Public Managers and Hospital Performance*

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Abstract

We study the extent to which and how managers can increase government productivity in the context of public health provision. Using a staggered difference-in-differences design, we evaluate a reform that introduced a competitive recruitment system to appoint CEOs in public hospitals in Chile. We present three main findings. First, we show that the policy reduced hospital mortality between 9% and 14%, an effect that persisted after 3 years. This effect is not explained by a change in patient composition and is robust to several alternative explanations. Second, we document that the policy changed the pool of CEOs by displacing older doctors in favor of younger CEOs with undergraduate degrees whose majors included management coursework. We show that the reform affected hospital mortality only when newly appointed managers had management studies. Third and finally, we find that the reform operated through more efficient use of medical resources and better personnel practices. We complement these findings with an alternative empirical strategy that exploits the rotation of managers across public hospitals and find similar results.

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1 Introduction

Government spending explains on average more than 40% of GDP (OECD, 2021). On that account, enhancing state efficiency is central to any effort to boost overall productivity. One popular policy to raise state productivity is to place emphasis on public sector managers, who directly supervise the delivery of public goods and services, and might be a key lever for strengthening state capacity (Pollitt and Bouckaert, 2017). Yet research on whether and how public sector managers can improve their organization's performance is limited (Bertrand et al., 2020). Empirical progress is difficult because of two important challenges. First, it is hard to come by with sources of quasi-experimental variation in state personnel selection processes. Second, it is challenging to study managerial effectiveness due to the lack of objective and verifiable performance outcomes in the public sector (Besley et al., 2022).

In this paper, we overcome these challenges by analyzing a policy reform in Chile that changed the selection process for senior executives in the public sector. We focus on public hospital CEOs, which allows us to observe objective and relevant short-term outcomes to assess their managerial performance. Our focus on public hospitals is also important for other reasons. First, the health sector is large and costly; healthcare represents almost 20% of government expenditures in the average OECD country, and costs are increasing rapidly over time.[1] Second, public hospitals are relevant with respect to access and equity in healthcare. In most countries, public hospitals are the largest medical bed providers, and they maintain a minimum level of access and quality for underserved communities.[2]

In 2003, the Chilean Congress passed a law designed to attract talent to public sector top management positions by introducing a new recruitment system. The reform had two main components. First, it introduced a public, competitive, and transparent selection system for senior executive positions. Second, it offered financial incentives in the form of performance pay and base wage increases to narrow wage gaps with similar positions in the private sector. After a position is subject to the recruitment reform, all future managers in that position have to be hired accordingly. The reform affected top-level positions in public agencies and was gradually implemented across all Ministries and other public organizations. In 2004, eight managers in senior executive positions were hired using this selection system; by 2019, more than 3,400 senior executives in 1,400 positions had been appointed.

[1] For instance, between 2000 and 2019 healthcare costs increased by 15% as a share of GDP on average in OECD countries and have boomed in the post-pandemic world.

[2] In the OECD, medical beds in public hospitals represent 72% of total supply. See Appendix Figure A.1.

To study how the reform affected hospital performance, we draw on several sources of information and build a novel and comprehensive dataset including the identity, tenure, educational background, cognitive skills, and demographics of managers in all public hospitals between 2001 and 2019. We complement these data with restricted-use employer-employee data from the Ministry of Health for all public hospitals, albeit available for a shorter period. To measure hospital performance, we rely on nationwide individual-level inpatient discharges between 2001 and 2019 for all public hospitals and country-wide individual-level death records. We complement these data with hospital inputs and procedures. We thus have an extraordinarily rich window into hospital mortality, procedures and inputs, patient characteristics, and managers' characteristics and tenure in every public hospital.

We present three main findings. First, the selection reform significantly improved hospital performance. We exploit the gradual adoption of the new selection system for public hospital CEOs to estimate its causal effects on hospital performance using a staggered difference-in-differences research design. We follow the literature in using hospital mortality rates, as our key measure of outcome-based productivity (e.g., Bloom et al., 2015; Doyle et al., 2015, 2019). We show that the reform decreased death rates between 9% and 14% in public hospitals in the three-year window after adoption. These effects are similar to the impact on hospital mortality of other policies studied in the literature, such as increasing patient expenditures by a 10% (Doyle et al., 2015) and improved practices in VA hospitals in the United States (Chan et al., 2022).

Our empirical analysis is subject to two econometric concerns. The first relates to the adequacy of the underlying identification assumption of the staggered difference-in-differences model. The working assumption is that hospitals adopting the selection reform are not on different trends to those that have not adopted it. We justify this assumption in several ways. First, we show that pre-reform, the growth of an exhaustive list of variables, including hospital outcomes, patient characteristics, and political variables, does not differ between hospitals that adopt and do not adopt the reform. We also show that within adopters, the timing is not explained by these variables. Second, using an event study design, we show graphically that hospitals are not on different trends before the adoption of the policy. The lack of pre-trends eases the concerns of an Ashenfelter-style dip, which is a natural threat in settings where management changes can respond to a decline in performance. Third, we show that the dynamic effects of the reform gradually grow during the early quarters post-adoption and flatten after, which is the expected trajectory if new managers are to have an impact on performance. Additionally, we provide event-study evidence showing that CEO transitions in hospitals that have not adopted the selection reform have zero impact on hospital performance, which rules out mechanical effects of the reform due to CEO turnover.

The second concern is that our estimates might pick up patient selection instead of the true effect of the policy. Perhaps after the reform, managers selected healthier inpatients, or the latter self-selected into hospitals that were improving their performance. We provide several pieces of evidence to address these concerns. First, the Chilean public health system is particularly well suited for this study because there is minimal scope for patient selection. Within the public health network, patients cannot choose their hospital provider and are referred to public hospitals following strict guidelines from primary care. By the same token, hospitals cannot select patients based on their characteristics (Ley 19,937, 2004; Decreto 38, 2005). Consistent with the setting's features, we do not find evidence of impacts of reform adoption on the composition of patients' diagnoses. Second, in our baseline estimates we control by an exhaustive set of case mix controls that include detailed information of patient demographics and diagnoses. We also provide estimates for risk-adjusted mortality rates following the prediction procedure used by the Centers for Medicare and Medicaid Services (CMS) in the United States. Third, to deal with further concerns of unobserved selection, we examine the effect of the reform on deaths outside treated hospitals. To the extent that patients rejected from a given hospital die, they would show up in the statistics of other hospitals or be recorded as home deaths. We find evidence of zero effects in neighboring hospitals or aggregate home deaths at the municipality level. Finally, we also find that the reform had similar effects when focusing exclusively on locked-in patients who cannot access healthcare in the private sector.

Our second finding is that the reform was most effective when the appointed CEOs had management studies. We first document that the reform replaced doctors working as CEOs ("doctor CEOs" for short) for CEOs with undergraduate degrees in management-related majors.[3] Before the reform, almost 90% of CEOs were doctors. The reform increased by 21 percentage points the share of CEOs with undergraduate degrees in management and decreased the share of doctor CEOs by a similar magnitude. Interestingly, this result masks heterogeneity between the impact on doctor CEOs with and without management studies. The reform actually increased the share of doctors CEOs with management studies but had a larger negative effect for the rest of doctor CEOs. We find suggestive evidence that through this channel, the reform also had an effect incentivizing doctors who wanted to apply for a CEO position to further invest in management studies. Motivated by these findings, we explore whether CEOs with management studies are more effective. We find that the effects of the reform are mostly explained by CEOs who hold management studies. We also find that new managers are around two years younger and that the reform had no effect on the likelihood that the CEO is female.

[3]Management-related majors include public administration, business and economics, accounting, and engineering.

We also explore the impact of the financial incentives included in the recruitment reform and find no evidence of them driving the results. First, we rule out that performance pay affected managerial performance. We show that incentives were poorly designed and were not binding, a feature that was true across all positions appointed using the reform's selection system and not specific only to public hospitals. We document that the policy increased the CEO wages by around a third relative to pre-reform wages. To examine whether higher wages induced by the reform had an effect on managerial performance, we leverage an amendment to the reform that increased the pay for managers that are doctors *and* that were appointed after November 2016. We find that the amendment significantly increased wages for treated managers but had no discernible effects on their performance. This evidence suggests that efficiency wages do not drive the main results.

Our third and final finding is that managers increased hospital performance by means of more efficient use of medical resources and better personnel practices. We find that the reform increased operating utilization by 30% three years after, an effect that is equivalent to closing the gap between the average efficiency in high-complexity hospitals in our setting to the average efficiency in the National Health System of the United Kingdom. Consistent with our finding, we also show that surgical procedures increased by a similar magnitude. We also show that the reform significantly reduced the turnover of doctors, the most scarce human resource in the hospital's production function. These findings are in line with recent literature in personnel economics showing that better-managed firms retain workers with higher human capital (Bender et al., 2018).

We conclude our analysis by leveraging the rotation of managers across public hospitals to examine the extent to which they affect hospital performance, similar to the research design in Fenizia (2022). One of the advantages of this strategy is that it allows us to leverage different data spans and serves as a complement to the evidence presented so far, which relies mostly on the adoption of the recruitment reform. We estimate a model with CEO and hospital fixed effects following the seminal work by Abowd et al. (1999) and Card et al. (2013), and find that CEOs explain a non-trivial share of the variation in death rates across hospitals. To connect these results with the findings from the selection reform, we provide evidence of the impact of the latter on CEO managerial quality, which we measure using the estimates of the CEO fixed effects. Using an event study design, we find that the recruitment reform had a significant effect on CEO managerial quality and that the trajectory of the estimates is consistent with the impact of the reform on hospital mortality rates.

This paper contributes to multiple strands of the literature. First, it adds to recent research on the impact of managers and managerial practices in public organizations (Bloom et al., 2015; Limodio, 2019; Rasul and Rogger, 2018; Janke et al., 2020; Fenizia, 2022). Second, it contributes

to the literature studying the impacts of discretionary appointments (Myerson, 2015; Padró i Miquel et al., 2018; Xu, 2018; Colonnelli et al., 2020; Voth and Xu, 2022) and civil service recruitment in the public sector (Dal Bó et al., 2013; Muñoz and Prem, 2022; Moreira and Pérez, 2021). More generally, by studying a reform aimed at improving the selection of state personnel in a developing country, our research also connects to studies on bureaucratic effectiveness and its impact on development (see Besley et al., 2022 for a review). Finally, our work complements previous studies on the efficiency of health spending (Propper and Van Reenen, 2010; Gaynor et al., 2013; Bloom et al., 2015; Doyle et al., 2019; Chan et al., 2022).

The rest of the paper proceeds follows. Section 2 briefly describes the public tertiary health sector in Chile, discusses the recruitment reform and its implementation, and presents the data. Section 3 presents the main effects of the reform on health quality and discusses the validity of the results. Section 4 explores the effect of the reform on managers' characteristics. Section 5 asks which factors can explain the effectiveness of the new managers. Section 6 explores potential mechanisms through which new managers affect hospital quality. In section 7, we leverage the rotation of managers to study the effect of managerial talent on hospital performance. Finally, Section 8 concludes. The Appendix provides additional results.

2 Setting and Data

2.1 The Healthcare System in Chile

Chile's healthcare system comprises public and private health providers and public and private insurers. Public insurance is funded by general taxation and payroll taxes on enrolled people. Individuals can opt-out and use their health contributions to buy private insurance.[4] Individuals without the ability to pay can freely access the public system, which results in nearly universal health coverage.

Approximately 78% of the population is under public health coverage, 15% have private insurance, and the remainder is under special regimes exclusive to the police and armed forces.[5] The

[4]The healthcare system in Germany features an analogous mechanism. Upon meeting certain conditions, individuals can use their health contribution to buy private insurance (known as PKV) and opt-out of the public health insurance system (known as GKV).

[5]For comparison, private compulsory health insurance spending explains around 10% of health expenditures, similar to Germany and France. In 28 out of 35 OECD economies, however, it comprises less than 5% of health expenditures (OECD, 2022b).

ability of individuals to freely use the health contribution to buy private coverage has induced a stark market segmentation. Private insurers are able to charge differentiated premiums and select healthier and more affluent customers. While this has led to massive sorting across the private and public health sector,[6] there is little scope for selection within the public health sector. The reason is that individuals with public coverage cannot choose their health provider within the public network.[7] Individuals need to register in the healthcare center that provides primary attention in their local area and patients who need specialized attention are referred to specialty clinics or to the hospital. Referrals follow strict guidelines mostly based on the geographical location of the patient's primary care center (Ley 19,937, 2004). In Appendix A, we describe the referral process and empirically show the lack of selection within the public network. Patients can also be admitted directly to public hospitals in emergency cases.

Public healthcare providers are organized geographically under 29 "Health Services," the administrative units within which the referral and counter referral system is organized. These are decentralized organizations subject to oversight by the Ministry of Health and are responsible for the articulation, management, and development of public healthcare establishments in municipalities in their territory. It includes primary, secondary, and tertiary public healthcare and other private establishments that maintain agreements with the respective Health Service. Appendix Figure A.2 shows the geographic distribution of the 29 Health Services and the municipalities under their scope.

The head of the Health Service is also the immediate superior of CEOs of public hospitals that fall under their territorial scope. CEOs are in charge of the management, organization, and administration of their hospital, and their duties include (i) the administration of personnel, (ii) the allocation of hospital inputs and human resources, (iii) the management of financial resources and proposal of the annual budget, (iv) infrastructure and technological equipment resources decisions, and (v) the integration of the hospital into the health network and with the community, among others. CEOs are supported in these tasks by a team of deputy managers or *middle* managers. The typical hospital has three deputy managers: the Deputy Medical Manager, the Deputy Administration Manager, and the Deputy Human Resources Manager. In some cases, there is a fourth deputy manager overseeing nursing care.

[6]Almost 70% of people at the top 10% of the income distribution have private coverage, while only 4% in the bottom 50% buy private coverage (CASEN, 2017).

[7]While private insurers may provide coverage in public hospitals, this is rarely seen in the data. The reason is that individuals under private insurance are already self-selected into the private health sector and have little incentive to choose public healthcare providers. In the universe of admissions, 96% of patients at public hospitals have public insurance. Under public coverage, individuals can choose private health centers, although it is more expensive than public hospitals. Around 25% of inpatients at private hospitals have this coverage.

2.2 The Recruitment Reform

In 2003, a political scandal exposed illegal payments to top government officials. In response to and product of a broad political consensus, Congress enacted Law Nº 19,882, creating a new framework to regulate the public sector's personnel policy (Ley 20,955). Under this new framework, the law created the Senior Executive Service System with the aim *"to provide government institutions—through public and transparent competitions—with executives with proven management and leadership capacity to execute effectively and efficiently the public policies defined by the authority."*[8]

The reform includes two main components. First, it changes the recruitment process of top managers in government agencies. Before the reform, most senior executive positions were discretionary appointments by the superior officer. After the reform, top managers are elected through public, competitive, and transparent competitions.

The job announcement for a top management position starts with the position being posted online on the Civil Service's website and in a newspaper with national circulation. In some cases, the Civil Service may additionally hire headhunters to widen the pool of applicants. Applicants need at least a professional degree, and depending on the position, other competencies are desired. After the job posting finishes, the Civil Services sends the set of eligible applicants to a third-party human resources firm that evaluates each individual's job trajectory according to the job profile. They also evaluate their motivation and overall competencies. The consultant gives every applicant a grade based on an objective rubric and provides a shortlist to the Civil Service. In the next phase, a committee formed by representatives of the Civil Service and the Ministry where the position is based interviews the remaining candidates and selects a short list of three individuals based on objective criteria. In the last step, the superior officer selects the winning candidate from the final roster with discretionary authority. Appendix Figure A.3 provide a visual illustration of the recruitment process.

The reform also increased CEO pay by providing higher base wages and performance incentives. The size of the wage increase varies across positions and is paid as a monthly bonus.[9] In the case of public hospital CEOs, we document that the reform increased the position's pay by 33% (see Appendix D for further details). The financial package also includes a performance pay

[8] According to the Civil Service's statement of purpose of the reform, available in https://www.serviciocivil.cl/sistema-de-alta-direccion-publica-2/.

[9] Two limits cap the extra bonus. First, it cannot be larger than a 100% of the base wage (which in the public sector is substantially lower than the total remuneration due, for example, to another tenure- and sector-specific bonuses). Second, the total wage cannot be higher than that of the Under Secretary of the Ministry where the position is based.

component, under which the yearly wage is penalized if the manager does not meet certain performance thresholds. We provide more details of the performance pay schedule and performance scores in Section 5.

The adoption of the recruitment process occurred gradually across public agencies over time. For example, the head of the Tax Authority has to be appointed using this selection system. The law mandated that between 2004 and 2010, the Ministry of Finance had to determine a minimum of 100 top executive positions to adopt the new recruitment system. Panel A in Figure 1 depicts the number of positions in public agencies that adopted the recruitment reform between 2004 and 2019. All new top management positions created after the law was enacted must select their top manager using the new selection system. For existing positions, once they are subject to the new recruitment system, all future managers must be hired by the same process (i.e., treatment is an absorbing state). Panel B in Figure 1 shows the number of recruitment processes run by the Civil Service in a given year. The spikes we observe in 2011, 2015 and 2019 are evidence of substantial turnover in senior executive positions after a new government is in place.

In the case of public hospitals, adoption is mainly driven mainly by their size and complexity: high, medium, and low, which is defined by the number of beds and the number of procedures they offer. Note that once the Ministry of Finance approves the recruitment process for a given position, it only starts after a manager transition. Therefore, the timing is also explained by transitions of incumbent managers. In Appendix Figure A.4 we plot a histogram of the adoption of the recruitment policy in public hospitals between 2005 and 2019. The first time a public hospital adopted the selection system was during the fourth quarter of 2005, after which other hospitals started adopting it gradually over time. In total, 88 out of 188 hospitals adopt the selection reform in the time window of the study.

Each adoption is costly and therefore the Ministry of Finance has to approve it. The reason is twofold. First, the Civil Service has constrained capacity and can run only a limited number of processes without increasing its personnel. Second, adopting the recruitment process for a position implies higher wages and the costs of running the process (which includes, among others, hiring a human resources firm to lead part of the selection process). Since adopting the reform triggers the new selection process for all future managers, each adoption implies a permanent expense.

2.3 Data Sources

For this paper, we build a novel dataset that identifies the manager and middle managers in every public hospital in the country, spanning every month between January 2001 and December 2019.

These data are not available in a systematic way and are the result of nearly a thousand FOAI requests to local hospitals and health authorities, who, in many cases, had to collect archive data. We complement these data with background and performance records. For background characteristics, we collected date of birth, gender, test scores, and educational attainment. We gather this information from several sources, including a national registry of all medical personnel in the country, CVs requested at the Civil Service, LinkedIn profiles, articles from local newspapers, and information provided by universities upon request, among others. Finally, via a series of FOIA requests to the Civil Service, we also have access to the pay-for-performance agreements and the job performance scores obtained by the CEOs appointed under the new selection system.

We also access restricted-use administrative records covering the universe of employees in all public hospitals between 2014 and 2019. The data is collected by the Ministry of Health and unified in a single registry for HR purposes, the "Human Resources Information System". Data include detailed payroll information and wages at the monthly level. Among other characteristics, we observe the establishment, their job (and in the case of doctors, their specialty), the number of hours worked, the date of birth and gender, and a detailed wage breakdown.

In terms of hospital performance, we use detailed inpatient data collected by the Ministry of Health (DEIS, 2019). We access individual-level inpatient events that end in a discharge or death in all public hospitals in Chile between 2001 and 2019, encompassing almost 29 million events. Data include the diagnosis classified according to the tenth edition of the International Classification of Diseases (hereafter, ICD-10 codes), the type of admission (e.g., emergency cases or referrals), the date of discharge or the date of death in case the individual died in the hospital, and a set of individual characteristics, including date of birth, gender, county of residence, and type of health insurance. Data also include an encoded id that enables us to follow individuals over time and to merge individual health events with deaths occurring outside the hospital. We have individual-level records processed by the Vital Records Office for all deaths in the country between 2001 and 2018, which encompass more than 1.5 million deaths. For each person, we observe the date, cause and place of death. For each hospital, we also collect a host of inputs and procedures, such as the number of medical beds, the number of surgeries, and hours of use of the operation room, among others. These data come from the REMs ("Resúmenes Estadísticos Mensuales") collected by the Ministry of Health, starting in 2009. We complement the data with a set of characteristics describing the hospital, such as hospital size, whether it is public or not, and location, among others.

Finally, to compute the timing of the policy, we draw on data of all the recruitment processes run by the Civil Service, which are publicly available on their website. The information includes

the recruited individual's identity, the appointment date, and the Ministry where the agency and the position are based.

2.4 Sample and Descriptive Statistics

We use records on the universe of public hospitals overseen by the network of Health Services and aggregate the data at the hospital-by-quarter level for the analysis. Aggregating the data for each hospital at the quarter level is useful to avoid observations with too few discharges and to reduce volatility in the data, but results are robust to alternative aggregations of the data. We start by constructing death indicators at the patient level following a hospital event. We merge the inpatient and death records, regardless of whether they occurred in the hospital or at another location. It is important to observe the effect on deaths outside the hospital in the analysis because in-hospital deaths could miss patients who die shortly after discharge (Gaynor et al., 2013). We construct the hospital mortality rate as the share of inpatients that either died in the hospital or died outside the hospital 28 days after admission. Since we can follow individuals over time, we also compute death rates for different time horizons after discharge, which will be useful to perform benchmark comparisons of our results with the literature.

Our final sample consists of 188 public hospitals, of which 88 adopted the recruitment reform at some point between 2004 and 2019, for a total of 13,988 observations of hospitals-by-quarter. Table 1 presents descriptive statistics. In our sample, 33 and 15 percent of hospitals are classified as high- and medium-complexity hospitals, respectively. The average hospital in our sample discharges 1,491 patients per quarter, while the median hospital discharges 587 patients. On average, 59% of these discharges correspond to female inpatients, and 36% of them to inpatients younger than 29 years old. Importantly, 96% of the patients discharged from public hospitals have public insurance. Regarding hospital outcomes, the average hospital experiences 38 deaths per quarter with a corresponding in-hospital death rate of 2.46%. Relatedly, the 28-days from admission death rate— which considers both in- and out-of hospital deaths—is larger and corresponds to 4.21%. Regarding emergency room admissions, the average death rate among ER patients is 3% when considering all diagnoses and 12.2% when considering only ER admissions with an acute myocardial infarction diagnosis.

