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Pablo Muñoz and Cristóbal Otero

Managers and Public Hospital Performance

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CINCH competence in competition + health series

Pablo Muñoz[‡] and Cristóbal Otero[§]

Managers and Public Hospital Performance[*]

Abstract

We study whether the quality of managers can affect public service provision in the context of public health. Using novel data from public hospitals in Chile, we show how the introduction of a competitive recruitment system and better pay for public hospital CEOs reduced hospital mortality by 8%. The effect is not explained by a change in patient composition. We find that the policy changed the pool of CEOs by displacing doctors with no management training in favor of CEOs who had studied management. Productivity improvements were driven by hospitals that recruited higher quality CEOs.

***JEL classification**: H11, H40, I18, M50.*

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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 important. One way to achieve this is through policies that improve the quality of public sector managers, who directly supervise the delivery of goods and services (Pollitt and Bouckaert, 2017). However, research on these policies is limited (Bertrand et al., 2020), because quasi-experimental variation in state personnel selection processes and objective and verifiable performance outcomes in the public sector are rare (Besley et al., 2022; Best et al., 2023).

This paper studies the impacts of a policy that aims to improve public sector managers' performance in a setting that allows us to overcome these challenges. We analyze a reform for senior executive positions in the public sector in Chile that introduced competitive recruitment and pay improvements, which are typically introduced together in civil service reforms designed to attract talent to the public sector.[1] 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 managerial performance. This is an important setting, because government expenditures are large and growing, outcomes for patients are high-stakes, and disadvantaged communities are particularly likely to interact with the public sector.[2]

The policy reform we study was enacted by the Chilean Congress in 2003 and resulted in the introduction of a new personnel selection system in the public sector. The reform replaced an opaque and discretionary selection process with a public, competitive, and transparent selection system for senior executive positions. In addition, 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.

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

[1] In Appendix Table A.1, we present a non-exhaustive list of major civil service reforms since the late 1970s across more than 20 countries spanning North America, South America, Europe, Asia, and Africa. In virtually all cases, these reforms involve coordinated changes in recruitment and financial incentives. In the two cases in which we observe a reform that included only competitive hiring, we observe that it was later followed by additional reforms that included financial incentives.

[2] 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).

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. We also observe detailed employment and wage records for healthcare workers across all public hospitals. The data thus provide a rich window into hospital inputs and performance, patient characteristics, and the characteristics and tenure of CEOs in every public hospital in Chile.

We start our empirical investigation into how managers affect productivity in public organizations by analyzing the impact of the reform on public hospital performance. For identification, we exploit the gradual adoption of the new selection system in a stacked event study design (Cengiz et al., 2019; Atal et al., 2024; Aneja and Xu, 2024). Concretely, for every treated hospital, we construct an event-specific control group that excludes already treated units, and thus avoids comparisons between early- and late-treated hospitals. This procedure allows us to deal with concerns regarding treatment effect heterogeneity that might bias the estimates in settings with staggered adoption. 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).

We find that the reform reduced in-hospital death rates by approximately 8% in the 3 years following the adoption of the new system, from a 2.5% death rate average pre-reform. We find very similar results from alternative empirical strategies that also allow for treatment effect heterogeneity when the treatment is binary and the design is staggered.[3] Our main result is robust to considering alternative ways of measuring our outcome variable, 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). We also estimate a stacked Poisson event study model using the count of deaths (Chen and Roth, 2023)—instead of the rate of deaths—and find consistent results.

To provide evidence for the validity of our research design, we show that the quarter before a hospital adopts the reform, the growth of an exhaustive set of variables—including hospital outcomes, patient characteristics, and political variables—does not differ between adopters and their control group. We also do not find evidence that treated hospitals exhibit different trends in mortality rates before 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. 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.

[3]We compute the results using the estimators proposed by Callaway and Sant'Anna (2021); Sun and Abraham (2021); and Borusyak et al. (2024), which properly account for dynamic effects in our setting. See De Chaisemartin and d'Haultfoeuille (2023) for a comprehensive survey.

A potential concern is that our estimates reflect changes in patient composition rather than improvements in hospital performance. Following the reform, managers potentially could have admitted healthier patients, or patients might have self-selected into hospitals experiencing performance improvements. Although this is unlikely in our setting—given that the institutional design of the Chilean public health system leaves minimal scope for patient selection—we present several pieces of evidence to address this concern.[4] First, we examine the effect of the reform on mortality 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 no evidence of spillover effects on mortality in neighboring hospitals or in aggregate home deaths at municipality level. 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 fit a flexible mortality prediction model using patient data prior to the reform, and using this model we do not find any evidence of impacts of reform adoption on hospital deaths that could be predicted based on patients' demographics and diagnoses. Finally, we 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 observed to strictly follow the referrals mandated by the public health network.

Rather than patient selection, we find evidence that the reform primarily reduced doctor turnover, which has been documented to correlate with public hospital death rates (Moscelli et al., 2024). This result also speaks to findings by Bloom et al. (2015), who document a positive correlation between improved management practices, reduced staff turnover, and hospital performance, and is in line with recent research in personnel economics showing that better-managed firms tend to retain workers with higher human capital (Bender et al., 2018).

Based on these results and the nature of the reform, we next turn to understanding the role of managers in explaining our findings on hospital performance. We begin by documenting the importance of CEO identity for hospital quality and show that in our setting, CEO fixed effects increase the explained share of performance variation at a magnitude comparable to that found by Bertrand and Schoar (2003) for CEOs of publicly traded U.S. firms and by Fenizia (2022) for managers in Italy's administrative public sector. In order to compute a measure of CEO quality, we estimate individual CEO fixed effects, which we identify by leveraging their rotation across hospitals in a two-way fixed-effects model (Abowd et al., 1999). We adjust these estimates by their reliability using empirical Bayes shrinkage (Chandra et al., 2016; Walters, 2024). We validate the CEO fixed effects, by exploiting CEO arrivals and departures as quasi-experiments and document

[4]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)

that residualized death rates change sharply, as predicted by the fixed effects, when high- or low-talent CEOs enter or exit a hospital, which suggests that our measures of CEO talent are "forecast unbiased" (Chetty et al., 2014). Also, we document that CEO turnovers are not influenced by preexisting trends in hospital performance and that match effects, if any, are negligible, which ameliorates concerns regarding exogenous mobility and model misspecification (Card et al., 2013).

Equipped with these estimates, we assess how the reform changed the characteristics of the appointed managers. We show that the reform attracted more talented managers, as measured by their CEO fixed effects. Recruitment under the new selection system led to a 0.25 standard deviation decrease in the fixed effect of appointed CEOs—an impact that maps to a 5.8% decrease in death rates.[5] Consistent with anecdotal evidence, we also find that the reform substantially increased the share of hospital CEOs with managerial training—a shift from the previous norm, whereby nearly all hospital CEOs were doctors.[6] In 2004, the year before the first hospital adopted the reform, 99% of hospital CEOs were doctors ("doctor CEOs"). We document that the reform played a substantive role in shifting this norm because it increased the share of CEOs with management training by more than 20 percentage points. This shift largely resulted from replacing doctor CEOs with new CEOs with undergraduate degrees in management-related majors, such as public administration, business and economics, accounting, and engineering. We also find that the reform only displaced doctor CEOs without management training, while it increased the share of doctor CEOs with managerial qualifications by nearly 15 percentage points. Taken together, these shifts account for more than a 35 percentage point change in the composition of CEOs with management training. 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.

We next examine the role of changes in manager characteristics in explaining the reform's impact on performance. Consistent with our findings that CEO identity matters for performance, we show that the cross-sectional variation in the effectiveness of the reform is largely driven by CEOs in the top half of the managerial talent distribution. We then examine whether CEO managerial training also predicts the effects of the reform on hospital performance. This is important because, as opposed to the estimated CEO effects, it is an easily observable and policy-actionable characteristic. We find that the effects of the reform are mostly explained by post-reform CEOs with managerial qualifications.[7] The effects are not statistically different from zero for post-reform man-

[5]Since the standard deviation of our empirical-Bayes adjusted CEO fixed effects is 0.23, the impact on the CEO fixed effects is -0.058 (i.e., -0.25×0.23), which implies a 5.8% decrease in death rates.

[6]This pattern, in which top executives rise 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 our setting, we posit that this pattern was upheld by a strong social norm that reserved these positions exclusively for doctors. We discuss this further in Section 4.

[7]Naturally, this correlation does not imply that management training improves the performance of hospital CEOs, since we cannot rule out that the effect is explained by differential selection (i.e., better managers are more likely to study management).

agers without managerial training, and the average effect is largely driven by post-reform CEOs with management training. Also, we do not find differential effects between doctor CEOs with management training and other CEOs with management studies.

In the last part of the paper, we examine the reform's financial incentives—namely, performance pay and higher base wages for newly appointed CEOs. We document that the policy was an effective tool for increasing wages, with post-reform CEOs earning approximately one-third more than pre-reform CEOs. We also document that performance pay incentives were not binding and were poorly designed—a feature that was common across appointments in all government agencies using the reform's new selection system, and not specific to public hospitals (CPPUC, 2013; Barros et al., 2018).

Given the large pay increase, financial incentives likely helped to attract a different—and eventually more talented—pool of CEO candidates. Since we do not observe the pre-reform pool of candidates, we cannot isolate the effect of this aspect of the reform from the competitive selection process alone in shaping the characteristics of the selected applicant. We thus interpret the impacts of the reform in shaping CEO quality as a combination of both financial incentives and competitive recruitment. One concern, however, could be that if financial incentives motivate managers to work harder, the effects of the reform could simply reflect CEOs' higher effort and not necessarily better selection. To address this concern, we compare a limited number of cases in which hospitals reappointed the incumbent manager through the selection reform. We find no impact on performance in these cases. As a second test, we estimate period-specific CEO fixed effects for individuals who served as CEOs before and after the reform. We find no evidence that these estimates changed for CEOs reappointed following the reform. Finally, we leverage a 2016 amendment to the reform that increased pay only for doctors appointed as CEOs. While the amendment raised wages for treated managers, we find no discernible effect on their performance. Taken together, these results suggest that intensive margin effects are unlikely to drive the results of the reform on CEO performance.

This paper contributes to a nascent literature that studies the impact of top managers on public sector organizations, and lags well behind similar research on politicians or private sector managers (Bertrand et al., 2020). Prior literature has focused on public schools (Lavy et al., 2023); public R&D labs (Choudhury et al., 2019); social security claims, (Fenizia, 2022); and public procurement (Best et al., 2023). Closest to our work, Janke et al. (2024) study the impact of CEOs in NHS hospitals in the UK and find little evidence of CEOs' impact on different dimensions of hospital performance.[8] These papers show that managers matter by exploiting rotation of managers across

[8]In contrast to our setting, CEO recruitment in NHS hospitals is relegated to local boards and does not have strict selection criteria. NHS reforms focused on increasing autonomy, accountability, and financial incentives, without placing additional emphasis on selection. Indeed, only one-quarter of CEOs have postgraduate management training in the NHS (Janke et al., 2018), which is similar to the average in our setting before selection reform adoption. The reform in Chile increased the share of CEOs with postgraduate management qualifications to almost 65% the quarter

units. We contribute to this literature by exploiting a reform that changed the quality of managers and improved organizational outcomes.

