Managers and Public Hospital Performance*

Cristóbal Otero† Pablo Muñoz‡

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Abstract

We study whether, and how, managers can increase government productivity in the context of public health provision. Using novel data from public hospitals in Chile, we document that top managers (CEOs) account for a significant amount of variation in hospital mortality. We then use a staggered difference-in-differences design, and show that a reform which introduced a competitive selection system for recruiting CEOs in public hospitals reduced hospital mortality by around 10%. The effect is not explained by a change in patient composition and is robust to several alternative explanations. The financial incentives included in the reform—performance pay and higher wages—do not explain our findings. Instead, we show that the policy changed the pool of CEOs by displacing older doctors with no management training in favor of younger CEOs who had studied management. The mortality effects were driven by hospitals in which the new CEOs had managerial qualifications. These CEOs improved operating room efficiency and reduced staff turnover.

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1 Introduction

Government spending on publicly provided goods and services more than doubled between 1980 and 2019 and explains around 30% of global GDP (Gethin, 2022). 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 goods and services provided by the state 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; Fenizia, 2022). 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 top managers (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. On the one hand, 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] Public hospitals, on the other hand, 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

[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.

positions had been appointed.

To study CEO impact on 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 CEOs 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. To measure hospital performance, we rely on nationwide individual-level inpatient discharges 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 CEOs' characteristics and tenure in every public hospital.

We begin by documenting that CEO managerial talent matters for hospital performance. 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) and show that CEOs can account for a significant amount of variation in mortality rates across hospitals. We follow the approach pioneered by Bertrand and Schoar (2003), and gradually include hospital and CEO fixed effects in a regression on hospital mortality. Inclusion of CEO fixed effects increases the R^2 by a similar magnitude to that in Bertrand and Schoar (2003) for CEOs in publicly traded US firms and in Fenizia (2022) for managers in the administrative public sector in Italy.[3] Having established *prima facie* evidence that CEO managerial talent matters for hospital performance, we next study a reform that introduced a competitive and transparent selection process for appointing public hospital CEOs.

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 show that the reform decreased death rates around 10% in public hospitals in the 3-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 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 identification assumption of the staggered difference-in-differences model, namely that hospitals that adopt the selection reform are not on trends that differ from those that have not adopted it. We justify this assumption in several ways. First, we show that pre-reform, the growth of an exhaus-

[3] We also exploit the rotation of CEOs across hospitals and estimate a model with CEO and hospital fixed effects. This procedure allows us to compute measures of managerial talent (CEO fixed effects) and to decompose the variance of mortality and quantify the contribution of fixed hospital characteristics and CEO talent.

tive list of variables, including hospital outcomes, patient characteristics, and political variables, does not differ between hospitals that adopt and do not adopt the reform. 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 concern regarding an Ashenfelter-style dip, which is a natural threat in settings in which 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 this concern. 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; Decreto 38). Consistent with the setting's features, we do not find any evidence of impacts of reform adoption on the hospital-level risk scores predicted based on patients' demographics and diagnoses. Second, in our baseline estimates we use an exhaustive set of case mix controls that include detailed information on 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 regarding selection on unobservables, we examine the effect of the reform on deaths outside treated hospitals. To the extent that patients rejected by 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 we focus exclusively on "locked-in" patients who cannot access healthcare in the private sector or when we restrict the analysis to the set of patients that followed the referrals mandated by the health system.

Consistent with the previous results, we find that newly appointed managers increased hospital performance by more efficient use of medical resources and better personnel practices. We find that the reform increased operating room utilization by 25% three years after, a large effect, but not large enough to close the gap between the average efficiency in high-complexity hospitals in our

setting and the average efficiency in the National Health System (NHS) of the United Kingdom. In line with this finding, we also show that surgical procedures increased by a similar magnitude; and that most of this increase was led by surgeries in diagnoses amenable to death prevention through surgery (e.g., Cardiac Arrhythmia, Renal Failure, Metastatic Cancer). Despite null effects on workers salaries, the reform significantly reduced the turnover of doctors. These results are consistent with findings in Bloom et al. (2015), who document a positive association between improved management practices, hospital performance, lower waiting time, and reduced staff turnover, and are also in line with recent research on personnel economics, which documents that better-managed firms retain workers with higher human capital (Bender et al., 2018).

Second, we examine the impact of CEO financial incentives included in the recruitment reform—performance pay and higher base wages—and find no evidence of their driving the results. We start by ruling out the possibility 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 also document that the policy increased CEO wages by around a third relative to pre-reform wages. To examine whether higher CEO wages had an effect on managerial performance, we leverage an amendment to the reform that increased the pay for managers who are doctors *and* who were appointed after November 2016. We find that the amendment significantly increased wages for treated managers but had no discernible effects on their performance. We interpret these results as evidence that efficiency wages do not drive our main findings.

Our third finding is that the reform dramatically increased the share of CEOs with management education, which we show is the main predictor of the efficacy of the reform on reducing hospital mortality. We first document that the reform replaced doctors who worked as CEOs ("doctor CEOs" for short) with CEOs who have undergraduate degrees in management-related majors.[4] Before the reform, 93% 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 doctor CEOs with management studies but had a larger negative effect on the ones with out this type of training. We find suggestive evidence that through this channel, the reform also had an effect by incentivizing doctors who wanted to apply for a CEO position to further invest in management studies. Overall, we find that, as soon as it was adopted, the policy boosted the percentage of CEOs having management training by over 40 percentage points. We then show that the reform increased

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

CEO managerial talent—as proxied by their estimated CEO fixed effect—, but had no significant effect on skills—as measured by standardized college entrance exams scores. We also find that new managers are around 2 years younger and that the reform had no effect on the likelihood that the CEO is female.

Motivated by these findings, we next examine whether correcting the mismatch between CEOs' skills and the skills demanded by the job enhanced the organization's performance. We interact the reform with CEO managerial training and find that the effect on hospital mortality is mostly explained by CEOs with an educational background in management. To further explore this result, we estimate stacked event studies leveraging all CEO transitions from CEOs without management qualifications to CEOs with managerial studies.[5] We find that these transitions consistently produce a significant and large mortality drop, while transitions between CEOs with no management training have zero effect on hospital performance. Note, however, that since we do not measure the ceteris paribus effect of a management degree on performance, this could be explained by differential selection (i.e., better managers are more likely to study management) and not necessarily as a causal effect of management training.

We conclude by discussing plausible underlying frictions that could support the paucity of CEOs with management training we observe prior to the implementation of the reform. The social norm was that CEO positions were reserved for doctors: Before the reform, doctors made up 98% of public hospital CEOs, and by 2019, the fraction of doctor CEOs was 53%. Based on qualitative evidence, we posit that the social norm was upheld by a shared belief across doctors that individuals with no medical training should be restrained from CEO positions and only be permitted to advance to middle management. According to them, there is an insurmountable information asymmetry between the medical staff and non-doctors, which renders non-doctors unqualified to make the final decision about how to run a hospital. Furthermore, biased beliefs about the importance of management could have discouraged doctors from investing in management training.

This paper contributes to multiple strands of the literature. We contribute to the literature on 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, 2022; Dahis et al., 2022). To the best of our knowledge, this is the first paper to evaluate the impact of state personnel selection processes on health outcomes. More broadly, our research also contributes novel evidence on bureaucratic effectiveness and its impact on development (see Besley et al. (2022) for a review).

[5]To avoid a mechanical impact of the reform, we exclude transitions induced by the reform adoption.

Our work also adds to a nascent literature studying the impact of top managers (Choudhury et al., 2019; Limodio, 2021; Fenizia, 2022; Best et al., 2022) and management practices (Bloom et al., 2015; Tsai et al., 2015; Rasul and Rogger, 2018; Azulai et al., 2020; Bloom et al., 2020) on organization-level performance in public sector organizations. Closest to our work, Janke et al. (2020) study the impact of CEOs in NHS hospitals in the UK and document little evidence of CEOs' impact on different dimensions of hospital production. In contrast to our setting, CEO recruitment in NHS hospitals does not have a strict selection criteria and is delegated to local boards. Interestingly, in the NHS only a quarter of CEOs hold postgraduate management education, which is the same to the average in our setting before the selection reform adoption within hospitals that adopted it. The reform increased the share of CEOs with postgraduate management qualifications to almost 60% the quarter after adoption.[6] By studying a selection reform that dramatically shifted CEOs' management training, we also provide novel evidence on the impact of correcting top managers' skill mismatches in public sector organizations.

This paper also contributes to the literature on management styles (e.g., Hambrick and Mason, 1984; Bertrand and Schoar, 2003). In contrast to recent work by Acemoglu et al. (2022) that focuses on private sector firms, we find null effects of appointing CEOs with management studies on the wages of public employees. Instead, we document that performance was improved in hospitals in which the new CEOs had management training.[7] Moreover, our study of the impact of CEOs on personnel turnover adds to existing work on management and employee attrition (Hoffman and Tadelis, 2021), and underscores its importance for high-skilled workers (Bender et al., 2018; Gosnell et al., 2020). Finally, our work complements previous research studying how to improve OR efficiency (e.g., He et al., 2012) and the efficacy of alternative policies to improving hospital performance (e.g., 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 as follows. Section 2 briefly describes the setting and data, provides descriptive evidence of CEO impact on hospital performance, and introduces the recruitment reform. Section 3 presents the main effects of the reform on hospital mortality, and discusses the validity of the results and potential mechanisms. Section 4 examines the effects of the financial incentives included in the reform. Section 5 examines the recruitment effect of the reform on managers' characteristics. Section 6 concludes and the Appendix provides additional results.

[6]The share increases up to 66% if we also consider undergraduate management degrees.

[7]In terms of methodology, our paper departs from previous field experimental literature focusing on management practices Bloom et al. (2015) and connects with the literature leveraging CEO turnover such as Bennedsen et al. (2020) and Acemoglu et al. (2022).

2 Setting, Data, and Descriptive Evidence

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 employees. Individuals can opt-out and use their health contributions to buy private insurance.[8] 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 are covered under special regimes exclusive to the police and armed forces.[9] The ability of individuals to freely use their health contribution to buy private coverage has induced stark market segmentation, because 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 sectors,[10] 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.[11] Individuals need to register in the healthcare center that provides primary care in their local area and patients who need specialized attention are referred to specialty clinics or a hospital. Referrals follow strict guidelines, mostly based on the geographic location of the patient's primary care center (Ley 19,937). 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 ad-

[8]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).

[9]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).

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

[11]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.

ministrative units within which the referral and counter referral system is organized. These are decentralized organizations subject to oversight by the Ministry of Health, and each is responsible for the articulation, management, and development of public healthcare establishments in municipalities in their territory. This 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 within their scope.

The head of the Health Service is also the immediate superior of CEOs of public hospitals within their territory. CEOs are in charge of the management, organization, and administration of their hospital, and their duties include, among others, (i) the administration of personnel, (ii) the allocation of hospital inputs and human resources, (iii) the management of financial resources and proposing the annual budget, (iv) infrastructure and technological equipment resources decisions, and (v) integration of the hospital into the health network and with the community, among others.

2.2 Data Sources

For this paper, we build a novel dataset that identifies the CEO in every public hospital in the country, spanning every month between January 2001 and December 2019. Because these data were not available in a systematic way, we filed nearly 1,000 Freedom of Information Act (FOIA) requests to local hospitals and health authorities—who, in many cases, had to collect archived 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 by the Civil Service, LinkedIn profiles, articles from local newspapers, and information provided by universities, among others. Finally, via a series of FOIA requests to the Civil Service, we also have access to pay-for-performance agreements and job performance scores for CEOs appointed under the new selection system.

We also access restricted-use administrative records that cover the universe of employees in all public hospitals between 2014 and 2019. The data are 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, the person's job (and, in the case of doctors, their specialty), number of hours worked, date of birth and gender, and a detailed wage breakdown.

In terms of hospital performance, we use detailed administrative 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, which encompasses almost 29 million events. Data include the diagnosis according to the 10th edition of the International Classification of Diseases (hereafter, ICD-10 codes); the type of admission (e.g., emergency case or referral); 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. We link the data at the individual level with country-wide death records processed by the Vital Records Office between 2001 and 2018, which cover more than 1.5 million deaths. We observe the date, cause, and place of death. Finally, we also collect a host of inputs and procedures at the hospital level, such as the number of medical beds, the number of surgeries, and hours of operating room use, 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 that describe 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 use data on all recruitment processes conducted 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 in which the agency and the position are based.

2.3 Hospital mortality and CEO 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 of the analysis. The Chilean public health setting is well suited for this analysis because the selection of patients is limited by the institutional design. Public hospitals receive patients following strict referral guidelines based on the patient's county of residence, age, and diagnosis. Also, hospitals cannot reject patients or discretionally counter-refer them to other hospitals, and must abide by the protocols.[12] We provide further details in Appendix A.

[12]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.

We start by studying the extent to which variation in hospital quality can be explained by CEO managerial talent. Specifically, we compare the adjusted R^2 estimated from a regression of the logarithm of death rates on different sets of explanatory variables including CEO and hospital fixed-effects. We report the results in Table 1. 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), which implies that hospital effects account for substantial variation in the outcome. It further increases to 0.76 in column (4) after inclusion of CEO fixed effects—an increase of similar magnitude to that reported by Bertrand and Schoar (2003) and Fenizia (2022).[13] Formally, an F-test strongly rejects the null hypothesis that all the CEO effects are zero (p-value=0.00).

In light of research casting doubt on this type of approach (Fee et al., 2013), in Appendix B we also assess the relative importance of hospitals and managers estimating a two-way fixed effects model, which allows us to perform a variance decomposition analysis.[14] We identify the model using the rotation of CEOs across hospitals, in the same spirit as the rotation of workers identifies worker and firm fixed effects in Card et al. (2013).[15] Using bias-corrected measures of the variance components (Andrews et al., 2008), we find that CEO fixed effects explain 44% of the variation in mortality after accounting for case-mix and time effects, a magnitude similar to that of the permanent component of productivity associated with different hospitals (54%). We also find that the (bias-corrected) covariance between CEO and hospital fixed effects is negative, suggesting the best managers work at the least productive hospitals.

2.4 The Recruitment Reform

In 2003, a political scandal exposed illegal payments to top government officials. In response to and as a product of broad political consensus, Congress enacted Law Nº 19,882, which created 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 gov-*

[13]This finding stands in contrast to Janke et al. (2020) who—in the context of public hospitals in the English National Health Service (NHS)—document lack of CEO effects in hospital production, despite substantial and persistent differences in their pay.

[14]This model also provides us with estimates of CEO fixed effects, which are a useful measure of managerial talent we use throughout the paper.

[15]Models with additive hospital and manager components may raise some concerns. One may worry, for instance, that managers are assigned to hospitals on the basis of unobserved factors that determine their comparative advantage. It could also be that manager rotation is correlated with hospital-specific trends. Following Card et al. (2013) we empirically test these concerns and find no evidence to support them.

ernment 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."[16]

The reform has two main components. First, it changes the recruitment process for 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 selected through public, competitive, and transparent competitions.

The job announcement for a top management position starts with the position's being posted online on the Civil Service's website and in a newspaper with national circulation. In some cases, the Civil Service may also hire headhunters to widen the pool of applicants. Applicants must have a professional degree and, depending on the position, other competencies may be desired. After the job posting closes, 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 candidates' motivation and overall competencies. The consultant assigns every applicant a grade based on an objective rubric and provides a short list to the Civil Service. In the next phase, a committee formed by representatives of the Civil Service and the Ministry in which 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.[17] In the case of public hospital CEOs, we document that the reform increased the position's pay by 33% (see Appendix E for further details). The financial package also includes a performance pay 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 4.

Adoption of the recruitment process occurred gradually across public agencies over time. The law mandated that between 2004 and 2010, the Ministry of Finance had to determine a minimum

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

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

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 conducted 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.

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 oversee 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 include, among others, hiring a certified 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.

In the case of public hospitals, adoption is mainly driven 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 when the Ministry of Finance approves the recruitment process for a given position, it only takes effects 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 adopted it gradually over time. In total, 88 out of 188 hospitals adopt the selection reform during the time window of the study.

2.5 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 hospital-by-quarter level for the analysis. Aggregating the data for each hospital at 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. We start by constructing death indicators at patient level following a hospitalization event. We merge inpatient and death records, regardless of whether death 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 who 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 for performing 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 2 presents descriptive statistics. In our sample, 33% and 15% 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% to inpatients younger than 29 years. Importantly, 96% of 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 after admission death rate—which considers both in- and out-of hospital deaths—is larger and corresponds to 4.21%. Finally, regarding emergency room admissions, the average death rate for ER patients is 3% when considering all diagnoses and 12.2% when considering only ER admissions with an acute myocardial infarction diagnosis.

3 The Reform's Impact on Hospital Performance

3.1 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. This feature 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 hospitals that never adopted it (never-treated). For ever-treated, we consider a window of six quarters before adoption. Column (1) in Table 3 shows the average at never-treated hospitals for a set of variables related to patient demographics, hospital outcomes, and political outcomes at the hospital's municipality. 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 who are slightly younger and less likely to use public health insurance; they are also located in municipalities that exhibited more support for right-wing politicians in the 2004 mayoral election.

To assess whether adopting the reform is associated with hospital characteristics that trend 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. We do not observe that treated units are on significantly different trends than never-treated hospitals, in terms of patient composition, hospital outcomes, or political determinants. We consider these results as supporting evidence for our research design.

3.2 Main Results

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

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

where y_{ht} is an outcome variable at hospital h and quarter t level, and $Reform_{ht}$ is a dummy variable that takes value 1 if a hospital adopts the new selection process and 0 otherwise. Recall that once a hospital selects a CEO via the new recruitment system, it has to select all future managers using the same recruitment system. Thus, 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.

