Managers and Public Hospital Performance[*]

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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. Using a difference-in-differences design, we show how the introduction of a competitive selection system for recruiting public hospital CEOs reduced hospital mortality by approximately 7%. The effect is not explained by a change in patient composition and is robust to several alternative explanations. Instead, we provide suggestive evidence that the reform led to more efficient use of medical resources and improved personnel practices. We then show that the policy changed the pool of CEOs by displacing doctors with no management training in favor of CEOs who had studied management. The mortality effects were largely driven by hospitals in which the new CEO had managerial qualifications.

JEL Codes: H11, H40, I18, M50

1   Introduction

Global government spending on publicly provided goods and services more than doubled between 1980 and 2019 and accounts for approximately 30% of world GDP (Gethin, 2024). Given the scale and scope of this spending, enhancing state efficiency is an important channel for increasing total productivity. Government policies that are intended to strengthen state efficiency often focus on improving the job performance of public sector managers, who directly supervise the delivery of goods and services and are uniquely situated to advance the goals of the state (Pollitt and Bouckaert, 2017). However, 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 challenges. First, quasi-experimental variation in state personnel selection processes is rare. Second, it is difficult to study managerial effectiveness due to the lack of objective and verifiable performance outcomes in the public sector (Besley et al., 2022; Best et al., 2023).

In this paper, we study the impacts of public sector managers in a setting that allows us to overcome these challenges and shed light on key drivers of public sector productivity. Specifically, we analyze a novel policy in Chile that reformed the selection process for senior executives in the public sector. Although this reform applied broadly across all public agencies and departments, we focus on the top managers (CEOs) of public hospitals, which allows us to observe objective and relevant short-term outcomes in assessing their managerial performance. Focusing on public sector hospitals also allows us to study a setting in which government expenditures are large and growing, outcomes for patients are high-stakes, and disadvantaged communities are particularly likely to interact with the public sector.[1]

The policy reform we study was enacted by the Chilean Congress in 2003 and resulted in the introduction of a new personnel recruitment system designed to attract talent to top management positions in the public sector. The reform had two main components. First, it replaced an opaque and discretionary selection process with a public, competitive, and transparent selection system for senior executive positions. Second, it included performance pay incentives and base wage increases to narrow compensation differentials relative to similar positions in the private sector. 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 the new selection system; by 2019, the new system had been used to appoint more than 3,400 senior executives to 1,400 positions.

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[1]Healthcare represents almost 20% of government expenditures in the average OECD country. Between 2000 and 2019, healthcare costs increased by 15% as a share of GDP on average in OECD countries. In the OECD, public hospitals account for an average of 72% of all medical beds (see Appendix Figure A.1).

To study the impacts of public sector CEOs on hospital performance, we build a novel and comprehensive dataset with information on the identity, tenure, educational background, and demographic characteristics of CEOs in all public hospitals. To measure hospital performance, we use data on nationwide individual-level inpatient discharges for all public hospitals, which include detailed patient characteristics, diagnoses, type of admission, and condition at discharge. We complement these data with death records that cover the whole country, which enables us to perform complementary exercises and robustness checks. We also observe inputs for all public hospitals, such as operating room availability and detailed employment and wage records. The data thus provide an extraordinarily rich window into hospital inputs and performance, patient characteristics, and the characteristics and tenure of CEOs in every public hospital in Chile.

We begin by documenting that CEO identity matters for hospital performance. We follow the literature by using hospital mortality rates as our key measure of outcome-based productivity (e.g., Bloom et al., 2015; Doyle et al., 2015; Chandra et al., 2016; Doyle et al., 2019), and show that individual CEOs can account for a significant amount of variation in mortality rates across hospitals. Inclusion of manager fixed effects increases the R^2 by a magnitude similar 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. By further exploiting the rotation of CEOs across hospitals using a two-way fixed-effects model (Abowd et al., 1999; Card et al., 2013), we document that managers account for slightly less than half of the variance associated with different hospitals.

Having established that managers matter, we study the effects of the selection reform. We obtain three main findings. First, we find that the 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 in-hospital death rates by approximately 7% in the 3 years following the adoption of the new system, from a 2.5% death rate average pre-reform. Reassuringly, this result is robust to using alternative measures of hospital performance such as (i) the 28-day death rate, including out-of-hospital deaths; (ii) the death rate among emergent and among non-deferrable admissions (Card et al., 2009); and (iii) risk-adjusted mortality (Ash et al., 2012). Our result is also robust to an alternative difference-in-differences model using the count of deaths in a Poisson regression (Chen and Roth, 2023).

To provide evidence for the validity of our research design, we first show that in the years before the first hospital adopted the reform, the growth of an exhaustive set of variables—including hospital outcomes, patient characteristics, and political variables—does not differ between hospitals that do and do not adopt the new selection process, and neither do they correlate with the timing of adoption by hospitals that ever adopted the new selection system. Second, using an event study design, we show graphically that hospitals do not exhibit different trends in mortality rates before

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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 changes in performance. Third, we show that the dynamic effects of the reform gradually increase during the early quarters post-adoption, which is the expected trajectory if new managers are presumed to have an impact on performance. Also, we provide event study evidence showing that CEO turnovers per se have no impact on hospital performance, which rules out mechanical or Hawthorne effects of the reform due to CEO turnover.

One might also worry that our estimates reflect changes in patient composition rather than improved CEO performance: Perhaps after the reform, managers admitted healthier inpatients or patients self-selected into hospitals with improving performance. We provide several pieces of evidence to address this concern. First, we underscore that 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 by primary care centers to public hospitals following strict guidelines; also, hospitals cannot legally select patients based on their characteristics (Ley 19,937; Decreto 38). Second, in our baseline estimates we use an exhaustive set of case-mix controls that include detailed information on patient demographics and diagnoses, and show that our results are robust to using alternative risk-adjusted death rate measures. Third, we do not find any evidence of impacts of reform adoption on hospital deaths that could be predicted based on patients' demographics and diagnoses. Fourth, to address further concerns regarding selection on unobservables, we examine the effect of the reform on deaths outside treated hospitals. To the extent that patients who were 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 municipality level. Finally, we also find that the reform had similar effects when we focus exclusively on lower-income patients who cannot easily access healthcare in the private sector and are "locked-in," or when we restrict the analysis to a subset of patients who, based on observable characteristics, are strictly following the referrals mandated by the health system.

Consistent with the effects on hospital mortality, we find suggestive evidence that the reform induced more efficient use of hospital resources and improved personnel practices. We find that in the 3 years following the reform, access to fully equipped and readily available operating rooms increased by 17%, from 8.6 hours to 10 hours per day. We also find that the reform reduced the turnover of doctors despite finding null effects on their compensation. These results are consistent with findings by Bloom et al. (2015), who document a positive correlation between improved management practices, hospital performance, and reduced staff turnover. The results are also in line with recent research in personnel economics, whereby better-managed firms retain workers with higher human capital (Bender et al., 2018).

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Our second finding is that the reform dramatically increased the share of CEOs with management training, which we show is the main predictor of the reform's efficacy in reducing hospital mortality. We first document that the reform replaced doctors who worked as CEOs ("doctor CEOs") with new CEOs with undergraduate degrees in management-related majors.[2] Before the reform, virtually all CEOs were doctors. After the reform, the share of CEOs with undergraduate degrees in management increased by almost 25 percentage points while the share of doctor CEOs decreased by a similar magnitude. Interestingly, the reform had a large and negative effect only on doctor CEOs without management training, while it substantially increased the share of doctor CEOs with managerial qualifications. We present suggestive evidence that through this channel, the reform also had an effect by incentivizing doctors who wanted to apply for a CEO position to invest in management studies. Overall, we find that, soon after it was adopted, the new selection process boosted the percentage of CEOs with management training by 40 percentage points. We also show that the reform increased CEO managerial talent—as proxied by their estimated CEO fixed effect (Abowd et al., 1999)—by 0.18 standard deviations. In terms of CEO demographics, the reform led to the appointment of managers who are approximately 2 years younger, and 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 management training and find that the beneficial effects on hospital mortality are primarily driven by CEOs with managerial qualifications. While this suggestive result is consistent with the hypothesis that management training improves the performance of hospital CEOs, we cannot rule out that the effect is explained by differential selection (i.e., better managers are more likely to study management). Before the reform, the norm was that CEO positions in public hospitals were reserved for doctors.[3] This pattern, in which top executives rise up from the lower ranks of their profession, is ubiquitous in public sector organizations such as police departments, school districts, and universities (McGivern et al., 2015). In this regard, our results suggest that management skills are transferable across organizations and, importantly, that management training yields benefits for public sector organizations even when managers acquire such training later in life. A natural policy implication is that public sector organizations may wish to emphasize management education when recruiting executives, even if those candidates rise up from the lower ranks of their respective professions.

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

[3]Based on Civil Service's qualitative interviews, we posit that the norm was upheld by a shared belief across doctors that individuals with no medical training should be largely excluded from CEO positions and should 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 final decisions about how to run a hospital. Furthermore, biased beliefs about the importance of management may have previously discouraged doctors from investing in management training.

The third finding is that the reform's financial incentives—namely, performance pay and higher base wages for newly appointed CEOs—did not play a role in driving the mortality effects. We start by ruling out the possibility that performance pay affected managerial performance. We show that incentives were poorly designed and not binding—a feature that was common across appointments in all government agencies using the reform's new selection system, and not specific to public hospitals. We also document that the policy increased CEO wages by approximately one-third relative to pre-reform wages. To examine whether these higher 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 increased wages for treated managers but had no discernible effects on their performance. While we cannot rule out the possibility that the financial incentives impacted the applicant pool, we interpret these results as evidence that efficiency wages did not affect the level of effort exerted by post-reform CEOs and did not drive the impact on mortality.

This paper contributes to several strands of the literature. Our work relates to research in the public sector that examines the impacts of discretionary appointments (Myerson, 2015; Martinez-Bravo et al., 2022; Xu, 2018; Colonnelli et al., 2020; Voth and Xu, 2022) and personnel selection and civil service recruitment (Dal Bó et al., 2013; Estrada, 2019; Ashraf et al., 2020; Muñoz and Prem, 2024; Moreira and Pérez, 2022; Dahis et al., 2023; Aneja and Xu, 2024). More broadly, our research adds novel evidence on bureaucratic effectiveness and its impact on development (see Besley et al. (2022) for a review). We contribute to this literature by focusing on hospital managers and showing that a civil service reform led to the improvement of health outcomes. By documenting the null effects of financial incentives, our work also connects with a large literature on the role of pay for performance in the public sector (Lazear, 2000; Khan et al., 2015; Biasi, 2021; Deserranno et al., 2023).

Our work also contributes to a nascent literature on public sector organizations that studies the impact of top managers (Coelli and Green, 2012; Choudhury et al., 2019; Limodio, 2021; Fenizia, 2022; Best et al., 2023) and management practices (Bloom et al., 2015; Tsai et al., 2015; Rasul and Rogger, 2018; Azulai et al., 2020) on organization-level performance.[4] This paper also contributes to a related literature on the effect of management practices on hospital outcomes (McConnell et al., 2013; Bloom et al., 2015; Chandra et al., 2016; Bloom et al., 2020) and medical practices (La Forgia, 2023).[5] Moreover, our work complements previous research on methods for improving operating room efficiency (e.g., He et al., 2012) and the efficacy of alternative policies for improving hospital performance (e.g., Propper and Van Reenen, 2010; Gaynor et al., 2013;

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[4]Management's effects on organizational performance can operate through the manager herself, organizational-level management practices, or a combination of both (see Metcalfe et al. (2023) for a discussion).

[5]Other research examines the effects of shifting ownership within healthcare organizations, such as the privatization of public hospitals (Duggan et al., 2023) and private equity buyouts of hospitals (Cerullo et al., 2022) and nursing homes (Gupta et al., 2023).

Doyle et al., 2019).

Closest to our work, Janke et al. (2020) study the impact of CEOs in NHS hospitals in the UK and find little evidence of CEOs' impact on different dimensions of hospital performance. In contrast to our setting, CEO recruitment in NHS hospitals does not have strict selection criteria and is relegated to local boards.[6] By studying a selection reform that dramatically shifted CEOs' managerial qualifications, 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). Recent work on the impact of management training yields mixed results. Acemoglu et al. (2023) focus on CEOs in private firms and find no effects on firm performance, alongside negative effects on wages. Giorcelli (2024) examines management training for middle managers and supervisors in US wartime industrial facilities, and finds significant performance improvements. Relative to these papers, we focus on public sector organizations and document that post-reform CEOs with management training improve outcomes.[7] We also find no effects on employee wages, which is consistent with wage rigidity in the public sector. Finally, our results on reduced physician 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).

The rest of the paper proceeds as follows. Section 2 describes the setting and data and presents descriptive evidence on the effect of managers on hospital performance. 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 recruitment effect of the reform on managers' characteristics. Section 5 examines the effects of the financial incentives included in the reform, and Section 6 concludes.

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.

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[6]Interestingly, in the NHS, only one-quarter of CEOs have postgraduate management training, which is similar to the average in our setting before selection reform adoption. The reform increased the share of CEOs with postgraduate management qualifications to almost 60% the quarter after adoption (and up to 66% if we also consider undergraduate management degrees).

[7]This is consistent with findings by Bloom et al. (2020), who document a positive correlation between the share of hospital middle managers with MBA-type degrees and hospital performance.

Individuals can opt out and use their health contributions to buy private insurance.[8] Individuals who are unable to pay can freely access the public system, which results in nearly universal health coverage.

Approximately 78% of the population is covered by a public health scheme, 15% by private insurance; and the remainder by insurance programs exclusive to the police and armed forces. Whereas the ability of individuals to use their health contribution to buy private coverage has led to sorting across the private and public health sectors, there is little scope for selection within the public health sector.[9] This is because individuals with public coverage cannot choose their health provider within the public network.[10] Individuals must register with 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 the closest public hospital in emergency cases.

Public healthcare providers are organized geographically under 29 "Health Services," the administrative units within which the referral and counter-referral system is organized. These are decentralized organizations subject to oversight by the Ministry of Health, and each is responsible for the articulation, management, and development of public primary, secondary, and tertiary healthcare establishments in municipalities in their territory. The head of the Health Service is also the immediate superior of CEOs of public hospitals within their territory.

Public hospital CEOs are in charge of the management, organization, and administration of their hospitals, and their duties include, among others, (i) the administration of personnel, (ii) allocation of hospital inputs and human resources, (iii) management of financial resources and proposing the annual budget, (iv) decisions regarding infrastructure and technological equipment resources, and (v) integration of the hospital into the health network and the community.

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[8]This is similar, for example, to the public healthcare system in Germany, where individuals, upon meeting certain criteria, 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]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).

[10]Although 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, 97% of patients in public hospitals have public insurance. Under some public insurance plans, individuals can choose private health centers, although they are more expensive than public hospitals. Around 25% of inpatients in private hospitals have this coverage.

2.2 The Recruitment Reform

In 2003, a political scandal exposed illegal payments to top government officials. In response to—and as a product of— broad political consensus, Congress enacted Law N° 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 "to provide government institutions—through public and transparent competitions—with executives with proven management and leadership capacity to execute effectively and efficiently the public policies defined by the authority."[11]

The reform had two main components. First, it changed the recruitment process for top managers in government agencies. Before the reform, most senior executive positions were discretionary appointments by the superior officer. Under 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 Service 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 a predetermined 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 predetermined criteria. In the last step, the superior officer selects the winning candidate from the final roster with discretionary authority. Appendix Figure A.2 illustrates 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.[12] In the case of public hospital CEOs, we document that the reform increased the position's pay by 33%. The financial package also included a performance pay component, under which the yearly wage is reduced if the manager does not meet certain performance thresholds. We provide more details of changes in the pay schedule in Section 5.

Adoption of the new recruitment process occurred gradually across public agencies. The law

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[11]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/.

[12]Two limits cap the extra bonus. First, it cannot be larger than 100% of the base wage. Second, the total wage cannot be higher than that of the Under Secretary of the Ministry in which the position is based.

mandated that between 2004 and 2010, the Ministry of Finance would designate a minimum of 100 top executive positions across different government agencies to adopt the new recruitment system. All new top management positions created after the law was enacted were required to select their top manager using the new system. For existing positions, the Ministry of Finance had to approve adoption of the new selection system for each position within any agency that adopted it.[13] Upon approval, the new recruitment mechanism becomes effective after the agency initiates a new selection process. Once a position in a given agency is subject to the new recruitment system, all future appointees in that position must be hired by the new process.

Panel A in Figure 1 depicts the number of positions in public agencies that adopted the recruitment reform between 2004 and 2019.[14] Panel B focuses on adoption of the recruitment policy for CEOs in public hospitals between 2005 and 2019, which is the variation we leverage in our empirical design. The first time a public hospital adopted the selection system was during the fourth quarter of 2005, after which other hospitals adopted it gradually.

