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ABSTRACT

We investigate whether top managers affect the performance of large and complex public sector organizations, using as a case study CEOs of English public hospitals (large, complex organizations with multi-million turnover). We study the extent to which CEOs are differentiated in terms of their pay, as well as a wide range of hospital production measures including inputs, intermediate operational outcomes and clinical outcomes. Pay differentials suggest that the market perceives CEOs to be differentiated. However, we find little evidence of CEOs' impact on hospital production. These results question the effectiveness of leadership changes to improve performance in the public sector.

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1 Introduction

The impact of CEOs in private sector organizations has been explored in a number of influential studies beginning with Bertrand & Schoar (2003).[1] Several recent papers have shown that CEOs can also impact the performance of public sector organizations. This result, however, has only been documented for relatively small organizations such as schools and development projects, where top managers may have a greater chance of having an impact.[2] In contrast, the effect of top managers on large and complex public sector organizations has hardly even been examined. Can CEOs make a difference in this context? Addressing this question is important because a popular reform model in the public sector is to give greater autonomy to CEOs to run their organizations, accompanied by the use of manager-specific compensation policies, performance-related pay, tighter monitoring and dismissals (e.g. Besley & Ghatak (2003), Grand (2003)).

We contribute to the literature by examining the impact of CEOs of very large and complex organizations in the public sector. We study public hospitals in the English National Health Service (NHS). The NHS is the fifth biggest employer in the world with approximately 1.2 million employees, and NHS hospitals have on average 4,500 employees, multi-million turnover and labour costs accounting for around 70% of the costs of production. This setting is an ideal test-bed for several reasons. First, in the late 1980s the English government embarked on a large reform program that replaced an administrative approach to hospital management with a highly decentralized managerial model, in which CEOs were given responsibility for the management and performance of individual public hospitals, and individual hospital boards could select and reward individual CEOs in an autonomous fashion.[3] These changes were accompanied by frequent movements of the same CEOs across different but comparable NHS hospitals, providing an ideal setting to examine the role of managerial discretion for hospital performance in isolation from other persistent differences in hospital characteristics. Second, data are available for these individual NHS hospitals on inputs, throughputs, outputs (both clinical and financial) and

[1] Recent examples include Bamber et al. (2010), Dejong & Ling (2013) and Bennedsen et al. (2006).

[2] A number of papers investigate the impact of principals on student performance. Böhlmark et al. (2016) present evidence of principal fixed effects in students' outcomes. Other papers include Branch et al. (2012), Coelli & Green (2012) and Grissom et al. (2015). Lavy & Boiko (2017) find that superintendents affect student performance. Bloom, Lemos, Sadun & Van Reenen (2015) find managerial practices adopted by school principals are correlated with school performance. Fenizia (2019) finds managers affect the productivity of workers in a government agency. Limodio (2018) examines World Bank employees and documents evidence of negative matching between high performing managers and low performing countries. Rasul & Rogger (2018) examine management practice in the Nigerian Civil Service and find they affect the behaviour of government bureaucrats.

[3] Similar reforms were adopted in a number of public healthcare systems and in public administration more generally (Pollitt & Bouckaert 2000).

staff satisfaction, allowing us to examine a uniquely large and varied set of measures usually unavailable to researchers examining organizations in both the private and the public sector. Third, the NHS requires hospitals to publish the pay awarded to their top managers, thus allowing us to complement the performance analysis with an examination of CEO compensation and compare perceived differences in managerial ability (as proxied by their compensation) to actual differences (as they emerge from the analysis of the objective production measures).

We begin by showing that there are considerable and persistent differences in managerial pay by implementing the approach of Abowd et al. (1999). Moving from the 25^{th} percentile to the 75^{th} percentile of the CEO pay effects distribution represents a 12% increase in pay relative to mean CEO pay. These persistent differences in pay across individuals suggest that managers are perceived to be differentiated by their employers.

We then turn to examining whether CEOs are differentiated in terms of their *actual* performance. To do so, we use the method pioneered by Bertrand & Schoar (2003) and examine whether CEOs have a "style" regardless of the hospital they manage by testing whether there are statistically significant and "portable" CEO fixed effects, i.e. systematic differences in hospital production that are associated with the movement of CEOs across different hospitals. We extend the Bertrand & Schoar (2003) method and "stack" our hospital production variables into distinct sets of input, throughput, output (clinical and financial) and staff job satisfaction measures to take advantage of the fact we have multiple measures of production, and to simplify the exposition of our results while maximizing the number of observations. Our results show little consistent evidence that CEOs are able to generate persistent performance differentials across the organizations they lead. While we find the estimated CEO fixed effects are jointly statistically significant, these CEO fixed effects are essentially period-hospital-specific shocks rather than true CEO effects, since large deviations in one production measure in one hospital are typically not replicated by the same CEO in another hospital.[4]

We examine a number of possible reasons for this apparent lack of CEO effects in hospital production, which is in contrast with the substantial and persistent differences in pay. First, we examine whether the lack of persistence in the CEO fixed effects may be driven by the endogenous assignment of good CEOs to poorly performing hospitals, or hospitals that have structural features that may negatively affect the possibility of achieving good performance. If this were the case, differences in pay across CEOs would

[4]We repeat all analyses for each variable separately and show that the results are not sensitive to the use of stacking. We also undertake a non-parametric approach that compares changes in hospital production measures after a CEO turnover event to changes experienced by matched hospitals without such an event. The results confirm the parametric findings and are reported in Appendix B.

not necessarily be mirrored by differences in hospital production, since the best CEOs would be systematically assigned to harder-to-change organizations. Second, we examine whether the lack of persistent CEO effects may be driven by the fact that mover CEOs in the NHS tend to have short tenure. In this case, CEOs may in fact differ in terms of their potential effect on hospital performance, but these effects may fail to materialize over short time horizons. Finally, we examine whether there is evidence of CEO-hospital match effects, i.e. whether specific types of CEO may perform better when in a specific type of hospital, which would imply that CEOs matter, but only when the fit between the manager and the hospital is appropriate. We find little evidence of endogenous assignment. We do, however, find some evidence of tenure and match effects, which–combined with the evidence of substantial and persistent pay differentials–suggest that employers may overestimate the ability of CEOs to bring about change over short time periods and/or regardless of the circumstances they face.

Our results indicate that the CEOs of large public hospitals do not necessarily impact hospital performance, a result that stands in stark contrast with earlier findings relating to the private sector and to smaller public sector organizations. Various structural factors may account for this lack of effect, including the public sector nature of the NHS, which may have distorted the effort of NHS CEOs towards the pursuit of political targets rather than performance-enhancing policies. However, the lack of a CEO effect may also be due, more broadly, to the complexity of hospital production, which transcends the fact that the NHS is publicly owned. From this perspective, our results cast doubts on the effectiveness of a "turnaround CEO" approach–the model in which top managers frequently rotate across hospitals to induce meaningful changes in performance–for large public sector organizations.

Section 2 provides an overview of the institutional background, focusing on the evolution of the market for CEOs in the NHS. Section 3 describes the data. Section 4 presents the econometric methods used to identify CEO fixed effects in the pay and hospital production data. Section 5 present the results of the analysis of CEO fixed effects. Section 6 examines possible mechanisms behind the results. Section 7 concludes.

2 Institutional Background

2.1 Hospital Autonomy and the CEO Role in the English NHS

From the early 1990s, English public hospitals started operating as free-standing organizations known as NHS Hospital Trusts, earning their revenue from contracts won in competition with other public hospitals. From the early 2000s, the government sought to further stimulate competition by placing contracts with a small number of private hospitals, known as Independent Sector Treatment Centres (ISTCs), that provided a selected set of planned operations and diagnostic tests. This policy was later expanded to include any private provider for all elective treatments.[5] The overall policy goal was for English NHS hospitals to operate subject to market forces rather than central guidance. Within this general policy framework, which applied to *all* hospitals, if a hospital achieved certain targets (relating primarily to financial performance and access) they were formally given a higher level of autonomy, known as Foundation Trust (FT) status. The aim was that all Trusts would get FT status by 2008, though in practice this was not achieved.[6]

These policy changes were supported by significant reforms to the management of hospitals which gradually replaced a consensus management system with a general manager who had overall responsibility for service performance and management (Baggott 1994).[7] During the wave of mid-1990s market reforms, these general managers were renamed Chief Executives, and hospitals started being subject to corporate governance standards similar to the ones brought into private sector firms in the UK in 1992 (Cadbury 1992). The role of hospital boards was strengthened, and they became responsible for managing the day-to-day operation of a hospital. Trust boards had to include the Chief Executive, a Finance Director, a Nursing Director and a Medical Director, but could have more positions.[8] These executive positions were matched by their non-executive director counterparts, who were hired with the expectation that they would need to dedicate at least three days a month to the hospital.[9] They were also, in contrast to the USA, remunerated. Boards generally met monthly (Jha & Epstein 2013) and were hands-on in terms of monitoring of the financial performance and the quality of care provided by the hospital.[10]

[5]ISTCs were privately owned and specialised in the provision of a limited set of planned treatments such as hip replacements or cataract procedures. ISTCs initially received favourable five-year contracts with revenue that did not vary with the number of patients treated (Naylor & Gregory 2009) but later on all providers (public or private) were paid according to the same DRG type tariffs.

[6]By the end of our sample period, 62% of hospitals in our sample had FT status. We control for FT status in all regressions in the paper.

[7]The initial push in favor of general managers and private sector managerial practices more broadly for the NHS came from the Griffiths' report of 1983 (Lord Griffiths was at the time the Deputy Chairman of the supermarket chain J. Sainsbury plc, and was tasked by Margaret Thatcher to study the management of the NHS).

[8]Good practice for NHS boards is set out in `https://www.leadershipacademy.nhs.uk/wp-content/uploads/2013/06/NHSLeadership-HealthyNHSBoard-2013.pdf`. A statutory instrument (`http://www.legislation.gov.uk`) sets out the board voting members.

[9]The minimum number of non-executive directors was a Chair and three others.

[10]Jha & Epstein (2013) found that approximately 40% of Boards had received formal training in quality management and that they frequently reviewed and monitored quality of care issues. 98% of Boards reported that quality of care was on agenda at every board meeting, 77% reported to actively use

In addition to managing day-to-day hospital operations, Chief Executives and their boards were also responsible for delivering government policy, which was embodied both in targets and in guidance. Performance against targets was subject to close scrutiny by central government. During most of the period we study, targets were predominantly focused on financial performance and reducing waiting times.[11] From 2001 onwards, the central government regulator published hospital ratings, which were based on detailed quantitative data on both financial and process metrics. From 2011 the targets started including clinical quality metrics.[12] Missing key performance targets could place a CEO under threat of dismissal. Ballantine et al. (2008) document a strong association between a limited number of hospital performance measures and CEO turnover between 1998 and 2005. In sum, NHS CEOs were responsible for both meeting government targets and day-to-day operations of large and complex organizations that operate in a potentially competitive market.[13]

2.2 CEO Selection, Hiring and Remuneration

Boards had guidance from the central government regulator on making senior appointments, including the CEO. CEOs were hired in a manner similar to those of private sector firms. The Chair of the Board and the appointment committee would generally use private sector headhunters for the selection and hiring of the CEO. They would also either consult with, or include, a representative from the national government organization responsible for overseeing the NHS.[14] CEOs had career paths that involved movement between different parts of the NHS. CEOs were predominantly individuals who had entered the NHS relatively early in their career (either as managers or as clinicians) and promotion was often achieved by moving between organizations in the NHS. Thus, a typical CEO would have considerable experience of working across a number of NHS organizations. However, individuals who had private sector experience (either as private consultants in the health sector or in running private sector organizations) were also sometimes appointed to CEO positions. There was also movement of CEOs to the private sector, often to posts within

patient safety data to provide staff feedback.

[11]For example, achieving FT status was conditional on meeting these targets. Propper et al. (2010) provide details on waiting times targets and their impact on performance.

[12]The focus on clinical quality was the result of an extensive investigation into systemic failure at a single hospital, Mid-Staffordshire, which revealed appalling clinical and operational practices. The final recommendations were published in 2013 in `https://www.gov.uk/government/publications/report-of-the-mid-staffordshire-nhs-foundation-trust-public-inquiry`.

[13]Bloom, Propper, Seiler & van Reenen (2015) show that NHS hospital management quality responds positively to greater product market competition.

[14]Non-FT hospitals had to include a representative of the central government regulator.

the wider healthcare sector.

CEO remuneration was set by the Board. From 2003 hospitals that had achieved FT status were free to set CEO and other executive and non-executive director pay, decided upon by the remuneration committee as in any private company.[15] The remuneration committee could also decide whether to link CEO (and other director) remuneration to corporate and individual performance. Performance, particularly against government targets, could affect CEO pay, job tenure and future rewards. Poorly performing CEOs could be dismissed and well performing CEOs rewarded by appointment to a more prestigious NHS (or private sector) organization. In addition, good performance could also be recognized by the award of a national public honour granted by the Head of State. In contrast, the pay of clinical staff (including physicians) and lower level managerial staff in all NHS hospitals was (and is) set at national level (with some regional uplifts) by a public sector pay review body and was therefore essentially the same across all hospitals.

2.3 Changes in CEO Pay

These institutional changes were accompanied by significant growth in CEO pay, both relative to the level at the beginning of the 2000s and relative to the level of pay for clinical staff and middle managers. This is shown in Figure 1, which plots the level of CEO pay over our sample period of 2000 to 2013, together with the mean pay of nurses, consultants (senior physicians) and middle managers. Over this period CEO pay was, not surprisingly, higher than pay of other NHS employees, but also increased faster. The increase in pay was larger at the top than at the bottom of the CEO pay distribution, with the difference between the 10^{th} and the 90^{th} percentile (the shaded areas in the Figure) increasing from £40,000 in 2000 to £65,000 in 2013. At the top of the distribution CEO pay increased from £120,000 in 2000 to £175,000 in 2013.[16]

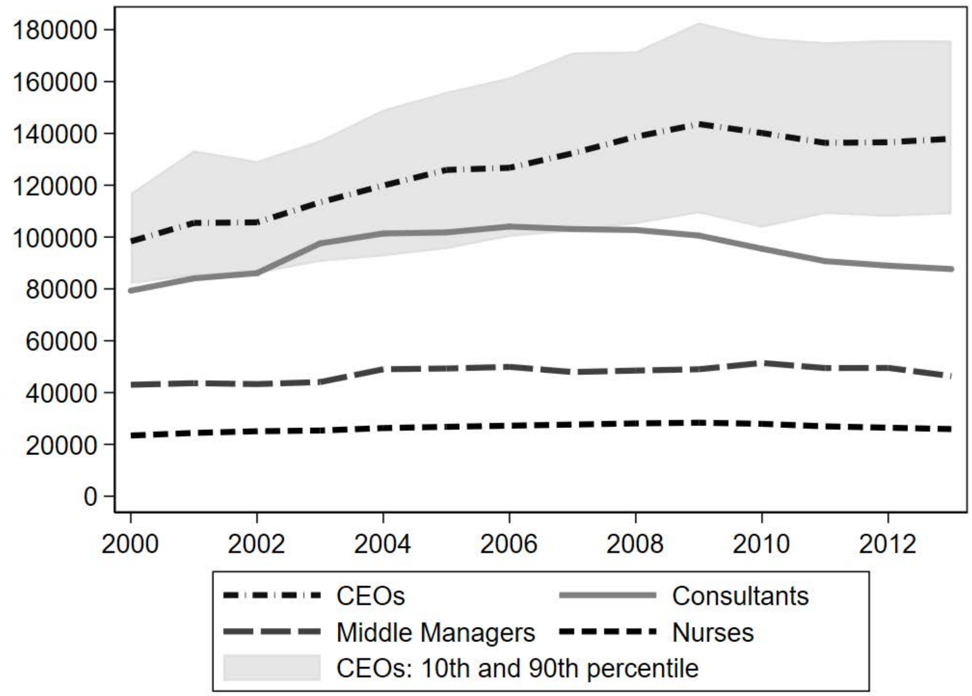
This growth in pay did not compensate for differences in pay relative to CEOs in the UK corporate sector and hospital executives in the US.[17] However, these trends put the

[15] The remuneration committee is composed of at least three independent non-executive directors. It decides on pay of all executive directors and is to position its NHS FT relative to other NHS FTs and comparable organizations (Monitor 2014). Boards of non-FTs were more constrained in their decisions on pay of both executive and non-executive directors and had to follow regulator guidance. For CEOs see `https://improvement.nhs.uk/resources/supporting-providers-executive-hr-issues/` and for Chairs and non-executive directors see `https://improvement.nhs.uk/resources/terms-and-conditions-nhs-trust-chairs-and-non-executive-directors`. Executive and non-executive directors of FT hospitals are more highly paid than directors of non-FT hospitals.

[16] NHS CEOs also receive generous pension benefits which are excluded from this analysis.

[17] Bell & Van Reenen (2016) report mean total compensation of CEOs of the top 300 UK primary-listed companies increased from £900,000 in 1999 to £1,900,000 in 2014. These remuneration packages are larger by an order of magnitude. Joynt et al. (2014) report that mean compensation of CEOs of US non-profit

Figure 1: Annual means of pay for NHS staff by job type



Note: Adjusted for inflation using Consumer Price Index, base year = 2000

remuneration packages of NHS CEOs at the high end of the compensation distribution of the UK public sector and of UK public service providers more generally.[18] To show this we examine pay data from the Quarterly Labour Force Survey, the largest household study in the UK, from April 2000 to March 2017.[19] The survey includes respondents' gross weekly pay and industry classification (SIC), occupation classification and whether they work in the public or private sector. We focus on respondents whose occupation classification is "Directors and Chief Executives of Major Organisations".[20]

Figure 2 presents pay split by industry and public and private sector. As the industry classification was changed substantially in 2009, we present separate graphs for 2000-2008

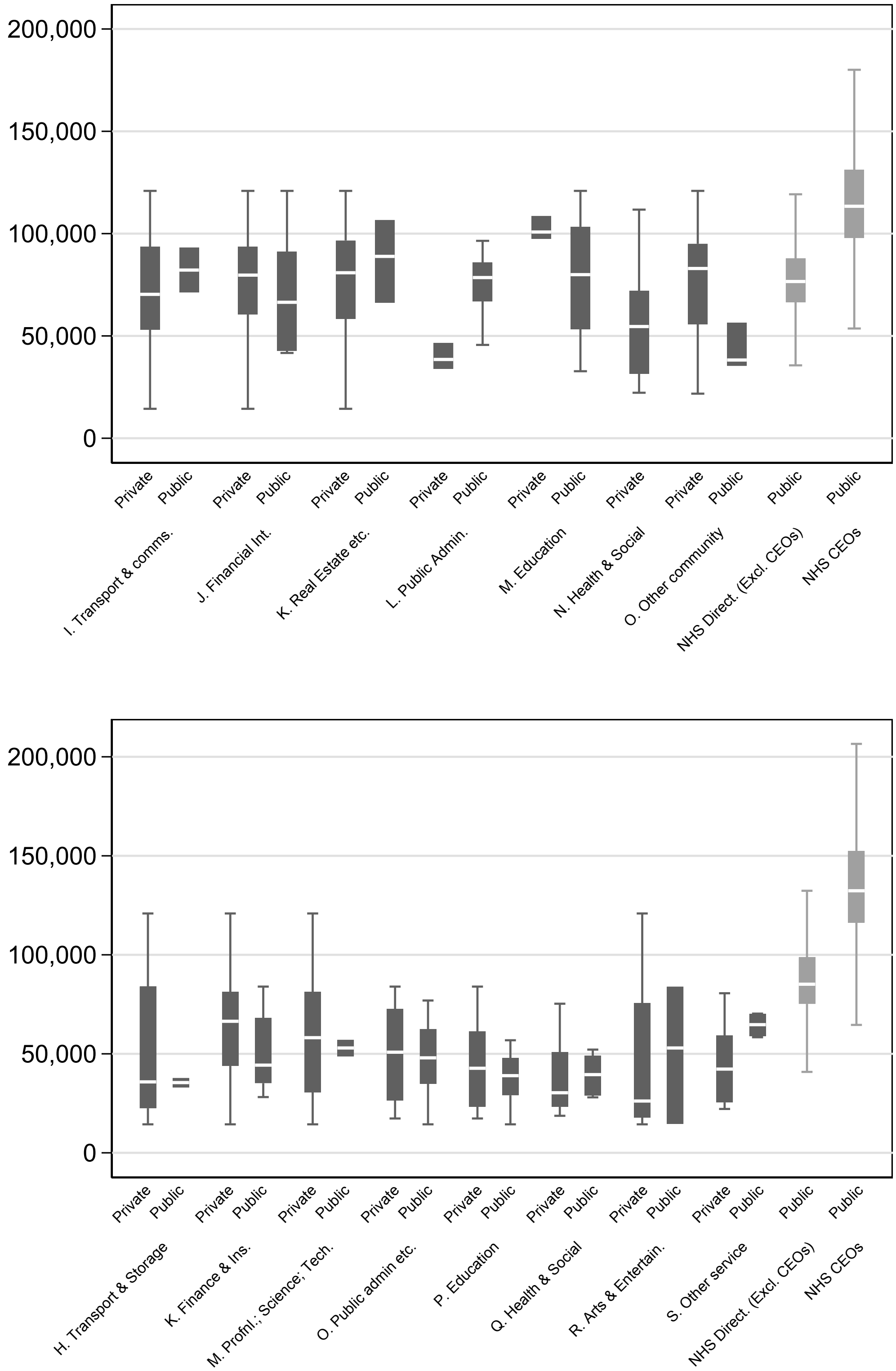
hospitals was \$596,000 (approximately £400,000) in 2009. The majority of CEOs in their sample served at hospitals with fewer than 300 beds, well below even the 25th percentile of 446 in our sample. Similarly, figures Joynt et al. (2014) report for the highest decile of the compensation distribution, which has the largest mean number of beds (310), show mean compensation of \$2,100,000 (approximately £1,400,000).

[18]The Prime Minister's salary is around £145,000. The higher pay of NHS CEOs has traditionally attracted considerable negative attention from the popular media. Articles about "NHS fat cats" receiving "six-figure salaries" or "earning more than the Prime Minister" are common (Ham et al. 2011).

[19]The QLS provides the official measures of employment and unemployment.

[20]We convert weekly gross pay to annual gross pay and adjust for inflation using the consumer price index (base year = 2000). To deal with outliers and limited cell sizes for some industry-sector combinations, we windsorize the pay data at the 5% level, with the top 5% of data replaced with the 95th percentile and the bottom 5% replaced with the 5th percentile.