To account for differences on patient characteristics, we follow the literature and include X_{ht}, a comprehensive set of hospital-by-quarter level variables that pick up differences in case mix characteristics (Propper and Van Reenen, 2010; Gaynor et al., 2013). Specifically, the vector X_{ht} includes the share of female inpatients, the share of inpatients within each of 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 interaction of the various shares. To control for the socioeconomic status of patients, X_{ht} also includes the share of inpatients with each type of health insurance. We cluster standard errors at hospital level, which is the treatment-level unit. The coefficient of interest is β, which 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.[18] In Table 4 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 after admission, and includes both in- and out-of-hospital deaths. Reassuringly, the point estimate shows that effects are not driven by higher out-of-hospital deaths. Column (5) focuses on emergency admissions, which should be less prone to non-random sorting, and finds a similar impact of the reform in this sample. Finally, in column (6) and (7) we use a Poisson model to estimate the effect of the policy on the number of deaths. Column (6) focuses on all deaths and shows 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). Column (7) shows that the death rate among emergency cases with acute myocardial infarctions (AMI, commonly known as "heart attacks") decreased by around 14.6%, although this coefficient is more imprecisely estimated.[19]

3.3 Validity of Results and Alternative Explanations

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

Testing for Parallel Trends: Event Study Evidence

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

[18] For hospitals that had a CEO turnover, we include a window of 6 quarters before and 12 quarters after reform adoption to facilitate study of the timing of the effect.

[19] Note that the number of observations drops from 8,104 to 1,956. Following the literature, we define AMI deaths as deaths that occurred 28 days after admission of patients (through the emergency room) with an 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.

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 that indicates the reform was adopted k periods earlier (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 quarter, relative to the quarter before 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 measures of hospital-level death rates. When inspecting the dynamic effects of reform adoption, we observe that—across all panels—the pre-period estimates tend to be small, around zero, and not significant, which indicates that treated and control units were not on different trends prior to reform adoption. Furthermore, after the reform, the estimates turn negative and significant and increase gradually. In this case, it does not seem that the change in management is driven by a previous worsening (improvement) in managerial performance, which would overestimate (underestimate) the true impact (if any) of the treatment. In Appendix Figure A.5, we conduct 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 log of predicted death rates and on the ratio of actual over predicted death rates using a two-way fixed effects model (Panel B). Finally, in Appendix Figure A.6 we present estimation results using the models suggested by 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. Results are robust and follow the same dynamic trajectory regardless of estimation strategy.

Risk-Adjusted Results

To ease selection concerns, we also estimate the impact of the reform on the actual over the predicted death rate and only on the predicted death rate as a placebo exercise. We follow the prediction procedure used by the CMS (Ash et al., 2012), which predicts the likelihood of death at the individual level on a detailed set of patient characteristics. 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) to obtain "predicted" death rates at the hospital level. Finally, to

ease interpretation and following the UK's NHS (Health and Centre, 2015), we construct a "risk-adjusted mortality rate" that divides the actual hospital-level death rate by the predicted death rate, such that an increase (decrease) from one means more (fewer) deaths than predicted deaths.[20]

Table A.2 shows the robustness of our result to using alternative risk-adjusted measures. Columns (1)-(3) show 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's 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. We find that the policy had no effect on the predicted death rate, but a significant impact on the ratio. After the new CEO selection process was adopted, the ratio of actual over predicted death rates decreased by 8% from a base of 0.79 in our estimation sample. Results are stable regardless of the incorporation of more covariates in the logit model. This is reassuring because, according to recent research that leverages 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).

Testing for Patient Selection

The risk-adjustment procedure is fundamentally based on 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.[21] 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.

Careful consideration of our setting's characteristics suggests that 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 the hospital CEO could manipulate diagnoses. On the other hand, the law forbids CEOs

[20] We provide further details of the the risk-adjustment procedure in Appendix C.

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

from selecting patients based on their condition and must adhere to referral and counter-referral guidelines. Furthermore, we can empirically assess these three concerns by examining whether hospital-level risk-score changes upon a new manager appointment. For this purpose, we use the patient-level data to fit a logit model of (pre-reform) mortality on patients' demographics and diagnoses (for details, see Appendix C). Then, we predict the probability of death for each patient, and use these predictions (i.e., patient level risk scores) to construct hospital-level predicted death rates and number of deaths. We estimate Equation 1, but now replace the dependent variable with these predictions. Figure A.7 plots event study evidence on the null effects of the reform on mortality predicted based on patients' risk-scores.

Although our results are robust to adding case mix controls and using risk-adjusted mortality measures, there could be selection on unobservable patient characteristics that is not picked up by diagnosis data or the list of available patient characteristics. For instance, perhaps managers are able to reject sicker patients in a way that does not change patient composition (supply-side selection on unobservables), or healthier patients are more likely to go to a given public hospital if they observe that public hospital performance is improving (demand-side selection on unobservables).

To indirectly test whether supply-side selection on unobservables lead our estimates to be upward biased, we consider the impact of the reform on mortality rates in 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 geographic area. For this exercise, we estimate Equation 1 again but now use as dependent variables the at-home death rate (in the municipality where each hospital is located) and the in-hospital death rate in nearby hospitals. Panel A in Figure 3 shows our results, with 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 nearby hospitals.

Finally, to examine whether sorting on unobservables is biasing our results, we exploit two features of our setting. First, we leverage the fact that a set of individuals are locked-in in the public health network, i.e., lower-income individuals receive public insurance for free but cannot use it in private providers. Second, we can use our data to empirically identify the set of patients that comply with the referral guidelines described in Appendix A. For this analysis, we estimate Equation 2 using smaller samples comprised exclusively of locked-in patients and referrals compliers patients. The results from this approach—which should mute demand-side sorting if any—are presented in Figure 4. Reassuringly, we in both restricted samples we find a similar impact of the reform on hospital performance.

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 for 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.[22] 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:

$$y_{hte} = \alpha_{he} + \gamma_{te} + \sum_{k=-4}^{12} \beta_k D^k_{hte} + \epsilon_{hte}, \tag{3}$$

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

Figure 5 presents the effect of a CEO transition on death rates. 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 the CEO transition itself.

3.4 How is the Reform Improving Hospital Performance?

Now we ask what are the underlying mechanism that explain the drop in mortality that we observe after the adoption of the reform. We focus on the efficient use of hospital resources and personnel practices. Most data used for this analysis is only available at yearly frequency. Thus, we perform

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

the analysis at this level of aggregation. For completeness, in Appendix Figure A.8 we present the effect of the reform on mortality rates using this level of aggregation.

Operating rooms (ORs) are one of the most critical hospital resources and typically account for more than 40% of total expenses (Association et al., 2003; Denton et al., 2007; Guerriero and Guido, 2011). Inefficient use of ORs is extremely costly for patients and can impact overall hospital performance.[23] The efficient use of ORs is a highly complex operational and management problem, and management practices are a crucial lever for improving OR efficiency (see, e.g., He et al., 2012).[24] Another critical element for hospital performance is high-skilled personnel. Recent literature in personnel economics shows that better-managed firms recruit and retain workers with higher human capital (Bender et al., 2018). These reasons lead us to examine OR efficiency and personnel practices as mechanisms through which new managers improved hospital performance after the reform.

We find that the recruitment reform had a significant and economically meaningful effect on OR efficiency. We run the same specification as in Equation 2 on the logged ratio of OR utilization (i.e., the number of hours ORs are used) to OR capacity (i.e., the aggregate available number of OR hours). Panel A in Figure 6 shows the effect of the reform on the ratio between OR utilization and capacity. We find that the reform did change the number of hours ORs are effectively used: 3 years after the reform adoption, the number of OR hours used increased by 25%. Although this number might seem big at face value, it is not large enough to close the gap between the average efficiency in high-complexity hospitals and the average in the UK's NHS.[25]

Panel B in Figure 6 examines the other side of the coin of higher OR usage, the number of surgeries performed. We find that the number of surgical procedures increased by a magnitude similar to utilization of ORs (yellow diamonds). To deepen this analysis, we follow an agnostic procedure to classify diagnoses as amenable to death prevention through surgery. Specifically, we use our patient-level records to estimate several logit regressions of (pre-reform) mortality on surgery

[23]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 the inefficient use of ORs is that hospitals can treat fewer patients, and hence patients face longer waiting times (Durán et al., 2017).

[24]For instance, planning and scheduling must consider OR availability and match the workload to medical staffing, the material resources required, and the availability of post-surgical recovery beds (Wang et al., 2021). Furthermore, OR planning and scheduling must incorporate the uncertainty entailed in surgery duration and emergent admissions that require a surgical procedure (Latorre-Núñez et al., 2016).

[25]Out of 9 hours of daily capacity, the average in a sample of high-complexity hospitals in Chile is 4.8 hours and in the NHS is 6.4 hours (CNEP, 2020).

indicators while controlling for patients' demographics. We estimate one regression for each of the 31 categories of the enhanced Elixhauser comorbidity index Elixhauser et al. (1998); Quan et al. (2005) and then based on the estimated coefficient for the surgery indicator, we classify the diagnoses into amenable to death prevention through surgery if $z \leq -2.576$. As shown by Figure 6, Panel B, we find that most of the increase in surgeries was led by a surge in surgeries related to diagnoses amenable to death prevention through surgery (e.g., Cardiac Arrhythmia, Renal Failure, Metastatic Cancer).

We examine the reform's effects on personnel turnover and wages. For this, we use administrative data on hospital personnel coming from the Human Resources Information System used for HR purposes by the Ministry of Health. We run the same specification as in Equation 2 on the likelihood that a worker will leave the next period (either job to job or job to unemployment transitions) and on their logged hourly wages. Panels C and D of Figure 6 show our results. We find that the reform reduced the turnover of doctors, but it did not change wages, which is expected given that in the public sector, wages are rules-based. From anecdotal evidence based on conversations with managers and doctors in the public sector, we posit that the reduced turnover rate might be explained by unobservable benefits and amenities that the manager can negotiate with doctors.

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 the literature and in ours after we match it according to patients' characteristics. Note, however, that although we can match the sample of patients in, say, age-bracket and type of admission, patient composition will still differ across settings. Comparisons should thus serve as a benchmark and not as a horserace competition between policies. Results are summarized in Table 5.

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 receiving higher payments from Medicare. They find that a 10% increase in Medicare reimbursement per capita decreases death rates by 6%. Their sample of patients includes emergency admissions arriving by ambulance, over 65 years old, and with non-deferrable medical conditions. Since we do not have records on whether a patient arrives by ambulance, we only compute the effect of our policy on the sample of patients over 65 admitted via the 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 admitted from an ambulance. We find a similar effect in the sample of emergency admissions over 65 years. As noted above, we cannot observe whether a patient is arriving by 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 between 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) also reports that increasing competition by 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 CEO selection has a comparable effect of 15% and 20% reduction in deaths rates, but over a much larger sample mean.

4 Reform Financial Incentives Effects

The reform included a change in the recruitment system and financial incentives, in the form of pay for performance and higher base wages. Low-powered incentives and low wages in the state are often pointed to as one source of the inefficient performance of high-end public employees. For instance, recent empirical research shows that financial incentives can increase the performance of employees in the public sector (Khan et al., 2015; Biasi, 2021; Deserranno et al., 2022).

Perhaps post-reform managers improved hospital performance simply because they exerted more effort due to the newly introduced financial incentives. In this section, we examine the financial incentives effects of the reform and find that our results are not explained by either performance pay or higher wages.

4.1 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 a performance contract in agreement with the hospital CEO (i.e., the agent) for a 3-year period. At the end of each year, the CEO gets a final score based on the parameters in the contract. The yearly wage is impacted by the performance agreement according to the following schedule:

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

Two things are worth noting about the schedule in Equation 4. First, the wage in 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 2 and 3 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 available performance contracts for the first manager appointed after the reform adoption and their yearly performance scores.[26] Figure 10 presents the distribution of the 3-year average performance score. Note that 60% of the distribution is above the 95% threshold and avoids any wage penalization. The rest are between 95% and 65%, which is the lowest threshold to avoid 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 D, we empirically analyze whether CEOs' performance scores implied better managerial performance at the hospital. We find that managers with high and low performance scores were equally effective in improving hospital performance.

We note 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% on their performance scores in 2013 (CPPUC, 2013), and more than 90% achieved a 100% performance score in 2016 (CADP, 2017). This tool's failure 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.

[26]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 access to 77 contracts.

4.2 Results Are Not Driven by Higher 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.10 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, 43% of the quarterly wage, and the middle 50% of the distribution is between 37%and 46%. In Appendix E, we present event study evidence on the reform's effect on CEO wages when it is adopted by 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 CEO's remuneration but rather 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 examine 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.[27] The medical pay law is much more generous than the public employees' pay law. Therefore the amendment implied an increase in remuneration 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 in which new managers are doctors *and* receive a pay boost. To study this question, we perform a stacked event study, in which an event is a transition after November 2016 that uses the new selection system and 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

[27] 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.[28] We then append the data for all valid events and estimate an event study following Equation 3.

Panels A and B in Figure 11 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 doctor 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, the evidence suggests that financial incentives do not explain the performance improvement we observe after adoption of the selection reform. In other words, had the reform only included financial incentives and not changed the recruitment system, we do not expect to observe an impact on hospital performance. Therefore, the key component of the reform was the introduction of the competitive recruitment system, which changed the identity and characteristics of hospital CEOs. We examine this mechanism in the next section.

It is important to note, however, that although we have ruled out the possibility 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 public sector 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* 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 management studies would have been less likely to apply in the absence of the wage hike.

5 The Recruitment Effects of the Reform

5.1 Impact of the Reform on CEO Characteristics

The evidence so far suggests that the impact of the reform on hospital performance is not driven by the financial incentives in the reform. In this subsection, we examine the recruitment effects

[28] 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.

of the policy and evaluate how the new recruitment process changed the characteristics of new CEOs. To this end, 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 individual-specific traits such as educational background, skills, and demographics, and $M(h,t)$ is a function that indicates the identity of the CEO of hospital h at time t.

We start by computing the impact of the policy on educational background. We focus on management, which is one of the targeted backgrounds of the policy. 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. This variable takes the value 1 if in a given quarter an individual has postgraduate management studies and 0 otherwise. Postgraduate management studies include master's degrees and diplomas related to management and administration. For example, the former include master's degrees in public health administration, public administration, and business administration, among others. Diplomas are shorter executive education courses, akin to professional certificates in the US.

Figure 7 presents the results. Panel A shows that the reform increased the share of CEOs with undergraduate management degrees by 21 percentage points, from a baseline of only 2%.[29] The increase in the number of CEOs with this background came at the expense of displacing almost one-to-one doctor CEOs, who before the policy adoption accounted for 93% of CEO positions, and a slight negative effect on health professionals other than doctors. The reform did not have an effect in hiring professionals with a background in other disciplines. Importantly, Panel B in Figure 7 shows that the displacement of doctor CEOs masks heterogeneous effects. In fact, the policy increased the number of doctor CEOs with postgraduate management studies by 14 percentage points—from a baseline of 18%—while substantially decreasing the number of doctor CEOs without management studies by 31 percentage points—from a baseline of 75%.

To further examine this heterogeneity, in Figure 8 we plot the the dynamic effects of the policy in a 3-year window after adoption. An interesting finding is that the displacement effect on doctor CEOs significantly wanes over time. Panel 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 degree by around 25 percentage points the quarter immediately after adoption, but

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

the effect decreased over time to slightly less than 15 percentage points.[30] In the case of doctors, we observe the opposite effect. After adoption, there is an initial displacement of around 20 percentage points. But by the end of the 3-year window, doctors were able to revert the loss in their likelihood of securing a CEO position to only 10 percentage points. Panel B in Figure 8 decomposes the total effect on doctor CEOs into the change coming from doctor CEOs with management training and doctor CEOs with no management training. While on impact the reform had a negligible effect on the likelihood that doctor CEOs have postgraduate studies related to management, by the end of the 3-year window the effect increased up to 20 percentage points—more than duplicating the pre-reform average. The flip side is that the policy decreased permanently—and on impact— the share of doctor CEOs with no management studies by around 30 percentage points.

In Figure 9 we study the impact of the reform on the likelihood that the CEO had completed *any* management studies before her appointment. The average across ever adopters 1.5 years before the reform was 21%.[31] The reform increased the likelihood that the CEOs holds a management undergraduate degree or management postgraduate studies by 37 percentage points. Importantly, the effect is stable over time and is explained both by professionals with management undergraduate degrees and doctors with management training taking over CEO positions after the reform.

The increase in the share of doctor CEOs with management studies is a combination of two phenomena. First, the reform likely increased the chances of being appointed CEO for the pool of doctors who would have had management studies in the absence of the reform. But the policy also incentivized doctors who wanted to be appointed 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 before 2003—the year when the reform was enacted—there was no supply of master's degrees in health management. Indeed, as shown in Appendix Figure A.9, the timing of opening of health management postgraduate programs coincides with the timing of the reform we study. The figure also shows that management postgraduate programs focused on areas other than health were available for a long time before and gradually increased over time. Qualitative anecdotal evidence further

[30]The effect changes over time because CEOs' tenure is, on average, shorter than 3 years, and therefore the effect is picking up the characteristics of more than one post-policy manager.