Our work also adds to recent research that has produced mixed findings on the impact of managers with management training on organizational performance. Acemoglu et al. (2023) examine private firm CEOs with business training and find no effects on firm performance, along with negative effects on wages. In contrast, Giorcelli (2024) studies management training for middle managers and supervisors in U.S. 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.[9] We also find no effects on employee wages, consistent with public sector wage rigidity. This paper also contributes to a related literature on the importance of management on death rates in hospitals (e.g., Bloom et al., 2015; Chandra et al., 2016; Bloom et al., 2020) and on clinical outcomes in physician practices (La Forgia, 2023). While this literature focuses on management practices, we focus on managers.[10] More generally, our work complements previous research on the efficacy of alternative policies for improving hospital performance (e.g., Propper and Van Reenen, 2010; Gaynor et al., 2013).

Finally, our study on a reform designed to attract talent to state personnel also relates to research on 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; Moreira and Pérez, 2022; Dahis et al., 2023; Aneja and Xu, 2024; Muñoz and Prem, 2024). We contribute to this literature by focusing on hospital managers and showing that a civil service reform led to the improvement of outcomes in tertiary healthcare. More broadly, our research adds novel evidence on bureaucratic effectiveness and its impact on development (see Besley et al., 2022, for a review).

The rest of the paper proceeds as follows. Section 2 describes the setting and data. 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 extent to which the identity and characteristics of managers matter for performance. Section 5 examines the effects of the financial incentives included in the reform, and Section 6 concludes.

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after adoption.

[9]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.

[10]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).

2 Setting, Data, and Descriptive Evidence

2.1 The Healthcare System in Chile

Chile's healthcare system comprises public and private health providers and public and private insurers. Public insurance is funded by general taxation and payroll taxes on enrolled employees. Individuals can opt out and use their health contributions to buy private insurance.[11] Individuals who are unable to pay can freely access the public system, which results in nearly universal health coverage.

Around 80% 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. This is because individuals with public coverage cannot choose their healthcare provider within the public network.[12] 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, the (i) administration of personnel, (ii) allocation of hospital inputs, (iii) management of financial resources and proposing the annual budget, (iv) decisions regarding infrastructure and technological equipment resources, and (v) integration

[11] 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).

[12] 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.

of the hospital into the health network and the community.

2.2 The Recruitment Reform

In 2003, a political scandal exposed illegal payments to top government officials (Waissbluth, 2006). In response to—and as a product of—broad political consensus, Congress enacted Law Nº 19,882, which created a new framework to regulate the public sector's personnel policy (Ley 20,955). Under this new framework, the law created the Senior Executive Service System "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."[13]

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. 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 increases 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.[14] In the case of public hospital CEOs, we document that the reform increased the position's pay around 25%. The financial package also includes a small performance pay component, under which the yearly wage is slightly reduced if the manager does not meet certain performance thresholds. In Section 5, we provide further details on the changes to the pay schedule and argue that this incen-

[13] 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 (accessed on November 25, 2024).

[14] 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.

tive was unlikely to be binding, since the performance agreements were easily manipulated and most managers readily met their targets.

Once a position in a given agency is subject to the new recruitment system, all future appointees in that position must be hired using the new process. In terms of adoption, all new top management positions created after the law was enacted are required to select their top manager using the new system. For existing agencies, adoption occurred gradually over time. Initially, the law mandated that before 2010, the Ministry of Finance would designate a minimum of 100 top executive positions across different government agencies to adopt the new recruitment system. In practice, the central government gave priority to some sectors and, within sectors, to some organizations.[15] In the case of public hospitals, the government gave priority to larger hospitals; we provide evidence of this intent in Section 3.1. The new recruitment mechanism comes into effect only after the agency initiates a new selection process, which is likely to occur after a new government takes office. In Appendix Figure A.3, we illustrate 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.

Panel A in Figure 1 illustrates the number of positions in public agencies that adopt the recruitment reform over time. In 2024, we observe the first 10 positions across different ministries adopting the reform, and by 2019 an additional 130 positions from various agencies across multiple ministries had adopted the new process. 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

[15]The Ministry of Finance had to approve adoption of the new selection system for each position within any agency that adopted it. 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 managing 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.

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 (SIRH, 2019). Data include detailed payroll information and wages. Among other characteristics, we observe the establishment, the person's occupation, number of hours worked, date of birth, and gender, as well as a detailed wage breakdown. Between 2011 and 2013, records were only collected for the month of December.

To measure 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 if the individual died in the hospital; and a set of individual characteristics: date of birth, gender, municipality of residence, and type of health insurance. For robustness checks, we link data at 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. We complement the data with a set of hospital characteristics such as size, location, and whether it is public, among others (DEIS, 2021).

Finally, to determine the timing of the policy, we use data on all hospital CEO recruitment processes conducted by the Civil Service (Servicio Civil, 2021). The information includes the hospital, the recruited individual's identity, and the date of appointment.

Hospital Performance: Our main outcome of hospital performance is 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 their place 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.[16] We provide further details in Appendix A.

[16]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

Throughout the paper, we consider in-hospital death rate as our main measure of hospital performance and 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. 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 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 in order to avoid observations with too few discharges and to reduce volatility in the data. 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 182 public hospitals—of which 88 adopted the recruitment reform at some point between 2005 and 2019—for a total of 10,310 hospital-by-quarter observations. Appendix Table A.2 presents descriptive statistics of this sample. The average hospital in our sample discharges 1,555 patients per quarter, while the median hospital discharges 611 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 41 deaths per quarter, with a corresponding in-hospital death rate of 2.88%. The 28-day death rate—which considers both in- and out-of-hospital deaths—is larger and corresponds to 4.47%.

3 The Reform's Impact on Hospital Performance

We begin our empirical investigation into the role of managers in public hospital performance by examining the recruitment reform's impact on hospital mortality rates.

hospital is based and is approved by the Ministry of Health.

3.1 Research Design

In this subsection, we explore 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 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 begin by comparing the characteristics of hospitals that adopted the selection reform with those of non-adopters at that point in time. For each treated hospital, we create a control group composed of units that do not adopt the reform 6 quarters before or 12 quarters after. We consider a set of variables related to inpatient characteristics, hospital outcomes, and political outcomes at municipality level where the hospital is located. To focus on the pre-adoption period, we consider the quarter prior to each hospital's reform adoption.

In Panel A in Figure 2, we present OLS estimates of a dummy variable that takes a value of 1 for adopters within the event window and 0 for other hospitals in the control group. The regression includes event-window fixed effects so that the contrast is between treated hospitals and their control group. We also standardize each variable for ease of comparison. We find that adopters have a larger number of inpatients, consistent policymakers' intent to give priority to larger hospitals. Adopters also have slightly higher in-hospital death rates, and serve patients who are younger and less likely to use public health insurance. Treated hospitals also serve patients with diagnoses that differ from those of their control group (see Panel A in Appendix Figure A.4). Finally, they are located in municipalities that exhibited slightly more support for right-wing politicians in the closest-in-time mayoral election. In sum, hospitals that adopted the selection reform differ from those that did not or had not done so around the time of treated hospitals' adoption.

However, the growth of these variables prior to the reform's adoption is not correlated with the timing of adoption. To evaluate whether reform adoption is associated with hospital characteristics that follow different trends (e.g., hospitals that perform better over time being more likely to adopt the new recruitment system), Panel B presents the OLS coefficients from regressions using the same dummy variable as before, but on the difference in each characteristic between the quarter before reform adoption and 1 year prior. The regression includes event-window fixed effects and standardizes each dependent variable. We do not observe that units that adopted the reform exhibit significantly different trends from other hospitals that had not adopted the reform in terms of the number of patients, their characteristics, hospital outcomes, or political determinants. Panel B in Appendix Figure A.4 also indicates no difference in the growth of the share of patients with different diagnoses. This suggests that the timing of adoption is uncorrelated with trends in these factors.

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. For each treated hospital, we define a time window around adoption of the policy and identify an event-specific control group that excludes already-treated units. Excluding these units helps address concerns regarding bias due to treatment effect heterogeneity, which is a natural threat when treatment is staggered (De Chaisemartin and d'Haultfoeuille, 2023). Concretely, we consider the following stacked event study model (Cengiz et al., 2019; Atal et al., 2024; Aneja and Xu, 2024):

$$y_{hte} = \alpha_{he} + \gamma_t + \sum_{\tau=-6}^{12} \beta_\tau D^\tau_{hte} + \epsilon_{hte}, \tag{1}$$

where e is an event, y_{hte} is an outcome variable at hospital h and quarter t level, α_{he} represent hospital-by-event fixed effects that control for the within-event unobservables specific to the hospital, and γ_t are time-by-region fixed effects to account for unobservable time shocks specific to each region. D^τ_{hte} is a dummy variable that indicates that in a given event e, the reform was adopted τ periods earlier (or will be adopted τ periods ahead for negative values of τ).[17] 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, which we normalize to zero. We focus on a window of 6 quarters before and 12 quarters after adoption. 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.

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; Doyle et al., 2015; Gaynor et al., 2013). Specifically, we include the share of female inpatients, the share of inpatients within each of eight 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 patient 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, and weight the regression by the number of inpatients in the hospital in 2005.

[17] 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.

[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.

3.2 The Hiring Reform Decreased Hospital Mortality

Figure 3 displays the point estimates of β_τ and their confidence intervals. We observe that after the reform adoption, mortality rates gradually decrease by around 8%. These effects are statistically significant and economically meaningful. An 8% effect implies a decrease in the death rate toward 2.26, over a sample mean of 2.46 deaths per 100 patients (i.e., two fewer deaths per 1,000 patients).

One of the main threats to interpreting these improvements in hospital quality as causal effects is that the timing of the reform's adoption might be correlated with changes in hospital performance. However, the dynamic effects show that pre-reform estimates tend to be small—around zero—and not significant, which indicates that treated and control units were not on different trends before reform adoption. In our setting, it does not seem that the change in management is driven by previous worsening in hospital performance, which would lead us to overestimate the true impact, if any, of the treatment.