Figure 2: Annual gross pay for “Directors and Chief Executives of Major Organisations” and basic pay for NHS CEOs and non-CEO directors in 2000-2008 (top) and 2009-2017 (bottom)



Notes: All pay values adjusted for inflation using Consumer Price Index (base year = 2000). Non-NHS pay data from Quarterly Labour Force Survey, winsorized at the 5% level.

and 2009-2017.[21] In both periods NHS hospital CEOs and non-CEO directors were well paid relative to top managers at a wide range of organizations in both the public and private sector. On average NHS CEOs received the largest pay packages. Further, comparison between the upper and the lower graphs suggests the gap between the pay of NHS directors and that of directors in other organizations grew rather than diminished over the period.[22] Thus, while perhaps NHS hospitals were unable to provide pay comparable to that offered in similar large and complex private sector companies, over the time period we consider NHS CEOs were among the most highly rewarded of similar executives in public sector organizations.

In sum, the reform program resulted in a significant shift in the role of hospital CEOs and in the way in which they were selected and remunerated. These changes were predicated on the belief that individual top managers–when adequately selected, compensated and monitored–could deliver improvements in efficiency and quality. We investigate the extent to which this belief is supported by evidence in the rest of the paper.

3 Data

Our analysis is based on information derived from various administrative data sources, which we have brought together for the first time. In this section, we introduce the main data sets used in the analysis and present summary statistics on the hospitals and CEOs included in the sample as well as basic information on the movement of CEOs across hospitals, which we rely on to separately identify hospital and CEO fixed effects.

3.1 Main Data Sources

Hospital-Level Variables Our starting point is a rich set of production measures at the hospital level for the financial years 2000/01 to 2013/14.[23] The NHS made these data publicly available as part of the reforms described in Section 2. Thanks to this policy of transparency, we were able to access a wide range of data on hospital production, including measures of inputs, throughputs (e.g. access to care metrics such as waiting times, which are important in a system where care is rationed), outputs (financial performance and measures of the quality of clinical care) and staff job satisfaction. Variable definitions and

[21]Details of the industries in Figure 2 and changes in the classification are in Web Appendix.

[22]These comparisons do not take into account pension entitlements which are also more generous in the NHS than in other public and private sector organizations.

[23]Technically, these data are available at NHS hospital trust level. For readability we refer to NHS hospital trusts as hospitals. The financial year runs from 1 April to 31 March.

details of the sources are in Appendix A.

CEO-level Variables We combine the hospital-level data with information on CEO pay from the NHS Boardroom Pay Reports published by IDS Incomes Data Services for 2000/01 to 2010/11, which we extended by hand-collecting data from hospitals' annual reports for 2011/12 to 2013/14. These reports provide data on salary, taxable benefits and total remuneration of executive directors for nearly all NHS hospitals.

The core executive director positions present on all hospital boards are CEO, Medical Director, Nursing Director, Finance Director and HR Director.[24] In the later years of our panel we also regularly observe a Chief Operating Officer. Additionally, there is a range of other positions such as Director of Facilities and Estate Development or Director of Information Management and Technology, which we categorize as "Other".[25] To supplement the CEO pay data, we hand-collected data on the CEO characteristics of gender, educational achievements, clinical background, private sector experience and public honours.[26]

Summary Statistics Table 1 presents descriptive statistics for the hospital production measures and executive director pay. For each variable, we show the overall mean and standard deviation as well as the mean at the beginning, in the middle and at the end of our sample period. The number of observations for the hospital production measures is determined by their availability and reflects the observations used in the estimations reported below.

On average, the hospitals included in the sample have 75,000 admissions per year, and these figures have steadily grown due to the consolidation experienced in the sector (see Figure 3b below). Our input measures reflect this consolidation. The ratio of the most skilled staff to number of beds–a measure of the labour-to-capital ratio–and the ratio of senior doctors to staff–a measure of the labour skills ratio–grew by around 70% over our sample period. The summary statistics show broad improvements in some throughput measure. For example, waiting times–the time between decision to admit and actual admission–declined from 93.5 days in 2000 to 48.9 days in 2013 and length of stay declined from 7.29 days to 4.33 days over the same time period. There were also improvements in clinical outcomes, such as acute myocardial infarction (AMI) mortality

[24]The CEO of an NHS hospital is known as the Chief Executive, but the role is that of a CEO.

[25]To ensure comparability, we drop from the pay data all observations that refer only to part of the financial year (for example, because an executive director left the hospital at some point during the year).

[26]The British honours system recognizes people who have made achievements in public life. Titles bestowed on hospital CEOs include Knight, Dame, Commander/Officer/Member of the Order of the British Empire (CBE/OBE/MBE).

Table 1: Descriptive statistics for outcome variables

	Obs.	Mean	St. Dev.	Mean of variable in 2000	2006	2013
Input measures:						
Doctors + nurses/beds	2,382	2.27	0.78	1.70	2.24	2.98
Senior doctors/staff (%)	2,396	8.57	2.64	6.24	7.89	10.6
Nurses/staff (%)	2,396	32.2	3.82	33.7	32.5	31.1
Technology index	2,399	0.38	0.24	0.29	0.39	0.43
Throughput measures:						
Admissions (count)	2,392	74,488	42,778	54,000	74,229	92,422
Length of stay, mean (days)	2,386	5.23	2.87	7.29	4.80	4.33
Day cases (%)	2,383	31.3	8.7	29.5	30.0	34.9
Waiting time, mean (days)	2,356	70.5	30	93.5	73.9	48.9
Cancelled operations (count)	2,332	373	290	401	301	404
Clinical performance measures:						
AMI deaths (%)	1,757	7.25	2.87	9.18	6.75	5.44 (2012)
Stroke deaths (%)	1,965	22.7	5.29	27.1	23.0	17.5 (2012)
FPF deaths (%)	1,920	8.94	2.58	9.16	9.20	7.21 (2012)
Readmissions (%)	2,070	9.80	1.66	8.34	10.2	11.2 (2011)
MRSA rate (per 10,000 bed days)	2,055	10.2	8.36	15.7 (2001)	16.6	2.4
Other hospital production measures:						
Staff job satisfaction (1=dissatisfied, 5=satisfied)	1,838	3.47	0.10	3.47 (2003)	3.39	3.61
Surplus (£000)	2,396	-1,965	15,101	259	-796	-4,975
Executive director pay:						
Basic pay, CPI adjusted (£)	8,749	91,182	26,675	93,672	89,944	96,753
Total pay, CPI adjusted (£)	8,760	92,353	27,454	98,010	90,813	98,069
Executive director total pay, CPI adjusted (£), by position:						
CEO	1,851	126,230	28,612	98,756	126,852	137,917
Finance Director	1,479	94,270	18,993	88,600	94,436	101,300
Chief Operating Officer	779	87,118	18,955	na	85,137	90,666
Nursing Director	1,444	80,428	15,478	71,600	80,078	84,722
HR Director	1,044	76,991	15,060	71,700	75,115	81,531
Other	2,163	79,312	18,271	66,200	78,791	83,686

AMI deaths are deaths within 30 days of emergency admission for acute myocardial infarction. Stroke deaths are deaths within 30 days of emergency admission for stroke. FPF deaths are deaths within 30 days of emergency admission for fractured proximal femur. Means of total pay in 2000 are based on only 1 observation each for Finance Director, Chief Operating Officer, Nursing Director, HR Director and Other. Definitions and sources of all variables in Table A-1 in Appendix A.

Table 2: Descriptive statistics for hospital-level control variables

	Observations	Mean	St. Dev.	Mean of variable in 2000	2006	2013
Governance measures:						
Foundation Trust (%)	2,396	30.1		0	28.8	61.6
Year of merger (%)	2,396	1.75		4.1	0.59	1.26
Years since merger	2,396	1.02	2.67	0	0.89	2.40
Acquisition (%)	2,396	1.38		0	1.18	5.03
Capital measures:						
Beds	2,396	722	402	702	727	683
Technology index	2,396	0.38	0.24	0.29	0.39	0.43
Case-mix measures:						
Patients aged 0 to 14 (%)	2,396	13.5	13.2	14.5	13.4	12.5
Patients aged 60 to 74 (%)	2,396	21.2	6.35	20.3	20.9	22.6
Patients aged 75+ (%)	2,396	20.9	6.85	18.8	20.3	23.6
Male patients (%)	2,396	44.0	5.42	43.3	43.9	44.6

Foundation Trust is a dummy variable that takes the value 1 once a hospital has achieved Foundation Trust status. Year of merger takes the value 1 in the year a hospital newly created through a merger enters the sample and 0 otherwise. Years since merger takes the value 1 in the year after a hospital newly created through a merger enters the sample, the value 2 in the following year and so on. Acquisition takes the value 1 once a hospital has been involved in a merger that is more like an acquisition, i.e. following the merger the hospital keeps its provider code while the provider code of the other hospital disappears from any records. Definitions and sources of all variables in Table A-2 in Appendix A.

rates declining from 9.18% in 2000 to 5.44% in 2012 and meticillin-resistant staphylococcus aureus (MRSA) rates declining from 15.7% in 2001 to 2.4% in 2013. For other clinical outcomes, however, the data show more stagnant results. Deaths after admission for fractured proximal femur (FPF) did not reduce much and readmissions increased. In line with the earlier literature, there is also evidence of large variations in performance across hospitals, for inputs, throughputs and outcomes.[27] Financial performance nose dived over our sample period, moving from an average surplus of £259,000 in 2000 to an average deficit of £4,975,000 in 2013. Average CEO pay (adjusted for inflation) went from £99,000 in 2000 to 138,000 in 2013. Average pay of executive directors other than the CEO also increased but less steeply.

Table 2 provides descriptive statistics on time-varying hospital characteristics that we use as controls in the hospital production function. They include governance measures, capital measures and case-mix measures. Again, we show the overall mean and standard deviation, as well as the mean at the beginning, in the middle and at the end of our sample period. The proportion of hospitals who achieved Foundation Trust status steadily increased over our sample period, reaching 62% in 2013. Merger activity fluctuated over our sample period. On average each year 1.8% of hospitals in our sample were just created through a merger. The number of beds initially increased with hospital consolidation, but then decreased as efficiency improvements in care delivery were achieved (as indicated by the decline in length of stay and the increase in the day case rate). Case-mix became more challenging over our sample period, with the proportion of older patients steadily increasing.

Table 3 summarizes the main characteristics of the 469 CEOs in our sample. About a third are women, many of whom emerged from nursing careers. About a quarter have a clinical background, the majority being nurses or allied health professionals rather than doctors. About a quarter hold a postgraduate management qualification.[28] 10% have prior experience in the private sector, sometimes between stints as CEOs in the NHS. Industries range from health care and pharmaceuticals to manufacturing, retail, transport, communications and management consultancy. About 13% of the total sample received a public honour at some point in their career. In terms of tenure as CEO, we observe the vast majority of the sample for more than one year, though their tenure is typically short (only 17% are observed for more than 10 years as CEO). The median number of years a CEO is observed in a particular CEO job is 3 years and the mean is 3.7 years.[29]

[27] Chandra et al. (2016) show large and persistent performance differentials across U.S. hospitals.

[28] These qualifications include straightforward MBAs but many CEOs hold qualifications such as an MSc in Healthcare Management or a Diploma in Health Services Management.

[29] The number of years a CEO is recorded in a CEO job is sometimes slightly less than the actual job

Table 3: Demographic and sample characteristics of CEOs

	Number	Proportion
Female	147	31%
Clinical background, of which	112	24%
Nurse or allied health professional	79	17%
Doctor	33	7%
Postgraduate management qualification	121	26%
Private sector experience	49	10%
Public honour	60	13%
Number of years observed as CEO:		
1 year	75	16%
2 to 5 years	211	45%
6 to 9 years	105	22%
10 to 13 years	59	13%
14 years	19	4%
Number of CEO jobs observed in:		
1 job	324	69%
2 jobs	105	22%
3+ jobs	40	9%
Observations	469	

3.2 Identifying Movements of CEOs across Hospitals

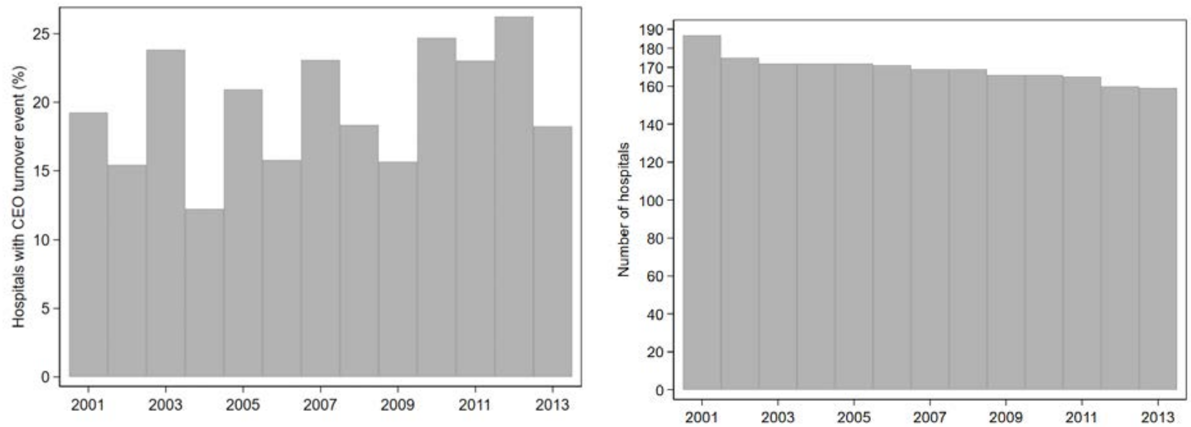
The CEO pay data provide information on where each Chief Executive worked and when, which we use to identify CEO turnover events and movements of CEOs across hospitals.[30] The NHS hospital sector is characterized by very high CEO turnover rates. Figure 3a shows that every year between 12 to 25% of hospitals in our sample have a turnover event.[31] The high turnover rates are partly due to hospital consolidation. Figure 3b shows the decrease in the number of hospitals on which the turnover rates in Figure 3a are based. From the late 1990s to the early 2000s, the NHS experienced a wave of consolidation activity. Over half the stock of NHS acute hospitals in 1997 had been involved in some kind of merger or reconfiguration with other NHS hospitals by the end

duration. If the CEO served for only part of a financial year we do not know the number of months they served in that year.

[30]To identify moves of CEOs across hospitals, we manually checked the personal identifiers for all executive directors in the data.

[31]As our data start in 2000 we report turnover events only from 2001 onward. Some hospitals experienced more than one CEO turnover event in a financial year.

Figure 3: CEO turnover and hospital consolidation



(a) Annual proportion of hospitals with CEO turnover event

(b) Annual number of hospitals

of 2003 (Gaynor et al. 2012).[32]

Figure 4a shows sample entry and exit of hospitals between 2000 and 2013. At the beginning of our sample period in 2000 and 2001 over 10% of hospitals exit the sample. Hospitals only enter the sample because of mergers: following a merger, the merged entity was generally given a new NHS code. We treat each new code as a separate hospital in our analysis.[33] Figure 4b shows sample entry and exit of CEOs over the same period. Hospital consolidations mechanically led to increased CEO turnover, since at the very least only one of the CEOs of the formerly separate hospitals continued in post, and frequently a new CEO was appointed to lead the consolidated hospital. However, CEO turnover appears to have increased even in absence of merger events. CEO sample entry and exit is on average considerably higher than hospital sample entry and exit, at around 14% for the whole period. CEO entry and exits in our sample are highest during the period of consolidation in the early 2000s and then fall and remain relatively stable after 2004, but are still both over 10% at the end of the period.[34]

Figure 5a shows CEO turnover per hospital for the subset of hospitals observed for at least 11 years.[35] These hospitals have on average 3.5 CEOs during the sample period. Only a minority of hospitals have the same CEO throughout and the majority have two to five CEOs over the sample period of 11 to 14 years. Hospitals with more CEOs over the

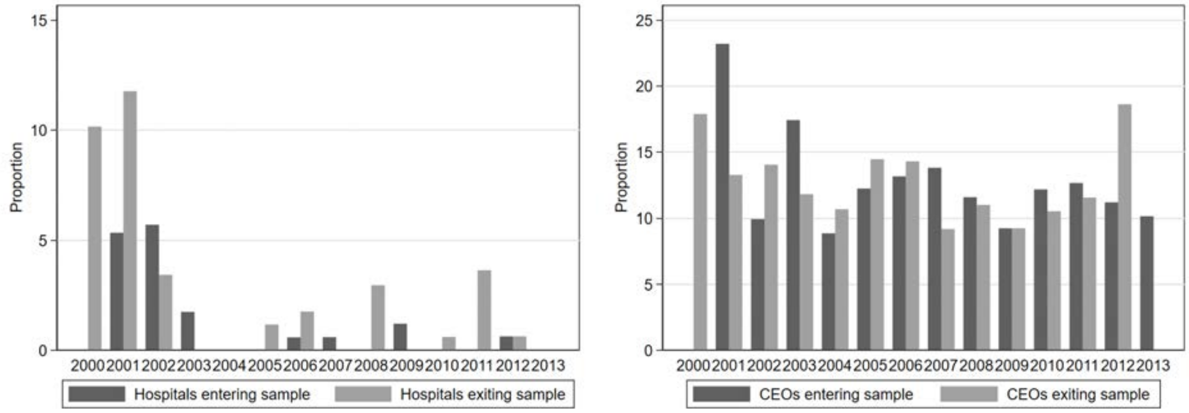
[32] All consolidation was within the NHS. Consolidation meant that NHS hospitals grew in size, providing services from a number of sites in the same local area. There are no NHS hospital chains.

[33] Mergers in which a much larger hospital absorbed a smaller hospital and kept its name and NHS code are captured by the acquisition dummy variable.

[34] The rise in exits in 2012 reflects the uptick in hospital consolidation in 2011.

[35] As our data set excludes CEOs that served for a part year only, the number of CEOs per hospital is a lower bound.

Figure 4: Sample entry and exit of hospitals and CEOs



(a) Proportion of hospitals entering and exiting sample in each year

(b) Proportion of CEOs entering and exiting sample in each year

sample period tend to be in certain broad regions of England but few other time-invariant characteristics are associated with the number of CEOs a hospital has over our sample period.[36] Figure 5b shows that a sizeable number of CEOs are observed as CEOs in 2 or more NHS hospitals, reflecting the numbers in Table 3.

To examine whether mover CEOs are different, we regress fixed characteristics of the CEO against a dummy variable indicating whether a CEO is one of the 95 CEOs in our fixed effects estimation sample (which requires they are a mover and that they are in each hospital for at least two years). The characteristics we examined were gender, whether the CEO has a clinical qualification and whether they have a postgraduate management qualification. Mover CEOs are slightly more likely to have a postgraduate management qualification but do not differ in terms of gender or clinical background from the rest of the CEOs in our sample.[37] Tenure for these CEOs is short, the median being 4 years and the mean being 4.5 years.[38]

4 Methods

4.1 CEO Fixed Effects in Pay

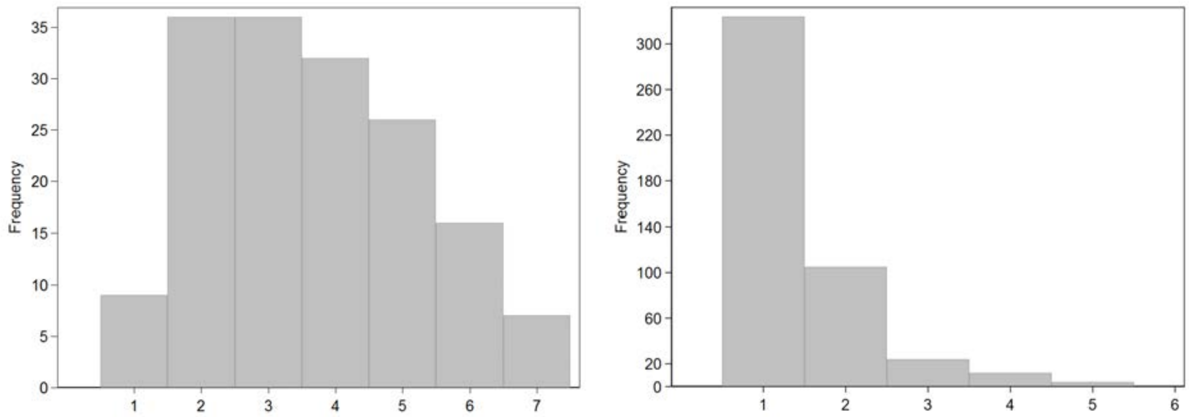
We follow Abowd et al. (1999) to estimate CEO fixed effects in pay. We use pay data for all executive directors (i.e. including COOs, Finance Directors, HR Directors, Nursing

[36]Details are in Table W-1 in the Web Appendix.

[37]For details see Table W-2 in the Web Appendix.

[38]For these 95 CEOs, real pay rises after a move by just over 18 percent. 60 percent of CEO moves are across different large geographical areas (NHS regions).

Figure 5: Number of CEOs per hospital and number of hospital spells per CEO



(a) CEOs per hospital (N = 162)

(b) Hospital spells per CEO (N = 469)

Notes: CEOs per hospital is the number of CEOs observed per hospital for hospitals observed for at least 11 years. Hospital spells per CEO is the number of CEO spells at different hospitals for executive directors that are observed in a CEO position at least once.

Directors and other directors but excluding Medical Directors as only some of their remuneration is recorded in the executive pay data).[39] As discussed by Abowd et al. (1999), between hospital mobility of the executive directors is essential for the identification of the hospital effects. Including all executive directors, and not just CEOs, increases the size of the set of hospitals connected by worker mobility, and also produces more reliable estimates of the hospital effects.

Including all executive directors requires adding controls for the different director positions, since different positions receive markedly different pay packages. Identifying the coefficients on the director position controls, however, is very challenging because, in a regression with hospital fixed effects and executive director fixed effects, the coefficients on the director position controls are identified only by the handful of executive directors changing director position. We therefore employ a two-step estimation procedure in which we first regress executive directors' pay on a set of dummy variables indicating their board level position:

$$\begin{aligned} pay_{it} = \delta_1 + \delta_2 COO_{it} + \delta_3 finance\_director_{it} + \delta_4 HR\_director_{it} \\ + \delta_5 nursing\_director_{it} + \delta_6 other\_director_{it} + \varepsilon_{it} \end{aligned} \tag{1}$$

where pay_{it} denotes pay of executive director i in financial year t. COO_{it} is an indicator

[39]Medical Directors salaries in the directors' data are lower than those of other directors. The majority of Medical Director remuneration is for clinical work, which is excluded from the directors' data.

variable that takes the value one if the job title of executive director i during financial year t was Chief Operating Officer and zero otherwise. Similarly, the other variables indicate a board position as Finance Director, HR Director, Nursing Director and Other type of executive director, respectively. CEO is the omitted category. We estimate this regression using the same observations that we include in the second step.[40] We extract the residuals to use as the outcome variable in Equation 2.