[31]As a point of comparison, in NHS hospitals, (Janke et al., 2020) report that 26% of CEOs hold postgraduate managerial education. Bloom et al. (2020) provide an additional antecedent and document that in a sample of hospitals in nine developed and developing economies, on average only a quarter of managers (including non-CEO managers) report having received managerial training.

supports the claim that these new programs are geared toward doctors seeking careers in health administration.[32]

In Table 6, we further investigate the impact of the policy on other CEO characteristics. We use the estimated CEO fixed effects from Appendix B as a measure of managerial skills, and the performance on the standardized national university entrance exam as measure of cognitive ability. For demographics, we consider age and gender. Column (1) shows that the reform led to the appointment of CEOs with higher managerial talent—as proxied by their CEO fixed effect. Column (2) shows that the new managers performed slightly worse on college entrance exams, although this difference is not significant. Considering that the new managers are displacing doctor CEOs and that to secure a position in medical school individuals need to achieve top scores on college entrance exams, this result implies that on average, new managers are also top performers in college entrance exams. In columns (3) and (4), we focus on the set of CEOs who took the older version of the college entrance exam in Chile, in which 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 fact that the reform increased the share of CEOs with management-related undergraduate degrees.

Columns (5) and (6) focus in demographics. We find that the new managers are on average almost 2 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; this phenomenon is 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 unwritten impartial hiring processes in the public sector does not have an effect on gender hiring disparities (Mocanu, 2022).

5.2 Could the Attenuation of Skills Mismatch Drive the Results?

We now 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 between their skills and the skills demanded by the job. In the public sector, several factors may create skill

[32]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.

mismatches that may hinder performance.[33] In the case of public hospitals, as discussed below, the social norm before the reform was that CEO positions were reserved for doctors. The reform mitigated the skills mismatch by displacing doctor CEOs for professionals with management degrees and also incentivized doctors who wanted to pursue careers as hospital CEOs to invest in management education. We examine whether correcting the skills 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, in which individuals have a higher or lower educational attainment than needed for their job. While a nascent literature studies horizontal mismatch in the private sector, to the best of our knowledge there is limited or no research in the public sector (Nordin et al., 2010; Besley et al., 2022).

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.[34]

Columns (1)-(3) in Table 7 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 in which the manager has *any* management studies, including undergraduate and postgraduate studies. We find that now the 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 cases in which appointed doctor CEOs had management studies and negligible effects otherwise.

Another way to examine treatment effect heterogeneity based on management studies is to

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

[34]One limitation of this analysis is that due to a lack of data, we neglect heterogeneities in management experience for individuals without formal studies in management. Implicitly, by abstracting management experience from the analysis, we 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).

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 Equation 3 in an stacked event study framework. We define an event as a CEO transition, and select a set of *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.[35] To avoid a mechanical correlation with the results presented in columns (2) and (3) in Table 7, we exclude all 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.[36]

Columnns (4)-(5) in Table 7 present the results. Column (4) presents the 3-year effect of a CEO transition without any management studies to a CEO with management studies. Column (5) is a placebo exercise that estimates the effect of transitions between CEOs *without* management studies. Consistent with the findings in the interacted DiD, we find that when the hospital transitions from a CEO without any management studies to a CEO who has management studies, the event is followed by a significant decrease in death rates. We find no effect on hospital death rates when we examine 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.[37] This evidence suggests that hospitals transitioning to CEOs with management studies drive, for the most part, the effect of the selection reform on hospital performance.[38].

Interestingly, the finding that management studies improve CEO performance might be at odds with the results of 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

[35] 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.

[36] Appendix Table A.4 presents the number of events by type of transition.

[37] Appendix Figure A.14 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.

[38] In Appendix Table A.6 we regress the CEO managerial talent—measured by the CEO fixed effect estimated in Appendix B—on CEO observable characteristics. These characteristics include gender, age, age squared, and a set of indicators for their educational background. Consistent with the findings above, management studies are correlated with better managerial performance (i.e., lower CEO fixed effects)

practices.[39] One key difference is that in our setting, business managers perform in the public sector, in which they have fewer incentives to reduce their employees' wages and fewer ways 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).

5.3 What explains skills mismatch in public hospital CEO positions?

Given the significant impact on performance delivered by CEOs with management training, why are not all public hospitals run by CEOs with this background? A primary reason is that before the implementation of the policy, there was a strong social norm in the public health sector that hospital CEO positions were reserved for doctors. Although there is no statutory rule prohibiting non-medical professionals from being selected as CEOs, in 2004—the year before the first hospital implemented the selection reform—doctors made up 98% of public hospital CEOs. The policy had a big effect on changing this empirical fact: By 2019, the fraction of CEOs with medical degrees in treated public hospitals was 53%.

Anecdotal evidence allows us to understand why this norm emerged and was sustained over time. According to the responses in a small survey conducted by Civil Service on public hospital CEOs, doctors tend to believe that individuals with no medical training should be restrained from CEO positions. For instance, the view of one doctor CEO was that *"the ideal place for the engineer is as an advisor to a doctor CEO. The engineering vision is super positive and necessary for organizing finances, indicators, goals, etc., but they have a very large information asymmetry with the medical team. A doctor can tell the non-medical CEO 'you don't understand this, you can't comment' and that's it."* (Servicio Civil, 2014).[40]

This same belief may have discouraged doctors from investing in management training. If doctors thought management training would not improve their performance as CEOs, there was no reason for them to pay for management postgraduate studies.[41] Furthermore, according to the same survey, the forgone earnings for doctors working as CEOs are high, considering their alternative is to work as clinical doctors. The high opportunity cost further disincentivizes doctors to invest in

[39] Panel D in Figure 6 shows that the reform did not impact hospital employees' wages.

[40] The norm could sustain because CEOs were elected by the head of the Health Service where hospitals are located, who in turn also were doctors and shared the belief that doctors would overperform professional managers.

[41] This is similar to findings in Bloom et al. (2015), who show that one of the major initial barriers to adoption of management practices was that firms thought they would not be profitable to adopt.

postgraduate management education in the absence of future monetary returns.

6 Conclusion

In this paper, we study the extent to which CEOs in the public sector can improve their organization's performance. We first document that the identity of CEOs matters for public hospital performance in Chile and explains a substantial share of the variation in mortality across hospitals. We then leverage the gradual adoption of a reform which introduced a competitive recruitment process for hiring public sector CEOs, and find that it reduced hospital mortality by around 10 percent. We show that this result is not explained by patient selection and is robust to other explanations. In contrast, we find evidence that the reform operates through more efficient use of medical resources and better personnel practices.

We then examine whether the financial incentives included in the reform in the form of performance pay and higher base wages explain the findings. We document that the performance pay incentives are poorly designed and are not binding across the board. Leveraging a later amendment to the reform, we also show that higher wages do not impact managerial performance in our setting. We thus rule out that, conditional on the characteristics of a given CEO, the financial incentives in the reform drive the results.

Instead, we show that the reform displaced older doctors in favor of younger CEOs with educational training in management and that it incentivized doctors who wanted to pursue careers as hospital CEOs to invest in management education. Furthermore, we find that management training is the main predictor of treatment effect heterogeneity. Since this result may be due to differential selection, we view this piece of evidence as suggestive and consider that examining the causal effect of management education on CEO performance in the public sector is an important open question for future research.

To conclude, we note that the 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 in cases in which "outsiders" are implicitly banned from certain positions—which we show was the case in our setting for individuals without medical degrees—is likely to improve the allocation of talent. Second, as discussed above, the extra pay likely plays a role by attracting higher-quality candidates to the pool of applicants. Disentangling these two margins is also a promising avenue for future research.

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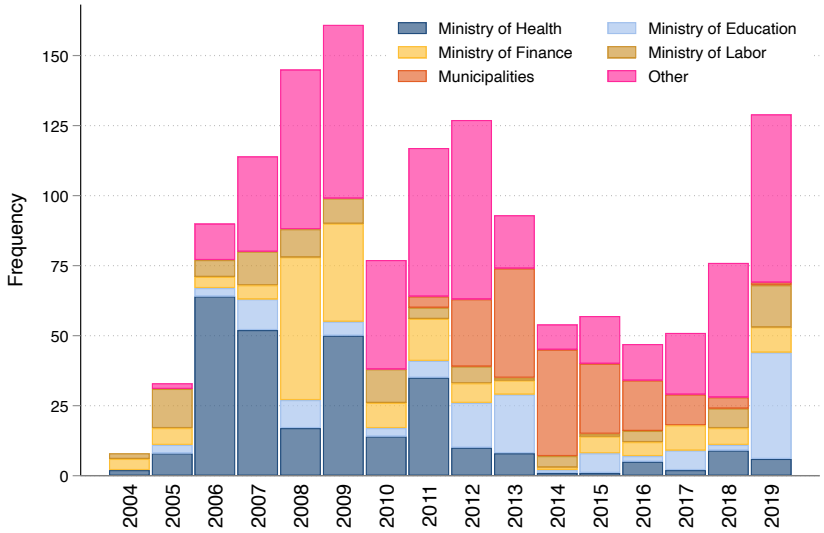
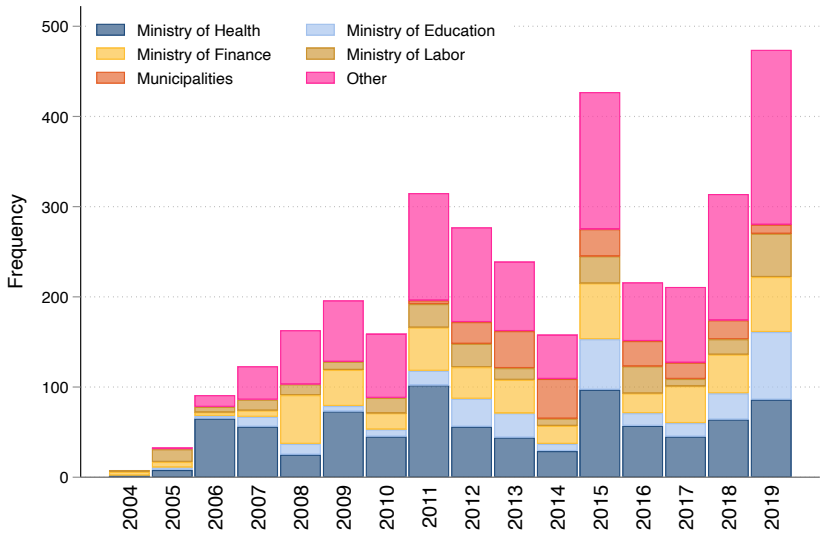
Figure 1: Adoption of the recruitment process in positions across government agencies



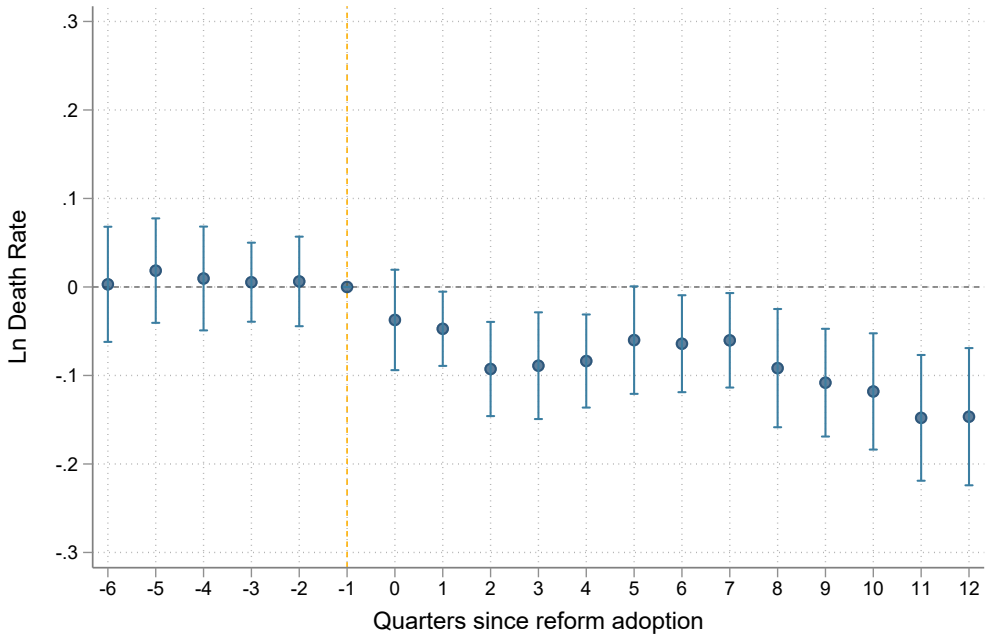
(a) Positions adopting the selection process for first time



(b) Yearly recruitment processes overseen by the Civil Service

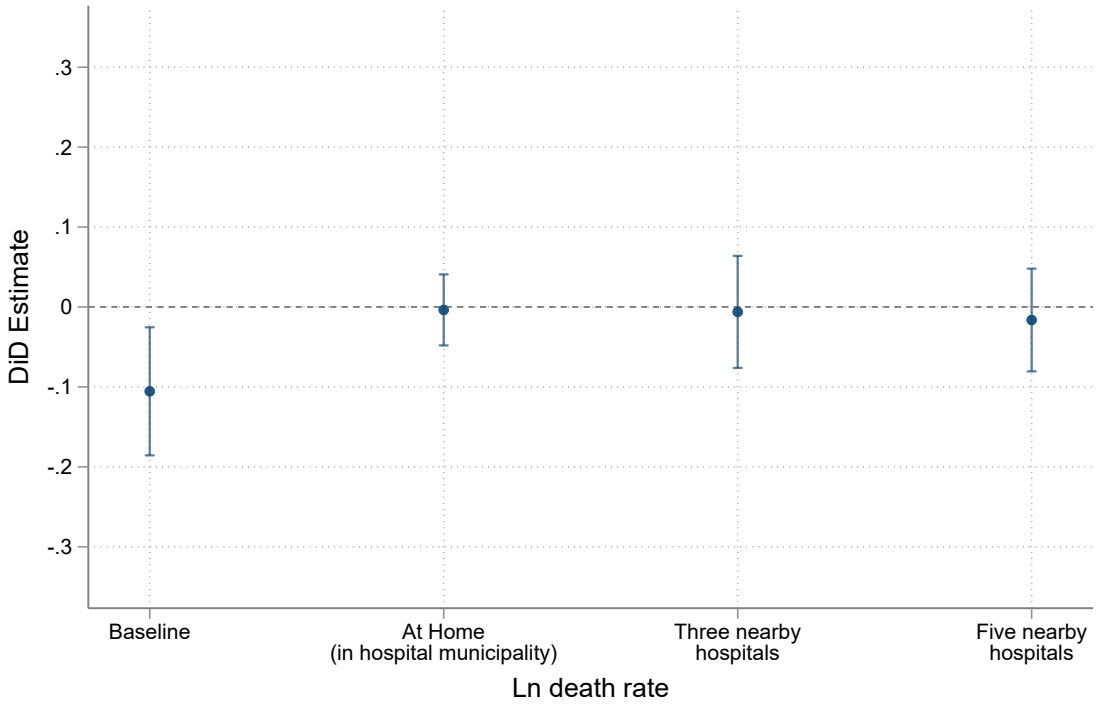
Notes: This figure displays the rollout of the selection reform across government agencies. Panel A presents the number of management positions that adopt the selection reform for the first time. After that, every new manager in that position has to be selected using this mechanism. All senior executive 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 the Civil Service oversees every year. We use the ending date of the process to allocate the process to 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



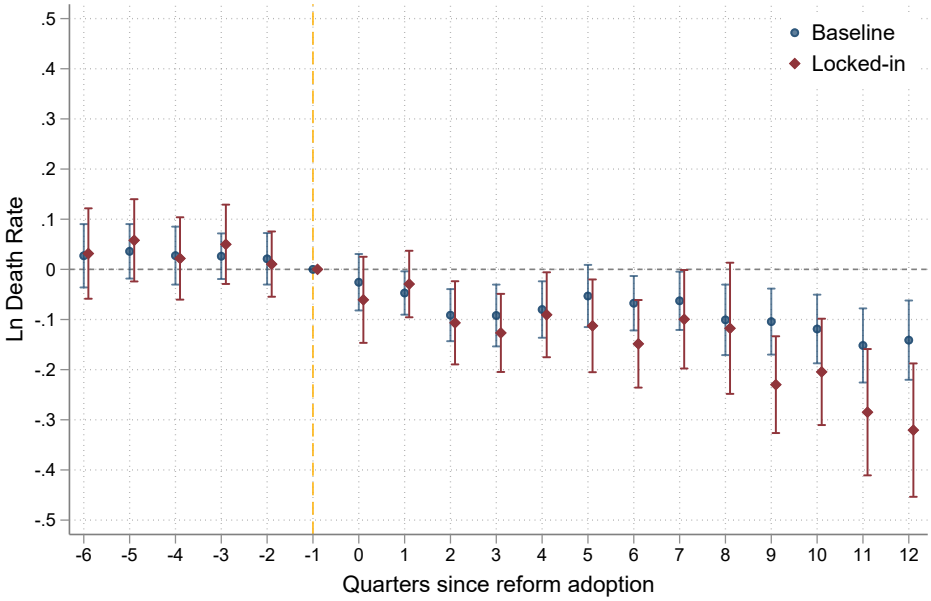
Notes: This figure presents event study evidence of the reform's effect on hospital deaths, following Equation 2. The empirical analysis uses quarterly panel data on public hospitals in a time window comprehending 6 quarters before and 12 quarters after the reform was adopted by each hospital, and exploits the gradual adoption of the selection reform in public hospitals during 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 vertical lines indicate the corresponding 95% confidence intervals. The dashed yellow line represents the omitted coefficient. Standard errors are clustered at hospital level.

