1}^{13} \beta_{1k} X_{kit} + +\mu_i + \epsilon_{it}, \tag{3}$$

where F is the outcome development (post-treatment time) and L is the pre-treatment time, with $F \geq l \geq L$. With respect to (2), D_{it} is now a dummy variable with relative difference to treatment start defined on l.

[4] Note that the assumption of no anticipation effect can be imposed by setting $\delta = 0$, as in Callaway and Sant'Anna (2020).

TABLE 1 Hospitals by their merging status over time; England, years 2000–08

Year	Total number of trusts	Number of merged trusts	Number of merging trusts	% merged trusts	% merging trusts
2000	195	0	19	0%	10%
2001	186	10	21	5%	11%
2002	175	10	6	6%	3%
2003	172	3	0	2%	0%
2004	172	0	0	0%	0%
2005	172	0	2	0%	1%
2006	171	1	3	1%	2%
2007	169	1	0	1%	0%
2008	169	0	0	0%	0%

In order to test the conditional PTA implied by (3), two tests must be performed: one using time leads and one using time-trend. If both $\delta_{t-l} = 0$ with $l = L$ hypothesis and $\delta_{t-l} = 0$ with $F \geq l \geq L$ hypothesis are not rejected, we can conclude that both tests are passed and therefore that there is no anticipation effect (Cerulli & Ventura, 2019).

4 | DATA

Our data is longitudinal, available annually for a period of nine years from 2000 to 2008.[5] It contains information on all acute and specialist trusts in England with a unique identifier for each trust. Our unique data set combines information from several data sources: administrative data providing information on activity, expenditure, resource use, performance and staffing, as well as hospital characteristics, extracted and/or derived from the Hospital Episode Statistics (HES), the Hospital Activity Statistics (HAS), the NHS Foundation Trust Directory, the Medical and Dental Workforce Census (Department of Health), and from individual trusts' websites.

The data set contains 1,581 observations for: 195 hospitals in year 2000, 186 in year 2001, 175 in 2002, 172 in years 2003, 2004 and 2005, 171 in 2006 and 169 hospitals in years 2007 and 2008. According to Table 1, the number of hospital providers in England has decreased by 13%, from 195 acute and specialist trusts in 2000 to 169 in 2008.

4.1 | Variable definitions and measurements

4.1.1 | Dependent variables, policy variable and controls

In order to account for all hospital services provided and their possible combinations, we consider a very large set of hospital output measures. In particular, our dependent variables are: the number of inpatient spells, the number of elective admissions, the number of emergency admissions, the number of patients attending an outpatient appointment for the first time, the number of patients

[5] Years represent financial years that, in the UK, run from 1 April until 31 March of the next year: that is, the year 2000 in our data covers the period 1 April 2000 to 31 March 2001.

attending the A&E department for the first time, and the number of daycases.[6] The output analysis is completed with three more dependent variables built on a selection of the above variables: the ratio of elective and emergency admissions, the ratio of inpatients and outpatients, and the ratio of daycases and elective admissions. These extra variables will be used to assess if and how hospital mergers alter the combination of services provided. As trusts differ mostly in the volume of services provided, rather than the decision to provide a service at all, we will focus on the intensive margin of the degree of providing a service, which we will measure by a log-transformation of the dependent variables.

To assess the impact of the restructuring program associated with hospital mergers on our output measures, we construct a dummy variable for hospital merger status. Specifically, the policy variable Merged equals 1 from the year the newly merged trust is established onwards, and zero otherwise.

To account for other variables that may be correlated with our output measures, we control for hospital inputs, various control variables and hospital characteristics.

First, we include the number of available acute beds to control for overall hospital capacity, the number of operating theatres to control for the trust size, and the share of medical staff on total (medical and non-medical) staff to account for the labor force involved in hospital services.

To account for differences in the complexity of the patients among hospitals (as in Herr, 2008; Bloom et al., 2015), we include the average length of stay (ALOS) as more severe patients stay in hospital longer. ALOS is often used as a patient complexity measure since it allows capturing the variation of severity not only between, but also within diseases (Wagstaff, 1989). Moreover, we control for the median waiting time to account for differences in the quality of the service provided and for the number of tests dispensed to patients to account for the overall use of hospital resources. We also account for differences in the population served by considering the proportion of patients aged 0–14, the proportion of patients aged 60 and over and the proportion of female patients. In addition, to capture market competition, we include the Herfindahl Index (HHI) built on hospital market shares of bed days within a 30-mile radius area around each hospital.

Regarding hospital characteristics, we include a dummy variable for the Foundation Trust status (FT) that equals 1 from the year the hospital becomes an FT onwards, and zero otherwise.[7] The main reason why we also account for this characteristic is related to the fact that many

[6] We exclude from the analysis both subsequent outpatient appointments and total outpatient attendances, to avoid patients' double counting. For the same reason, A&E follow-ups and total A&E visits are excluded as well.

[7] In 2003 the UK Parliament passed the HSC Act 2003, a bill that allowed some NHS Trusts to acquire a new legal status—Foundation Trust—and become non-profit public benefit corporations in charge of providing goods and services for the purposes of the NHS in England (HSC Act 2003, Part 1, section 1). Several hospitals have thus experienced an organizational change by acquiring this status. FTs have acquired a new set of freedoms in comparison to non-FTs. Specifically, FTs have a higher degree of independence from the Department of Health and more freedom in their corporate governance decisions. For example, more control over appointing and rewarding staff, directors and board members; as well as more control over their long/short-term strategies and the way services are managed and operated. More decentralization, managerial and governance flexibility also brings more financial freedoms. In particular, FTs can retain their surpluses, obtain faster access to capital by raising it from both the public and private sectors, invest in the best mix of services for their patients and thus develop business strategies that better coordinate their financial and operating structure with the needs of their local communities. Moreover, these freedoms should also facilitate outsourcing of both medical and non-medical services (e.g., laundry, cleaning, catering, lab analysis, etc.) allowing further increases in efficiency. As a result of all these organizational changes, one can expect that FTs would be encouraged to change their behavior, and ultimately their performance (HSC Act 2003; Commission for Healthcare Audit and Inspection, 2005). In fact, FT policy advocates tend to argue that the new freedoms of FTs should lead to their better organizational performance, including lower costs and improved efficiency.

mergers were motivated to allow NHS trusts to acquire the FT status through the merger transaction (Collins, 2015). We also include the variable Teaching that equals 1 if the trust is a teaching hospital, and zero otherwise, and the categorical variable labelled Performance rating that takes values from 1 (the poorest level of performance) to 4 (the highest level of performance).[8] All continuous control variables will be log-transformed.

4.1.2 | Variables used in the pre-processing and in the PSM

We use the variable retained surplus, the difference between income and expenditures adjusted by interest receivable, interest payable and dividends payable, to control for hospital financial stability. Retained surplus is a very volatile measure that shows whether the trust has achieved break-even in the year. It is a sort of reserve money, which is available to the hospital management for reinvesting back.