In the second step we estimate the wage equation for the largest subset of hospitals that are connected by executive directors moving between them:

$$pay\_residual_{it} = \boldsymbol{X}'_{j(i,t)t}\boldsymbol{\beta} + \gamma\, tenure_{ij(i,t)t} + \lambda_t + \alpha_i + \psi_{j(i,t)} + \varepsilon_{it} \tag{2}$$

The left-hand side variable, $pay\_residual_{it}$, is the residual from Equation 1, i.e. the pay of executive director i in period t net of the impact of their board level position. The function $j(i,t)$ maps executive director i to hospital j in financial year t. $\boldsymbol{X}_{jt}$ is a vector of time-varying observable hospital characteristics that includes merger status, number of beds, a technology index and case mix measures (more details are in Appendix A). $tenure_{ij(i,t)t}$ is the tenure of executive director i at hospital $j(i,t)$ in financial year t. A full set of financial year effects, λ_t, provides non-parametric control for trends in pay that are national in scope while a full set of hospital effects, $\psi_{j(i,t)}$, controls for non-time varying unobserved differences between hospitals. The estimates of interest are the executive director effects α_i, which capture non-time varying unobserved characteristics that affect directors' pay. ε_{it} represents the error term.

For observations not connected by worker mobility it is not possible to identify separate executive director effects α_i and hospital effects $\psi_{j(i,t)}$ (Abowd et al. 1999). In our sample this is not an issue as there are only 162 pay observations in 17 hospitals that are not connected by worker mobility. The connected set, on the other hand, has 478 movers that connect 196 hospitals.

We calculate the proportion of the variance in the pay variable, $pay\_residual_{it}$, that is explained by the covariates, $\boldsymbol{X}_{j(i,t)t}$, $tenure_{ij(i,t)t}$ and λ_t, the hospital effects, $\psi_{j(i,t)}$, and the executive director effects, α_i, respectively. For the hospital effects this proportion is simply $[\mathrm{Cov}(pay\_residual_{it}, \hat{\psi}_{j(i,t)})/\mathrm{Var}(pay\_residual_{it})] \times 100$. For the executive director effects it is $[\mathrm{Cov}(pay\_residual_{it}, \hat{\alpha}_i)/\mathrm{Var}(pay\_residual_{it})] \times 100$. To obtain the proportion explained by the covariates, we first calculate the pay residual predicted by the coefficient estimates for the covariates, $pay\_\widehat{residual}_{it} = \boldsymbol{X}'_{j(i,t)t}\hat{\boldsymbol{\beta}} + \hat{\gamma}\, tenure_{ij(i,t)t} + \hat{\lambda}_t$. In the next step we use this prediction to calculate the covariance:

[40] In the second step we only include observations for which we can separately identify executive director and hospital effects.

$$[\text{Cov}(pay\_residual_{it}, \widehat{pay\_residual_{it}})/\text{Var}(pay\_residual_{it})] \times 100.$$

4.2 CEO Fixed Effects in Hospital Production

Our approach exploits movement of the same CEO across different hospitals–the fixed effects approach pioneered by Bertrand & Schoar (2003). We adapt this approach to take advantage of our many measures of hospital production, which we group into sets reflecting inputs, throughputs and clinical performance. We begin by outlining the basic Bertrand & Schoar (2003) approach and then discuss how we take advantage of our many production measures in a "stacked" version.

Basic version The fixed effects approach proposed by Bertrand & Schoar (2003) involves estimating regressions of the following form:

$$y_{jt} = \boldsymbol{X}'_{jt}\boldsymbol{\beta} + \lambda_t + \psi_j + \alpha_{i(j,t)} + \varepsilon_{jt} \tag{3}$$

The left-hand side variable, y_{jt}, is a production measure of hospital j in financial year t. The function $i(j,t)$ maps hospital j to CEO i in financial year t. $\boldsymbol{X}_{j(i,t)t}$ is the same set of time varying hospitals variables included in Equation 2. We also include a full set of financial year effects, λ_t, which non-parametrically controls for trends in hospital performance that are national in scope while a full set of hospital effects, ψ_j, controls for non-time varying unobserved differences between hospitals. The estimates of interest are the CEO effects $\alpha_{i(j,t)}$. ε_{jt} represents the error term.

The estimated CEO effects are essentially the mean of the residuals of a regression of y_{jt} on $\boldsymbol{X}_{jt}$, λ_t and ψ_j over the observations of the hospitals the CEO has been observed in, for the financial years the CEO has been observed there. Following Bertrand & Schoar (2003), we estimate CEO effects $\alpha_{i(j,t)}$ only for the subset of CEOs observed in two hospitals for at least two years each.[41] As Bertrand & Schoar (2003) discuss, the estimated CEO effect for a CEO observed in only one hospital for only part of the time for which that hospital is observed would be identified but would be a period-hospital-specific effect rather than a CEO effect.[42] We follow Bertrand & Schoar (2003) and present F-statistics from tests

[41] This sampling requirement means effects that matter require that corporate practices be correlated across two hospitals when the same CEO is present and gives CEOs time to "make their mark" (Bertrand & Schoar 2003). For CEOs observed in three or four hospitals for at least two years in each, we use only the two most recent spells to be comparable with the other CEOs.

[42] There are two further issues when determining the CEOs for whom we estimate CEO effects. One, some CEOs are observed in a hospital for two years but they served for only part of each of these two years. We define these observations as not complying with our requirement of being observed for at least two years. Two, the CEO effect for a CEO observed in one hospital for the exact same time period we

of the joint significance of the estimated CEO effects to assess the statistical significance of these estimates.

To enable comparison of the CEO effects in hospital production with those in pay, we present the equivalent to the variance proportions described in Section 4.1. We calculate the proportion of the variance in the hospital production measures, y_{jt}, that is explained by the covariates, $\boldsymbol{X}'_{jt}\boldsymbol{\beta}$ and λ_t, the hospital effects, ψ_j, and the CEO effects, $\alpha_{i(j,t)}$. The proportion of the variance explained by the hospital effects and the CEO effects are $[\text{Cov}(y_{jt}, \hat{\psi}_j)/\text{Var}(y_{jt})] \times 100$ and $[\text{Cov}(y_{jt}, \hat{\alpha}_{i(j,t)})/\text{Var}(y_{jt})] \times 100$, respectively. To obtain the proportion explained by the covariates, we calculate $\hat{y}_{jt} = \boldsymbol{X}'_{j(i,t)t}\hat{\boldsymbol{\beta}} + \hat{\gamma} + \hat{\lambda}_t$ and use this prediction to calculate the covariance: $[\text{Cov}(y_{jt}, \hat{y}_{jt})/\text{Var}(y_{jt})] \times 100$.

Stacked Version To take advantage of our large number of hospital production measures , we implement stacked versions of Equation 3 and estimate a common CEO effect $\alpha_{i(j,t)}$ for sets of variables. We stack our four input measures, our five throughput measures, and our five clinical performance measures and estimate a single $\alpha_{i(j,t)}$ for each set.[43] We also examine two production measures–staff satisfaction and surplus–separately as these measures are not standard input, throughput or clinical performance measures.

The stacked regressions take the following form:

$$y_{jt}^{k_s} = \sum_{k_s=1}^{K_s} z_{k_s}[\boldsymbol{X}'_{jt}\boldsymbol{\beta} + \lambda_t + \psi_j] + \alpha_{i(j,t)} + \varepsilon_{jt}^{k_s} \tag{4}$$

The left-hand side variable, $y_{jt}^{k_s}$, is one of the K_s outcome variables in set s. All K_s outcome variables in set s are stacked, so the sample size is approximately K_s times the number of hospital-year observations jt. Each outcome variable is standardised to have mean zero and a standard deviation of one. Furthermore, variables such as waiting times and stroke deaths are multiplied by (-1), so an increase in an outcome variable can be interpreted as an improvement. The additional (compared to Equation 3) variable z_{k_s} is a dummy variable that takes the value one if the left-hand side variable $y_{jt}^{k_s}$ is outcome variable k_s. For each of the K_s outcome variables in set s, we estimate separate coefficients on the time-varying observable hospital characteristics $\boldsymbol{X}_{jt}$, a separate set of financial year effects λ_t and a separate set of hospital effects ψ_j by interacting the hospital characteristics, financial year effects and hospital effects with the dummy variables z_k. Only the estimates for the CEO effects $\alpha_{i(j,t)}$ are common to all k_s outcome variables in set s. We estimate

observe the hospital for would be perfectly collinear with the hospital effect ψ_j. Therefore we ignore such observations when determining which CEO observations meet our estimation requirements.

[43]Table 1 presents descriptive statistics for the components of the stacked sets.

standard errors that are clustered at hospital level.

The stacked version of the Bertrand & Schoar (2003) method realizes the full potential of our data. Firstly, it tackles the issue of missing observations for some of the hospital production measures.[44] When estimating Equation 3, for each y_{jt} we have to use only those CEO observations which are observed at two hospitals for at least two years. The stacked approach in Equation 4 allows us to relax this requirement. We require CEOs to have at least two observations for at least two of the outcome variables in set s at two different hospitals. This relaxed requirement increases the number of CEOs for whom we can estimate a CEO effect $\alpha_{i(j,t)}$. Secondly, the stacked approach increases the sample size and therefore gives us more statistical power to detect CEO effects. Thirdly, it allows for correlations between the outcome variables in a set and reduces potential multiple comparison issues. Finally, it simplifies the exposition of the results. For completeness, we report results from analyses examining each hospital production measure separately in the Web Appendix. Reassuringly, these results are consistent with the results for the stacked production measures.

Placebo Experiments Using Random Assignments of CEOs A possible shortcoming of the Bertrand & Schoar (2003) approach, in the basic and stacked versions, is that a large residual in one hospital might result in a large mean residual and therefore a CEO effect estimate $\hat{\alpha}_{i(j,t)}$ that is statistically significantly different from zero as a consequence of a period-hospital-specific effect rather than as a consequence of a persistent CEO effect. We therefore assess the validity of F-tests on CEO effects by randomly assigning CEOs to both their first and second hospital and re-estimating.

Our starting point for the random assignment are those CEO spells used in Equation 4. For example, a CEO might be observed at Hospital A from 2001/02-2004/05 and at Hospital B from 2005/06-2008/09. We randomly assign this CEO to a hospital for the period 2001/02-and to another hospital for 2005/06-2008/09. The set of hospitals used in the random assignment is the same set as that used in the non-random assignment estimates. To ensure that each hospital is assigned to only one CEO at a time, we sample hospitals without replacement and remove a hospital that has been assigned to a CEO spell from the pool for the duration of the CEO spell it has been assigned to. We then estimate Equation 4 for the sample with the random CEO-hospital matches $i(j,t)$, test the joint significance of the CEO effects using an F-test, count the number of CEO effects that are individually statistically significant, and calculate the proportion of the variance

[44] Observations are missing because certain production measures are not relevant for the particular hospital. For example, some specialist hospitals have no admissions for acute myocardial infarction (AMI), so we have no observations on AMI deaths for these hospitals.

of the left-hand side variable y_{it} that is explained by the CEO effects. We repeat this process 100 times and compare the means over the 100 replications to the values obtained using the actual CEO-hospital matches $i(j,t)$.

Two-step Procedure Bertrand & Schoar (2003) propose an alternative two-step procedure for assessing the impact of individual CEOs on firm behaviour. Again, for ease of exposition, we begin by describing this procedure for the non-stacked case. In the first step, each measure of production, y_{jt}, is regressed on the vector of time-varying observable hospital characteristics $\boldsymbol{X}_{jt}$, the financial year effects λ_t and the hospital effects ψ_j:

$$y_{jt} = \boldsymbol{X}'_{jt}\boldsymbol{\beta} + \lambda_t + \psi_j + \varepsilon_{jt} \tag{5}$$

For each CEO observed in two hospitals for at least two years duration in each, two residual means are calculated from Equation 5. These are the mean of the residuals for the financial years $t_1^{i,A}$ to $t_n^{i,A}$ when CEO i is observed in hospital A and the mean of the residuals for the financial years $t_1^{i,B}$ to $t_n^{i,B}$ when CEO i is observed in hospital B. In the second step, the mean for CEO i's spell in hospital B is regressed on the mean for CEO i's spell in hospital A:

$$\frac{1}{n^{i,B}} \sum_{t=t_1^{i,B}}^{t_n^{i,B}} e_{Bt} = \delta_1 + \delta_2 \frac{1}{n^{i,A}} \sum_{t=t_1^{i,A}}^{t_n^{i,A}} e_{At} + \varepsilon_i \tag{6}$$

The coefficient of interest is δ_2. A positive value indicates that individual CEOs' deviations from the expected level of the dependent variable y_{jt} are similar across two different hospitals, which would be supportive of a persistent CEO effect.

To implement the stacked approach in the two-step procedure, we estimate the stacked equivalent of Equation (5):

$$y_{jt}^{k_s} = \sum_{k_s=1}^{K_s} z_{k_s}[\boldsymbol{X}'_{jt}\boldsymbol{\beta} + \lambda_t + \psi_j] + \varepsilon_{jt}^{k_s} \tag{7}$$

As in Equation 4, each outcome variable $y_{jt}^{k_s}$ is standardised to have mean zero and a standard deviation of one and variables such as waiting times and stroke deaths are multiplied by (-1). We extract the residuals $e_{jt}^{k_s}$ and for each hospital-year observation jt generate the mean residual across all k_s outcome variables in set s: $\overline{e_{jt}^{s}} = \frac{1}{K_s}\sum_{k_s=1}^{K_s} e_{jt}^{k_s}$.[45]

[45] In the case of a missing observation for one or two of the K_s outcome variables, the mean residual could be a mean over $K_s - 1$ or $K_s - 2$ outcome variables.

For each CEO with at least two observations for at least two of the outcome variables in set s in both hospitals, we generate the mean of the mean residuals $\overline{e^s_{jt}}$ for each of the CEO's spells and regress them on each other as in (6):

$$\frac{1}{n^{i,B}} \sum_{t=t_1^{i,B}}^{t_n^{i,B}} \overline{e^s_{Bt}} = \delta_1 + \delta_2 \frac{1}{n^{i,A}} \sum_{t=t_1^{i,A}}^{t_n^{i,A}} \overline{e^s_{At}} + \varepsilon^s_i \quad (8)$$

We check the validity of the two-step procedure by extending the above-described placebo experiment using random assignment of CEOs to Equations 7 and 8.

Pre-assignment Trends As a robustness test, we run a placebo regression proposed by Bertrand & Schoar (2003). Instead of using the mean of the mean residuals at hospital B during the time the CEO was observed there, $\frac{1}{n^{i,B}} \sum_{t=t_1^{i,B}}^{t_n^{i,B}} \overline{e^s_{Bt}}$, we use the mean of the mean residuals, $\overline{e^s_{jt}}$, at hospital B during the three financial years before the CEO arrived at hospital B. The idea is that a positive δ_2 in Equation 8 might wrongly suggest that individual CEOs have an impact on hospital production. Instead, hospital boards might recruit CEOs that have experience of an environment similar to the one the hospital is currently operating in. For example, a CEO who has overseen a shift to more day case procedures at hospital A might be recruited to oversee a similar shift to more day case procedures at hospital B. In this case, deviations from the expected proportion of day case procedures at hospital B might precede the new CEO's arrival. A positive association between CEO i's deviations from the expected proportion of day cases at hospital A and hospital B's deviation from the expected proportion during the three years before CEO i arrived there is suggestive of selection of the CEO rather than of the CEO imposing their style. On the other hand, if hospital B's deviations from the expected proportion during the three years before CEO i arrived are completely unrelated to the deviations during CEO i's spell at hospital B, we can have more confidence that a positive estimate of δ_2 from Equation 8 indicates the impact of the CEO on hospital performance.

Non Parametric Approach The fixed effects approach relies heavily on our statistical model of hospital production, since both Equation 4 and Equation 7 use the residuals from this statistical model to estimate the impact of individual CEOs. Both equations also rely on CEOs having an impact on the same dimension of hospital production across two hospitals. We use a non-parametric approach to sidestep both problems. Our approach resembles a difference-in-difference estimator combined with matching. We compare the changes in hospital production following a CEO turnover event to changes in hospital production at matched hospitals without a CEO turnover event. If there is any impact of CEOs on hospital production, we expect to see different changes after a CEO turnover event compared to changes at otherwise similar hospitals with no CEO turnover event. We describe the non parametric approach and associated results in detail in Appendix B.

5 Results

5.1 CEO Effects in Pay

Table 4 reports the results from estimating Equation 2 and shows the proportions of the variance in the pay variables that are explained by the covariates, the hospital effects, the director effects and the residuals, respectively.[46] We estimate Equation 2 only for the pay observations in the largest connected set, which comprises of 478 movers that connect 196 hospitals (the vast majority of the sample). Table 4 shows that in the connected set the director effects are jointly statistically significant. The hospital effects, executive director effects and covariates jointly explain more than 85% of the variation in executive director pay, with each accounting for around 30% of the variation.

Table 4 also presents results for the subset of directors observed in a CEO position at least once (397 of the 2,111 executive directors in the connected set) and for the further subset of CEOs that are included in the parametric analyses of CEO impacts on hospital production (95 of the 397 CEOs). The director effects are jointly statistically significant in both subsets and the variance decompositions are similar across all three sets.

The interquartile range in hospital (firm) effects is around £16,000. In the Web Appendix we present correlates of this variation. We find that hospital effects in pay are higher in teaching hospitals and lower in specialist hospitals. There is also considerable regional variation, reflecting regional differences in the cost of living.

Figure 6a shows the distribution of the pay effects for all directors in the connected set, the 397 who were ever CEOs and the subset of 95 CEOs. Since the $\hat{\alpha}_i$ are estimated relative to an arbitrary omitted director, we have transformed the estimates into deviations from the mean of all $\hat{\alpha}_i$. The distribution for the 95 CEOs included in the hospital production analyses lies slightly to the right of the distribution for all CEOs. The interquartile range is around £18,000 for the full sample of director effects and for the subsample of directors ever observed as CEO. For the 95 CEOs used in the hospital production analyses the range is slightly smaller at £15,000.[47]

[46] We report only the results for total pay as the results for basic pay are very similar.

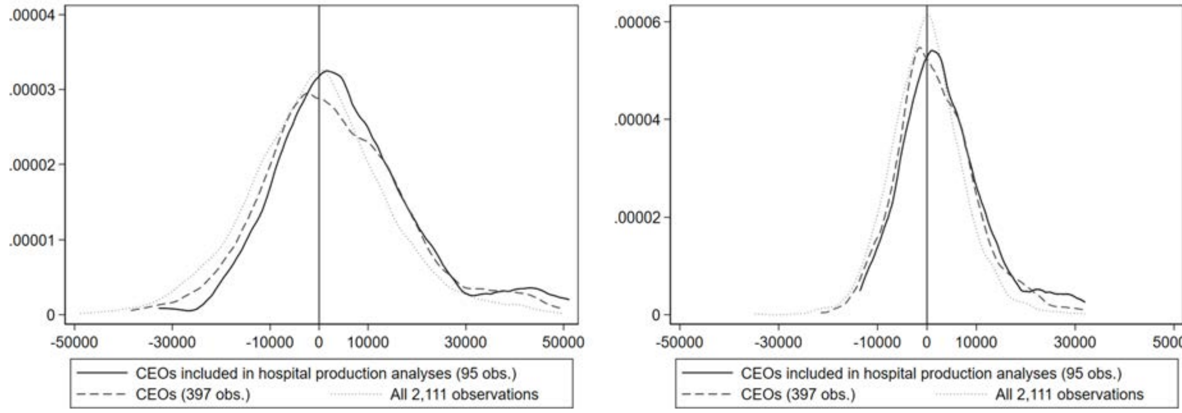
[47] By position, the interquartile range is smallest for Nursing Directors at £15,000, followed by HR Directors at £16,000. For Finance Directors and COOs the range is around £17,000. For directors that

Table 4: Summary statistics for the pay regressions

	F-test of joint significance of director effects (p-value, df1, df2)	R^2	Obs.	Variance proportions (%) Co-variates	Hospital effects	Director effects	Re-siduals	Hospitals	Persons	Movers
Connected set	5.73 (0, 2111, 6430)	0.87	8,760	27.5	32.2	27.1	13.2	196	2,111	479
Subset of directors observed as CEO at least once	7.49 (0, 397, 6430)		1,851	26.1	25.6	31.9	16.3	196	397	171
Subset of CEOs included in fixed effects regressions	11.15 (0, 95, 6430)		633	20.8	27.8	31.2	20.3	122	95	94
Outside connected set			162					17	47	

The dependent variable is the residual from a regression of total pay (CPI adjusted) on job title dummies. Variance proportion is the proportion of variance in the pay variable that is explained by the covariates, the hospital effects, the director effects and the residuals, respectively. Covariates are financial year effects, foundation trust status, year of merger, years since merger, beds, technology index, case mix variables and tenure.

Figure 6: Kernel density plots of director effects in total pay



(a) Estimates

(b) Shrunk estimates

Notes: Kernel density plots of deviations of estimated director effects in total pay, CPI adjusted, from mean. Shrunk estimates obtained using empirical Bayes shrinkage estimator.

Due to sampling error the distribution of the estimates of the director effects in pay will overstate the true distribution of the effects. We therefore adjust the effect estimates using an empirical Bayes procedure and shrink the effect estimates towards the overall mean.[48] Figure 6b presents the distribution of the shrunk effect estimates. As for the unadjusted estimates, the distribution for the 95 CEOs included in the hospital production analyses lies slighty to the right of the distribution for all CEOs. As expected, the distribution is much narrower. At around £9,000 the interquartile range is approximately halved compared to the unadjusted estimates.

In Table 5 we examine which personal and sample-specific characteristics are associated with the CEO pay effects (original estimates as well as shrunk estimates) and test whether there are differences in these associations between all CEOs in our sample and the 95 CEOs in the hospital production analyses. We report results from a series of regressions, each examining one CEO characteristics.[49] We find that female CEOs are paid statistically significantly less, with the effect being even stronger for the subset of CEOs included in the hospital production analyses. As being female and some of the other characteristics such as clinical background are highly correlated, we include in all remaining regressions a control for female and for the interaction of female and being in the 95 CEOs subset. WE find CEOs who received a public honour and CEOs with a clinical background are paid

we categorize as "Other" the interquartile range is £19,500 and therefore exceeds the £18,000 range for the directors that were ever observed as CEO.

[48]We employ the empirical Bayes shrinkage estimator described in Chandra et al. (2016).

[49]In the Web Appendix we present results from a regression that includes all CEO characteristics simultaneously.