Figure 3: Testing for patient selection: supply-side

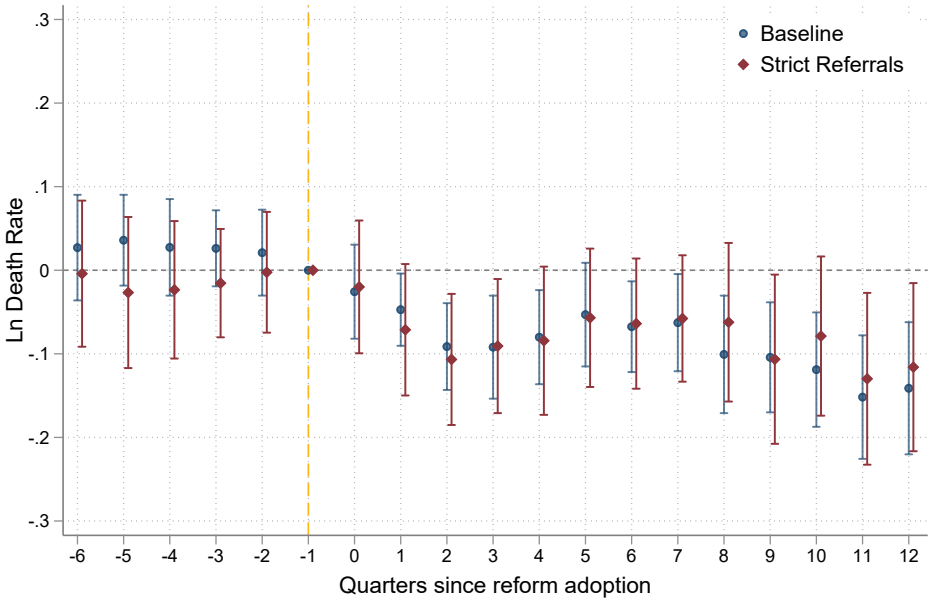


Notes: This figure presents evidence to assess patients' selection as a confounder of our main results. We plot the estimates and confidence intervals obtained by estimating Equation 1 for the logged at-home death rate and for logged death rates at nearby hospitals. All regressions consider standard errors clustered at hospital level.

Figure 4: Testing for patient selection: demand-side



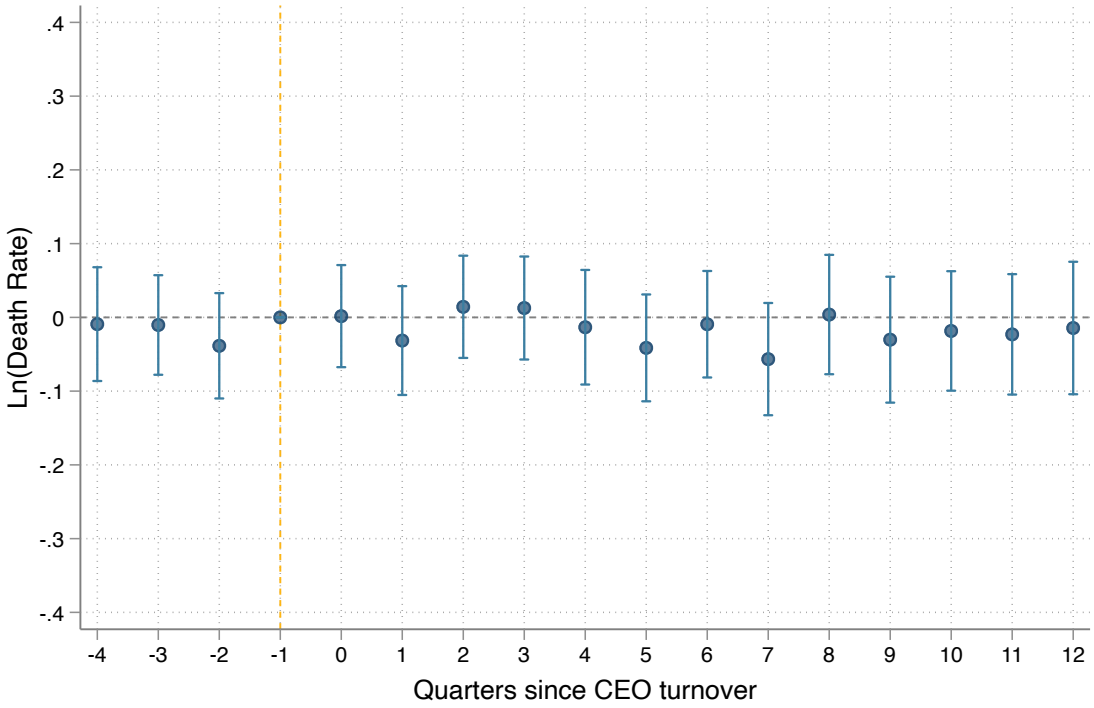
(a) Locked-in patients



(b) Strict-referrals patients

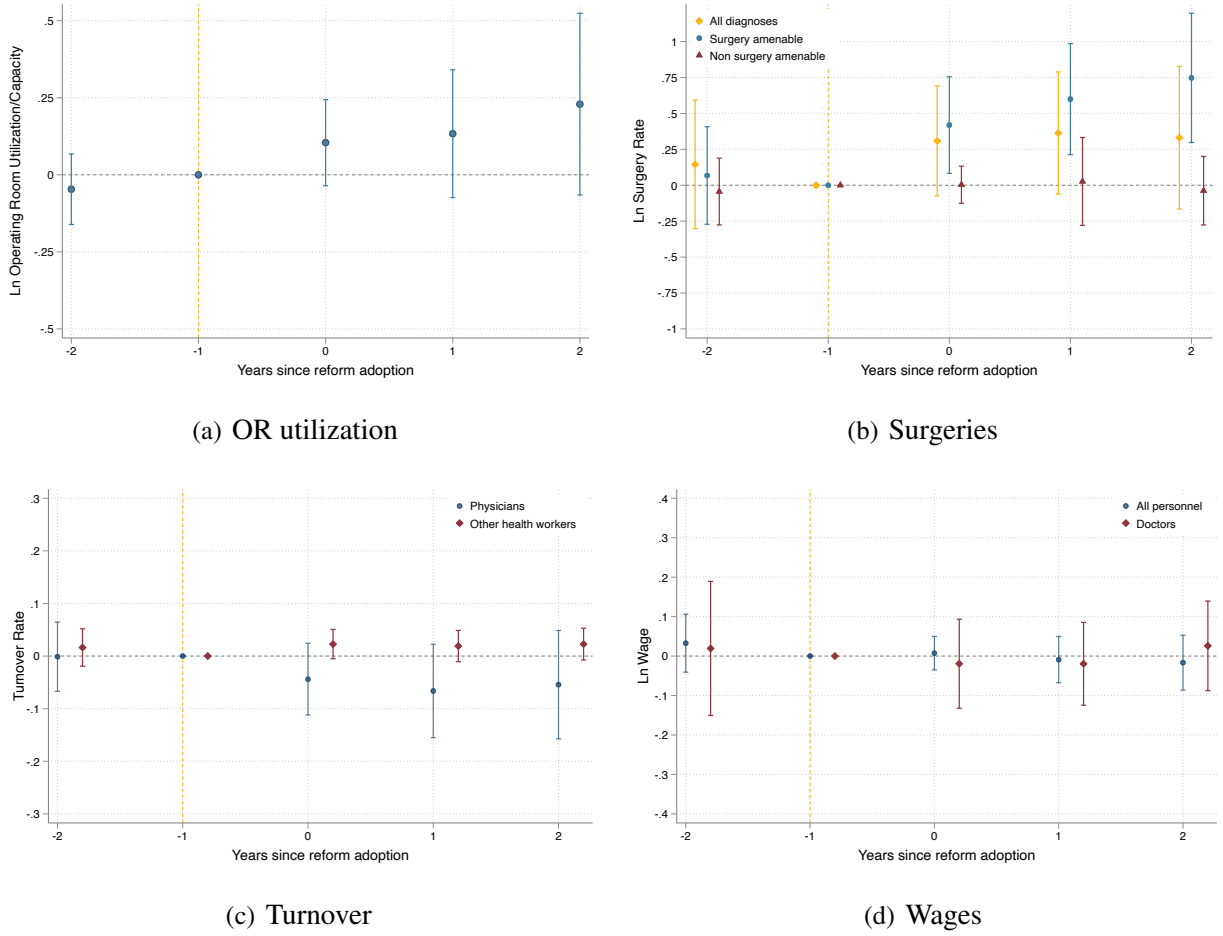
Notes: This figure presents evidence to assess patients' selection as a confounder of our main results. Panel A presents event study evidence on the reform's effect on hospital deaths, following Equation 2, but on a restricted sample of locked-in patients only. Panel B presents event study evidence on the reform's effect on hospital deaths, following Equation 2, but on a restricted sample of patients that followed the referrals mandated by the health system. These figures also include baseline estimates for a comparison. All regressions consider standard errors clustered at 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 the corresponding 95% confidence intervals. Standard errors are clustered at hospital level.

Figure 6: How is the reform improving hospital performance?



(a) OR utilization

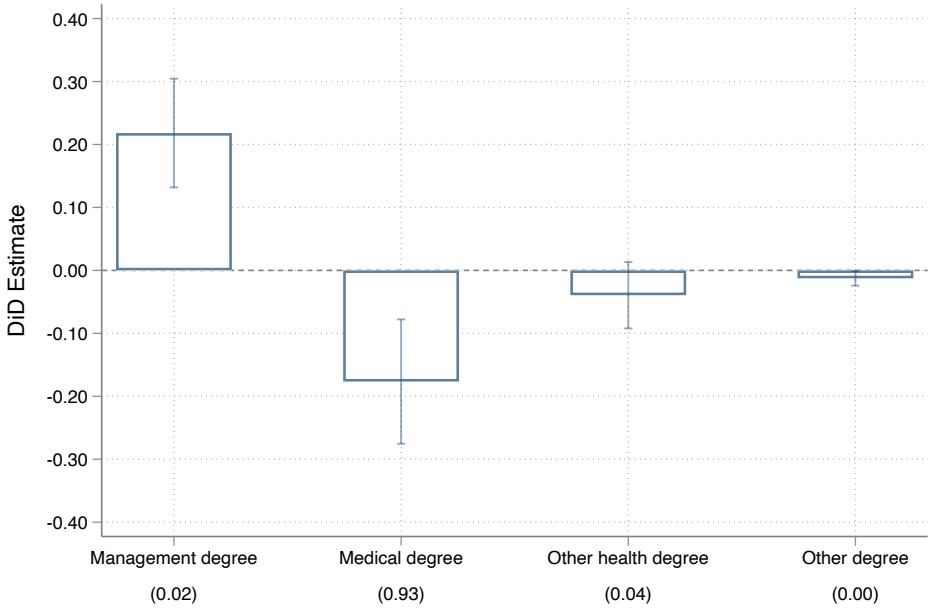
(b) Surgeries

(c) Turnover

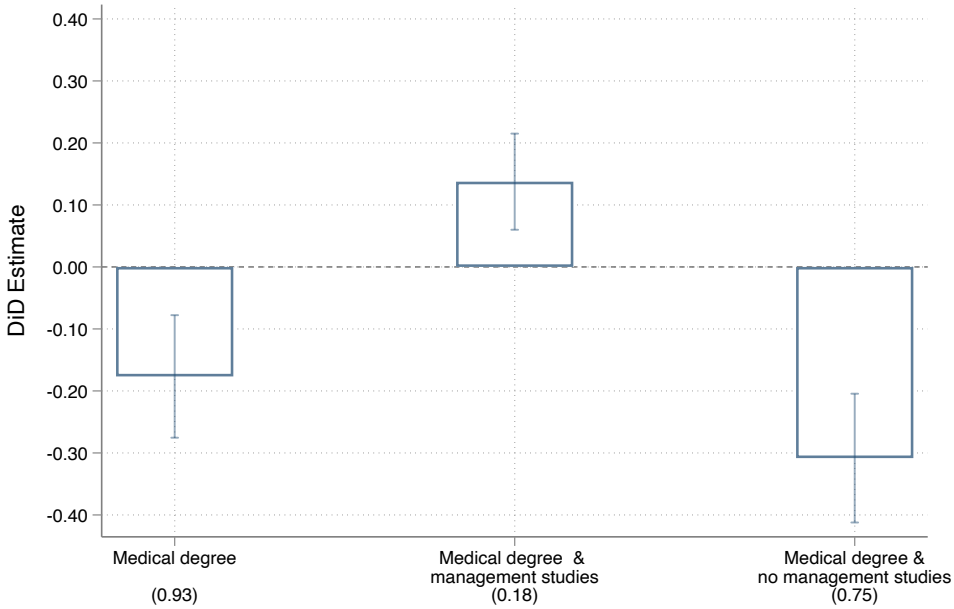
(d) Wages

Notes: This figure presents event study evidence on the reform's effect on several hospital outcomes, following Equation 2 estimated at the year level. Panel A examines the logarithm of operation room utilization over capacity. Panel B focuses on logged surgery rates and distinguishes by diagnoses amenable to death prevention through surgery. Panel C replaces the dependent variable with the turnover of doctors (circle markers in blue) and other health workers (diamond markers in red). 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 in group j working in h at time t. Panel D consider logged hourly wages as the dependent variable and plots the impact of the policy for wages of all hospital personnel (circle markers in blue) and of doctors (diamond markers in red). Each dot corresponds to an estimated coefficient, and vertical lines indicate the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure 7: The policy displaced doctor CEOs with no management studies



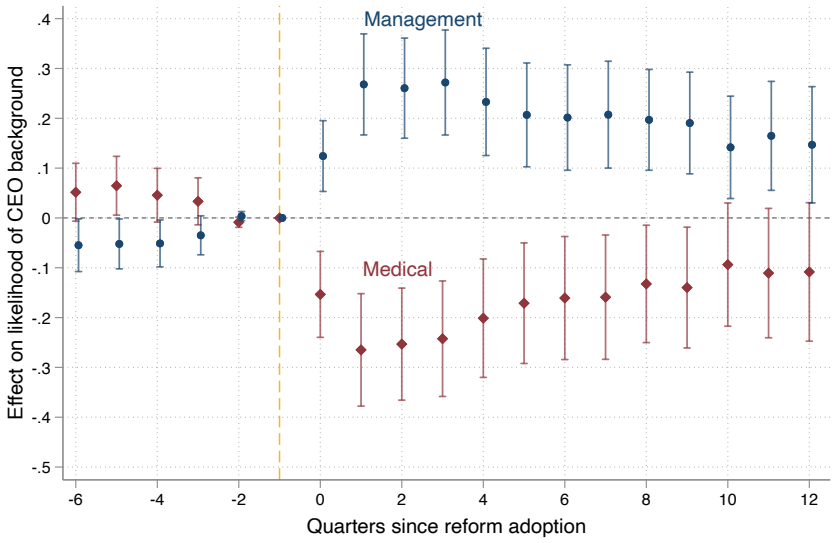
(a) All degrees



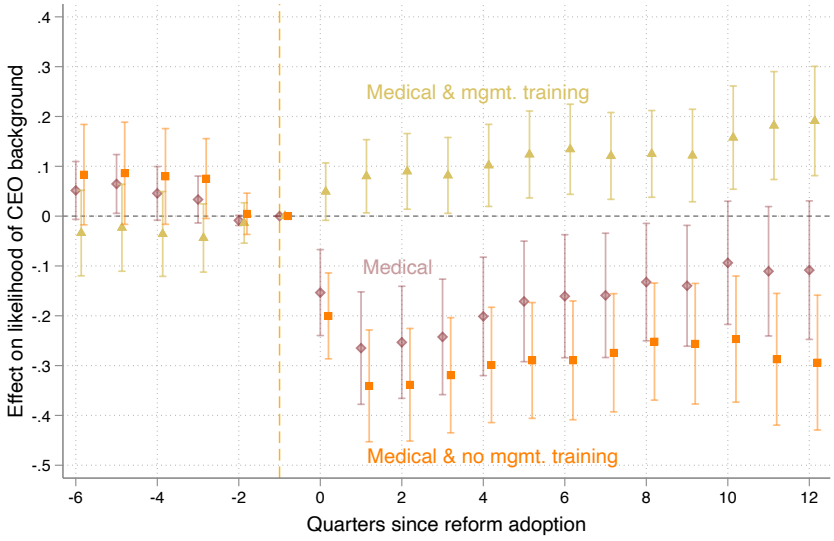
(b) Doctors

Notes: This figure presents the effect of the policy on CEO educational background. Panel A presents the average 3-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 performs separate estimations for doctors with and without management studies (as of the date of their appointment as CEOs). Bars represent the estimate from Equation 1 on each outcome and vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at hospital level.