As restructuring programs are often implemented to solve financial performance issues (Collins, 2015), we match treated and non-treated trusts on pre-treatment financial characteristics. In particular, we consider a measure of total hospital costs, a measure of personnel costs and a measure of performance. Personnel costs include both managers' and directors' costs as a share of total hospital costs, while the measure of performance (labelled Pseudo-ROI) is built on the hospital surplus as a share of total hospital costs.[9] All these variables will be log-transformed except variable Pseudo-ROI built on retained surplus which can be positive or negative.

4.2 | Descriptive statistics

Table 2 shows descriptive statistics for our sample overall. Among 1,581 observations in our sample, about 11% represents activities under the merger hospital program. The average number of inpatient spells per year is about 67,476, of which around 43,430 are elective admissions and the remaining 24,074 are emergency admissions. On average, the number of patients attending the first outpatient appointment (77,768) and the number of patients visiting the A&E department for the first time (78,749) are both very high. This is perhaps not surprising for different reasons: given the complexity and variety of patients in the A&E department, unplanned care tends to be higher than planned care; given that many hospital visits do not require hospitalizations, outpatient care tends to be higher than inpatient care. The average number of elective admissions is almost double the number of planned admissions without overnight stay (daycases). The ratio between planned and unplanned hospital activities (labelled as elective/emergency ratio) shows that on average planned care is three times higher than unplanned care, while the ratio between inpatient spells and outpatient appointments (labelled as inpatient/outpatient ratio) reveals on

[8] The performance rating is a complex indicator to evaluate hospital performance, built on the NHS Performance Rating system for years 2000, 2001 and 2002, on the Star Rating system for years 2003 and 2004, and on the Health Check for years 2005, 2006, 2007 and 2008. The complex index score is based on several indicators including clinical indicators, patient-level indicators, indicators for capacity and capability, key financial targets.

[9] As the Return on Investment, ROI, represents the return on a particular investment relative to the investment's cost, we build a pseudo-ROI that represents the return associated with a potential investment—the surplus—relative to its costs—total hospital costs.

TABLE 2 Summary statistics; England, years 2000–08

Variable	*N*	Mean	Std. Dev.	Min.	Max.
Dependent variables					
Inpatient spells	1579	67475.69	38128.685	2264	232033
Elective admissions	1577	43430.348	25184.739	2119	154926
Emergency admissions	1579	24073.538	14090.597	13	85135
Outpatients (first visit)	1576	77768.218	44429.45	1006	257783
A&E attendances (first visit)	1568	78749.177	45778.437	0	279532
Daycases	1574	22944.841	14652.876	0	82856
Elective/emergency ratio	1577	320.788	1252.306	61.908	43969.23
Inpatient/outpatient ratio	1574	93.873	47.654	6.746	818.52
Daycases/elective ratio	1571	51.367	11.244	0	96.904
Policy variable					
Merged (dummy)	1581	0.111	0.314	0	1
Hospital inputs					
Available acute beds	1575	581.803	343.654	44	2142
Operating theaters	1567	15.836	9.168	0	57
Medical staff (%)	1559	11.159	2.197	4.61	19.362
Controls					
ALOS	1577	5.531	1.78	1	23
Median waiting time (days)	1559	50.638	20.109	6	163
Total diagnostic tests	1567	178634.843	95586.141	6730	626807
Patients aged 0–14 (%)	1553	13.999	12.778	0	94.988
Patients aged 60 and over (%)	1563	40.704	10.12	0	70.400
Female patients (%)	1578	50.914	6.662	29.909	112.241
HHI	1581	2984.791	2866.884	310.415	10000
Hospital characteristics					
FT (dummy)	1581	0.166	0.372	0	1
Teaching (dummy)	1581	0.341	0.474	0	1
Performance rating	1556	2.867	1.008	1	4
Variable used in the pre-processing					
Retained surplus (000)	1568	−149.087	6144.63	−84823	55990
Variables used in the PSM					
Total hospital costs (000)	1369	190579.218	126978.351	10434	845474
Managers and directors costs (%)	1030	3.296	1.055	0.79	10.222
Pseudo-ROI	1281	2.589	3.121	−19.908	13.696

average a balanced combination of the two activities. The ratio between daycases and elective care (labelled as daycases/elective ratio) shows that on average elective admissions double daycases.

Summary statistics on control variables also reveal that in our sample on average hospitals operate with a capacity of about 582 beds dedicated to acute care, though variation can be quite large (44 vs. 2142 beds), about 16 operating theatres, and about 11% of medical staff. ALOS in a hospital is between 5 and 6 days, while the median waiting time is around 51 days, though variation is very high (6 vs. 163 days). On average hospitals carry out over 170,000 diagnostic tests per year. Patients aged 0–14 years and those aged 60 and over represent over 50% of the treated patients (14% and 41% respectively), while around 51% of the treated patients are female. The hospital market is moderately concentrated as on average the HHI is almost equal to 3,000 points, but there is a noticeable variation between hospitals (310 vs. 10,000 points). Finally, about 17% of hospitals operate as FTs, 34% are teaching hospitals and performance index is almost equal to 3 points (mean value, ranging the index between 1 and 4).

On average hospitals have around 150,000 GBP deficit (negative retained surplus), against over 190 mil GBP (total hospital costs), while hospital surplus represents only 2.6% of total hospital costs (pseudo-ROI). Managers and directors costs represent only 3% of total hospital costs.

In Table 3, we compare the summary statistics between trusts that went through organizational change and merged by the end of our sample period with those that did not (i.e., for them variable Merged=0 in all years). The mean differences (reported in the last column of Table 3) for all dependent variables, but elective/emergency ratio and inpatient/outpatient ratio, suggest that merged hospitals provide more services than those that did not merge. When significant, differences in mean are significant at 1%. Moreover, splitting inpatient spells between elective and emergency admissions suggests that larger activity adjustments would occur among planned than unplanned activities. This is consistent with the fact that lifting inefficiency constraints and providing better allocation of resources as a result of hospital reorganization would be much more important for unplanned, emergency care than for planned, elective care. However, merged hospitals do not provide significantly larger ratios in the combined outcomes of both elective/emergency and inpatient/outpatient, which may further raise the question of how merger policy interacted with hospital efficiency in the short term. Mean differences in hospital beds, operating theaters and medical staff suggest that merged hospitals tend to reorganize internal resources to reduce medical staff in favor of physical capital (beds and theaters). Significant differences in means for most control variables, between merged versus non-merged trusts, also suggest that hospital heterogeneity and other factors will play an important role when it comes to teasing out the impact of organizational change imposed by merger policy. Mean differences in ALOS, diagnostic tests, age and gender of the patients, and HHI reveal that merged hospitals are more efficient than non-merged trusts. Mean differences in FT status confirm the importance of being a Foundation Trust in the merging process in the long term. Finally, merged hospitals are more likely to be also teaching hospitals, even though performance may be slightly lower. The goal of our empirical analyses described below is to further explore these data patterns.