Table 5: Associations between estimated director effects in total pay and personal characteristics for subset of directors observed in a CEO position at least once

	Original effect estimates	Shrunk effect estimates	Obs. in category
Female	-3,547* (1,935)	-1,929* (994)	125
Female × in 95 CEOs subset	-5,923 (3,917)	-3,542* (2,013)	31
R^2	0.033	0.043	
Clinical background	5,252** (2,280)	2,057* (1,175)	96
Clinical backgr. × in 95 CEOs subset	-5,527 (4,644)	-1,921 (2,393)	25
R^2	0.046	0.051	
Nurse or allied health professional	360 (2,685)	70 (1,390)	69
Nurse or AHP × in 95 CEOs subset	-4,055 (5,424)	-1,937 (2,808)	20
Doctor	13,835*** (3,419)	5,544*** (1,770)	27
Doctor × in 95 CEOs subset	-6,057 (7,903)	-690 (4,091)	5
R^2	0.077	0.073	
Postgraduate managm. qualification	-3,822* (2,060)	-1,582 (1,060)	107
PG managm. qual. × in 95 CEOs subset	3,373 (3,987)	1,262 (2,052)	31
R^2	0.041	0.049	
Public honour	6,684** (2,782)	3,966*** (1,420)	51
Public honour × in 95 CEOs subset	2,215 (5,178)	2,091 (2,643)	16
R^2	0.057	0.079	
Private sector	-5,499* (3,110)	-2,364 (1,599)	42
Private sector × in 95 CEOs subset	10,337* (5,467)	5,144* (2,811)	14
R^2	0.043	0.052	
Observed as CEO for 2 to 9 years	-2,120 (2,922)	-1,051 (1,492)	290
Observed as CEO for 10 plus years	7,051* (3,827)	4,116** (1,954)	76
10 plus years × in 95 CEOs subset	-773 (4,306)	-9.6 (2,198)	43
R^2	0.074	0.096	
Observed in 2 CEO jobs	2,031 (2,955)	1,053 (1,514)	102
Observed in 3+ CEO jobs	7,183* (4,263)	3,906* (2,184)	38
3+ CEO jobs × in 95 CEOs subset	2,665 (6,224)	1,925 (3,189)	24
R^2	0.051	0.067	

All regressions from the second panel onwards control for female and female × In 95 CEO subset. The executive director effects are extracted from the regressions reported in Table 4 and transformed into deviations from the mean of all estimated executive director effects. Standard errors in (parentheses). 397 observations in all regressions. *Significant at 10%, **significant at 5%, ***significant at 1%

more (the effect being driven by doctors rather than nurses). CEOs with a postgraduate management qualification are paid less. CEOs with leadership experience in the private sector are paid less, but the association is positive for the 95 CEOs subset. CEO observed in our sample for 10 years or longer are paid more, as are CEOs observed in 3 or more CEO jobs. Importantly, there are few differences in the patterns of these associations between the 95 CEOs included in the parametric analyses of CEO impacts on hospital production and the CEOs not used in these analyses.

In sum, we find sizeable differences in pay across CEOs in the NHS, suggesting that employers perceive CEOs to be differentiated and pay them accordingly. We now investigate whether there is also evidence of CEO differentiation in terms of hospital production.

5.2 Are there CEO Effects in Hospital Production?

Main Estimates We start by estimating Equation 4 using the sets of production measures. We first use actual CEO-hospital matches and then run the placebo experiment using random matches. In Table 6 we present the stacked input, throughput and clinical performance measures results.[50] In Table 7 we present staff job satisfaction and financial surplus results.

Results for the actual CEO-hospital matches are presented in the first row of each panel. The R^2 (Column 3) is high for all variables, suggesting that the hospital effects, the CEO effects, the financial year effects and our measures of time-varying hospital characteristics jointly explain a large proportion of the variation in these measures. The F-tests (Column 1) suggest that the estimated CEO effects are jointly statistically significantly different from zero for all our hospital production measures, including staff job satisfaction and surplus. The proportion of CEO effects that are individually significantly different from zero (Column 2) varies from 24.2% for surplus to 47.1% for inputs.

Columns 5 to 8 present, for the subsample of hospital-year observations with at least one CEO effect $\alpha_{i(j,t)}$ (i.e. hospital-year observations when at least one of the 95 CEOs is present), the proportion of the variance in the hospital production measures that is explained by each term in Equation 4: the covariates (time-varying hospital characteristics + year effects), the hospital effects, the CEO effects and the residuals. A considerable proportion of the variance is accounted for by the observed covariates and the hospital effects (with the exception of surplus). The CEO effects, despite being jointly statistically significant as measured by the F-test, explain less than 6% of the variance in the performance measures. Again, surplus is the exception, as the variance proportion explained by

[50] We repeat all analyses presented in this section for each of the variables in the sets separately. These results corroborate the stacked results. See Tables W-5, W-6 and W-7 in the Web Appendix.

Table 6: Estimates of CEO effects for stacked outcome measures using actual CEO-hospital matches as well as random CEO-hospital matches

	F-test of joint significance of CEO effects (p-value/rejection frequency using 1% signif. level, df1, df2)	Number (prop.) of CEO effects statist. signif. at 5%	R^2	Total hospital-year obs.	Variance proportions (%) for subsample of obs. with at least one non-zero CEO effect: Co-variates	Hospital effects	CEO effects	Re-siduals	Subsample hospital-year obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Input variables									
Actual matches	54.8 (<0.001, 95, 224)	39 (41.1%)	0.89	9,573	26.4	58.8	2.84	11.9	3,298
Random matches:									
Means	98.4 (100%, 93.6, 224)	31.1 (33.2%)	0.89	9,573	25.1	62.4	1.50	11.0	3,348.0
(Std. dev.)	(80.6) (n.a., 1.15, 0)	(4.61, 4.93)	(0.0004)		(1.51)	(1.88)	(0.61)	(1.05)	(48.4)
Throughput variables									
Actual matches	75.0 (<0.001, 95, 224)	35 (36.8%)	0.86	11,849	56.0	26.0	1.65	16.4	4,091
Random matches:									
Means	65.4 (100%, 93.6, 224)	30.9 (33.0%)	0.86	11,849	47.4	36.5	1.17	14.9	4,163.0
(Std. dev.)	(42.0) (n.a., 1.16, 0)	(4.66, 4.93)	(0.0004)		(5.36)	(5.55)	(0.40)	(1.00)	(62.6)
Clinical performance variables									
Actual matches	12.6 (<0.001, 82, 223)	23 (28.1%)	0.63	9,767	37.9	24.7	1.72	35.7	3,098
Random matches									
Means	39.7 (100%, 79.4, 223)	25.7 (32.4%)	0.63	9,767	30.2	29.7	1.99	38.1	3,056.0
(Std. dev.)	(24.7) (n.a., 1.73, 0)	(5.03, 6.42)	(0.0008)		(1.75)	(1.70)	(0.49)	(1.11)	(69.0)

df = degrees of freedom. df1 is the number of CEO effects, df2 is the number of hospital clusters. All outcome variables in a stacked set are standardised to have a mean of zero and a standard deviation of one. Some outcome variables in the stacked sets are multiplied by (-1), so that "more" means "better". Standard errors used for the statistical significance tests are clustered at hospital level. Variance proportion is the proportion of variance in the pay variable that is explained by the covariates, the hospital effects, the director effects and the residuals, respectively. Covariates are financial year effects, foundation trust status, year of merger, years since merger, beds (except for input measures), technology index (except for input measures) and case mix variables. The results for random CEO-hospital matches are means and standard deviations across 100 replications.

Table 7: Estimates of CEO effects for staff job satisfaction and surplus using actual CEO-hospital matches as well as random CEO-hospital matches

	F-test of joint significance of CEO effects (p-value/rejection frequency using 1% signif. level, df1, df2) (1)	Number (prop.) of CEO effects statist. signif. at 5% (2)	R^2 (3)	Total hospital-year obs. (4)	Variance proportions (%) for subsample of obs. with at least one non-zero CEO effect: Co-variates (5)	Hospital effects (6)	CEO effects (7)	Re-siduals (8)	Subsample hospital-year obs. (9)
Staff job satisfaction									
Actual matches	14.85 (<0.001, 73, 176)	24 (32.9%)	0.76	1,838	44.7	24.1	5.3	25.9	609
Random matches:									
Means	48.8 (100%, 72.9, 176)	23.7 (32.6%)	0.77	1,838	42.2	29.0	7.0	21.7	623.7
(Std. dev.)	(33.9) (n.a., 1.12, 0)	(4.68, 6.46)	(0.004)		(2.32)	(3.36)	(2.49)	(1.67)	(11.0)
Surplus									
Actual matches	32.6 (<0.001, 95, 224)	23 (24.2%)	0.31	2,396	4.7	13.9	19.5	61.9	830
Random matches:									
Means	44.6 (100%, 93.8, 224)	23.9 (25.5%)	0.29	2,396	4.9	17.8	13.8	63.5	843.6
(Std. dev.)	(36.9) (n.a., 1.06, 0)	(4.27, 4.56)	(0.01)		(1.42)	(2.99)	(3.42)	(3.16)	(12.1)

df = degrees of freedom. df1 is the number of CEO effects, df2 is the number of hospital clusters. All outcome variables are standardised to have a mean of zero and a standard deviation of one. Standard errors used for the statistical significance tests are clustered at hospital level. Variance proportion is the proportion of variance in the pay variable that is explained by the covariates, the hospital effects, the director effects and the residuals, respectively. Covariates are financial year effects, foundation trust status, year of merger, years since merger, beds, technology index and case mix variables. The results for random CEO-hospital matches are means and standard deviations across 100 replications.

the covariates and the variance proportion explained by the hospital effects is less than the variance proportion explained by the CEO effects.

Table 8: Association of (1) means of residuals for CEO spells in first and second hospital and (2) pre-assignment trend and mean of residuals for CEO spell in second hospital

	1			2		
	CEO spells at 1st and 2nd hospital			Pre-assignment trend and CEO spell at 2nd hospital		
	Coefficient (std. error)	R^2	Obs.	Coefficient (std. error)	R^2	Obs.
Input measures	0.055	0	95	0.13	0.01	92
	(0.11)			(0.11)		
Throughput measures	0.20*	0.03	95	0.10	0.01	92
	(0.11)			(0.12)		
Clinical perform. measures	-0.04	0	82	-0.10	0.02	79
	(0.10)			(0.09)		
Staff job satisfaction	-0.07	0	73	-0.11	0.01	73
	(0.11)			(0.17)		
Surplus	-0.05	0	95	0.16	0.01	92
	(0.30)			(0.22)		
Total pay	0.28***	0.10	93			
	(0.09)					

Results in Column 1 are from regressions of mean of residuals for CEO spell in second hospital on mean of residuals for CEO spell in first hospital. Results in Column 2 are from regressions of mean of residuals for CEO spell in second hospital on mean of residuals in second hospital during the three years before CEO was appointed. The residuals are from a regression of the standardised stacked measures or the standardised individual measures on hospital characteristics, financial year effects and hospital effects. For the stacked measures, the residuals for the 4 or 5 outcome measures in each group are then averaged by hospital-year before the mean of residuals for each CEO spell is being calculated. *Significant at 10%, **significant at 5%, ***significant at 1%

Robustness While these results suggest the existence of statistically significant CEO effects, a deeper look at the data suggests otherwise. First, from the random CEO-hospital matches reported in the second and third row of each panel, it is clear the means of the F-statistics across the 100 replications are as large as for the actual CEO-hospital matches. The F-test rejects the null hypothesis of the randomly generated CEO effects jointly being equal to zero for every one of the 100 replications, a rejection frequency of 100% at a nominal significance level of 1%. Similarly, the mean of the proportion of CEO effects that are individually statistically significantly different from zero is very similar to that for the actual CEO-hospital matches. Finally, the mean variance proportion explained by the CEO effects when CEOs are randomly assigned to hospitals is very similar to the variance proportions explained by the CEO effects using the actual assignments. The largest difference is for surplus, but even for this measure the ratio of variance explained by the CEO effect to the residual is similar across actual and random matches. These results suggest that the CEO effect estimates, and therefore the F-tests, may be capturing period-hospital-specific shocks rather than true CEO effects.

Second, the results from the alternative two-step method of Equations 7 and 8 also indicate issues in the estimated CEO fixed effects. These two-step results are presented in Column 1 of Table 8.[51] In these regressions a positive coefficient indicates that a positive deviation from the expected level of a set of production measures during a CEO's spell at the first hospital is associated with a positive deviation from the expected level of that set of production measure during the CEO's spell at the second hospital. A statistically significant association would suggest that these deviations can be attributed to the CEO and not to period-hospital-specific effects. However, the coefficients are very small or even negative, suggesting that there is no association between the residuals at the first and second hospital. The only exception is for the set of throughput measures, which has a coefficient of 0.20.[52] For comparison, the last row of Table 8 presents results from a regression that uses mean residuals from a regression of executive director pay on hospital characteristics, tenure, financial year effects and hospital effects–essentially the regression in Equation 2 without the executive director effects α_i. In contrast with the production measures, the statistically significant coefficient of 0.28 suggests that a CEO who was paid more than expected in their first hospital is also paid more than expected in the second.

Third, examination of pre-assignment trends suggests that the positive correlation for the throughput measures could be due to selection rather than CEOs imposing their style. Column 2 of Table 8 presents associations between the mean of the residuals in the second hospital where each CEO is assigned during the three years *before* the CEO arrived and those *after* the CEO is appointed. For the throughput measures the coefficient in the test for pre-assignment trends is 0.10, similar to the coefficient of 0.20 in Column 1. While, in general, the coefficients in Column 2 are similar or larger than those in Column 1, none are statistically significant, so indicating the absence of significant pre-assignment trends.

Overall, our analysis indicates that the statistical significance of the estimated CEO fixed effects is driven by hospital-period-specific shocks rather than persistent CEO effects.

[51]We repeat these analyses for each of the variables in the sets separately (Table W-8 in Web Appendix.)

[52]We run the placebo experiment using random assignments of CEOs to check the validity of the two-step procedure (Table W-9 in Web Appendix). They show no impact of CEOs, as expected for random matches, indicating the two-step procedure is valid.

Additional Results We use the spell-specific residual means to explore if there is any indication of "management styles". For example, we might observe that CEO spells with positive deviations in one set of outcome measures also tend to have positive deviations in another set of outcome measures. To investigate if any such patterns exist, we examined the correlations between the spell-specific residual means across the different sets of production measures. Four of the ten correlation coefficients among the five sets of stacked variables that we consider in the analysis are negative and six are positive but they are all small and none are statistically significant.[53] We also examined the correlations between the shrunk CEO pay effects and the spell-specific residual means of the stacked production measures derived from the two step procedure. Again, the five correlation coefficients are small and none are statistically significant.[54]

6 Potential Reasons for Lack of Persistent CEO Effects

The results show significant and persistent differences in pay across CEOs but little evidence of persistent CEO effects in production. We explore three potential explanations that might reconcile these two seemingly conflicting findings. First, we study whether there is evidence of endogenous assignment. If "better" CEOs have a higher probability of being hired by more problematic hospitals, it could make it harder for these CEOs to replicate a positive impact across hospitals. Second, we examine the effects of CEO tenure on hospital performance. This analysis is motivated by the observation that mover CEOs tend to have short tenure on average, so the finding of no CEO impacts may reflect the inability to affect hospital production in a short time frame. Finally, we consider whether there is evidence of match effects. Match effects would imply that CEO impacts are specific to the fit between CEO and hospital, rather than being portable across hospitals.

6.1 Endogenous Assignment

The lack of persistent CEO fixed effects we find might have been generated by endogenous assignment of CEOs to hospitals. A CEO who experiences a positive shock in one hospital may subsequently be hired by a hospital at which it is difficult to bring about positive changes. In this case, an above expected performance would be followed by a

[53]The largest coefficients are: -0.17 for (inputs, surplus), -0.08 for (inputs, staff satisfaction), 0.09 for (throughputs, staff satisfaction), and 0.08 for (inputs, clinical performance).

[54]The correlation coefficients are: 0.03 for (inputs, CEO pay effect); 0.11 for (throughputs, CEO pay effect); -0.004 for (clinical performance, CEO pay effect); 0.045 for (staff job satisfaction, CEO pay effect) and -0.09 for (surplus, shrunk CEO pay effects).

below average performance, resulting in no apparent CEO fixed effect and a negative correlation between performance in the first hospital and performance in the second. Other indications of this type of assignment would be if CEOs who have higher variability in their performance are at some point in a hospital that is hard to manage, or if CEOs who are well paid are assigned at some point to a hard-to-manage hospital.

To test these ideas, we begin by identifying hospitals which may be hard-to-manage because they are experiencing problems. We consider four definitions of "problematic". These are (i) having received a poor rating from the government regulator of hospitals before the CEO arrived at the hospital, (ii) having poor financial performance, defined as being in the lowest 25th percentile of surplus in the year before the CEO arrived, (iii) being a 'new' hospital that was created through a merger at some point during our sample period, and (iv) holding a contract for a large capital investment (a PFI contract) at some point during the CEO's tenure.[55]

We first examine whether CEOs who were good performers at their first hospital are subsequently hired by a problematic hospital. We define CEO performance using the mean of the mean residuals $\overline{e^s_{jt}}$ from Equation 7 for the financial years $t_1^{i,A}$ to $t_n^{i,A}$ when CEO i is observed in hospital A. We classify as good performers those CEOs whose mean of the mean residuals is at or above the 75$^{\text{th}}$ percentile. We estimate linear probability models of the impact of good performance in the first hospital on moving to a probleamatic hospital. Table 9 presents the results.[56] There are 4 definitions of problematic and 4 sets of performance measures, generating 16 coefficient estimates. Only one of the estimates (the association between higher than expected throughputs in the first hospital and moving to a hospital created through a merger) is statistically significant and then it is negative, indicating that good performers are *less* likely to subsequently be hired by a hospital that has experienced a merger event.

Second, we examine whether variability in CEO performance is associated with being at some point matched with a problematic hospital. We generate a measure of the variability in CEO performance as follows. We use the mean of the mean residuals from Equation 7 for the financial years $t_1^{i,A}$ to $t_n^{i,A}$ when CEO i is observed in hospital A and the mean of the mean residuals for the financial years $t_1^{i,B}$ to $t_n^{i,B}$ when CEO i is observed in hospital B. To measure variability in CEO performance, we calculate the absolute value

[55]NHS hospitals have to borrow for large capital investments from the private market. Borrowing is through vehicles with long-term fixed interest rates and long payback periods, known as private finance initiative (PFI) contracts. Hospitals with these contracts have often struggled to meet financial performance requirements once the payback period has begun. Regulator ratings were not issued each year. Details are in Appendix A.

[56]Tables W-17 and Table W-18 in Web Appendix repeat Tables 9 and 10 using each variable separately.

Table 9: Linear probability models of the impact of good performance in first hospital on moving to a “problematic” hospital

	Hospital commission rating poor in year before CEO arrived			Hospital with surplus below 25th percentile in year before CEO arrived			‘New’ hospital created through merger during sample period			Hospital with PFI contract at some point during CEO’s tenure		
	Good perf.	Const.	N	Good perf.	Const.	N	Good perf.	Const.	N	Good perf.	Const.	N
Throughput measures	0.06	0.41	71	-0.01	0.46	91	-0.19*	0.36	95	-0.15	0.46	95
	(0.14)	(0.07)		(0.12)	(0.06)		(0.11)	(0.05)		(0.12)	(0.06)	
Clinical performance measures	0.02	0.42	70	-0.19	0.53	78	-0.19	0.38	82	-0.21	0.49	82
	(0.14)	(0.07)		(0.13)	(0.07)		(0.12)	(0.06)		(0.13)	(0.06)	
Staff job satisfaction	0.13	0.33	52	0.13	0.40	72	0.04	0.28	73	0.21	0.37	73
	(0.16)	(0.08)		(0.13)	(0.07)		(0.12)	(0.06)		(0.13)	(0.07)	
Surplus	0.14	0.39	71	0.02	0.46	91	-0.07	0.33	95	0.08	0.40	95
	(0.14)	(0.07)		(0.12)	(0.06)		(0.11)	(0.06)		(0.12)	(0.06)	

Each entry in this table refers to a separate regression of an indicator of a CEO moving to a “problematic” hospital on an indicator of good performance at the CEO’s first hospital. “Problematic” hospital is defined as either poor hospital commission rating, surplus below 25^{th} percentile, hospital created through merger or hospital with PFI contract. The 25^{th} percentile of surplus is calculated separately for each financial year to ensure the categorisation is net of year effects. Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%

Table 10: Impact of ever being observed at a “problematic” hospital on variability in CEO performance as measured by the absolute difference in the mean residuals for the CEO spells at each of their two hospitals for each production measure

	Hospital commission rating poor in year before CEO arrived	Hospital with surplus below 25th percentile in year before CEO arrived	‘New’ hospital created through merger during sample period	Hospital with PFI contract at some point during CEO’s tenure	Mean (st. dev.) [obs.] of dependent variable (Absolute difference in mean residuals at both hospitals)
Input measures	0.03 (0.02) [89]	0.03 (0.02) [92]	-0.02 (0.02) [95]	-0.03 (0.02) [95]	0.14 (0.11) [95]
Throughput measures	0.003 (0.02) [89]	0.02 (0.02) [92]	0.05** (0.02) [95]	0.02 (0.02)	0.13 (0.10) [95]
Clinical performance measures	-0.005 (0.04) [79]	-0.08* (0.05) [79]	-0.09** (0.04) [82]	-0.08* (0.04) [82]	0.20 (0.19) [82]
Staff job satisfaction	-0.03 (0.06) [70]	-0.03 (0.06) [73]	-0.03 (0.06) [73]	-0.15*** (0.06) [73]	0.31 (0.24) [73]
Surplus	0.18 (0.16) [89]	-0.05 (0.17) [92]	-0.10 (0.16) [95]	0.18 (0.16) [95]	0.46 (0.76) [95]

Each entry in this table refers to a separate regression of a performance variability measure on a dummy variable indicating that the CEO has ever been observed at a “problematic” hospital defined as either poor hospital commission rating, surplus below 25^{th} percentile, hospital created through merger or hospital with PFI contract. The 25^{th} percentile of surplus is calculated separately for each financial year to ensure the categorisation is net of year effects. Standard errors in (parentheses) and number of observations in [brackets]. *Significant at 10%, **significant at 5%, ***significant at 1%

of the difference in these two means. We generate this variability measure for all sets of production measures and examine whether the variability measure is larger for CEOs who at some point in their career work in problematic hospitals. To do this, for each of the four definitions of "problematic", we regress each of the 5 variability measures against a dummy variable indicating that the CEO was ever observed in a problematic hospital.

Table 10 presents the results. 5 out of the 20 coefficients are statistically significantly different from zero at the 10% significance level. However, only one of these (for the set of throughput measures) is positive, where a positive coefficient would suggest that being at a "problematic" hospital is associated with higher variability in CEO performance. For the other four statistically significant coefficients the estimated association is negative, suggesting that CEOs who are at some point at a more problematic hospital actually have lower variability in their performance across hospitals.