3 Reform Impact on Hospital performance

3.1 Hospital Mortality as a Measure of Hospital Performance

Our main outcome of managerial performance is hospital mortality, which the literature uses extensively to measure outcome-based hospital quality in different settings (e.g., Geweke et al., 2003; Gaynor et al., 2013; Bloom et al., 2015; Doyle et al., 2015; Hull, 2020; Gupta, 2021; Chan et al., 2022). A critical concern, however, is that hospital death rates might reflect shifts in the observed and unobserved characteristics of patients, potentially biasing the results in the analysis.

As discussed in Section 2, the Chilean public health setting is well suited for this analysis because selection of patients is limited by the institutional design. Public hospitals receive patients following strict referral guidelines based on the county of residence, age, and diagnosis of the patient. Also, hospitals cannot reject patients or discretionally counter-refer them to other hospitals and have to abide by the existing protocols.[10] We provide further details in Appendix A.

To ease selection concerns, we also present the results of the reform on the risk-adjusted mortality rate following the prediction procedure used by the Centers for Medicare and Medicaid Services (CMS) in the United States (Ash et al., 2012). According to recent research leveraging quasi-random variation on death rates, this risk-adjustment procedure provides mortality outcomes that are reliable and valid indicators of hospital quality in the United States, where the institutional setting is prone to patient selection (Doyle et al., 2019). We describe the risk-adjustment procedure in Appendix B.

3.2 Research Design: Reform Adoption in Public Hospitals

Public hospitals that adopted the selection reform differ systematically from those that did not. However, the growth of a set of variables before the reform was passed is not clearly correlated with whether the hospital adopted the reform or not. Also, the listed variables are not predictive of the timing of adoption of the reform. These features allow us to use the adoption as a plausible source of exogenous variation to estimate the impact of the reform on performance outcomes.

We compare the characteristics of hospitals that adopted the selection reform at some point (ever treated) to the characteristics of the hospitals that never adopted it (never treated). Naturally,

[10] It might be contested that CEOs could change the referral protocols in their hospitals to avoid sicker patients. However, the referral and counter-referral system for each hospital is set and revised by the Health Service where the hospital is based and is approved by Subsecretaría de Redes Asistenciales.

for this exercise we focus on predetermined characteristics before the first hospital adopted the new selection process in 2005. Column (1) in Table 2 shows the average for a set of variables related to the demographics and outcomes of patients at never adopter hospitals. Column (2) presents the OLS estimate on a dummy that takes value 1 for ever treated hospitals and 0 otherwise. We find that, on average, adopters have higher death rates and served patients that are slightly younger and less likely to use public health insurance (93% vs. 96%); they are also located in municipalities that exhibited more support for right-wing politicians in the 2004 mayor election.

To assess whether adopting the reform is associated to hospital characteristics trending differently (e.g., hospitals that are performing better over time are more likely to adopt the new recruitment system), column (3) presents the OLS coefficients of a regression of the first difference of each characteristic on a dummy that takes value 1 for ever treated hospitals and 0 otherwise. Except for small pre-reform increases in terms of death rate, the share of patients between ages 60 to 69, and the share of patients with public insurance, adoption does not seem strongly correlated with predetermined hospital characteristics. Indeed, the number of deaths, the death rate in the emergency room, the death rate 28-days from admission (in- and out-of hospital), the death rate associated with acute myocardial infarction, and the ratio of the actual over the predicted death rate do not seem to be trending differently among adopters. Finally, to examine the timing of adoption among adopters, we rank all public hospitals by their adoption date and estimate an ordered logit model of this ranking on all listed variables. Column (4) in Table 2 shows the goodness of fit of this model—as proxied by its pseudo-R^2—after the removal of each predetermined variable, one a time. Two things are worth noticing from this exercise. First, these variables do not have much explanatory power for the timing of adoption, i.e., pseudo-$R^2 < 0.1$. Second, the goodness of fit of the model is not sensitive to the removal of any of these variables.

3.3 Main Results

We begin by estimating the following staggered difference-in-differences model:

$$y_{ht} = \alpha_h + \gamma_t + \beta \times Reform_{ht} + X'_{ht}\Delta + \epsilon_{ht}, \tag{1}$$

where y_{ht} is an outcome variable at the hospital h and time t level, $Reform_{ht}$ is a dummy variable that takes value 1 if a hospital adopts the new selection process and 0 otherwise. Recall that once a position selects their manager via the new recruitment system, it has to select all future managers using the same recruitment system. Thus, the adoption of the recruitment reform is an absorbing treatment and the dummy variable takes the value 1 for all periods after the first manager is hired

under the new regime. The control group consists of yet-to-be treated and never-treated hospitals. α_h represent hospital fixed effects that control for unobservables specific to the hospital and γ_t are time fixed effects to account for unobservable shocks specific to a quarter-by-year.

To account for potential selection based on patient characteristics, we follow Propper and Van Reenen (2010) and Gaynor et al. (2013) and include X_{ht}, a comprehensive set of hospital-level variables that pick up differences in case mix characteristics. Specifically, the vector X_{ht} includes the share of female inpatients, the share of inpatients within eight age bands, the share of inpatients within each of the 31 categories of the enhanced Elixhauser comorbidity index (Elixhauser et al., 1998; Quan et al., 2005), and the interaction of the aforementioned shares. To control by the socioeconomic status of patients, X_{ht} also includes the share of inpatients with each type of health insurance. We cluster standard errors at the hospital level, which is the treatment level unit. The coefficient of interest is β, and it summarizes the impact of the reform on hospital quality.

For estimation, we consider the universe of public hospitals and weight each regression by the number of inpatients as of 2005.[11] In Table 3 we report the $\hat{\beta}$ obtained from estimating equation 1 using different death-related measures of hospital performance. Columns (1)-(3) consider the logged in-hospital death rate. Column (1) shows that reform adoption led to a 13% decrease in in-hospital death rates, and columns (2)-(3) confirm that the result is robust to adding the set of case mix characteristics discussed above, either separately or interacted. Column (4) considers the log of the death rate 28-days from admission. Importantly, this measure includes both in and out of hospital deaths. Reassuringly, the point estimate shows that reform adoption led to a 10% decrease of 28-days from admission death rates. Columns (5) and (6) of Table 3 focus on emergency admissions. Column (5) shows that death rate among emergent cases decreased by 15%, while column (6) shows that death rate among emergent cases with acute myocardial infarctions (AMI, commonly known as "heart attacks") decreased by around 27%, although this coefficient is more imprecisely estimated.[12]

We also estimate the impact of the reform on the actual over the predicted death rate, following the prediction procedure by the CMS. We first fit a logit model for the outcome of death using the set of case mix controls and more than 5.5 million patient-level observations from 2001 to 2004. Then, we use the model's predicted death probability for each patient (based on patient case mix)

[11] For those hospitals that had a CEO turnover, we include a window of six quarters before and twelve quarters after the reform adoption to facilitate the study of the timing of the effect.

[12] Note that the number of observations drops from 8,104 to 771. Following the literature, we define AMI deaths as deaths that occurred 28-days from admission of patients (through the emergency room) with ICD 10 diagnosis of I21 (Acute myocardial infarction) or I22 (Subsequent myocardial infarction). For estimation, we weight this regression by the number of inpatients with emergency room AMI admission, as of 2005.

to obtain "predicted" death rates at the hospital level. Finally, to ease the interpretation, and following the English National Health Service (Health and Centre, 2015) we construct a "risk-adjusted mortality rate" dividing the actual hospital-level death rate by the predicted death rate; such that an increase (decrease) from one means more (less) deaths than predicted deaths. Column (7) presents the results. We find again that the policy had a significant effect on hospital performance. After the new CEO selection process was adopted, the ratio of actual over predicted death rates decreased by 9% from a base of 0.74 in our estimation sample.[13] Finally, in column (8) we use a Poisson model to estimate the effect of the policy on the number of deaths. Reassuringly, we find that the reform decreased deaths by around 5.7% (i.e., $\exp(\hat{\beta}) - 1$, where $\exp(\hat{\beta})$ is the incidence rate ratio of deaths).

3.4 Validity of Results and Alternative Explanations

In this subsection, we discuss the validity of the results presented above. We first present event study evidence that provides visual support to the assumption of parallel trends. Next, we discuss whether patient selection could be driving our results. Finally, we explore whether CEO transitions have per-se a mechanical effect on hospital quality.

3.4.1 Testing for Parallel Trends: Event Study Evidence

We start by assessing the existence of pre-trends. The concern here is that hospitals adopting the selection reform might be on different trends to those that have not adopted it, which could be biasing our results. To partially assess the validity of the underlying parallel trends assumption, we estimate the following event study:

$$y_{ht} = \alpha_h + \gamma_t + \sum_{k=-6}^{12} \beta_k D_{ht}^k + X_{ht}'\Delta + \epsilon_{ht}, \tag{2}$$

where D_{ht}^k is a dummy variable indicating the reform was adopted k periods ago (or will be adopted k periods ahead for negative values of k). Reform adoption is an absorbing treatment. The β_k coefficients can be interpreted as the effect of the reform on hospital quality for each k month, relative to the date before the adoption. We normalize the coefficients such that $\beta_{k=-1} = 0$, and we consider a window of 6 quarters before and 12 quarters after adoption.

Figure 2 displays the point estimates of our β_k and their confidence intervals for different mea-

[13] In Appendix B we provide additional robustness checks following alternative risk-adjustment procedures.

sures of hospital-level death rates. When inspecting the dynamic effects of the reform adoption, we observe that—across all panels—the pre-period estimates tend to be small, around zero, and not significant, indicating that treated and control units were not on different trends prior to the reform adoption. Furthermore, after the reform, the estimates turn negative and significant and grow gradually. In this case, it does not seem that the change in management is driven by a previous worsening or improvement in managerial performance, which would overestimate (underestimate) the true impact (if any) of the treatment. In Appendix Figure A.5, we present robustness checks and plot the impact of the reform on the count of in-hospital deaths using a dynamic Poisson model (Panel A), and on the logged ratio of actual over predicted death rates using a two-way fixed effects model (Panel B). Finally, in Appendix Figure A.6 we present the estimation results using the models suggested in De Chaisemartin and d'Haultfoeuille (2020) and Borusyak et al. (2022), which are robust even if the treatment effects are heterogeneous over time or across groups. Reassuringly, results are robust and follow the same dynamic trajectory regardless of the estimation strategy.

3.4.2 Testing for Patient Selection

The risk-adjustment procedure is fundamentally based on the patient diagnoses, which raises three potential concerns. First, new managers may have incentives to influence the diagnoses for billing or revenue purposes (Silverman and Skinner, 2004). Second, new managers may reject sicker patients based on the severity of their illness. Finally, there could be substantial variation in diagnostic practices across doctors and regions unrelated to patients' characteristics.[14] If, for example, managers bring in new doctors who, in turn, have systematic differences in diagnostic practices, our results could be explained by a mechanical effect of doctor composition.

A careful consideration of our setting's characteristics suggests the first two concerns are unlikely to drive our results. On the one hand, the diagnoses in our data come from a nationwide mandatory program that aims to characterize the morbidity profile of patients for policy purposes and are recorded directly by the lead physician (Decreto 1671 Exento, 2010). Therefore, there is no clear way how the hospital CEO could manipulate the diagnoses. On the other hand, the law forbids the CEO to select patients based on their condition and have to stick to the referral and counter-referral guidelines. We can, furthermore, empirically assess these three concerns, by examining whether the diagnoses composition at the hospital level changed after adopting the new CEO recruitment process is adopted. For this purpose, we estimate equation 1, but now replacing the dependent variable with the share of patients with each of the 31 categories of the enhanced

[14] See, for example, Song et al. (2010) and Finkelstein et al. (2017), who document and discuss this phenomenon in the United States.

Elixhauser comorbidity index. Figure 3 plots the t-stat of the difference-in-difference coefficient associated with each of these regressions. Reassuringly, we do not find strong evidence of impact of the reform on patient diagnoses.

Although our results are robust to adding case mix controls and using risk-adjusted mortality measures, there could be unobserved selection that is not picked up by the diagnoses data or by the list of available patient characteristics. Perhaps managers are able to reject sicker patients in a way that does not change patient composition (supply-side unobserved selection), or healthier patients are more likely to enroll in public insurance if they observe that public hospital performance is improving (demand-side unobserved non-random sorting).

To indirectly test if supply-side unobserved selection is overestimating our results, we consider the impact of the reform on mortality rates in other nearby hospitals and deaths at home. To the extent that rejected patients die, they would still show up in the mortality statistics of the hospital's geographical area. For this exercise, we estimate equation 1 again but now using as dependent variables the at-home death rate (in the municipality where each hospital is located) and the in-hospital death rate in neighboring hospitals. Panel A in Figure 4 shows our results and the baseline estimates as a reference. We find that adopting the reform in a given hospital has no significant impact on at-home death rates in the hospital's municipality or on the death rates of neighboring hospitals.

Finally, to explore if unobserved sorting is biasing our results, we exploit the fact that in our setting, a set of individuals are locked-in in the public health network. The reason is that under public insurance, lower-income individuals receive insurance for free but cannot use the insurance in private providers. For this analysis, we estimate equation 2 using a smaller sample comprised of locked-in patients only. The results from this approach—which should mute demand-side sorting if any—are presented in Figure 4, Panel B. We find that the impact of adopting the new CEO selection process is also present, and even larger, in this restricted sample.

3.4.3 Manager transition Mechanical Effect

We next examine the extent to which there is a mechanical effect on death rates due to the CEO transition itself. For instance, an alternative explanation to our results could be that the decline in the death rate reflects the effect of the arrival of a new manager, by means of a Hawthorne effect (Acemoglu et al., 2022).

Exploring this mechanism requires slightly modifying our empirical strategy since all hospitals have several transitions in the examination period. To deal with multiple events and the lack of

clean controls, we perform a stacked event study (Cengiz et al., 2019; Baker et al., 2022; Atal et al., 2022). We define an event as a CEO transition in a never treated or yet-to-be-treated hospital in any quarter between 2001 and 2019. For each transition event, we define a time window around it and a control group of hospitals with no transitions in the time window.[15] Next, we define a set of valid events as those that are balanced in the time window and do not overlap with another transition in the pre-period within the time window. Finally, we append the data for all valid events and estimate the following equation:

$$y_{hte} = \alpha_{he} + \gamma_{te} + \sum_{k=-3}^{7} \beta_k D_{hte}^k + \epsilon_{hte}, \tag{3}$$

where e is a valid transition event. Equation 3 is analogous to Equation 2, but the observation is at the hospital-by-time-by-event and replaces the hospital and time fixed effects with hospital-by-event and time-by-event fixed effects. We cluster standard errors at the hospital level.

Figure 5 presents the effect of a CEO transition on death rates. Reassuringly, the effect is a precisely estimated zero and confirms that a CEO transition before the reform has no significant effect on hospital quality. This evidence suggests that the impacts of the recruitment reform reported so far are not explained by a mechanical effect driven by CEO transition itself.

3.5 CEO Selection Reform in the Context of Other Policies

We conclude this section by benchmarking our results to the effects of other policies studied in the literature. One of the advantages of our data is that we can check the impact of the policy on different samples of patients, which allows us to match some of the characteristics in the sample of patients studied elsewhere. For each comparison, we present the average death rate in the sample in literature and in ours after we match it according to the patients' characteristics. Note, however, that although we can match the sample of patients in, say, age brackets type of admission, the patient composition will still differ across settings. Comparisons should serve as a benchmark and not as a horserace competition between policies. Results are summarized in Table 4.

We start by comparing the effect of the CEO selection reform with the impact of increasing health spending. Doyle et al. (2015) examine the effect of increasing the number of hospitals receiving higher payments from Medicare. They find that a 10% increase in Medicare reimbursement

[15]Note that there is a trade-off between the length of the window and the number of events and controls. We use 4 quarters priors to the transition and 12 quarters after the transition, although the results are robust to other time windows.

per capita decreases death rates by 6%. Their sample of patients includes emergency admissions coming from an ambulance, over 65 years old, and with non-deferrable medical conditions. Since we do not have records on whether a patient is coming from an ambulance, we only compute the effect of our policy in the sample of patients over 65 admitted via ER. We find a similar effect over a very similar average death rate in the sample.

As a second comparison, we consider recent evidence on the impact of public vs. private provision of healthcare. Chan et al. (2022) study the case of VA hospitals in the US and find that public provision reduces 1-year mortality by 7.7% in veterans over 65 years old admitted from an ambulance. We find a similar effect in the sample of emergency admissions over 65 years old. As mentioned before, we cannot see whether a patient is coming from an ambulance. Nonetheless, we find a very similar effect size over a very similar average death rate.

Finally, we focus on policies related to the impact of increasing competition in the health sector. Bloom et al. (2015) examine the effect of adding competition of health providers in the UK. They find that adding one extra hospital in the neighborhood decreases the in-hospital 28-day death rate by 10% following emergency admissions for AMI. The policy we study in this paper finds a similar effect, although over a higher average death rate in the same sample group. Previous work by Gaynor et al. (2013) had also reported that increasing competition by a 10%, as measured by a decrease in the Herfindahl-Hirschman Index (HHI), reduces the 28-day in-hospital death rate by 1% and the overall death rate by 20%. In this regard, improving the CEO selection has a comparable effect of 15 and 20% reduction in deaths rates, but over a much larger sample mean.

4 Reform's Impact on CEO characteristics

The reform changed the recruitment process for hospital CEOs. In this section, we examine how the reform changed the characteristics of new CEOs relative to their counterfactual in the absence of the policy.

4.1 Main results

To explore the effects of the policy on manager characteristics, we use the same research design as before but replace the independent variable with manager-specific characteristics. Concretely, we estimate Equation 1 on $X_{M(h,t)}$, where X are an individual-specific traits such as management studies, cognitive ability and demographics, and $M(h,t)$ is a function indicating the identity of the

CEO of hospital h at time t.

We measure management studies using two complementary variables. First, we construct a variable that takes the value 1 if the individual has an undergraduate degree with management coursework and 0 otherwise. We consider the following majors to include management courses: public administration, business and economics, accounting, and engineering. The second variable relates to postgraduate education in management. Again, this variable takes the value 1 if an individual holds postgraduate management studies and 0 otherwise. Postgraduate management studies include master's and diplomas related to management and administration. For example, master's include master in public health administration, master in public administration, and master of business administration, among others. We refer to cognitive ability as the performance in the standardized national university entrance exam. For demographics, we compute age and gender.

Column (1) of Table 5 considers the impact on the likelihood that the new CEO holds undergraduate studies in management. The reform increased the share of CEOs with undergraduate management degrees by 21 percentage points, from a baseline of 5%.[16] In terms of management postgraduate studies, column (2) in Table 5 shows an increase of 33 percentage points, from a baseline of 24%. Column (3) summarizes the effect of holding any management degree and finds that the policy increased the likelihood of the manager having *any* management studies by 37 percentage points. Since there is an important correlation between holding management undergraduate and postgraduate studies, in column (4) we present the effect on postgraduate studies, conditional on the hospital having a doctor CEO. We find that the policy not only increased the number of non-doctor CEOs, but also more than duplicated the number doctor CEOs that have postgraduate management studies (from a 20% in the baseline, it increased by 22 percentage points).

Figure 6 complements these findings. Panel A shows that the increase in the number of CEOs with an undergraduate management degree came at the expense of displacing almost one-to-one doctor CEOs, who before the policy adoption made up almost 90% of CEO positions. The figure also shows that the reform did not impact CEOs with other degrees, such as health professionals other than doctors and professionals in other areas (lawyers, and sociologists, among others). Panel B shows that the displacement of doctors masks heterogeneous effects. In fact, the policy increased the number of doctor CEOs with management studies while substantially decreasing the number of doctor CEOs without management studies by almost 30 percentage points from a baseline of 72%.[17]

[16] Since the timing of adoption varies across hospitals, we compute the baseline in the period before each hospital adopted the reform.

[17] One limitation of this analysis is that, due to the lack of data, we neglect heterogeneities in management experi-

In columns (5)-(7) in Table 5 we focus on cognitive skills. Column (5) shows that the new managers performed slightly worse in college entrance exams, although this difference is not significant. To secure a position in medical school, individuals need to achieve top scores in college entrance exams. Since the new managers are displacing doctor CEOs, column (5) result implies that, on average new managers are also top performers in college entrance exams. In columns (6) and (7), we focus on the set of CEOs who took the older version of the college entrance exam in Chile, where applicants had to choose which *specific* exam to take. We find that new managers are more likely to take the math-specific exam and less likely to take the science exam. This finding is consistent with the results in column (1).

Finally, we focus on demographics in columns (8) and (9). We find that the new managers are on average almost two years younger than the CEO would have been in the absence of the policy. One interesting finding is that the reform did not have any impact on female participation in CEO positions. The average pre-policy share of female CEOs is 22%, which is in line with the widely documented under-representation of female CEOs in the private sector, a phenomenon known as the glass ceiling (Bertrand, 2018). We find that in this setting, the reform had no discernible effect on the likelihood of women making it to the top. This is also consistent with recent research showing that the application of non-written impartial hiring processes in the public sector does not have an effect on gender hiring disparities (Mocanu, 2022).

Anecdotal evidence from conversations with doctors working in the public sector points to the recruitment reform as a turning point in management in public hospitals. Before the reform, it was embedded in the culture of the public health sector that top management positions were reserved for high-status doctors with long careers in the institution. Overall, our findings are consistent with this anecdotal evidence: the reform displaced doctor CEOs in favor of slightly younger managers with a background in administration.

4.2 Event Study Evidence

One limitation of summarizing the policy's impact in a single estimate is that it misses dynamic effects if present. In this subsection, we explore whether there are strategic reactions from the group that was most affected by the policy, in this case, doctor CEOs without management studies.

Figure 7 presents the dynamic effects of the policy in a three-year window after adoption. Panel

ence for individuals without formal studies in management. Implicitly, by abstracting management experience from the analysis, assume that any management skill acquired in the job is firm-specific, while skills acquired from formal education are general management skills and are transferable across units (Frydman, 2019).

A focuses on the likelihood that the CEO has a management undergraduate degree or a medical degree. The reform increased CEOs with a management undergraduate degrees by around 25 percentage points after a year of the reform adoption, but the effect decreased over time to slightly less than 15 percentage points. The effect changes over time because CEOs' tenure is on average, shorter than three years, and therefore the effect is picking up the characteristics of more than one post-policy manager. In the case of doctors, we observe the opposite effect. After the initial post-policy quarters, there is a substantial displacement of around 20 percentage points. But by the end of the three-year window, doctors were able to revert their loss in the likelihood of securing a CEO position to only 10 percentage points.