Figure 8: Dynamic effects on CEO educational background



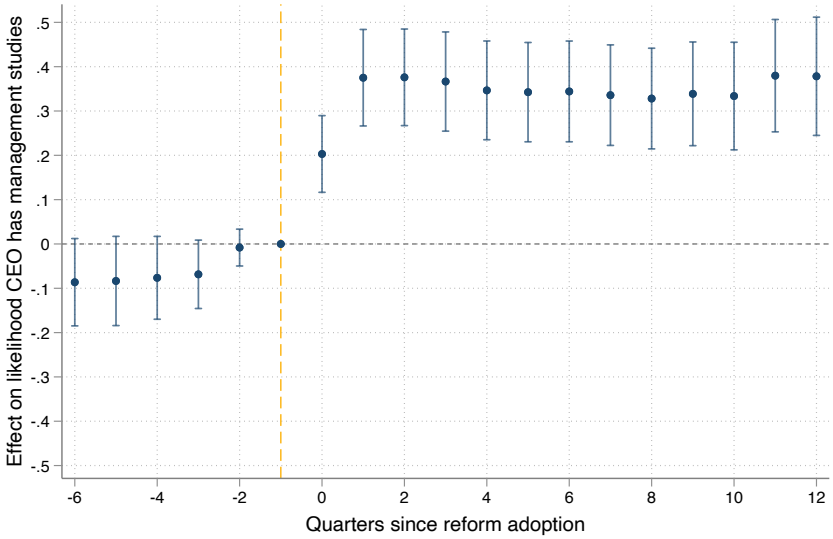
(a) Medical degree vs. management undergraduate degree



(b) Medical & mgmt. training vs. medical & no mgmt. training

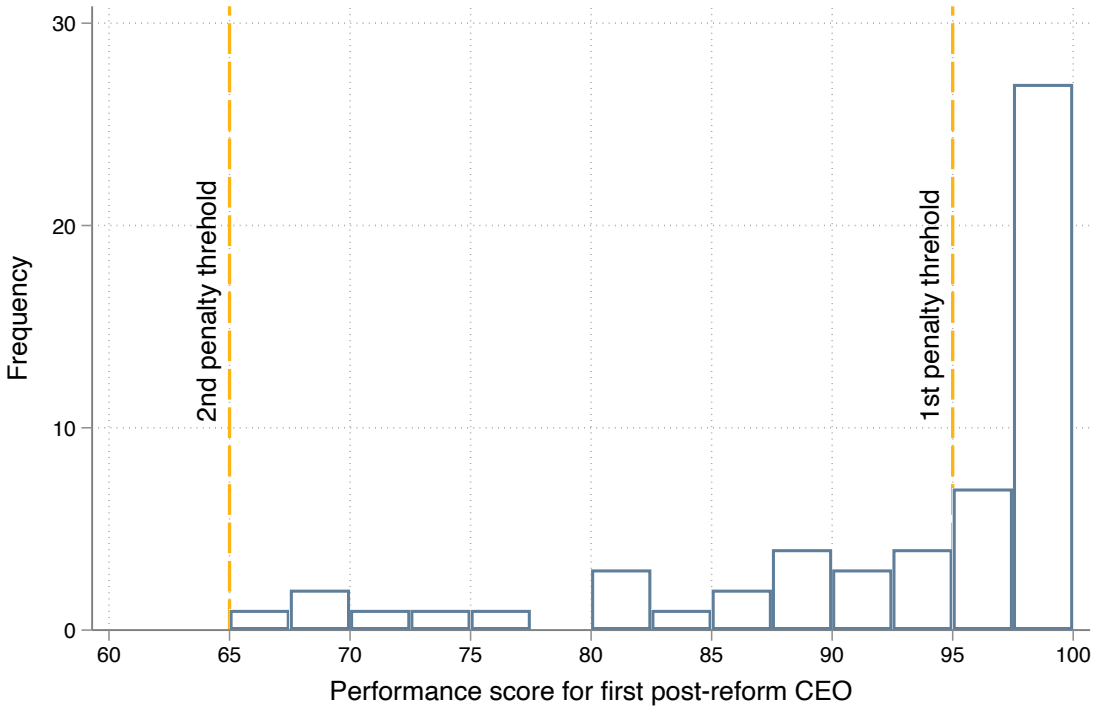
Notes: This figure presents event study evidence of the reform's effect on CEO educational background, following Equation 2. In Panel A, the figure overlays the estimation of two dependent variables. The first is a dummy variable that takes value 1 if the CEO has a management-related undergraduate degree (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 decomposes the total effect on doctor CEOs (in light red with diamond markers) into the change coming from doctor CEOs with management training (the beige triangle markers) and doctor CEOs with no management training (the orange square markers). Management training refers to whether the CEO holds a master's degree or a diploma in management (as of the date of their appointment as CEO). Dots indicate estimated coefficients. The vertical lines indicate the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure 9: The reform made more likely that public hospital CEOs have management training



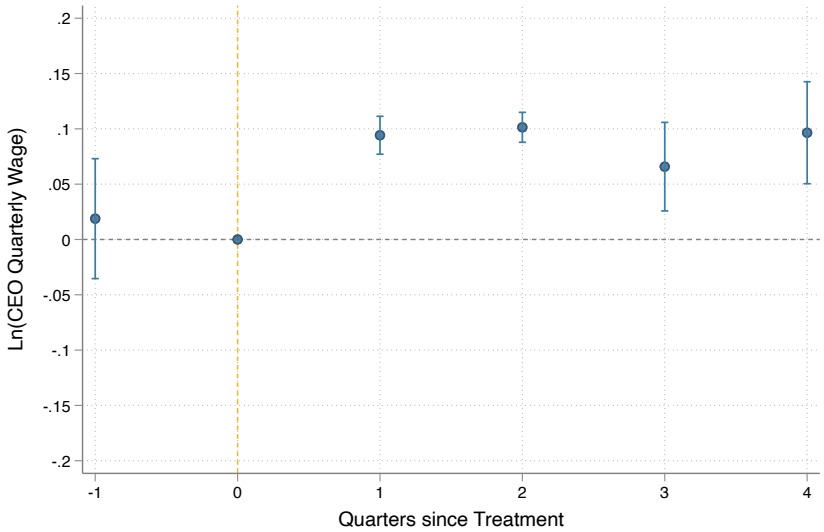
Notes: This figure presents event study evidence of the reform's effect on the likelihood that the CEO has management studies, following Equation 2. Management studies is a dummy that takes value 1 if, as of the date of their appointment as CEO, the individual holds an undergraduate degree in a management-related major, or a master's or diploma in management. Dots indicate estimated coefficients. The vertical lines indicate the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure 10: Distribution of performance scores for post-reform CEOs

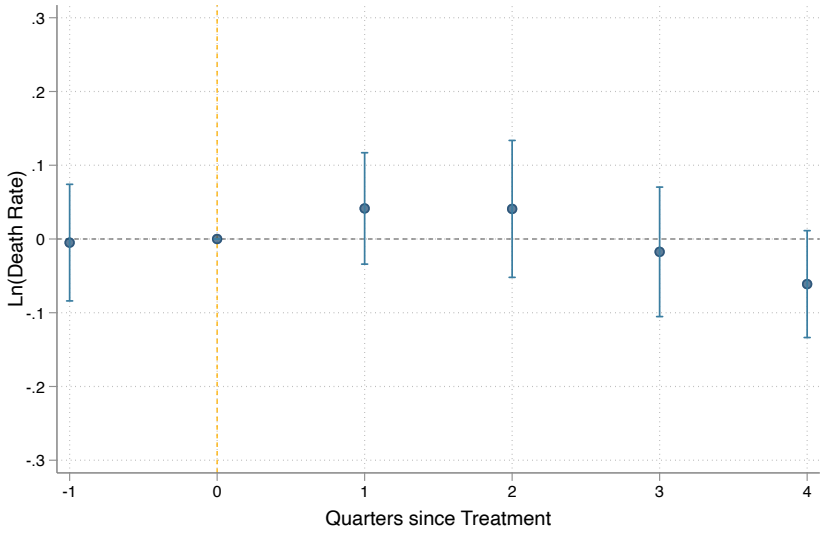


Notes: This figure displays average performance scores for the first post-reform CEO. Before the reform, managerial performance did not affect the wage schedule. After the reform, CEOs face wage penalties if they perform below specific performance thresholds. Performance scores are computed following a 3-year performance contract the CEO defines with her superior. We accessed all available performance contracts and yearly performance scores. Unfortunately, some of the oldest contracts and performance scores are lost, and the Civil Service has no available records. Of the 87 CEOs hired for the first time under the new selection system, we have performance scores for at least 1 year for 57 CEOs. An observation is the average of all available scores for a CEO in her 3-year contract. Dashed yellow 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 11: Do efficiency wages impact death rates?



(a) Effect on CEO wages



(b) Effect on death rates

Notes: This figure examines 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 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 CEO 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 hospital level.

Table 1: Explanatory power of managerial talent to account for hospital performance

	Ln Death Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Observations	6,712	6,712	6,712	6,712	6,712	6,712
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
CEO FE	No	No	No	Yes	Yes	Yes
CEO-by-hospital FE	No	No	No	No	No	Yes
F-statistic for CEO FEs	-	-	-	3.4	10.06	-

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.

Table 2: 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

Notes: This table presents descriptive statistics for the universe of public hospitals included in our main analysis. 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 3: Balance in observable characteristics before the reform

	Avg. never adopter	β Ever adopter (Levels)	β Ever adopter (First-Diff)
	(1)	(2)	(3)
Patient composition:			
% Age < 29	0.381	0.042	0.004
		(0.060)	(0.003)
% Age ∈ (30,49)	0.220	0.005	0.003
		(0.021)	(0.002)
% Age ∈ (50,69)	0.185	0.009	-0.003
		(0.024)	(0.003)
% Age ∈ (70,89)	0.197	-0.047**	-0.004*
		(0.021)	(0.002)
% Age > 89	0.018	-0.009***	-0.000
		(0.002)	(0.001)
% Female	0.605	-0.027	0.000
		(0.018)	(0.003)
% Public insurance	0.972	-0.043***	0.003
		(0.009)	(0.002)
Hospital outcomes:			
Number of deaths	5.970	47.943***	0.999
		(16.157)	(1.053)
Death rate	1.389	0.497	0.083
		(0.366)	(0.083)
Death rate ER	1.483	1.325**	0.137
		(0.618)	(0.116)
Death rate 28 days	3.305	-0.046	0.155
		(0.504)	(0.143)
Death rate AMI	23.465	-19.993***	6.085
		(6.446)	(14.883)
Political variables:			
% Votes for right	25.764	8.186*	2.674
		(4.792)	(5.691)
% Votes for center	19.107	5.499	2.046
		(5.633)	(3.970)
% Votes for left	24.435	-8.226	-4.579
		(5.256)	(4.275)

Notes: This table studies differences between ever- and never-adopter hospitals in terms of predetermined characteristics. We consider a window of six quarters before adoption. Column (1) shows the average of each characteristic for never adopters. Column (2) presents the coefficient obtained from a regression of each variable on a dummy that equals 1 if the hospital was an ever adopter. Column (3) replicates column (2) but replaces the dependent variable with its first differences. The political variables correspond to the vote share of right wing, center, and left wing parties in the 2000 and 2004 mayoral elections in the municipalities where hospitals are located. The first differences of these variable correspond to the difference in vote shares between the 2000 and 2004 elections. Standard errors are clustered at the hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Impact of the reform on death rates

	Ln Death Rate					Poisson (# Deaths)	
	All			28-days	ER	All	ER: AMI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1 if reform adopted	-0.131***	-0.081***	-0.095***	-0.061***	-0.156***	-0.054***	-0.146
	(0.025)	(0.022)	(0.023)	(0.016)	(0.036)	(0.018)	(0.134)
Observations	8,104	8,104	8,104	8,104	6,592	8,104	1,956
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case-Mix Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Flexible Case-Mix Interactions	No	No	Yes	Yes	Yes	Yes	No
# of Hospitals	181	181	181	181	175	181	132
Mean Dep. Variable	2.625	2.625	2.625	4.726	3.088	21.85	16.22

Notes: This table presents our estimates of the impact of the selection reform on public hospital's performance, as measured by death outcomes. Estimates are from the staggered DiD specification in Equation 1. The empirical analysis uses quarterly panel data for 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 during 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 and add case mix controls sequentially. Column (4) replaces the dependent variable with 28 days after admission death rate, and thus considers in- and out-of-hospital deaths. In column (5) we study the impact of the reform on death rates of ER admissions. Finally, columns (6) and (7) reports estimates from a Poisson regression of death counts. Column (7) focuses on the subset of emergency room admissions with AMI (Acute Myocardial Infarctions, commonly known as "heart attacks") diagnoses. Results in columns (1)-(6) are weighted by the number of the hospital's inpatients as of 2005. For columns (1)-(6), the mean dependent variable is presented in levels instead of logs. Standard errors are displayed in parentheses and are clustered at hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: CEO selection reform v. other policies

Policy (1)	Paper (2)	Death rate definition (3)	Average death rate (4)	Impact on death rate (5)	Sample of patients (6)
Spending					
↑ 10% p/capita	Doyle et al. *JPE* '15	All, 1-year	37%	↓ 6%	ER + Amb. + ≥ 65*
	Ours		32%	↓ 7%	ER + ≥ 65
Public vs Private					
VA v. Non-VA hospitals	Card & Chan '22	All, 1-year	29%	↓ 7%	ER + Amb.+ ≥ 65
	Ours		32%	↓ 7%	ER + ≥ 65
Competition					
+1 hospital in neighborhood	Bloom et al. *ReStud* '15	In-hospital, 28-day	15%	↓ 10%	ER + AMI
↓ 10% HHI	Gaynor et al. *AEJ EP* '13	In-hospital, 28-day	1.6%	↓ 1%	All patients
	Ours		2.3%	↓ 15%	All patients

Notes: This table compares the impact of the CEO selection reform we study with the impact of other policies previously studied in 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 Subsection 3.5. Acronyms used in the table: ER: Emergency Room; AMI: Acute Myocardial Infarction; Amb: arriving by ambulance; *: non-deferrable medical condition.

Table 6: Effect of the reform on managers' skills and demographics

	Skills				Demographics	
	CEO Fixed Effect (1)	Avg. PSU Score (2)	Math Specific Exam (3)	Science Specific Exam (4)	Age (5)	Female (6)
1 if reform adopted	-0.09***	-0.12	0.08	-0.13**	-1.87*	-0.03
	(0.03)	(0.10)	(0.08)	(0.05)	(1.06)	(0.05)
Observations	4,391	7,053	5,561	5,561	7,906	8,085
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
# of Hospitals	111	177	162	162	180	180
Mean Dep. Variable	0.570	2.000	0.740	0.990	50.190	0.210

Notes: This table presents our estimates of the impact of the selection reform on public hospital CEOs' skills and demographics. 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 and 2019 and exploits the gradual adoption of the selection reform by public hospitals in that period. In columns (1), we focus on our CEO fixed effects estimates as a measure of managerial ability. Columns (2)-(4) examine the impact on college admission test scores as a proxy for cognitive skills. The 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, in which applicants had to choose which exam to take. Columns (5)-(6) 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 hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Heterogeneity in CEO performance by managerial education background

Identifying variation is:	Reform adoption			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 heterogeneous effects of the reform by CEOs managerial education background. Columns (1)-(3) focus on the differential effect of the selection reform on death rates, following the staggered DiD 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 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 transitions associated with the first time that a CEO was appointed after the selection reform was adopted by a given hospital. In total, there are 94 valid CEO transitions, as described in Appendix Table A.4. The dependent variable is the death rate at hospital level in a given quarter. Dots indicate estimated coefficients and vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at hospital level. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

Managers and Public 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 by 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.11 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 a white diamond marker) and CESFAM Pablo de Rokha (in blue with a white a star marker), which are located in adjacent Health Services. Although individuals in each primary care center might live close to each other, 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 with a white a cross marker) and CESFAM Pablo de Rokha refers them to "Hospital Sótero del Río" (in red with a white H marker).

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 patients are referred. The example shows that referrals depend exclusively on the primary care center and the diagnosis and demographics of the patient. For example, a medical oncology patient older than 15 in CESFAM Colina is referred to "Instituto Nacional del Cáncer Dr. Caupolicán Pardo Correa."

To empirically assess 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, between 1 and 15, and more than 15) and diagnosis (as reported by the hospital from which they are discharged). If the guidelines are strictly followed, we should expect all patients within a cell to attend the same hospital. To visually evaluate this, Figure A.12 plots a spikeline with the share of patients in each cell who are discharged exclusively from one hospital; 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 diagnosis 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 address to register in the health system. Finally, there might also be measurement error in the address and age of patients.

B Managers matter for hospital performance

In this appendix, we explore the rotation of CEOs across hospitals to study the extent to which CEOs affect hospital quality. Specifically, we follow the approach used by Fenizia (2022) and exploit the rotation of CEOs across hospitals to estimate the following model:

$$\text{Ln(death rate)}_{ht} = \alpha_h + \psi_{M(h,t)} + \gamma_t + X'_{ht}\Delta + u_{ht}, \tag{A.1}$$

where α_h are hospital fixed effects that capture time-invariant characteristics of the hospital (e.g., size and the type of procedures performed there), and $\psi_{M(h,t)}$ are CEO fixed effects, which capture managerial talent (specific to a given CEO) and 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 patients' demographics and diagnoses.

For estimation, we first identify the set of hospitals that are connected by CEOs' mobility (Abowd et al., 1999; Card et al., 2013) and define our main estimation sample, which consists of 789 CEOs, 113 hospitals, and 19 connected sets created by 86 movers. Then, we estimate the model via constrained OLS and recover CEO fixed effects that can be compared *within* connected sets.

It is worth noticing that models with additive hospital and CEO components may raise some concerns. One may worry, for instance, that CEOs are assigned to hospitals on the basis of unobserved factors that determine their comparative advantage. It could also be that manager rotation is correlated with hospital-specific trends. In the next subsection we empirically assess these concerns as in Card et al. (2013) and Fenizia (2022). All in all, the evidence suggests that the two-way fixed effects model fits the data well, and match effects, if any, are small.

B.1 Threats to Identification

We follow Card et al. (2013) and Fenizia (2022) to assess the two main threats to the identification of $\hat{\psi}_{M(h,t)}$. The first concern is that CEO mobility is endogenous due to a systematic relation with hospital-specific trends—for example, if good CEOs are rotating to hospitals that are improving their quality over time. This pattern would overestimate our CEO fixed effects. Relatedly, one might 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, in response, 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 CEO transitions into terciles. Intuitively, the classification allows us to distinguish whether the new CEO implies an average increase, a small change, or an average decrease in manager quality.