Table 11: Linear probability models of the impact of a large positive CEO fixed effect in pay ($\geq 75^{th}$ percentile) on ever being observed in a "problematic" hospital

	Hospital commission rating poor in year before CEO arrived	Hospital with surplus below 25^{th} percent. in year before CEO arrived	'New' hospital created through merger during sample period	Hospital with PFI contract at some point during CEO's tenure
CEO fixed effect in total pay $\geq$ £11,370	-0.11	-0.19*	-0.03	0.13
	(0.12)	(0.11)	(0.12)	(0.12)
Constant	0.59	0.74	0.41	0.54
	(0.06)	(0.06)	(0.06)	(0.06)
R^2	0.01	0.03	0.00	0.01
Observations	89	92	95	95

Each column refers to a separate regression of an indicator of a CEO ever being observed at a "problematic" hospital on an indicator of a large positive CEO fixed effect in total pay. "Problematic" hospital is defined as either poor hospital commission rating, surplus below 25^{th} percentile, hospital created through merger or hospital with PFI contract. The 25^{th} percentile of surplus is calculated separately for each financial year to ensure the categorisation is net of year effects. The CEO fixed effects in total pay are the estimated executive director effects from the total pay regression in Table 4, transformed into deviations from the mean of all estimated executive director effects and extracted for the subset of CEOs included in the analyses of CEO fixed effects in hospital production. Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%

Third, we examine whether CEOs who have a large positive pay effect–and are therefore viewed by the market as good performers–were ever hired at a problematic hospital. Table 11 presents the results. The first column shows that CEOs with large positive pay effects are less likely to be hired at hospitals rated as low quality, though the estimate is not statistically significantly different from zero. The second column of the table presents the association of the CEO pay effect and the financial performance of the hospitals they join. CEOs with large positive pay effects are significantly less likely to work at hospitals with low surplus. The third column examines whether more highly paid CEOs are ever hired at a hospital created through a merger. The estimate indicates that this is not the case. The fourth column examines whether any of the hospitals in which highly paid CEOs are observed had PFI contracts. The estimate is positive but not statistically significant.

We conclude that there is little evidence that CEOs viewed by the market as good performers are more likely to be hired by problematic hospitals. If anything, CEOs with large positive pay effects are less likely to be hired by such hospitals during their careers.

6.2 Short Tenure

Another reason for the lack of persistent impact of the CEOs may be that their tenure is too short for their effect on hospital production to materialize consistently across hospitals (the average period for which a CEO is in post in our full sample is only around 3.5 years). To examine this possibility, we look at the association between tenure and CEO production. To do this we use the residuals from Equation 7, i.e. hospital-year deviations from the expected level of our different hospital production measures.

Since tenure is measured separately for each CEO spell, for this analysis we can examine all CEOs in our data set (i.e. we can include CEOs who were not observed at two different hospitals or who did not spend at least two years at a hospital.) We estimate the following regression:

$$\overline{e^s_{jt}} = \alpha + \delta tenure_{i(j,t)} + \gamma tenure\_unsure_{i(j,t)} + \varepsilon^s_{jt} \tag{9}$$

The dependent variable is $\overline{e^s_{jt}} = \frac{1}{K_s}\sum_{k_s=1}^{K_s} e^{k_s}_{jt}$, the mean of residuals across all k_s outcome variables in set s for hospital year jt. The *tenure* variable takes discrete values between 1 and 14. We allow for left censoring in our data by including the indicator variable *tenure_unsure*, which takes the value 1 for CEO spells that start in 2000 and 0 otherwise. For the two non-stacked production measures (staff satisfaction and surplus) we use the residuals e_{jt} generated by Equation (5) as the dependent variable.

Table 12 presents the results. These results are very similar for both the 95 CEOs included in the fixed effects approach and for all CEO spells. For the stacked sets of production measures the effect sizes are very small. Even the largest coefficient estimate (for throughputs) suggests that 10 years of tenure would result in these outcomes being only one-sixteenth of standard deviation above the expected level. For staff job satisfaction

Table 12: Association of tenure and residuals for CEO spells included in main analysis and all observed CEO spells

	CEO spells included in fixed effects approach			All CEO spells		
	Coefficient on tenure var. (std. error)	R^2	Obs.	Coefficient on tenure var. (std. error)	R^2	Obs.
Input measures	0.002 (0.005)	0.017	854	-0.001 (0.002)	0.001	2,538
Throughput measures	0.006* (0.003)	0.006	854	0.000 (0.002)	0.000	2,534
Clinical performance measures	0.008 (0.007)	0.009	733	-0.002 (0.003)	0.000	2,307
Staff job satisfaction	0.025* (0.013)	0.012	629	0.015** (0.006)	0.005	2,012
Surplus	0.025** (0.012)	0.004	858	0.010* (0.006)	0.001	2,541

All regressions include a dummy variable indicating that tenure is unsure. The residuals are from a regression of the standardised stacked measures and the extra measures on hospital characteristics, financial year effects and hospital effects. For the stacked measures, the residuals for the 4 or 5 outcome measures in each set are then averaged by hospital-year. "All CEO spells" excludes spells at hospitals which we observe for only one year since hospital effects predict the outcome variable perfectly. "All CEO spells" also excludes hospital-year observations for which we observe fewer than 2 of the hospital production measures in a stacked set. Standard errors clustered at hospital level. *Significant at 10%, **significant at 5%, ***significant at 1%

and surplus, however, there is a positive and significant correlation with CEO tenure. A 10-year tenure is associated with staff job satisfaction and surplus being one quarter of a standard deviation above the expected level. The positive association between long tenure on staff job satisfaction mirrors the finding from our event study in Table B-1 (described in detail in Appendix B) that a CEO turnover event reduces staff job satisfaction. More generally, these results provide some support to the idea that the short tenure of CEOs in the NHS may dampen their ability to systematically impress their mark on the organizations they lead, though the possible endogeneity of CEO tenure make it hard to pin down the direction of causality behind these correlations.

6.3 Match Effects

As discussed, the overall significance of the estimated CEO fixed effects is, in fact, a reflection of hospital-period-specific variations in hospital production. This consideration led us to examine whether certain individuals may perform better in certain environments. Put another way, are there match effects, such that some individuals can achieve better outcomes in certain environments but that this performance is not portable to another environment?

To estimate the importance of such match effects, we adopt an approach similar to Jackson (2013). We explore CEO characteristics and hospital characteristics that may produce different matches. The CEO characteristics we focus on are those associated with differences in CEO pay effects (see Table 5). They are being female, having a clinical background, having leadership experience in the private sector and holding a postgraduate management qualification. The hospital characteristics are standard dimensions on which NHS (and other) hospitals differ: teaching status, foundation trust (hospitals which were granted more autonomy by the regulator), hospitals located in competitive markets, and large hospitals (measured in terms of beds).

We estimate the effect of interactions of CEO characteristics W_i and hospital characteristics W_j on the sets of production variables, staff job satisfaction and surplus using the following regression:

$$y_{jt}^{k_s} = \sum_{k_s=1}^{K_s} z_{k_s}[\boldsymbol{X}'_{jt}\boldsymbol{\beta} + \lambda_t + \psi_j] + \delta W_i + \gamma(W_i \times W_j) + \varepsilon_{jt}^{k_s} \tag{10}$$

As in Equation 4, $y_{jt}^{k_s}$ is one of the K_s outcome variables in set s, $\boldsymbol{X}_{jt}$ are time-varying observable hospital characteristics, λ_t are a set of financial year effects and ψ_j a set of hospital effects. The interaction term $W_i \times W_j$ replaces the CEO effects $\alpha_{i(j,t)}$

Table 13: Estimates of quality of CEO-hospital matches for CEO spells included in main analysis

	Input measures	Through-put measures	Clinical performance measures	Job satis-faction	Surplus
Female * teaching hosp.	-0.06	-0.26***	0.09**	-0.07	0.08
	(0.09)	(0.04)	(0.04)	(0.08)	(0.11)
Female * foundation hosp.	0.07	-0.06	0.01	-0.14	-0.09
	(0.06)	(0.04)	(0.06)	(0.17)	(0.16)
Female * competitive	-0.03	0.04	0.03	-0.13	0.34
	(0.06)	(0.06)	(0.06)	(0.18)	(0.26)
Female * beds (100s)	-0.00	-0.00	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Clinical backgr.* teaching hosp.	-0.02	0.11	0.20**	-0.07	0.42
	(0.07)	(0.14)	(0.08)	(0.12)	(0.26)
Clinical backgr. * foundation	0.01	-0.01	0.07	-0.24	-0.20
	(0.05)	(0.06)	(0.07)	(0.15)	(0.18)
Clinical backgr. * competitive	0.03	0.10	0.05	0.22	-0.10
	(0.07)	(0.06)	(0.08)	(0.15)	(0.24)
Clinical backgr. * beds (100s)	-0.00	0.00	0.01	0.01	-0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)
Private sector * teaching hosp.	-0.07	-0.03	0.21**	-0.02	0.13
	(0.06)	(0.12)	(0.09)	(0.18)	(0.29)
Private sector * foundation hosp.	0.03	0.08	0.03	0.20	0.34**
	(0.09)	(0.09)	(0.16)	(0.32)	(0.16)
Private sector * competitive	0.10**	-0.09	-0.11	-0.42***	-0.19
	(0.04)	(0.05)	(0.07)	(0.14)	(0.17)
Private sector * beds (100s)	-0.00	-0.01	0.02*	-0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
PG managm. qual. * teaching	0.04	0.03	0.03	-0.09	-0.73
	(0.05)	(0.10)	(0.10)	(0.11)	(0.70)
PG managm. qual. * foundation	0.01	0.03	0.04	0.00	0.04
	(0.04)	(0.05)	(0.05)	(0.13)	(0.16)
PG managm. qual. * competitive	-0.07	0.07	0.02	0.11	0.17
	(0.04)	(0.05)	(0.07)	(0.13)	(0.19)
PG managm. qual. * beds (100s)	0.01	0.00	0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Observations	11853	9576	9772	1838	2391

Each estimate is from a separate regression of the stacked measures on hospital characteristics, financial year effects, hospital effects, the relevant CEO characteristic and the interaction of the relevant CEO characteristic and hospital characteristic. All outcome variables in a stacked set as well as the individual outcome variables are standardised to have a mean of zero and a standard deviation of one. Some outcome variables in the stacked sets are multiplied by (-1), so that "more" means "better". Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%

from Equation 4. We also include the constitutive terms of the interaction term: the CEO characteristic directly as W_i and the hospital characteristic indirectly through the hospital fixed effects ψ_j.[57]

Table 13 presents the results. Each entry is the coefficient on the interaction term from a separate regression. The rows indicate the interactions and are ordered by person characteristics. The columns are the production measure sets. The table indicates that, whilst there are not match effects for all combinations of person and hospital type, there do seem to be hints of match effects which fit with the institutional set-up of the hospitals we study.

First, female CEOs have better clinical outcomes in teaching hospitals, but at the expense of lower throughputs (which are more easily observed than clinical outcomes, perhaps explaining the lower pay of female CEOs). Second, CEOs with a clinical background are associated with better clinical performance in teaching hospitals. These hospitals are exactly those settings where it might be expected that clinically trained CEOs perform best. Third, CEOs with leadership experience in the private sector appear to respond to external incentives more than those with an NHS background, in that they achieve higher surpluses when paired with a foundation trust, a setting in which surpluses are important because they were required to achieve foundation trust status. On the other hand, as perhaps would be expected of those regarded as adhering less to 'NHS values', they have less staff job satisfaction when operating in a competitive market.[58]

Overall, for the CEO characteristics gender, clinical background and private sector experience, it appears that certain CEO-hospital matches results in higher production than other matches. This analysis is subject to the caveat that our samples are small and any match effects may therefore be driven by a small number of individuals observed in particular hospital settings. It is also likely that matching happens along CEO and hospital characteristics that are unobservable to us (e.g. CEO social or leadership skills.)

7 Conclusions

In this paper we examine whether the CEOs of large public sector entities have an impact on the performance of those organizations. Our focus is large public hospitals, for which we assemble rich information on CEOs and a large number of hospital production measures. We find that the pay of NHS CEOs differs systematically and persistently. However,

[57] We can extend this analysis to include all CEO spells in our data set but, as we are exploring whether match effects explain the absence of persistent CEO impacts, we present results for only the 95 CEOs used in the fixed effects approach. Results for all CEO spells are in Web Appendix.

[58] Results for all observed CEO spells in Table W-19 in the Web Appendix are broadly similar.

we find little evidence of CEOs being systematically able to change the performance of hospitals.[59] Our results do not seem to be driven by the allocation of better performing CEO, as measured either by performance or in terms of their individual pay effect, to worse performing hospitals. But we provide suggestive evidence that NHS CEOs may be in post for too short a time to have an effect, and that certain CEOs may matter in certain conditions, i.e. that there are CEO-hospital match effects. These match effects, together with the short tenure of hospital CEOs in the English NHS system, may be the reason why certain CEOs are paid more than others: when the average tenure period is short, the market cannot distinguish between a good match and good performance in all settings.

More broadly, there are at least two possible explanations for our findings. The first is public sector-specific. The NHS is central in political discourse in the UK. Its importance means that politicians are very concerned about NHS performance, particularly negative performance, and are also keen to be seen to be doing something, which is generally manifest in a desire to implement new policies. The lack of persistent CEO effects is consistent with a scenario in which top managers simply chase political goals, rather than policies that might actually improve hospital performance (see, for example, qualitative studies in Powell & Davies (2016). In this context, the rational response of a NHS CEO is not necessarily to improve the long-term performance of the hospital but, instead, to minimize the amount of bad news that ends up on the Secretary of State's desk. This situation may explain why there is a CEO effect in remuneration that is not associated with observed hospital performance but is associated with receiving a public honour. Furthermore, the political nature of the NHS may also lead to reluctance of high performers to seek CEO appointments, thus inducing negative sorting.

A second explanation is that hospitals are large complex organizations, in which highly trained (and hard to monitor) individuals run separate but interconnected production processes. Management at the very top of such organizations may find it difficult to engage in coordination and getting a large number of actors, who traditionally have not worked together, to work cooperatively. Put another way, a possible interpretation of our finding is that the organizational inertia of a large hospital is too strong for a single manager – even if this person is the CEO – to be able to impact performance within the short time period in which they are in office, and consistently across organizations. This situation, of course, is not specific to public sector hospitals. But it may have more of an effect in hospitals, public or private, where there are many dimensions of performance

[59] In a complementary non-parametric study reported in Appendix B we also find no evidence that a *change* in CEO brings about an improvement (or even just a change) in performance.

(clinical, access, financial) that can be pursued and can in the short run conflict. This inertia may also be exacerbated by the often much longer contract durations of clinical staff relative to CEOs.

Regardless of the underlying drivers of our results, they raise concerns about the plausibility of policy approaches that focus on the use of transient "turnaround" CEOs to improve the performance of individual hospitals. A leading NHS manager recently argued that it takes five years for a CEO to make a difference but the average time in post is much shorter than that.[60] Coupled with the findings of Tsai et al. (2015) and Bloom, Propper, Seiler & van Reenen (2015) that the management capabilities of middle managers in hospitals are systematically associated with better outcomes, our paper suggests that rather than seeking to rapidly change hospital performance through the appointment of a cadre of "superheads", strategies for improvement should instead focus on nurturing and sustaining the skills of middle managers.

[60] `https://www.hsj.co.uk/workforce/so-what-does-it-take-to-be-a-chief-executive-in-the-nhs/5091689.article`).

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Appendix A Definitions and Sources of Variables Used in Analysis

Tables A-1 and A-2 provide the data sources for all variables. The pay data are available only in bands of £5,000. We use the midpoint for each band as an approximation of the underlying continuous variable. For example, a basic salary reported as £120,000-£125,000 is recorded as £122,500 in our data set. The time-varying observable hospital level variables, $\boldsymbol{X}_{j(i,t)}$ are foundation trust status, year of merger, years since merger, beds, technology index and case mix variables.

Technology Index The technology index can take any value between 0 and 1. It is the weighted average of 7 dummy variables indicating the availability of advanced technologies: a neonatal intensive care unit, a cardiology unit, magnetic resonance imaging, imaging using radio-isotopes, heart or lung transplants, open heart surgery and percutaneous coronary interventions. The weight for each of these technologies is the proportion of hospitals that do not possess that technology at the beginning of our sample in 2000/01. The resulting index value increases over the sample period as hospitals add technologies.

We use data from a wide range of administrative sources to generate the 7 dummy variables. A hospital is defined as having a neonatal intensive care unit if it has at least one bed in a neonatal intensive care unit, as reported in the beds data published annually in the Hospital Activity Statistics by NHS England. A hospital is defined as having a cardiology unit if according to annual Hospital Episode Statistics it delivered at least 10 finished consultant episodes in a cardiology speciality. We define a hospital as offering magnetic resonance imaging if according to the annual imaging data published as part of Hospital Activity Statistics by NHS England it delivered at least 100 examinations or tests using magnetic resonance imaging. Numbers in this data set tend to be around 1,000 to 30,000; so numbers smaller than 100 might be data entry errors. Similarly, we define a hospital as offering imaging using radio-isotopes if the annual imaging data reports at least 100 examinations or tests using radio-isotopes.

Further, we define a hospital as providing heart or lung transplants if the annual Hospital Episode Statistics report at least 2 transplant procedures (HRGs E01 and E02), as providing open heart surgery if the annual Hospital Episode Statistics report at least 10 open heart surgery procedures (HRGs E01 to E04) and as providing percutaneous coronary interventions (OPCS codes K49 and K75) if the annual Hospital Episode Statistics report at least 10 such interventions.

Once a dummy variable takes the value one, we set its value to one in all following

Table A-1: Variable definitions and sources: Outcome variables

Variable	Definition	Source
Basic pay	Basic remuneration, CPI adjusted (£)	IDS Incomes Data Services and remuneration reports in hospitals' annual reports
Total pay	Total remuneration excluding redundancy payments, CPI adjusted (£)	
(Doctors + nurses)/beds	Ratio of all medical staff and nurses (full-time equivalent) to beds	NHS Hospital and Community Health Service in England workforce statistics, Health and Social Care Information Centre, now NHS Digital
Senior doctors/staff	Consultants, associate specialists, staff grade, registrars as proportion of all staff (%)	
Nurses/staff	Qualified nursing, midwifery, health visiting staff as prop. of all staff (%)	
Technology index	Details in text	Various sources
Beds	Average daily number of available beds	NHS England
Admissions	Number of admissions (count)	Hospital Episode Statistics: Admitted Patient Care, Health and Social Care Information Centre, now NHS Digital
Length of stay	Mean of spell duration, excluding day cases (days)	
Day cases	Proportion of finished consultant episodes relating to day cases (%)	
Waiting time	Mean time waited between decision to admit and actual admission (days)	
Cancelled operations	Operations cancelled for non-clinical reasons (count)	NHS England
AMI deaths	Deaths within 30 days of emergency admission for acute myocardial infarction, age 35-74 (%)	Clinical and Health Outcomes Knowledge Base (NCHOD), since relaunched as Compendium of Population Health Indicators
Stroke deaths	Deaths within 30 days of emergency admission for stroke, all ages (%)	
FPF deaths	Deaths within 30 days of emerg. adm. for fractured proximal femur, all ages (%)	
Readmissions	Emerg. readmissions to hospital within 28 days of discharge, age 16+ (%)	
MRSA rate	MRSA bacteraemia rate per 100,000 bed days	Public Health England
Staff job satisfaction	Scores from 1 to 5, 1 = dissatisfied, 5 = satisfied, mean	NHS Staff Survey
Surplus	Retained surplus/deficit (£000)	Trust Financial Returns

Table A-2: Variable definitions and sources: Control variables

Variable	Definition	Source
Foundation Trust	Dummy variable taking value 1 once a hospital has achieved Foundation Trust status, 0 otherwise	Monitor, now NHS Improvement
Year of merger	Dummy variable taking value 1 in year hospital newly created through merger enters sample, 0 otherwise	Information on hospitals' websites and Statutory Instruments (www.legislation.gov.uk)
Years since merger	Variable taking value 1 in year after hospital newly created through merger enters sample, value 2 in following year and so on, 0 otherwise	
Acquisition	Dummy variable taking value 1 once hospital has been involved in merger that is more like acquisition, i.e. following merger hospital keeps its provider code while provider code of other hospital disappears from any records, 0 otherwise	
Beds	Average daily number of available beds	NHS England
Technology index	Details in text	Various sources
Patients aged 0 to 14	Finished Consultant Episodes (FCEs) involving patients aged 0 to 14/Total FCEs	Hospital Episode Statistics: Admitted Patient Care, Health and Social Care Information Centre, now NHS Digital
Patients aged 60 to 74	FCEs involving patients aged 60 to 74/Total FCEs	
Patients aged 75+	FCEs involving patients aged 75+/Total FCEs	
Male patients	FCEs involving male patients/Total FCEs	
Major teaching hospital	Dummy variable taking value 1 if hospital serves medical school as their major NHS partner, 0 otherwise	The Guardian Healthcare Professionals Network, Wikipedia and information on medical schools' websites
Minor teaching hospital	Dummy variable taking value 1 if hospital is not major teaching hospital but member of the Association of UK University Hospitals	Association of UK University Hospitals, now University Hospital Association
Specialist status	Hospital is specialist acute, children's or orthopaedic hospital	NHS Staff Survey
Hospital commission rating	Details in text	Various sources
Competitive market	Details in text	Own calculations

years, to avoid fluctuations that are most likely caused by data entry errors rather than real changes.

Hospital Commission Rating We use ratings for the financial years 2002/03 to 2007/08. Ratings for the years 2002/02 to 2004/05 used stars, with three stars awarded to hospitals with the "highest levels of performance", two stars awarded to hospitals that are "performing well overall, but have not quite reached the same consistently high standards", one star awarded to hospitals "where there is some cause for concern regarding particular key targets" and zero stars awarded to hospitals "that have shown the poorest levels of performance against key targets" (Department of Health 2001). We classify zero stars and one star as a poor rating.

For the years 2005/06 to 2007/08 the Hospital Commission published ratings using a four-point scale of "excellent", "good", "fair" and "weak" (Healthcare Commission undated). Each hospital received two scores, one for quality of services and one for use of resources (Healthcare Commission undated). We use the score for quality of services and classify scores of "fair" and "weak" as a poor rating. Ideally, we want to use the hospital commission rating from the year before the CEO arrived. Because of data limitations we can use this definition only for the financial years 2003/04 to 2008/09. For the financial year 2002/03 we use the contemporanous rating, for 2009/10 the rating from two years before the CEO arrived and for 2010/11 the rating from three years before the CEO arrived.

Competitive Market The competitive market variable is a dummy variable that takes the value 1 if the hospital is located in a competitive market. To measure hospital competition we use a method similar to Bloom, Propper, Seiler & van Reenen (2015). First, we calculate the Euclidean distance (in km) for each pairwise combination of all hospitals in our data. For this calculation we use the geographical coordinates associated with the postcode of the hospital trust's headquarters. Next, we count the number of competitors within a 30 km radius. Finally, we calculate the quartiles of these counts for each region. If a hospital is in the top quartile for its region, we classify it as operating in a competitive market.