2.3 Data

For this paper, we build a novel dataset that identifies the CEO in every public hospital in the country, spanning every month between January 2005 and December 2019. Because these data were not available in a systematic way, we filed several hundred 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 collect date of birth, gender, 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 post-reform CEOs.

We also access restricted-use administrative records that cover the universe of employees in all public hospitals between 2011 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. Among other characteristics, we observe the

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[13]The reason is twofold. First, the Civil Service, which operates under the purview of the Ministry of Finance, 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 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 financial commitment.

[14]In Appendix Figure A.3, we show 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.

establishment, the person's occupation, number of hours worked, date of birth and gender, and a detailed wage breakdown. Between 2011 and 2013, records were only collected for the month of December.

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 2005 and 2019, which encompasses around 16.5 million events. Data include the diagnosis according to the International Classification of Diseases, Tenth Revision (hereafter, ICD-10 code); type of admission (e.g., emergency case or referral); date of discharge or the date of death in case the individual died in the hospital; and a set of individual characteristics: date of birth, gender, county of residence, and type of health insurance. For robustness checks, we link data at the individual level with country-wide death records processed by the Vital Records Office, which we can access until 2018. We observe the date, cause, and place of death. Finally, we also collect the usable hours for operating rooms from the REMs (*Resúmenes Estadísticos Mensuales*) compiled by the Ministry of Health, starting in 2009. We complement the data with a set of hospital characteristics such as size, location, and whether it is public or not, among others.

Finally, to determine the timing of the policy, we use data on all hospital CEO recruitment processes conducted by the Civil Service, which are publicly available on their website. The information includes the hospital, the recruited individual's identity, and the date of appointment date.

Hospital Performance: Our main outcome of hospital performance are death rates, 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; Chandra et al., 2016; Hull, 2020; Gupta, 2021; Chan et al., 2023). A critical concern, however, is that hospital mortality might reflect shifts in the observed and unobserved characteristics of patients, which potentially biases the results of the analysis. The Chilean public health setting is well suited for our 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 or work, age, and diagnosis. Also, hospitals cannot reject patients or unilaterally counter-refer them to other hospitals and must abide by the protocols.[15] We provide further details in Appendix A.

Throughout the paper, we consider the in-hospital death rate as our main measure of hospital performance. Nonetheless, we check the robustness of our results to alternative measures. First, to account for deaths that occur shortly after discharge (Gaynor et al., 2013), we construct a measure that considers deaths in the hospital or at any other location 28 days after a patient's admission.

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[15]Importantly, hospital CEOs cannot unilaterally change the referral protocols in their hospitals to avoid sicker patients. 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 the Ministry of Health.

Second, to assess hospital performance among patients for whom immediate medical attention is critical, we leverage information on patients' diagnoses and whether they are admitted through the emergency unit to calculate the death rate among emergent patients and among patients with non-deferrable diagnoses, who are more likely to need urgent medical attention (Card et al., 2009). Finally, following the procedure described by Ash et al. (2012), we also construct a risk-adjusted measure of performance as the ratio between the actual hospital-level death rate and the death rate predicted based on the risk score of hospital patients.

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 analysis. Aggregating the data for each hospital at quarterly 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. Then, we compute each hospital death rate as the number of deaths over admissions in a given quarter.

Our final sample consists of 178 public hospitals—of which 83 adopted the recruitment reform at some point between 2005 and 2019—for a total of 10,010 hospital-by-quarter observations. Table A.2 presents descriptive statistics of this sample. The average hospital in our sample discharges 1,512 patients per quarter, while the median hospital discharges 586 patients. On average, 59% of these discharges correspond to female inpatients and 34% to inpatients younger than 29 years old. 97% of patients discharged from public hospitals have public insurance. Regarding hospital outcomes, the average hospital experiences 40 deaths per quarter, with a corresponding in-hospital death rate of 2.87%. The 28-day death rate—which considers both in- and out-of-hospital deaths— is larger and corresponds to 4.46%.

Do Hospital Managers Matter? Before examining the effects of the reform on hospital performance, we start by discussing the extent to which the individual identity of CEOs explains part of the observed variation in hospital mortality. To this end, 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 (1) includes only case-mix controls. Column (2) incorporates time fixed effects, while column (3) introduces hospital effects, and column (4) adds CEO effects. The adjusted R^2 increases from 0.46 in column (2) to 0.69 in column (3), which implies that hospital effects account for substantial variation in hospital mortality. The R^2 further increases to 0.75 in column (4) after inclusion of CEO fixed effects—an increase of magnitude similar to that reported by Bertrand and Schoar (2003) and Fenizia (2022). Formally, an F-test rejects the null hypothesis that all CEO effects are zero.

In light of research that casts doubt on this type of approach (Fee et al., 2013; Janke et al., 2020), in Appendix C we also assess the relative importance of hospitals and managers by estimating a two-way fixed effects model, which allows us to perform a variance decomposition analysis.

Importantly, this model also provides us with estimates of CEO fixed effects, which are a useful measure of managerial talent we use later in the paper. 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 Abowd et al. (1999). As shown in Appendix Table A.4, bias-corrected measures of the variance components within the largest connected set (Andrews et al., 2008) suggest that CEO fixed effects explain 21% of the variation in mortality—about half the magnitude of the permanent component of mortality associated with different hospitals (44%). We also find that the (bias-corrected) covariance between CEO and hospital fixed effects is 6%, which suggests that the best managers are slightly more likely to work at higher-mortality hospitals.

Having established that the identity of the hospital managers is correlated with hospital performance, we now turn to evaluate the consequences of a reform that modernized the selection process of hospital CEOs.

3 The Reform's Impact on Hospital Performance

3.1 Research Design

In this subsection, we explore the adoption of the selection reform by public hospitals and show that the timing of adoption is not correlated with changes in hospital performance prior to adoption. Based on these findings, we then leverage the adoption of the selection reform over time as a source of quasi-experimental variation to study its impact on hospital performance.

Reform Adoption in Hospitals: We start by comparing the characteristics of hospitals that adopted the selection reform at some point (ever-adopters) with the characteristics of hospitals that never adopted it (never-adopters) on a set of variables related to inpatients, hospital outcomes, and political outcomes at the hospital's municipality level. We consider before-adoption data and define a window of 1 year before the period of adoption for ever-adopters and a window of 1 year before the first adoption in the country for never-adopters.

In column (1) of Table 2, we present the average for hospitals that did not adopt the reform within our study period (2005-2019). Column (2) presents the OLS estimate on a dummy variable that takes a value of 1 for ever-adopter hospitals and 0 otherwise. We find that adopters have a larger number of inpatients—consistent with the intent of policymakers to give priority to larger and more complex hospitals. They also have slightly higher in-hospital death rates, and serve patients who are relatively younger and less likely to use public health insurance. Finally, they are located in municipalities that exhibited less support for left-wing politicians in the 2004 mayoral election. In sum, public hospitals that adopted the selection reform differ from those that had not

adopted it by the end of 2019.

However, the quarterly growth of a set of variables before the reform was enacted is not correlated with whether the hospital adopted the reform. To assess whether adopting the reform is associated with hospital characteristics that trend differently (e.g., hospitals that perform better over time are more likely to adopt the new recruitment system), column (3) reports the OLS coefficients of a regression of the first difference of each characteristic on a dummy that takes value 1 for ever-adopter hospitals and 0 otherwise. We do not observe that units that adopted the reform exhibit significantly different trends than never-adopters in terms of the number of patients, their characteristics, hospital outcomes, or political determinants. We next assess whether, within adopters, early adopters exhibit different trends compared with late adopters. To this end, we use the cross-section of ever-adopters the year before adoption and estimate an ordered logit using the order in which ever-adopters begin to use the new recruitment system as the dependent variable and all characteristics in first differences as explanatory variables. Column (4) reports coefficients and standard errors from this regression. All of the coefficients are statistically indistinguishable from zero, which suggests that within adopters, the timing of adoption is uncorrelated with trends in the number of patients, their characteristics, hospital outcomes, or political determinants.

Empirical Strategy: The above results lead us to consider the timing of adoption as a plausible source of exogenous variation to estimate the causal impact of the reform on hospital performance using a staggered difference-in-differences research design. The identifying assumption is that, in the absence of the reform, adopters would follow parallel trends compared with never-adopters and yet-to-adopt hospitals. Concretely, we consider the following model:

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

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.[16] α_h represent hospital fixed effects that control for unobservables specific to the hospital, and γ_t are time-by-region fixed effects to account for unobservable time shocks specific to each region.[17]

Since changes in the outcome variable could reflect changes in patient composition, we follow the literature and include 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, we include the share of female inpatients, the share of inpatients within each of eight

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[16]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, the adoption of recruitment reform is an absorbing treatment, and the dummy variable takes the value of 1 for all periods after the first manager is hired under the new regime.

[17]For each hospital, we focus on a time window of 12 months before and 18 months after adoption of the new recruitment system, but we do not impose a window restriction. Instead, we fully saturate the model with event time dummies for periods outside the endpoints.

age bands (0-29, followed by 10-year increments up to 90+), and interactions between these demographic shares. We further account for hospitals' inpatient risk by including the share of inpatients within each of the 31 categories of the enhanced Elixhauser comorbidity index (Elixhauser et al., 1998; Quan et al., 2005). To control for of patients' socioeconomic status, X_{ht} also includes the share of inpatients for each of 6 categories of health insurance.[18] 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 performance weighted by the hospital's size in 2004.

3.2 Main Results and Threats to Identification

In Table 3, we report the coefficients and their standard errors obtained by estimating Equation 1 using different death-related measures of hospital performance. Column (1) shows that reform adoption led to a 7.4% decrease in in-hospital death rates, which implies a decrease in the death rate toward 2.67, over a sample mean of 2.87 deaths per 100 patients (i.e., 2 fewer deaths per 1,000 patients). Likewise, column (2) shows that the logged 28-day death rate, which includes in- and out-of-hospital deaths, decreased by 5.5% after reform adoption, from a sample mean of 4.47 to 4.22 deaths per 100 patients. Thus, the impact of the reform is robust to accounting for deaths that occur outside the hospital within 28 days after discharge.

To further assess the impact of the reform, we focus on patients for whom immediate medical attention is critical and selection is unlikely: emergent patients and patients with non-deferrable conditions.[19] Column (3) shows that reform adoption led to a 7.7% decrease in the death rate of patients with non-deferrable diagnoses, while column (4) shows that the reform led to an 8.5% decrease in the death rate among emergency admissions. Finally, in column (5), we use a Poisson model to estimate the effect of the policy on the number of deaths. Our result shows that the reform decreased the number of in-hospital deaths by around 4%—i.e., $\exp(\hat{\beta}) - 1$, where $\exp(\hat{\beta})$ is the incidence rate ratio of deaths.

Event Study Evidence: One of the main threats to identification is that the timing of the reform's adoption is correlated with changes in hospital performance. To partially assess the validity of the parallel trends assumption, we estimate the following event study model:

$$y_{ht} = \alpha_h + \gamma_t + \sum_{\tau} \beta_\tau D_{ht}^\tau + \epsilon_{ht}, \qquad (2)$$

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[18]Public insurance has four levels, with copays varying by income and family size. Including these, there are six health insurance categories: four public levels, private insurance, and one for missing data.

[19]Following Card et al. (2009), we classify diagnoses as more or less deferrable based on whether their admission rates are similar on weekdays and weekends. Inpatients whose diagnoses have weekend admission rates above the median are classified as non-deferrable. Inpatients who were admitted through the emergency unit are classified as emergent.

where D_{ht}^{τ} is a dummy variable that indicates the reform was adopted τ periods earlier (or will be adopted τ periods ahead for negative values of τ). The β_{τ} coefficients can be interpreted as the effect of the reform on hospital quality for each τ quarter, relative to the quarter before adoption. We normalize the coefficients such that $\beta_{\tau=-1} = 0$, and we focus on a window of 6 quarters before and 12 quarters after adoption.[20]

Figure 2 displays the point estimates of β_{τ} and their confidence intervals. The dynamic effects show that the pre-reform 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. After the reform, the estimates turn negative and statistically significant, and increase gradually. In this case, it does not seem that the change in management is driven by a previous worsening (improvement) in hospital performance, which would lead us to overestimate (underestimate) the true impact (if any) of the treatment. As a robustness check, in Appendix Figure A.4, Panel A, we present estimation results using the model suggested by Borusyak et al. (2024), which is robust to treatment effect heterogeneity. Results are robust and follow the same dynamic trajectory regardless of the estimation strategy. In Panel B, we consider a dynamic Poisson model that uses death counts—as opposed to the death rate—and find analogous results. Finally, in Appendix B, we consider whether CEO turnovers—through, for example, Hawthorne effects—could partially explain our results. Leveraging CEO turnover, we do not find any evidence of this phenomenon.

Testing for Patient Selection: A second threat to our estimates is that they might reflect changes in patient composition rather than actual improvement in hospital performance. Although we already control for detailed patient characteristics, we provide additional robustness checks to address this concern. To this end, we fit a logit model for the outcome of death using the set of case-mix controls and more than 1.1 million patient-level pre-reform data, and tpredict aggregate deaths at hospital level. With this measure, we estimate the effect of the reform on the "risk-adjusted mortality rate," which, following the UK's NHS (Health and Centre, 2015), we define as the ratio between the actual hospital-level death rate and the predicted death rate—i.e., an increase (decrease) from 1 means more (fewer) deaths than predicted deaths.

Table 4 presents the results. 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' health insurance, a proxy for socioeconomic status. Finally, column (3) corresponds to our preferred measure, which also includes patients' diagnoses based on the enhanced Elixhauser comorbidity index. Results are stable across columns and show that the reform decreased the ratio of actual over predicted death rate by 8%, which implies that hospitals' death rate decreased by more than

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[20]We do not impose a window restriction for estimation. Instead, we fully saturate the model with event time dummies for periods outside the endpoints.

what can be predicted based on their patients' case mix.[21] 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 US, where the institutional setting is more prone to patient selection (Doyle et al., 2019).

While the results are robust to using alternative risk-adjusted mortality rates, the risk-adjustment procedure is fundamentally based on patients' characteristics, which raises additional concerns. Managers may have incentives to influence diagnoses for billing or revenue purposes (Silverman and Skinner, 2004), or they may reject older or sicker patients based on the severity of their illness.[22] While careful consideration of our setting's characteristics suggests that these concerns are unlikely to drive our results, we can empirically assess them by examining whether the hospital-level risk score changes upon a new CEO's appointment.[23] Column (4) in Table 4 presents difference-in-differences estimates of the reform on log mortality rates predicted by patients' risk scores, and Panel B in Appendix Figure A.5 presents event study evidence. Overall, we find no evidence of changes in hospital risk scores after adoption of the reform.

Finally, there could also be selection on patient characteristics unobservable to the econometrician. Perhaps managers are able to reject sicker patients in a way that does not change observable patient characteristics (supply-side selection on unobservables), or healthier patients are more likely to go to a given public hospital if they observe that its performance is improving (demand-side selection on unobservables). To indirectly test whether supply-side selection on unobservables causes 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 appear 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 of nearby hospitals. Panel A in Figure A.6 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.

To examine whether sorting on unobservables is biasing our results, we exploit two features of our setting. First, we leverage the fact that lower-income patients have access to free or very

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[21]In Appendix Figure A.5, Panel A, we show results for event study estimates on the ratio of actual over predicted death rates, using our preferred measure of risk-adjusted mortality. Reassuringly, we do not observe pre-trends on this outcome.

[22]Also, the reform may induce mechanical effects on patients' diagnoses if, for example, new managers bring in new doctors who differ systematically in their diagnostic practices (Song et al., 2010; Finkelstein et al., 2017; Badinski et al., 2023).

[23]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); there is no clear way the hospital CEO could manipulate diagnoses. On the other hand, the law forbids CEOs from selecting patients based on their condition and they must adhere to referral and counter-referral guidelines.

low-cost healthcare in public hospitals; some of them cannot buy private healthcare using their public insurance, and consequently are "locked-in" in the public health network. Second, we can empirically identify the set of patients who strictly comply with the referral guidelines described in Appendix A.[24] For this analysis, we estimate Equation 2 using smaller samples that comprise "locked-in" patients and strict referral complier patients. The results of this approach—which should mute demand-side sorting, if any—are presented in Figure A.7. Reassuringly, in both restricted samples we find a similar impact of the reform on hospital performance.