For robustness checks, in Panel A in Appendix Figure A.5 we present results using alternative dynamic models suggested by recent literature on the staggered adoption of binary treatments, which are also robust to treatment effect heterogeneity (De Chaisemartin and d'Haultfoeuille, 2023). We uncover the same dynamic trajectory regardless of the estimation strategy. In Appendix B, we further examine whether CEO turnovers per se could partially explain our results—for instance, if CEO turnovers naturally shake things up or employees change their behavior due to new leadership. Leveraging CEO turnovers before the policy adoption, we find no evidence of this phenomenon.

To summarize the impact of the reform and analyze related mortality outcomes, we estimate the following stacked difference-in-differences model:

$$y_{hte} = \alpha_{he} + \gamma_t + \beta \times Reform_{hte} + \epsilon_{hte}, \tag{2}$$

where $Reform_{hte}$ is a dummy variable that takes the value of 1 if a hospital adopts the new selection process and 0 otherwise.[19] In Table 1, we report the results. Column (1) shows that reform adoption led to an average 8.2% decrease in in-hospital death rates over the 3 years following the reform. Likewise, column (2) shows that the logged 28-day death rate, which includes in- and out-of-hospital deaths, decreased by 6.1% after reform adoption, from a sample mean of 4.11 to 3.86 deaths per 100 patients.

We next focus on patients for whom immediate medical attention is critical and selection is unlikely: emergent patients and patients with non-deferrable conditions.[20] Column (3) shows that

[19]For each event, we estimate the model on a time window of 6 quarters before and 12 quarters after adoption of the new recruitment system.

[20]Following Card et al. (2009), we classify diagnoses as more or less deferrable based on whether their admission

reform adoption led to a 7.4% decrease in the death rate of patients with non-deferrable diagnoses, while column (4) shows that the reform led to a 9.6% decrease in the death rate among emergency admissions. Finally, in column (5) we estimate the policy's effect on the number of deaths using a Poisson model, which focuses on death counts rather than death rates.[21] Our result shows that the reform decreased the number of in-hospital deaths by around 5%—i.e., $\exp(\hat{\beta}) - 1$, where $\exp(\hat{\beta})$ is the incidence rate ratio of deaths.[22] In Appendix C, we compare our findings with the effects of other policies examined in the literature and find that they are of similar orders of magnitude.

In light of multitasking concerns in the public sector (Dixit, 2002), one might worry that reducing mortality came at the expense of other aspects of care. For instance, if patients discharged from one hospital require emergency care within a short time, it could indicate poor initial treatment. To assess this, we examine the reform's impact on readmission rates (patients discharged alive but readmitted via emergency at any hospital within 30 days), median length of stay, and complication rates (the share of inpatients with diagnoses related to infections, hemorrhage, or other complications). As shown in Appendix Table A.4, we find no significant effects of the reform on these outcomes, suggesting that mortality reductions did not come at the expense of overall quality of care.

One might also worry that managers influence diagnoses for billing or revenue purposes (Silverman and Skinner, 2004) or that they may reject patients based on the severity of their illness.[23] Although careful consideration of our setting suggests that these concerns are unlikely to drive our results, we present evidence from additional exercises to show that our findings are not driven by changes in patient composition.[24] For this purpose, we fit a series of logit models for the outcome of death using the set of case-mix controls and more than 1.1 million patient-level pre-reform observations, and obtain patients' risk scores and predicted aggregate deaths at hospital level. With these measures, we estimate the effect of the reform on the risk-adjusted mortality rate, which, following the UK's NHS (NHS Digital, 2016), is defined as the ratio between the actual hospital-level death rate and the predicted death rate—i.e., an increase from 1 indicates more deaths than

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.

[21] In our main specification, we use logged death rates as the outcome variable, which mechanically excludes observations when hospitals report no deaths in a given quarter. Appendix Table A.3 provides alternative robustness checks for these cases and shows very similar results.

[22] In Panel B in Figure A.5, we consider a stacked Poisson event study model and do not find evidence of pre-trends.

[23] The reform may also 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).

[24] 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. In addition, the law forbids CEOs from selecting patients based on their condition and they must adhere to referral and counter-referral guidelines.

predicted, and a decrease from 1 indicates fewer deaths than predicted.

Table 2 presents the results. Columns (1)-(3) show estimates from Equation 2 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' type of health insurance as a proxy for socioeconomic status. Finally, column (3) corresponds to our preferred measure, and includes patients' diagnoses based on the enhanced Elixhauser comorbidity index (Elixhauser et al., 1998; Quan et al., 2005). Results are stable across columns and show that the reform decreased the ratio of actual over predicted death rate by around 8.5%, which implies that hospitals' death rates decreased by more than what can be predicted based on their patients' case mix.[25] This is reassuring, because—according to recent research that leverages quasi-random variation in death rates—risk-adjusted mortality measures are reliable and valid indicators of hospital quality in the U.S., where the institutional setting is more prone to patient selection (Doyle et al., 2019). Finally, Column (4) in Table 2 presents difference-in-differences estimates of the impact of the reform on log aggregate mortality rates predicted based on patients' risk scores.[26] Overall, we find no evidence of changes in hospital risk scores after adoption of the reform.

The selection of patient characteristics might operate through variables unobservable to the econometrician. Perhaps managers can 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 2 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.7 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 demand-side selection 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 low-cost healthcare in public hospitals; some cannot buy private healthcare using their public insurance, and consequently are locked into the public health network. Second, we can empirically identify the set of patients who strictly comply with the referral guidelines described

[25] In Appendix Figure A.6, 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. We do not observe pre-trends on this outcome.

[26] In Appendix Figure A.6, Panel B, we present the corresponding event study evidence.

in Appendix A.[27] For this analysis, we estimate Equation 1 using smaller samples consisting of low-SES patients who are more likely to be locked in, as well as patients who strictly comply with observed referrals. The results of this approach—which should mute demand-side sorting, if any—are presented in Figure A.8. Reassuringly, in both restricted samples we find a similar impact of the reform on hospital performance.

Having established that patient selection does not drive the observed effects on hospital performance, we shift our focus to hospital-level changes, specifically regarding hospital personnel. Recent studies show a robust relationship between reduced turnover among doctors and nurses and lower mortality rates in NHS hospitals (Moscelli et al., 2023, 2024), and literature from personnel economics shows that better-managed organizations recruit and retain workers with higher human capital (Bender et al., 2018). Bloom et al. (2015) further highlight a positive correlation between effective management practices and lower staff turnover. Recent evidence also suggests that high personnel turnover adversely impacts outcomes in public-sector organizations (Akhtari et al., 2022), and emphasizes staff retention as a key challenge, particularly in public healthcare organizations (NHS, 2020).

Recognizing the importance of staff retention, we examine how hospital workforce turnover responded to the appointment of new managers. To examine this outcome, we use novel administrative data on healthcare personnel collected by the Ministry of Health for HR purposes (SIRH, 2019).[28] We estimate the model given by Equation 1 on the average turnover rate, defined as the number of workers who will leave in the next period. Panel A in Appendix Figure A.9 shows that the reform reduced the turnover of doctors by 7.5% and had no discernible impact on other health workers. While higher pay is a lever for skilled health worker retention (Antwi and Phillips, 2013), we do not find that the reform affected wages, as shown in Panel B in the same figure. This result is expected, given that wages in this context are determined by public-sector-wide adjustments. Based on anecdotal evidence from conversations with public sector managers and doctors, we posit that the lower turnover may be driven by better incentives for high-skilled personnel through the unobservable benefits and amenities managers can negotiate directly with doctors, such as schedule flexibility.

[27] 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.

[28] A drawback of this dataset is that it only starts in 2011, and until 2013 is only available at yearly frequency. Thus, we perform the analysis at the annual level within the time frame 2011-2019.

4 Managers Matter for Hospital Performance

Since the key intent of the reform was to hire more talented managers, we next examine the role CEOs play in hospital performance and the extent to which better CEO selection can explain our findings.

4.1 CEO Identity and Hospital Performance

We begin by examining the role of CEO identity in explaining observed variation in hospital performance. We follow the literature (e.g., Bertrand and Schoar, 2003; Fenizia, 2022) and compare the adjusted R^2 from different regressions of logged death rates on controls, sequentially including hospital and CEO fixed effects. As shown in Column (3) of Table 3, adding CEO fixed effects increases the adjusted R^2 from 0.69 to 0.75. This increase, similar in magnitude to that reported in the literature, indicates that CEOs account for a considerable portion of the variation in hospital mortality in Chile. An F-test formally rejects the null hypothesis that all CEO effects are zero.

Motivated by this finding, we compute individual measures of CEO talent following the approach pioneered by Abowd et al. (1999) to disentangle the components of wage variation and later used by Fenizia (2022) to model public sector productivity. We decompose the logged death rate of hospitals as

$$\text{Ln(death rate)}_{ht} = \psi_{M(h,t)} + \alpha_h + X'_{ht}\Delta + u_{ht}. \tag{3}$$

The parameters of interest are CEO fixed effects, $\psi_{M(h,t)}$, which capture managerial talent specific to a given CEO and are assumed to be portable across hospitals. Hospital fixed effects, α_h, capture time-invariant characteristics of the hospital (e.g., size and types of procedures performed), and X_{ht} include time-varying characteristics of patients' case-mix and time fixed effects that capture seasonal shocks to patients' health and health provision. Identification of $\psi_{M(h,t)}$ requires that CEO mobility is as good as random, conditional on α_h and X_{ht}. Also, $\psi_{M(h,t)}$ and α_h are separately identified within a set of hospitals that are connected by CEO mobility (Abowd et al., 1999; Card et al., 2013).

Estimation and Validation: Our primary estimation sample includes 673 CEOs, 112 hospitals, and 22 connected sets generated by 78 movers. We estimate the model via constrained OLS. Since our interest is on CEO fixed effects as a measure of managerial talent, we perform empirical Bayes shrinkage to adjust these estimates by their reliability (Chandra et al., 2016; Walters, 2024). Appendix Figure A.10 shows the distribution of the adjusted CEO fixed effect estimates, with a standard deviation of 0.23.

To validate the use of CEO fixed effects as a proxy for CEO quality, we implement the quasi-experimental methodology developed in Chetty et al. (2014), and assess whether hospital performance changes after CEO turnover *as predicted* by the change in CEO fixed effects. Panels A to D in Figure 4 show the impact of the entry and exit of high- and low-quality CEOs on residualized hospital death rates around the event of CEO turnover. We classify a CEO as high or low quality if their fixed effect is below or above the median in the distribution of fixed effects within their connected set. In general, we find that our estimates of CEO talent are in line with the changes in mortality observed upon CEO turnover, which suggests that our proxy of quality is forecast unbiased—i.e., the observed change in mortality rates after CEO turnover is not significantly different from what one would predict based on the change in the CEO's fixed effects.