Appendix B Non-parametric Approach

The fixed effects approach relies heavily on our statistical model of hospital production, since both Equation 4 and Equation 7 use the residuals from this statistical model to estimate the impact of individual CEOs. Both equations also rely on CEOs having an impact on the same dimension of hospital production across two hospitals. We use a non-parametric approach to sidestep both problems. Our approach resembles a difference-in-difference estimator combined with matching. We compare the changes in hospital production following a CEO turnover event to changes in hospital production at matched hospitals without a CEO turnover event. If there is any impact of CEOs on hospital production, we expect to see different changes after a CEO turnover event compared to changes at otherwise similar hospitals with no CEO turnover event.

The starting point for this analysis are all CEO turnover events in our sample (i.e. not just the turnover events of the CEOs who have at least two observations for at least two of the outcome variables in set s at two different hospitals). We begin by identifying hospitals that had a CEO turnover event that resulted in subsequent stable leadership of at least two years. Next, we select from this set of observations those CEO turnovers events that were preceded by two years of stable leadership. This selection criterion excludes those NHS hospitals characterized by frequent CEO turnovers within a short time period – most likely hospitals in a crisis – for which it is hard to find a suitable control group. In summary, the treated observations are hospitals with a CEO turnover event in t and the new CEO staying on in $t + 1$ and no CEO turnover in $t - 1$ and $t - 2$.

We match each treated observation to one or three control hospitals with no CEO turnover event over the period $t - 2$ to $t + 1$. We then compare the difference in our hospital production measures between the year before the CEO turnover event and the end of the two-year period, i.e. between $t - 1$ and $t + 1$, to the equivalent difference in the matched hospitals. For our sets of input, throughput and clinical performance measures we implement a stacked version by simply comparing the differences in all (standardised) production measures at the treated hospital to the differences in all measures at the control hospital.

We match – with replacement – treated hospitals to control hospitals exactly on year, teaching status, specialist status and foundation trust status in $t - 1$.[61] This tends to result in more than one match for each treated hospital. Therefore, in the next step, we use closest neighbor matching on beds in $t - 1$ to choose one or three control hospitals from among the exactly matched hospitals. Where closest neighbor matching on beds results in ties, we choose from among the (usually two) hospitals with the same absolute difference in number of beds the closest neighbor in terms of the technology index in $t - 1$.[62]

[61]Matching on teaching status implies matching treated major teaching hospitals to control major teaching hospitals and treated minor teaching hospitals to control minor teaching hospitals. For specialist status we match only on the broad definition of specialist hospital. Teaching and specialist status are fixed characteristics, foundation trust status is time-varying.

[62]Matching exactly on year implies that we compare, for example, the difference in waiting times

For the sets of input, throughput and clinical performance measures we report the mean of the change, $\frac{1}{\sum n^{k_s}} \sum_{k_s=1}^{K_s} \sum_{j=1}^{n^{k_s}} \left(y_{j(t+1)}^{k_s} - y_{j(t-1)}^{k_s} \right)$, and its standard error for both the treated and the control hospitals. We then present the difference between the two means as well as the standard error and p-value from a two-sample t-test with equal variance. We report the same statistics for the staff job satisfaction and surplus measures, with the mean of the change being $\frac{1}{n} \sum_{j=1}^{n} \left(y_{j(t+1)} - y_{j(t-1)} \right)$.

The sample used for this analysis differs from that used for the parametric analyses as it (a) uses CEO turnover events (multiplied by the number of variables in each stacked set) rather than all hospital-year observations and (b) includes as potential treated observations all CEO turnover events rather than only the turnover events for the 95 CEOs for whom we estimated CEO fixed effects.

Estimates Table B-1 presents the results for the stacked input, throughput and clinical performance measures as well as staff job satisfaction and surplus.[63] 1:1 and 1:3 matching give similar results. These suggest there are no changes in the hospital production measures after a new CEO is employed except for staff satisfaction which grows at a slower pace following a CEO turnover.

To assess the balance of the matched samples, Table B-2 reports for the treated and the control observations the means of the hospital characteristics that we include as control variables in the wage equation (2) and in the regressions (4) and (7). Table B-2 also show the means of the characteristics we match exactly on (teaching status, specialist status and foundation trust status). For almost all the measures there is little difference between the treated and the control samples, with the exception of the number of beds, which is slightly larger for the treated sample. We conclude that the matching produces good balance on observables.[64]

Table W-14 in the Web Appendix presents a robustness test that applies our non-parametric approach to only the 95 CEOs used in our parametric estimates.[65] The re-

between 2006 and 2008 for a hospital with a CEO turnover event in 2007 to the difference in waiting times between 2006 and 2008 for a hospital with no CEO turnover event in 2007. Thus our results will not be confounded by period effects (Gaynor et al. 2012).

[63] We repeat these analyses for each of the variables in the sets separately. These results corroborate the findings presented here. See Tables W-10 and W-11 in the Web Appendix.

[64] Table W-12 in Web Appendix checks the common trend assumption by examining changes in hospital production in the two-year period preceding the CEO turnover event analysed in Table B-1. Changes observed in treated are similar to changes in control hospitals, supporting the parallel trend assumption. The only exception is for the input measures which increase more in treated hospitals (Table W-13 in Web Appendix). This apparent favourable trend in inputs prior to the CEO turnover event makes our findings of a fall in staff job satisfaction even more striking.

[65] We limit the potential treated observations to the 95 CEOs. The number of CEO turnover events in this analysis is less than 95 x 2 because of our selection criteria. See notes for Table W-14.

Table B-1: Changes in hospital production measures following a CEO turnover event compared to one or three matched control hospitals with no CEO turnover event

		Obs.	Mean change in variable (std. error)	Difference in mean changes (std. error)	p-value
Input measures	Treated	820	0.136 (0.017)		
	Controls	820	0.135 (0.016)	0.001 (0.023)	0.95
	Controls	2,384	0.138 (0.010)	-0.002 (0.019)	0.92
Throughput measures	Treated	1,011	0.151 (0.015)		
	Controls	1,011	0.123 (0.014)	0.028 (0.021)	0.18
	Controls	2,941	0.141 (0.009)	0.010 (0.018)	0.57
Clinical performance measures	Treated	845	0.128 (0.033)		
	Controls	845	0.103 (0.032)	0.025 (0.046)	0.58
	Controls	2,499	0.103 (0.018)	0.026 (0.036)	0.48
Staff job satisfaction	Treated	163	0.124 (0.076)		
	Controls	163	0.312 (0.071)	-0.189 (0.104)	0.07
	Controls	468	0.247 (0.042)	-0.123 (0.084)	0.14
Surplus	Treated	205	0.096 (0.072)		
	Controls	205	0.139 (0.055)	-0.044 (0.091)	0.63
	Controls	596	0.007 (0.048)	0.089 (0.092)	0.33

All outcome variables are standardised to have a mean of zero and a standard deviation of one. Some outcome variables in the stacked sets are multiplied by (-1), so that "more" means "better". Treated observations are hospital-years with a CEO turnover event in t, the new CEO still in post in $t+1$ and no CEO turnover event in $t-1$ and $t-2$. One or up to three controls are chosen from hospital-years with no CEO turnover event in t, $t+1$, $t-1$ and $t-2$. The change in outcome variable is $y_{j(t+1)} - y_{j(t-1)}$. Controls are matched exactly on year, major teaching hospital, minor teaching hospital, specialist hospital and foundation trust status, followed by closest neighbour matching on beds. In case of ties, closest neighbour matching on beds is followed by closest neighbour matching on technology index. Foundation trust status, beds and technology index as of $t-1$; teaching status and specialist status are permanent characteristics. Standard error and p-value for difference in means from two-sample t-tests with equal variance.

sults are similar to those here: there are no changes in the hospital production measures following a CEO turnover event with the exception of staff job satisfaction.[66] Table W-15 in the Web Appendix examines changes over the three years following the CEO turnover event rather than only two years.[67] Again, there is a negative impact on staff satisfaction, though due to the smaller sample size this effect is less precisely estimated. We conclude that, with the possible exception of a negative impact on staff satisfaction, a CEO turnover event does not appear to impact on hospital production.[68]

[66]For the throughput measures 1:1 matching suggests a statistically significant impact of a CEO turnover event. The estimated effect size, however, is very small, especially compared to the estimated effect size for staff job satisfaction. Job satisfaction is estimated to drop by one-quarter of a standard deviation. Throughput measures are estimated to increase by one-twentieth of a standard deviation.

[67]The number of CEO turnover events included in this analysis is smaller than in Table B-1 as we require the new CEO to be still in post in $t+2$ to calculate changes between the year before the CEO turnover event, $t-1$, and three years later, $t+2$.

[68]We also examine the impact of a CEO turnover event on the quality of middle management, using data from the 2006 and 2009 waves of the World Management Survey. There are only 9 treated observations, so the effect estimate is imprecise but, if anything, it suggests that a turnover event decreases management quality. See Web Appendix.

Table B-2: Means of matching variables and other hospital characteristics for treated and control groups and means of exactly matched hospital characteristics

				Means of variables measured in $t-1$						Means of vars. measured in t		Exactly matched characteristics			
			Unique		Tech-	Prop. in each category (%)					Years	Teaching		Spec.	Foundation trust
		Obs.	controls	Beds	nology	0-14	60-74	75+	male	Year of merger	since merger	Major	Minor	hosp.	in $t-1$
Inputs	Treated	820		721	0.35	5.31	8.14	8.29	43.5	0	1.22	12.2	9.3	9.8	26.8
	Controls	820	161	714	0.39	5.06	8.28	8.23	44.3	0.98	0.82				
	Controls	2,384	348	718	0.38	4.90	8.12	8.35	43.8	0.50	0.98				
Throughputs	Treated	1,011		724	0.35	5.29	8.14	8.32	43.5	0	1.22	12.4	9.3	9.3	26.8
	Controls	1,011	163	717	0.39	5.07	8.28	8.26	44.3	0.99	0.84				
	Controls	2,941	354	721	0.38	4.90	8.12	8.41	43.8	0.51	0.99				
Clinical perform.	Treated	845		770	0.36	5.05	8.21	8.60	43.4	0	1.12	13.8	7.2	4.0	23.0
	Controls	845	179	756	0.39	5.03	8.10	8.33	43.8	0.24	0.96				
	Controls	2,499	367	756	0.38	4.87	8.01	8.39	43.5	0.12	1.07				
Staff satisfaction	Treated	163		713	0.36	5.18	8.18	8.46	43.5	0	1.48	11.0	9.8	9.2	33.7
	Controls	163	126	711	0.40	5.06	8.36	8.42	44.5	1.23	0.99				
	Controls	468	272	727	0.40	4.85	8.23	8.60	43.9	0.64	1.23				
Surplus	Treated	205		722	0.35	5.31	8.14	8.29	43.5	0	1.22	12.2	9.3	9.8	26.8
	Controls	205	160	714	0.39	5.05	8.29	8.24	44.3	0.98	0.82				
	Controls	596	347	718	0.38	4.91	8.12	8.35	43.8	0.50	0.98				

Web Appendix: For Online Publication

W-1 Differences between SIC 92 and SIC 2007 industry classifications

Figure 2 in Section 2.3 presents pay of top managers in industries with both public and private sector organizations over the period 2000 to 2017. As the industry classification was changed substantially in 2009, we present separate graphs for 2000-2008 and 2009-2017. Here we describe the relevant changes in more detail.

Data for 2000 to 2008 use the "UK Standard Industrial Classification of Economic Activites - SIC 92" while data for 2009 to 2017 use the the "UK Standard Industrial Classification of Economic Activities - SIC 2007". In SIC 92 the section "K - Real estate, renting and business activities" includes "73.10 Research and experimental development on natural sciences and engineering" and "73.20 Research and experimental development on social sciences and humanities". In SIC 2007 these activities have been subsumed into the new section "M - Professional, scientific and technical activities". The SIC 92 section "I - Transport, storage and communication" includes "60.1 Transport via railways", "60.21 Other scheduled passenger land transport" and "64.11 National post activities". These industries are comparable to the SIC 2007 section "H - Transportation and storage", which includes "49.10 Passenger rail transport interurban", "49.31 Urban and suburban passenger land transport" and "53.10 Postal activities under universal service obligation".

The SIC 92 section "J - Financial intermediation" includes "65.11 Central banking" and "66.02 Pension funding". Similarly, the SIC 2007 section "K - Financial and Insurance Activities" includes "64.11 Central banking" and "65.30 Pension funding". The SIC 92 section "L - Public administration and defence, compulsory social security" includes "75.24 Public security, land and order activities", which is comparable to the SIC 2007 section "O - Public administration and defence, compulsory social security", which includes "84.23 Justice and judicial activities" and "84.24 Public order and safety activities". The SIC 92 section "O - Other community, social and personal service activities" includes "92 Recreational, cultural and sporting activities". In SIC 2007 these activities have been subsumed into the new section section "R - Arts, entertainment and recreation".

W-2 Predictors of the number of CEOs per hospital over the sample period

Table W-1: Association between number of CEOs observed per hospital and time-invariant hospital characteristics

	Coefficient	Obs. in category
North West	1.29** (0.59)	29
North East (omitted category)		
Yorkshire and Humber	1.43** (0.64)	15
West Midlands	1.77*** (0.62)	19
East Midlands	3.20*** (0.75)	7
East of England	1.86*** (0.62)	18
London	1.53*** (0.59)	27
South West	1.59** (0.62)	18
South East	0.95 (0.61)	21
Major teaching hospital	-0.16 (0.36)	19
Minor teaching hospital	-1.11*** (0.35)	23
Specialist acute	-0.47 (0.46)	12
Specialist orthopaedic	-0.67 (0.76)	4
Constant	2.41*** (0.52)	
R^2/Observations	0.20	162

A major teaching hospital serves a medical school as their main NHS partner, a minor teaching hospital is only a member of the Association of UK University Hospitals. Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%

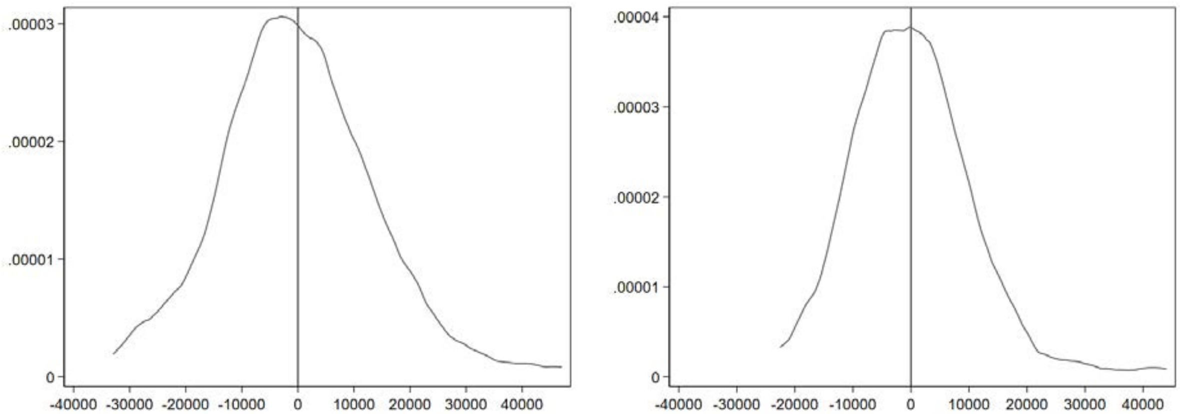
W-3 Predictors of mover CEOs

Table W-2: Linear probability model of CEO being one of the 95 CEOs included in fixed effects regressions

	Coefficient	Obs. in category
Female	0.003 (0.043)	147
Clinical background	0.019 (0.047)	112
Postgraduate management qualification	0.070 (0.043)	121
Constant	0.18*** (0.025)	
R^2/Observations	0.01	468

Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%

Figure W-1: Kernel density plots of hospital effects in total pay



(a) Estimates

(b) Shrunk estimates

Notes: Kernel density plots of deviations of estimated hospital effects in total pay, CPI adjusted, from mean. Estimates shrunk towards regional means using empirical Bayes shrinkage estimator.

W-4 Properties of the hospital effects in pay

Figure W-1a shows the distribution of the estimated hospital effects in pay, $\widehat{\psi_{j(i,t)}}$. Since the $\psi_{j(i,t)}$ are estimated relative to an arbitrary omitted hospital, we transform the estimates into deviations from the mean of all $\psi_{j(i,t)}$. At 25% of hospitals the executive directors are paid an extra £7,690 or more in total pay, holding our basic set of time-varying hospital characteristics and all time-invariant executive director characteristics constant. Similarly, at 25% of hospitals the executive directors receive pay packages that are £8,500 or more below the average pay package.

Figure W-1b shows the distribution of the hospital effects in pay after adjusting the estimates for sampling error using the empirical Bayes procedure, with the estimates shrunk towards the regional means.[69] The distribution is only slightly narrower, with the interquartile range dropping from £16,000 to £13,000 after shrinkage.

Table W-3 explores the determinants of the hospital effects in pay using linear regressions of the estimated hospital effects on a set of dummy variables indicating time-invariant hospital characteristics. Results in the first major column are for the original effect estimates while results in the second major column are for the hospital effect estimates shrunk towards the regional means. In general, the results are similar but unsurprisingly the coefficients for the shrunk hospital effect estimates are estimated more precisely.

The first specification in Table W-3 explores the impact of the region where the hospital

[69]We employ the empirical Bayes shrinkage estimator described in Chandra et al. (2016).

Table W-3: Association between estimated hospital effects in pay and time-invariant hospital characteristics

	Original effect estimates		Shrunk effect estimates		Obs. in each category
North West (omitted category)					34
North East	13,317***	10,719**	14,387***	12,534***	9
	(4,872)	(4,324)	(3,544)	(3,105)	
Yorkshire and Humber	8,506**	5,903*	7,944***	6,055**	20
	(3,663)	(3,268)	(2,664)	(2,346)	
West Midlands	3,831	4,357	3,542	3,893*	22
	(3,556)	(3,205)	(2,587)	(2,301)	
East Midlands	6,870	972	6,538*	2,027	10
	(4,676)	(4,222)	(3,401)	(3,031)	
East of England	4,446	3,714	4,431	3,909	18
	(3,789)	(3,357)	(2,756)	(2,410)	
London	14,584***	12,266***	14,739***	12,970***	38
	(3,068)	(2,737)	(2,232)	(1,965)	
South West	2,412	1,708	2,799	2,349	19
	(3,723)	(3,291)	(2,708)	(2,363)	
South East	8,278**	8,449***	8,177***	8,310***	26
	(3,386)	(3,033)	(2,463)	(2,178)	
Major teaching hospital		14,752***		11,640***	26
		(2,512)		(1,804)	
Minor teaching hospital		7,868***		5,761***	25
		(2,603)		(1,869)	
Specialist acute		-9,968***		-6,664***	12
		(3,563)		(2,558)	
Specialist orthopaedic		-18,174***		-12,939***	4
		(5,922)		(4,252)	
Constant	-6,828***	-7,618***	-6,603***	-7,325***	
	(2,229)	(2,069)	(1,623)	(1,486)	
R^2	0.14	0.35	0.24	0.44	

The hospital effects are extracted from the regressions reported in Table 4 and transformed into deviations from the mean of all estimated hospital effects. A major teaching hospital serves a medical school as their main NHS partner, a minor teaching hospital is only a member of the Association of UK University Hospitals. 196 observations in each regression. Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%

is based on executive director pay. We expect the hospital effects to reflect differences in the cost of living across the different regions in England. The omitted region being the North West, the constant of −£ 6,828 for the original effect estimates is the North West average of the deviations from the mean of all estimated hospital effects for total pay. As the coefficient estimates for all other regions are positive, the North West is the region with the lowest hospital effects. The regions with the largest hospital effects are London and the South East, which reflects the higher cost of living in these regions.

The ranking of the coefficients for the remaining regions does not reflect the ranking of the cost of living. The North East dummy, the Yorkshire and Humber dummy and the East Midlands dummy have the next largest coefficients, while the coefficient on the South West dummy is small and not statistically significantly different from zero. Once we add dummy variables indicating whether a hospital is a major teaching hospital, a minor teaching hospital, a specialist acute hospital or a specialist orthopaedic hospital the coefficients on the North East dummy, the Yorkshire and Humber dummy and the East Midlands dummy drop by £2,000 to £4,000, suggesting some of these unexpectedly large hospital effects are driven by the teaching status and specialist status of hospitals in these regions. However, average hospital effects for hospitals in the North East, Yorkshire and Humber and the West Midlands are still larger than for hospitals in the South West and the East of England, which tend to have higher cost of living.

Potentially, factors other than the cost of living drive these regional differences in the estimated hospital effects. For example, hospitals in the North East might have more difficulties in attracting and retaining good managers than hospitals in the South West and therefore have to offer a pay premium.

Hospital effects at teaching hospitals are statistically significantly larger than at non-teaching hospitals while hospital effects at specialist hospitals are statistically significantly smaller than at general hospitals. Combining the two largest coefficient estimates, the hospital effect of a major teaching hospital in London is on average −£ 7,618 + £ 12,266 + £ 14,752 = £ 19,400 above the sample average of the hospital effects in total pay, well above the 75^{th} percentile of the distribution of deviations from the mean of all hospital effects displayed in Figure W-1.

Overall, the region dummies, the teaching status dummies and the specialist status dummies jointly explain around 40% of the variation in the hospital effects.

W-5 Predictors of CEO pay effects - all CEO characteristics included simultaneously

Table W-4: Joint estimates of the association between estimated director effects in total pay and personal characteristics for subset of directors observed in a CEO position at least once

	Original effect estimates	Shrunk effect estimates	Obs. in category
Female	-5,837** (2,072)	-2,864*** (1,057)	125
Female × in 95 CEOs subset	-357 (4,375)	-598 (2,231)	31
Clinical background	5,445** (2,240)	2,148* (1,142)	96
Clinical backgr. × in 95 CEOs subset	-4,013 (4,641)	-991 (2,367)	25
Postgraduate managm. qualification	-3,747* (2,023)	-1,513 (1,032)	107
PG managm. qual. × in 95 CEOs subset	3,639 (3,947)	1,316 (2,013)	31
Public honour	4,735* (2,847)	2,974** (1,452)	51
Public honour × in 95 CEOs subset	2,285 (5,271)	1,961 (2,689)	16
Private sector	-2,869 (3,098)	-1,041 (1,580)	42
Private sector × in 95 CEOs subset	8,167 (5,426)	4,109 (2,767)	14
Observed as CEO for 2 to 9 years	-1,938 (2,936)	-1,129 (1,498)	290
Observed as CEO for 10 plus years	5,146 (4,023)	2,849 (2,052)	76
10 plus years × in 95 CEOs subset	-2,054 (4,713)	-975 (2,404)	43
Observed in 2 CEO jobs	1,170 (2,915)	595 (1,487)	102
Observed in 3+ CEO jobs	5,274 (4,306)	2,823 (2,196)	38
3+ CEO jobs × in 95 CEOs subset	1,876 (6,409)	1,455 (3,269)	24
In 95 CEOs subset	-2,352 (4,175)	-1,089 (2,129)	95
Constant	4,628 (2,824)	2,405 (1,440)	
R^2 / Observations	0.12	0.14	397

The executive director effects are extracted from the regressions reported in Table 4 and transformed into deviations from the mean of all estimated executive director effects. Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%

W-6 Parametric estimates of CEO effects in hospital production using hospital production measures separately in their original units

Tables W-5, W-6 and W-7 present estimates of Equation 3 using the actual CEO-hospital matches as well as the random CEO-hospital matches for the separate input, throughput and clinical performance measures. As for the stacked estimations reported in Section 5.2, when using the actual CEO-hospital matches the F-tests suggest that the estimated CEO effects are jointly statistically significantly different from zero and the proportion of individually statistically significant CEO effects ranges from 25.0% for MRSA rates to 36.1% for stroke deaths. And as for the stacked estimations reported in Section 5.2, when using the random CEO-hospital matches the F-statistics across the 100 replications are as large as they are for the actual CEO-hospital matches and the proportion of CEO effects that are individually statistically significant is similar to the proportion for the actual CEO-hospital matches.