3.3   How is the Reform Improving Hospital Performance?

In this subsection, we examine key hospital inputs that may have served as mechanisms through which managers improved hospital performance following the reform. Concretely, we focus on operating rooms (ORs) and personnel. 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.[25] The efficient use of ORs is a large operational and management problem, and management practices are a crucial lever for improving it (see, e.g., He et al., 2012).[26] 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), and personnel turnover can negatively impact outcomes in public sector organizations (Akhtari et al., 2022). Moreover, staff retention has been highlighted as one of the primary challenges in the context of public healthcare organizations (NHS, 2020).

We find that the recruitment reform had a significant and economically meaningful effect on the availability of ORs. We run the specification given by Equation 2 on the logged number of usable hours per operating room. Data on the number of operating rooms and usable hours are from monthly summaries collected by the Ministry of Health (*Resúmenes Estadísticos Mensuales*). The number of ORs reflects the number of ORs that have the equipment, instruments, and other elements needed for major surgical intervention in conditions of asepsis and safety. Usable hours correspond to hours available for surgical interventions plus hours of preparation (concurrent and

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[24]Cases not classified as strict compliers do not necessarily imply non-compliance with the established referral and counter-referral guidelines; this could be due to data limitations. For details, see Appendix A.

[25]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, medical staff must 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).

[26]For instance, planning and scheduling must consider OR availability and match the workload to medical staffing, 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).

terminal disinfection, cleaning instruments, etc.). These data start in 2009, and since many hospitals adopted the reform around that year, we restrict our pre-reform window of analysis to a shorter length. We consider 4 quarters before and only consider hospitals that have non-missing data for all quarters. Panel A of Figure 3 shows event study evidence of the positive effects the reform had on operating room availability. The results show that, by the last quarter within our time window, the reform had increased the number of usable OR hours by 17%, which implies around 1.5 more usable hours per OR each day.

We now turn to the reform's effects on personnel turnover. We use administrative data on hospital personnel from the Human Resources Information System used by the Ministry of Health. A drawback of this high-quality data is that it only starts in 2011, and until 2013 is only available at yearly frequency. Thus, we perform the analysis at annual level within the timeframe 2011-2019, estimating the model given by Equation 2 on the average turnover rate. Hospital turnover is defined as the number of workers who will leave the next period (either job-to-job or job-to-unemployment transitions) over the number of workers currently employed. Panel B of Figure 3 shows the result. Although the results are noisy due to the limited number of events post-2010, this is suggestive evidence that the reform reduced the turnover of doctors by approximately 8% and had no discernible impact on other health workers. As shown by Appendix Figure A.8, the reform 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 the manager can negotiate directly with doctors, such as schedule flexibility.

3.4   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 with those of patients studied elsewhere. For each comparison, we present the average death rate in different samples used in the literature and in our sample after we match them according to patients' characteristics. Note, however, that although we can match the sample of patients in some dimensions, such as 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 A.3.

We first compare the effect of the CEO selection reform with the impact of increasing health spending. Doyle et al. (2015) examine the effect in the US 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 data 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 that the reform we study has a similar effect (9%) over a similar average death rate in the sample.

Second, 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 (emergency admissions for AMI). 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%. In this regard, improving CEO selection has a larger effect over a slightly larger sample mean.

4 The Recruitment Effects of the Reform

The evidence so far suggests that the reform's impact on hospital performance is not driven by patient selection. Since the key component of the reform was the introduction of a competitive recruitment system—which changed the identity and characteristics of hospital CEOs—we next examine this mechanism.

4.1 Impact of the Reform on CEO Characteristics

To examine the recruitment effects of the policy and evaluate how the new selection process changed the characteristics of new CEOs, we use the same research design as before but replace the independent variable with manager-specific characteristics. Concretely, we estimate Equation 1 using $X_{M(h,t)}$ as the dependent variable, where X are individual-specific traits such as educational background, CEO fixed effects, 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 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 4 presents the results. Panel A shows that the reform increased the share of CEOs with undergraduate management degrees by 24 percentage points, from a baseline of only 2%.[27] The increase in the number of CEOs with this background came almost completely at the expense of displacing doctor CEOs—who before the policy's adoption accounted for 95% of CEO positions—and a slight negative effect on CEOs from health professions other than doctors. The reform did not have an effect on hiring professionals with a background in other disciplines. Importantly, Panel B in Figure 4 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 13 percentage points, from a baseline of 17%, while substantially decreasing the number of doctor CEOs without management training by 33 percentage points, from a baseline of 77%.

In Figure 5, Panel A, we focus on the dynamic effects of the policy on whether the new CEO has management undergraduate studies or is a doctor CEO with postgraduate management training. An interesting finding is that the effect on the likelihood of recruiting CEOs without a medical background wanes over time. The reform increased CEOs with a management undergraduate degree by around 30 percentage points the quarter immediately after adoption, but the effect decreased over time to slightly less than 20 percentage points.[28] In the case of doctors, we observe the opposite effect. The effect gradually increases from around 5 percentage points immediately after adoption to more than 15 percentage points by the end of the 3-year window.

The increase in the share of doctor CEOs with management training 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 training in the absence of the reform. But, since the reform was gradually adopted across the public health sector, the policy may also have incentivized doctors who wanted to be appointed CEOs to pursue formal management studies. To explore this hypothesis further, in Panel B of Figure 5 we present estimates of the impacts on the likelihood that the CEO is a doctor with postgraduate management training interacted with whether the hospital is an early or late adopter. Early (late) adopters are hospitals that adopted the selection reform in a quarter before (after) the median date of adoption. In line with our hypothesis, we find that the policy did not increase doctor CEOs with management training in early adopters, but only did so in hospitals that adopted the reform later. This finding is consistent with the fact that before

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[27]Since the timing of adoption varies across hospitals, we compute the baseline in a period of 6 quarters before each hospital adopted the new recruitment system.

[28]The effect changes over time because CEOs' tenure is, on average, shorter than 3 years. Therefore, the effect is detecting the characteristics of more than one post-policy manager.

2003—the year the reform was enacted—there were no health management postgraduate programs in the country. Indeed, as shown in Appendix Figure A.9, the opening of the first health management postgraduate programs coincides with the timing of the reform. The figure also shows that management postgraduate programs in areas other than health were available for a long time before. Qualitative anecdotal evidence further supports the claim that these new programs are geared toward doctors seeking careers in health administration.[29]

In Table 5, we summarize the impact of the policy on whether the CEO has management training, regardless of whether she is a doctor, and on other CEO characteristics. Column (1) presents the effect of reform adoption on the likelihood of appointing CEOs with management training. The average across ever-adopters 1.5 years before the reform was 19%.[30] The reform increased the likelihood that CEOs have a management undergraduate degree or management postgraduate training by 40 percentage points, which is explained by both professionals with management undergraduate degrees and doctors with management training being appointed to CEO positions after the reform. In column (2), we use CEO fixed effects as a measure of managerial talent, and—since fixed effects can only be compared within connected sets—we saturate the regression and include connected set indicators (see Appendix C for details). We find that the reform led to a 0.18 standard deviation increase in the average managerial talent of appointed CEOs.[31] Columns (3) and (4) focus on demographics. We find that managers are almost 2 years younger than they would have been in the absence of the policy. An interesting finding is that the reform did not have any impact on female appointments to CEO positions. In line with the widely documented underrepresentation of female CEOs in the private sector (Bertrand, 2018), the average pre-policy share of female CEOs was 19% in our setting.

4.2   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. Concretely, we refer to skill mismatch as the extent to which individuals are employed in an occupation unrelated to their main field of study.

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[29]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.

[30]As a point of comparison, in NHS hospitals, Janke et al. (2020) report that 26% of CEOs have postgraduate managerial training. 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 one-quarter of managers (including non-CEO managers) report having received management training.

[31]This effect translates to a 12% decrease in death rates (given that the mean and standard deviation of the death rate in this sample are 2.81 and 1.87, respectively). Note, however, that this effect is not directly comparable to the headline impact of the reform on hospital mortality because it is estimated within connected sets and in the sample in which we can compute CEO fixed effects.

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 jobs. 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). In the public sector, several factors may create skill mismatches that may hinder performance.[32] 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 study whether correcting the skills mismatch in this setting enhances the organization's performance. To examine whether CEOs with management training 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 training and 0 otherwise. The working assumption is that CEOs with management training are well matched, and the rest represent mismatches.

Table 6 presents the results. Column (1) replicates our main specification in the sample for which we have CEO background information and shows a similar effect. Columns (2)-(4) assess the skills-mismatch hypothesis. In column (2), we compute the differential effects of the policy in cases in which the manager has *any* management training, including undergraduate and postgraduate studies. We find that newly appointed CEOs who have management training are associated with a significant decrease in death rates (8.6%) vis-a-vis newly appointed CEOs without management training (2.7%). The differential impact of post-reform CEOs with and without management training is statistically significant (p-value = 0.028). Column (3) replicates the previous analysis using the sample of doctor CEOs and finds quantitatively similar results. Finally, in column (4), we compute the differential effects between CEOs with no management training, doctor CEOs with management training, and non-doctor CEOs with management training. Again, we find that the reform only had significant effects when the appointed CEO had management training. We do not find statistical differences in performance between doctor and non-doctor CEOs when both have management training. This suggests that management training is the main predictor of performance compared with other educational background characteristics.[33]

The finding that CEOs with management training improve organizational performance might be at odds with the results of Acemoglu et al. (2023), who show that managers with a business

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

[33]To complement this evidence, we regress CEO managerial talent—measured by the CEO fixed effect estimated in Appendix C—on observable CEO characteristics: gender, age, age squared, and a set of indicators for educational background. Appendix Table A.5 presents our results. We find that younger and female CEOs are associated with higher managerial talent; also, consistent with the findings presented in this section, we find that management training is highly correlated with better managerial performance.

degree do not improve firm performance and reduce employees' wages by means of rent-sharing practices.[34] A key difference is that in our setting, business managers perform in the public sector, in which they have fewer incentives to reduce employees' wages and fewer ways to do so, given the rigidity of public sector wages. Further, 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; Ashraf et al., 2020).

4.3   What Explains Skills Mismatch in Public Hospital CEO Positions?

Given the significant impact on performance delivered by CEOs with management training, why aren't all public hospitals managed by CEOs with this background? A primary reason is that before 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 was no statutory rule prohibiting non-medical professionals from being selected as CEOs, in 2004—the year before the first hospital implemented the selection reform—99% of public hospital CEOs were doctors. The policy had a big effect on changing this stylized fact: By 2019, the fraction of CEOs with medical degrees in treated public hospitals had decreased to 53%.[35]

Anecdotal evidence allows us to conjecture why this norm emerged and was sustained over time. According to responses to a small survey administered by the Civil Service to public hospital CEOs, doctors tend to believe that individuals with no medical training should be barred 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).[36]

This belief may have discouraged doctors from investing in management training: If doctors believed that management training would not improve their performance as CEOs, there was no reason for them to pay for management postgraduate studies.[37] Further, given that 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

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[34]Appendix Figure A.8 shows that the reform did not impact hospital wages of employees other than the CEO.

[35]The finding that de facto prohibiting individuals with non-medical degrees from becoming CEOs hinders organizational performance is consistent with recent research showing that discrimination against qualified managers can reduce organizational performance (Huber et al., 2021) and, more broadly, that talent misallocation reduces aggregate output. (Hsieh et al., 2019).

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

[37]This is consistent with the findings of Bloom et al. (2015), who show that a significant initial barrier to adopting management practices was the belief among firms that the practices would not be profitable.

to invest in postgraduate management education in the absence of future monetary returns.

5 The Financial Incentives Effects of the Reform

The recruitment reform also included a change in financial incentives in the form of pay for performance and higher base wages for CEOs in hospitals that adopted it. Low-powered incentives and low wages in the state are often highlighted as one source of the inefficient performance of public employees. Perhaps post-reform managers improved hospital performance simply because they exerted more effort due to the newly introduced financial incentives. This concern is supported by recent research indicating that financial incentives can improve employee performance in the public sector. (Khan et al., 2015; Biasi, 2021; Deserranno et al., 2023). In this section, we examine the effects of the reform on financial incentives and do not find evidence supporting the hypothesis that our results are explained by performance pay or higher wages.

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

$$\text{Yearly Wage}_t = \begin{cases} 100\% & \text{if} \quad \text{performance}_{t-1} \geq 95\% \\ 98.5\% & \text{if} \quad 65\% \leq \text{performance}_{t-1} < 95\% \\ 93\% & \text{if} \quad \text{performance}_{t-1} < 65\%. \end{cases} \tag{3}$$

Two things are worth noting about the schedule. 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 adoption of the reform and their yearly performance scores.[38] Figure A.15 presents the cumulative distribu-

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[38]Unfortunately, some of the oldest contracts and performance scores are lost, and the Civil Service has no available records. We have performance score data for 57 post-reform CEOs.

tion of all available performance scores for the first post-reform CEO in each hospital that adopted the reform. Note that 70% of the distribution is at or above the 95% threshold and avoids any wage penalization. The rest is between 95% and 65%, which is the lowest threshold to avoid a 7% wage penalty, and there are virtually no observations below 65%. This suggests that the performance agreements were not binding, and most managers easily met performance goals.

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 that call for its amendment (see, e.g., CPPUC, 2013; Barros et al., 2018). In light of this stylized fact, we conclude that in our setting, performance pay is not likely to be a relevant driver of managerial productivity.

5.2   Results Are Not Driven by Higher Wages

An alternative mechanism is that the results are driven by efficiency wages. According to this hypothesis, paying managers a wage above their outside option creates an incentive to exert extra effort and can elicit productivity growth (Katz, 1986). If the reform's pay increase creates labor rents, this mechanism might be at play in our setting.

We start by analyzing the wage increase. The pay hike 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, Panel A, we present a box plot showing the share of CEO wages accounted for by the reform's wage bonus. It reveals that, on average, the bonus explains around 44% of the wage for post-reform CEOs. Second, in Panel B, we complement this descriptive evidence with an event study that assesses the reform's effect on CEO wages.[39] On average, we find an effect of the same order of magnitude, albeit somewhat smaller. Part of the difference might be explained by the fact that we do not observe the change in the CEO's remuneration but rather in the position's remuneration. Hence, the effect is a composite 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 modifica-

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[39]We use yearly data for every December between 2011 and 2019.

tion, CEOs appointed after November 2016 can choose to be paid according to the medical pay laws instead of the public employees' pay grade, but *only if* they are medical doctors.[40] The medical pay law is 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 educational 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 answer this question, we perform a stacked event study in which an event is a turnover 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 turnover and determine an event-specific control group that excludes hospitals that experienced an event as described above. We select events that are balanced in the time window, then append the data for all valid events and estimate the following event study regression:

$$y_{hte} = \alpha_{he} + \gamma_t + \sum_{k=\tau} \beta_\tau D_{hte}^\tau + \epsilon_{hte}, \tag{4}$$

where e is an event. An observation is at hospital-by-time-by-event level and includes hospital-by-event and time fixed effects. We cluster standard errors at hospital level.[41]

Panels A and B in Figure 6 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 an approximately 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, which leads us to 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. An important caveat is 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 attracts 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—or better career opportunities (Ashraf et al., 2020; Bertrand et al., 2020)—could widen the pool of high-quality applicants in our setting and,

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

[41]There is a trade-off between the length of the window and the number of valid events. We consider 2 periods before treatment and 4 periods post-treatment, for which we have 17 valid events.

through this mechanism, affect performance.

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 that introduced a competitive recruitment process for hiring public sector CEOs, and find that it reduced hospital mortality by approximately 7%. We show that this result is not explained by patient selection and is robust to other explanations. In contrast, we find suggestive evidence that the reform improved operating room availability and reduced physician turnover.

We also examine whether the financial incentives included in the reform explain our findings. First, we document that the performance pay incentives were poorly designed and not binding. Then, leveraging an amendment to the reform, we 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 studies. Further, we find that management training is a good predictor of treatment effect heterogeneity. Since this result may be due to differential selection, we view this evidence as suggestive and highlights an important avenue for future research.

To conclude, we note that the reform shifted two margins of personnel selection that could be important 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 a medical degree—is likely to improve the allocation of talent in the public sector. Second, as discussed above, the extra pay likely plays a role by attracting higher-quality candidates to the pool of applicants. Disentangling the two margins is another promising avenue for future research.