To further validate our estimates, we follow Card et al. (2013) and assess two potential concerns for identification of CEO fixed effects $\hat{\psi}_{M(h,t)}$. The first concern is that CEO mobility may be endogenous if better CEOs systematically move to hospitals that are already improving or are hired in response to temporary productivity shocks. To address this concern, we follow Fenizia (2022) and classify CEO transitions into terciles based on the difference between the incoming and incumbent CEO's fixed effect. We find that hospitals with CEO changes in the first tercile experience sustained declines in death rates, while those in the third tercile see symmetric increases. Hospitals with small changes in CEO quality show no significant effect. Importantly, CEO transitions do not correlate with pre-trends in hospital performance. Panel A in Appendix Figure A.11 presents these results.

The second concern is the possibility of match effects between CEOs and hospitals, which could bias our estimates if certain CEOs perform better in specific hospitals. To gauge the importance of match effects, we divide the estimated manager and hospital fixed effects into quartiles and assess whether the mean residuals from the model in Equation 3 are abnormally high or low for a given pair of hospitals and CEOs. Reassuringly, all residuals are small, which suggests that the match effects, if present, are negligible.[29] Panel B Appendix Figure A.11 presents this results.

4.2 Impact of the Reform on CEO Characteristics

To examine whether the reform was able to recruit more talented managers, as measured by their estimated CEO fixed effect, we apply the same research design as in Equation 2 but replace the dependent variable with the standardized fixed effects of the manager in each hospital and quarter. Since fixed effects can only be compared within connected sets, we also saturate the regression with connected set indicators.

[29]This result is consistent with column (4) of Table 3, which shows that including hospital-by-manager fixed effects—thus allowing for match effects—does not improve model fit, as measured by the R^2.

We find that the hiring reform was successful in recruiting more talented managers. Column (1) in Table 4 presents this result. Within connected sets, the reform led to a 0.25 standard deviation decrease in the fixed effect of appointed CEOs. Since the standard deviation of our empirical-Bayes adjusted CEO fixed effects is 0.23, the impact on CEO fixed effects is -0.058 (i.e., -0.25 $\times$ 0.23), which implies a 5.8% decrease in death rates.[30]

We now zoom in on the policy's impact on the educational background of public hospital CEOs. We focus on this variable because before the policy's implementation, a strong social norm in the public health sector held that these positions were reserved exclusively for doctors. Although no statutory rule explicitly barred non-medical professionals from being selected as CEOs, in 2004—the year before the first hospital adopted the selection reform—99% of public hospital CEOs held medical degrees. The policy substantially altered this norm: By 2019, the proportion of CEOs with medical degrees in treated public hospitals had decreased to 53%. The de facto exclusion of individuals with non-medical degrees from CEO roles is relevant in light of recent research showing that barring qualified managers for reasons unrelated to their professional credentials can hinder organizational performance (Huber et al., 2021) and, more broadly, that talent misallocation reduces aggregate output (Hsieh et al., 2019).

Figure 5 presents the results of stacked difference-in-differences regressions, as specified in Equation 2, on various variables that capture the educational background of hospital CEOs.[31] Panel A shows that the reform increased the share of CEOs with undergraduate management degrees by more than 20 percentage points, from a baseline of 5% in ever-adopters the quarter prior to adoption. The increase in the number of CEOs with this background came almost completely at the expense of displacing doctor CEOs and had a slight negative effect on CEOs from health professions other than doctors. Importantly, Panel B in Figure 5 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 around 15 percentage points, from a baseline of 20%, while substantially decreasing the number of doctor CEOs without management training by 30 percentage points.

Column (2) of Table 4 summarizes the impact of the policy on the CEO's management training

[30]Note that this effect is not directly comparable to the headline impact of the reform on hospital mortality, since it is estimated within connected sets and in the sample for which CEO fixed effects can be computed.

[31]We measure educational background using two complementary variables. First, we construct a variable that takes the value of 1 if the individual has an undergraduate degree with management coursework and 0 otherwise. We consider the following undergraduate 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 of 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 U.S.

status—regardless of whether the CEO is a doctor—as well as on demographics. We find that the reform increased the likelihood that CEOs have a management undergraduate degree or management postgraduate training by 36 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.[32] The average across ever-adopters the period prior adoption was 28%. As a point of comparison, in NHS hospitals, Janke et al. (2018) 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.

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. 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 less than 25%.

4.3 Is the Reform Working Through New CEOs?

We conclude this section by investigating whether changes in CEO characteristics can explain the observed effects of the reform on hospital mortality. If the effects of the reform on death rates are driven by the new CEOs, a natural test is to examine whether hospitals that post-reform appointed a CEO of better quality experienced a stronger effect on performance. Column (1) in Table 5 replicates our main specification in the sample for which we have information on educational background and estimated CEO fixed effects. We find that the reform leads to an almost 7% decrease in mortality in this sample. Next, we ask whether reform-induced changes in CEOs that led to appointing more talented managers are associated with differential impacts of the reform. Column (2) shows that hospitals whose appointed CEO has a fixed effect below the median of their connected set experienced a decrease in death rates of about 14%, which suggests that the effect of the reform was largely driven by hospitals that hired higher-quality managers.

To further investigate the extent to which changes in CEO-specific characteristics can account

[32]This finding is consistent with the surge of health management postgraduate degrees in the country. Before 2003—the year the reform was enacted—there were no health management postgraduate programs in the country. As shown in Appendix Figure A.12, 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. See, for example, this news report: https://www.americaeconomia.com/articulos/notas/mba-en-salud-para-que-medicos-chilenos-entren-al-mundo-del-management.

for the effectiveness of the reform, we turn to the CEO's educational background. Specifically, we focus on managerial training for three reasons. First, it is correlated with the CEO's fixed effects.[33] Second, it is the characteristic that most drastically changed post-reform. Third, the pre-reform social norm of reserving CEO positions for doctors led to a skill mismatch, since individuals were employed in roles unrelated to their primary field of study. The reform mitigated this mismatch by replacing doctor CEOs with professionals holding management degrees and encouraging doctors who aspired to hospital CEO roles to invest in management education.[34]

In column (3) of Table 5, we replicate our main specification in the sample for which we have information on educational background. In column (4), we interact the reform dummy in Equation 2 with a dummy variable that takes the value of 1 if the CEO has *any* management training, including both undergraduate and postgraduate studies, and 0 otherwise. We find that hospitals that appoint CEOs with management studies after the reform experienced a decrease in death rates of almost 10%, while the effect in hospitals with post-reform CEOs without management background is not statistically different from zero. Finally, in column (5), we compute the differential effects between CEOs with no management training, doctor CEOs with management training, and non-doctor CEOs with management training. Again, 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, which suggests that management training is the primary predictor of performance compared with other educational background characteristics.

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 degree do not improve firm performance and reduce employees' wages by means of rent-sharing practices.[35] A key difference is that in the public sector, CEOs face different incentives and have less scope to reduce employees' pay, given public sector wage schedules. Further, CEOs with management training who self-select into the public sector might have higher levels of prosocial motivation and be better aligned with the organization's mission than those in the private sector (Finan et al., 2017; Ashraf and Bandiera, 2018).

[33] Appendix Table A.5 presents the results from regressing CEOs' managerial talent on their observable characteristics. We find that management studies are correlated with managerial talent as proxied by CEO fixed effects. CEOs with management studies have an estimated fixed effect 0.22 standard deviations below that of CEOs without management studies. Also, we find a negative association between CEOs' age and talent; and similar to findings in Fenizia (2022), we observe that female CEOs are associated with higher levels of managerial talent.

[34] This phenomenon is known as horizontal mismatch—distinct from vertical mismatch, whereby individuals possess a higher or lower level of educational attainment than required for their jobs. While emerging literature examines horizontal mismatch in the private sector, there is limited research on this issue in the public sector (Nordin et al., 2010; Besley et al., 2022). In the public sector, factors such as low exit rates among public employees and technological change may contribute to skill mismatches and potentially hinder performance (Besley et al., 2022).

[35] Panel B of Appendix Figure A.9 shows that the reform did not impact the hospital wages of employees other than the CEO.

Why Only Doctor CEOs Pre-Reform? Given the significant impact on performance delivered by CEOs with management training, why are all public hospitals managed only by doctor CEOs before the reform? 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]

5 The Financial Incentives of the Reform

The recruitment reform introduced higher base wages and performance-based pay for CEOs in hospitals that adopted it. In this section, we describe these financial incentives and examine the extent to which they mediate the effects on hospital mortality.

Wage Increase: We begin by examining 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 reform bonus relative to pre-reform pay in Appendix Figure A.13, where we present event study evidence to assess the reform's effect on CEO wages. We find an effect, on average, of around a 25% increase in pay relative to the period before the reform.[38]

Performance Pay: In our setting, the head of the Health Service jointly drafts a performance contract with the hospital CEO for a 3-year period. At the end of each year, the CEO receives a final score based on the parameters set in the contract. The yearly wage is determined by the performance level from the previous period. If the performance in the previous period is 95% or higher, the yearly wage remains at 100%. If the performance is between 65% and 95%, the yearly wage is reduced by 1.5%. If the performance is below 65%, the yearly wage is reduced by 7%. Two points are worth noting about this schedule: First, the first-year wage remains unaffected since it is

[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.

[38]Note that the observed wage might also depend on CEO characteristics, so the effect is a composite of mechanical changes in pay due to changes in the manager's identity and the pay increase.

based on the previous year's performance, with penalties applying only in years 2 and 3. Second, the incentive structure introduces a modest penalty, with no opportunity for a wage increase, and a maximum penalty of only 7% of the yearly wage for very poor performers.

5.1 Extensive and Intensive Margin Effects of Financial Incentives

Low wages and low-powered incentives in the state are often highlighted as one source of the inefficient performance of public employees. To understand the impacts of financial incentives on performance, we distinguish two mechanisms that could be at play: Financial incentives may attract higher-ability CEOs to apply for these positions (extensive margin) and may also motivate selected CEOs to work harder (intensive margin).

Extensive Margin: The first way in which financial incentives can affect performance is by attracting more talented workers. For instance, Dal Bó et al. (2013) provide experimental evidence showing that higher pay in the public sector attracts a higher quality pool of candidates. Ashraf et al. (2020) show that material benefits—in the form of career advancement—improve the quality of the pool of applicants, and through this mechanism have a positive effect on the performance of community healthcare workers.

In our setting, it is not possible to observe the pool of CEO applicants in each hospital before the policy adoption, and we cannot identify the impact of financial incentives on the quality of the applicant pool. Given the magnitude of the pay increase, it likely played an important role in attracting talented candidates to apply. We thus interpret our findings of the effects of the reform on the quality of recruited managers as a result of both financial incentives and competitive recruitment.