Panel B in Figure 7 shows that within the pool of doctor CEOs (i.e., conditional on hospitals with doctor CEOs), the reform substantially increased the likelihood that doctor CEOs hold postgraduate studies related to management. By the end of the three-year window, the reform increased doctor CEOs with postgraduate studies related to management by almost 30 percentage points, from a pre-reform average of 16%. This finding allows us to explain the attenuation of the policy on doctor CEOs overtime and is consistent with the fact that the reform increased doctor CEOs with management studies, as shown in Figure 6, Panel B.

The increase in the share of doctor CEOs with management studies after the policy adoption is a combination of two phenomena. First, the reform likely increased the chances of being appointed as CEO for the pool of doctors that would have had management studies in the absence of the reform. But also, the policy incentivized doctors who wanted to be appointed as CEOs to pursue formal management studies in order to improve their competence and the likelihood of passing the recruitment process. The reason is that securing a CEO position is more likely if the candidate has management studies. As the reform was gradually adopted across the public health sector, management studies were also more demanded by doctors. This second explanation is consistent with the fact that most of the master's in management that doctor CEOs hold did not exist before the reform started in 2005 and only started as a response to the policy.[18]

5 Why do New Managers Matter?

In this section, we ask which factors can explain the effectiveness of the new managers. In particular, we examine the extent to which new managers are higher performers due to a better match

[18]See, for example, this news report as a case study: https://www.americaeconomia.com/articulos/notas/mba-en-salud-para-que-medicos-chilenos-entren-al-mundo-del-management.

between the skills demanded by the job and their skills. We next study to what extent the financial incentives introduced by the reform motivated CEOs to exert extra effort.[19]

5.1 Skills Mismatch

Several factors may create skill mismatches in public sector employees that may hinder performance.[20] In the case of public hospitals, the social norm before the reform was that CEO positions were reserved for doctors. The reform displaced doctor CEOs for professionals with undergraduate management degrees and also incentivized doctor CEOs to pursue postgraduate management studies, potentially improving the skill mismatch. We explore whether correcting the skill mismatch in this setting enhances the organization's performance.

Concretely, we refer to skill mismatch as the extent to which individuals are employed in an occupation unrelated to their main field of study. This phenomenon is known as horizontal mismatch, as opposed to vertical mismatch, where individuals have a higher or lower educational attainment than needed for their job. While there is a nascent literature studying horizontal mismatch in the private sector, to the best of our knowledge, there is limited to no research in the public sector (Nordin et al., 2010; Besley et al., 2022).

5.1.1 Interacted Difference-in-Differences Evidence

To examine whether CEOs with management studies perform better than those without, we interact the reform dummy in Equation 1 with a dummy that takes value 1 if the CEO has management studies and 0 otherwise. The working assumption is that CEOs with management studies are well matched, while the rest represent mismatches.

Columns (1)-(3) in Table 6 present the results. In column (1), we find that when the appointed CEO has management undergraduate studies, the policy has a larger effect than when she does not. The point estimate for matches is larger than the effect for mismatches, although the difference is not significant at standard confidence levels. In column (2), we use a less demanding definition of mismatch and compute the differential effects of the policy in the cases where the manager has *any* management studies, including undergraduate and postgraduate studies. We find that now the

[19] Another relevant margin is the degree of autonomy CEOs enjoy to run their organization. Empirical research shows that autonomy from the local authorities can lead to performance gains (Clark, 2009). Unfortunately, we do not have a design to test how higher autonomy interacts with competitive selection.

[20] For instance, a combination of low exit rates among public employees and technological change (Besley et al., 2022).

difference is starker and statistically significant at a 99% confidence level. In column (3), we focus only on the sample of CEOs who are doctors. The interaction captures the differential effect of the policy between doctor CEOs who have management studies and those who do not. We find that the policy had a significant effect in the cases where the appointed doctor CEOs had management studies and negligible effects otherwise.

5.1.2 Stacked Event Study Evidence from CEO Transitions

Another way to explore treatment effect heterogeneity based on management studies is to compare transitions from CEOs *without* any management studies to CEOs with management studies (as of the time of the transition). As before, management studies refer to undergraduate and postgraduate studies related to management. Concretely, we estimate a equation 3 in an stacked event study framework. An event is a CEO transition. We select valid events that are balanced in the time window and do not overlap with other transitions for at least four periods before the event. For each event, we define a time window around a transition event and a control group of hospitals with no transitions in the time window.[21] To avoid a mechanical correlation with the results presented in columns (2) and (3) in Table 6, we exclude all the CEO transitions that occurred the first time the selection reform was implemented in a given hospital. Finally, we append the data for all valid events and estimate an event study.[22]

Columnns (4)-(5) in Table 6 present the results. Columns (4) presents the 3-year effect of a CEO transitions without any management studies to a CEO with management studies. Columns (5) is placebo exercise that estimates the effect of transitions between CEOs *without* management studies. Consistent with the findings in the interacted difference-in-differences, we find that when the hospital transitions from a CEO without any management studies to a CEO that has management studies, the event is followed by a significant decrease in death rates. Reassuringly, we find no effect on hospital death rates when we explore transitions between CEOs without management studies. Both effects are also consistent with the evidence presented in columns (2) and (3) for the effect of the reform when the appointed manager had and did not have management studies.[23] This evidence suggests that hospitals transitioning to CEOs with management studies drive, for the

[21] As noted before, recall that there is a trade-off between the length of the time window and the number of valid transitions and control units

[22] Appendix Table A.3 presents the number of events by type of transition.

[23] Appendix Figure A.10 provides visual event study evidence of the effect of transitions from CEOs without management studies to CEOs with management studies. Importantly, we find no pre-trends and the same trajectory as the effect of the reform displayed in Figure 2. The lack of pre-trends suggests that the hospital's performance does not drive the change in the education of the CEO.

most part, the effect of the selection reform on hospital performance.

Interestingly, the finding that management studies improve CEO performance might come at odds with the results in Acemoglu et al. (2022), who show that managers with a business degree do not improve their firm performance and reduce their employees' wages by means of rent-sharing practices.[24] One key difference is that in our setting, business managers perform in the public sector, where they have less incentives to reduce their employee's wages and fewer means to do so given the rigidity of public sector wages. Furthermore, business CEOs who self-select into the public sector might have higher levels of prosocial motivation than those in the private sector (Finan et al., 2017).

5.2 Financial Incentives of the Reform

Low-powered incentives and low wages in the state are often pointed out as one source of inefficient performance of high-end public employees. For instance, recent empirical research show that financial incentives can increase performance of employees in the public sector (Khan et al., 2015; Biasi, 2021; Deserranno et al., 2022). Since the recruitment reform included both a change in the selection system of new managers and financial incentives, new managers could have improved their performance simply because they exerted more effort due to the newly introduced financial incentives. Below we explore this hypothesis for each component of the financial incentives separately and show that our results are not explained either by performance pay or by efficiency wages.

5.2.1 Results are not Driven by Performance Pay

According to performance-related pay models, performance pay incentives attract higher-ability workers and also induce them to exert greater effort (Lazear, 2000). In our setting, the head of the Health Service (i.e., the principal) writes down a performance contract in agreement with the hospital CEO (i.e., the agent) for a three-year period. At the end of each year, the CEO gets a final score based on the parameters written in the contract. The yearly wage is impacted by the

[24] In Appendix Table A.12 we show that the reform did not impact hospital employees' wages.

performance agreement according to the following schedule:

$$\text{Yearly Wage}_t = \begin{cases} 100\% & \text{if performance}_{t-1} \geq 95\% \\ 98.5\% & \text{if} \quad 65\% \leq \text{performance}_{t-1} < 95\% \\ 93\% & \text{if performance}_{t-1} < 65\%. \end{cases} \tag{4}$$

Two things are worth noting about the schedule in Equation 4. First, the wage of the first year is not affected by the schedule because it is based on the previous year's performance, and the performance pay penalty only affects years two and three of the agreement. Second, the reform introduces only a small penalty and no possibility of a wage increase. The maximum penalty is a 7% discount of the yearly wage.

We accessed all the available performance contracts of the first manager appointed after the reform adoption and their yearly performance scores.[25] Figure 8 presents the distribution of the three-year average performance score. Note that 60% of the distribution is above the 95% threshold to avoid any wage penalization. The rest of the mass is between 95 and 65%, which is the lowest threshold to avert a 7% wage penalty. No manager receives a score below the 65% performance threshold. This evidence suggests that the performance agreements were not binding, and most managers easily met performance goals. In Appendix C, we empirically analyze whether CEOs' scores on their performance pay measure predicted better managerial performance at the hospital. We find that managers with high and low performance scores where equally effective to improve hospital performance.

It is worth noting that the performance agreements included in the recruitment reform were poorly designed across the board and their lack of effectiveness is not specific to public hospitals. For example, in all government positions that used the recruitment system, less than 5% scored less than 80% in their performance scores in 2013 (CPPUC, 2013), and more than 90% achieved a 100% performance score in 2016 (CADP, 2017). The failure of this tool to be a useful management control has been addressed in several policy reports calling for its amendment (see, e.g., CPPUC, 2013; Barros et al., 2018). We conclude that in our setting performance pay is not likely to be a relevant driver of managerial productivity.

[25] Unfortunately, some of the oldest contracts and performance scores are lost, and the Civil Service has no available records. Out of 87 processes, we have performance data for 57 and we have access to 77 contracts.

5.2.2 Results are not Driven by Efficiency Wages

An alternative mechanism is that the results are driven by efficiency wages. According to this hypothesis, wages above their outside option create an incentive for managers to exert extra effort and can elicit productivity growth (Katz, 1986). If the reform bonus creates labor rents, then this mechanism might be at play.

We start by analyzing the reform bonus. The bonus consists of an increase in the base salary, which is defined for each position by the Ministry of Finance. We document the size of the reform bonus relative to the position's pre-reform pay in two ways. First, in Appendix Figure A.7 we present a box plot of the share of the quarterly remunerations that is explained by the reform wage bonus. The reform bonus explains on average, a 43% of the quarterly wage, and the middle 50% of the distribution is between 37 and 46%. In Appendix D, we present event study evidence of the reform's effect on wages once it is adopted for the first time in a hospital. On average, we find an effect of the same order of magnitude, albeit somewhat smaller. However, it is important to note that we do not observe the change in the CEOs' remuneration but in the position's remuneration. Hence the effect is a composition of mechanical changes in pay due to changes in the manager's identity and the pay increase.

To explore the potential effects of efficiency wages in this setting, we exploit a 2016 amendment to the law that created the recruitment reform (Ley 20,955). Among other things, the amendment changed the pay scheme in the following way. Before the amendment, all CEOs were paid according to the public employees' pay grade, regardless of their profession. After the modification, CEOs appointed after November 2016 can choose to be paid according to the medical pay laws instead of the public employees' pay grade *only if* they are doctors.[26] The medical pay law is much more generous than the public employees pay law. Therefore the amendment implied an increase in remunerations for doctor CEOs but not for CEOs with other backgrounds.

If the efficiency wage hypothesis is at play in this setting, we should expect that a wage increase is followed by an improvement in performance in hospitals where new managers are doctors *and* receive a pay boost. To study this question, we perform a stacked event study, where an event is a transition after November 2016, that uses the new selection system, and where the incoming CEO is a doctor. For each event, we define a time window around the transition and determine an event-specific control group that includes hospitals with no transition and units with transitions to professionals other than doctors. We select valid events that are balanced in the time window and

[26]More precisely, doctors can choose to be paid according to Law 19,664 instead of Law 18,834.

that do not overlap with other transitions one period before the event.[27] We then append the data for all valid events and estimate an event study following Equation 3.

Panels (a) and (b) in Figure 9 present the impact of the 2016 amendment on doctor CEO wages and hospital performance, respectively. As expected, the change in the regulation increased wages for incoming doctors CEOs. The effect is around a 10% quarterly wage increase. However, we do not observe any effect on death rates. In other words, the wage increase was not followed by an improvement in CEO performance. This finding suggests that the efficiency wage hypothesis is unlikely to play a substantial role in this setting. Therefore, we rule out this hypothesis as a significant driver of our main results.

All in all, this evidence suggests that financial incentives do not explain the performance improvement we observe after the reform selection introduction. It is important to note that, although we have ruled out that financial incentives play a role in managerial performance, this result is conditional on the selected CEO. The extra pay likely plays a role in the decision to apply. For instance, Dal Bó et al. (2013) show that higher pay for sector public positions attract more competent applicants. Unfortunately, we do not have a design to test this hypothesis because we do not observe the pool of applicants *before* the adoption of the recruitment reform in each hospital. It is an open question to what extent higher wages widen the pool of high-quality applicants in our setting, and through this mechanism, higher wages impact performance. For instance, it could be the case that appointed CEOs with a management studies would have been less likely to apply in the absence of the wage hike.

6 What are New Managers Doing Differently?

The reform displaced doctor CEOs and decreased the degree of skills mismatch in CEO positions. Now we ask what the new managers did differently that allowed them to steer their organization's performance. We focus on the efficient use of hospital resources and personnel practices.

6.1 Operating Room Efficiency

Operating rooms (ORs) are considered one of the most critical and expensive hospital resources. They are typically the largest cost center in hospitals and have been estimated to account for more

[27] As noted before, there is a trade-off between the length of the window and the number of valid events. In total, there are 33 events and 24 valid events.

than 40% of total expenses, most of which are associated with labor costs (Association et al., 2003; Denton et al., 2007; Guerriero and Guido, 2011). The efficient use of ORs is a highly complex operational and management problem and has drawn much attention from practitioners and researchers.[28] For instance, planning and scheduling need to consider ORs availability, matching the workload to medical staffing, the material resources required, and the availability of post-surgical recovery medical beds (Wang et al., 2021). Furthermore, OR planning and scheduling must incorporate the uncertainty in surgery duration and emergent admissions needing a surgical procedure (Latorre-Núñez et al., 2016).

Inefficient use of ORs is extremely costly for patients and can impact the overall hospital performance. Late starts or longer-than-expected surgeries trigger delays or rescheduling for patients next in line. In turn, to deal with surgeries that finish after their rostered times, the medical staff has to work overtime, which implies direct costs to the hospital and can lead to higher levels of burnout, medical errors, and patient dissatisfaction (Rogers et al., 2004; Denton et al., 2007; Stimpfel, 2012). The other main effect of inefficient use of ORs is that hospitals can treat fewer patients, and hence patients face longer waiting times (Durán et al., 2017). Importantly, management practices are a crucial lever for improving OR efficiency (see, e.g., He et al., 2012).

These reasons lead us to examine OR efficiency as one mechanism through which new managers improved hospital performance after the reform. Furthermore, according to a recent report by the National Productivity Commission in Chile, one of the key challenges in our setting is more efficient utilization of ORs, where there is a significant gap with international standards (CNEP, 2020). To examine the impact of the recruitment reform on OR efficiency, we run the same specification as in Equation 2 on OR capacity (i.e., the aggregate available number of hours of ORs) and OR utilization (i.e., the number of hours the ORs are used).[29]

Panel A in Figure 10 shows the effect of the reform on the ratio between OR utilization and capacity. We find that The reform, however, did change the number of hours the ORs are effectively used. Three years after the reform adoption, the number of OR hours of use increased by 25%. Although this number might seem big at face value, it is not even large enough to closing the gap between the average efficiency in high-complexity hospitals to the average in the NHS in the United Kingdom.[30] Panel B examines the other side of the coin of higher OR usage: the number

[28] See Hans and Vanberkel (2012) for a formal formulation of the problem.

[29] For some hospitals, the data only include reliable information at the yearly level. For this reason, we aggregate the data at the yearly level and perform the analysis at this level of aggregation. For completeness, in Appendix Figure A.11 we present the effect of the reform on mortality rates using this level of aggregation and starting from 2009, which is the first year with OR available data.

[30] Out of 9 hours of daily capacity, the average in a sample of high-complexity hospitals in Chile is 4.8 hours and

of surgeries performed. Reassuringly, we find that the number of surgical procedures increased by a similar magnitude to the utilization of the OR.

6.2 Personnel Turnover

Recent literature in personnel economics shows that better-managed firms recruit and retain workers with higher human capital (Bender et al., 2018). Following this strand of the literature, we examine the reform's effects on the tenure of high-quality doctors.

Panel B in Figure 10 shows that the reform reduced the turnover of doctors, the most scarce human resource in the hospital's production function. In Appendix Figure A.12 we examine whether higher wages explain the reduced turnover. We find no effect on personnel hourly wage, which is expected given that in the public sector, wages are rules-based. From anecdotal evidence from conversations with managers and doctors in the public sector, we posit that the reduced turnover rate might be explained by unobservable perks that the manager can negotiate with doctors.

7 An Alternative Source of Variation: Rotation of Managers

In this section, we explore an alternative source of variation to study the extent to which CEOs affect hospital quality and to assess the impact of the selection reform on managerial talent. We follow an approach similar to that in Fenizia (2022), and exploit the rotation of managers across hospitals to study their impact on hospital quality. Specifically, we consider the following model:

$$\text{Ln(death rate)}_{ht} = \alpha_h + \psi_{M(h,t)} + \gamma_t + X'_{ht}\Delta + u_{ht}, \tag{5}$$

where α_h are hospital fixed effects that capture time-invariant characteristics of the hospital (e.g., size and the type of procedures realized therein), $\psi_{M(h,t)}$ are CEO fixed effects, which capture managerial talent (specific to a given CEO) and which are assumed to be portable across hospitals. We also include time fixed effects γ_t to capture seasonal shocks to patients' health and health provision as well as case mix controls, X_{ht}, to account for differences in patient composition.

For estimation, we first identify the set of hospitals connected by managers' mobility (Abowd et al., 1999; Card et al., 2013) and define our main estimation sample consisting of 691 managers,

in the NHS is 6.4 hours (CNEP, 2020).

105 hospitals, and 27 connected sets given by 74 movers.[31] Then, we estimate equation 5 via constrained OLS and recover managers' fixed effects that can be compared *within* connected sets. In this setting, the main threats to identifying $\hat{\psi}_{M(h,t)}$ are the potential endogenous mobility of managers (if related to hospital-specific trends) and the potential failure of the additive separability assumption between the hospital and manager component (i.e., existence of match effects). We present and discuss evidence against these threats in Appendix E.

7.1 Do Managers Matter in the Public Sector?

To study the extent to which variation in hospital quality can be explained by managerial talent we follow the seminal work by Bertrand and Schoar (2003). Specifically, we compare the adjusted R^2 estimated from a regression of the logarithm of death rates on different sets of explanatory variables, including the set given by equation 5 which includes manager fixed effects. We report the results in Panel A of Table 7. Column (2) excludes hospital and CEO effects, column (3) adds hospital effects, and column (4) includes CEO effects. The adjusted R^2 increases from 0.42 in column (2) to 0.67 in column (3), implying that hospital effects account for substantial variation in the outcome. It further increases up to 0.76 in column (4) after inclusion of manager fixed effects, an increase of similar magnitude to that reported in and Bertrand and Schoar (2003) and Fenizia (2022). Formally, an F-test strongly rejects the null hypothesis that all the manager effects are zero (p-value=0.00).

While the above evidence suggests that CEOs explain a non-trivial share of the variation in death rates across hospitals, it is not clear how the manager and hospital effects compare. To formally assess the relative importance of hospital characteristics and CEO effects, we perform a variance decomposition. Following equation 5, the variance of log death rates after accounting for patient characteristics and time effects can be decomposed as:

$$\mathbb{V}(\text{Ln(death rate)}_{ht} - X'_{ht}\Delta - \gamma_t) = \underbrace{\mathbb{V}(\alpha_h)}_{\text{Hospital Effect}} + \underbrace{\mathbb{V}(\psi_{M(h,t)})}_{\text{CEO Effect}} + \underbrace{2\mathbb{C}(\alpha_h, \psi_{M(h,t)})}_{\text{Sorting}} + \underbrace{\mathbb{V}(u_{ht})}_{\text{Residual}}. \tag{6}$$

Panel B of Table 7 presents the magnitude of each term in equation 6, estimated within the largest connected set.[32] Since sampling error could bias the estimates in the presence of limited mobility, we correct the estimates following the procedure of Andrews et al. (2008). We find that

[31] To balance our sample, we require outgoing managers to be observed for at least 3 quarters before a change in leadership. Results are robust to not imposing this restriction.

[32] The largest conected set in our setting contains 1,975 observations: 195 managers, 29 hospitals, and 27 movers.

manager fixed effects explain around 39.2% of the variance in residual death rates, about as much as the permanent component associated with different hospitals (39.4%). Our results also show that the (bias-corrected) covariance between CEO and hospital effects is negative, which implies the most talented managers work at worse hospitals (i.e., there is negative assortative matching).

7.2 Did the CEO Selection Reform Improve Managerial Quality?

To connect this research design with the findings from the selection reform, we begin by regressing the estimated manager fixed effects on observable characteristics. These characteristics include: gender, age, age squared, and set of indicators for their educational attainment. Appendix Table A.5 presents our result. Female managers are on average more productive than their male counterparts and, interestingly, managerial talent is negatively correlated with experience. Moreover, the results suggest that management studies improve managerial performance. Although this is a purely correlational exercise, the results go in line with the findings we presented in Section 4.

Finally, we formally assess whether the CEO selection reform improved managerial quality. To do so, we estimate equation 2 again but now using CEO fixed effect (instead of death rates) as the dependent variable. Since managers' fixed effects can only be compared within connected sets, we also add connected-set fixed effects to this regression. Figure 11 displays the point estimates of our $\hat{\beta}_k$ and their confidence intervals. Reassuringly, we find that the reform significantly increased CEO quality, i.e., the average manager fixed effect decreased by around 0.8 after a hospital adopted the new selection process. Furthermore, when inspecting the dynamic effects of the reform adoption, we observe that the pre-period estimates tend to be small, around zero, and not significant; and that after the reform the estimates turn negative and significant and stabilize after six quarters.

8 Conclusion

In this paper, we study the extent to which CEOs in the public sector can improve their organization's performance. To this end, we leverage a reform in Chile that introduced a competitive recruitment process to hire top managers in senior executive positions. We leverage the staggered adoption of the reform and focus on public hospitals to examine the effect of the reform on hospital performance. We find that upon adoption, hospital mortality decreased between 9% and 14% in the 3 years post policy adoption.

We then ask how the reform affected the characteristics of newly appointed managers. We

find that the reform displaced older doctors in favor of younger CEOs with undergraduate degrees whose majors included management courses. The former partially reverted the policy's impact by studying master's and diplomas in management. We find that the reform had an effect for the most part when the appointed CEO had management studies. We then provide evidence suggesting that the financial incentives incorporated in the reform did not affect managerial performance.

We also discuss mechanisms through which new managers may have impacted their organization's performance. We find that the reform operates through more efficient use of medical resources and better personnel practices.