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Figure 1: Adoption of the new recruitment process
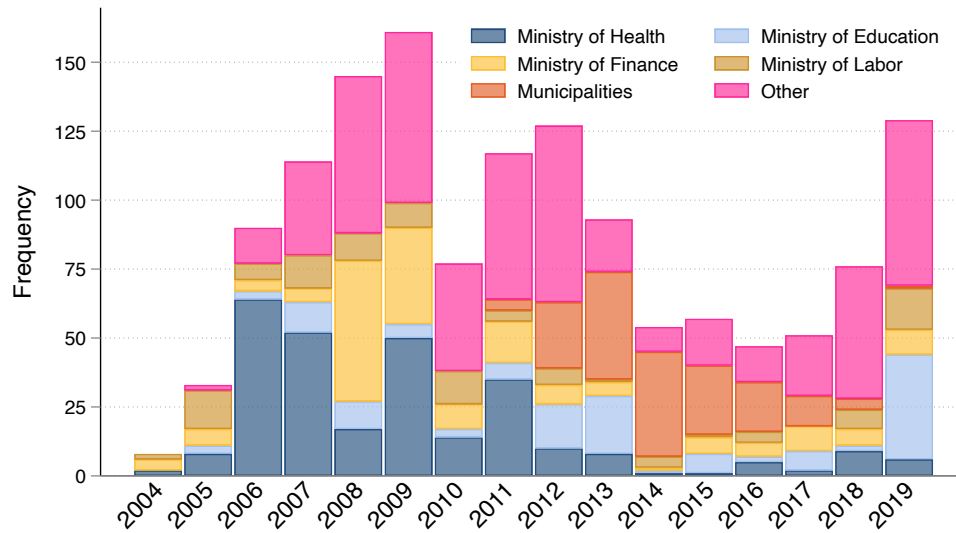
(A) Adoption of the reform across all government agencies
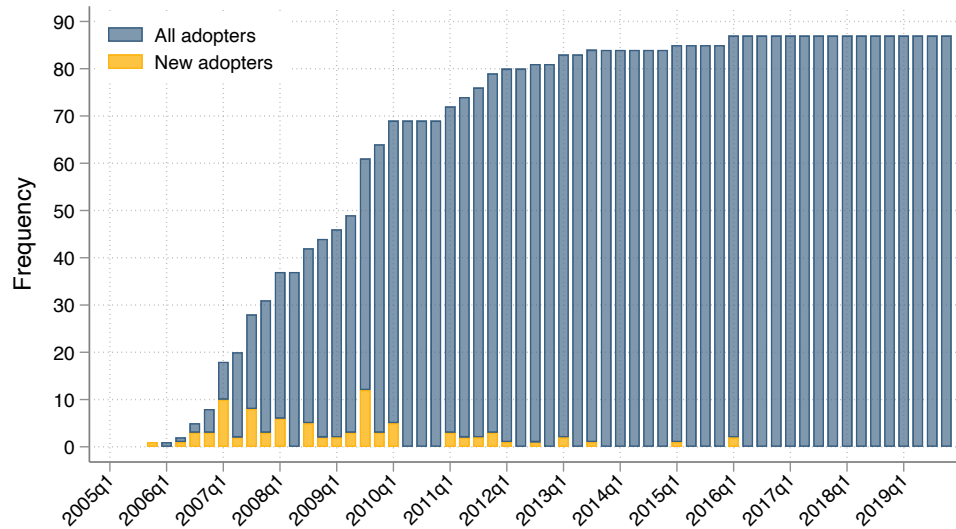
(B) Adoption of the reform across public hospitals

Notes: Panel A displays the rollout of the selection reform across government agencies. An observation is a position in any government agency that uses the new selection system 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 shows the adoption of the selection reform for CEOs in public hospitals. A new adopter represents a hospital that uses the new 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.

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Figure 2: Dynamic effects of the reform on hospital quality
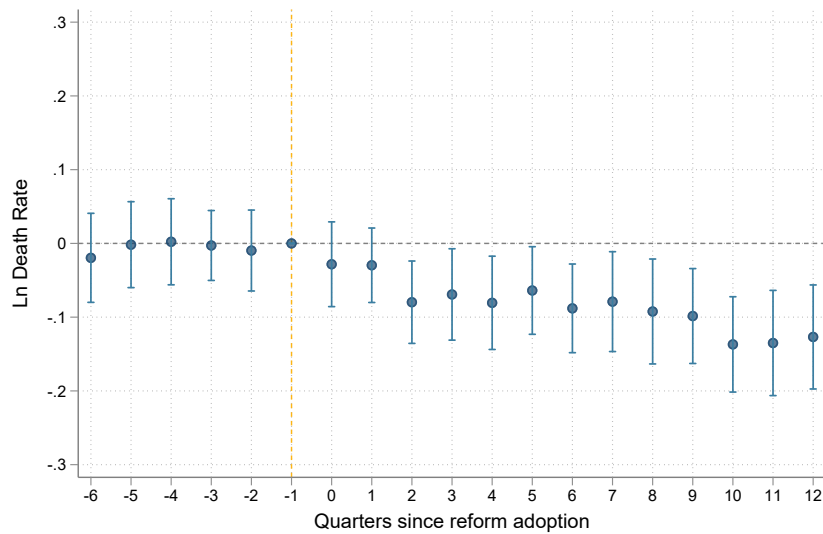
(A) In-hospital Death Rate

Notes: This figure presents event study evidence of the reform's effect on hospital death rates, following Equation 2. The empirical analysis uses quarterly panel data on public hospitals and includes case-mix controls. We focus on a time window covering 6 quarters before and 12 quarters after the reform was adopted by each hospital and exploit the gradual adoption of the selection reform in public hospitals during that period. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Estimates are weighted by the pre-policy number of inpatients. The dashed yellow line represents the omitted coefficient. Standard errors are clustered at hospital level.

Figure 3: How is the reform improving hospital performance?
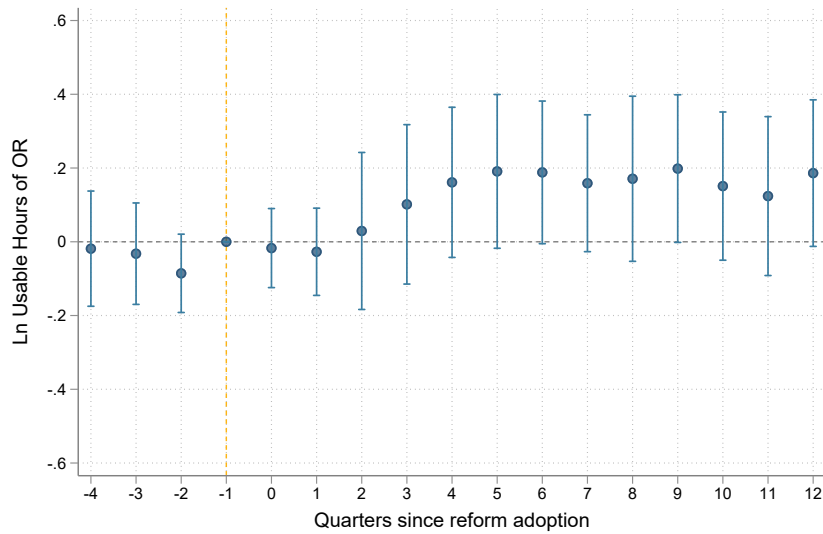
(A) Usable hours of operating rooms
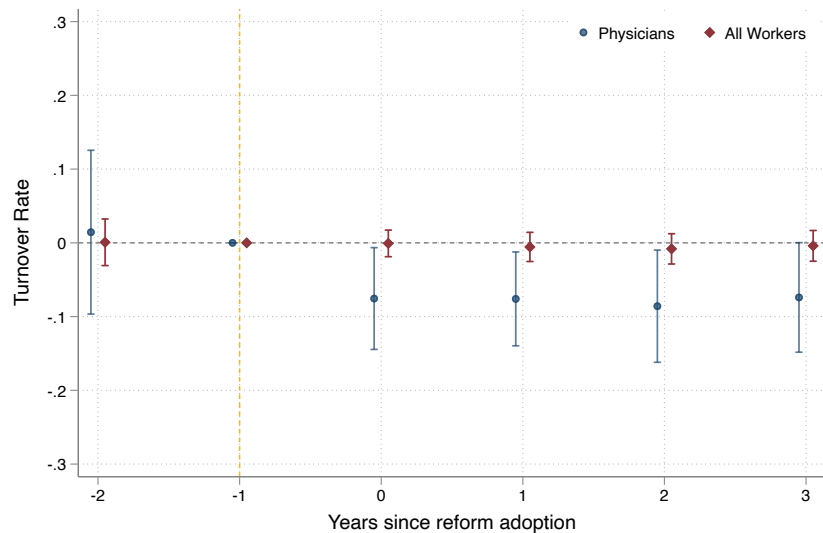
(B) Turnover of health workers

Notes: This figure presents event study evidence on the reform's effect on OR availability and personnel turnover, following Equation 2. Panel A uses data at quarterly level on the logarithm of usable hours per operating room and includes case-mix controls. Estimates are weighted by the pre-policy number of inpatients who went through surgery. Panel B uses data at yearly level on the turnover of doctors (circle markers in blue) and hospital personnel (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. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure 4: The policy displaced doctor CEOs with no management training
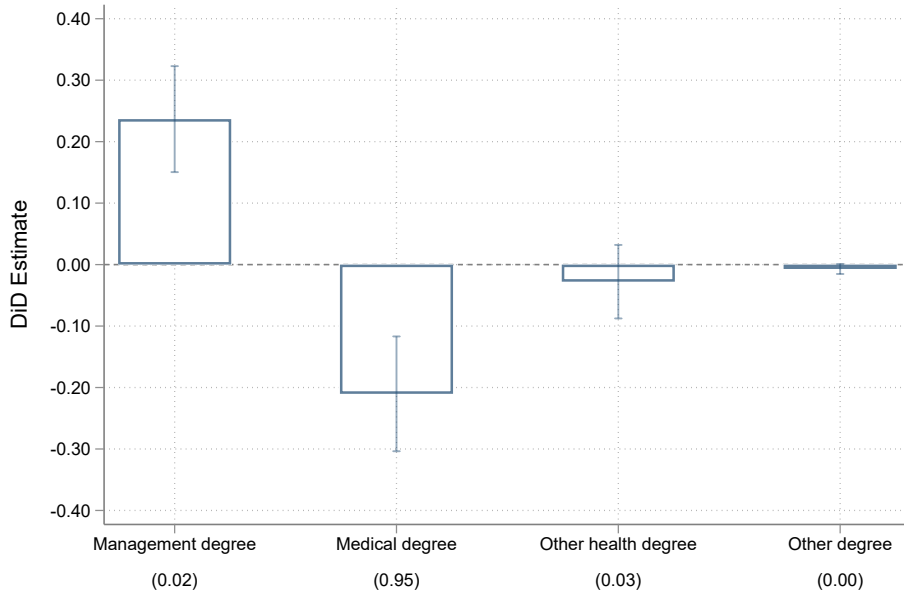

(A) All degrees
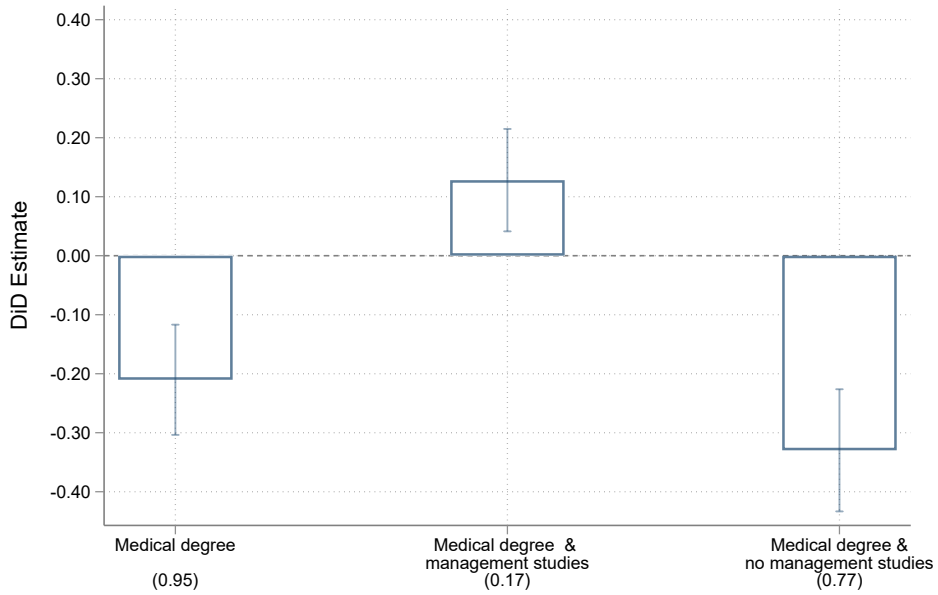
(B) Doctors

Notes: This figure presents the effect of the policy on the CEO's 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 to assess the impact of the reform on the likelihood that the CEO is a doctor with and without management training (as of the date of their appointment as CEO). Bars represent the estimate from Equation 1 on each outcome, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are clustered at hospital level.

Figure 5: Dynamic effects by CEO educational background
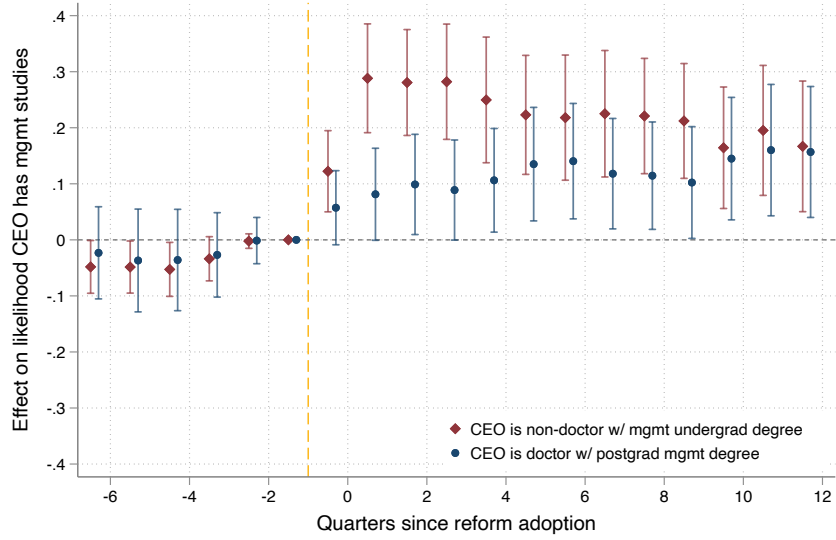

(A) By CEO background
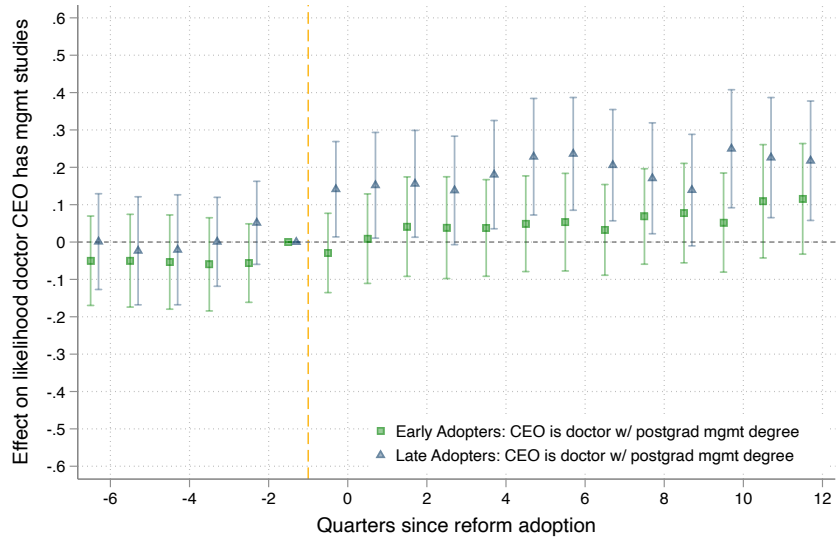
(B) Doctor CEOs with management training: Early v. Late Adopters:

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 red with diamond markers). The second corresponds to a dummy variable that takes value 1 if the CEO is a doctor and has a postgraduate degree in management (in blue with dot markers). Panel B presents estimates of the impacts of the reform on the likelihood that the CEO is a doctor with postgraduate management training interacted with whether the hospital is an early or late adopter. Early (late) adopters are hospitals that adopted the selection reform in a quarter before (after) the median date of adoption. Early adopter impacts are depicted by light green squares, and late adopter impacts by light blue triangles. Vertical lines indicate corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure 6: Do efficiency wages impact death rates?
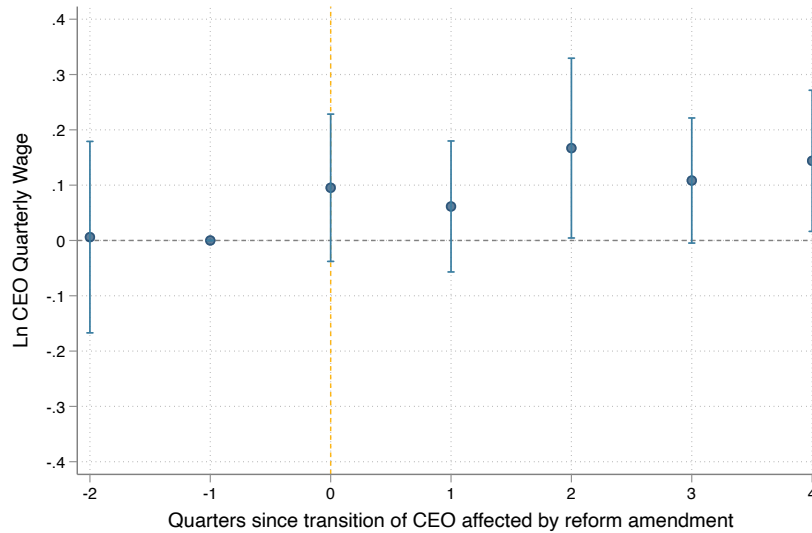
(A) Effect on CEO wages
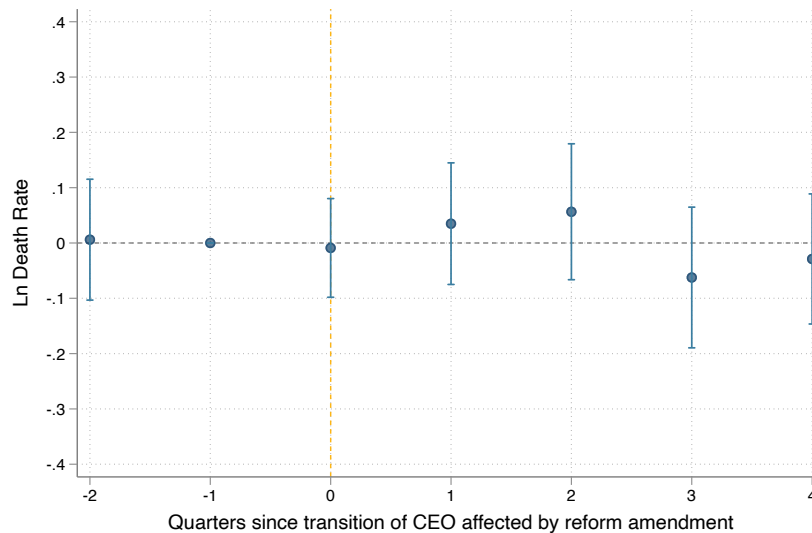
(B) Effect on death rates

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 were doctors *and* were appointed using the selection reform after November 2016. For each event, we define a time window around the event and determine an event-specific control group that excludes otherwise treated hospitals. We select events that are balanced in the time window. There are a total of 17 valid events. We append the data for all valid events and estimate an event study following Equation A.1. Panel A presents estimates of the amendment's effect on CEO wages, and Panel B displays the impacts on death rates. The regression of wages includes a quadratic polynomial of age and a dummy that indicates whether the individual is a doctor. The regression of death rates includes case-mix controls and estimates are weighted by the pre-policy number of inpatients. Dots indicate estimated coefficients, and vertical lines indicate corresponding 95% confidence intervals.

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

	Ln Death Rate				
	(1)	(2)	(3)	(4)	(5)
Observations	10,010	10,010	10,010	9,912	9,912
R^2	.45	.47	.70	.78	.78
Adj. R^2	.45	.46	.69	.75	.75
Time FE	No	Yes	Yes	Yes	Yes
Hospital FE	No	No	Yes	Yes	No
Manager FE	No	No	No	Yes	No
Hospital-Manager FE	No	No	No	No	Yes
F-statistic for Manager FEs	-	-	-	7.10	-
F-statistic for Hospital-Manager FEs	-	-	-	-	10.35

Notes: This table investigates how much of the variance in mortality is explained by the hospital, manager, and time components. We report the R^2 from a regression of logged death rates on the set of fixed effects reported in the table. All regressions include hospital patients' case-mix controls (share of female inpatients, share of inpatients within each of eight age bands, and interactions between these demographic shares; share of inpatients within each of the 31 categories of the enhanced Elixhauser comorbidity index (Elixhauser et al., 1998; Quan et al., 2005), and the share of inpatients with each of 6 categories of health insurance). F-statistics at the bottom of the table come from testing the null hypotheses that manager and hospital-manager effects are jointly zero.

Table 2: Balance in observable characteristics before the reform

	Avg. never adopter	β Ever adopter (Levels)	β Ever adopter (First-Diff)	Timing of Entry
	(1)	(2)	(3)	(4)
% Age < 29	0.375	0.162	0.003	1.279
		(0.130)	(0.005)	(2.044)
% Age ∈ (30,49)	0.219	-0.050	0.003	-2.392
		(0.044)	(0.005)	(1.723)
% Age ∈ (50,69)	0.187	-0.022	-0.004	14.976
		(0.047)	(0.004)	(13.082)
% Age ∈ (70,89)	0.200	-0.078	-0.002	-14.971
		(0.041)	(0.003)	(16.718)
% Age > 89	0.019	-0.012	0.000	28.804
		(0.003)	(0.001)	(36.695)
% Female	0.603	-0.082	0.013	-4.591
		(0.044)	(0.009)	(5.318)
% Public insurance	0.973	-0.047	0.001	-9.009
		(0.009)	(0.004)	(17.326)
Number of Inpatients	432	3183	10	-0.000
		(1246)	(40)	(0.001)
Number of Deaths	6.4	65.5	-1.0	0.003
		(30.5)	(1.6)	(0.009)
In-hospital Death Rate	1.498	0.183	0.002	0.207
		(0.610)	(0.085)	(0.298)
% Votes for Right-wing parties	25.787	6.406	6.005	0.007
		(8.151)	(6.831)	(0.010)
% Votes for Center parties	19.070	11.838	7.707	-0.006
		(7.135)	(6.274)	(0.008)
% Votes for Left-wing parties	24.453	-14.293	-6.598	0.008
		(5.538)	(4.857)	(0.009)

Notes: This table studies differences between ever- and never-adopter hospitals in terms of predetermined characteristics. We consider a window of one year before adoption for ever-adopters and one year before the first adoption in the country for never-adopters. 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. Column (4) uses the cross-section of hospitals that adopted the new selection system and reports coefficients and standard errors from an ordered logit using the order in which hospital adopted as the dependent variable—the first hospital that adopted has a value of 1, the second hospital that adopted has a value of 2, and so on—and all characteristics in first differences as explanatory variables. The political variables correspond to the vote share of right-, center, and left-wing parties in the most recent (pre-reform) municipalities where hospitals are located. The first differences of these variables correspond to the difference in vote shares between both elections. Standard errors are clustered at hospital level.

Table 3: Impact of the reform on death rates

	Ln Death Rate				# Deaths
	In-hosp	28-days	Non Def	Em. Room	In-hosp
	(1)	(2)	(3)	(4)	(5)
1 if reform adopted	-0.074	-0.055	-0.077	-0.085	-0.048
	(0.021)	(0.020)	(0.023)	(0.028)	(0.019)
Observations	10,010	10,010	9,545	8,351	10,010
Time FE	Yes	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes	Yes
# of Hospitals	178	178	176	156	178
Mean Dep. Variable	2.87	4.47	3.97	3.27	40.02

Notes: This table presents the impact of the selection reform on public hospital performance, as measured by mortality outcomes. Estimates are from the staggered difference-in-differences specification in Equation 1. The empirical analysis uses quarterly panel data for public hospitals and a time window comprising 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. In column (1), we focus on in-hospital death rates while column (2) replaces the dependent variable with the 28-day death rate, which considers in- and out-of-hospital deaths. In columns (3) and (4), we study the impact of the reform on nondeferrable and emergency admissions. Finally, column (5) reports estimates from a Poisson regression of death counts. Results in columns (1)-(4) are weighted by the pre-policy number of inpatients. For columns (1)-(4), the mean dependent variable is presented in levels instead of logs. All specifications include case-mix controls. Standard errors are displayed in parentheses and are clustered at hospital level.

Table 4: Impact on risk-adjusted mortality measures

	Ln Actual/Predicted Death rate			Ln Predicted Death rate
	(1)	(2)	(3)	(4)
1 if reform adopted	-0.080	-0.081	-0.080	0.017
	(0.021)	(0.021)	(0.021)	(0.015)
Observations	10,010	10,010	10,010	10,010
Time FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
# of Hospitals	178	178	178	178
Mean Dep. Variable	0.88	0.89	0.91	0.66
Logit Model:				
Patient Demographics	Yes	Yes	Yes	Yes
Type of Insurance	No	Yes	Yes	Yes
Enhanced Elixhauser Comorbidity Index	No	No	Yes	Yes
Pseudo-R^2 Logit	0.149	0.151	0.168	0.168

Notes: This table presents the impact of the selection reform on risk-adjusted death rates and on predicted death rates. For this exercise, we use patient-level data to fit a logit model of (pre-reform) mortality on patients' demographics and diagnoses. 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. Estimates are from the staggered difference-in-differences specification in Equation 1. The risk-adjusted death rate is defined as the actual hospital-level death rate divided by the hospital-level predicted death rate. All specifications include case-mix controls and are weighted by the pre-policy number of inpatients. Standard errors are displayed in parentheses and are clustered at hospital level.

Table 5: Effect of the reform on managers' skills and demographics

	Has Mgmt. Studies	CEO Fixed Effect	Age	Female
	(1)	(2)	(3)	(4)
1 if reform adopted	0.40	-0.18	-1.88	0.00
	(0.05)	(0.06)	(1.12)	(0.05)
Observations	9,851	6,032	9,851	9,851
Time FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
# of Hospitals	177	107	177	177
Mean Dep. Variable	0.19	0.06	50.27	0.19

Notes: This table presents 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. In column (1), we consider an indicator of whether the CEO has managerial training. Column (2) focuses on our CEO fixed-effects estimates as a measure of managerial ability. In this case, and since CEO fixed effects can only be compared within connected sets, we augment the specification by including connected set indicators and weighted by the pre-policy number of inpatients. Columns (3) and (4) 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.

Table 6: Heterogeneity in CEO performance by managerial educational background

	Ln Death (%)			
	(1)	(2)	(3)	(4)
1 if reform adopted	-0.064			
	(0.021)			
Reform & CEO w/ mgmt. training		-0.086	-0.086	
		(0.022)	(0.028)	
Reform & CEO w/o mgmt. training		-0.027	-0.031	-0.027
		(0.028)	(0.028)	(0.028)
Reform & Non-doctor CEO w/ mgmt. training				-0.071
				(0.024)
Reform & Doctor CEO w/ mgmt. training				-0.095
				(0.029)
Observations	9,950	9,950	6,682	9,950
Sample	All	All	Doctor	All
	CEOs	CEOs	CEOs	CEOs
Time FE	Yes	Yes	Yes	Yes
Hospital FE	Yes	Yes	Yes	Yes
# of Hospitals	177	177	174	177
Mean Dep. Variable	2.875	2.875	2.783	2.875
p-value *Mgmt. = No mgmt.*	-	-	0.028	0.098
p-value *Non-doctor w/ mgmt. = Doctor w/ mgmt.*	-	-	-	0.466

Notes: This table examines heterogeneous effects of the reform by CEO managerial educational background. We follow the staggered difference-in-differences design in Equation 1 to examine to what extent the reform has differential effects depending on the CEO's educational background. Column (1) replicates our main analysis using the sample for which we have data on CEOs' educational background. Column (2) focuses on whether the CEO has *any* management training, which include undergraduate and postgraduate studies related to management. Column (3) considers doctor CEOs with and without management training. Column (4) focuses on whether the CEO with *any* management training is a doctor. The dependent variable is the death rate at hospital level in a given quarter. All specifications include case-mix controls and are weighted by the pre-policy number of inpatients. Standard errors, in parentheses, are clustered at hospital level.

ONLINE APPENDIX

Managers and Public Hospital Performance

Pablo Muñoz and Cristóbal Otero

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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 mainly on the location of the primary care center and patient's diagnosis and demographics. 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 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 cross marker), and CESFAM Pablo de Rokha 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 hospitals to which patients are referred. The example shows how referrals vary depending on the primary care center and the patient's diagnosis and demographics. 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 2005 and who were not admitted into the hospital via ER. In this sample, we classify patients into cells defined by the patient's 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 histogram with the share of patients in each cell who are discharged exclusively from one hospital; around 75% 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 noncompliance with 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 the patient's home address, but they could have used their work address to register with the health system. Finally, there might also be measurement error in the address and age of patients.

B Hawthorne Effects

A potential explanation for the effects of the reform could be attributed to Hawthorne effects, which might occur as a result of the appointment of a new CEO under the reform. A direct impact on death rates could arise from the CEO appointment itself. Exploring this mechanism requires slightly modifying our empirical strategy, since all hospitals have several CEO turnovers during 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., 2024).

We define an event as a CEO turnover in a never-treated or yet-to-be-treated hospital in any quarter between 2001 and 2019. For each turnover event, we define a time window around it and a control group of hospitals with no turnovers in the time window.[42] Next, we define a set of valid events as those that are balanced in the time window and do not overlap with another turnover in the pre-period within the time window. Finally, we append the data for all valid events and estimate the following equation

$$y_{hte} = \alpha_{he} + \gamma_{te} + \sum_\tau \beta_\tau D^\tau_{hte} + \epsilon_{hte}, \tag{A.1}$$

where e is a valid turnover event. Equation A.1 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.

Appendix Figure A.13 presents the effect of a CEO turnover on death rates. The effect is a precisely estimated zero and confirms that a CEO turnover 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 appointment itself.

C 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:

$$\text{Ln(death rate)}_{ht} = \alpha_h + \psi_{M(h,t)} + \gamma_t + X'_{ht}\Delta + u_{ht}, \tag{B.2}$$

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 (age and sex) and socioeconomic status (proxied by type of insurance).

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[42]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 turnover and 8 quarters after the turnover, although the results are robust to other time windows.

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 673 CEOs, 112 hospitals, and 22 connected sets created by 78 movers. Then, we estimate the model via constrained OLS and recover CEO fixed effects that can be compared *within* connected sets.

It is worth noting that models with additive hospital and CEO components may raise some concerns. One might 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. All in all, the evidence suggests that the two-way fixed effects model fits the data well, and match effects, if any, are small.

C.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 rotation of CEOs in an event study framework. Specifically, we calculate the difference between the incumbent and the incoming CEO fixed effect (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.14 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 change, 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) do not exhibit different trends and that turnovers do not seem to correlate with pre-trends of hospital performance. We believe that this evidence 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 (5) of Table 1, we report a saturated version of Equation B.2, in which we include hospital-by-manager fixed effects. If the match component is sizable, this model should have a better fit than that in column (4). We find that model fit—measured by the

R^2 does not change. 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 hospitals and CEOs. 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.14. We find that all residuals are small and lower than 0.02 in absolute value. A final piece of evidence comes from the symmetry of the effects depicted in Panel A. 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.

C.2 Variance Decomposition

In this subsection, we perform the variance decomposition. Following Equation B.2, and abstracting from patients' characteristics, the variance of log death rates can be decomposed as

$$\mathbb{V}(\text{Ln(death rate)}_{ht}) = \mathbb{V}(\alpha_h) + \mathbb{V}(\psi_{M(h,t)}) + \mathbb{V}(\gamma_t) + 2\mathbb{C}(\alpha_h, \psi_{M(h,t)}) \quad \text{(B.3)}$$
$$+ 2\mathbb{C}(\alpha_h, \gamma_t) + 2\mathbb{C}(\psi_{M(h,t)}, \gamma_t) + \mathbb{V}(u_{ht}),$$

Table A.4 presents the magnitude of each term in Equation B.3, estimated within the largest connected set.[43] 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 most of the variance in log death rates is explained by hospitals (44%). Manager fixed effects explain around 21% of the variance in death rates, which is about 48% of the permanent component associated with different hospitals. Our results also show that the (bias-corrected) covariance between CEO and hospital effects is positive (6%), which implies that the most talented CEOs are slightly more likely to work at better hospitals (i.e., there is positive assortative matching).

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[43]The largest connected set in our setting contains 2,676 observations: 280 CEOs, 46 hospitals, and 39 movers.