Intensive Margin: Theoretically, both performance-based incentives and higher base wages can drive CEOs to exert greater effort and achieve better results.[39] For instance, offering wages above a manager's outside option creates a motivation to increase effort, which can lead to productivity gains (Katz, 1986). In our context, if the reform's pay increase generates labor rents, this mechanism may be at play and post-reform managers could have enhanced hospital performance due to the introduction of new financial incentives. If so, one could be worried that our findings can be simply explained by the intensive margin effects of these incentives, rather than by the effects of bringing in better CEOs.

First, we explore the extent to which performance pay was binding in our setting. We accessed all available performance contracts and scores for the first post-reform managers.[40] Figure A.14

[39]Empirically, recent studies show that financial incentives can boost employee performance in the public sector (Muralidharan and Sundararaman, 2011; Khan et al., 2015; Burgess et al., 2017; Deserranno et al., 2023)

[40]Some of the oldest contracts and performance scores were lost, and the Civil Service has no available records.

shows the cumulative distribution of performance scores for the first post-reform CEO in each adopting hospital. Around 70% of scores are at or above the 95% threshold—and thus avoid wage penalties—and most of the remainder falls between 95% and 65%, for which CEOs only face a 1.5% wage penalty. Almost no CEO scores below 65%, which would imply a 7% wage penalty. This indicates that the performance agreements were likely not binding in our setting, and most managers easily met their targets. Later studies on performance agreements across all public agencies found similar issues, suggesting that targets were met due to poor mechanism design rather than effective performance incentives (CPPUC, 2013; CADP, 2017). For instance, in 2013, fewer than 5% of government employees under the recruitment system scored below 80% on their performance evaluations (CPPUC, 2013), and by 2016, over 90% had received a perfect score (CADP, 2017). The failure of this tool as an effective management control has been highlighted in several policy reports calling for its amendment (see, e.g., Zaviezo and Undurraga, 2007; CPPUC, 2013; Barros et al., 2018). In light of this evidence, we conclude that, in our context, performance pay is unlikely to be a relevant driver of managerial productivity.

Nonetheless, given the pay increase, efficiency wages might still play a role. To study this hypothesis, we leverage the fact that in some cases the incumbent manager was reappointed through the new selection process. In these cases, the manager's identity remained unchanged despite the reform. If the higher pay explained the observed effects on performance, we would expect to see *some* impact of the reform on those managers and hospitals. To explore this, we focus on a shorter window of 1 year before the reform and 1.5 years afterward. Within this window, 13 out of 88 CEOs remain the same. Column (1) in Appendix Table A.6 presents the average effect of the reform in this narrower window, and column (2) computes the differential effects between hospitals in which the CEO remained the same and those in which the CEO changed. In the cases in which the reform did not lead to the appointment of a new CEO, we find no significant effects on mortality, which suggests that the financial incentives did not play an important role in incentivizing selected managers to exert more effort. We complement this evidence with two additional exercises. First, we examine whether CEO fixed effects change for individual CEOs as a result of the reform. Within-CEO changes in their fixed effects would be consistent with CEOs exerting higher effort in response to the reform. For this exercise, we estimate the model given by Equation 3, but using period-specific CEO fixed effects, which we obtain by interacting manager identity with an indicator for whether their hospital has implemented the reform. Focusing on the restricted sample for which these fixed effects can be computed, columns (3) and (4) of Appendix Table A.6 show no evidence of changes in managerial talent when the incumbent CEO is reappointed. Second, as a final piece of evidence, in Appendix D, we leverage a 2016 amendment to the reform that increased the pay by around 15% for a subset of managers, and we do not find any discernible effects on their performance. All in all, the evidence suggests that the *intensive margin* effects of

We have performance score data for 57 post-reform CEOs.

financial incentives do not explain the performance improvement we observe after the adoption of the selection reform and that our results are consistent with better CEO selection.

6 Conclusion

In this paper, we examine a civil service reform aimed at improving the performance of public sector managers. The reform introduced competitive recruitment and higher pay for top senior executives across all public sector agencies in Chile. Leveraging the staggered adoption across public hospitals, we estimate that the reform reduced hospital mortality by 8%. Our findings suggest that this improvement does not reflect a change in patient composition and that it was primarily driven by changes in the quality of the appointed CEO, as measured by their fixed effect. We also document that the reform displaced doctors with no management studies in favor of CEOs with formal management training, and we show that this training predicts the reform's effectiveness.

Our results suggest that CEO skills are transferable across organizations and that management training could serve as an effective screening tool for leadership positions. Like doctors serving as CEOs in public hospitals, many top executives in the public sector rise from within their professions. For instance, police commissioners often start as officers, school superintendents as teachers, and deans as tenured professors. The findings in this paper may be relevant for these organizations, because they might benefit from emphasizing management education when selecting executives, even for candidates who rise through the ranks of their professions.

We conclude by discussing two limitations of the paper we view as promising avenues for future research. First, our findings do not imply that management training improves CEO performance in the public sector. If talented individuals are more likely to pursue formal management degrees, the observed correlation could be a result of differential selection. Disentangling the causal effects of management training from selection effects remains an important open question for future research. Second, in our setting, we cannot separately identify the effects of competitive recruitment and financial incentives on the quality of the selected manager. Investigating how financial incentives shape the applicant pool for top management positions in the public sector is another exciting avenue for future research.

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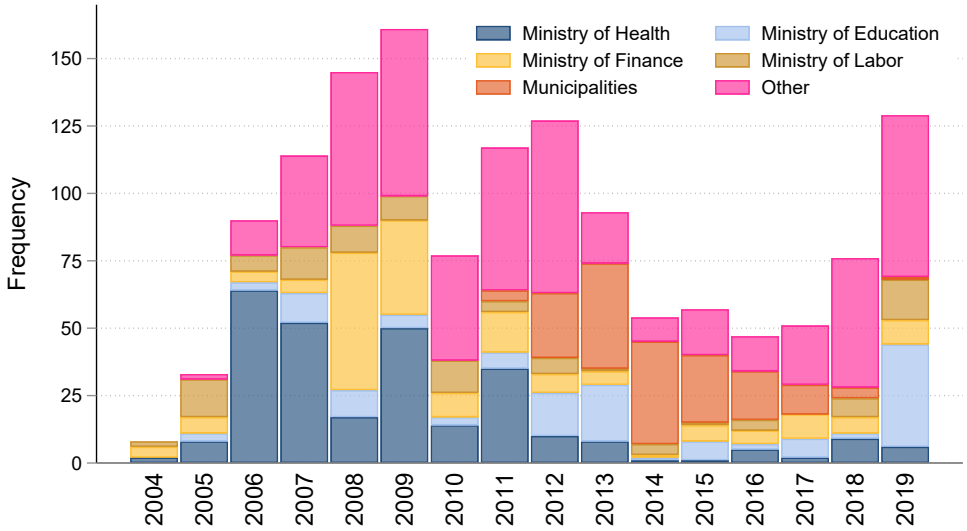
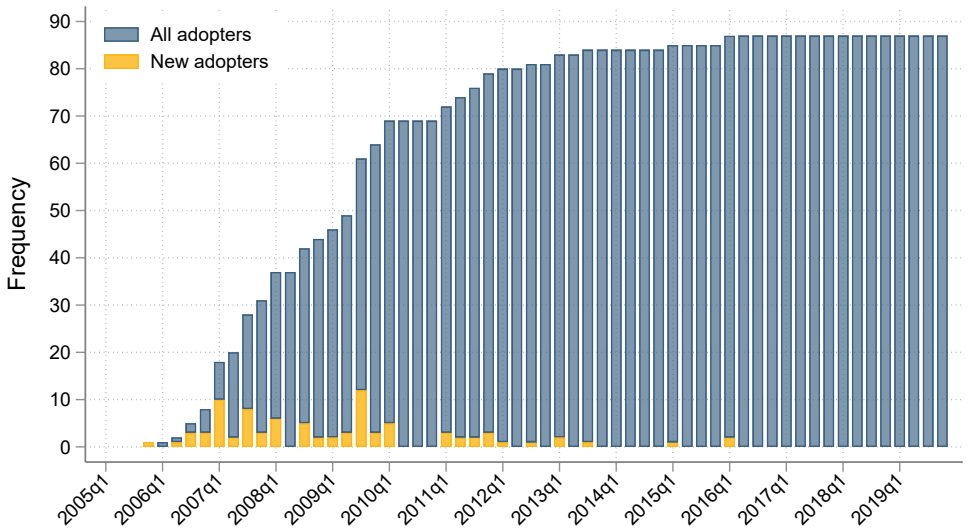
Figure 1: Adoption of the new recruitment process



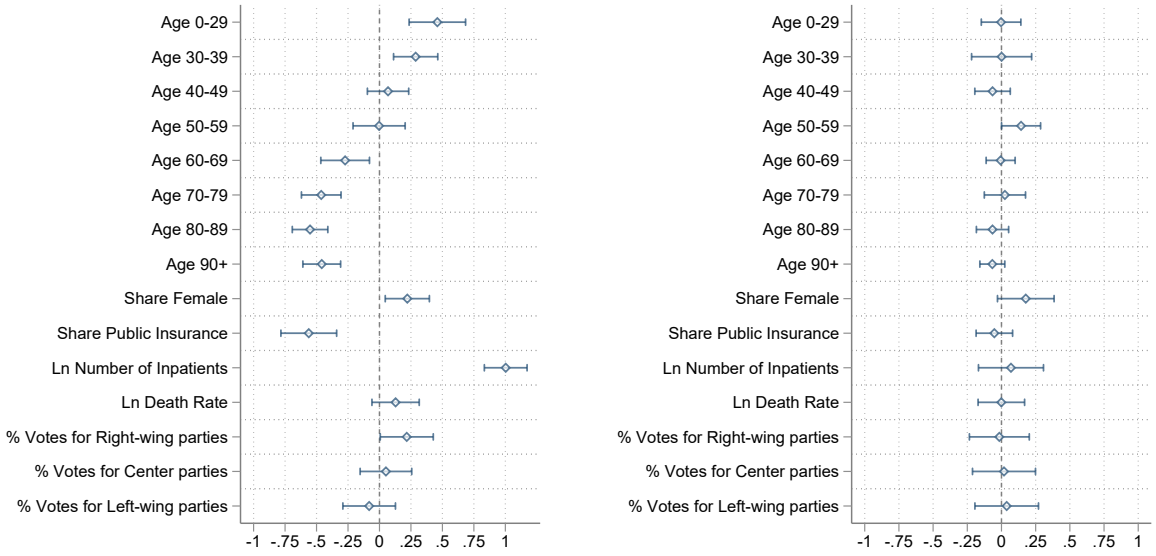
(A) Adoption of the reform across all government agencies



(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.