As the DID identification strategy relies on the assumption that trends in the dependent variable are similar in both treated and untreated groups in the absence of the treatment and therefore that any deviation from the common trend should be induced only through the treatment, we need to first verify the presence of a common trend looking at trends in output variables. Figure 1 represents trends in output variables for treated (merged) and non-treated (non-merged) trusts. Since the restructuring program has been administrated in a staggered way, in Figure 1 we split hospitals into different "waves" according to the year in which the newly merged hospital is operative. We refer to them as wave 1 (newly merged trust in 2001), wave 2 (newly merged trust in 2002), wave 3 (newly merged trust in 2003), wave 4 (newly merged trust in 2006), wave 5 (newly merged trust in 2007), and non-merged (all never merged hospitals in a given year, i.e., the control group). Since a close observation of Figure 1 reveals that the PTA does not always hold, in Section 5.1.1 we test for conditional PTA following Cerulli and Ventura (2019).

TABLE 3 Mean comparison, merged versus non-merged trusts; England, years 2000–08

Variable	Merged = 1 by 2008 N	Merged = 1 by 2008 Mean	Merged = 0 in all years N	Merged = 0 in all years Mean	Difference
Dependent variables					
Inpatient spells	175	111409.943	1404	61999.554	49410.389***
Elective admissions	175	71063.971	1402	39981.073	31082.899***
Emergency admission	175	40345.971	1404	22045.278	18300.693***
Outpatients (first visit)	174	123366.845	1402	72109.045	51257.800***
A&E attendances (first visit)	174	130871.971	1394	72243.175	58628.796***
Daycases	175	39328.966	1399	20895.362	18433.604***
Elective/emergency ratio	175	177.626	1402	338.657	−161.031
Inpatient/outpatient ratio	174	93.401	1400	93.932	−0.531
Daycases/elective ratio	175	55.054	1396	50.905	4.149***
Hospital inputs					
Available acute beds	175	952.714	1400	535.439	417.275***
Operating theaters	175	24.034	1392	14.805	9.229***
Medical staff (%)	175	10.709	1384	11.216	−0.507***
Controls					
ALOS	175	5.28	1402	5.562	−0.282**
Median waiting time (days)	175	48.977	1384	50.848	−1.870
Total diagnostic tests	174	274983.356	1393	166599.925	−108383.431***
Patients aged 0–14 (%)	175	12.352	1378	14.209	−1.857*
Patients aged 60 and over (%)	175	42.489	1388	40.479	2.010**
Female patients (%)	175	50.833	1403	50.925	−0.091
HHI	175	3367.654	1406	2937.137	430.517*
Hospital characteristics					
FT (dummy)	175	0.194	1406	0.162	0.032
Teaching (dummy)	175	0.469	1406	0.325	0.144***
Performance rating	175	2.737	1381	2.883	−0.146*
Variable used in the pre-processing					
Retained surplus (000)	175	−338.383	1393	−125.306	−213.077
Variables used in the PSM					
Total hospital costs (000)	170	287141.112	1199	176888.208	110252.904***
Managers and directors costs (%)	126	2.976	904	3.341	−0.364***
Pseudo-ROI	155	2.278	1126	2.632	0.354

4.3 | Data structure in the flexible conditional DID

Before presenting the empirical results, we need to spend a few words on the structure of the data when using a flexible conditional DID approach (Dettmann et al., 2020). As mergers occurred in a staggered way, every year is a mix of hospital pre-treatment, treatment and post-treatment

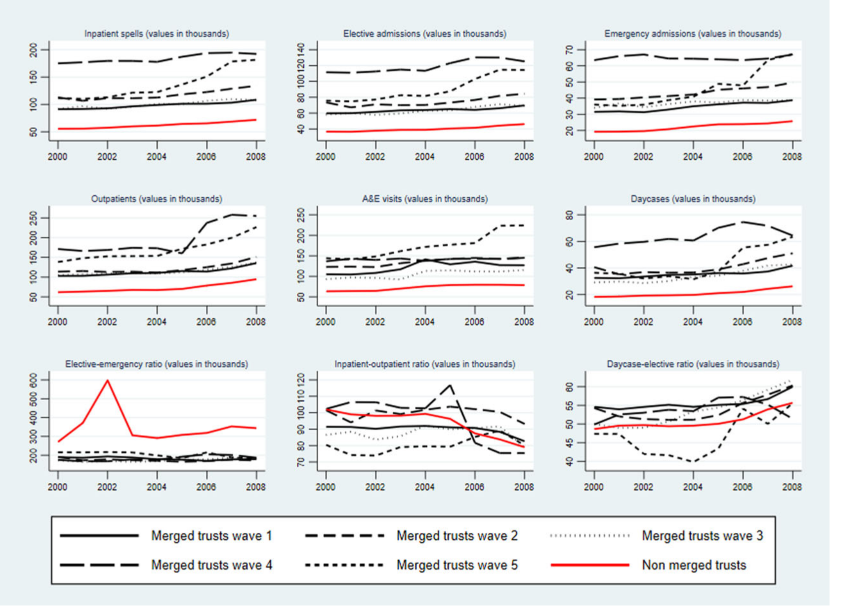

FIGURE 1 Verification of common trends; England, years 2000–08 [Colour figure can be viewed at wileyonlinelibrary.com]

Note: Using our full data set of hospitals, the figure shows the trends in hospital output for each wave (1 to 5) of merged and non-merged trusts. We split hospitals into different "waves" according to the year in which they started operating as newly merged trusts: wave 1 (merged in 2001), wave 2 (merged in 2002), wave 3 (merged in 2003), wave 4 (merged in 2006), wave 5 (merged in 2007), and non-merged trusts (control group).

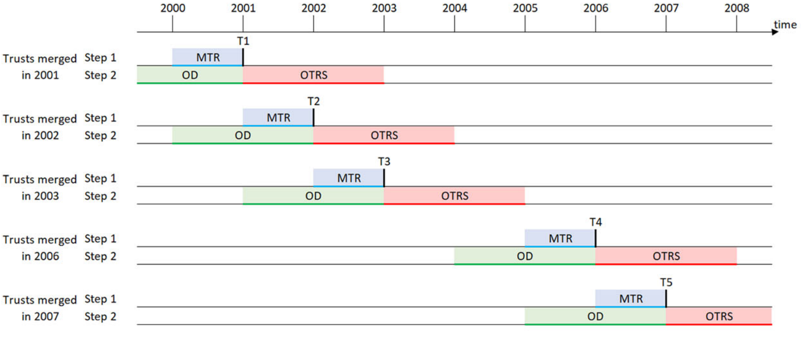

FIGURE 2 Data structure in the flexible conditional DID; England, years 2000–08 [Colour figure can be viewed at wileyonlinelibrary.com]

characteristics which implies that in theory every treated trust could as well act as a non-treated one, according to the year we consider.