**Figure A.1:** Share of medical beds provided by public hospitals in OECD economies
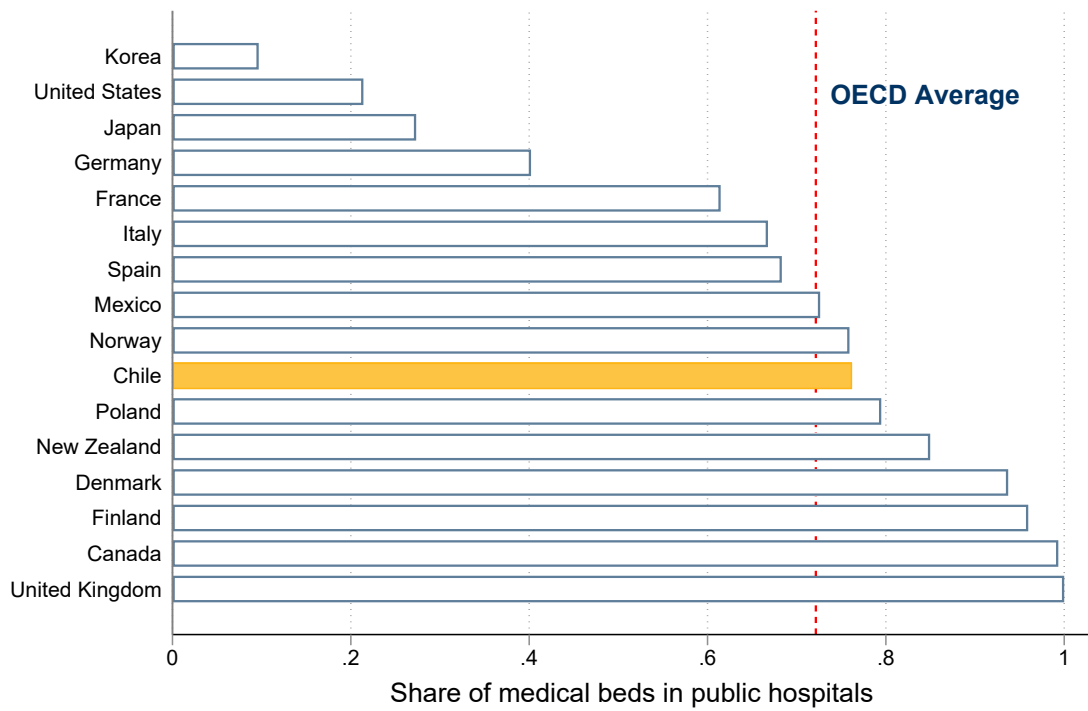
Notes: This figure displays the share of medical beds provided by public hospitals in a set of selected OECD countries in 2019. The dashed red line represents the average share in all OECD countries. The share is computed as the ratio between the total number of hospital beds in publicly owned hospitals and the total number of hospital beds in the country. Both variables are reported in OECD (2022).

Figure A.2: Selection process after the recruitment reform
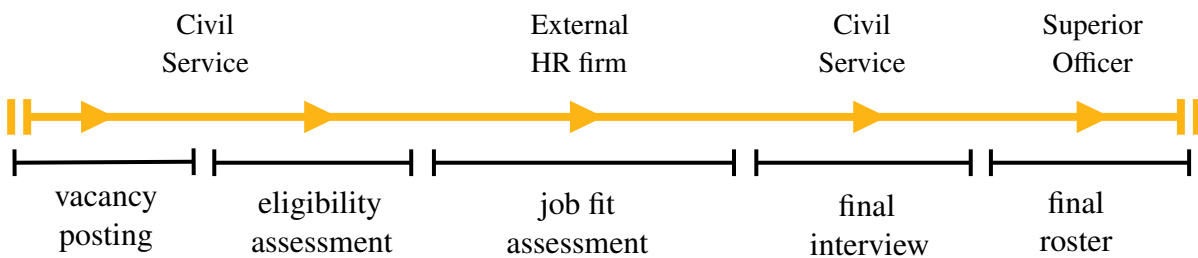
Notes: This figure illustrates the selection process for senior executive positions when the selection reform has been adopted. The job call starts with the position posted online on the Civil Service's website and in a newspaper with national circulation. In some cases, the Civil Service may also hire 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.3: Yearly recruitment processes overseen by the Civil Service
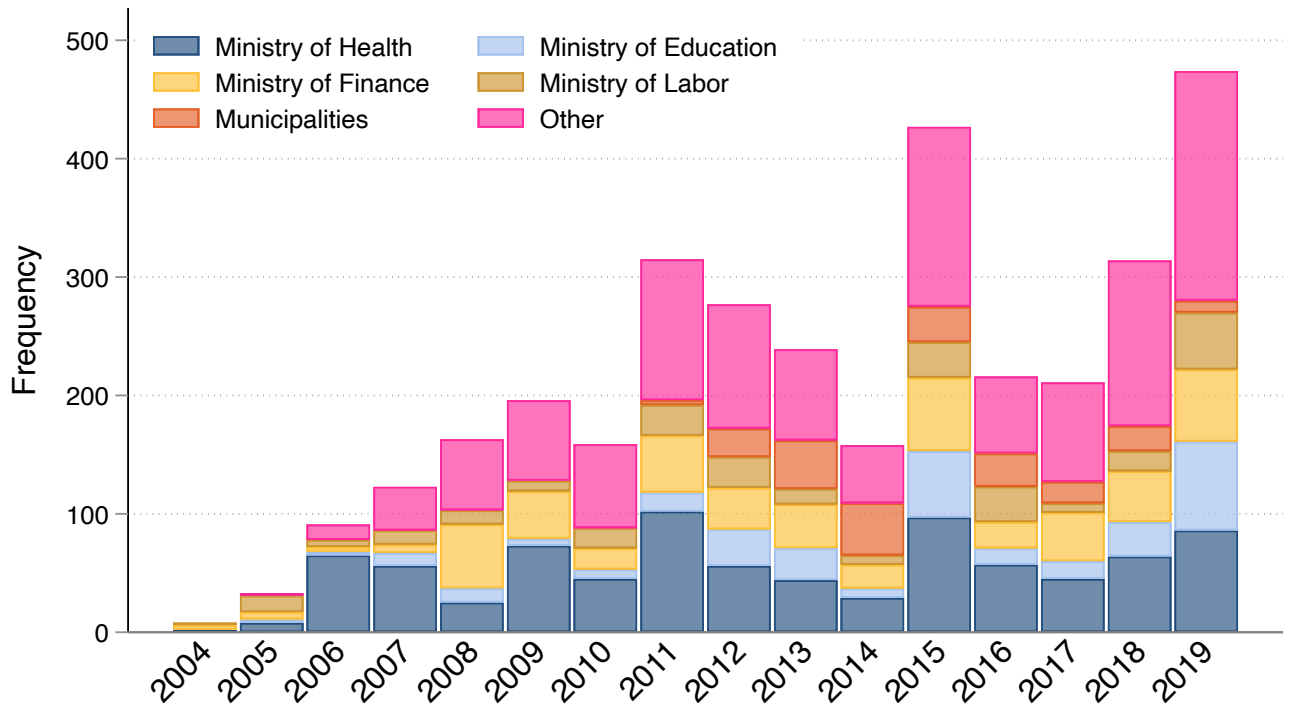
Notes: This figure displays the number of selection processes the Civil Service oversees every year. We use the ending date of the process to allocate the process to a given year. Yearly observations include positions using the selection system for the first time and positions that had already adopted 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 A.4: Alternative event study models and estimation methods
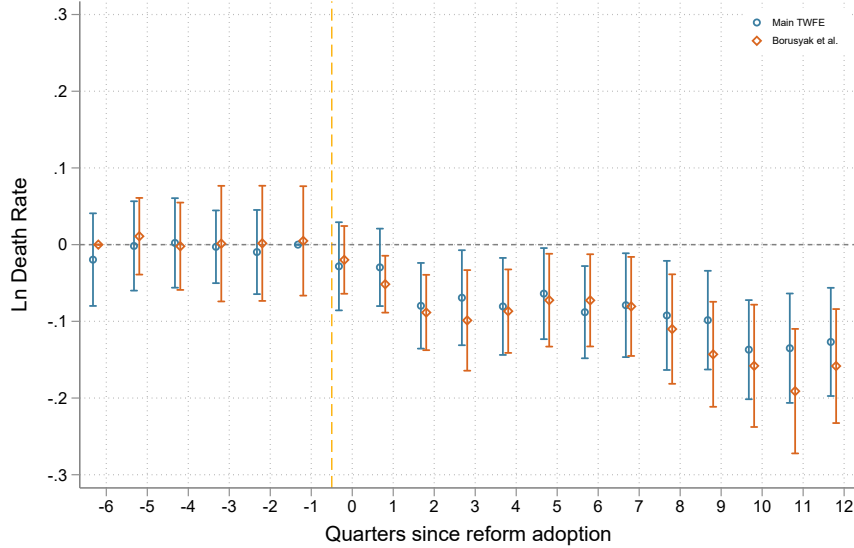
(A) Robustness to treatment effect heterogeneity
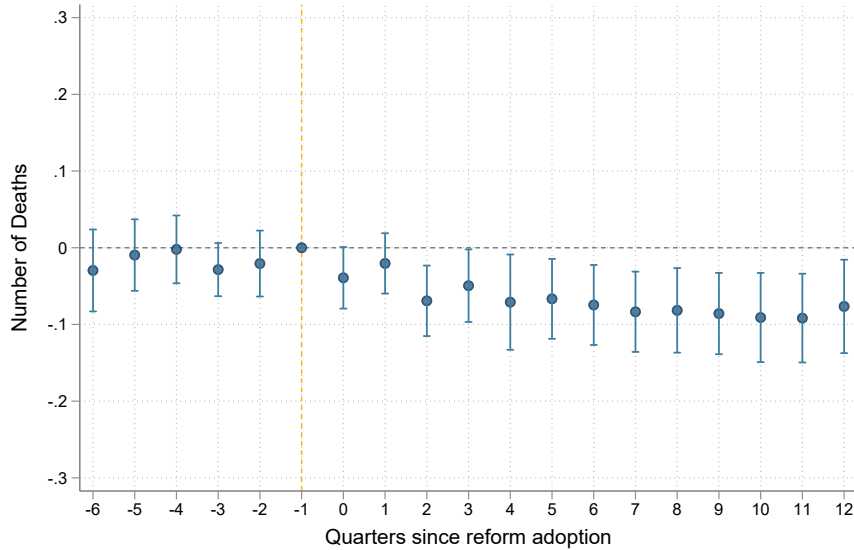
(B) Poisson Model

Notes: This figure plots the estimates and confidence intervals obtained using different event study models and estimation methods. Panel A presents results obtained using the model suggested by Borusyak et al. (2024) (in orange with diamond markers), which are robust to treatment effect heterogeneity. For comparison, we overlay them to our main results from Figure 2 (in blue with circle markers and labeled as TWFE in the figure). Estimates are weighted by the pre-policy number of inpatients on both models. Panel B presents event study evidence of the reform's effect on hospital deaths, using the number of deaths as the dependent variable in a dynamic Poisson regression. Specifications in both panels include case-mix controls. Markers represent an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure A.5: Dynamic effects of the reform on risk-adjusted death rate and predicted death rate
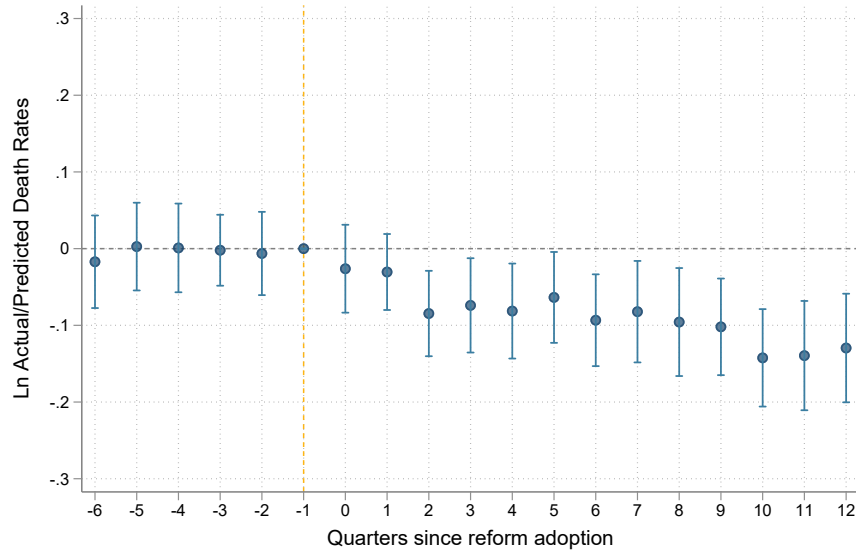

(A) Risk-adjusted mortality
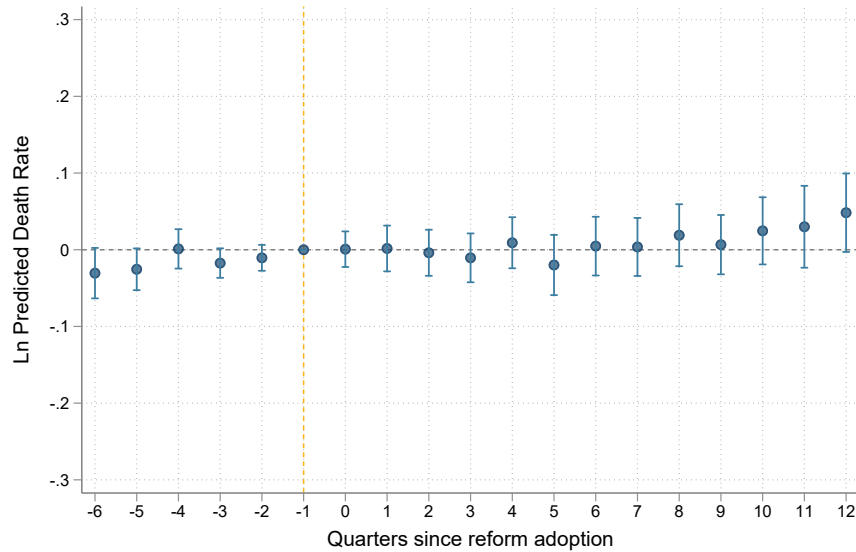

(B) Predicted death rate

Notes: This figure presents event study evidence, following Equation 2, on the impact of the selection reform on the risk-adjusted death rate and on the predicted death rate. Panel A reports estimates for the logged risk-adjusted death rate, and Panel B reports estimates for the logged predicted death rate. For this exercise, we use the patient-level data to fit a logit model of (pre-reform) mortality on patients' demographics and diagnoses. 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. The risk-adjusted death rate is defined as the actual hospital-level death rate divided by the hospital-level predicted death rate. Both specifications include case-mix controls and are weighted by the pre-policy number of inpatients. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

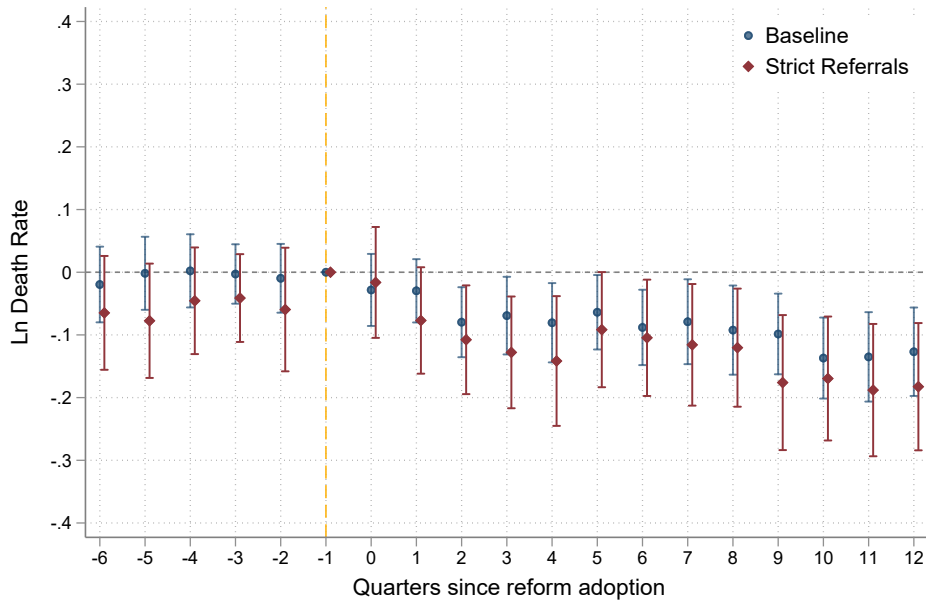**Figure A.6:** Testing for patient selection: Supply side
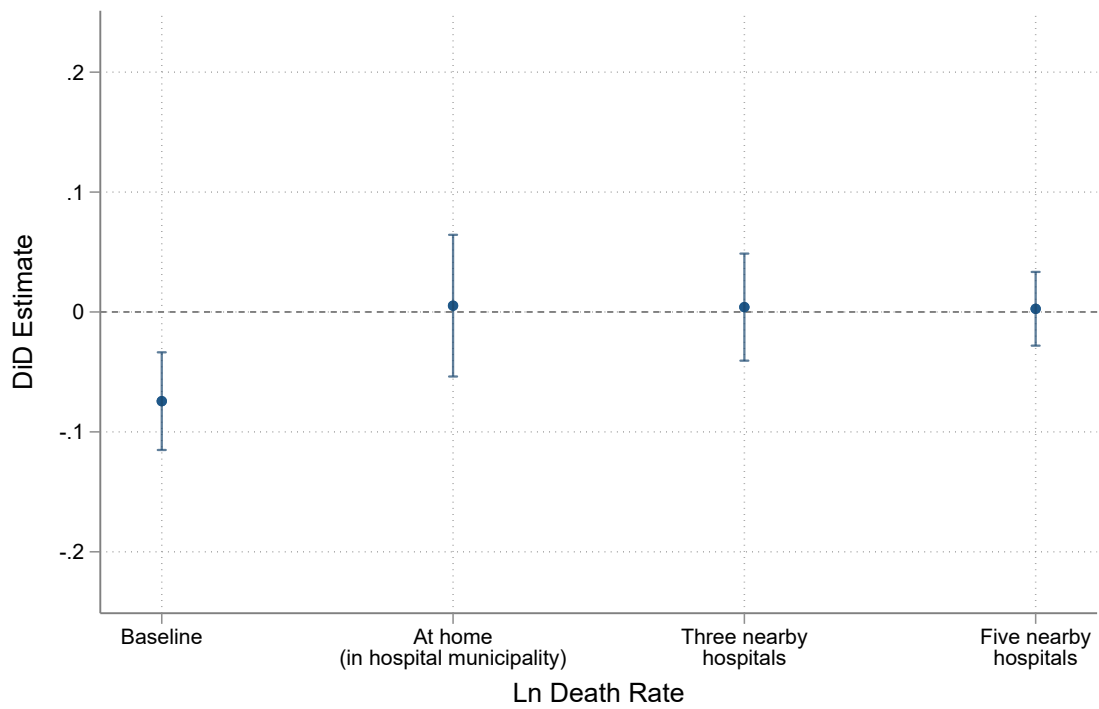
Notes: This figure presents evidence to assess patients' selection as a confounder of our main results. We plot the estimates and confidence intervals obtained by estimating Equation 1 for the logged at-home death rate and for logged death rates at nearby hospitals. All regressions include case-mix controls and standard errors are clustered at hospital level.