For some of the measures the variance proportions explained by the covariates, hospital effects and CEO effects are invalid due to one of the proportions being negative. For the measures with valid variance proportions, the mean variance proportion explained by the CEO effects using the random CEO-hospital matches tends to be close to the variance proportion explained by the CEO effects using the actual CEO-hospital matches.

Table W-8 presents estimates of Equation 6 for the separate input, throughput and performance measures. For most variables, we do not find evidence of any impact of individual CEOs on hospital performance. The results for MRSA rates hint at larger than expected MRSA rates in the first hospital being associated with larger than expected MRSA rates in the second hospital, with the coefficient estimate $\hat{\delta_2}$ taking the value 0.10. This estimate, however, is not statistically significantly different from zero with a p-value of 0.33. Also, the explanatory power of the average deviations from the expected MRSA rates in the first hospital is very low with an R^2 of 0.01. For AMI deaths the coefficient estimate $\hat{\delta_2}$ takes the value -0.17, suggesting larger than expected AMI death rates in the first hospital are associated with smaller than expected AMI death rates in the second hospital and vice versa. This coefficient is more precisely estimated with a p-value of 0.12 and the explanatory power of the average deviations from the expected AMI death rates in the first hospital is slightly larger with an R^2 of 0.04. However, the negative coefficient suggests that there is no impact of individual CEOs on AMI death rates.

Table W-5: Estimates of CEO effects for input measures using actual and random CEO-hospital matches

	F-test of joint significance of CEO effects (p-value/rejection frequency using 1% signif. level, df1, df2)	Number (prop.) of CEO effects statist. signif. at 5%	R^2	Total hospital-year obs.	Variance proportions (%) for subsample of obs. with at least one non-zero CEO effect: Co-variates	Hospital effects	CEO effects	Re-siduals	Subsample hospital-year obs.
Doctors + nurses/beds									
Actual matches	32.8 (<0.001, 94, 223)	31 (33.0%)	0.90	2,382	38.2	49.6	4.9	7.4	819
Random matches:									
Means	54.8 (100%, 92.0, 223)	27.2 (29.6%)	0.90	2,382	36.4	51.4	4.9	7.3	826.8
(Std. dev.)	(31.8) (n.a., 1.23, 0)	(4.87, 5.22)	(0.002)		(3.05)	(3.92)	(2.27)	(1.07)	(12.4)
Senior doctors/staff									
Actual matches	54.4 (<0.001, 95, 224)	33 (34.7%)	0.89	2,396	44.9	35.3	3.7	16.1	830
Random matches:									
Means	75.2 (100%, 93.7, 224)	29.9 (31.9%)	0.89	2,396	45.6	38.5	3.2	12.7	842.3
(Std. dev.)	(49.6) (n.a., 1.09, 0)	(5.01, 5.32)	(0.002)		(3.21)	(3.82)	(1.75)	(3.14)	(12.2)
Nurses/staff									
Actual matches	67.9 (<0.001, 95, 224)	27 (28.4%)	0.86	2,396	4.8	75.0	9.1	11.2	830
Random matches:									
Means	70.58 (100%, 93.7, 224)	29.1 (31.1%)	0.86	2,396	4.5	79.8	5.4	10.3	842.3
(Std. dev.)	(54.1) (n.a., 1.09, 0)	(4.92, 5.23)	(0.002)		(2.03)	(2.95)	(2.15)	(1.32)	(12.2)
Technology									
Actual matches	256.29 (<0.001, 95, 224)	33 (33.7%)	0.95	2,398	*4.1*	*91.1*	*-0.04*	*4.8*	830
Random matches									
Means	96.4 (100%, 93.8, 224)	33.1 (35.3%)	0.0.95	2,398	4.4	86.9	3.8	4.8	843.6
(Std. dev.)	(71.0) (n.a., 1.09, 0)	(4.97, 5.23)	(0.001)		(1.59)	(1.86)	(1.62)	(0.78)	(12.1)

df = degrees of freedom. df1 is the number of CEO effects, df2 is the number of hospital clusters. Standard errors used for the statistical significance tests are clustered at hospital level. Variance proportion is the proportion of variance in the pay variable that is explained by the covariates, the hospital effects, the director effects and the residuals, respectively. Covariates are financial year effects, foundation trust status, year of merger, years since merger, beds (except for (doctors + nurse)/beds), technology index (except for technology index) and case mix variables. The results for random CEO-hospital matches are means and standard deviations across 100 replications.

Table W-6: Estimates of CEO effects for throughput measures using actual and random CEO-hospital matches

	F-test of joint significance of CEO effects (p-value/rejection frequency using 1% signif. level, df1, df2)	Number (prop.) of CEO effects statist. signif. at 5%	R^2	Total hospital-year obs.	Variance proportions (%) for subsample of obs. with at least one non-zero CEO effect: Co-variates	Hospital effects	CEO effects	Re-siduals	Subsample hospital-year obs.
Admissions									
Actual matches	32.4 (<0.001, 95, 224)	30 (31.6%)	0.98	2,392	*25.1*	*73.1*	*-0.39*	*2.2*	826
Random matches:									
Means	70.2 (100%, 93.6, 224)	28.7 (30.6%)	0.98	2,392	20.4	74.6	2.7	2.3	839.3
(Std. dev.)	(43.1) (n.a., 1.16, 0)	(4.69, 5.00)	(0.0005)		(2.02)	(2.32)	(1.23)	(0.54)	(12.5)
Length of stay									
Actual matches	45.5 (<0.001, 94, 224)	31 (33.0%)	0.95	2,386	48.5	38.5	0.5	12.5	815
Random matches:									
Means	58.5 (100%, 92.9, 224)	26.0 (28.0%)	0.95	2,386	47.8	40.3	1.6	10.3	831.8
(Std. dev.)	(40.1) (n.a., 1.32, 0)	(4.90, 5.23)	(0.001)		(9.16)	(8.26)	(2.72)	(2.36)	(12.8)
Day cases									
Actual matches	100.3 (<0.001, 95, 223)	33 (34.7%)	0.86	2,383	27.8	47.9	13.3	10.9	824
Random matches:									
Means	64.2 (100%, 93.4, 223)	28.8 (30.9%)	0.85	2,383	26.1	55.4	7.8	10.8	837.1
(Std. dev.)	(36.9) (n.a., 1.21, 0)	(4.42, 4.68)	(0.003)		(5.94)	(6.80)	(3.18)	(1.73)	(12.7)
Waiting times									
Actual matches	61.2 (<0.001, 93, 223)	29 (31.2%)	0.84	2,356	52.1	25.1	7.8	15	804
Random matches									
Means	79.2 (100%, 91.7, 223)	29.9 (32.6%)	0.84	2,356	53.7	28.0	6.1	12.3	815.8
(Std. dev.)	(62.5) (n.a., 1.64, 0)	(4.29, 4.67)	(0.003)		(2.71)	(3.21)	(2.40)	(1.54)	(15.5)
Cancelled operations									
Actual matches	77.0 (<0.001, 90, 199)	25 (27.8%)	0.73	2,332	*-4.8*	*78.1*	*2.6*	*24.1*	786
Random matches:									
Means	66.4 (100%, 90.4, 199)	25.9 (28.6%)	0.73	2,332	0.11	68.6	9.2	22.0	813.0
(Std. dev.)	(69.5) (n.a., 1.53, 0)	(4.80, 5.29)	(0.005)		(4.30)	(6.09)	(3.90)	(2.01)	(15.7)

See notes for table W-5

Table W-7: Estimates of CEO effects for clinical performance measures using actual and random CEO-hospital matches

	F-test of joint significance of CEO effects (p-value/rejection frequency using 1% signif. level, df1, df2)	Number (prop.) of CEO effects statist. signif. at 5%	R^2	Total hospital-year obs.	Variance proportions (%) for subsample of obs. with at least one non-zero CEO effect: Co-variates	Hospital effects	CEO effects	Re-siduals	Subsample hospital-year obs.
AMI deaths									
Actual matches	23.6 (<0.001, 61, 200)	18 (29.5%)	0.48	1,757	21.5	27.6	5.5	45.4	490
Random matches:									
Means	28.4 (100%, 53.4, 200)	16.9 (31.7%)	0.48	1,757	17.9	18.1	12.5	51.5	430.8
(Std. dev.)	(23.0) (n.a., 3.25, 0)	(3.65, 6.72)	(0.005)		(2.90)	(4.24)	(3.73)	(4.40)	(25.9)
Stroke deaths									
Actual matches	25.1 (<0.001, 72, 200)	26 (36.1%)	0.68	1,965	40.1	24.0	9.1	26.8	596
Random matches:									
Means	38.7 (100%, 64.8, 200)	21.1 (32.6%)	0.68	1,965	40.7	20.2	7.3	31.8	552.2
(Std. dev.)	(26.0) (n.a., 2.30, 0)	(3.98, 6.03)	(0.003)		(2.62)	(2.70)	(2.49)	(2.47)	(20.9)
FPF deaths									
Actual matches	23.9 (<0.001, 72, 195)	20 (27.8%)	0.48	1,920	20.9	16.5	10.9	51.7	588
Random matches:									
Means	32.2 (100%, 64.3, 195)	20.4 (31.7%)	0.49	1,920	21.3	17.8	11.2	49.7	544.3
(Std. dev.)	(20.8) (n.a., 2.33, 0)	(3.53, 5.56)	(0.005)		(1.60)	(2.49)	(3.20)	(2.96)	(21.2)
Readmissions									
Actual matches	30.2 (<0.001, 78, 222)	25 (32.0%)	0.78	2,070	39.6	27.0	12.8	20.5	636
Random matches:									
Means	38.2 (100%, 71.0, 222)	23.2 (32.6%)	0.78	2,070	26.9	39.4	9.7	24.0	583.3
(Std. dev.)	(31.2) (n.a., 1.44, 0)	(4.26, 5.93)	(0.004)		(7.04)	(5.91)	(3.14)	(3.37)	(13.1)
MRSA rate									
Actual matches	34.5 (<0.001, 80, 165)	20 (25.0%)	0.77	2,055	54.8	19.6	6.3	19.3	684
Random matches:									
Means	61.8 (100%, 85.5, 165)	26.9 (31.5%)	0.77	2,055	53.3	22.5	5.9	18.3	748.3
(Std. dev.)	(54.1) (n.a., 1.64, 0)	(4.10, 4.68)	(0.004)		(2.46)	(2.46)	(2.30)	(1.58)	(16.2)

See notes for table W-5

Table W-8: Association of (1) means of residuals for CEO spells in first and second hospital and (2) pre-assignment trend and mean of residuals for CEO spell in second hospital for hospital production mreasures separately in their original units

	1			2		
	CEO spells at 1st and 2nd hospital			Pre-assignment trend and CEO spell at 2nd hospital		
	Coefficient (std. error)	R^2	Obs.	Coefficient (std. error)	R^2	Obs.
Doctors + nurses/beds	-0.01	0	94	-0.05	0	91
	(0.15)			(0.09)		
Senior doctors/staff	0.03	0	95	-0.08	0.01	92
	(0.12)			(0.11)		
Nurses/staff	0.08	0.01	95	0.10	0.01	92
	(0.10)			(0.11)		
Technology	0.001	0	95	-0.05	0	92
	(0.10)			(0.10)		
Admissions	0.11	0.01	95	-0.005	0	92
	(0.12)			(0.11)		
Length of stay	0.05	0.01	94	-0.04	0	91
	(0.06)			(0.09)		
Day cases	0.18*	0.04	95	0.19**	0.04	92
	(0.09)			(0.10)		
Waiting times	-0.01	0	93	0.01	0	90
	(0.08)			(0.08)		
Cancelled operations	-0.12	0.01	90	0.32	0.03	87
	(0.17)			(0.21)		
AMI deaths	-0.17	0.04	61	-0.01	0	58
	(0.11)			(0.08)		
Stroke deaths	0.001	0	72	0.02	0	69
	(0.10)			(0.12)		
FPF deaths	-0.08	0.01	72	0.01	0	69
	(0.11)			(0.12)		
Readmissions	0.07	0.01	78	0.03	0	75
	(0.10)			(0.10)		
MRSA rate	0.10	0.01	80	-0.05	0	78
	(0.10)			(0.12)		

Results in Column 1 are from regressions of mean of residuals for CEO spell in second hospital on mean of residuals for CEO spell in first hospital. Results in Column 2 are from regressions of mean of residuals for CEO spell in second hospital on mean of residuals in second hospital during the three years before CEO was appointed. The residuals are from a regression of the hospital production measures on hospital characteristics, financial year effects and hospital effects. *Significant at 10%, **significant at 5%, ***significant at 1%

W-7 Validity of two-step procedure

We assess the validity of the two-step procedure by estimating Equations 7 and 8 for both actual CEO-hospital matches and random CEO-hospital matches. Table W-9 presents the results for the stacked and the two individual hospital production measures.

Looking at the results for the regressions using random CEO-hospital matches in the second and third row of each panel, we see that the coefficient estimates $\hat{\delta}_2$ are very small, with the mean coefficient estimates across the 100 replications ranging from -0.01 to 0.003. The next column shows the proportion of t-tests across our 100 replications that reject the hypothesis that $\hat{\delta}_2$ is equal to zero when using a significance level of 10%. The rejection frequency is around 10% and therefore close to the nominal level of the test. Overall, the results for the random CEO-hospital matches show no impact of CEOs, exactly what we would expect for random matches, suggesting the two-step procedure is valid.

Table W-9: Association of (1) means of residuals for CEO spells in first and second hospital and (2) pre-assignment trend and mean of residuals for CEO spell in second hospital using actual CEO-hospital matches as well as random CEO-hospital matches

	1			2		
	CEO spells at 1st and 2nd hospital			Pre-assignment trend and CEO spell at 2nd hospital		
	Coefficient (std. error)	p-value/rejection freq. using 10% signif. level	Obs.	Coefficient (std. error)	p-value/rejection freq. using 10% signif. level	Obs.
Input measures						
Actual matches	0.055 (0.11)	0.62	95	0.13 (0.11)	0.23	92
Random matches:						
Means	0.001 (0.11)	6%	93.6	-0.002 (0.12)	9%	92.7
(Std. dev.)	(0.10, 0.01)		(1.16)	(0.12, 0.01)		(1.56)
Throughput measures						
Actual matches	0.20* (0.11)	0.075	95	0.10 (0.12)	0.42	92
Random matches:						
Means	-0.01 (0.10)	2%	93.6	0.02 (0.12)	13%	92.7
(Std. dev.)	(0.09, 0.01)		(1.16)	(0.12, 0.01)		(1.57)
Clinical performance						
Actual matches	-0.04 (0.10)	0.69	82	-0.10 (0.09)	0.28	79
Random matches:						
Means	-0.01 (0.12)	17%	79.4	-0.03 (0.14)	9%	78.5
(Std. dev.)	(0.12, 0.02)		(1.73)	(0.13, 0.03)		(2.06)
Staff job satisfaction						
Actual matches	-0.07 (0.11)	0.56	73	-0.11 (0.17)	0.50	73
Random matches:						
Means	-0.003 (0.14)	6%	72.9	-0.03 (0.14)	7%	72.7
(Std. dev.)	(0.13, 0.02)		(1.12)	(0.13, 0.02)		(1.20)
Surplus						
Actual matches	-0.05 (0.30)	0.87	95	0.16 (0.22)	0.47	92
Random matches:						
Means	0.003 (0.14)	10%	93.8	0.003 (0.13)	12%	92.9
(Std. dev.)	(0.14, 0.04)		(1.09)	(0.14, 0.03)		(1.48)

Results in Column 1 are from regressions of mean of residuals for CEO spell in second hospital on mean of residuals for CEO spell in first hospital. Results in Column 2 are from regressions of mean of residuals for CEO spell in second hospital on mean of residuals in second hospital during the three years before CEO was appointed. The residuals are from a regression of the stacked measures or the individual measures on hospital characteristics, financial year effects and hospital effects. For the stacked measures, the residuals for the 4 or 5 outcome measures in each group are then averaged by hospital-year before the mean of residuals for each of a CEO's spells is being calculated. The results for random CEO-hospital matches are means and standard deviations across 100 replications. *Significant at 10%, **significant at 5%, ***significant at 1%

W-8 Non-parametric estimates of CEO effects in hospital production using hospital production measures separately in their original units

Tables W-10 and W-11 present results for the non-parametric approach using the separate input, throughput and clinical performance measures. We see that, in the main, inputs do not change after a new CEO is post. There is one exception, a fall in length of stay. We also see that on balance clinical performance does not improve, with improvements on some measures matched by reductions in other measures.

Table W-10: Changes in input and throughput measures following a CEO turnover event compared to one or three matched control hospitals with no CEO turnover event

		Obs.	Mean change in variable (std. error)	Difference in mean changes (std. error)	p-value
Doctors + nurses/beds	Treated	205	0.20 (0.02)		
	Controls	205	0.20 (0.02)	-0.00 (0.03)	0.82
	Controls	596	0.21 (0.01)	-0.01 (0.02)	0.68
Senior doctors/staff	Treated	205	0.67 (0.13)		
	Controls	205	0.62 (0.10)	0.05 (0.17)	0.76
	Controls	596	0.75 (0.07)	-0.07 (0.14)	0.59
Nurses/staff	Treated	205	-0.25 (0.12)		
	Controls	205	-0.12 (0.13)	-0.13 (0.17)	0.46
	Controls	596	-0.24 (0.07)	-0.01 (0.14)	0.95
Technology	Treated	205	0.024 (0.005)		
	Controls	205	0.018 (0.004)	0.007 (0.006)	0.27
	Controls	596	0.016 (0.002)	0.008 (0.005)	0.08
Admissions	Treated	205	4,216 (404)		
	Controls	205	4,955 (542)	-739 (676)	0.28
	Controls	596	5,098 (367)	-882 (668)	0.19
Length of stay	Treated	205	-0.48 (0.07)		
	Controls	205	-0.35 (0.04)	-0.13 (0.08)	0.10
	Controls	596	-0.32 (0.03)	-0.16 (0.06)	0.01
Day cases	Treated	202	0.94 (0.26)		
	Controls	202	0.73 (0.31)	0.21 (0.40)	0.60
	Controls	586	1.16 (0.18)	-0.22 (0.35)	0.53
Waiting times	Treated	200	-9.83 (1.29)		
	Controls	200	-8.72 (1.10)	-1.11 (1.69)	0.51
	Controls	583	-8.98 (0.66)	-0.85 (1.36)	0.53
Cancelled operations	Treated	202	-15.8 (14.5)		
	Controls	202	-3.15 (11.3)	-12.6 (18.4)	0.49
	Controls	589	-13.0 (8.28)	-2.74 (16.5)	0.87

Treated observations are hospital-years with a CEO turnover event in t, the new CEO still in post in $t+1$ and no CEO turnover event in $t-1$ and $t-2$. One or up to three controls are chosen from hospital-years with no CEO turnover event in t, $t+1$, $t-1$ and $t-2$. The change in outcome variable is $y_{j(t+1)} - y_{j(t-1)}$. Controls are matched exactly on year, major teaching hospital, minor teaching hospital, specialist hospital and foundation trust status, followed by closest neighbour matching on beds. In case of ties, closest neighbour matching on beds is followed by closest neighbour matching on technology index. Foundation trust status, beds and technology index as of $t-1$; teaching status and specialist status are permanent characteristics. Standard error and p-value for difference in means from two-sample t-tests with equal variance.

Table W-11: Changes in clinical performance measures following a CEO turnover event compared to one or three matched control hospitals with no CEO turnover event

		Obs.	Mean change in variable (std. error)	Difference in mean changes (std. error)	p-value
AMI deaths	Treated	143	-0.64 (0.30)		
	Controls	143	-0.54 (0.27)	-0.10 (0.41)	0.80
	Controls	424	-0.50 (0.15)	-0.14 (0.31)	0.65
Stroke deaths	Treated	168	-2.21 (0.30)		
	Controls	168	-1.07 (0.34)	-1.15 (0.45)	0.01
	Controls	505	-1.33 (0.19)	-0.88 (0.37)	0.02
FPF deaths	Treated	165	-0.16 (0.23)		
	Controls	165	-0.38 (0.24)	0.22 (0.33)	0.51
	Controls	495	-0.31 (0.12)	0.14 (0.25)	0.57
Readmissions	Treated	172	0.54 (0.09)		
	Controls	172	0.50 (0.08)	0.03 (0.12)	0.78
	Controls	503	0.54 (0.04)	0.001 (0.09)	0.99
MRSA rate	Treated	197	-2.19 (0.40)		
	Controls	197	-2.30 (0.42)	0.11 (0.58)	0.85
	Controls	572	-2.34 (0.24)	0.15 (0.48)	0.75

Treated observations are hospital-years with a CEO turnover event in t, the new CEO still in post in $t+1$ and no CEO turnover event in $t-1$ and $t-2$. Up to three controls are chosen from hospital-years with no CEO turnover event in t, $t+1$, $t-1$ and $t-2$. The change in outcome variable is $y_{j(t+1)} - y_{j(t-1)}$. Controls are matched exactly on year, major teaching hospital, minor teaching hospital, specialist hospital and foundation trust status, followed by closest neighbour matching on beds. In case of ties, closest neighbour matching on beds is followed by closest neighbour matching on technology index. Foundation trust status, beds and technology index as of $t-1$; teaching status and specialist status are permanent characteristics. Standard error and p-value for difference in means from two-sample t-tests with equal variance.

W-9 Additional non-parametric estimates of CEOs' impact on hospital behaviour and performance

Table W-12 examines changes in the hospital production measures over the two-year period preceding the CEO turnover event as a check of the common trends assumption. Table W-13 repeats this analysis for the input measures separately in their original units. Table W-14 repeats the analysis in Table B-1 but limits the potential treated observations to the 95 CEOs included in our parametric approach.