To conclude, we note that this reform shifted two different margins of personnel selection that could account for the results. First, conditional on the same pool of individuals willing to take the position, the removal of discretionary appointments is likely to improve the quality of the appointed manager. This mechanism is of particular importance in the case that "outsiders" are implicitly banned from top positions, as anecdotal evidence suggests was the case in our setting. Second, as discussed above, the extra pay likely plays a role by attracting higher-quality candidates to the pool of applicants. Disentangling these questions is a promising avenue for future research.

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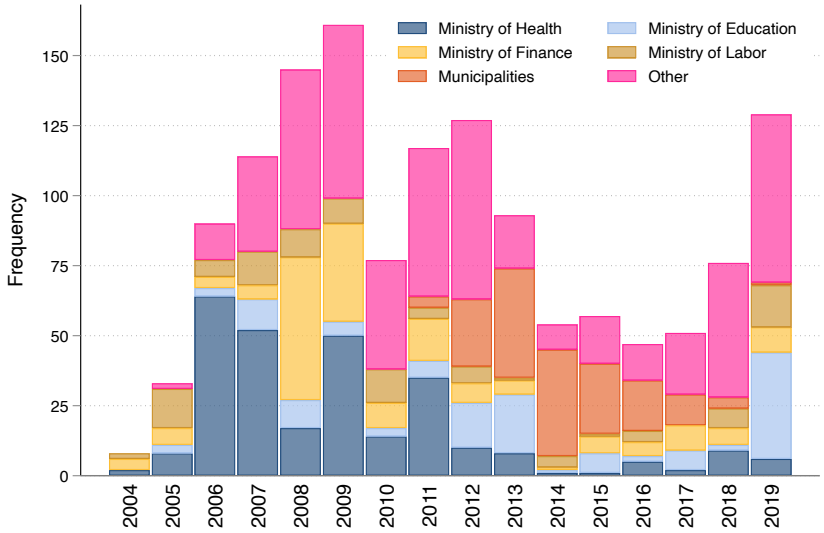
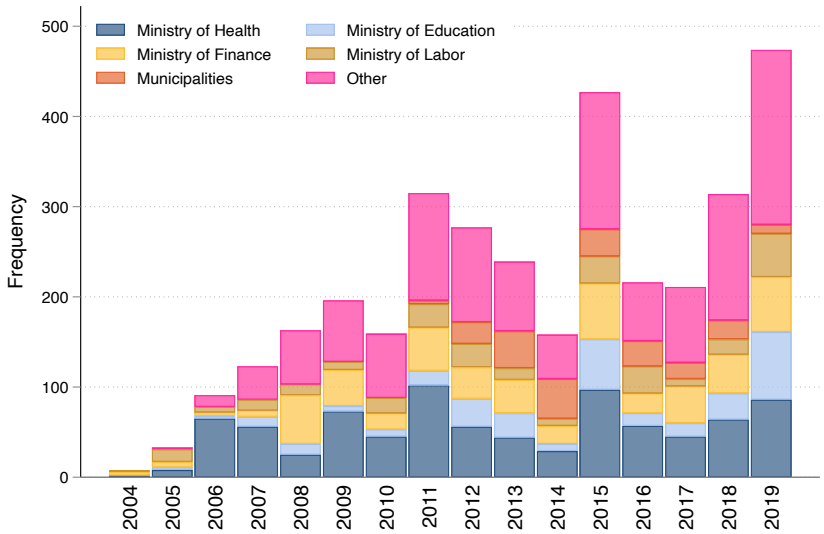
Figure 1: Adoption of the recruitment process in positions across government agencies



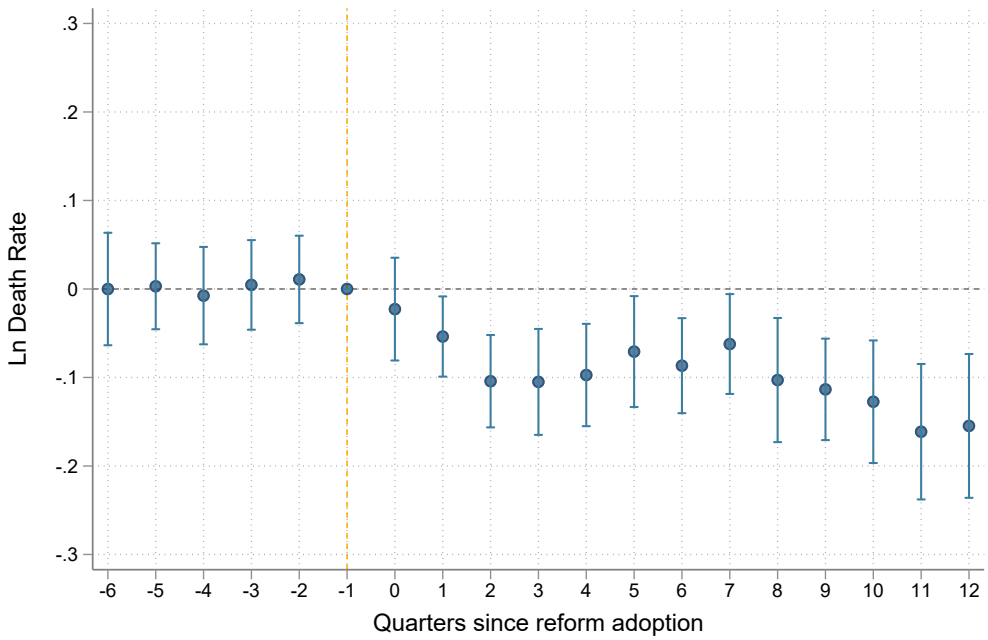
(a) Positions adopting selection process for first time



(b) Yearly recruitment processes run by the Civil Service

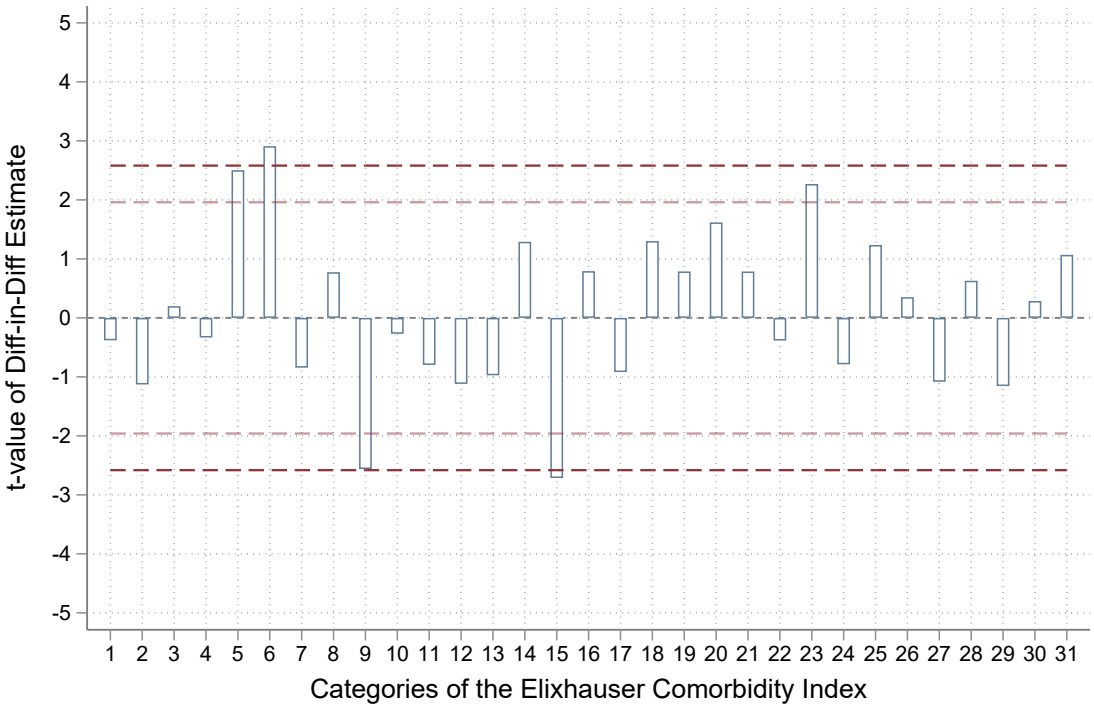
Notes: This figure displays the rollout of the selection reform across government agencies. Panel A presents the number of management positions that adopt the selection reform for the first time. After that, every new manager in that position has to be selected using this mechanism. All senior executive positions created after 2003 have to use the new selection system, and existing positions adopt it gradually. Panel B presents the number of selection processes that the Civil Service runs every year. We use the ending date of the process to allocate the process into a given year. Yearly observations include positions using the selection system for the first time and positions that had already adopted it in the past and are selecting a new manager. The spikes observed in 2011, 2015, and 2019 are evidence of substantial senior executive transitions after a new government is in place.

Figure 2: Dynamic effects of the reform on hospital quality



Notes: This figure presents event study evidence of the reform's effect on hospital deaths, following equation 2. The empirical analysis uses quarterly panel data of public hospitals in a time window comprehending 6 quarters before and 12 quarters after the reform was adopted by each hospital, and exploits the gradual adoption of the selection reform in public hospitals in that period. We do not impose a time window for hospitals that did not adopt the policy. Each dot corresponds to an estimated coefficient, and the vertical lines indicate the corresponding 95% confidence intervals. The yellow dashed line represents the omitted coefficient. Standard errors are clustered at the hospital level.

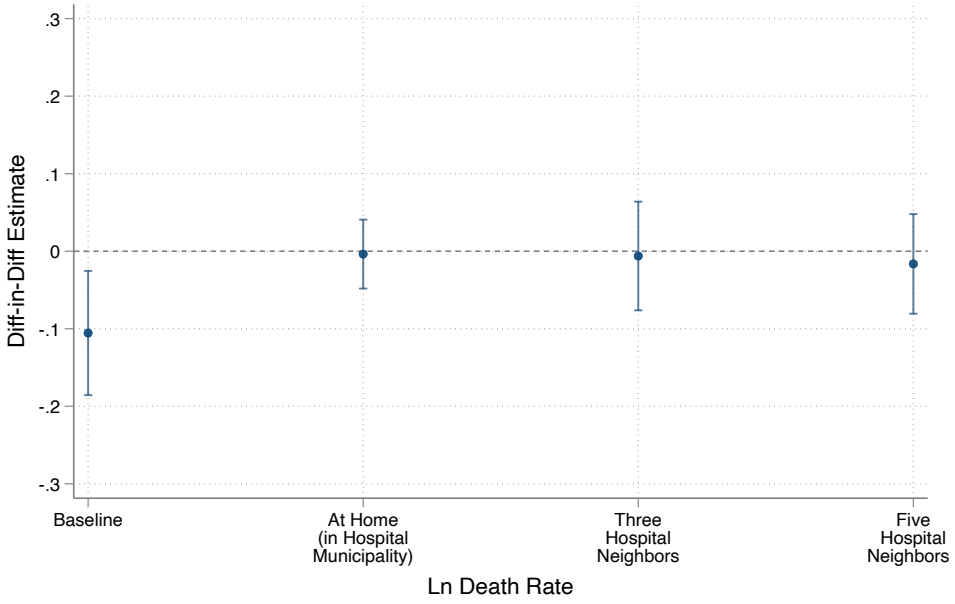
Figure 3: Testing for patient selection



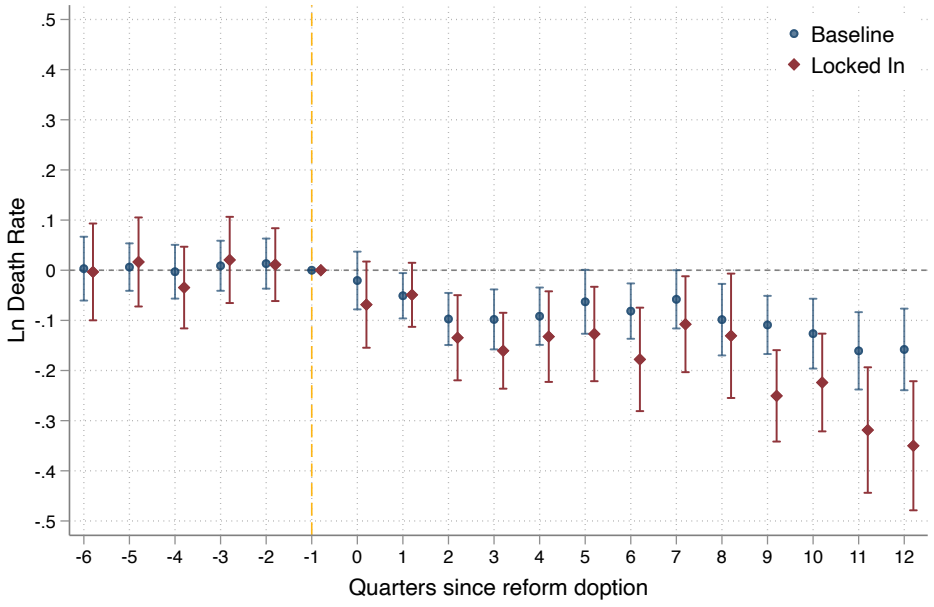
(a) Diagnoses Manipulation

Notes: This figure plots the t-stats of the difference-in-differences coefficients obtained from estimating equation 1 using the share of patients with each of the 31 categories of the enhanced Elixhauser comorbidity index (Elixhauser et al., 1998; Quan et al., 2005) as dependent variable. Dash red lines correspond to 95 and 99% tests critical t-values.

Figure 4: Testing for patient selection



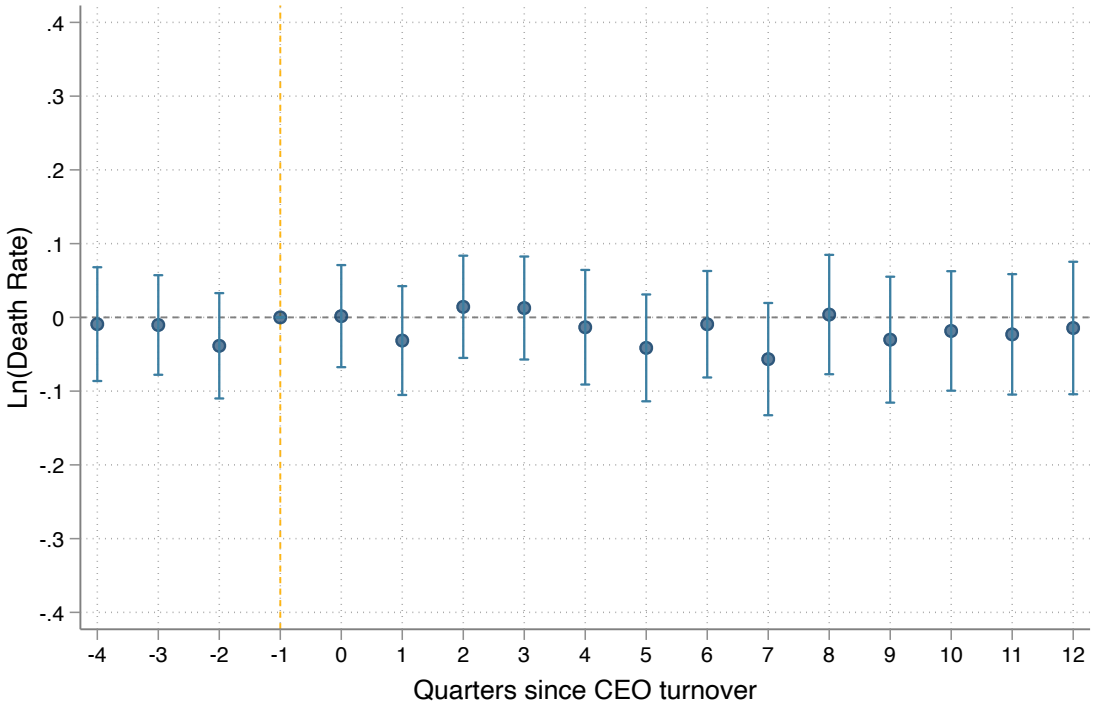
(a) Supply-side test



(b) Demand-side test

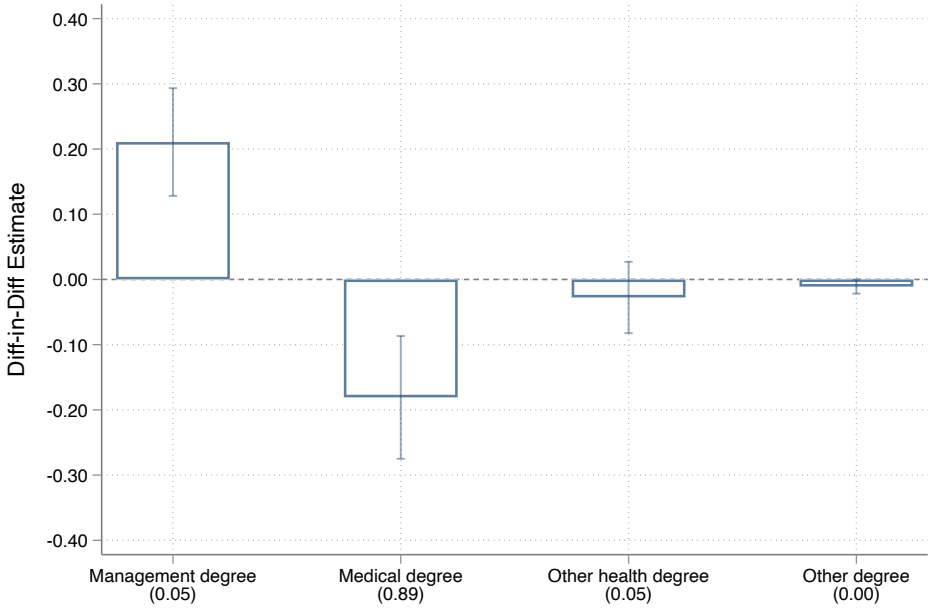
Notes: This figure presents evidence to assess patients' selection as a confounder of our main results. Panel A reports estimates and confidence intervals obtained from estimating equation 1 for the logged at-home death rate and for the logged death rates at close neighboring hospitals. Panel B presents event study evidence of the reform's effect on hospital deaths, following equation 2, but on a restricted sample of locked-in patients only. It also includes baseline estimates for a comparison. All regressions consider standard errors clustered at the hospital level.

Figure 5: Effect of CEO transition on death rates

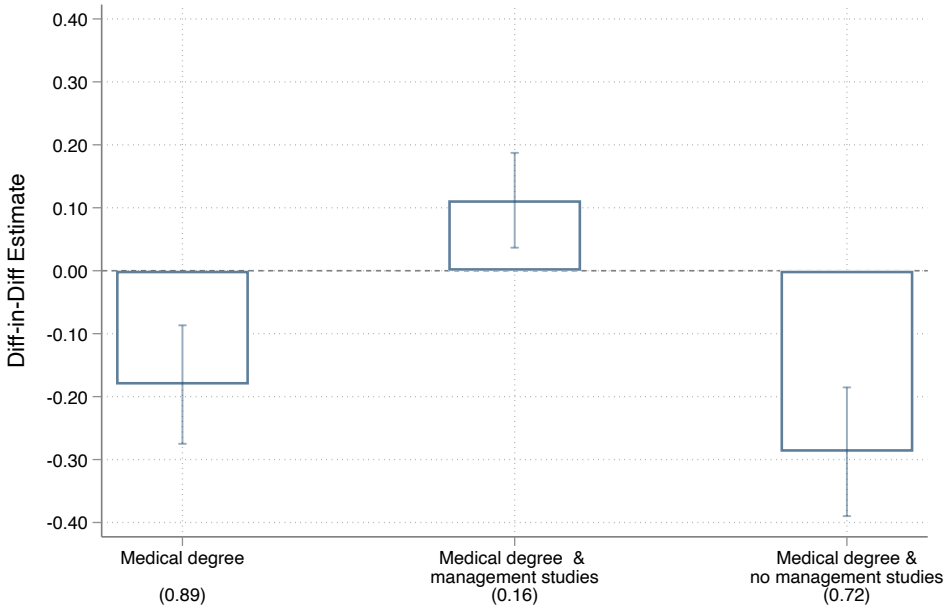


Notes: This figure presents the coefficients of the stacked event study specification in equation 3. An event is a CEO transition in a hospital that never adopts the new selection system. For each transition event, we define a time window around it and a control group of hospitals with no transitions in the time window. We define a set of valid events as those that are balanced in the time window and do not overlap with another transition in the pre-period within the time window. In total, there are 415 valid CEO transitions. The dependent variable is the death rate at the hospital level in a given quarter. The regression includes case mix controls. Dots indicate estimated coefficients and vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at the hospital level.

Figure 6: The policy displaced doctor CEOs with no management studies



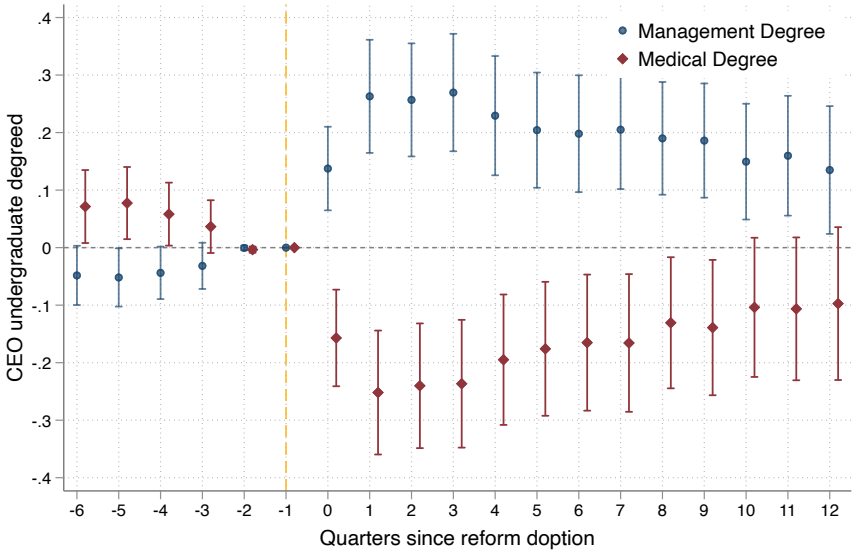
(a) All degrees



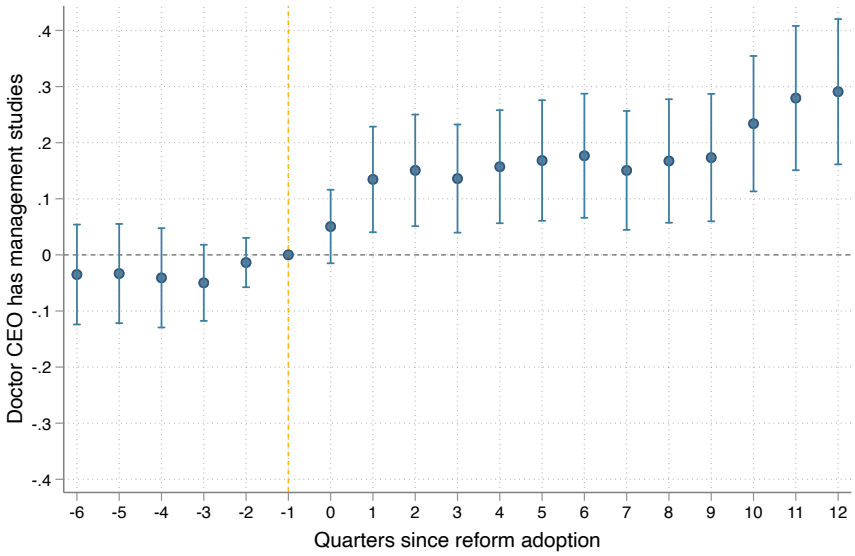
(b) Doctors

Notes: This figure presents the effect of the policy on CEO educational background. Panel A presents the average three-year effect of the reform on the likelihood that the CEO has an undergraduate management degree, a medical school degree, another health degree, or another major. All categories are mutually exclusive. Panel B focuses on doctors and perform separate estimations for doctors with and without management studies (as of the date of their appointment as CEOs). The bars represent the estimate from equation 1 on each outcome and the vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at the hospital level.