**Figure A.7:** Testing for patient selection: Demand side
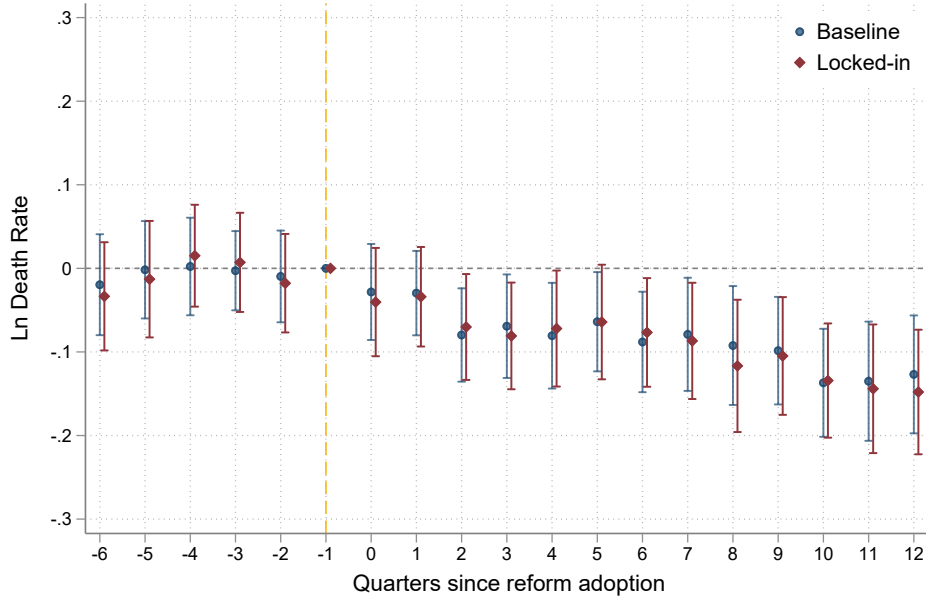
(A) Locked-in patients

(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 who followed the referrals mandated by the health system. These figures also include baseline estimates for comparison. All regressions consider standard errors clustered at hospital level.

Figure A.8: Effect of the reform on hospital personnel wages
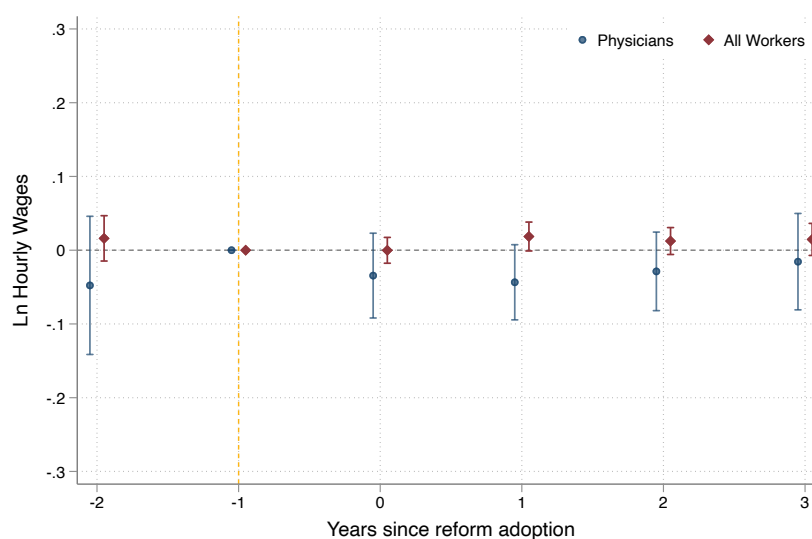

(A) Ln hourly wages

Notes: This figure presents event study evidence on the reform's effect on personnel wages, following Equation 2 estimated at year level. Wages correspond to the hospital's wage bill (in real terms) divided by the number of workers. Each circle marker (in blue) corresponds to an estimated coefficient when considering doctors' wages, and each diamond marker (in red) corresponds to an estimated coefficient when considering other health workers. Vertical lines indicate 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
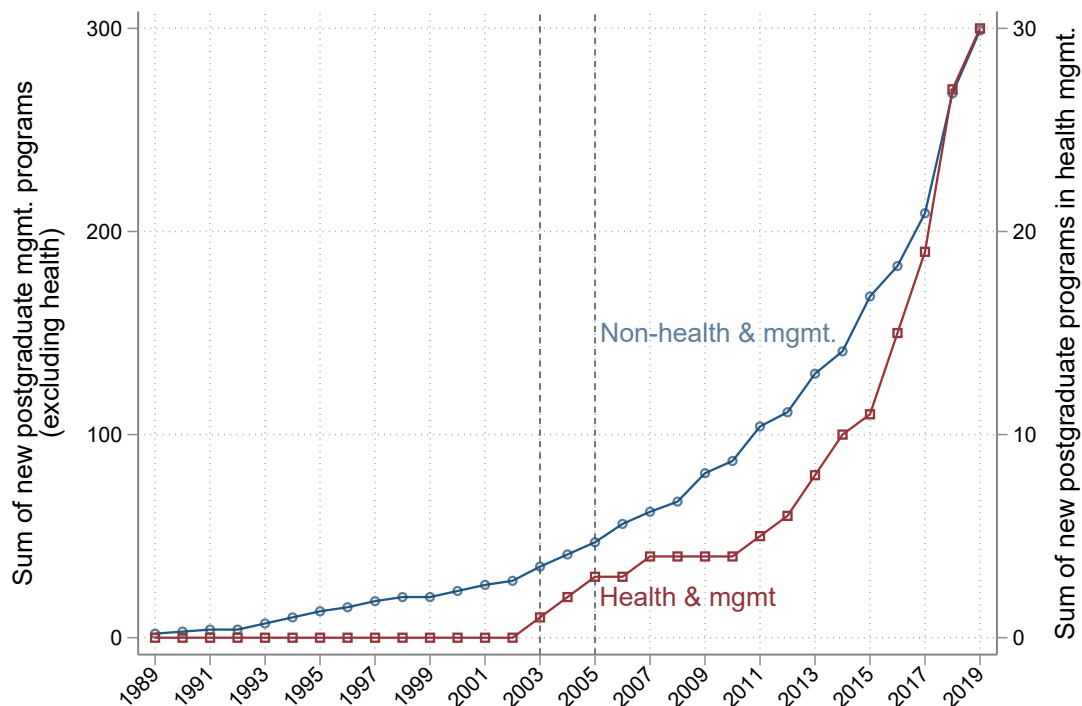
Notes: This figure shows the cumulative number of postgraduate management programs (diplomas and master's) by date of creation. Blue circles depict all management postgraduate degrees, excluding those related to health; corresponding frequencies are displayed on the left y-axis. Red squares depict new postgraduate degrees that include both management *and* health in their descriptions; corresponding frequencies are displayed on the right y-axis. Dashed gray lines indicate 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 that were actively running in 2019, as reported by the Consejo Nacional de Educación (https://www.cned.cl/bases-de-datos).

**Figure A.10:** Reform's bonus and CEO wages
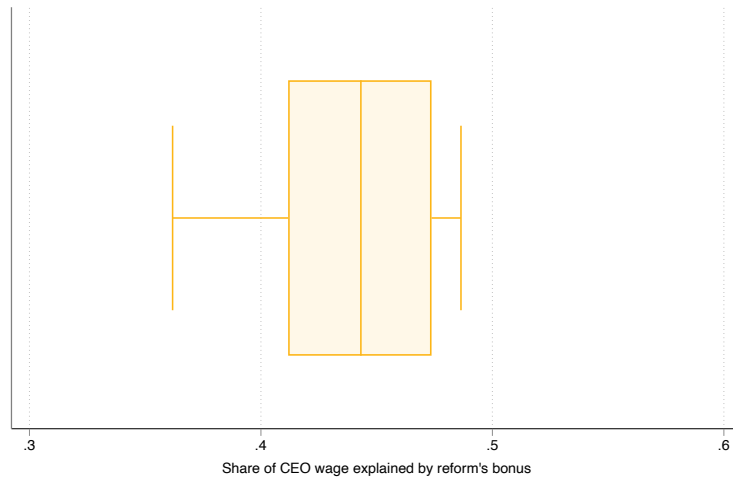
(A) Share of CEO wage explained by reform's bonus
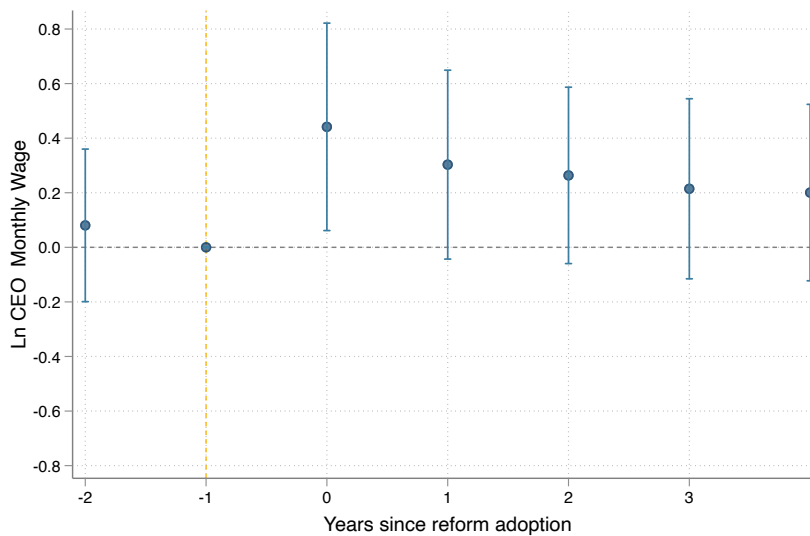
(B) Effect of the reform on CEOs' wages

Notes: Panel A 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 for the first time using the selection reform between 2011 and 2019. The average wage share explained by the reform's bonus is 44%. Panel B presents the impact of the reform on hospital CEO wages. The empirical design leverages the gradual adoption of the selection reform across hospitals on an event study design. Regression controls include a quadratic polynomial of 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 corresponding 95% confidence intervals. Standard errors are clustered at hospital level.