Panel (a) in 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 in the second tercile, in which changes in CEO quality are small. A second observation is that hospitals that hire 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 exhibit a trend similar 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 CEOs and hospitals; this dimension is neglected in the log model by the additive separability between CEO and hospital effects. Different CEOs may have different effects on hospital quality, depending on the value of their match component. If CEOs sort into hospitals in which they have a comparative advantage, this effect would be captured by the error term and would bias our estimates. To examine whether this concern is valid we consider two pieces of evidence. First, in column (6) in Table 1, we report a saturated version of Equation A.1, in which we include CEO-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 examine 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 and 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) in Figure A.13. We find that all residuals are small and lower than 0.05 in absolute value. A final piece of evidence comes from the symmetry of the effects depicted in Panel (a) in Figure A.13. Hospitals that move from a good CEO (in the first tercile) to a bad CEO (third tercile) face an opposite and symmetric effect to that of 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 match effects, if any, are small.

B.2 Variance Decomposition

In this subsection we perform the variance decomposition. Following Equation A.1, the variance of log death rates after accounting for patient characteristics and time effects can be decomposed as:

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

Table A.3 presents the magnitude of each term in Equation A.2, estimated within the largest connected set.[42] 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 manager fixed effects explain around 44% of the variance in death rates, which is about as much as the

[42]The largest connected set in our setting contains 3,276 observations: 322 CEOs, 41 hospitals, and 46 movers.

permanent component associated with different hospitals (54%). Our results also show that the (bias-corrected) covariance between CEO and hospital effects is negative, which implies that the most talented CEOs work at least productive hospitals (i.e., there is negative assortative matching).

C CMS Risk Adjustment

To ease selection concerns, we follow the UK's National Health Service (NHS) (e.g., Health and Centre, 2015) and construct a "risk-adjusted mortality rate" that divides the actual hospital-level death rate by the death rate predicted based on the 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 on 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 and 2004 (before reform adoption). These patients constitute the universe of discharges in the country. For them, we fit a logit model in which death is the dependent variable and different sets 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, which is a variable we use to construct the predicted death rate at hospital level.

D No Differential Effects of Performance Pay

In this appendix, we empirically examine whether CEOs' scores on their performance pay measure predicts better managerial performance in 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 introduction of the reform and study the impact of the reform for managers with high and low scores separately.

Online Appendix Table A.5 displays the results. Since we do not observe performance scores for all managers who 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 we use 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.

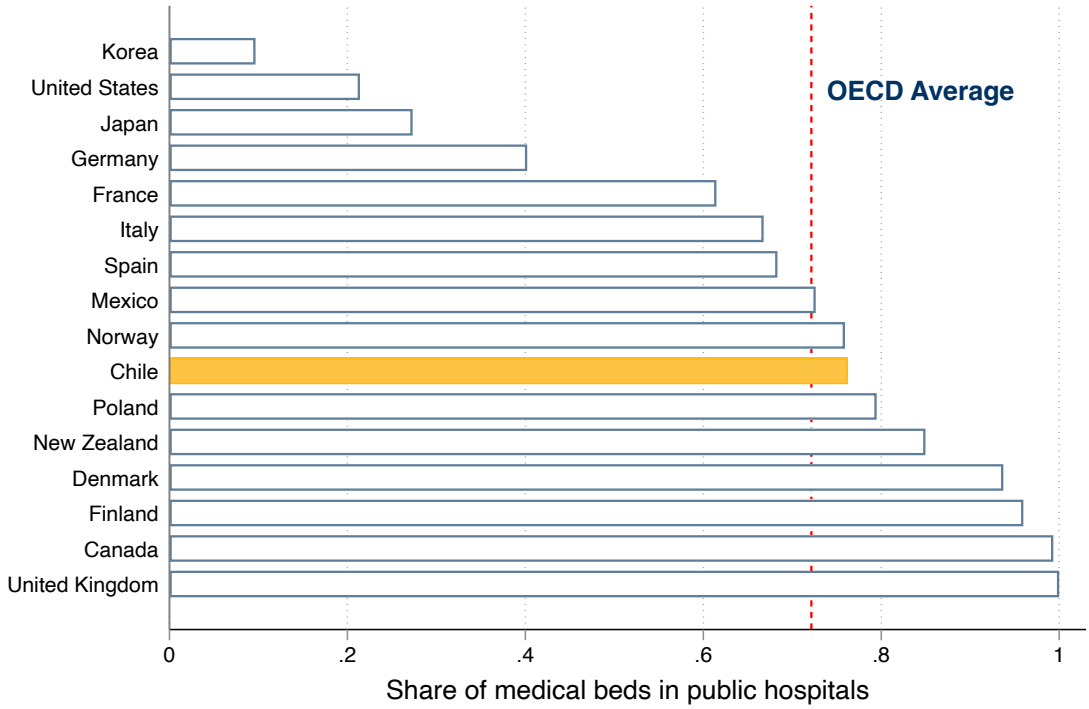
E Effect of the Selection Reform on CEO Wages

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. An important caveat is that the wage data panel starts in January 2014, after which 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.15 presents the results using quarterly data starting on 2014. Although the estimates are noisy due to the small number of events, the estimate is stable and the average quarterly wage increase in the 5 quarters post-adoption is 33%. We also do not find evidence of pre-trends, which means that hospitals that adopt the reform during a given period are not on a wage trend that differs from those that do not. Panel (b) presents estimates using monthly data for each December, starting in 2011. In both cases, standard errors are clustered at hospital level. We find quantitative and qualitatively similar results.

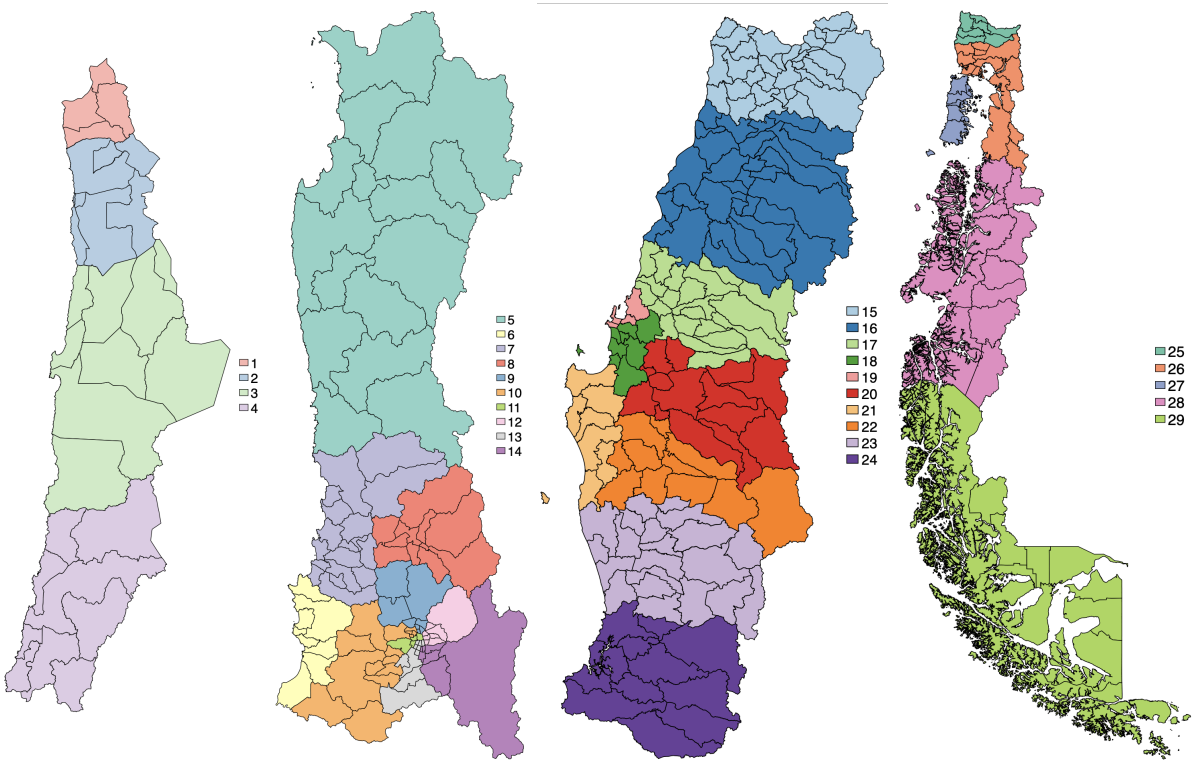
It is important to note that the results of this exercise reflect the change in the position's pay, and therefore is a composite of two effects. On the one hand, there are mechanical changes in pay due to changes in the manager's characteristics. For example, in the public sector, there are tenure bonuses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

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).

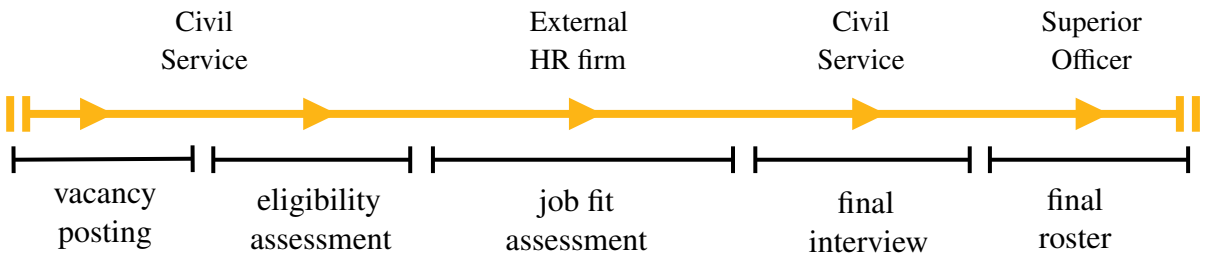
Figure A.2: Health Services are distributed geographically



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

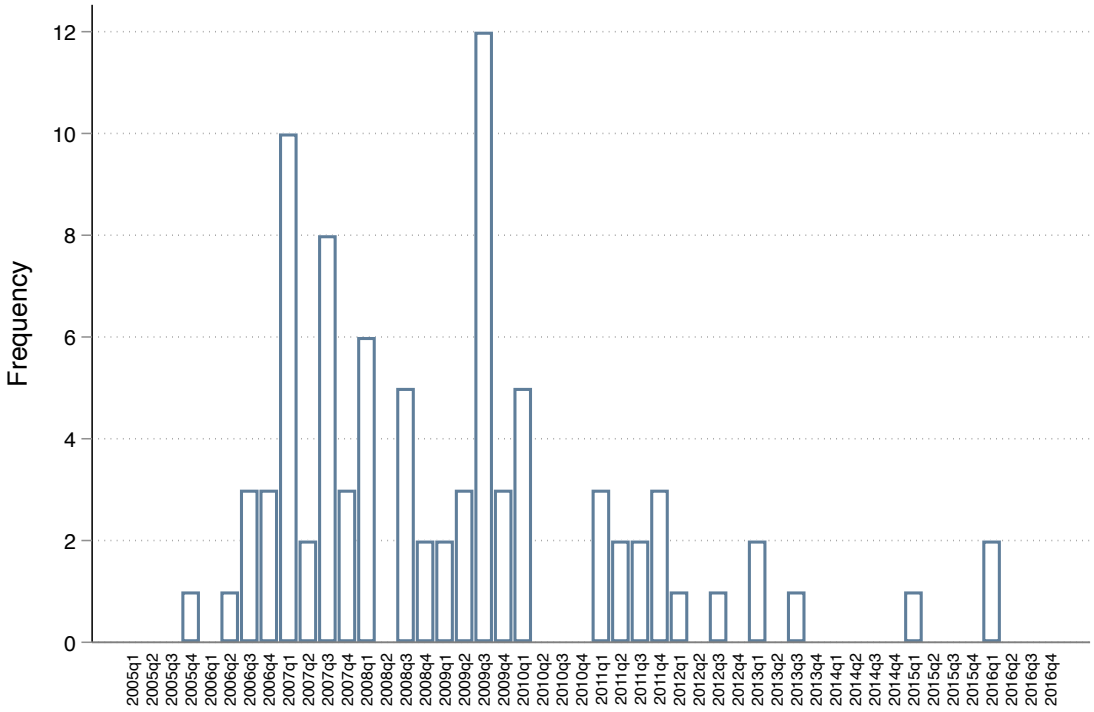
Notes: This figure shows the geographic distribution of the 29 Health Services in Chile. Each Health Service is responsible to oversee public health providers in the municipalities in their territory. Colors represent different Health Services and black lines represent municipal borders.

Figure A.3: Selection process after the recruitment reform



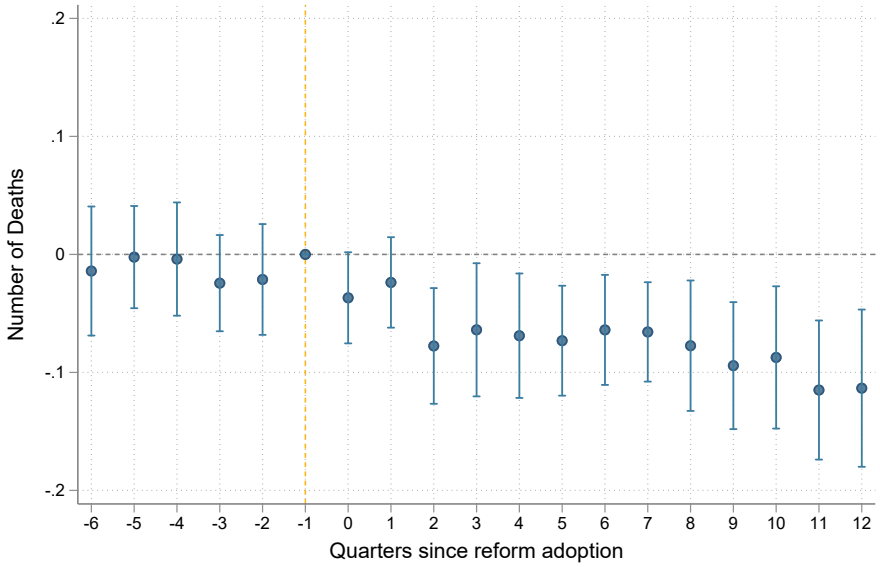
Notes: This figure illustrates the selection process for senior executives positions when the selection reform has been adopted. The job call starts with the position posted online on the Civil Service's website and in a newspaper with national circulation. In some cases, the Civil Service may also hire headhunters to widen the pool of applicants. After the job posting closes, an external HR firm evaluates each individual's job trajectory according to the job profile. They also assess motivation and overall competencies. The consultant gives every applicant a grade based on an objective rubric and provides a short list to the Civil Service. In the next phase, a committee consisting of representatives of the Civil Service and the Ministry in which 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: Gradual adoption of the reform by public hospitals

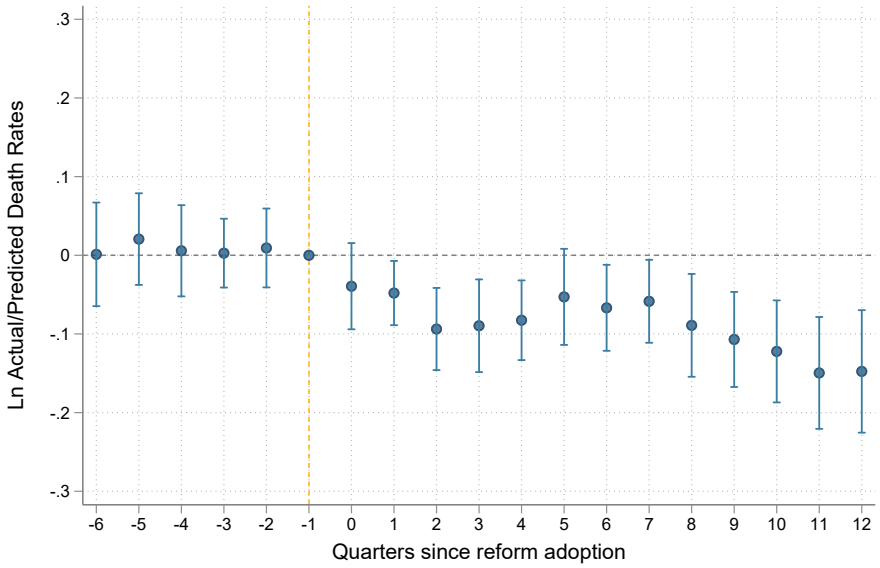


Notes: This figure displays adoption of the selection reform for CEOs in public hospitals. An observation represents a hospital that adopts the selection reform for the first time. After a hospital adopts the process, all future CEOs in that hospital have to be appointed using the new selection system.