Figure 2 clarifies the structure of our data set used to estimate the effects of staggered mergers on hospital outputs with the flexible conditional DID approach. First, we label with T_i with $i = 1, \ldots, 5$ the years merged trusts start to operate ($T_1 = 2001$, $T_2 = 2002$, $T_3 = 2003$, $T_4 = 2006$ and $T_5 = 2007$). Second, we label with Step 1 and Step 2 the two phases of the flexible conditional DID approach (pre-processing and estimation, respectively).

In the pre-processing phase (Step 1) we rearrange the data set according to a set of selected pre-treatment characteristics and according to the relative time specification, based on the year of the merger, that defines the time of matching (labelled matchtimerel in the STATA toolbox; Dettmann et al., 2020, p. 13 and MRT in Figure 2). As providers "can take from one to two years to identify their preferred merger partners, and one to four years to gain approvals and complete the merger process" (Collins, 2015, p. 23), we set the duration of the pre-treatment period (MRT) at one year before the merger.

In the estimation phase (Step 2) we define the pre-treatment outcome development and its relative time specification. As the pre-treatment outcome is a selected period of outcome development before the treatment starts, we set this time span (labelled outcomedev in the STATA toolbox, Dettmann et al., 2020, p. 15 and OD in Figure 2) from two years to one year before the merger, and we assume that the outcome will develop over two years after the merger (labelled outcometimerelstart in the STATA toolbox, Dettmann et al., 2020, p. 15 and OTRS in Figure 2). In this way, pre- and post-treatment outcome development will be somehow balanced.

5 | EMPIRICAL ANALYSIS

5.1 | Empirical results

In the pre-processing phase (flexpaneldid_preprocessing in the STATA toolbox, Dettmann et al., 2020, p. 13), we use the variable retained surplus as the exact matching variable (labelled matchvarsexact in the STATA toolbox, Dettmann et al., 2020, p. 13). The purpose of the exact matching option is to create, for every treated hospital, individual selection groups containing all potential controls. These matched sets are then refined by calibrated matching of additional covariates (labelled matchvars in the STATA toolbox, Dettmann et al., 2020, p. 13). In our analysis, these covariates are the total hospital costs, the managers and directors costs and the pseudo-ROI for matching. The result of this pre-processing phase is a temporary data set with information that is crucial for the use of the estimation phase.

Based on the temporary data set, we then estimate the ATT using the nearest neighbor matching algorithm with replacement under the assumption that the observation period, defined in relation to the treatment start (outcometimerelstart in the STATA toolbox, Dettmann et al., 2020, p. 15), is common for all treated units. To draw causal inference in presence of non-random sampling, as in the case of the merger restructuring program, the estimation command (flexpaneldid in the STATA toolbox, Dettmann et al., 2020, p. 14) also allows to apply a t-test with corrected standard errors.[10] Moreover, the option test allows to execute further quality tests on the matching variables (total hospital costs, managers and directors costs and pseudo-ROI, in our model). The ps-test computes the means in the treated and control groups, a measure for the standardized percentage difference between the means in both groups (labelled %bias), and a t-test if the means in the control group equal the ones in the treated group. The KS-test instead assesses whether continuous variable distributions between the treated and the control group are significantly different.[11]

[10] More details on the implementation of the correction terms are provided by Dettmann et al., 2020, p. 11.

[11] The option test also provides quantile–quantile plots of the continuous matching variables for a graphical impression on the comparability of the matched groups. These plots compare the distributions in both groups by means of the plotted

Table 4 returns a summary of the executed matching procedure. For 14 out of the 26 treated trusts, the matching procedure finds a partner. In the matching process, on average, 12 non-treated units are used as partners. The exact number of non-treated varies between 11 and 14. That means, in some cases, some of the non-treated units are used as partners for more than one treated, which is typical for the implemented nearest-neighbor matching with replacement. According to the t-tests, the means in the control group equal those in the treated group and are therefore balanced. Regarding the KS-tests, the corrected p-values the variables total hospital costs, managers and directors costs and pseudo-ROI and for the pre-treatment outcome development tell us that the variable distributions between the treated and the control group are not significantly different.

Table 5 displays the conditional DID estimation results for average treatment effect for the treated (ATT) from the start of the treatment until two years afterward. In general, mergers decrease the development of hospital activities. Specifically, we observe a positive development of the number of inpatient spells, elective and emergency admissions, and daycases, both for the treated and the controls. Unlike, the mean difference in these hospital activities development between treated and controls is negative, meaning that hospital mergers have a negative impact on hospital activities. To assess the statistical significance of these differences, we look at the p-values of the modified t-tests for corrected standard errors. The p-values relative to inpatient spells, elective and emergency admissions, and daycases indicate that the differences are not significant. Regarding A&E attendances and the inpatient/outpatient ratio, we observe a positive development, both for the treated and the controls, but the mean difference in these hospital activities development between treated and controls is positive, meaning that hospital mergers have a positive impact on both A&E attendances and the inpatient/outpatient ratio. The difference is however not significant. For outpatients and the daycases/outpatient ratio, we observe opposite developments for the treated and the controls, resulting in a negative mean difference for daycases and a positive one for the daycases/outpatient ratio. Even in this case, the difference is not significant. Finally, we observe a negative development in the inpatient/outpatient ratio, both for the treated and the controls. In this case, the mean difference in the inpatient/outpatient ratio development between treated and controls is positive, meaning that hospital mergers have a positive impact on this combination of activities. The difference is however not significant.

Finally, we allow for robustness checks (as in Dettmann et al., 2020) estimating (2) with canonical fixed effects DID model with standard errors allowing for within-group correlation under the assumption that the observation period is trimmed at the defined end of the outcome development, as reported in Table 6.

While the dummy variable M_i, that equals 1 from the year the newly merged trust is established onwards, and zero otherwise (β_1 coefficient), is dropped because of collinearity, the other two relevant variables, D_{it}, that is the count dummy variable with the relative difference to the treatment start (β_2 coefficients), and the interaction between D_{it} and M_i (δ coefficients) are used to verify the effect of hospital mergers on hospital outputs. In particular, both the count dummy D_{it}, that controls for all other unobservable temporal factors affecting the dependent variable, and the interaction between D_{it} and M_i dummies, that identifies the change in trust output for merged trust relative to untreated trusts, are modified to account for the relative distance from treatment start. For example, for a trust merged in 2001, D_{it} equals 1 in 2003, 2 in 2004, and so on; for a trust merged in 2002, D_{it} equals 1 in 2004, 2 in 2005, and so on; for a trust merged in 2005, D_{it} equals 1 in 2007 and 2 in 2008. The interaction between D_{it} and M_i dummies is modified accordingly, with

quantiles. The quantile–quantile plots of the continuous matching variables used in our empirical analysis (total hospital costs, managers and directors costs and pseudo-ROI) are available upon request.