**Figure A.11:** Examples of referrals from primary care centers
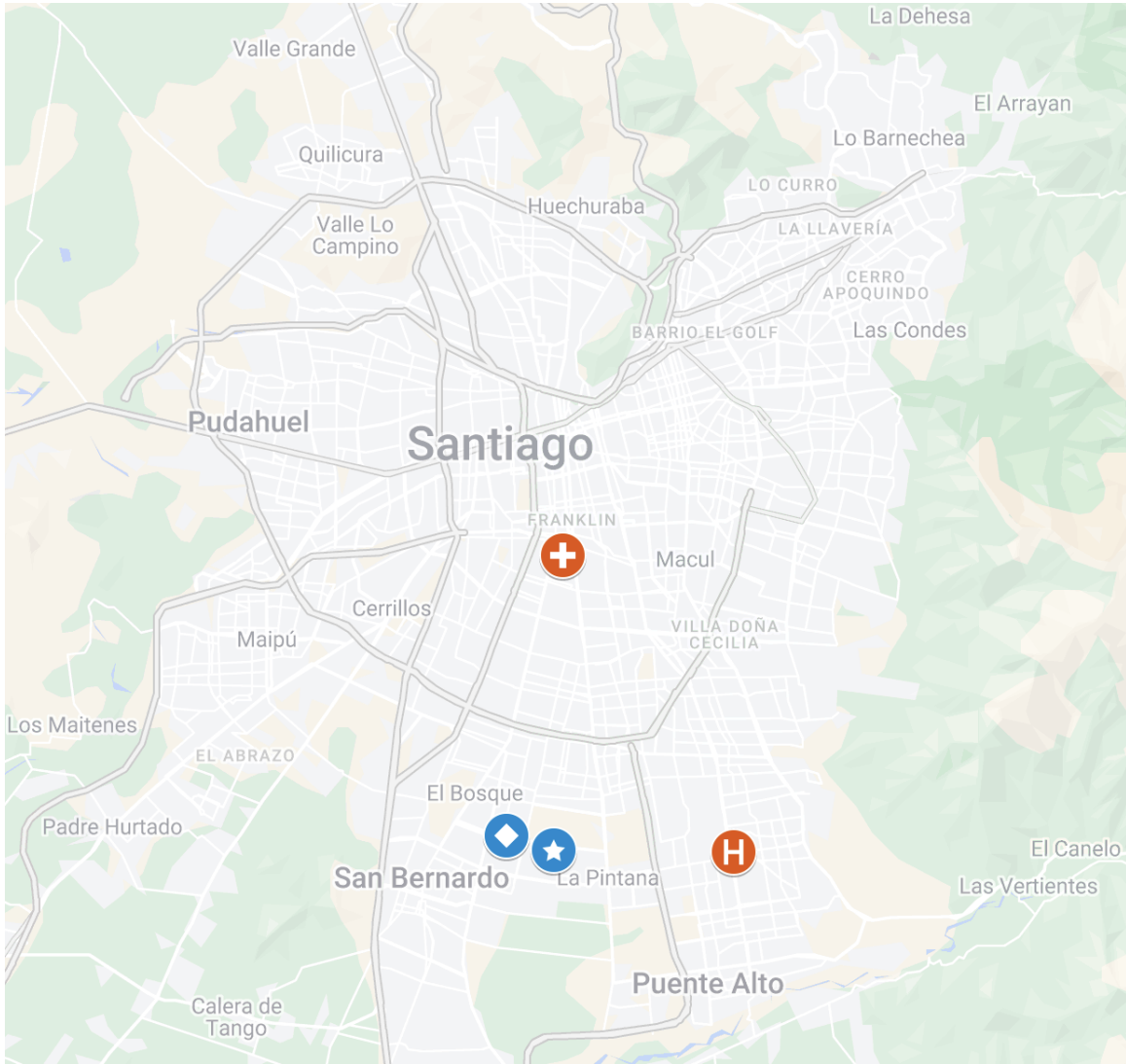
Notes: This figure illustrates an example of patient referral based on their primary care center. The figure depicts two primary care centers, CESFAM Dra. Haydee López Cassou (in blue with a white diamond marker) and CESFAM Pablo de Rokha (in blue with a white star marker), which are located in adjacent 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
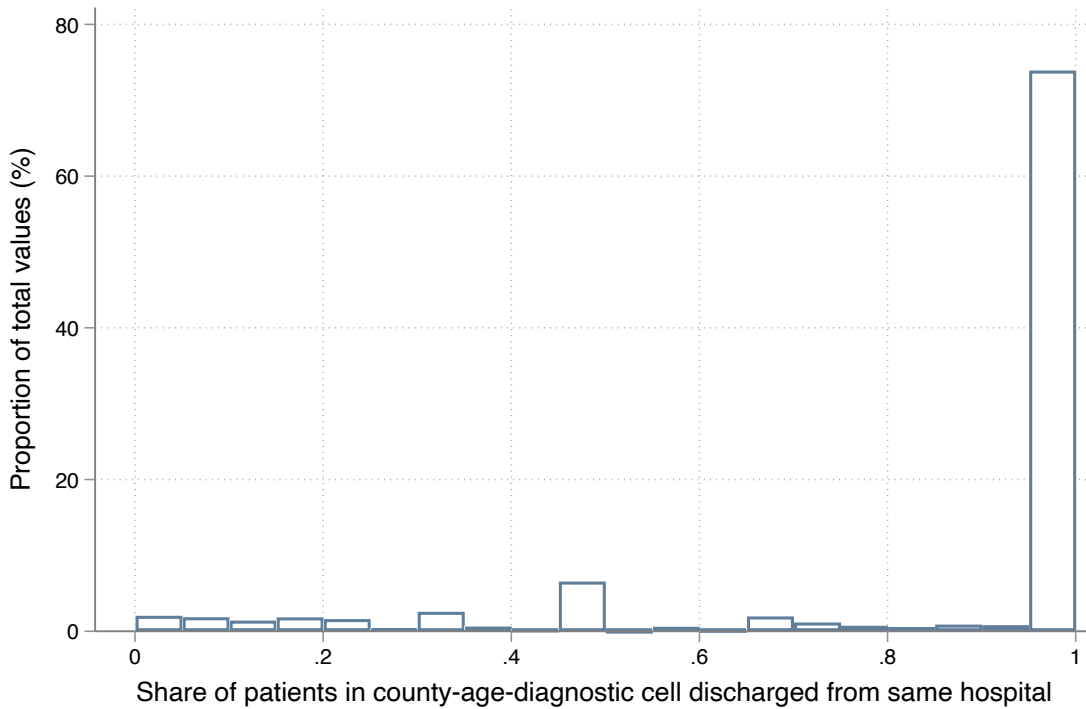
Notes: This figure plots a histogram with the share of patients in each cell who are discharged exclusively from one hospital. A cell is defined by the patient's county of residence, age group (less than 1 year, between 1 and 15 years, and more than 15 years), and diagnosis (as reported by the hospital 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, 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: Effect of CEO turnover on death rates
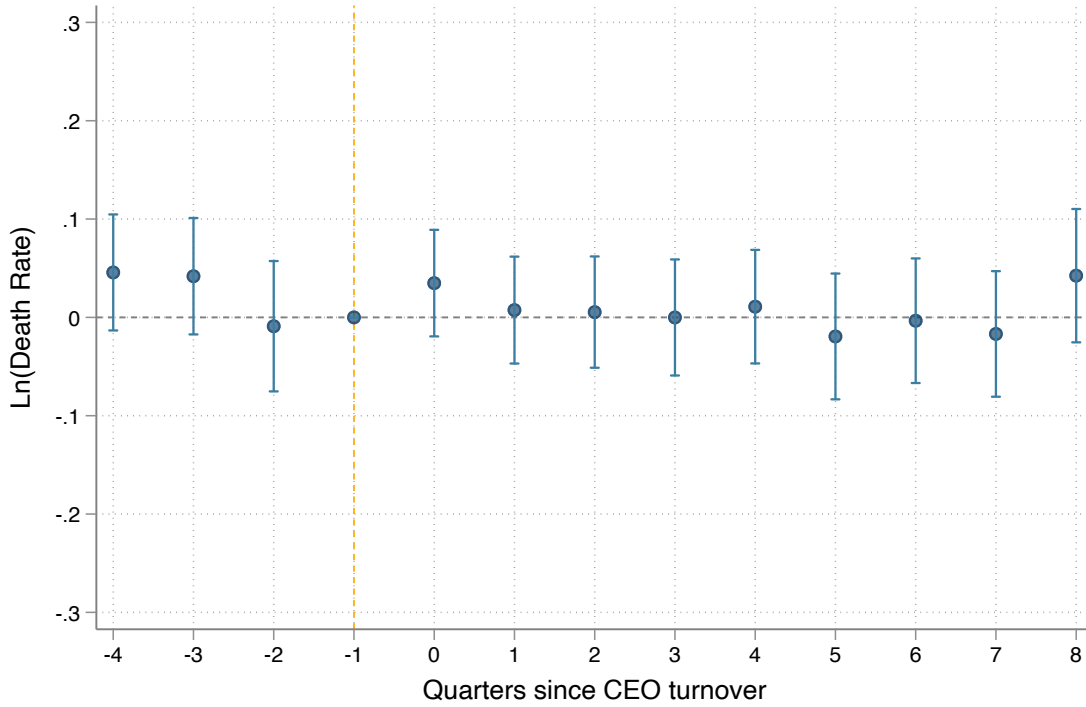
Notes: This figure presents the coefficients of the stacked event study specification in Equation A.1. An event is a CEO turnover in a hospital that never adopts the reform or in an adopter before the reform. For each turnover event, we define a time window around it and a control group of hospitals with no turnovers 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 turnover in the pre-period within the time window. 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 corresponding 95% confidence intervals. Standard errors are clustered at hospital level.

Figure A.14: Threats to the identification of managerial talent
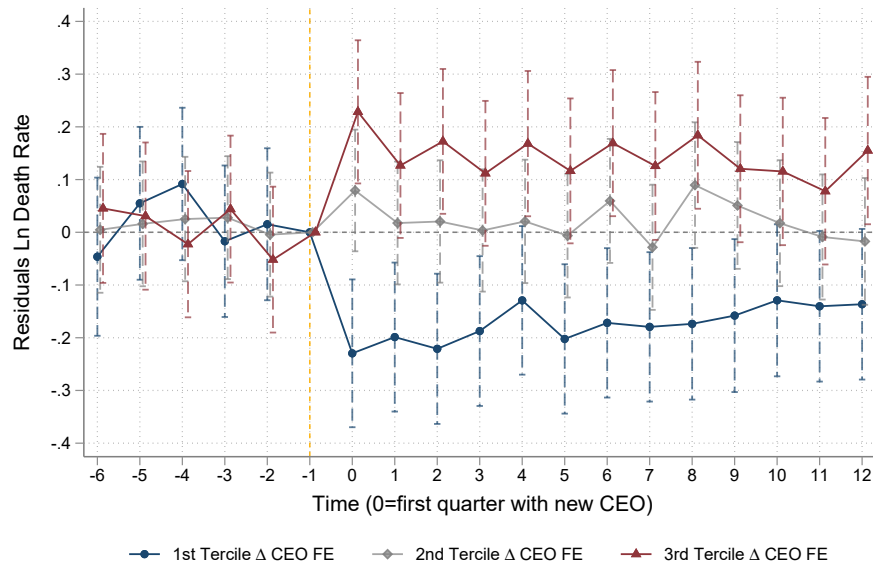
(A) Switchers
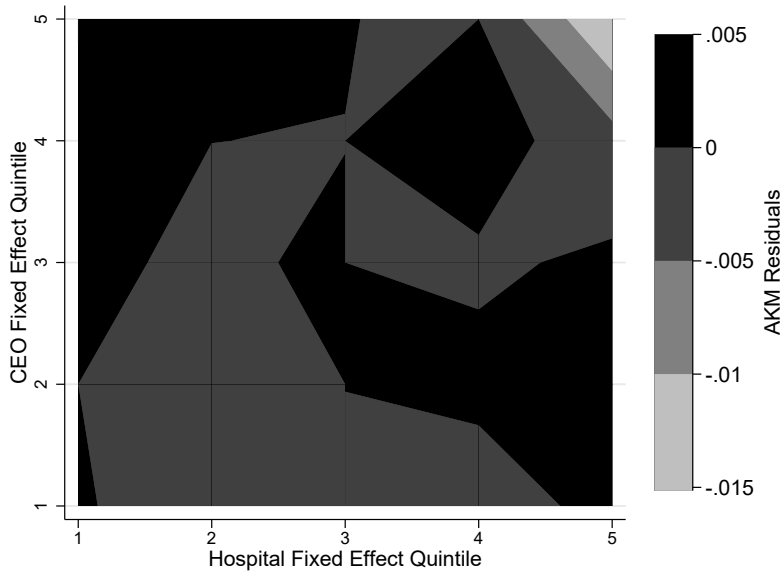
(B) Residuals

Notes: This figure presents evidence against the 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 corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Panel B shows mean residuals from model B.2 with cells defined by quintiles of estimated manager effect interacted with quintiles of estimated hospital effect.

Figure A.15: Distribution of performance scores for post-reform CEOs
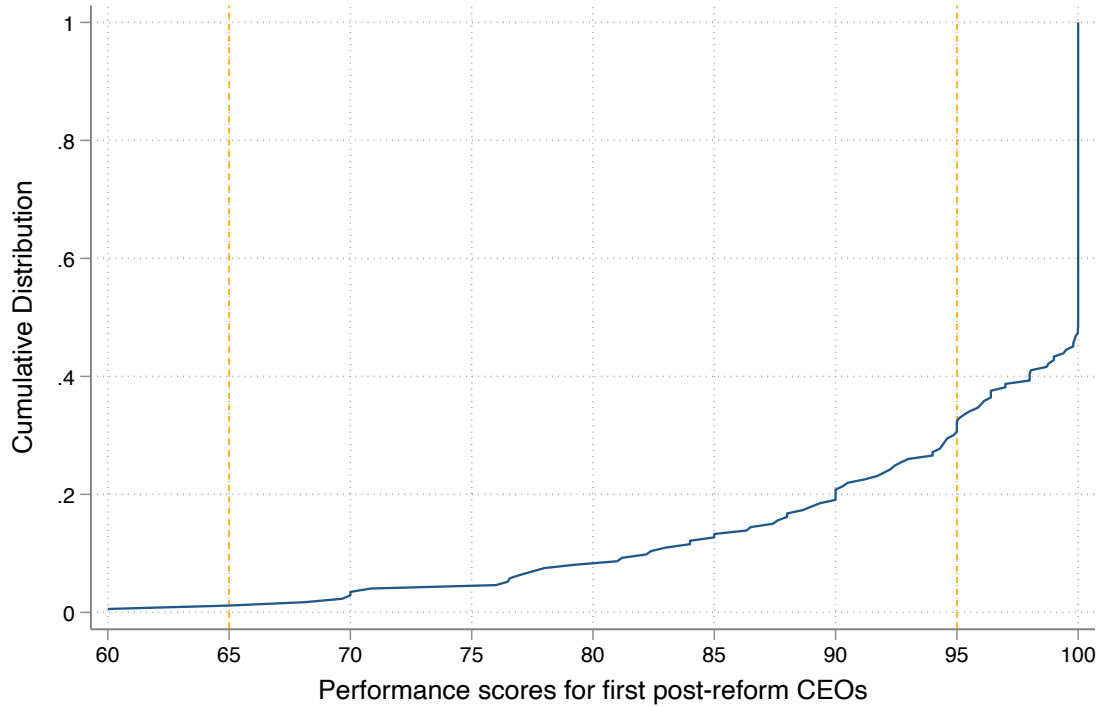
Notes: This figure displays the cumulative distribution of performance scores for the first post-reform CEOs. Before the reform, performance did not affect the wage schedule. After the reform, CEOs face wage penalties if they perform below specific performance thresholds. We accessed all available performance contracts and yearly performance scores. Unfortunately, some of the oldest contracts and performance scores are lost, and the Civil Service has no available records. We have performance scores for at least 1 year for 57 CEOs. An observation is a year-CEO. Dashed yellow lines represent the wage penalty thresholds described in Equation 3. Managers who scored below the first penalty threshold (score = 95) had a penalty equal to 1.5% of their annual wage. Below the second threshold (score = 65), the penalty is 7% of their annual wage.

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 diagnosis and 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: Descriptive statistics

	Mean	Std. Dev.	Median (p50)	# of Obs.
	(1)	(2)	(3)	(4)
Patient Characteristics:				
% Female	0.59	0.08	0.60	10,010
% Age < 29	0.34	0.15	0.36	10,010
% Age ∈ (30,29)	0.11	0.04	0.11	10,010
% Age ∈ (40,49)	0.09	0.03	0.10	10,010
% Age ∈ (50,59)	0.10	0.04	0.10	10,010
% Age ∈ (60,69)	0.11	0.04	0.11	10,010
% Age ∈ (70,79)	0.13	0.06	0.11	10,010
% Age ∈ (80,89)	0.09	0.06	0.08	10,010
% Age > 89	0.02	0.02	0.02	10,010
% Public Insurance	0.97	0.04	0.98	10,010
Hospital Characteristics:				
Total Number of Patients	1511.63	2014.64	586	10,010
Total Number of Beds	139.50	173.73	63	9,984
Physicians per 100 patients	6.14	7.62	4.71	5,997
Nurses per 100 patients	5.61	6.50	4.60	5,997
Hospital Outcomes:				
Number of Deaths	40.02	62.72	14	10,010
Death Rate	2.87	1.96	2.46	10,010
Death Rate 28-days	4.46	2.84	3.82	10,010
Actual over Predicted Death Rate	0.96	0.48	0.92	10,010

Notes: This table presents descriptive statistics for the universe of public hospitals included in our main analysis. Patient characteristics and hospital outcomes are from individual-level inpatient records collected by the Ministry of Health and encompass more than 16.5 million hospital events (DEIS, 2019). Hospital characteristics are 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 the Ministry of Health for HR purposes.

Table A.3: CEO selection reform v. other policies

Policy	Paper	Death-rate definition	Average death rate	Impact on death rate	Sample of patients
(1)	(2)	(3)	(4)	(5)	(6)
Spending					
↑ spending by 10%	Doyle et al. *JPE* '15	All, 1-year	37%	↓ 6%	ER + Amb. + ≥ 65*
	Ours		29%	↓ 9%	ER + ≥ 65
Competition					
+1 hospital in neighborhood	Bloom et al. *ReStud* '15	In-hospital, 28-day	15%	↓ 10%	ER + AMI
	Ours		23%	↓ 13%	ER + AMI
↓ 10% HHI	Gaynor et al. *AEJ EP* '13	In-hospital, 28-day	1.6%	↓ 1%	All patients
	Ours		2.3%	↓ 6%	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 3.4. Acronyms used in the table: ER: Emergency Room; AMI: Acute Myocardial Infarction; Amb: arriving by ambulance; *: non-deferrable medical condition; **: sample of ambulance rides with no prior ride within a year.

Table A.4: Hospital performance variance decomposition

	Component	Share of Total
	(1)	(2)
$\mathbb{V}$(Log Death Rate)	0.473	100%
$\mathbb{V}$(Hospital)	0.208	44%
$\mathbb{V}$(Manager)	0.100	21%
$\mathbb{V}$(Time)	0.043	9%
$2\mathbb{C}$(Manager, Hospital)	0.026	6%
$2\mathbb{C}$(Time, Manager + Hospital)	-0.004	-0.08%
$\mathbb{V}$(Residual)	0.099	21%

Notes: This table reports bias-corrected variances and covariances estimated on the largest connected set following Andrews et al. (2008). Hospital and manager fixed effects are estimated from Equation B.2 in the set of hospitals connected by managers' mobility (Abowd et al., 1999; Card et al., 2013). For details, see Appendix C.

Table A.5: Correlation between CEO fixed effects and manager characteristics

	CEO Fixed Effect			
	(1)	(2)	(3)	(4)
Female	-0.089	-0.092	-0.130	-0.120
	(0.028)	(0.028)	(0.031)	(0.031)
Age	0.110	0.106	0.109	0.110
	(0.008)	(0.009)	(0.008)	(0.008)
Age^2	-0.001	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Doctor		-0.036	-0.127	-0.028
		(0.024)	(0.036)	(0.046)
Mgmt. Studies			-0.278	-0.166
			(0.035)	(0.049)
Doctor $\times$ Mgmt. Studies				-0.160
				(0.062)
Observations	5,974	5,974	5,376	5,376
R-squared	0.268	0.269	0.280	0.281

Notes: This table presents the correlation between the CEO fixed effects estimated from Equation B.2 and manager characteristics. These characteristics include gender, age, age^2, and a set of indicators for educational background. "Mgmt. studies" refers to undergraduate studies in management or postgraduate studies related to management. All specifications include connected set indicators as control variables. Robust standard errors are in parentheses.