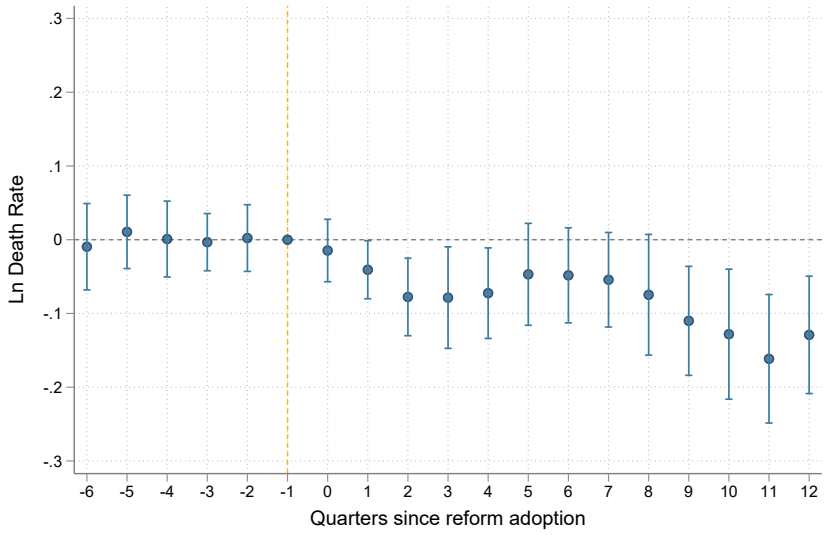
Figure 2: Balance in observable characteristics



(A) Levels (B) Growth

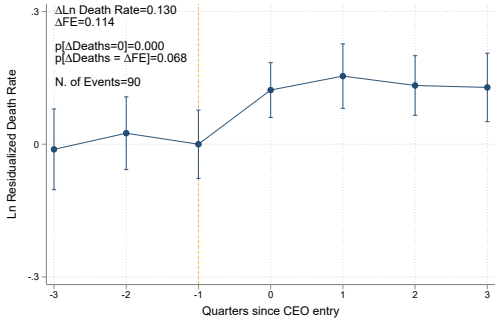
Notes: This figure studies differences between treated units and their control group in an array of observable characteristics, prior to adoption. The control group is composed of units that do not adopt the reform 6 quarters before or 12 quarters after. Panel A presents the coefficient obtained from a regression of each variable on a dummy that equals 1 if the hospital adopted the reform. The regression includes fixed effects for each event. Panel B replicates the analysis but replaces the dependent variable with its first difference between the quarter prior to adoption and 1 year before. The political variables correspond to the vote share of right-, center, and left-wing parties in the most recent pre-adoption municipal election in the municipalities where hospitals are located. The first differences of these variables correspond to the difference in vote shares between both elections. The dependent variables are standardized in both panels. Standard errors are displayed in parentheses and clustered at hospital level.

Figure 3: Dynamic effects of the reform on hospital quality

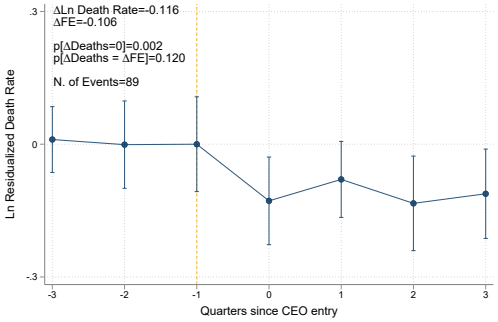


Notes: This figure presents event study evidence of the reform's effect on hospital death rates, following Equation 1. The empirical analysis uses quarterly panel data on public hospitals and includes hospital-by-event and time fixed effects, as well as case-mix controls. We focus on a time window covering 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 the 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.

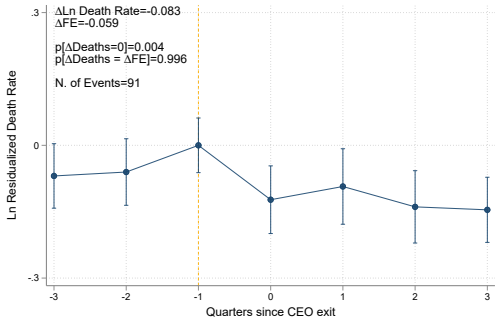
Figure 4: Impact of CEOs entry and exit on mortality



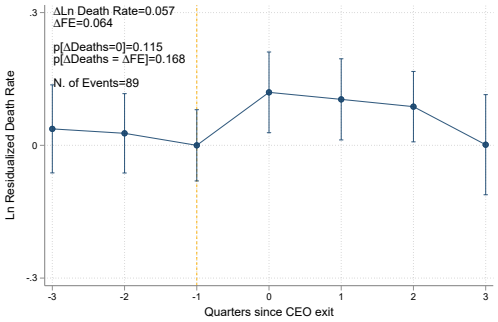
(A) Entry of Low-Quality CEO



(B) Entry of High-Quality CEO



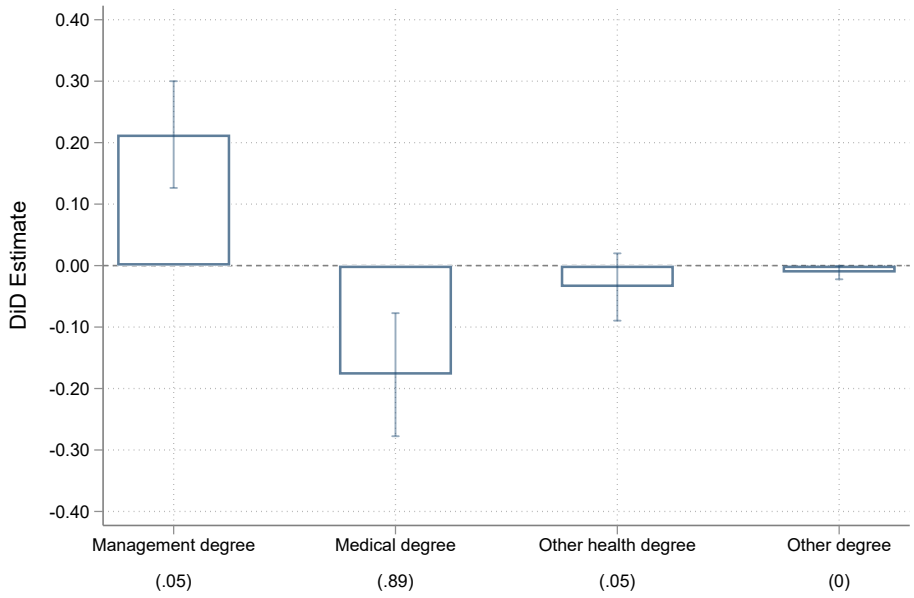
(C) Exit of Low-Quality CEO



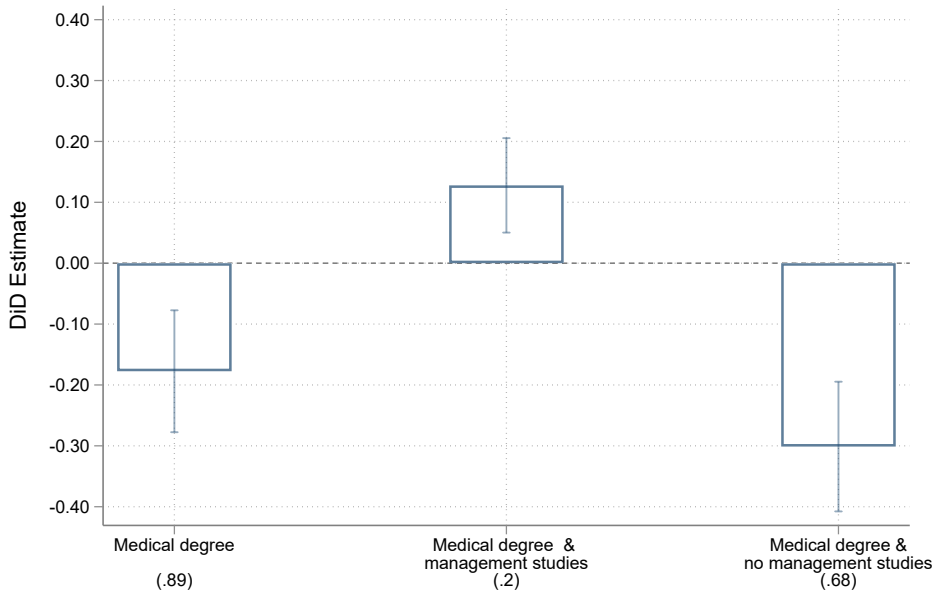
(D) Exit of High-Quality CEO

Notes: These figures plot the impact of the entry and exit of high- and low-quality CEOs (i.e., with fixed effects below and above the median within their connected set, respectively) on the residualized hospital death rate, around the event of CEO turnover. To construct each panel, we first identify the set of CEOs who were appointed and define event time as quarters relative to the period of appointment. We only include observations in which we observe hospital outcomes before and after the change of CEO. In the top left corner, we report the change in death rate and the change in the estimated fixed effects before and after CEO turnover, as well as p-values from two tests of the hypotheses: (i) that the change in death rate before/after turnover equals zero and (ii) that the change in death rate equals the change in CEOs fixed effects. In all panels, each dot corresponds to the mean residualized logged death rate, and vertical lines indicate the corresponding 95% confidence intervals constructed using the standard errors of the means.

Figure 5: 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 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 2 on each outcome, and vertical lines indicate the corresponding 95% confidence intervals. Standard errors are clustered at hospital level.

Table 1: Impact of the reform on death rates

	Ln Death Rate				# Deaths
	In-hospital	28-days	Non-Deferrable	Emergency	In-hospital
	(1)	(2)	(3)	(4)	(5)
1 if reform adopted	-0.082	-0.061	-0.076	-0.096	-0.052
	(0.025)	(0.022)	(0.028)	(0.034)	(0.023)
Mean Dep. Variable	2.52	3.57	3.85	3.12	73.77
Observations	204,466	204,466	191,196	182,664	221,544

Notes: This table presents the impact of the selection reform on public hospital performance, as measured by mortality outcomes. Estimates are from the stacked difference-in-differences specification in Equation 2. The empirical analysis uses quarterly panel data on public hospitals and includes hospital-by-event and time fixed effects, as well as 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. For each treated hospital, we determine an event-specific control group that excludes already-treated units. Column (1) focuses 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. Columns (3) and (4) present the impact of the reform on non-deferrable 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. The mean dependent variable is computed for ever adopters in the quarter before adoption. For columns (1)-(4), the mean dependent variable is presented in levels instead of logs. Standard errors are displayed in parentheses and clustered at hospital level.