Figure 7: Dynamic effects on CEO educational background



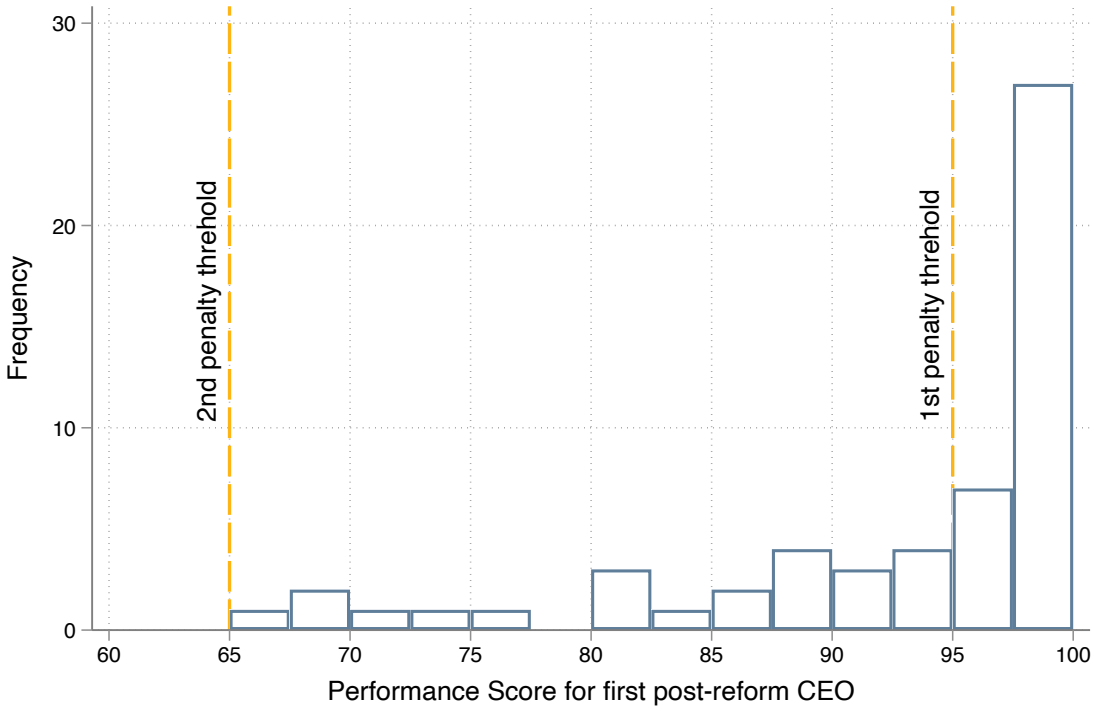
(a) Undergraduate degree



(b) Management studies for doctor CEOs

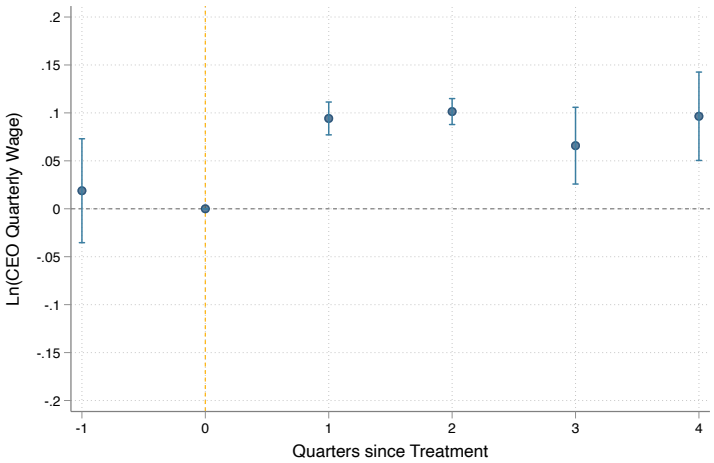
Notes: This figure presents event study evidence of the reform's effect on CEO educational background, following equation 2. In Panel A, the figure overlays the estimation of two dependent variables. The first is a dummy variable that takes value 1 if the CEO has a management-related undergraduate degree (in blue with dot markers). The second corresponds to a dummy variable that takes value 1 if the CEO has a medical degree (in red with diamond markers). Panel B examines the effect of the policy on whether the CEO has a master's or a diploma in management (as of the date of their appointment as CEOs), conditional on the hospital having a doctor CEO. The dots indicate estimated coefficients. The vertical lines indicate the corresponding 95% confidence intervals. The yellow dashed line represents the omitted coefficient. Standard errors are clustered at the hospital level.

Figure 8: Distribution of Performance Scores for Post-Reform CEOs

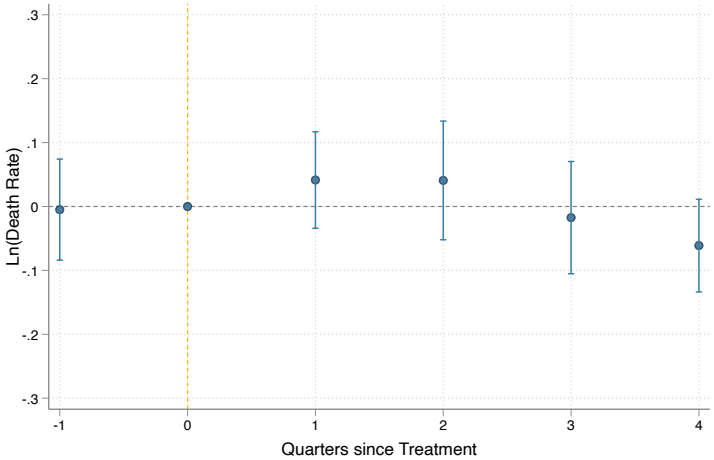


Notes: This figure displays the average performance scores for the first post-reform CEO. Before the reform, managerial performance did not affect the wage schedule. After the reform, CEOs face wage penalties if they perform below specific performance thresholds. Performance scores are computed following a three-year performance contract that the CEO defines with her superior. We accessed all the available performance contracts and the yearly performance scores. Unfortunately, some of the oldest contracts and performance scores are lost, and the Civil Service has no available records. Out of the 87 CEOs hired for the first time under the new selection system, we have performance scores for at least one year for 57 CEOs. An observation is the average of all available scores for a CEO in her three-year contract. The yellow dashed lines represent the wage penalty thresholds described in equation 4. Managers who score below the first penalty threshold had to pay a penalty equal to 1.5% of their annual wage. Below the second threshold, the penalty is 7% of their yearly wage.

Figure 9: Do efficiency wages impact death rates?



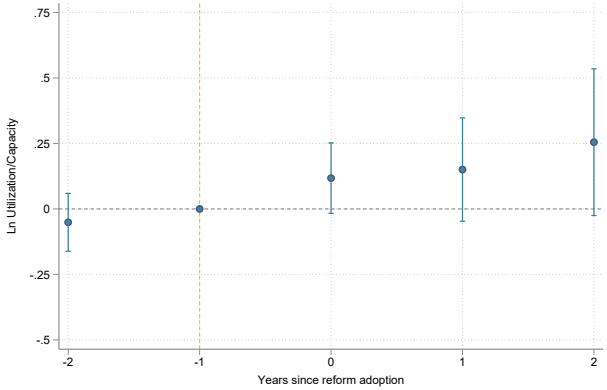
(a) Effect on Wages



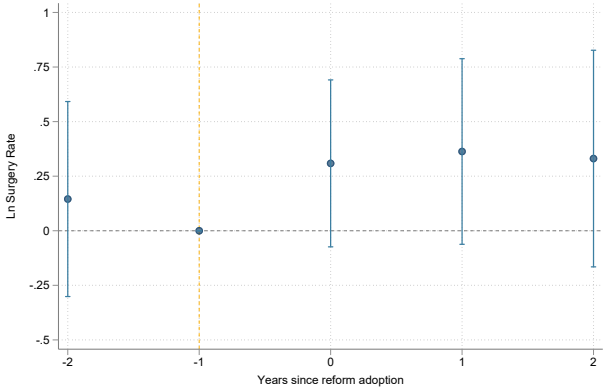
(b) Effect on Death Rates

Notes: This figure explores the impact of higher hospital CEO wages on hospital performance. The empirical design exploits an amendment to the recruitment reform, which increased wages for CEOs *only if* they are doctors *and* were appointed using the selection reform after November 2016. For each event, we define a time window around the transition and determine an event-specific control group that includes hospitals with no transition and units with transitions to professionals other than doctors. We select valid events that are balanced in the time window and that do not overlap with other transitions one period before the event. There are a total of 24 valid events. We then append the data for all valid events and estimate an event study following equation 3. Panel A presents the estimates of the amendment's effect on CEOs' wages, and Panel B displays the impacts on death rates. The regression on death rates includes include case mix controls. Dots indicate estimated coefficients, and vertical lines indicate the corresponding 95% confidence intervals. In Panel A, we cluster standard errors at the CEO's professional degree, which is the treatment unit. In Panel B, we cluster standard errors at the hospital level.

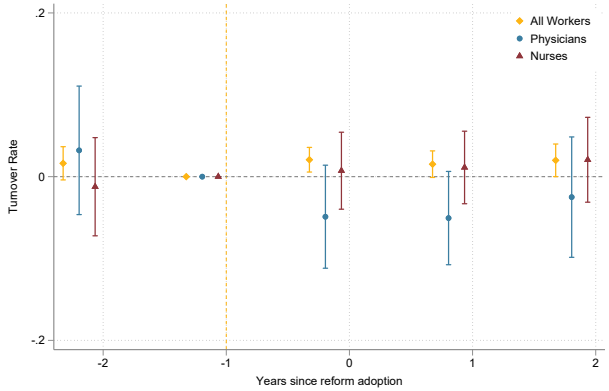
Figure 10: What are new managers doing differently?



(a) OR Utilization



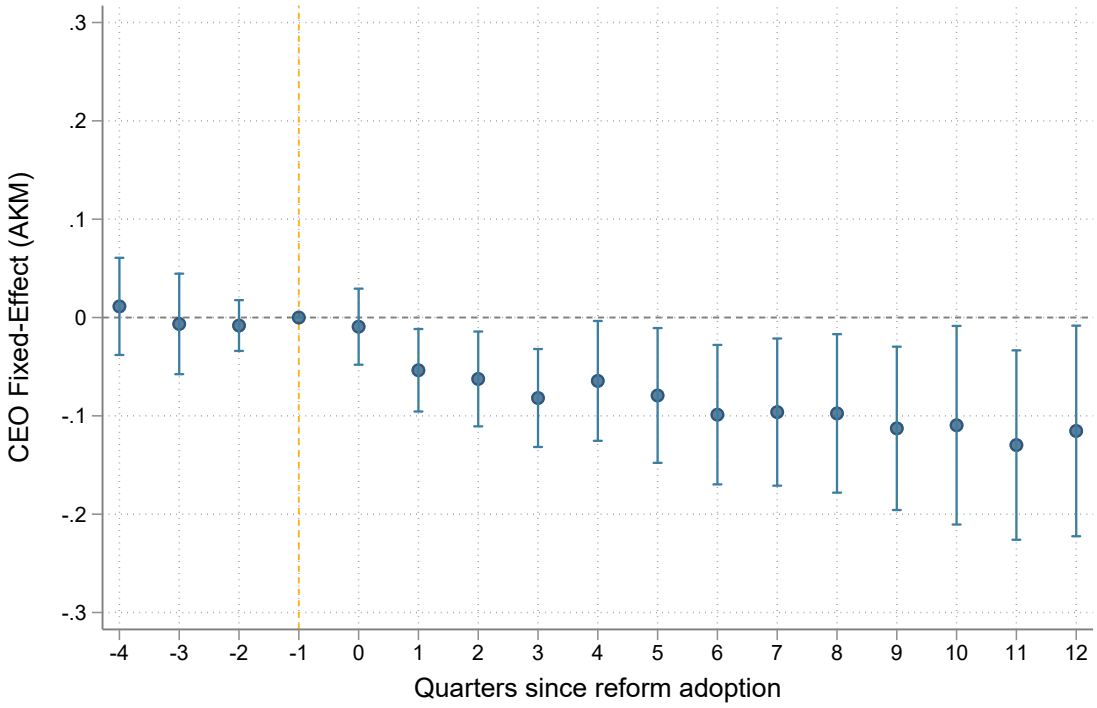
(b) Surgery Rate



(c) Turnover

Notes: This figure presents event study evidence of the reform's effect on several hospital outcomes, following equation 2 estimated at the yearly level. Panel A examines the logarithm of operation room utilization over capacity. Panel B focuses on logged surgery rate. Panel C replaces the dependent variable by the turnover of i) hospital personnel, ii) doctors, and iii) nurses. Turnover is defined as the number of workers in group j who are leaving hospital h in $t+1$ (job to job or job to unemployment transitions), over the number of workers of group j working in h at time t. Each dot corresponds to an estimated coefficient, and the vertical lines indicate the corresponding 95% confidence intervals. The yellow dashed line represents the omitted coefficient. Standard errors are clustered at the hospital level.

Figure 11: Effect of the reform on CEO fixed effects



Notes: This figure presents event study evidence of the reform's impact on managerial talent. We measure managerial talent using managers' fixed effects estimated from equation 5 in the set of hospitals connected by managers' mobility (Abowd et al., 1999; Card et al., 2013). For this event study we consider an augmented version of equation 2 that also includes connected set fixed effects. Each dot corresponds to an estimated coefficient, and the vertical lines indicate the corresponding 95% confidence intervals. The yellow dashed line represents the omitted coefficient. Standard errors are clustered at the hospital level.

Table 1: Descriptive statistics

	Mean	Std. Dev.	Bottom 10%	Median	Top 10%	# of Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Patient Characteristics:						
% Female	0.59	0.08	0.47	0.60	0.68	13,988
% Age < 29	0.36	0.16	0.14	0.37	0.49	13,988
% Age ∈ (30,29)	0.12	0.05	0.06	0.12	0.17	13,988
% Age ∈ (40,49)	0.10	0.04	0.06	0.10	0.13	13,988
% Age ∈ (50,59)	0.10	0.04	0.06	0.09	0.14	13,988
% Age ∈ (60,69)	0.11	0.05	0.07	0.10	0.16	13,988
% Age ∈ (70,79)	0.12	0.06	0.06	0.11	0.20	13,988
% Age ∈ (80,89)	0.09	0.06	0.03	0.07	0.16	13,988
% Age > 89	0.02	0.02	0	0.01	0.05	13,988
% Public Insurance	0.96	0.05	0.92	0.98	1.00	13,988
Hospital Characteristics:						
High-Level Hospital	0.33	0.47	0.00	0.00	1.00	13,988
Medium-Level Hospital	0.15	0.36	0.00	0.00	1.00	13,988
Low-Level Hospital	0.52	0.50	0.00	1.00	1.00	13,988
Total Number of Patients	1,491	2,006	101	587	4,568	13,988
Total Number of Beds	143	177	16	65	415	13,946
Total Number of Surgeries	461	867	0.00	4	1,730	13,988
Physicians per 100 patients	6.75	8.58	2.30	4.91	11.89	6,624
Nurses per 100 patients	6.17	7.72	2.22	4.79	9.89	6,624
Hospital Outcomes:						
Number of Deaths	38.21	63.27	1.00	12.00	116.00	13,988
Death Rate	2.46	1.94	0.38	2.15	4.69	13,988
Death Rate 28-days	4.21	2.87	1.18	3.66	7.83	13,988
Death Rate ER	3.01	3.53	0.15	2.55	5.69	11,087
Death Rate ER AMI	12.21	23.77	0.00	2.38	33.33	4,555
Actual over Predicted Death Rate	0.77	0.48	0.16	0.74	1.34	13,988

Notes: This table presents descriptive statistics for the universe of public hospitals in Chile, between 2001 and 2019. Patient characteristics and hospital outcomes come from individual-level inpatient records collected by the Ministry of Health, and encompass almost 29 million hospital events (DEIS, 2019). Hospital characteristics come from hospital-level public records, and restricted-use administrative data covering the universe of employees in all public hospitals between 2014 and 2019, which is collected by Ministry of Health for HR purposes.

Table 2: Balance in growth rate of observables characteristics before the reform

	Avg. Never adopters	β Ever adopters (Levels)	β Ever ADP (First-Diff)	Ord. Logit Pseudo-R^2
	(1)	(2)	(3)	(4)
Patient Characteristics:				
% Age < 29	0.401	0.048**	0.000	0.092
		(0.020)	(0.001)	
% Age ∈ (30,39)	0.135	0.009	-0.000	0.092
		(0.006)	(0.000)	
% Age ∈ (40,49)	0.090	0.010**	-0.000	0.092
		(0.005)	(0.000)	
% Age ∈ (50,59)	0.081	0.001	0.000	0.092
		(0.004)	(0.000)	
% Age ∈ (60,69)	0.095	-0.011**	0.001**	0.092
		(0.005)	(0.000)	
% Age ∈ (70,79)	0.111	-0.025***	-0.000	0.092
		(0.007)	(0.000)	
% Age ∈ (80,89)	0.072	-0.026***	-0.000	0.092
		(0.006)	(0.000)	
% Age > 89	0.015	-0.006***	-0.000	0.092
		(0.001)	(0.000)	
% Female	0.609	-0.001	0.000	0.092
		(0.011)	(0.001)	
% Public Insurance	0.958	-0.032***	0.001**	0.075
		(0.007)	(0.001)	
Hospital Outcomes:				
Number of Deaths	6.183	62.404***	0.334	0.084
		(8.772)	(0.236)	
Death Rate	1.517	0.719***	0.037**	0.084
		(0.200)	(0.014)	
Death Rate ER	2.420	1.405***	0.702	0.091
		(0.534)	(0.497)	
Death Rate 28-days	2.668	0.433*	-0.014	0.084
		(0.252)	(0.020)	
Death Rate AMI	22.327	-3.525	11.131	0.068
		(5.223)	(11.759)	
Actual over Predicted Death Rate	0.539	0.402***	0.005	0.088
		(0.053)	(0.004)	
Political Characteristics:				
% Votes for Right in hospital's municipality	25.890	5.253*	-0.567	0.092
		(2.738)	(3.151)	
% Votes for Center in hospital's municipality	19.043	1.808	1.868	0.092
		(3.403)	(2.559)	
% Votes for Left in hospital's municipality	24.553	-3.384	-1.514	0.090
		(3.324)	(2.751)	

Notes: This table studies differences between ever and never adopter hospitals in terms of predetermined characteristics (before the reform was enacted, 2001-2004). Column (1) shows the average of each characteristic for never adopters. Columns (2) and (3) show the coefficient obtained from a regression of each variable, in levels or in first-differences, on a dummy that equals one if the hospital was ever adopter. In Panel B, first-differences correspond to the difference in vote shares between the 2000 and 2004 elections. Column (4) assess the relevance of each variable to account for the timing of adoption among adopters. For this purpose, we rank all public hospitals by their adoption date and estimate an ordered logit model of this ranking on all listed variables. Then, we remove each variable, one at a time, and report the goodness of fit of the model as proxied by its Pseudo-R^2.

Table 3: Impact of the reform on death rates

	Ln Death Rate						Ln(Actual/Predicted)	Poisson
	All			28-days	ER	ER: AMI	Death Rate	# Deaths
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if reform adopted in hosp.	-0.131***	-0.091***	-0.141***	-0.099***	-0.153***	-0.269**	-0.089***	-0.055***
	(0.025)	(0.024)	(0.022)	(0.017)	(0.025)	(0.119)	(0.024)	(0.016)
Observations	8,104	8,104	8,104	8,104	6,592	771	8,104	8,104
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case-Mix Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Interaction of Case-Mix	No	No	Yes	Yes	Yes	No	Yes	Yes
# of Hospitals	181	181	181	181	175	89	181	181
Mean Dep. Variable (levels)	2.625	2.625	2.625	4.726	3.088	30.22	0.737	21.85

Notes: This table presents our estimates of the impact of the selection reform on public hospital's performance, as measured by death outcomes. The estimates are from the staggered difference-in-differences specification in equation 1. The empirical analysis uses quarterly panel data of public hospitals in a time window comprehending 6 quarters before and 12 quarters after the reform was adopted by each hospital, and exploits the gradual adoption of the selection reform in public hospitals in that period. We do not impose a time window for hospitals that did not adopt the policy. In columns (1)-(3), we focus on in-hospital death rates adding case mix controls sequentially. Column (4) replaces the dependent variable by 28-days from admission death rate, thus considering in and out of hospital deaths. In column (5) we study the impact of the reform on death rates of ER admissions, and column (6) focuses on the subset of emergency room admissions with AMI (Acute Myocardial Infarctions, commonly known as "heart attacks") diagnoses. In column (7) we consider risk-adjusted death rates. We define risk-adjusted death rate as the actual hospital-level death rate divided by the average death rate as predicted by patient-level characteristics using a logit model for deaths (for details see Appendix B). Finally, column (8) reports estimates from a Poisson regression of death counts. Except for columns (6) and (8), all regression are weighted by the number of hospital's inpatients as of 2005. In column (6) we use the number of hospital's inpatients with AMI diagnosis (as of 2005) as weights. For columns (1)-(7), the mean dependent variable is presented in levels, instead of logs. Standard errors are displayed in parentheses and are clustered at the hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: CEO selection reform v. other policies

Policy	Paper	Death Rate Definition	Average death rate	Impact on death rate	Sample of patients
(1)	(2)	(3)	(4)	(5)	(6)
Spending					
↑ 10% p/capita	Doyle et al. (2015)	All, 1-year	37%	↓ 6%	ER + Amb. + ≥ 65*
	Ours		32%	↓ 7%	ER + ≥ 65
Public vs Private					
VA v. Non VA hospitals	Chan et al. (2022)	All, 1-year	29%	↓ 7%	ER + Amb.+ ≥ 65
	Ours		32%	↓ 7%	ER + ≥ 65
Competition					
+1 hospital in nbhd.	Bloom et al. (2015)	In-hospital, 28-day	15%	↓ 10%	ER + AMI
	Ours		30%	↓ 27%	ER + AMI
↓ 10% HHI	Gaynor et al. (2013)	In-hospital, 28-day	1.6%	↓ 1%	All patients
	Ours		2.3%	↓ 15%	All patients
↓ 10% HHI	Gaynor et al. (2013)	All, 30-day	7%	↓ 3%	AMI + ages 35-74
	Ours		28%	↓ 20%	AMI + ages 35-74

Notes: This table compares the impact of the CEO selection reform studied by us to the impact of other policies previously studied by the literature. To construct this table, we estimate our main equation 1 for the different dependent variables—reported in column (3)—and in different samples of patients reported in column (6). For more details, see 3.5. Acronyms used in the table: ER: Emergency Room; AMI: Acute Myocardial Infarction; Amb: taken to hospital by ambulance; *: non-deferrable medical conditions.