TABLE 4 Selection of the appropriate comparator group and PSM by hospital output

		Non-treated		Treated				
	All	143		26				
Inpatient spells	Matched sample	11		14				
	ps-test							
	Mean			t-test		Combined KS test		
PSM	Treated	Controls	%bias	t	$p > \|t\|$	Diff.	P-value	Corrected
Total hospital costs	19.238	19.103	28.5	0.75	0.458	0.2857	0.617	0.505
Managers/directors costs	1.1459	1.1073	14.4	0.38	0.707	0.1429	0.999	0.997
Pseudo-Roi	3.1996	3.4241	−20.4	−0.54	0.595	0.1429	0.999	0.997
Outcome development	−0.04132	−0.02109	−24.8	−0.66	0.518	0.2143	0.905	0.847
		Non-treated		Treated				
	All	143		26				
Elective admissions	Matched sample	13		14				
	ps-test							
	Mean			t-test		Combined KS test		
PSM	Treated	Controls	%bias	t	$p > \|t\|$	Diff.	P-value	Corrected
Total hospital costs	19.238	19.187	12.4	0.33	0.745	0.2857	0.617	0.505
Managers/directors costs	1.1459	1.0669	30.1	0.80	0.434	0.2143	0.905	0.847
Pseudo-ROI	3.1996	3.5326	−27.2	−0.72	0.479	0.1429	0.999	0.997
Outcome development	−0.05827	−0.03174	−22.9	−0.61	0.550	0.2143	0.905	0.847
		Non-treated		Treated				
	All	143		26				
Emergency admissions	Matched sample	13		14				
	ps-test							
	Mean			t-test		Combined KS test		
PSM	Treated	Controls	%bias	t	$p > \|t\|$	Diff.	P-value	Corrected
Total hospital costs	19.238	19.156	18.2	0.48	0.635	0.2143	0.905	0.847
Managers/directors costs	1.1459	1.1087	14.3	0.38	0.708	0.2143	0.905	0.847

(Continues)

TABLE 4 (Continued)

		Non-treated		Treated				
	All	143		26				
Emergency admissions	**Matched sample**	13		14				
	ps-test							
	Mean			t-test		Combined KS test		
PSM	**Treated**	**Controls**	**%bias**	t	$p > \|t\|$	**Diff.**	P**-value**	**Corrected**
Pseudo-ROI	3.1996	3.4395	−21.8	−0.58	0.568	0.2143	0.905	0.847
Outcome development	−0.00711	0.01026	−46.9	−1.24	0.226	0.3571	0.334	0.237

		Non-treated		Treated				
	All	143		26				
Outpatients	**Matched sample**	12		14				
	ps-test							
	Mean			t-test		Combined KS test		
PSM	**Treated**	**Controls**	**%bias**	t	$p > \|t\|$	**Diff.**	P**-value**	**Corrected**
Total hospital costs	19.238	19.141	21.8	0.58	0.569	0.2857	0.617	0.505
Managers/directors costs	1.1459	1.1054	15.8	0.42	0.680	0.1429	0.999	0.997
Pseudo-ROI	3.1996	3.7221	−50.7	−1.34	0.191	0.3571	0.334	0.237
Outcome development	0.00749	0.00802	−1.5	−0.04	0.968	0.2857	0.617	0.505

		Non-treated		Treated				
	All	143		26				
A&E attendances	**Matched sample**	11		14				
	ps-test							
	Mean			t-test		Combined KS test		
PSM	**Treated**	**Controls**	**%bias**	t	$p > \|t\|$	**Diff.**	P**-value**	**Corrected**
Total hospital costs	19.238	19.145	19.2	0.51	0.616	0.3571	0.334	0.237
Managers/directors costs	1.1459	1.1419	1.6	0.04	0.967	0.2143	0.905	0.847

(Continues)

TABLE 4 (Continued)

		Non-treated		**Treated**				
	All	**143**		**26**				
A&E attendances	**Matched sample**	**11**		**14**				
	ps-test							
	Mean			***t*-test**		**Combined KS test**		
PSM	**Treated**	**Controls**	**%bias**	***t***	***p* > |*t*|**	**Diff.**	***P*-value**	**Corrected**
Pseudo-ROI	3.1996	3.4233	−20.3	−0.54	0.595	0.1429	0.999	0.997
Outcome development	0.00109	-0.00177	5.8	0.15	0.878	0.2857	0.617	0.505
		Non-treated		**Treated**				
	All	**143**		**26**				
Daycases	**Matched sample**	**14**		**14**				
	ps-test							
	Mean			***t*-test**		**Combined KS test**		
PSM	**Treated**	**Controls**	**%bias**	***t***	***p* > |*t*|**	**Diff.**	***P*-value**	**Corrected**
Total hospital costs	19.238	19.073	33.1	0.87	0.390	0.2857	0.617	0.505
Managers/directors costs	1.1459	1.0653	31.3	0.83	0.415	0.2143	0.905	0.847
Pseudo-ROI	3.1996	3.2627	−5.1	−0.13	0.894	0.1429	0.999	0.997
Outcome development	−0.08339	−0.0425	−20.4	−0.54	0.594	0.1429	0.999	0.997
		Non-treated		**Treated**				
	All	**143**		**26**				
Elective/emergency ratio	**Matched sample**	**11**		**14**				
	ps-test							
	Mean			***t*-test**		**Combined KS test**		
PSM	**Treated**	**Controls**	**%bias**	***t***	***p* > |*t*|**	**Diff.**	***P*-value**	**Corrected**
Total hospital costs	19.238	19.194	12.0	0.32	0.753	0.2857	0.617	0.505
Managers/directors costs	1.1459	1.1018	16.6	0.44	0.664	0.1429	0.999	0.997

(Continues)

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TABLE 4 (Continued)