Table 2: Impact on risk-adjusted mortality measures

	Ln Actual/Predicted Death rate			Ln Predicted Death rate
	(1)	(2)	(3)	(4)
1 if reform adopted	-0.085	-0.085	-0.086	0.010
	(0.024)	(0.024)	(0.025)	(0.014)
Mean Dep. Variable	1.01	1.01	1.01	0.61
Observations	204,466	204,466	204,466	204,466
Logit Model:				
Patient Demographics	Yes	Yes	Yes	Yes
Type of Insurance	No	Yes	Yes	Yes
Enhanced Elixhauser Comorbidity Index	No	No	Yes	Yes

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 stacked difference-in-differences specification in Equation 2. The empirical analysis uses quarterly panel data on public hospitals and includes hospital-by-event and time fixed effects, as well as 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. For each treated hospital, we determine an event-specific control group that excludes already-treated units. The risk-adjusted death rate is defined as the actual hospital-level death rate divided by the hospital-level predicted death rate. Results are weighted by the pre-policy number of inpatients. Standard errors are displayed in parentheses and clustered at hospital level.

Table 3: Explanatory power of managerial talent to account for hospital performance

	Ln Death Rate			
	(1)	(2)	(3)	(4)
R^2	.46	.70	.78	.78
Adj. R^2	.46	.69	.75	.75
Observations	10,310	10,310	10,205	10,205
Hospital FE	No	Yes	Yes	No
Manager FE	No	No	Yes	No
Hospital-Manager FE	No	No	No	Yes
F-statistic for Manager FEs	-	-	7.01	-
F-statistic for Hospital Manager FEs	-	-	-	10.63

Notes: This table shows how much of the variance in mortality is explained by the hospital and manager 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 4: Effect of the reform on managers' skills and demographics

	CEO Fixed Effect	Has Management Studies	Age	Female
	(1)	(2)	(3)	(4)
1 if reform adopted	-0.25	0.36	-1.82	-0.03
	(0.08)	(0.05)	(1.07)	(0.05)
Mean Dep. Variable	0.32	0.28	49.9	0.23
Observations	112,883	199,006	199,006	199,006

Notes: This table presents the impact of the selection reform on public hospital CEOs' skills and demographics. Estimates are from the stacked difference-in-differences specification in Equation 2, but using CEO characteristics as dependent variables. Column (1) focuses on our CEO fixed-effects estimates (adjusted by their reliability and standardized) as a measure of managerial ability. The specification includes connected set indicators and weights observations by the pre-policy number of inpatients in each hospital. In Column (2) we consider an indicator of whether the CEO has managerial training. Columns (3) and (4) study the effect of the reform on the age and gender of the CEO. The mean dependent variable is computed for ever adopters in the quarter before adoption. All specifications include hospital-by-event and time fixed effects. Standard errors are displayed in parentheses and clustered at hospital level.

Table 5: Heterogeneity in CEO performance by manager characteristics

	Ln Death Rate				
	(1)	(2)	(3)	(4)	(5)
Reform (1 if reform adopted)	-0.066		-0.071		
	(0.036)		(0.023)		
Reform × High-Quality CEO		-0.143			
		(0.057)			
Reform × Low-Quality CEO		-0.028			
		(0.034)			
Reform × CEO w/ Mgmt. Studies				-0.097	
				(0.027)	
Reform × CEO w/o Mgmt. Studies				-0.026	-0.026
				(0.024)	(0.024)
Reform × Non-Doctor CEO w/ Mgmt. Studies					-0.092
					(0.034)
Reform × Doctor CEO w/ Mgmt. Studies					-0.099
					(0.034)
Mean Dep. Variable	2.25	2.25	2.53	2.53	2.53
Observations	112,883	112,883	202,820	202,820	202,820
p-value *High-Quality CEO = Low-Quality CEO*	-	0.030	-	-	-
p-value *w/ Mgmt. Studies = w/o Mgmt. Studies*	-	-	-	0.007	-
p-value *Non-doctor w/ Mgmt. = Doctor w/ Mgmt.*	-	-	-	-	0.866

Notes: This table examines heterogeneous effects of the reform by CEO managerial talent and educational background. We follow the stacked difference-in-differences design in Equation 2 to examine to what extent the reform has differential effects depending on the CEO's fixed effect and educational background. Column (1) replicates our main analysis using the sample for which we have data on CEOs' talent as proxied by CEO fixed effects. Column (2) distinguishes cases where the reform led to the appointment of a High-Quality CEO, defined as a CEO with a fixed effect below the median within each connected set. Column (3) replicates our main analysis using the sample for which we have data on CEOs' educational background. Column (4) focuses on whether the CEO has *any* management training, which includes undergraduate and postgraduate studies related to management. Column (5) focuses on whether the CEO with *any* management training is a doctor. All specifications include event-by-hospital and time effects as well as case-mix controls. Results are weighted by the pre-policy number of inpatients. Specifications in columns (1) and (2) also include connected set indicators. The mean dependent variable is computed for ever adopters in the quarter before adoption. Standard errors are displayed in parentheses and 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 mainly depend on the location of the primary care center and the 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.

Table A.7 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 to the hospital via ER. In this sample, we classify patients into cells defined by the patient's municipality 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 we observed the catchment areas and guidelines were strictly followed, all patients within a cell should attend the same hospital. To visually evaluate this, Figure A.15 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. The fact that patients within a cell are being discharged from different hospitals is likely explained by the fact that we only observe the patient's municipality of residence, and catchment areas do not map one-to-one with municipalities, or by the fact that patients can also use their work address to register with the health system.

B CEO Turnovers Do Not Have an Automatic Impact on Performance

A potential explanation for the effects of the reform could be attributed to mechanical or 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. To explore this mechanism, we leverage CEO turnovers in never-treated or yet-to-be-treated hospitals in any quarter between

2005 and 2019.

To deal with multiple events and the lack of clean controls, we perform a stacked event study in which for each turnover event, we define a time window around it and a control group of hospitals with no turnovers in the time window.[41] Next, we define a set of valid events as those that 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 stacked event study regression:

$$y_{hte} = \alpha_{he} + \gamma_t + \sum_{\tau=-2}^{8} \beta_\tau D_{hte}^{\tau} + \epsilon_{hte}, \tag{A.1}$$

where an event e is a valid turnover. We cluster standard errors at hospital level.

Appendix Figure A.16 presents the effect of a CEO turnover on death rates on never-treated and yet-to-be-treated hospitals. 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 CEO Selection Reform in the Context of Other Policies

In this Appendix, we benchmark 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 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 horse race competition between policies. The results are summarized in Table A.8.

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 U.S. 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

[41] 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.

over 65 admitted via the ER. We find that the reform we study has a slightly larger effect (10%) over a slightly lower 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 over a very similar 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.

D Additional Test on Intensive Margin Effects of Financial Incentives

As an additional test to examine the potential effects of higher wages on CEO performance on the intensive margin, we exploit a 2016 amendment to the recruitment reform (Ley 20,955). Before the amendment, all CEOs were paid according to the public employees' pay grade, regardless of their profession. After the modification, CEOs appointed after November 2016 can choose to be paid according to the medical pay laws instead of the public employees' pay grade, but *only if* they are medical doctors.[42] 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, append the data for each event, and estimate the following stacked event study regression:

$$y_{hte} = \alpha_{he} + \gamma_t + \sum_{\tau=-2}^{4} \beta_\tau D_{hte}^{\tau} + \epsilon_{hte}, \tag{A.2}$$

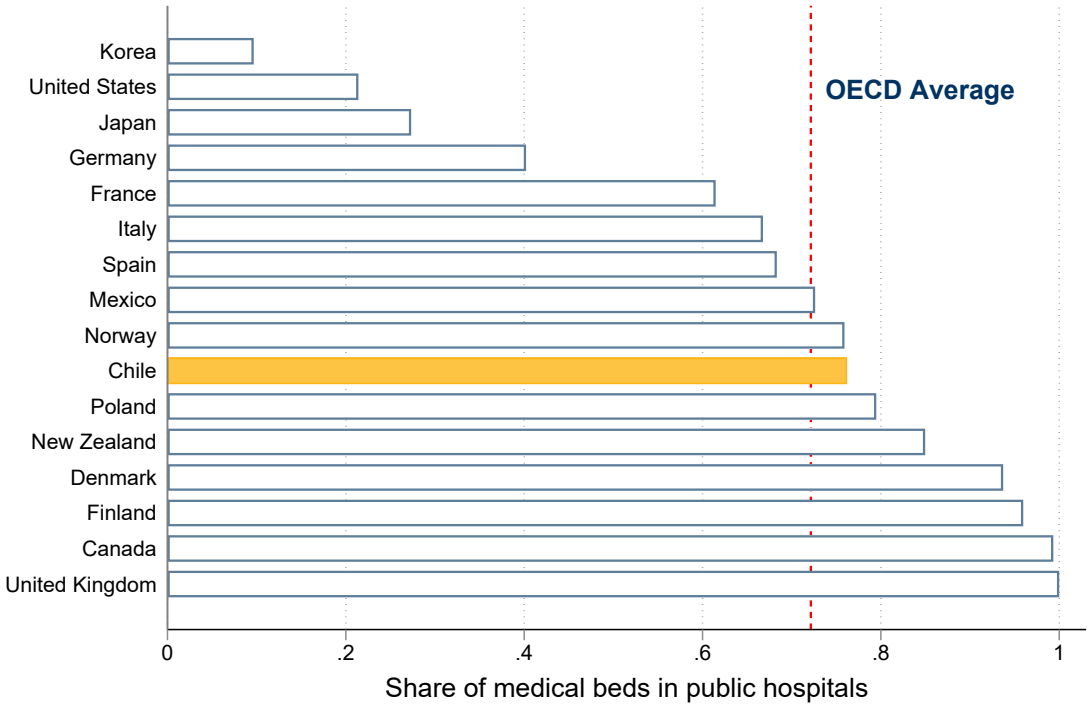
[42] More precisely, doctors can choose to be paid according to Law 19,664 instead of Law 18,834.

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.[43]

Panels A and B in Appendix Figure A.17 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 15% 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 provides further evidence that the efficiency wage hypothesis is unlikely to play a substantial role in this context.

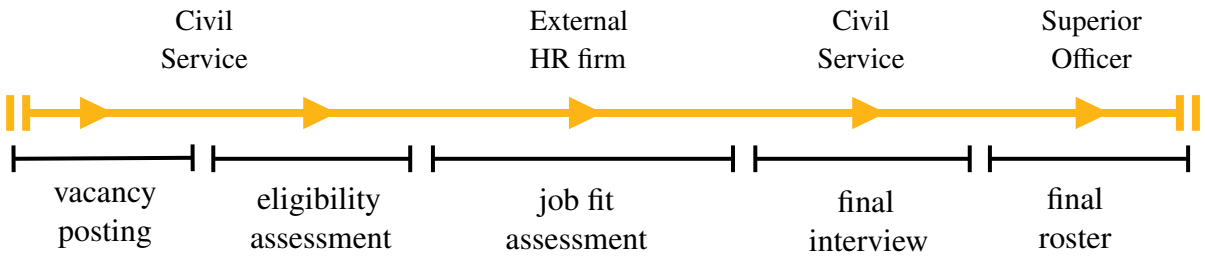
[43]There is a trade-off between the length of the window and the number of events. We consider 2 periods before treatment and 4 periods post-treatment, for which we have 17 events.