Table 5: Effect of the reform on managers' characteristics

	Management Education				Skills			Demographics	
	Mgmt. Undergrad.	Mgmt. Postgrad. Studies	Any Mgmt. Studies	Any Mgmt Studies	Avg. PSU Score	Math Specific Exam	Science Specific Exam	Age	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1 if reform adopted in hosp.	0.21***	0.33***	0.37***	0.22***	-0.12	0.09	-0.14***	-1.82*	-0.02
	(0.04)	(0.05)	(0.05)	(0.06)	(0.10)	(0.07)	(0.05)	(1.04)	(0.05)
Sample	All	All	All	Doctor CEOs	All	All	All	All	All
Observations	8,953	8,953	8,996	6,354	7,654	6,053	6,053	8,700	8,953
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of Hospitals	186	186	188	182	183	167	167	186	186
Mean Dep. Variable	0.05	0.24	0.25	0.20	1.97	0.71	0.96	49.83	0.22

Notes: This table presents our estimates of the impact of the selection reform on public hospital CEOs' characteristics. The estimates are from the staggered difference-in-differences specification in equation 1, but we switch the dependent variable for CEO characteristics. The empirical analysis uses quarterly panel data between 2001-2019 and exploits the gradual adoption of the selection reform in public hospitals in that period. In columns (1)-(4), we focus on management education. Undergraduate management degree is a dummy variable that takes value 1 of the CEO holds an undergraduate degree in a management-related undergraduate major. Management postgraduate studies include master's and diplomas related to management. *Any* management studies include undergraduate and postgraduate studies. The sample of "Doctor CEOs" are hospital-quarter observations where the CEO is a doctor. Columns (5)-(7) examine the impact on college admission test scores as a proxy for cognitive skills. Math (science) specific exam takes value 1 if the manager took the math (science) specific exam in the older version of the college entrance exam in Chile, where applicants had to choose which specific exam to take. Columns (8)-(9) study the effect on the age and gender of the CEO. The mean dependent variable is computed in the period before each hospital adopted the reform. Standard errors are displayed in parentheses and are clustered at the hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Heterogeneity in CEO performance by managerial education background

	Variation is Reform			Variation is CEO transition	
	Ln Death (%)	Ln Death (%)	Ln Death (%)	Ln Death (%)	Ln Death (%)
	(1)	(2)	(3)	(4)	(5)
Reform & Mgmt. Undergrad.	-0.111***				
	(0.029)				
Reform & Non Mgmt. Undergrad.	-0.076***				
	(0.026)				
Reform & Any Mgmt. Studies		-0.122***	-0.130***		
		(0.025)	(0.028)		
Reform & Non Mgmt. Studies		-0.028	-0.027		
		(0.027)	(0.027)		
CEO with management studies				-0.072***	
				(0.025)	
CEO with no management studies					-0.010
					(0.022)
Sample	All CEOs	All CEOs	Doctor CEOs		
Observations	8,085	8,085	5,732	71,027	193,177
Time FE	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes
Case mix Controls	Yes	Yes	Yes	Yes	Yes
# of Hospitals	181	181	176	168	175
Mean Dep. Variable	2.63	2.63	2.49	2.88	2.41
p-value *Mgmt. = Non Mgmt.*	0.22	0.00	0.00		

Notes: This table examines the heterogeneous CEO by managerial education background. Columns (1)-(3) focus on the differential effect of the selection reform on death rates, following the staggered difference-in-differences design in equation 1. In Panel A, we ask to what extent the reform has differential effects depending on the CEO's educational background. Column (1) interacts the adoption of the selection reform with whether the CEO holds an undergraduate degree in a management-related undergraduate major. Columns (2) and (3) focus on whether the CEO has *any* management studies, which include undergraduate and postgraduate studies related to management. Columns (4)-(5) present the results of the stacked event study specification in equation 3. In column (4), an event is a transition from a CEO without management studies to a CEO with management studies. In column (5), an event is a transition from a CEO without management studies to a CEO without management studies. For each transition event, we define a time window around it and a control group of hospitals with no transitions in the time window. We define a set of valid events as those that are balanced in the time window and do not overlap with another transition in the pre-period within the time window. We also exclude the transitions associated with the first time that a CEO was appointed after the selection reform was adopted in a given hospital. In total, there are 94 valid CEO transitions, as described in Appendix Table A.3. The dependent variable is the death rate at the hospital level in a given quarter. Dots indicate estimated coefficients and vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at the hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Explanatory power of managerial talent to account for hospital performance

Panel A: Analysis of variance of hospital quality

	Ln Death Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Observations	6712	6712	6712	6712	6712	6712
R^2	.41	.42	.67	.76	.73	.76
Adj. R^2	.40	.41	.66	.73	.69	.72
Case Mix Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	No	Yes	Yes	Yes	Yes	Yes
Hospital FE	No	No	Yes	Yes	No	Yes
Manager FE	No	No	No	Yes	Yes	Yes
Hospital-Manager FE	No	No	No	No	No	Yes
F-statistic for Manager FEs	-	-	-	3.4	10.06	-
F-statistic for Hospital Manager FEs	-	-	-	-	-	10.19

Panel B: Bias-Corrected Variance-Covariance Decomposition

	Component	Share of Total
	(1)	(2)
Var (Resid. Log Death Rate)	0.1709	100%
Var (Manager)	0.0671	39.2%
Var (Hospital)	0.0675	39.4%
Cov (Manager,Hospital)	-0.0422	-24.6%
Var (Residual)	0.0786	45.9%

Notes: This table shows the extent to which variation in hospital quality can be explained by managerial talent. Panel A compares the adjusted R^2 estimated from several regressions of the logarithm of death rates on different sets of explanatory variables. Panel B reports bias-corrected variances and covariances estimated on the largest connected sets following Andrews et al. (2008). Hospitals and managers' fixed effects are estimated from equation 5 in the set of hospitals connected by managers' mobility (Abowd et al., 1999; Card et al., 2013).

APPENDIX

Public Managers and Hospital Performance

Cristóbal Otero and Pablo Muñoz

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A Description of the Referral and Counter-Referral System

Other than patients admitted via ER, public hospitals only accept patients referred from other public care centers. Individuals are assigned to a primary care center depending on where they live or work. Referrals to a hospital depend on three main factors: the location of the primary care center and the diagnosis and demographics of the patient. Each Health Service develops detailed Referral and Counter Referral guidelines for all healthcare centers under their territorial scope. Each primary care center can only refer patients following the guidelines defined by the Health Service that supervises them.

Figure A.8 illustrates an example of patient referral based on their primary care center. The figure depicts two primary care centers, "CESFAM Dra. Haydee López Cassou" (in blue and white with diamond markers) and "CESFAM Pablo de Rokha" (in blue and white with star markers), which are located in adjacent Health Services. Although individuals in each primary care center might live close by, if they require tertiary care, they are referred to different hospitals. For most diagnoses, "CESFAM Dra. Haydee López Cassou" refers their adult patients to Hospital Barros Luco (in red and white with cross markers) and "CESFAM Pablo de Rokha" are refers them "Hospital Sótero del Río" (in red and white with H markers).

Table A.1 shows an example of referral guidelines from different primary care centers to public hospitals in two Health Services. Primary care centers in columns (1)-(2) and (3)-(4) are in two different Health Services, Metropolitano Norte and Metropolitano Oriente, respectively. The numbers in the table are the hospital to which patient are referred. The example shows that referrals depend exclusively on the primary care center, the diagnosis and the demographics of the patient. For example, a patient for medical oncology older than 15 years old in CESFAM Colina is referred to "Instituto Nacional del Cáncer Dr. Caupolicán Pardo Correa".

To empirically assess the compliance with the referral guidelines, we focus on a sample of patients with public insurance, who were discharged (dead or alive) at any point during the year 2004, and who were not admitted into the hospital via ER. In this sample, we classify patients into cells defined by patients' county of residence, age group (less than 1 year old, between 1 and 15 years old, and more than 15 years old) and diagnostic (as reported by the hospital from which they are discharged). If guidelines are strictly followed, we should expect all patients' within a cell to attend the same hospital. To visually evaluate this, Figure A.9 plots a spikeline with the share of patients in each cell that are discharged exclusively from one hospital. Reassuringly, more than 80% of patients within a cell are discharged from the same hospital. Importantly, the fact that patients within a cell are being discharged from different hospitals does not necessarily constitute evidence of non-compliance with the referral and counter referral guidelines. In our case, this may reflect censorship due to the fact that we do not observe the diagnostic at the primary care center, but only at the hospital. Likewise, this could be explained by the fact that we only observe patients' home address but they could have used their work addresses to register in the health system. Finally, there might also be measurement error in the age of patients.

B CMS Risk Adjustment

To ease selection concerns we follow the English National Health Service (e.g., Health and Centre, 2015) and construct a "risk-adjusted mortality rate" dividing the actual hospital-level death rate by the death rate predicted based on observable characteristics of hospitals' patients. This variable should be interpreted such that an increase (decrease) from one means a larger (smaller) death rate than predicted based upon hospital case-mix.

The prediction is built following the procedure described in Ash et al. (2012), which the Centers for Medicare and Medicaid Services (CMS) use in the United States. First, we focus on a sample of 5,740,496 patients between 2001-2004 (before reform adoption). These patients constitute the universe of discharges in the country. For them, we fit a logit model where death is the dependent variable and different set of patients' characteristics are the independent variables. Our preferred model includes the following set of covariates: gender, eight age buckets (< 30, $30 - 49$, $50 - 59$, $60 - 69$, $70 - 79$, $80 - 89$, and > 89), type of health insurance (private or one of 5 categories within public insurance that depend on income), and the 31 categories of the enhanced Elixhauser comorbidity index (Elixhauser et al., 1998; Quan et al., 2005). Then, we predict the probability of death for each patient, a variable we use to construct the predicted death rate at the hospital level.

In section 3 we present the results obtained when using our preferred risk-adjusted measure, the one considering all available covariates. Nonetheless, Table A.2, shows the robustness of our result to using alternative risk-adjusted measures. Columns (1)-(3) show the estimates from equation 1 obtained for different definitions of the risk-adjusted death rate. In Column (1), the risk-adjusted death rate is based on patients' demographics (gender and age). Column (2) also considers patients health insurance, a proxy for socioeconomic status. Finally, column (3) corresponds to our preferred measure that also includes patients' diagnoses based on the enhanced Elixhauser comorbidity index. Results are stable regardless of the incorporation of more covariates into the Logit model. This is reassuring because, according to recent research leveraging quasi-random variation on death rates, risk-adjusted mortality measures are reliable and valid indicators of hospital quality in the U.S., where the institutional setting is prone to patient selection (Doyle et al., 2019).

C No Differential Effects of Performance Pay

In this Appendix, we empirically examine whether CEOs' scores on their performance pay measure predicts better managerial performance at the hospital. We define a dummy variable that takes value 1 if the manager was above the performance score median and 0 otherwise. We interact this variable with the introduction of the reform and study the impact of the reform for managers with high and low scores separately.

Online Appendix Table A.4 displays the results. Since we do not observe the performance scores for all managers that took over after the reform, we miss several observations. For this reason, in column (1) we report the impact of the reform in the sample for which we have data. Importantly, we find the same effect as when using the whole sample. In column (2), we report the results of the reform for managers with high and low scores. We find that both estimates are almost identical. As posited above, this is evidence that performance pay did not have any effect on manager performance.

D Effect of the Selection Reform on CEO Wages

In this Appendix, we study the reform's effect on hospital CEOs' wages. We leverage the gradual adoption of the reform across public hospitals and estimate an event study specification on the position's wage. One important caveat is that the wage data panel starts in January 2014, after which there are only three hospitals adopted the selection reform for the first time. Fortunately, we also have data for December 2011-2013, which gives us a larger number of events. For this reason, we also estimate an event study using data only for December, between 2011 and 2019.

Panel (a) in Figure A.14 presents the results using quarterly data starting on 2014. Although the estimates are noisy due the small number of events, the estimate is stable and the average quarterly wage increase in the five quarters post-adoption is 33%. We also do not find evidence of pre-trends, which means that hospitals adopting the reform during a given period are not on a differential wage trend than those that do not. Panel (b) presents the estimates using monthly data for each December, starting on 2011. In both cases, standard errors are clustered at the hospital level. Reassuringly, we find quantitative and qualitatively similar results.

It is important to note that the results of this exercise reflect the change in the position's pay and therefore is a composition of two effects. On the one hand, there are mechanical changes in pay due to changes in the manager's characteristics. For example, in the public sector, there are tenure bonuses that increase with experience. On the other hand, there is an increase in the position's base wage. Since our wage data follows the position and not the individuals over time, we cannot separate both effects.

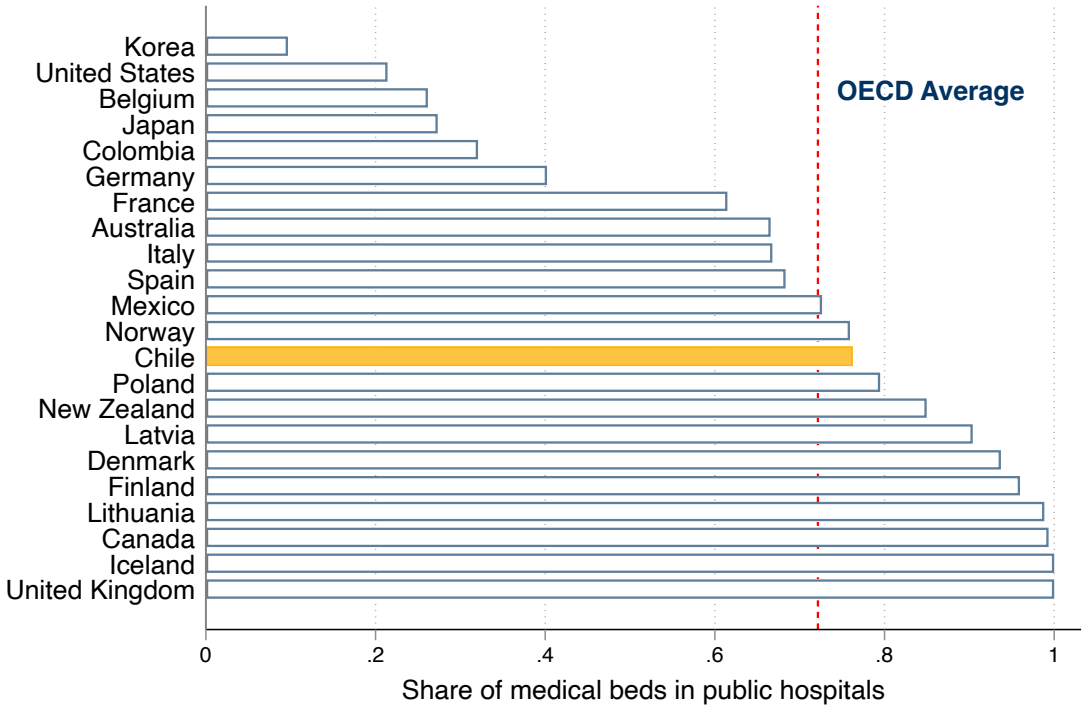
E Managerial Talent: Threats to Identification

As in Card et al. (2013) and Fenizia (2022) we assess two main threats to the identification of manager fixed effects. The first concern is that CEO mobility is endogenous due to a systematic relation with hospital-specific trends. For example, if good managers are rotating to hospitals that are improving their quality over time. This pattern would overestimate our CEO fixed effects. Relatedly, one may worry that CEOs move to a new hospital due to transitory productivity shocks in that hospital. This would be the case, for instance, if a given hospital performs poorly in a given period and, as a reaction, makes an extra effort to hire a good manager. To assess this concern, we exploit the rotations of CEOs in an event study framework. Specifically, we calculate the difference between the incumbent and the incoming CEO (hereafter, Δ CEO FE) and classify the CEOs transitions into terciles. Intuitively, the classification allows us to distinguish whether the new CEO implies an average increase, an small change, or an average decrease in manager quality.

Panel (a) of Figure A.13 plots the effect of CEO transitions on residualized death rates for each Δ CEO FE tercile. Several points are worth noting about this figure. First, hospitals with an event in the first tercile observe a significant decline in death rates after the CEO changes, and the opposite is true for events in the third tercile. In both cases, the effects persist over time. Moreover, we find no effect on hospital quality for Δ CEO FE tercile in the second tercile, where changes in CEO quality are small. A second observation is that hospitals hiring a good or bad incoming CEO (relative to the incumbent) are not on different trends; and that turnovers do not seem to correlate with pre-trends of hospital performance. Before a CEO turnover, hospitals that face a CEO move are in a similar trend to those that do not, consistent with evidence presented in Figure 2. In sum, we think that these event-studies should ameliorate concerns regarding endogenous mobility.

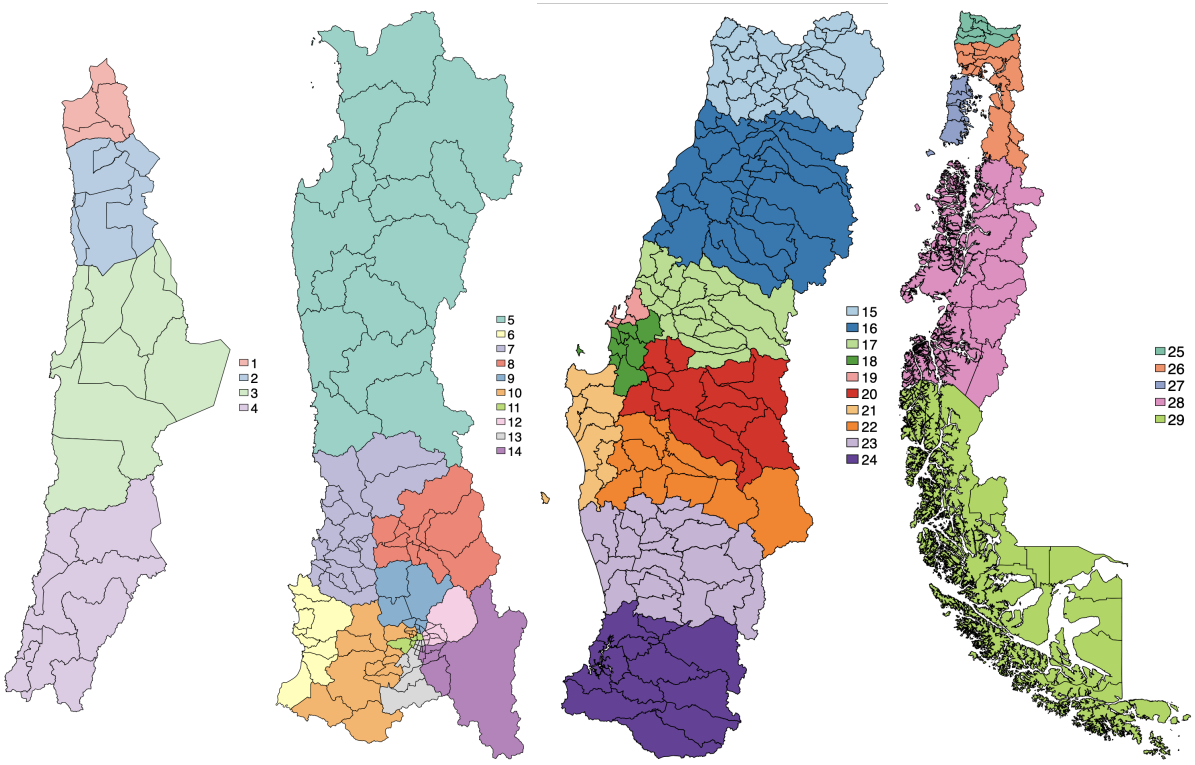
The second threat to the identification of manager fixed effects comes from the potential existence of match effects between managers and hospitals; a dimension that is neglected in the log model by the additive separability between the CEO and hospital effects. Different managers may have different effects on hospital quality, depending on the value of their match component. If managers sort into hospitals where they have a comparative advantage, this effect would be captured by the error term and would bias our estimates. To examine whether this concern holds ground we consider two pieces of evidence. First, in column (6) in Table 7 we report a saturated version of Equation 5, where we include manager-by-hospital fixed effects. If the match component is sizable, this model should have a better fit than that in column (4). We find that the adjusted R^2 increases from 0.69 to 0.72 after including manager-by-hospital fixed effects, a rather modest change in model fit. We further explore to what extent the model is overlooking match effects by analyzing whether the mean residuals are abnormally high or low for a given pair of hospital-CEO. With this in mind, we divide the estimated manager and hospital effects into quartiles and compute the mean residual for each pair. Results are depicted in panel (b) of Figure A.13. Reassuringly, we find that all values are small and lower than 0.1. A final piece of evidence comes from the symmetry of the effects depicted in panel (a) of Figure A.13. Hospitals moving from a good CEO (in the first tercile) to a bad CEO (third tercile) face an opposite and symmetric effect to that moving from a bad CEO to a good CEO, which would be implied by the lack of match effects in the model. All in all, the evidence suggests that the two-way fixed effects model fits the data well, and if any, match effects are small.