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



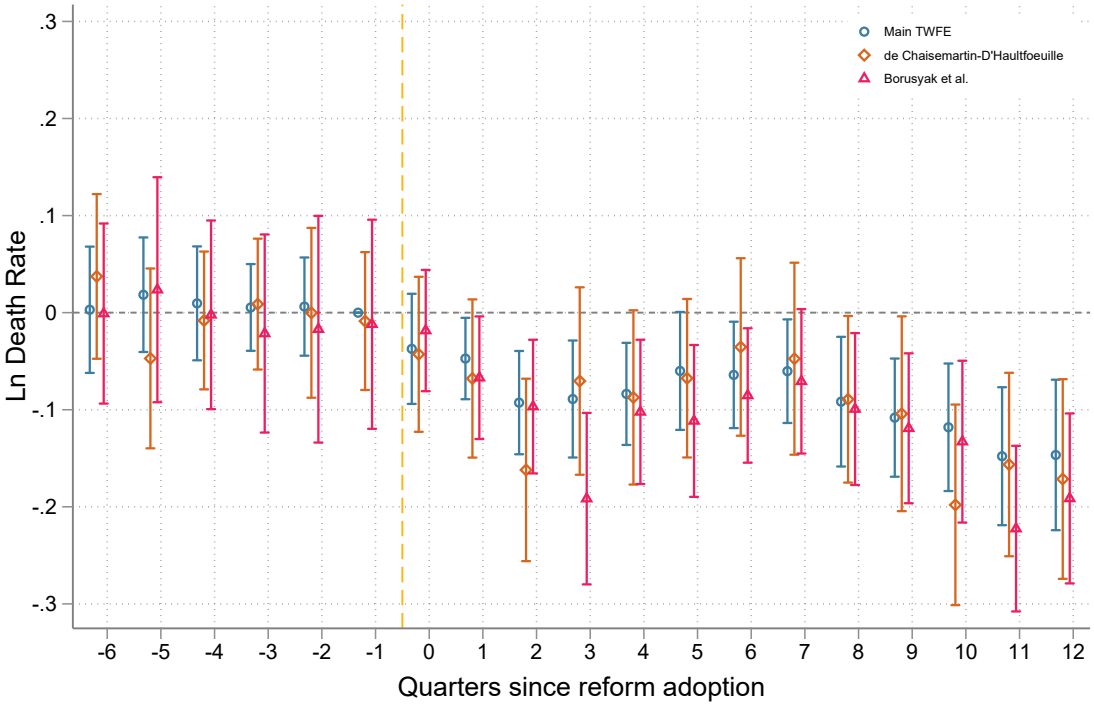
(a) Poisson Model



(b) Risk-Adjusted Mortality

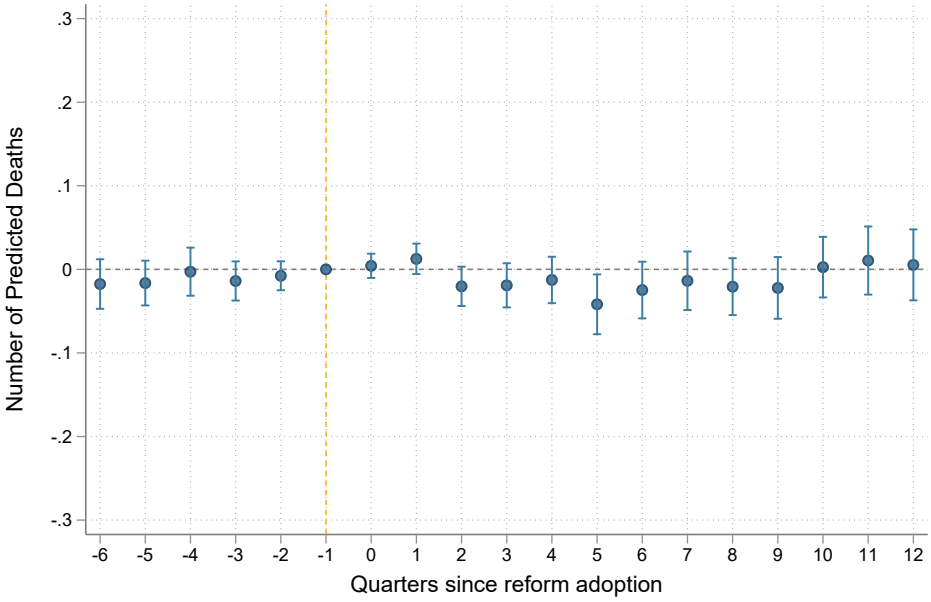
Notes: This figure presents the reform's effect on alternative outcome variables. Panel A reports estimates obtained from a dynamic Poisson regression of death counts. Panel B reports estimates and confidence intervals obtained from a two-way fixed-effects OLS regression of logged risk-adjusted death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

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

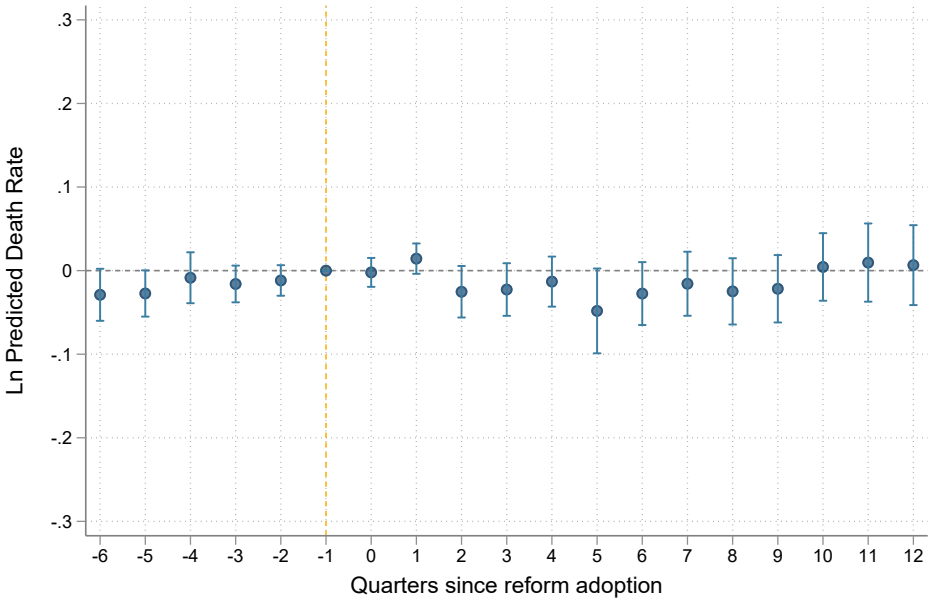


Notes: This figure plots the estimates and confidence intervals obtained from different event study models and estimation methods. The main event-study results using a two-way fixed-effects (TWFE) regression of logged death rates (see Equation 2) are presented under the label "Main TWFE" (in blue with circle markers). For comparison, we overlay the results obtained using the models suggested by 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 vertical lines indicate the corresponding 95% confidence intervals.

Figure A.7: Effects on predicted mortality



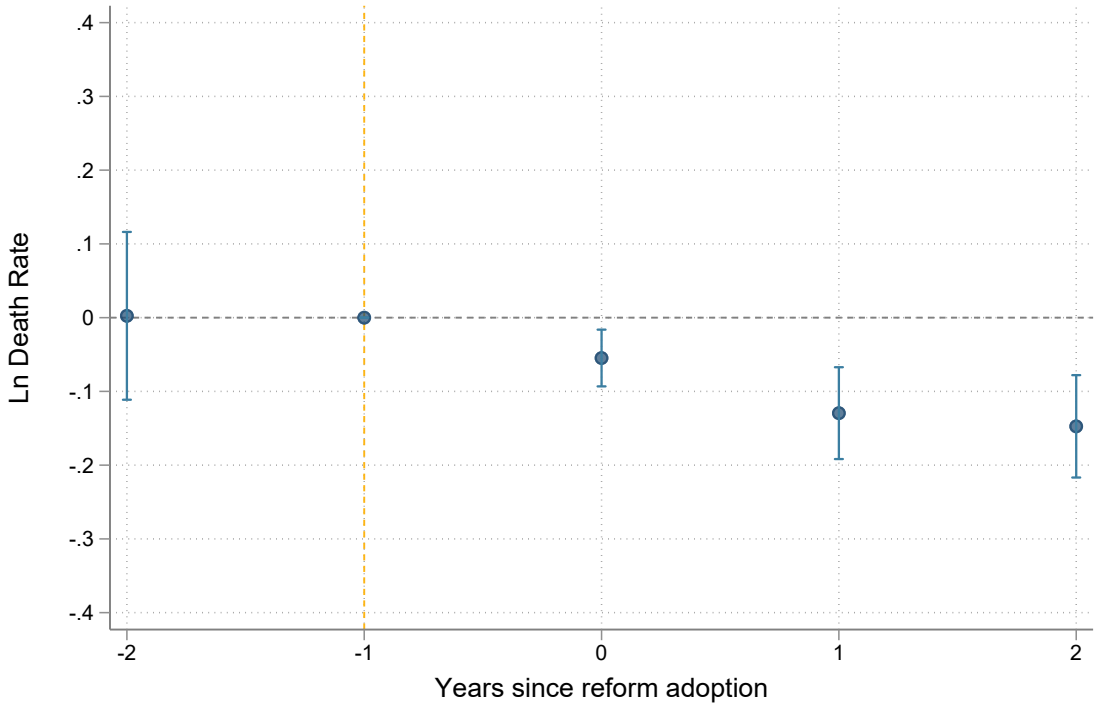
(a) Predicted Numer of Deaths



(b) Predicted Death Rate

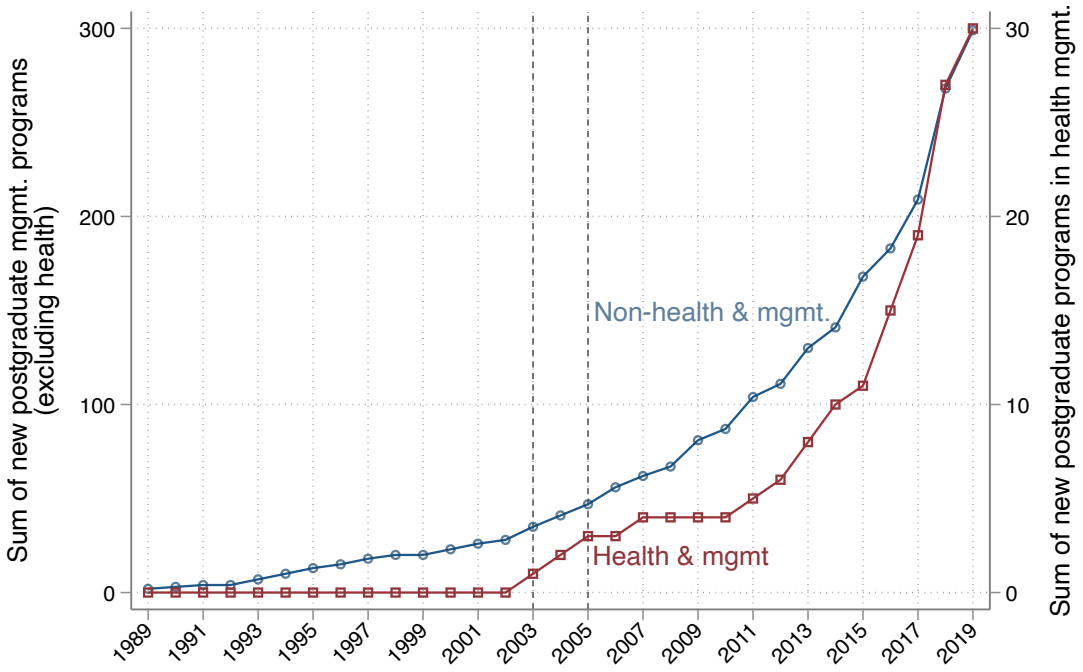
Notes: This figure presents event study evidence on the null effects of the reform on mortality when predicted based on patients' risk-scores. For this exercise, we fit a logit model of deaths on patients' demographics and diagnoses (for details, see Appendix C). Then, we predict the probability of death for each patient, and use these predictions (i.e., patient level risk scores) to construct the hospital-level predicted death rate and number of deaths. Panel A reports estimates obtained from a dynamic Poisson regression of predicted death counts. Panel B reports estimates obtained from a dynamic two-way fixed-effects OLS regression of logged predicted death rates. Each dot corresponds to an estimated coefficient, and vertical lines indicate the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure A.8: Effect of the reform on hospital performance (year-level aggregates)



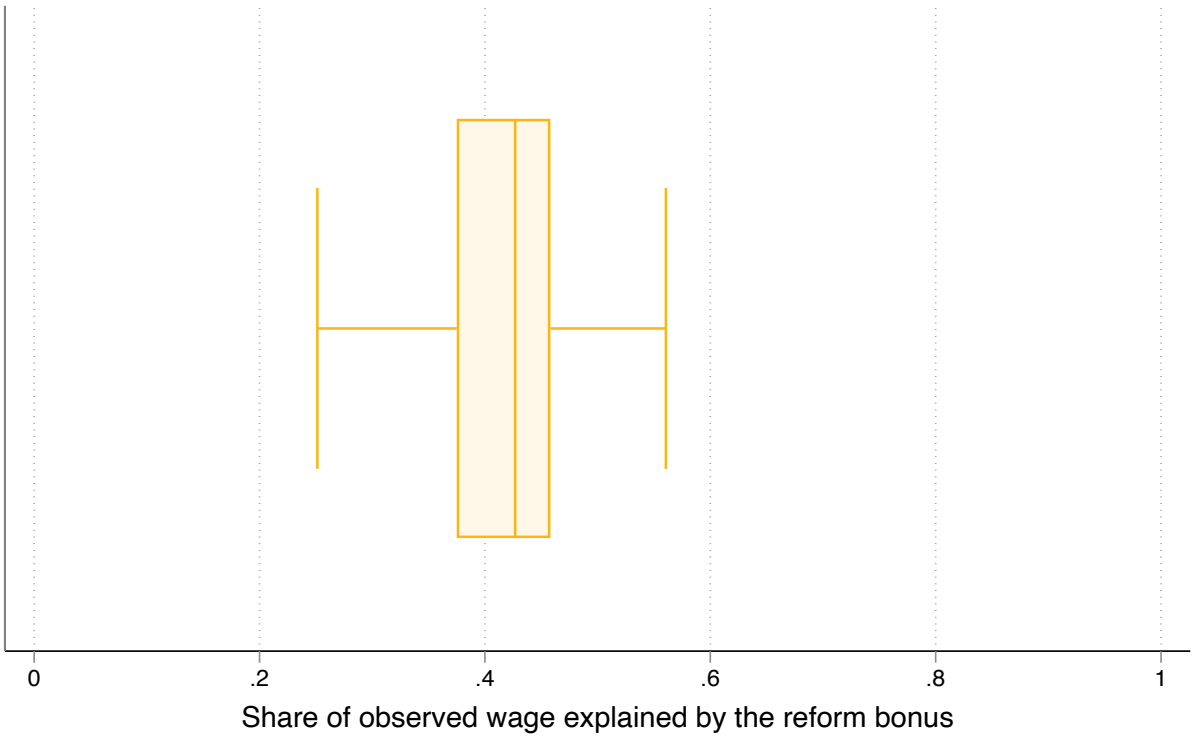
Notes: This figure presents event study evidence of the reform's effect on death rates when the outcome is logged hospital death rates at year level. Each dot corresponds to an estimated coefficient, and vertical lines indicate the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure A.9: Creation of postgraduate programs in health management



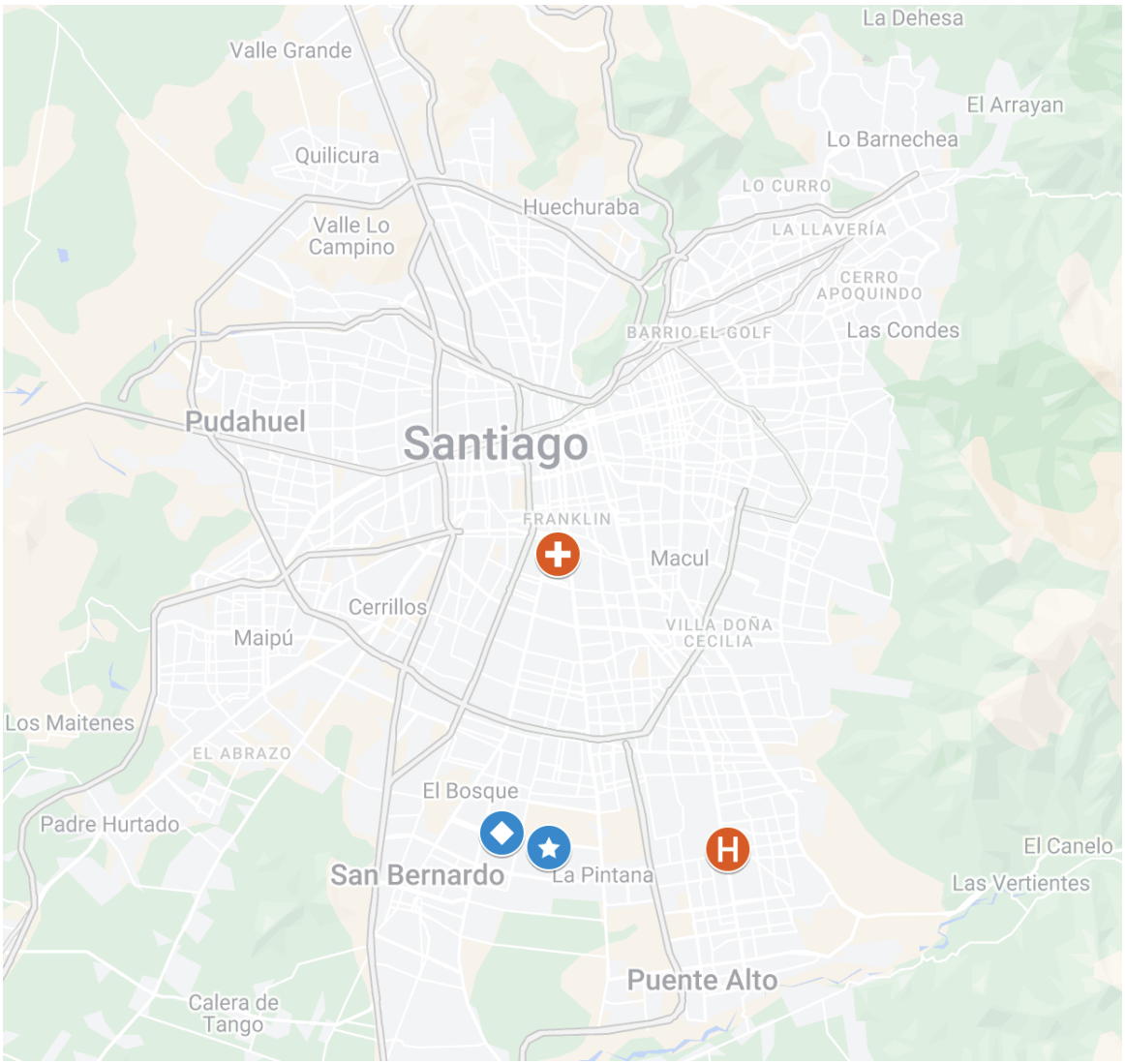
Notes: This figure shows the cumulative number of postgraduate management programs (diplomas and master's) by date of creation. The blue circles depict all management postgraduate degrees, excluding those related to health; corresponding frequencies are displayed in the left y-axis. The red squares depict new postgraduate degrees that include both management *and* health in their descriptions; corresponding frequencies displayed in the right y-axis. Dashed gray lines indicate the years when Law Nº 19,882 (which created the new selection system in the country) was enacted and when the first hospital adopted the new selection system. We use data from programs actively running in 2019, reported by the Consejo Nacional de Educación (https://www.cned.cl/bases-de-datos).