		Non-treated		**Treated**				
	All	**143**		**26**				
Elective/emergency ratio	**Matched sample**	**11**		**14**				
	ps-test							
	Mean			***t*-test**		**Combined KS test**		
PSM	**Treated**	**Controls**	**%bias**	***t***	***p* > |*t*|**	**Diff.**	***P*-value**	**Corrected**
Pseudo-ROI	3.1996	3.5669	−30.0	−0.79	0.435	0.2143	0.905	0.847
Outcome development	−0.05115	−0.04772	−3.0	−0.08	0.938	0.2857	0.617	0.505
		Non-treated		**Treated**				
	All	**143**		**26**				
Inpatient/outpatient ratio	**Matched sample**	**12**		**14**				
	ps-test							
	Mean			***t*-test**		**Combined KS test**		
PSM	**Treated**	**Controls**	**%bias**	***t***	***p* > |*t*|**	**Diff.**	***P*-value**	**Corrected**
Total hospital costs	19.238	19.185	11.5	0.30	0.763	0.2857	0.617	0.505
Managers/directors costs	1.1459	1.1753	−13.0	−0.34	0.733	0.2857	0.617	0.505
Pseudo-ROI	3.1996	3.4853	−24.5	−0.65	0.523	0.2143	0.905	0.847
Outcome development	−0.04881	−0.04596	−2.8	−0.07	0.942	0.2857	0.617	0.505
		Non-treated		**Treated**				
	All	**143**		**26**				
Daycases/elective ratio	**Matched sample**	**13**		**14**				
	ps-test							
	Mean			***t*-test**		**Combined KS test**		
PSM	**Treated**	**Controls**	**%bias**	***t***	***p* > |*t*|**	**Diff.**	***P*-value**	**Corrected**
Total hospital costs	19.238	19.062	36.7	0.97	0.340	0.2857	0.617	0.505
Managers/directors costs	11.459	1.097	19.9	0.53	0.604	0.1429	0.999	0.997
Pseudo-ROI	31.996	3.214	−1.2	−0.03	0.976	0.1429	0.999	0.997
Outcome development	−0.02512	−0.00631	−21.4	−0.57	0.576	0.2143	0.905	0.847

TABLE 5 Conditional DID estimates for ATT

	Mean Diff			AI robust		
	Treated	Controls	DID*	S.E.	z	p > |z|
Inpatient spells	0.0181	0.0699	−0.0518	0.1064	−0.4867	0.6346
Elective admissions	0.0078	0.0234	−0.0156	0.1015	−0.1536	0.8803
Emergency admissions	0.0334	0.1826	−0.1492	0.0959	−1.5562	0.1437
Outpatients (first visit)	−0.0012	0.0734	−0.0746	0.0851	−0.8771	0.3964
A&E attendances (first visit)	0.1525	0.0973	0.0552	0.1170	0.4719	0.6448
Daycases	0.0081	0.0103	−0.0022	0.1431	−0.0153	0.9880
Elective/emergency ratio	−0.0256	−0.1044	0.0788	0.0844	0.9332	0.3677
Inpatient/outpatient ratio	0.0193	0.0019	0.0174	0.0929	0.1873	0.8543
Daycases/outpatient ratio	0.0003	−0.0189	0.0192	0.0598	0.3209	0.7534

*Consistent bias-corrected estimator as proposed in Abadie and Imbens (2006, 2011).

M_i being a dummy variable equals 1 if the panel item is treated; 0 otherwise. The β_2 coefficients give the change in the dependent variable between year t and the year when the treatment started. The δ coefficients indicate the DID estimates for the change between year t and the year when the treatment started.

When significant, the β_2 coefficients are positive for inpatient spells, emergency admissions, and inpatient/outpatient ratio; negative for outpatients, elective/emergency ratio, and daycases/elective ratio. In other words, over time, hospital admissions, either planned or unplanned, and the ratio between admissions with overnight stay and consultant appointments are characterised by a positive trend, while consultant appointments, the ratio between planned and unplanned admissions and between admissions without and with overnight stay are characterized by a negative trend.

When significant, the δ coefficients are generally negative for all hospital outputs but the elective/emergency ratio. This suggests that on average the merger restructuring policy decrease the merged trusts output relative to non-merged ones. In particular, inpatients spells are reduced by around 7.5 percentage points between years 2003 and 2004 (δ_2) and 2004 and 2005 (δ_3); emergency admissions by 14–15 percentage points between years 2003 and 2004 (δ_2), 2004 and 2005 (δ_3) and 2006 and 2007 (δ_5) and by 7.3 percentage points between 2007 and 2008 (δ_6); inpatient/outpatient ratio by 10 percentage points between 2004 and 2005 (δ_3). The elective/emergency ratio instead increases by around 11 percentage points between years 2003 and 2004 (δ_2) and 2004 and 2005 (δ_3), by around 8 percentage points between years 2005 and 2006 (δ_4), and 14 percentage points between years 2006 and 2007 (δ_5).

Our results show that merged hospitals tend to prefer outpatients and daycases to inpatients (both elective and emergency admissions) in order to reduce costs and presumably gain efficiency.

5.1.1 | Conditional parallel trend assumption

Table 7 shows estimate results on conditional PTA (3) by using both leads and lags.

We set leads to one year before the merger ($l = L = t + 1$ in (3)), calculated as the average between relative time matching (one year before merger) and pre-treatment development (from two to one year before merger). In Table 7 this variable is labelled as Merged_{t-1}. We also align the

TABLE 6 FE DID estimates. A robustness check for conditional DID estimates

	Inp. spells	Elect. adm.	Emerg. adm.	Outp. (first visit)	A&E (first visit)	Daycases	Elect./emer. ratio	Inp./outp. ratio	Daycases/el. ratio
Policy variables									
year1 (β_{21})	0.033	0.016	0.060*	0.016	0.018	−0.013	−0.044	0.017	−0.029
	(0.025)	(0.025)	(0.033)	(0.030)	(0.092)	(0.038)	(0.030)	(0.040)	(0.023)
year2 (β_{22})	0.080***	0.030	0.155***	−0.013	−0.009	−0.001	−0.125**	0.094**	−0.031
	(0.028)	(0.034)	(0.045)	(0.036)	(0.116)	(0.054)	(0.054)	(0.041)	(0.033)
year3 (β_{23})	0.084**	0.022	0.171***	−0.109**	−0.021	−0.040	−0.149**	0.193***	−0.062
	(0.030)	(0.044)	(0.047)	(0.045)	(0.142)	(0.078)	(0.069)	(0.054)	(0.045)
year4 (β_{24})	0.014	−0.039	0.090	−0.108	−0.021	−0.132	−0.129*	0.119	−0.093*
	(0.068)	(0.077)	(0.075)	(0.107)	(0.169)	(0.096)	(0.067)	(0.082)	(0.046)
year5 (β_{25})	0.122**	0.053	0.221***	0.044	−0.045	−0.001	−0.167**	0.076	−0.055
	(0.057)	(0.069)	(0.065)	(0.124)	(0.205)	(0.090)	(0.075)	(0.097)	(0.058)
year6 (β_{26})	0.090	0.029	0.175**	0.033	−0.048	0.031	−0.146	0.054	0.002
	(0.060)	(0.087)	(0.064)	(0.126)	(0.232)	(0.125)	(0.110)	(0.118)	(0.076)
Merged × year1 (δ_1)	−0.025	−0.015	−0.047	0.003	0.009	−0.032	0.032	−0.026	−0.017
	(0.032)	(0.036)	(0.036)	(0.033)	(0.040)	(0.049)	(0.034)	(0.048)	(0.035)
Merged × year2 (δ_2)	−0.075**	−0.034	−0.145***	−0.003	−0.001	−0.089	0.111*	−0.071	−0.055
	(0.029)	(0.043)	(0.034)	(0.039)	(0.040)	(0.062)	(0.054)	(0.049)	(0.039)
Merged × year3 (δ_3)	−0.076**	−0.034	−0.141***	0.018	0.001	−0.065	0.107*	−0.101*	−0.031
	(0.030)	(0.044)	(0.035)	(0.043)	(0.046)	(0.073)	(0.056)	(0.052)	(0.044)
Merged × year4 (δ_4)	−0.017	0.012	−0.066	−0.018	−0.001	0.034	0.079*	0.002	0.022
	(0.062)	(0.072)	(0.055)	(0.104)	(0.047)	(0.092)	(0.045)	(0.085)	(0.041)