Figure A.1: Share of medical beds provided by public hospitals in OECD economies



Notes: This figure displays the share of medical beds provided by public hospitals in a set of selected OECD countries in 2019. The dashed red line represents the average share in all OECD countries. The share is computed as the ratio between the total number of hospital beds in publicly owned hospitals and the total hospital beds in the country. Both variables are reported in OECD (2022a).

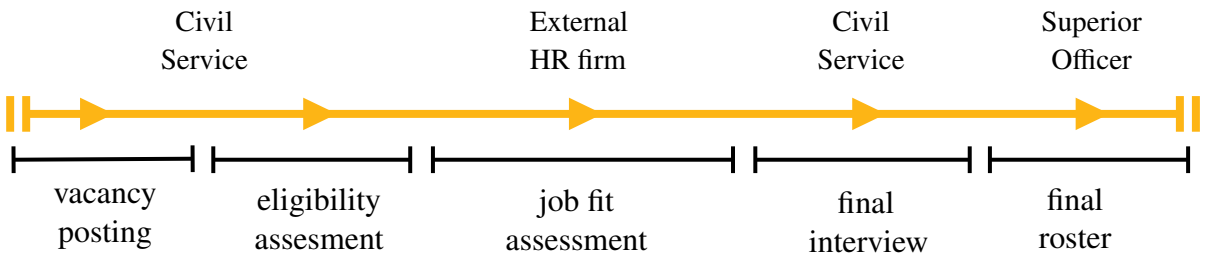
Figure A.2: Health Services are distributed geographically



(a) North (b) North-Center (c) Center-South (d) South

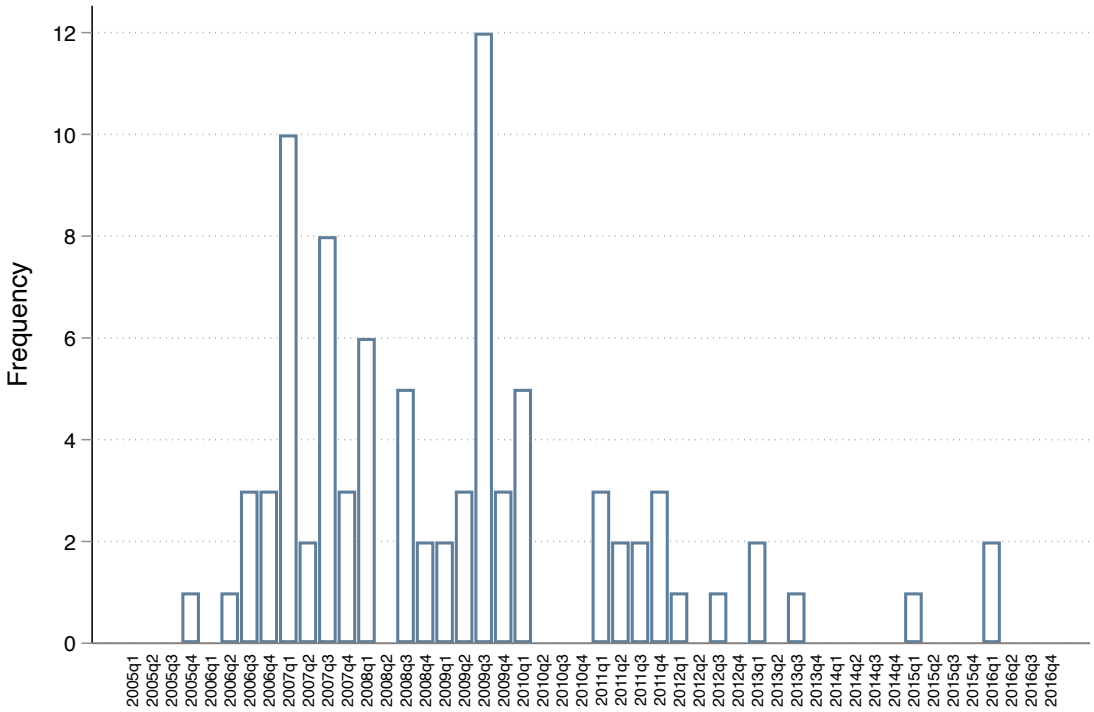
Notes: This figure shows the geographic distribution of the 29 Health Services in Chile. Each Health Service is responsible to oversee public health providers in the municipalities in their territory. Colors represent different Health Services and black lines represent municipal borders.

Figure A.3: Selection process after the recruitment reform



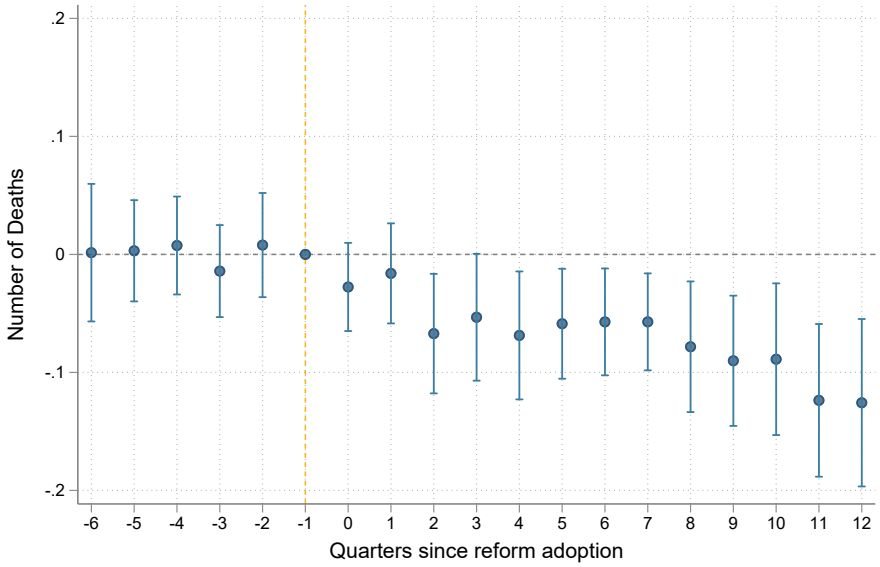
Notes: This figure illustrates the selection process for senior executives positions where the the selection reform has been adopted. The job call starts with the position posted online on the Civil Service's website and in a newspaper with national circulation. In some cases, the Civil Service may additionally hire headhunters to widen the pool of applicants. After the job posting finishes, an external HR firm evaluates each individual's job trajectory according to the job profile. They also assess their motivation and overall competencies. The consultant gives every applicant a grade based on an objective rubric and provides a shortlist to the Civil Service. In the next phase, a committee formed by representatives of the Civil Service and of the Ministry where the position is based interviews the remaining candidates and selects a short list of three individuals based on objective criteria. Finally, the superior officer appoints the winning candidate from the final roster with complete discretion.

Figure A.4: Reform gradual adoption in public hospitals

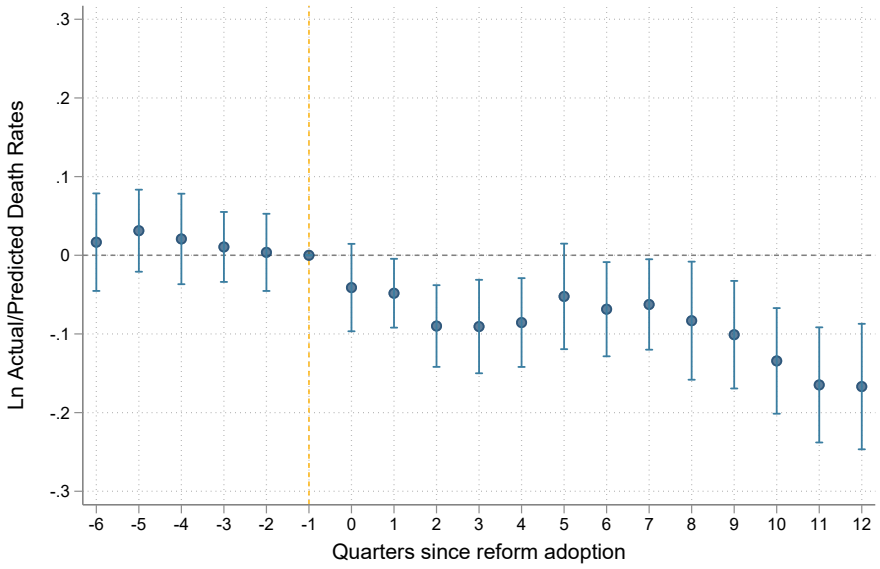


Notes: This figure displays the adoption of the selection for reform for the CEO position in public hospitals. An observation represents a hospital that adopts the selection reform for the first time. After a hospital adopts the process for the first time, all future CEOs in that hospital have to be appointed using the new selection system.

Figure A.5: Dynamic effects of the reform using alternative models



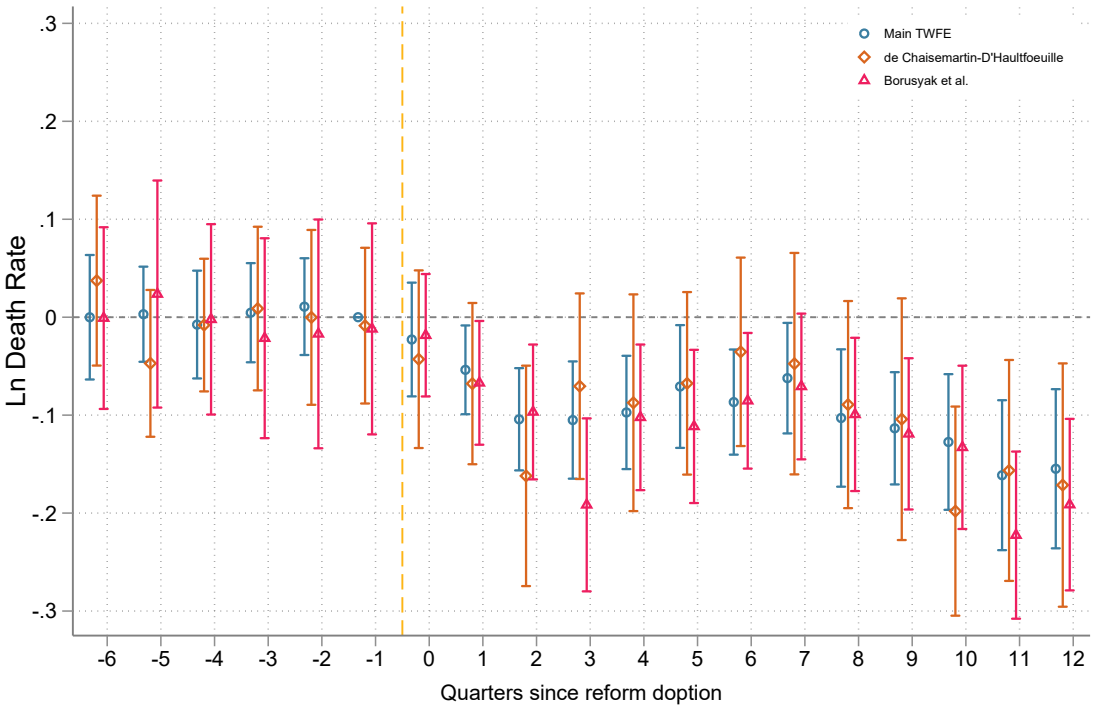
(a) Poisson



(b) Risk-Adjusted

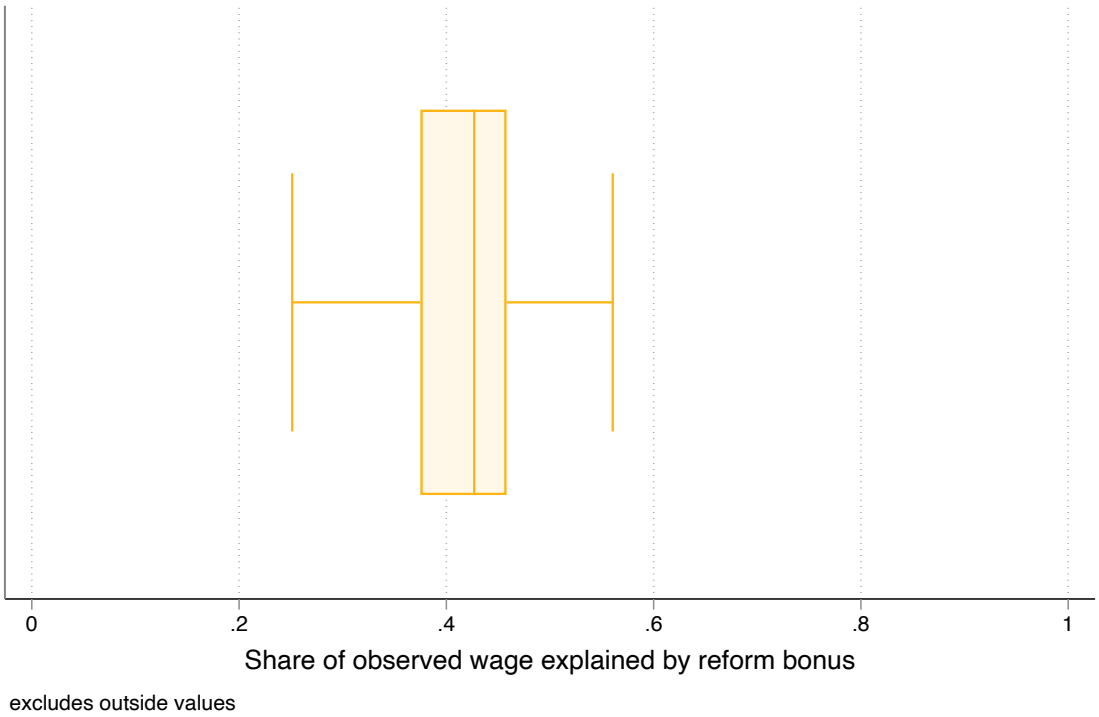
Notes: This figure presents the reform's effect on alternative outcome variables. Panel A reports estimates obtained from a dynamic Poisson regression of death counts. Panel B reports estimates and confidence intervals obtained from a two-way fixed effects OLS regression of logged risk-adjusted death rates. We define risk-adjusted death rate as the actual hospital-level death rate divided by the average death rate as predicted by patient-level characteristics using a logit model for deaths (for details see Appendix B). Each dot corresponds to an estimated coefficient, and the vertical lines indicate the corresponding 95% confidence intervals. The yellow dashed line represents the omitted coefficient. Standard errors are clustered at the hospital level.

Figure A.6: Alternative event study models and estimation methods



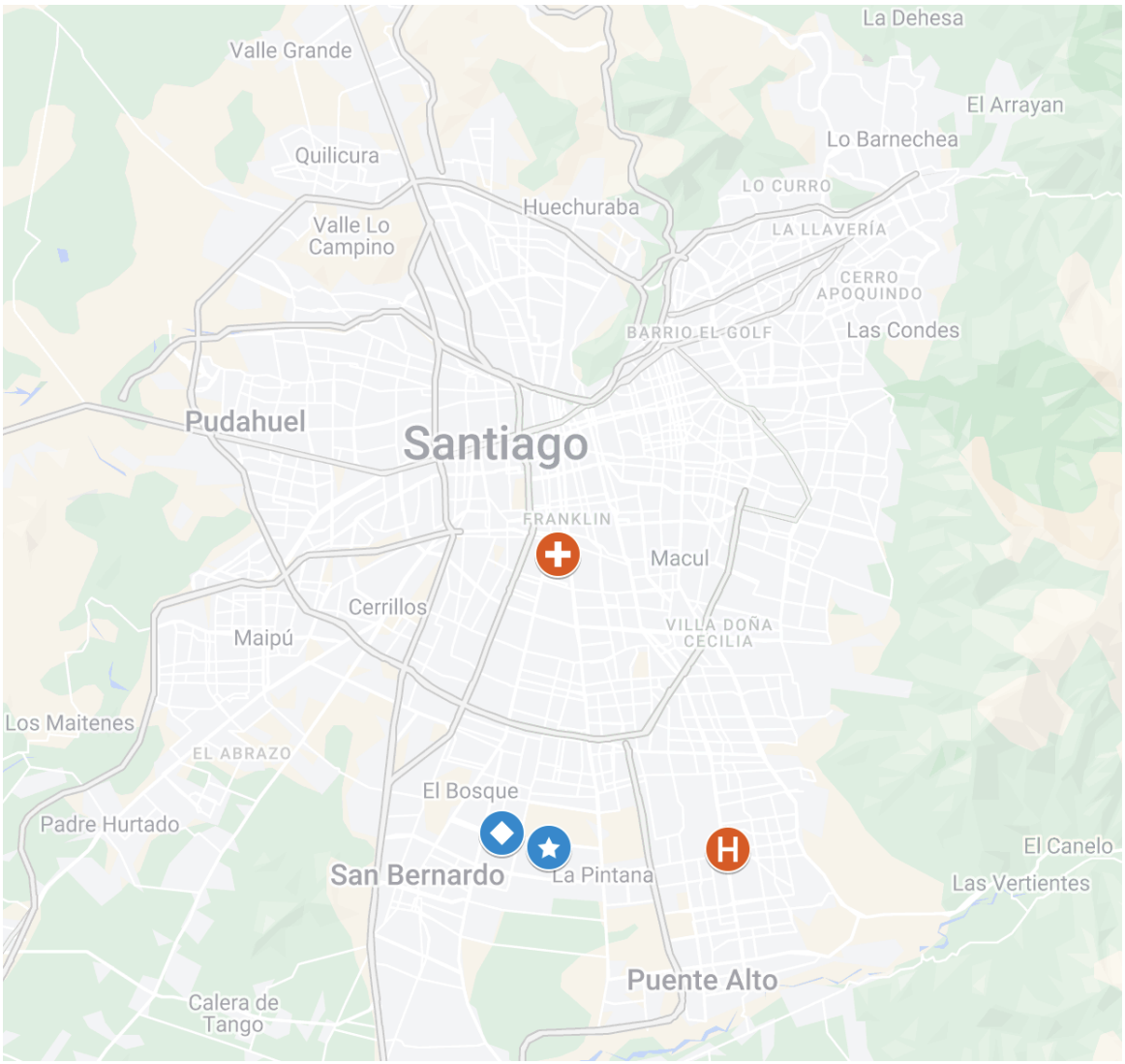
Notes: This figure plots the estimates and confidence intervals obtained from different event study models and estimation methods. The main event-study results using a two-way fixed effects (TWFE) regression of logged death rates (see equation 2) are presented under the label "Main TWFE" (in blue with circle markers). For comparison, we overlay the results obtained using the models suggested in De Chaisemartin and d'Haultfoeuille (2020) (in orange with diamond markers) and Borusyak et al. (2022) (in red with triangle markers), which are robust to treatment effect heterogeneity. Each dot, diamond, and triangle marker corresponds to an estimated coefficient, and the vertical lines indicate the corresponding 95% confidence intervals.

Figure A.7: Share of total CEO wage explained by reform's bonus



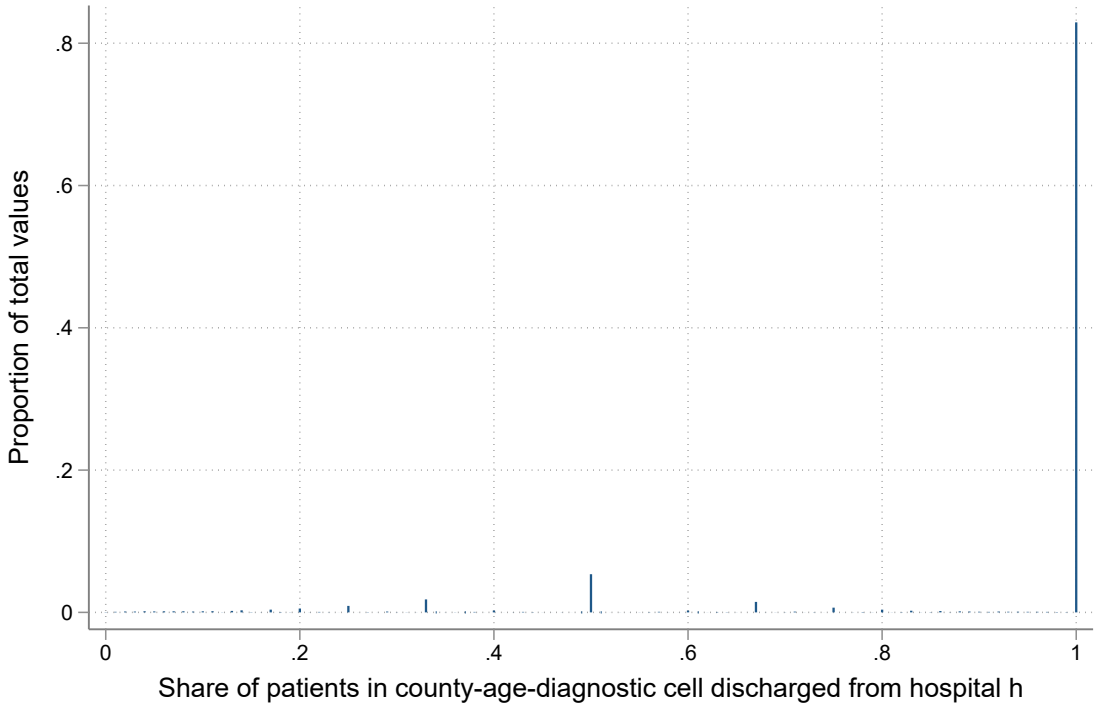
Notes: This figure displays a box plot of the share of the CEO wage explained by the reform's bonus. The sample consists of all CEO positions appointed using the selection reform between 2014 and 2019, which are the dates for which monthly data are available. The average wage share explained by the reform's bonus is 43 percent. The 25 and 75th percentiles are 37 and 46 percent, respectively.