Figure A.10: Share of total CEO wage explained by the reform's bonus



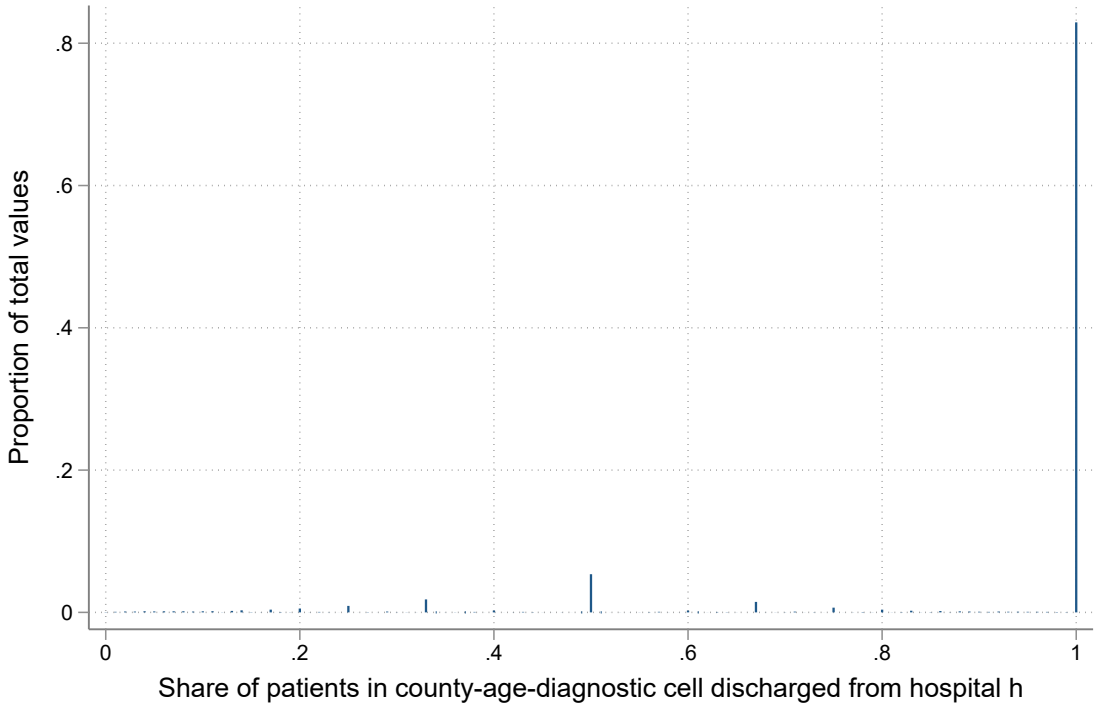
Notes: This figure displays a box plot of the share of the CEO wage explained by the reform's bonus. The sample consists of all CEO positions appointed using the selection reform between 2014 and 2019, which are the dates for which monthly data are available. The average wage share explained by the reform's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

Figure A.11: Examples of referral from primary care centers



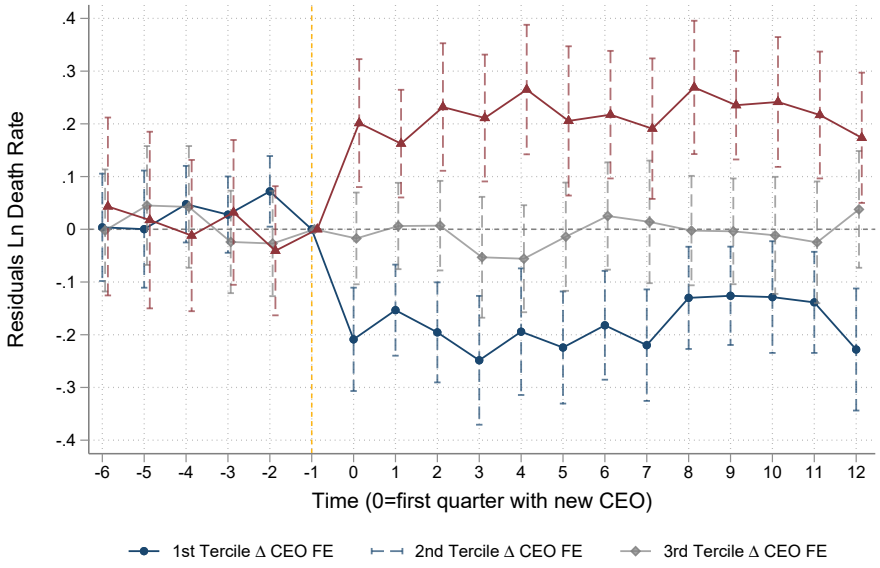
Notes: This figure illustrates an example of patient referral based on their primary care center. The figure depicts two primary care centers, CESFAM Dra. Haydee López Cassou (in blue with a white diamond marker) and CESFAM Pablo de Rokha (in blue with a white star marker), which are located in adjacent Health Services. Although individuals in each primary care center might live close to each other, 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 a white cross marker). Patients from CESFAM Pablo de Rokha are referred to Hospital Sótero del Río (in red with a white H marker). 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.12: Empirical test of patient selection

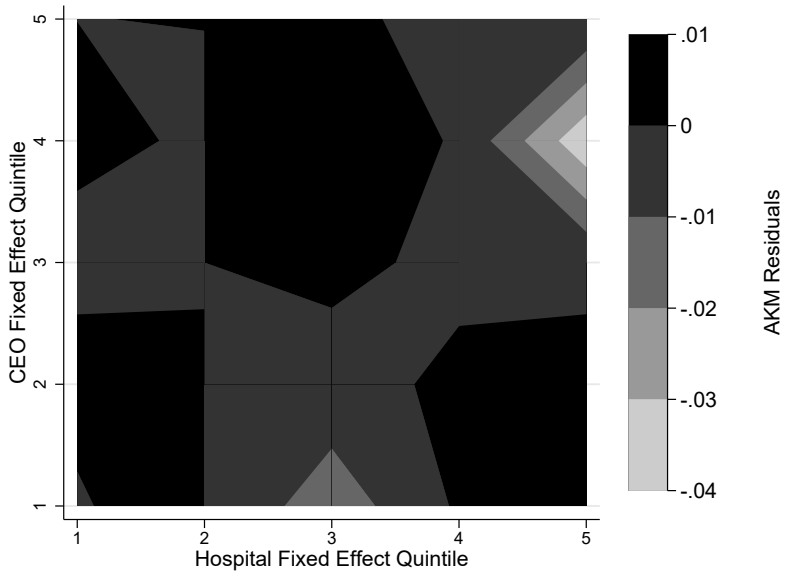


Notes: This figure plots a spikeline with the share of patients in each cell who are discharged exclusively from one hospital. A cell is defined by the patient's county of residence, age group (less than 1 year, between 1 and 15 year, and more than 15 year) and diagnosis (as reported by the hospital 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 diagnosis 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 address to register in the health system.

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



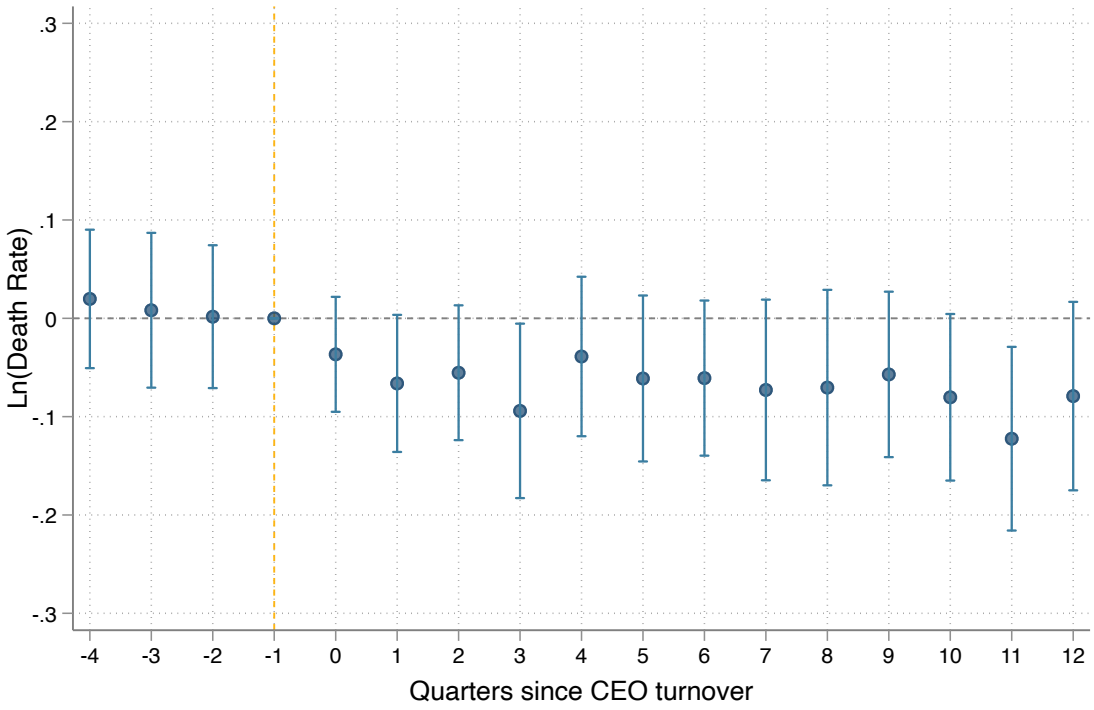
(a) Switchers



(b) Residuals

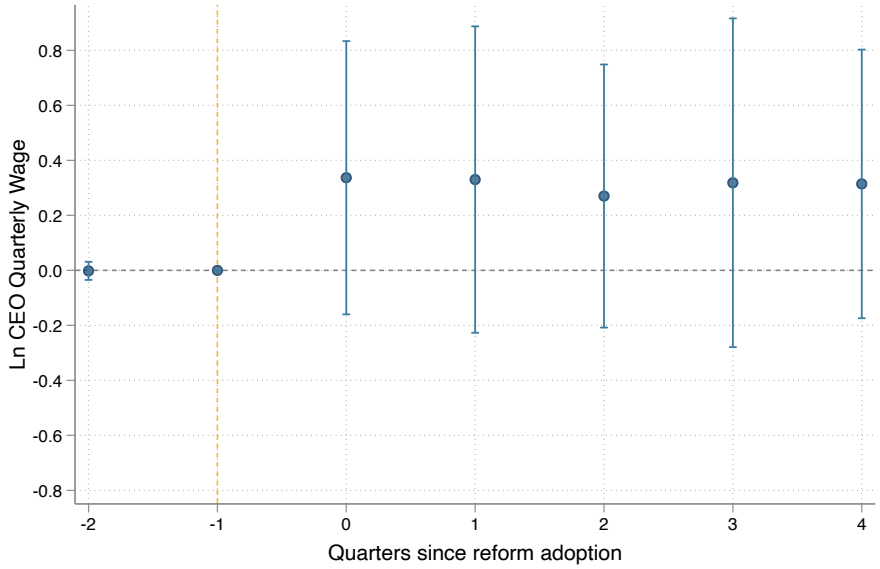
Notes: This figure presents evidence against potential endogenous mobility of managers and in favor of the additive separability assumption between hospital and manager components. Panel A plots the mean (residualized) log death rate against event time (relative to change in CEO events). The figure plots three types of leadership transitions, classified by terciles of the change in 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 vertical lines indicate the corresponding 95% confidence intervals. Panel B shows mean residuals from model A.1 with cells defined by quintiles of estimated manager effect, interacted with quintiles of estimated hospital effect.

Figure A.14: Differential effects of CEO transitions

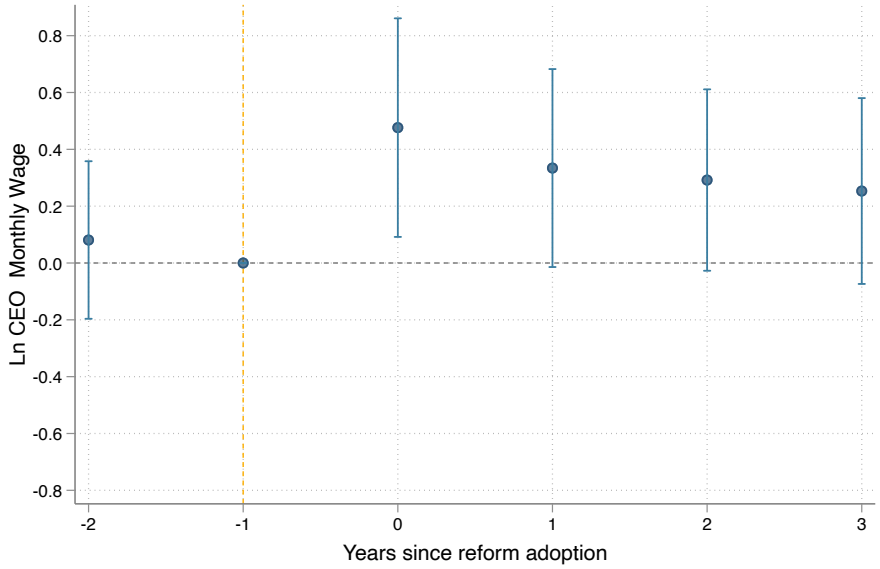


Notes: This figure presents the coefficients of the stacked event study specification in Equation 3, in which an event is a transition from a CEO without management studies to a CEO with management studies. For each transition event, we define a time window around it and a control group of hospitals 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 transitions associated with the first time a CEO was appointed after the selection reform was adopted by a given hospital. In total, there are 94 valid CEO transitions, as described in Online Appendix Table A.4. The dependent variable is the death rate at hospital level in a given quarter. Dots indicate estimated coefficients and vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at hospital level.

Figure A.15: 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 and 2019. Although the estimates are noisy due to the small number of events, the estimate is stable at around 33%. Panel B uses data for December between 2011 and 2019, which allows us to leverage a larger number of events. Regression controls include age and a dummy that indicates whether the individual is a doctor, which affects pay in the public sector. Dots indicate estimated coefficients and vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at 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. Numbers represent the hospital to which the patient is referred. For example, a patient for medical oncology older than 15 years in CESFAM Colina is referred to the Instituto Nacional del Cáncer Dr. Caupolicán Pardo Correa.

Table A.2: Impact on risk-adjusted mortality measures

	Death Rate Ln Actual/Predicted		
	(1)	(2)	(3)
1 if reform adopted in hospital	-0.080***	-0.081***	-0.080***
	(0.022)	(0.022)	(0.022)
Observations	8,104	8,104	8,104
Time FE	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes
Patient Demographics	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	0.787	0.722	0.747

Notes: This table presents our estimates of the impact of the selection reform on risk-adjusted death rates. The estimates are from the staggered DiD specification in Equation 1. We define the 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 C for details. Standard errors are displayed in parentheses and are clustered at hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Hospital performance variance decomposition

	Component	Share of Total	Component	Share of Total
	(1)	(2)	(3)	(4)
$Total$: Var (Log Death Rate)	0.27	100%	0.53	100%
Var (Manager)	0.24	90%	0.23	44%
Var (Hospital)	0.16	59%	0.28	54%
Var (Time)	0.01	2%	0.04	7%
Cov (Manager, Hospital)	-0.14	-52%	-0.08	-16%
Cov (Time, Manager + Hospital)	-0.00	-0.00%	0.00	0.01%
Residualized death rate using case-mix	Yes	Yes	No	No

Notes: This table 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 A.1 in the set of hospitals connected by managers' mobility (Abowd et al., 1999; Card et al., 2013).

Table A.4: 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 (mgmt. studies). We only consider transitions for which there is a time window of 4 periods before and 12 periods after the transition, and do not overlap with another transition in the pre-period within the time window.

Table A.5: 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 their performance contract. In column (1), we replicate the estimation of Equation 1 in the subsample 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 hospital level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Correlation between CEO fixed effect and manager characteristics

	CEO Fixed Effect				
	(1)	(2)	(3)	(4)	(5)
Female	-0.068*	-0.065*	-0.071*	-0.054	-0.052
	(0.037)	(0.036)	(0.036)	(0.035)	(0.035)
Age	0.166***	0.163***	0.163***	0.163***	0.163***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Age^2	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Phyisician		-0.084**	-0.166***	-0.101**	-0.115***
		(0.039)	(0.039)	(0.041)	(0.041)
Mgmt. Background			-0.105**	-0.093*	-0.106**
			(0.053)	(0.054)	(0.053)
Phyisician × Mgmt. Studies				-0.199***	-0.199***
				(0.037)	(0.037)
Observations	8,197	8,197	8,197	8,197	8,185
R-squared	0.101	0.102	0.102	0.109	0.110
Sample	All	All	All	All	Degree data available

Notes: This table presents the correlation between the CEO fixed effects estimated from Equation A.1 and manager characteristics. These characteristics include gender, age, age^2, and a set of indicators for educational background. "Mgmt. Background" refers to undergraduate studies in management and "Mgmt. Studies" refers to postgraduate studies related to management. Controls include connected set fixed effects. Robust standard errors in parentheses.