(Continues)

TABLE 6 (Continued)

	Inp. spells	Elect. adm.	Emerg. adm.	Outp. (first visit)	A&E (first visit)	Daycases	Elect./emer. ratio	Inp./outp. ratio	Daycases/el. ratio
Merged × year5 (δ_5)	−0.068	−0.012	−0.154***	−0.075	0.006	−0.018	0.142**	0.007	−0.005
	(0.040)	(0.052)	(0.042)	(0.072)	(0.053)	(0.083)	(0.057)	(0.070)	(0.043)
Merged × year6 (δ_6)	−0.021	0.014	−0.073*	−0.051	−0.018	−0.017	0.087	0.031	−0.031
	(0.048)	(0.068)	(0.042)	(0.076)	(0.063)	(0.107)	(0.077)	(0.072)	(0.050)
Hospital inputs									
Available acute beds	−0.019	−0.041	0.029	0.135	−0.003	−0.128	−0.070	−0.152	−0.087
	(0.041)	(0.061)	(0.043)	(0.097)	(0.051)	(0.083)	(0.077)	(0.090)	(0.073)
Operating theaters	0.033	0.057	−0.020	0.008	−0.017	0.094	0.077	0.026	0.037
	(0.051)	(0.078)	(0.075)	(0.097)	(0.057)	(0.105)	(0.121)	(0.094)	(0.062)
Medical staff	0.013	0.001	0.038	−0.045	−0.087	−0.020	−0.038	0.060	−0.021
	(0.085)	(0.098)	(0.078)	(0.115)	(0.076)	(0.134)	(0.064)	(0.131)	(0.081)
Controls									
ALOS	−0.083	−0.098	−0.062	0.131	−0.021	−0.056	−0.036	−0.215	0.042
	(0.083)	(0.111)	(0.053)	(0.113)	(0.053)	(0.119)	(0.100)	(0.142)	(0.059)
Median waiting time	−0.078**	−0.066	−0.097**	−0.035	−0.003	−0.179**	0.030	−0.043	−0.113**
	(0.037)	(0.048)	(0.044)	(0.075)	(0.029)	(0.071)	(0.057)	(0.078)	(0.044)
Total diagnostic tests	0.041	0.020	0.057	−.005	−0.053	−0.055	−0.037	0.053	−0.075
	(0.067)	(0.090)	(0.066)	(0.104)	(0.079)	(0.150)	(0.099)	(0.102)	(0.084)
Patients aged 0–14	−0.250	−0.447*	0.065	−0.114	−0.116	−0.382	−0.512**	−0.136	0.064

(Continues)

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TABLE 6 (Continued)

	Inp. spells	Elect. adm.	Emerg. adm.	Outp. (first visit)	A&E (first visit)	Daycases	Elect./emer. ratio	Inp./outp. ratio	Daycases/el. ratio
	(0.165)	(0.220)	(0.155)	(0.258)	(0.077)	(0.290)	(0.230)	(0.289)	(0.117)
Patients aged 60+	0.192	0.436	−0.246	0.051	−0.370	1.966**	0.682	0.145	1.530***
	(0.538)	(0.730)	(0.331)	(0.563)	(0.374)	(0.910)	(0.667)	(0.537)	(0.305)
Female patients	0.416	1.005*	−0.619	0.098	0.012	1.989**	1.624**	0.314	0.984**
	(0.418)	(0.586)	(0.373)	(0.555)	(0.305)	(0.819)	(0.661)	(0.570)	(0.420)
HHI	0.010	0.015**	0.001	0.010	−0.014	0.033***	0.014	−0.001	0.018**
	(0.006)	(0.007)	(0.008)	(0.014)	(0.015)	(0.010)	(0.009)	(0.014)	(0.006)
Hospital characteristics									
FT	0.042	0.071*	−0.008	0.083	0.001	0.080	0.079*	−0.042	0.009
	(0.028)	(0.037)	(0.027)	(0.050)	(0.026)	(0.060)	(0.040)	(0.048)	(0.033)
Teaching	0.025	−0.006	0.075**	−0.106	0.027	−0.003	−0.082*	0.133*	0.003
	(0.039)	(0.049)	(0.032)	(0.072)	(0.052)	(0.070)	(0.043)	(0.074)	(0.035)
Performance rating	0.008	0.006	0.011	0.006	−0.002	0.022	−0.005	0.003	0.016*
	(0.007)	(0.010)	(0.008)	(0.011)	(0.015)	(0.014)	(0.012)	(0.013)	(0.008)
Constant	9.565**	6.746	13.014***	10.343***	14.129***	−1.930	−1.662	3.722	−4.071*
	(4.315)	(6.200)	(2.631)	(3.610)	(3.032)	(7.384)	(6.399)	(2.851)	(2.190)

(Continues)

TABLE 6 (Continued)

	Inp. spells	Elect. adm.	Emerg. adm.	Outp. (first visit)	A&E (first visit)	Daycases	Elect./emer. ratio	Inp./outp. ratio	Daycases/el. ratio
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Trust fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	234	234	234	233	234	234	234	233	234
R-squared	0.663	0.569	0.704	0.617	0.552	0.634	0.382	0.338	0.554
Groups	27	27	27	27	27	27	27	27	27

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 7 FE DID estimates. A robustness check on conditional parallel trend assumption (FT dummy variable, hospital inputs, controls and hospital characteristics are not reported in the table; full table is available upon request)