Figure A.8: Examples of referral from primary care centers



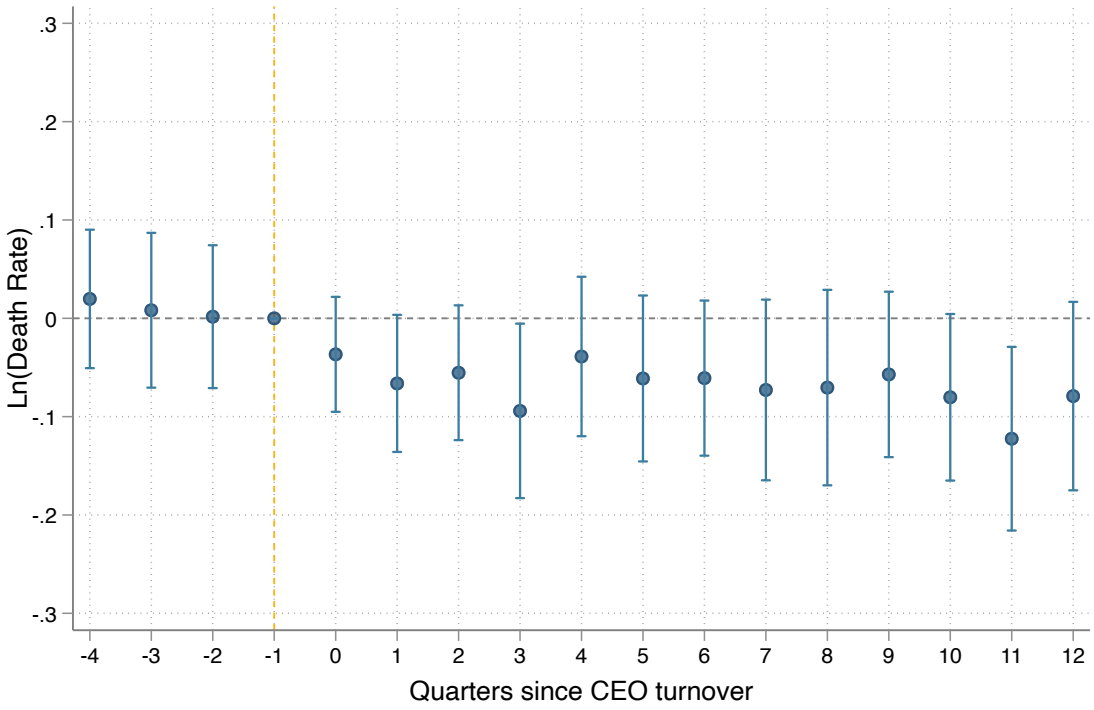
Notes: This figure illustrates an example of patient referral based on their primary care center. The figure depicts two primary care centers, "CESFAM Dra. Haydee López Cassou" (in blue with white diamond markers) and "CESFAM Pablo de Rokha" (in blue with white star markers), which are located in adjacent Health Services. Although individuals in each primary care center might live close by, if they require tertiary care, they will be referred to different hospitals. For most diagnoses, 'CESFAM Dra. Haydee López Cassou" refers their patients to Hospital Barros Luco (in red with white cross markers). Patients from "CESFAM Pablo de Rokha" are referred to "Hospital Sótero del Río" (in red and white H markers). Referrals depend exclusively on the location where the individual is enrolled, her diagnosis, and her demographics. Table A.1 shows an example of referrals to different public hospitals within the same Health Service based on the patient's diagnosis and demographics.

Figure A.9: Empirical test of patient selection



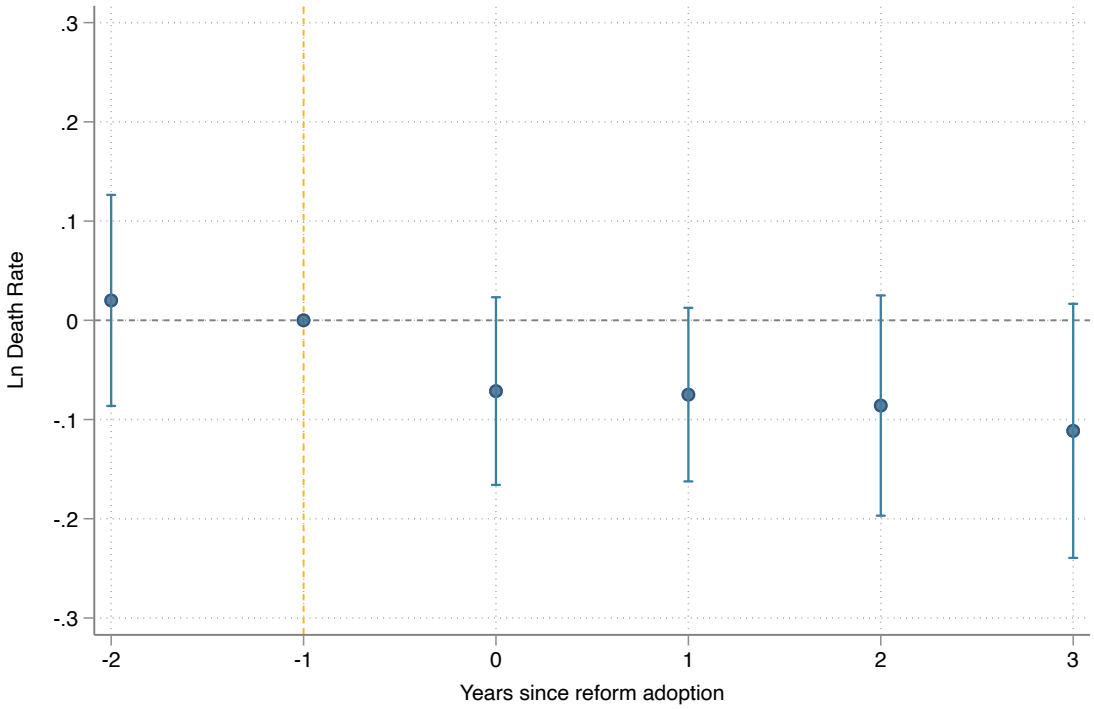
Notes: This figure plots a spikeline with the share of patients in each cell that are discharged exclusively from one hospital. A cell is defined by patients' county of residence, age group (less than 1 year old, between 1 and 15 years old, and more than 15 years old) and diagnostic (as reported by the hospital from which they are discharged). If referral guidelines are strictly followed, we should expect all patients' within a cell to attend the same hospital. However, in our data, patients within the same cell could be discharged from different hospitals due to the fact that we do not observe the diagnostic at the primary care center, but only at the hospital. Likewise, it may be due to the fact that we only observe patients' home address but they could have used their work addresses to register in the health system.

Figure A.10: Differential effects of CEO transitions



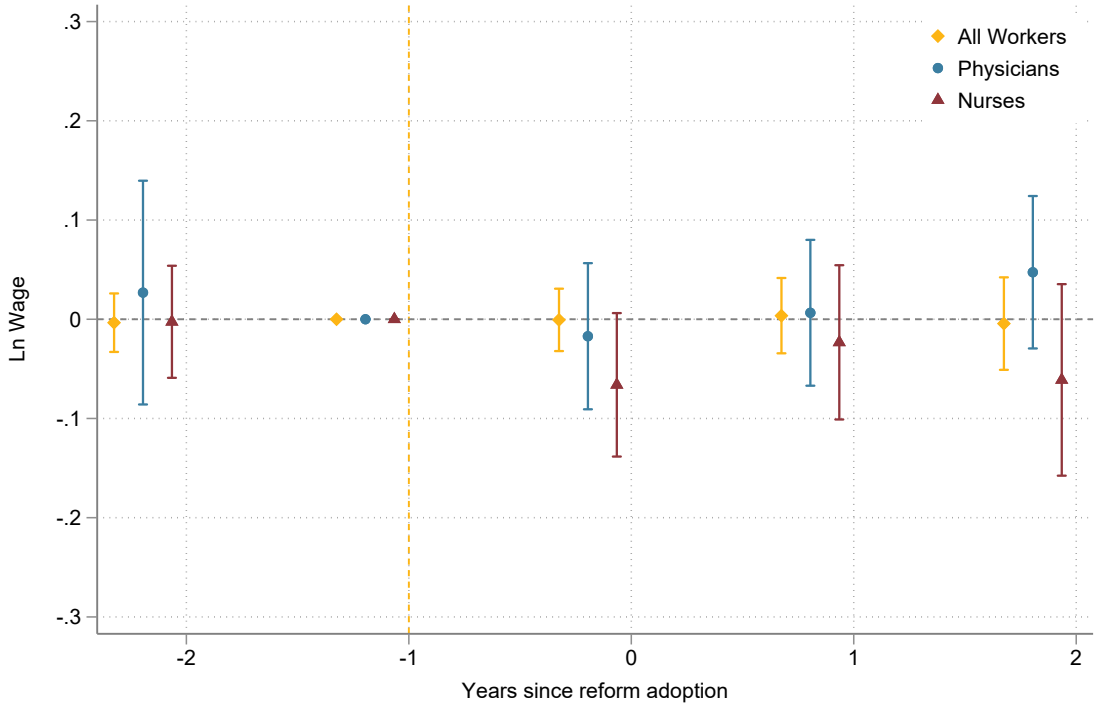
Notes: This figure presents the coefficients of the stacked event study specification in equation 3, where an event is a transition from a CEO without management studies to a CEO with management studies. For each transition event, we define a time window around it and a control group of hospitals with no transitions in the time window. We define a set of valid events as those that are balanced in the time window and do not overlap with another transition in the pre-period within the time window. We also exclude the transitions associated with the first time that a CEO was appointed after the selection reform was adopted in a given hospital. In total, there are 94 valid CEO transitions, as described in Online Appendix Table A.3. The dependent variable is the death rate at the hospital level in a given quarter. Dots indicate estimated coefficients and vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at the hospital level.

Figure A.11: Effect of the reform on hospital performance (yearly level aggregates)



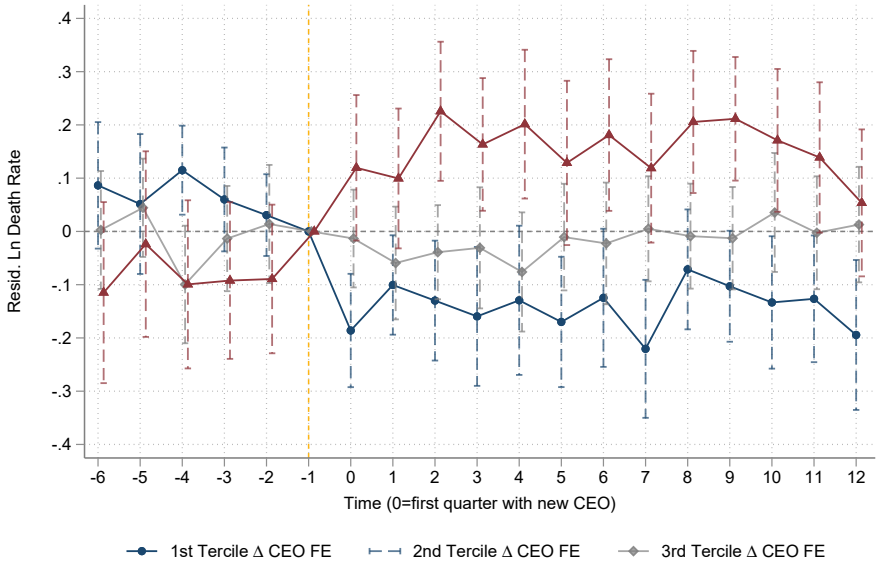
Notes: This figure presents event study evidence of the reform's effect on death rates when the outcome is logged hospital death rates at the yearly level and using data only from 2009 onwards. Each dot corresponds to an estimated coefficient, and the vertical lines indicate the corresponding 95% confidence intervals. The yellow dashed line represents the omitted coefficient. Standard errors are clustered at the hospital level.

Figure A.12: The reform does not affect other hospital employee wages

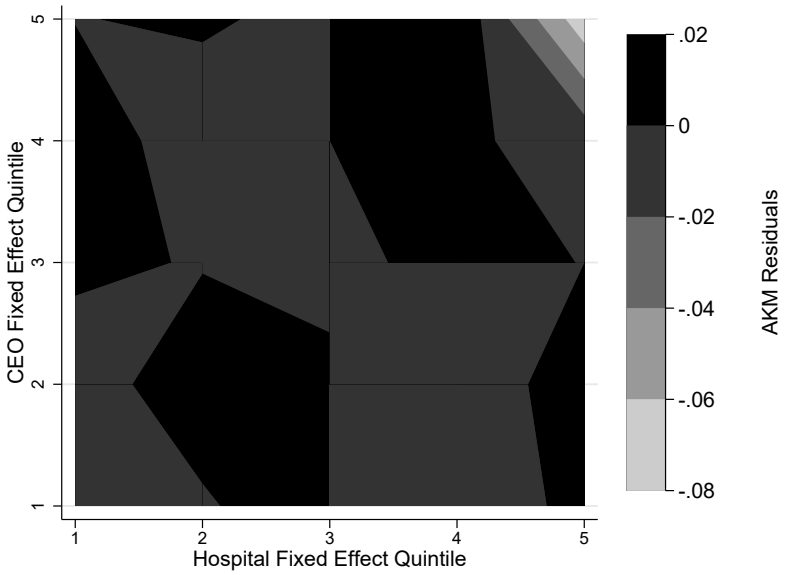


Notes: This figure shows the effect of the recruitment reform on hospital employees' wages. The variation is aggregated at the annual level, and the outcome variable consists of end-year wages between 2011 and 2019. Diamonds, dots, and triangle markers represent estimates for all workers, physicians, and nurses, respectively. The vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at the hospital level.

Figure A.13: Threats to the identification of managerial talent



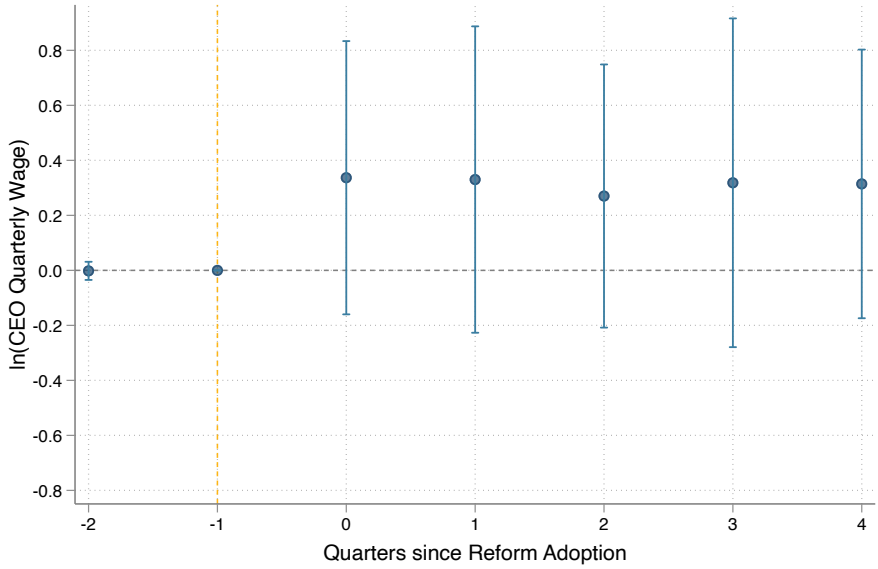
(a) Switchers



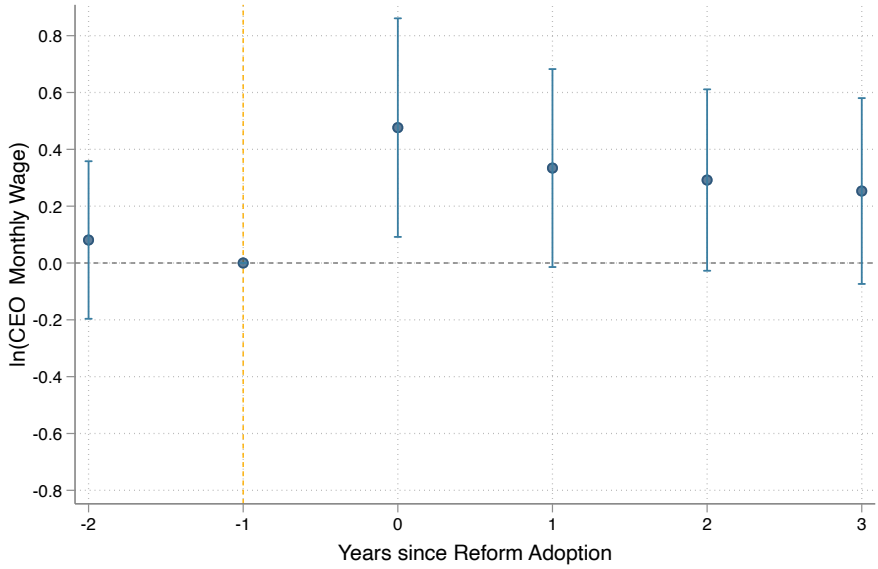
(b) Residuals

Notes: This figure presents evidence against potential endogenous mobility of managers and in favor of the additive separability assumption between the hospital and manager component. Panel A plots the mean (residualized) log death rate against event time (relative to change in CEO events). The figure plots three types of leadership transitions, classified by terciles of the change of managerial ability: (1) an overall increase (in blue with dot markers), (2) an overall decrease (in red with triangle markers), and (3) no significant change (in gray with diamond markers). Each dot, triangle, and diamond marker correspond to an estimated coefficient, and the vertical lines indicate the corresponding 95% confidence intervals. Panel B shows mean residuals from model 5 with cells defined by quintiles of estimated manager effect, interacted with quintiles of estimated hospital effect.

Figure A.14: Impact of recruitment reform on wages



(a) 2014-2019: Quarterly



(b) 2011-2019: December only

Notes: This figure presents the impact of the reform on hospital CEO wages. The empirical design leverages the gradual adoption of the selection reform across hospitals. Panel A presents the results using quarterly panel data between 2014-2019. Although the estimates are noisy due the small number of events, the estimate is stable around 33%. Panel B uses data for December between 2011-2019, which allows us to leverage a larger number of events. Regressions controls include age and a dummy indicating whether the individual is a doctor, which affects pay in the public sector. Dots indicate estimated coefficients and vertical lines indicate the corresponding 95 percent confidence intervals. Standard errors are clustered at the hospital level.

Table A.1: Referral guidelines example

Health Service Name	*Metropolitano Norte*		*Metropolitano Oriente*	
Primary Care	CESFAM Colina (1)	CESFAM Esmeralda (2)	CESFAM Aguilucho (3)	CESFAM La Faena (4)
Pediatrics				
Pediatric respiratory diseases	2	2	4	4
Internal Medicine				
Cardiology	1	1	5	4
Medical Oncology				
< 15 years	2	2	7	7
> 15 years	3	3	5	5
General Surgery				
Thoracic Surgery	3	3	6	6

1: Complejo Hospitalario San José;
2: Hospital Clínico De Niños Roberto Del Río;
3: Instituto Nacional Del Cáncer Dr. Caupolicán Pardo Correa;
4: Centro de Referencia de Salud Cordillera Oriente;
5: Hospital Del Salvador;
6: Instituto Nacional del Torax;
7: Hospital de Niños Dr. Luis Calvo Mackenna.

Notes: This table illustrates the referral guidelines from primary public care to public hospitals. Referrals depend on the primary care center and the demographics of the patient. Columns (1)-(2) and (3)-(4) are in two different Health Services, Metropolitano Norte and Metropolitano Oriente, respectively. The numbers are the hospital to which the patient is referred. For example, a patient for medical oncology older than 15 years old in CESFAM Colina is referred to "Instituto Nacional del Cáncer Dr. Caupolicán Pardo Correa".

Table A.2: Impact risk-adjusted mortality measures

	$\ln(y_{ht}/\hat{y}_{ht})$		
	(1)	(2)	(3)
1 if selection process adopted	-0.086***	-0.090***	-0.089***
	(0.023)	(0.024)	(0.024)
Observations	8,104	8,104	8,104
Time FE	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes
Case Mix Controls	Yes	Yes	Yes
Type of Insurance	No	Yes	No
Enhanced Elixhauser Comorbidity Index	No	No	Yes
Pseudo-R^2 Logit	0.147	0.158	0.176
# of Hospitals	181	181	181
Mean Dep. Variable	0.780	0.712	0.737

Notes: This table presents our estimates of the impact of the selection reform on risk-adjusted death rates. The estimates are from the staggered differences in differences specification in equation 1. We define risk-adjusted death rate as the actual hospital-level death rate divided by the average death rate as predicted by different patient-level characteristics used to fit a logit model for deaths See Online Appendix B for details.

Table A.3: CEO transitions according to management studies

	Current CEO has:			
Previous CEO had:	Non Mgmt. Studies (1)	Mgmt. Studies (2)	No Data (3)	*Total*
Non Mgmt. Studies	431	94	5	530
Mgmt. Studies	95	66	4	165
No Data	31	4	4	39
Total	557	164	13	734

Notes: This table presents the number of CEO transitions according to the characteristics of the incumbent and incoming manager in terms of management studies. We only consider transitions for which there is a time window of 4 periods before and 12 periods after the transition, and that do not overlap with another transition in the pre-period within the time window.

Table A.4: No differential effects according to performance pay scores

	Ln Death (%) (1)	Ln Death (%) (2)
Reform	-0.087***	
	(0.028)	
Reform & High Score		-0.086**
		(0.033)
Reform & Low Score		-0.089**
		(0.036)
Observations	7,670	7,670
Time FE	Yes	Yes
Hospital FE	Yes	Yes
Case Mix Controls	Yes	Yes
# of Hospitals	181	181
Mean Dep. Variable	2.61	2.61
p-value *High Score = Low Score*		0.94

Notes: This table examines differential effects of the recruitment reform depending on the CEO's average performance score. Their performance score is measured according to the performance contracts. In column (1), we replicate the estimation of 1 in the sub-sample for which we have performance scores. Column (2) interacts the reform with whether the CEO scored above or below the median in the performance score outcome. Standard errors are displayed in parentheses and are clustered at the hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Correlation between manager fixed effect and manager characteristics

	CEO's Fixed Effect			
	(1)	(2)	(3)	(4)
Female	-0.003	-0.005	-0.005	-0.033
	(0.039)	(0.039)	(0.039)	(0.041)
Age	0.031***	0.030***	0.031***	0.035***
	(0.009)	(0.010)	(0.010)	(0.012)
Age^2	-0.000***	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Physician		-0.013	-0.011	0.047
		(0.058)	(0.059)	(0.063)
Mgmt. Background		-0.015	-0.015	-0.013
		(0.066)	(0.067)	(0.067)
Physician × Mgmt. Studies			-0.019	-0.014
			(0.055)	(0.058)
Avg. Test Score				-0.018
				(0.028)
Constant	0.082	0.100	0.093	-0.011
	(0.205)	(0.237)	(0.241)	(0.275)
Observations	688	688	688	582
R-squared	0.418	0.418	0.418	0.425

Notes: This table presents the correlation between the manager fixed effects estimated from equation 5 and manager characteristics. These characteristics include gender, age, age^2, and a set of indicators for educational attainment. "Mgmt. Background" refers to undergraduate studies in management while "Mgmt. Studies" includes both undergraduate and postgraduate studies related to management. Controls include connected set fixed effects. Robust standard errors in parentheses.