Table W-12: Changes in hospital production measures *before* the CEO turnover events analysed in Section Appendix B

		Obs.	Mean change in variable (std. error)	Difference in mean changes (std. error)	p-value
Input measures	Treated	732	0.153 (0.019)		
	Controls	736	0.087 (0.014)	0.066 (0.023)	0.01
	Controls	2,144	0.109 (0.009)	0.044 (0.019)	0.02
Throughput measures	Treated	900	0.132 (0.016)		
	Controls	904	0.144 (0.017)	-0.011 (0.023)	0.62
	Controls	2,647	0.148 (0.010)	-0.015 (0.019)	0.43
Clinical performance measures	Treated	719	0.134 (0.038)		
	Controls	721	0.141 (0.037)	-0.007 (0.053)	0.89
	Controls	2,142	0.152 (0.021)	-0.018 (0.042)	0.67
Staff job satisfaction	Treated	123	0.009 (0.009)		
	Controls	123	0.004 (0.010)	0.005 (0.013)	0.70
	Controls	348	0.004 (0.005)	0.005 (0.011)	0.64
Surplus	Treated	183	-2607 (1359)		
	Controls	184	-2057 (993)	-549 (1682)	0.74
	Controls	536	-1001 (676)	-1606 (1403)	0.25

All outcome variables are standardised to have a mean of zero and a standard deviation of one. Some outcome variables in the stacked sets are multiplied by (-1), so that "more" means "better". The change in outcome variable is $y_{j(t-1)} - y_{j(t-3)}$. The number of treated observations is less than the number of treated observations in Table B-1 because for some treated observations we do not observe the lagged change in the outcome variable. For details on selection of treated and control observation refer to notes in Table B-1. Standard error and p-value for difference in means from two-sample t-tests with equal variance.

Table W-16 presents non-parametric results for the subset of hospitals for whom we observe an average management score in both the 2006 and the 2009 wave of the World Management Survey. Thus, we can include hospitals with a CEO turnover event in 2007 or 2008. There are only 9 treated observations, so the effect estimate is very imprecise. However, there is no indication of a CEO turnover event improving management practices.

Table W-13: Changes in input measures (separately in their original units) *before* the CEO turnover events analysed in Section Appendix B

		Obs.	Mean change in variable (std. error)	Difference in mean changes (std. error)	p-value
Doctors + nurses/beds	Treated	183	0.23 (0.02)		
	Controls	184	0.19 (0.02)	0.04 (0.02)	0.12
	Controls	536	0.20 (0.01)	0.03 (0.02)	0.12
Senior doctors/staff	Treated	183	0.82 (0.14)		
	Controls	184	0.64 (0.07)	0.19 (0.15)	0.23
	Controls	536	0.70 (0.05)	0.12 (0.11)	0.30
Nurses/staff	Treated	183	-0.32 (0.14)		
	Controls	184	-0.82 (0.13)	0.50 (0.19)	0.01
	Controls	536	-0.67 (0.07)	0.35 (0.15)	0.02
Technology	Treated	183	0.022 (0.004)		
	Controls	184	0.019 (0.004)	0.003 (0.006)	0.60
	Controls	536	0.023 (0.003)	-0.001 (0.005)	0.89

All outcome variables are standardised to have a mean of zero and a standard deviation of one. Some outcome variables in the stacked sets are multiplied by (-1), so that "more" means "better". The change in outcome variable is $y_{j(t-1)} - y_{j(t-3)}$. The number of treated observations is less than the number of treated observations in Table W-10 because for some treated observations we do not observe the lagged change in the outcome variable. For details on selection of treated and control observation refer to notes in Table W-10. Standard error and p-value for difference in means from two-sample t-tests with equal variance.

Table W-14: Changes in hospital production measures following a CEO turnover event compared to one or three matched control hospitals with no CEO turnover event with potential treated observations limited to the 95 CEOs observed in two hospitals for at least two years each

		Obs.	Mean change in variable (std. error)	Difference in mean changes (std. error)	p-value
Input measures	Treated	424	0.147 (0.019)		
	Controls	424	0.111 (0.021)	0.036 (0.028)	0.20
	Controls	1,232	0.133 (0.013)	0.014 (0.025)	0.57
Throughput measures	Treated	524	0.173 (0.021)		
	Controls	524	0.115 (0.019)	0.058 (0.028)	0.04
	Controls	1,520	0.153 (0.012)	0.020 (0.024)	0.42
Clinical performance measures	Treated	450	0.123 (0.045)		
	Controls	450	0.148 (0.043)	-0.025 (0.063)	0.69
	Controls	1,329	0.137 (0.024)	-0.015 (0.049)	0.76
Staff job satisfaction	Treated	84	0.097 (0.116)		
	Controls	84	0.393 (0.106)	-0.296 (0.157)	0.06
	Controls	242	0.262 (0.062)	-0.164 (0.125)	0.19
Surplus	Treated	106	0.148 (0.133)		
	Controls	106	0.233 (0.092)	-0.085 (0.162)	0.60
	Controls	308	0.112 (0.043)	0.037 (0.107)	0.73

All outcome variables are standardised to have a mean of zero and a standard deviation of one. Some outcome variables in the stacked sets are multiplied by (-1), so that "more" means "better". The maximum number of treated observations is less than 95 CEOs x 2 hospitals x 5 measures in a set = 950 for the following reasons: Treated observations are hospital-years with a CEO turnover event in t, the new CEO still in post in $t+1$ and no CEO turnover event in $t-1$ and $t-2$. We cannot use observations for 2000/01 and 2001/02 since we cannot establish whether there was no turnover event in $t-1$ and $t-2$. One or up to three controls are chosen from hospital-years with no CEO turnover event in t, $t+1$, $t-1$ and $t-2$. Controls are matched exactly on year, major teaching hospital, minor teaching hospital, specialist hospital and foundation trust status. Some treated observations remain without a match. Exact matching is followed by closest neighbour matching on beds. In case of ties, closest neighbour matching on beds is followed by closest neighbour matching on technology index. Foundation trust status, beds and technology index as of $t-1$; teaching status and specialist status are permanent characteristics. The change in outcome variable is $y_{j(t+1)} - y_{j(t-1)}$. Standard error and p-value for difference in means from two-sample t-tests with equal variance.

Table W-15: Changes in hospital production measures over a period of 3 years instead of 2 years

		Obs.	Mean change in variable (std. error)	Difference in mean changes (std. error)	p-value
Input measures	Treated	604	0.241 (0.024)		
	Controls	604	0.187 (0.020)	0.054 (0.031)	0.08
	Controls	1,732	0.184 (0.013)	0.058 (0.026)	0.03
Throughput measures	Treated	741	0.246 (0.022)		
	Controls	741	0.213 (0.021)	0.033 (0.030)	0.27
	Controls	2,125	0.229 (0.013)	0.018 (0.025)	0.47
Clinical performance measures	Treated	632	0.261 (0.039)		
	Controls	632	0.217 (0.040)	0.044 (0.056)	0.44
	Controls	1,826	0.201 (0.023)	0.060 (0.045)	0.18
Staff job satisfaction	Treated	114	0.227 (0.112)		
	Controls	114	0.419 (0.107)	-0.191 (0.155)	0.22
	Controls	322	0.389 (0.060)	-0.162 (0.121)	0.18
Surplus	Treated	151	-0.003 (0.102)		
	Controls	151	-0.020 (0.070	0.017 (0.124)	0.89
	Controls	433	-0.098 (0.058)	0.095 (0.115)	0.41

Treated observations are hospital-years with a CEO turnover event in t, the new CEO still in post in $t+1$ and $t+2$ and no CEO turnover event in $t-1$ and $t-2$. One or up to three controls are chosen from hospital-years with no CEO turnover event in t, $t+1$, $t+2$, $t-1$ and $t-2$. Controls are matched exactly on year, major teaching hospital, minor teaching hospital, specialist hospital and foundation trust status. Some treated observations remain without a match. Exact matching is followed by closest neighbour matching on beds. In case of ties, closest neighbour matching on beds is followed by closest neighbour matching on technology index. Foundation trust status, beds and technology index as of $t-1$; teaching status and specialist status are permanent characteristics. The change in outcome variable is $y_{j(t+2)} - y_{j(t-1)}$. Standard error and p-value for difference in means from two-sample t-tests with equal variance.

If anything, a turnover event decreases the average management score.

Table W-16 also presents estimates of the impact of a CEO turnover event on how much hospitals spend on CEO remuneration. The estimates suggest that as a result of a CEO turnover event hospitals' spending on CEO remuneration increases by about £7,500 more than it would have done in the absence of a turnover event. However, the last panel of Table W-16 shows that the parallel trend assumption for hospital spending on CEO pay is unlikely to be satisfied, since it increased by about £7,400 less in treated hospitals over the two-year period before the CEO turnover event.

Table W-16: Changes in average management score and hospital spending on CEO remuneration following a CEO turnover event compared to one or three matched control hospitals with no CEO turnover event

		Obs.	Mean change in variable (std. error)	Difference in mean changes (std. error)	p-value
Average management score	Treated	9	0.076 (0.162)		
	Controls	9	0.272 (0.243)	-0.195 (0.292)	0.51
	Controls	23	0.315 (0.156)	-0.239 (0.271)	0.39
Hospital spending on CEO remuneration	Treated	175	8,672 (2,140)		
	Controls	175	827 (1,088)	7,845 (2,400)	0.001
	Controls	509	1,225 (616)	7,448 (1,636)	0.00
Changes in spending on CEO remuneration *before* turnover event	Treated	150	2,117 (2,128)		
	Controls	149	9,532 (1,683)	-7,414 (2,716)	0.01
	Controls	427	9,538 (910)	-7,421 (1,987)	0.00

Treated observations are hospital-years with a CEO turnover event in t, the new CEO still in post in $t+1$ and no CEO turnover event in $t-1$ and $t-2$. Up to three controls are chosen from hospital-years with no CEO turnover event in t, $t+1$, $t-1$ and $t-2$. The change in outcome variable is $y_{j(t+1)} - y_{j(t-1)}$. Controls are matched exactly on year, major teaching hospital, minor teaching hospital, specialist hospital and foundation trust status, followed by closest neighbour matching on beds. In case of ties, closest neighbour matching on beds is followed by closest neighbour matching on technology index. Foundation trust status, beds and technology index as of $t-1$; teaching status and specialist status are permanent characteristics. For changes in spending on CEO remuneration *before* turnover event, the change in outcome variable is $y_{j(t-1)} - y_{j(t-3)}$. The number of treated observations is less than the number of treated observations for hospital spending on CEO remuneration because for some treated observations we do not observe the lagged change in hospital spending on CEO remuneration. Standard error and p-value for difference in means from two-sample t-tests with equal variance.

W-10 Endogenous assignment analyses using input, throughput and performance measures separately in their original units

We examine whether CEOs who did well at their first hospital are subsequently hired at a problematic hospital. We define doing well relative to a CEO's peers using the mean residual from Equation 5 for the financial years $t_1^{i,A}$ to $t_n^{i,A}$ when CEO i is observed in hospital A. For length of stay, waiting time, canceled operations, AMI deaths, stroke deaths, FPF deaths, readmissions and MRSA rate we classify as good performers CEOs whose mean residual is at or below the 25^{th} percentile. For technology, day cases and admissions we define as good performers CEOs whose mean residual is at or above the 75^{th} percentile. We omit from this analysis input variables (such as beds and labour skills ratios) because it is unclear what would be considered good performance along these dimensions.

Table W-17 presents results for linear probability models regressing an indicator of moving to a "problematic" hospital on an indicator of good performance at a CEO's first hospital. There are 11 production measures $\times$ 4 definitions of "problematic", generating a total of 44 coefficient estimates. 6 of these estimates, i.e. 13.6% are statistically significant at the 10% level, but there is no clear pattern in the direction of association. For 1 production measure better performance immediately prior to arrival is associated with being at problematic hospital, but for 5 measures the association is negative.

Next, we examine whether the variability measure is larger for CEOs who are at some point in their career assigned to "problematic" hospitals. For each of our four definitions of "problematic", we regress each of our 15 variability measures against a dummy variable indicating that the CEO was ever observed in a "problematic" hospital. Thus, we run 60 separate regressions and obtain 60 coefficients on a "problematic" hospital dummy variable. The results are in Table W-18. 6 out of the 60 coefficients, i.e. 10%, are statistically significantly different from zero at the 10% significance level, a result we would expect just by chance. Furthermore, only three of them (for waiting times, cancelled operations and admissions) are positive, suggesting that being at a "problematic" hospital is associated with higher variability in CEO performance. For the other three statistically significant coefficients the estimated association is negative, suggesting that CEOs who are at some point at a more problematic hospital actually have lower variability in their performance across hospitals.

Table W-17: Linear probability models of the impact of good performance in first hospital on moving to a “problematic” hospital

	Hospital commission rating poor in year before CEO arrived			Hospital with surplus below 25th percentile in year before CEO arrived			‘New’ hospital created through merger during sample period			Hospital with PFI contract at some point during CEO’s tenure		
	Good perf.	Const.	N	Good perf.	Const.	N	Good perf.	Const.	N	Good perf.	Const.	N
Technology	0.18	0.37	71	0.02	0.46	91	0.04	0.31	95	-0.04	0.43	95
	(0.13)	(0.07)		(0.13)	(0.06)		(0.11)	(0.06)		(0.12)	(0.06)	
Admissions	-0.15	0.46	71	0.14	0.43	91	0.19*	0.27	95	-0.01	0.42	95
	(0.13)	(0.07)		(0.12)	(0.06)		(0.11)	(0.05)		(0.12)	(0.06)	
Length of stay	-0.11	0.44	70	-0.04	0.48	90	0.02	0.31	94	0.10	0.40	94
	(0.14)	(0.07)		(0.12)	(0.06)		(0.11)	(0.06)		(0.12)	(0.06)	
Day cases	0.10	0.40	71	0.05	0.45	91	-0.07	0.33	95	-0.10	0.44	95
	(0.14)	(0.07)		(0.12)	(0.06)		(0.11)	(0.06)		(0.12)	(0.06)	
Waiting time	-0.11	0.44	70	-0.02	0.45	89	-0.21*	0.38	93	0.04	0.42	93
	(0.14)	(0.07)		(0.12)	(0.06)		(0.11)	(0.06)		(0.12)	(0.06)	
Canceled ops.	0.004	0.42	67	-0.08	0.48	86	0.11	0.28	90	-0.22*	0.48	90
	(0.14)	(0.07)		(0.13)	(0.06)		(0.11)	(0.06)		(0.12)	(0.06)	
AMI deaths	0.11	0.43	55	0.037	0.53	57	0.14	0.36	61	-0.16	0.53	61
	(0.15)	(0.08)		(0.16)	(0.08)		(0.14)	(0.07)		(0.15)	(0.07)	
Stroke deaths	-0.03	0.47	61	-0.10	0.54	68	-0.17	0.39	72	0.06	0.44	72
	(0.15)	(0.08)		(0.14)	(0.07)		(0.13)	(0.06)		(0.14)	(0.07)	
FPF deaths	-0.08	0.48	61	-0.43***	0.62	68	-0.09	0.37	72	-0.17	0.50	72
	(0.15)	(0.07)		(0.14)	(0.07)		(0.13)	(0.07)		(0.14)	(0.07)	
Readmissions	0.03	0.41	76	-0.01	0.48	75	-0.23*	0.39	78	-0.28	0.49	78
	(0.13)	(0.07)		(0.13)	(0.07)		(0.12)	(0.06)		(0.13)	(0.06)	
MRSA rate	-0.26*	0.44	58	-0.08	0.50	77	-0.03	0.33	80	0.03	0.42	80
	(0.14)	(0.07)		(0.13)	(0.07)		(0.12)	(0.06)		(0.13)	(0.06)	

Each entry in this table refers to a separate regression of an indicator of a CEO moving to a “problematic” hospital on an indicator of good performance at the CEO’s first hospital. “Problematic” hospital is defined as either poor hospital commission rating, surplus below 25^{th} percentile, hospital created through merger or hospital with PFI contract. Good performance at the CEO’s first hospital is defined as the mean residual for the CEO spell at the first hospital being at or below the 25^{th} percentile for length of stay, waiting time, cancelled operations, AMI deaths, stroke deaths, FPF deaths, readmissions and MRSA rate and as the mean residual being at or above the 75^{th} percentile for technology, day cases, admissions. Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%

Table W-18: Impact of ever being observed at a "problematic" hospital on variability in CEO performance as measured by the absolute difference in the mean residuals for the CEO spells at each of their two hospitals for each production measure

	Hospital commission rating poor in year before CEO arrived	Hospital with surplus below 25th percentile in year before CEO arrived	'New' hospital created through merger during sample period	Hospital with PFI contract at some point during CEO's tenure	Mean (st. dev.) [obs.] of dependent variable (Absolute difference in mean residuals at both hospitals)
Doctors + nurses/beds	-0.06 (0.04) [88]	-0.06 (0.04) [91]	-0.10 (0.04) [94]	-0.05 (0.04) [94]	0.19 (0.18) [94]
Senior doctors/staff	-0.04 (0.08) [89]	-0.02 (0.11) [92]	-0.11 (0.10) [95]	0.03 (0.10) [95]	0.58 (0.49) [95]
Nurses/staff	0.05 (0.20) [89]	-0.06 (0.21) [92]	0.15 (0.20) [95]	0.08 (0.19) [95]	1.21 (0.93) [95]
Technology	-0.008 (0.01) [89]	0.013 (0.01) [92]	0.002 (0.01) [95]	0.00 (0.01) [95]	0.052 (0.046) [95]
Beds	7.68 (12.3) [89]	6.14 (11.5) [92]	13.8 (11.8) [95]	15.5 (11.6) [95]	56.5 (56.4) [95]
Admissions	869 (933) [89]	-959 (980) [92]	-832 (907) [95]	1,603* (886) [95]	5,343 (4,326) [95]
Length of stay	-0.001 (0.08) [88]	-0.02 (0.07) [91]	0.10 (0.08) [94]	0.03 (0.08) [94]	0.38 (0.36) [94]
Day cases	0.06 (0.41) [89]	0.41 (0.42) [92]	-0.01 (0.39) [95]	0.44 (0.39) [95]	2.37 (1.87) [95]
Waiting time	4.00** (1.82) [88]	2.89 (1.95) [90]	1.10 (1.83) [93]	-1.02 (1.81) [93]	10.8 (8.66) [93]
Cancelled ops.	2.77 (25.5) [84]	25.4 (25.2) [87]	73.8*** (24.0) [90]	29.1 (24.6) [90]	124 (116) [90]
AMI deaths	0.14 (0.39) [59]	-0.33 (0.46) [58]	-0.35 (0.37) [61]	-0.79** (0.38) [61]	1.34 (1.46) [61]
Stroke deaths	0.19 (0.34) [69]	-0.19 (0.37) [69]	-0.34 (0.32) [72]	-0.10 (0.33) [72]	1.83 (1.35) [72]
FPF deaths	-0.13 (0.26) [69]	0.25 (0.30) [69]	-0.44* (0.25) [72]	-0.34 (0.26) [72]	1.06 (1.06) [72]
Readmissions	-0.02 (0.11) [76]	-0.09 (0.12) [75]	-0.25** (0.11) [78]	-0.15 (0.11) [78]	0.56 (0.47) [78]
MRSA rate	-0.69 (0.60) [76]	0.25 (0.64) [78]	-0.44 (0.59) [80]	0.27 (0.59) [80]	2.73 (2.59) [80]

Each entry in this table refers to a separate regression of a performance variability measure on a dummy variable indicating that the CEO has ever been observed at a "problematic" hospital defined as either poor hospital commission rating, surplus below 25th percentile, hospital created through merger or hospital with PFI contract. Standard errors in (parentheses) and number of observations in [brackets]. *Significant at 10%, **significant at 5%, ***significant at 1%

W-11 Estimates of quality of CEO-hospital matches for all observed CEO spells

Table W-19: Estimates of quality of CEO-hospital matches all observed CEO spells

	Input measures	Through-put measures	Clinical performance measures	Job satisfaction	Surplus
Female * teaching hosp.	-0.02	-0.27***	0.37**	0.13	-0.66
	(0.06)	(0.04)	(0.17)	(0.10)	(0.58)
Female * foundation hosp.	-0.01	-0.13***	0.02	-0.07	-0.05
	(0.04)	(0.04)	(0.05)	(0.09)	(0.10)
Female * competitive	0.00	0.02	-0.06	-0.12	0.11
	(0.05)	(0.04)	(0.05)	(0.13)	(0.13)
Female * beds (100s)	-0.01	-0.00	0.02**	0.00	-0.02
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)
Clinical backgr.* teaching hosp.	-0.05	0.04	0.17	0.16	0.18
	(0.04)	(0.09)	(0.17)	(0.11)	(0.53)
Clinical backgr. * foundation	0.01	-0.03	0.10	-0.12	-0.17
	(0.03)	(0.04)	(0.06)	(0.09)	(0.13)
Clinical backgr. * competitive	0.01	0.06	0.04	0.21*	-0.21
	(0.04)	(0.05)	(0.07)	(0.12)	(0.34)
Clinical backgr. * beds (100s)	-0.00	0.00	0.01	0.01	-0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)
Private sector * teaching hosp.	-0.04	-0.14	0.21**	0.02	0.06
	(0.05)	(0.11)	(0.09)	(0.13)	(0.21)
Private sector * foundation hosp.	0.01	0.04	0.03	-0.04	0.37****
	(0.06)	(0.05)	(0.09)	(0.16)	(0.12)
Private sector * competitive	-0.14	-0.05	-0.13	-0.17	0.00
	(0.20)	(0.05)	(0.10)	(0.16)	(0.19)
Private sector * beds (100s)	-0.00	-0.01*	0.02***	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
PG managm. qual. * teaching	0.06	-0.07	-0.11	-0.10	-0.42
	(0.05)	(0.07)	(0.13)	(0.08)	(0.48)
PG managm. qual. * foundation	-0.01	-0.03	0.01	-0.01	0.04
	(0.03)	(0.03)	(0.04)	(0.09)	(0.11)
PG managm. qual. * competitive	-0.08	0.03	-0.01	0.08	0.06
	(0.05)	(0.04)	(0.05)	(0.12)	(0.13)
PG managm. qual. * beds (100s)	0.01	-0.00	0.00	-0.01	-0.01
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	11853	9576	9772	1838	2391

Each estimate is from a separate regression of the stacked measures on hospital characteristics, financial year effects, hospital effects, the relevant CEO characteristic and the interaction of the relevant CEO characteristic and hospital characteristic. All outcome variables in a stacked set as well as the individual outcome variables are standardised to have a mean of zero and a standard deviation of one. Some outcome variables in the stacked sets are multiplied by (-1), so that "more" means "better". Standard errors in (parentheses). *Significant at 10%, **significant at 5%, ***significant at 1%