	Inp. spells	Elect. adm.	Emerg. adm.	Outp. (first visit)	A&E (first visit)	Daycases	Elect./emer. ratio	Inp./outp. ratio	Daycases/el. ratio
Merged$_{t-1}$ (δ_{-1})	−0.013	−0.026	0.0005	−0.039	0.016	−0.007	−0.027	0.024	0.020
	(0.029)	(0.036)	(0.033)	(0.082)	(0.046)	(0.052)	(0.037)	(0.067)	(0.025)
Merged$_t$ (δ_0)	0.015	0.008	0.015	−0.027	0.049	−0.020	−0.007	0.042	−0.028
	(0.014)	(0.020)	(0.022)	(0.044)	(0.087)	(0.027)	(0.030)	(0.042)	(0.016)
Merged$_{t+1}$ (δ_1)	0.013	−0.007	0.038	−0.057	−0.019	−0.015	−0.046	0.070	−0.008
	(0.040)	(0.036)	(0.050)	(0.037)	(0.048)	(0.051)	(0.032)	(0.039)	(0.025)
Merged$_{t+2}$ (δ_2)	−0.0003	−0.016	0.022	−0.111	−0.007	−0.017	−0.037	0.106	−0.002
	(0.0266)	(0.027)	(0.035)	(0.047)	(0.041)	(0.041)	(0.027)	(0.044)	(0.021)
PTA using the "leads"									
F(1, 26)	0.20	0.54	0.00	0.22	0.13	0.02	0.52	0.13	0.60
Prob > F	0.6587	0.4681	0.9887	0.6420	0.7252	0.8968	0.4754	0.7255	0.4468
Parallel-trend	passed	passed	passed	passed	passed	passed	passed	passed	passed
PTA using the "lags"									
F(1, 26)	2.15	1.17	1.94	0.03	1.57	1.09	0.23	3.11	0.47
Prob > F	0.1546	0.2895	0.1750	0.8647	0.2211	0.3065	0.6384	0.0897	0.4985
Parallel-trend	passed	passed	passed	passed	passed	passed	passed	passed	passed
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Trust fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	146	146	146	145	146	146	146	145	146
Groups	27	27	27	27	27	27	27	27	27

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

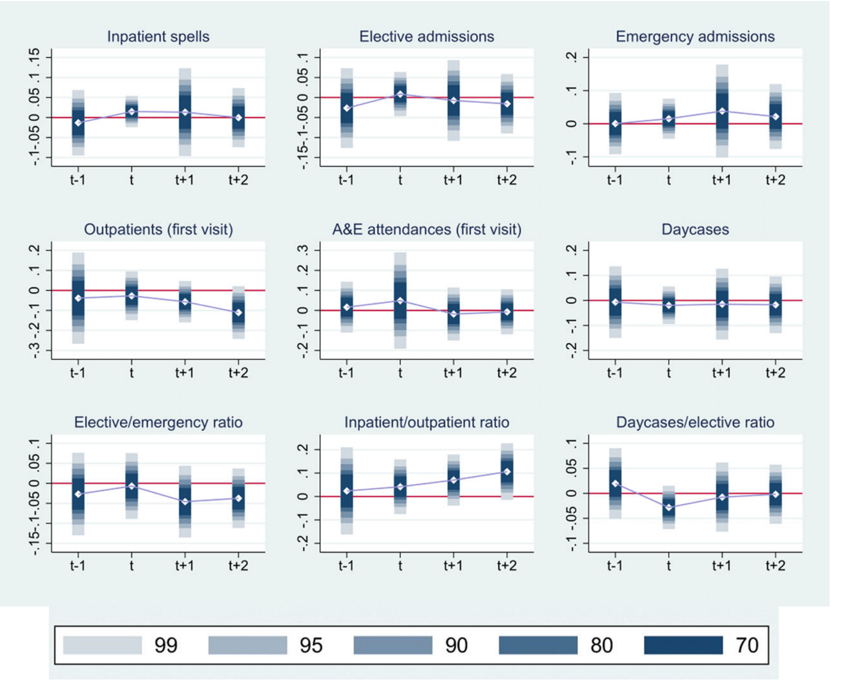

FIGURE 3 Pre- and post-treatment pattern for the relation between hospital mergers and hospital outputs [Colour figure can be viewed at wileyonlinelibrary.com]

post-treatment time to the outcome development ($l = F = t - 2$ in (3)), namely two years after the merger, in accordance to the above matching-DID estimation model (Goddard & Ferguson, 1997; Collins, 2015). These variables are labelled as Merged_{t+1} and Merged_{t+2}.

Finally, variable Merged_t is the binary time-varying treatment, defined as the tendency of treated hospitals to increase their output in a specific year compared with a baseline reference and measured as the average development of hospital output in the two years after the merger, given the pre-treatment features in the year before the merger.

According to the results in Table 7, the conditional PTA is passed in all specifications.

Finally, Figure 3 returns a graphical test for the conditional PTA for every hospital activity, over the period $(t - 1, t + 2)$.

This figure shows that from the time of treatment onwards, the ATE at time t (treatment year) is higher than its average development in the two years after the merger and then decreases until the last year after the treatment for all hospital activities but emergency admissions, the inpatient/outpatient ratio and the daycases/elective ratio.

The pattern is a sort of parabola, showing that the effect of the increase in the hospital mergers above the median has a transitory effect tending to fade away around two years after the treatment. This finding shows a quite sensible effect of hospital mergers on hospital activity. More specifically, we observe that the average difference between treated and untreated reaches its maximum value a year after the treatment and then it decreases in the subsequent year for every hospital activity except the inpatient/outpatient ratio and the daycases/elective ratio.

5.1.2 | Sensitivity analysis

To check the robustness of our results, we conduct a sensitivity analysis on Equation (3) using different combinations of control variables. In particular, we consider two measures of hospital size (total available beds vs. total available acute beds), three measures of performed diagnostic tests (total number of tests vs. CT scans and MRI scans), three measures of market competition (HHI built on 15- vs. 20- and 30-mile radius areas), and two performance indexes (one more focused on the use of the resources, the other on the quality of the services). Results across different specifications do not differ significantly. Results on the sensitivity analysis are available upon request.

6 | CONCLUSIONS

In this paper we provide evidence on the effect of hospital mergers on several measures of hospital outputs during the period 2000–2008 in England.

As the decision to merge takes place in different years and its duration is variable, we consider different groups of treated trusts. In addition, as the merger effect is heterogeneous between these groups, we adopt an alternative difference-in-differences approach. In particular, we adopt the flexible conditional DID approach (Dettmann et al., 2020) to take into account both time-varying treatment and variable duration. This approach also allows overtaking the potential limits of our analysis, associated with fundamental missing information (e.g., date of decision and announcement).

Our results show that on average the merger restructuring program adopted in England has reduced inpatient spells, emergency admissions and also the inpatient/outpatient ratio and has instead increased the elective/emergency ratio. In other words, merged hospitals tend to substitute admissions (inpatient, elective and emergency) with outpatient visits and daycases to redefine their offer in favor of less expensive activities.

Therefore, if the goal of the English hospital sector restructuring program was to quickly gain efficiency by centralization of activity, our findings suggest hospital mergers are not the most successful policy to pursue. Mergers reduce the scope for competition between hospitals by reducing more expensive activities and services (especially those with overnight stay) and do not create any incentive for poorly performing hospitals, thus penalising patients.

We reserve to future research to expand the present analysis with further empirical estimates on a longer time span data in order to corroborate our results.

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