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



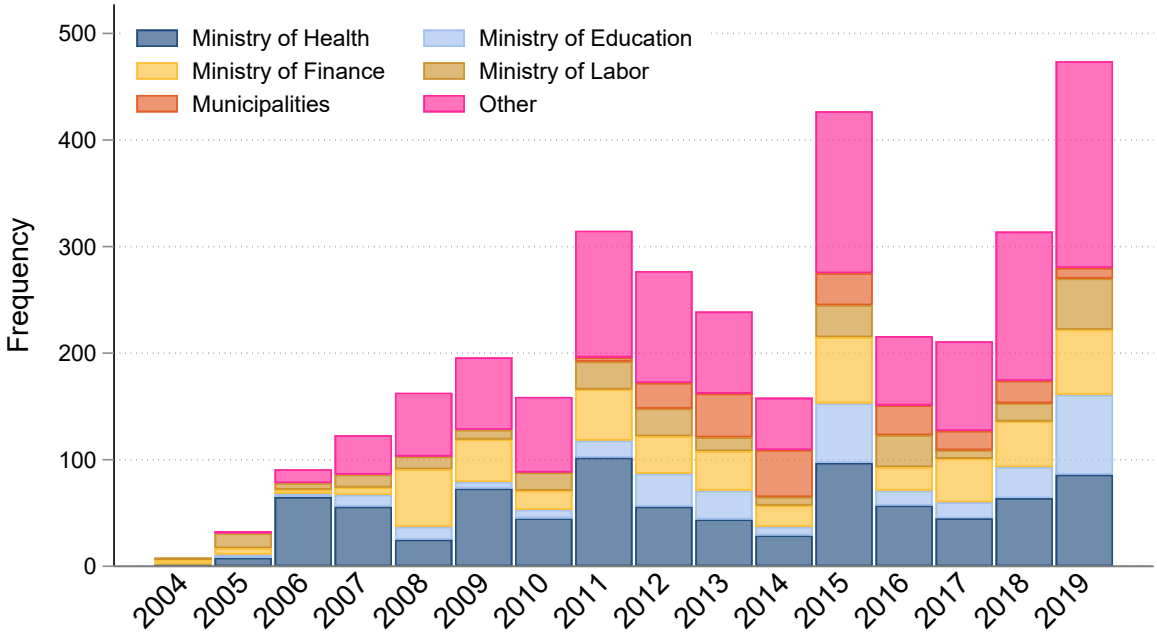
Notes: This figure displays the share of medical beds provided by public hospitals in a set of selected OECD countries in 2019. The dashed red line represents the average share in all OECD countries. The share is computed as the ratio between the total number of hospital beds in publicly owned hospitals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



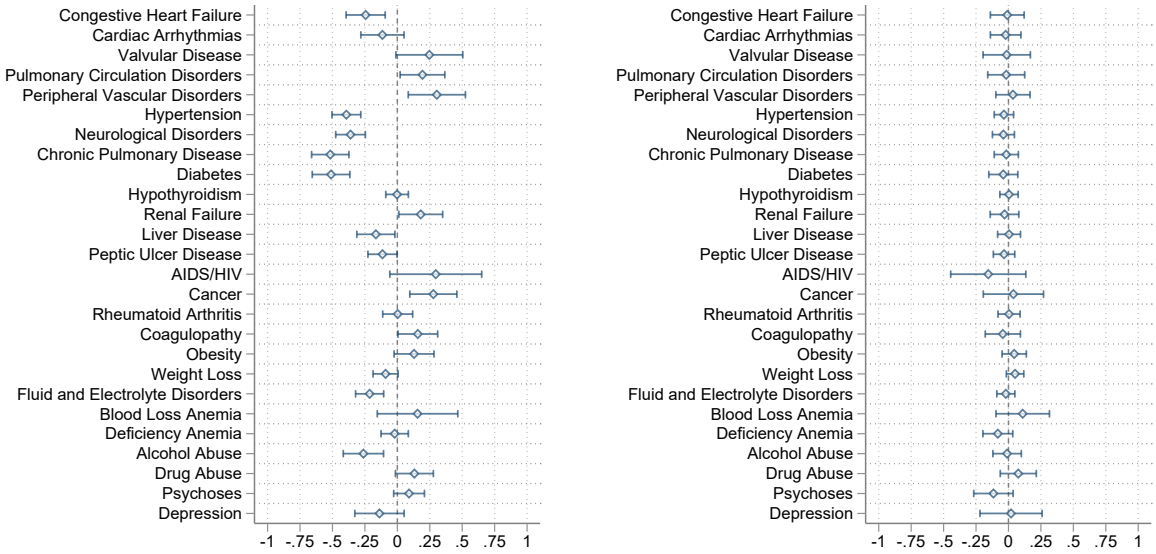
Notes: This figure illustrates the selection process for senior executive positions when the selection reform has been adopted. The job call starts with the position posted online on the Civil Service's website and in a newspaper with national circulation. After the job posting closes, an external HR 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.

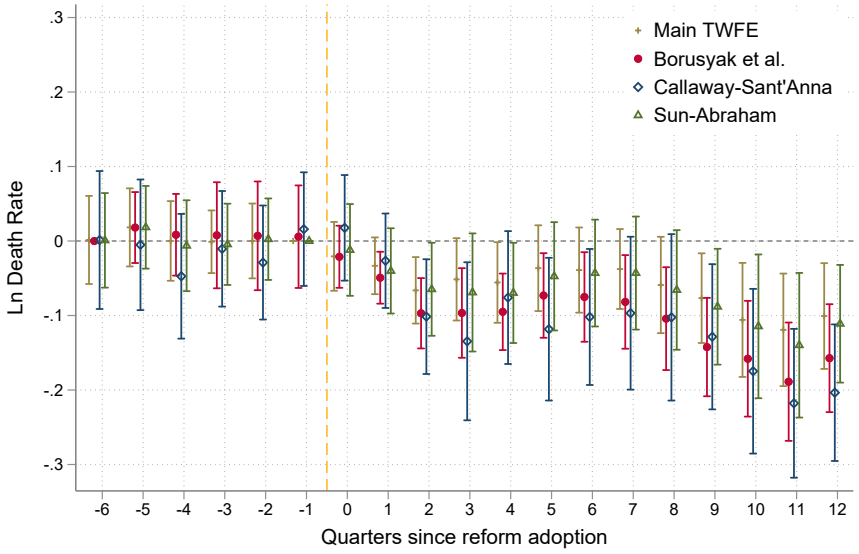
Figure A.4: Balance in observable characteristics: Elixhauser categories



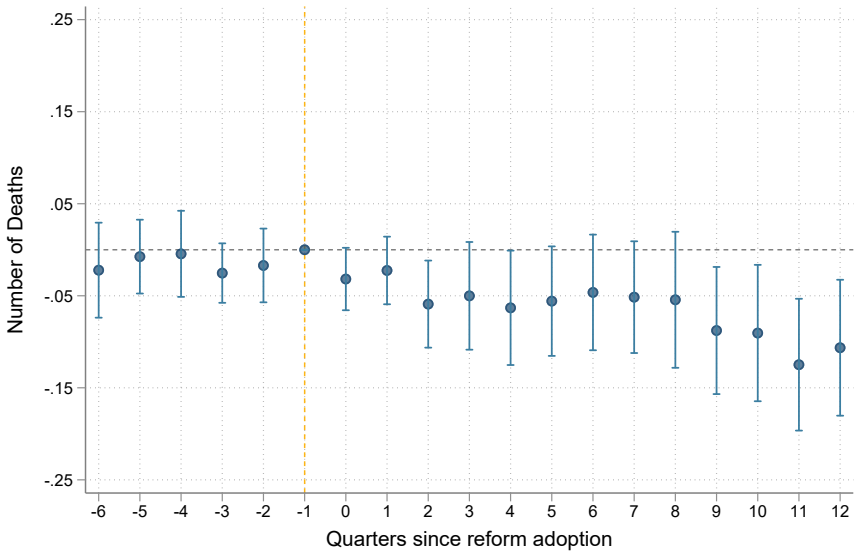
(A) Levels (B) Growth

Notes: This figure examines differences in patient diagnoses between treated units and their control group prior to adoption. The control group is composed of units that do not adopt the reform 6 quarters before or 12 quarters after. The dependent variable represents the proportion of inpatients with each diagnosis, categorized using ICD-10 codes grouped into Elixhauser categories. Panel A presents the coefficient obtained from a regression of each variable on a dummy that equals 1 if the hospital adopted the reform. The regression includes fixed effects for each event. Panel B replicates the analysis but replaces the dependent variable with its first difference between the quarter prior to adoption and 1 year before. The dependent variables are standardized in both panels. Standard errors are displayed in parentheses and clustered at hospital level.

Figure A.5: Alternative event study models and estimation methods



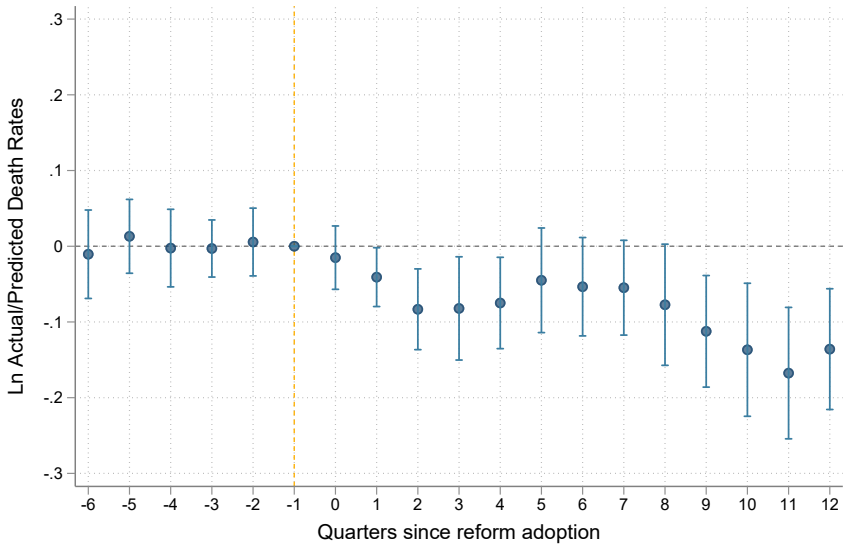
(A) Robustness to treatment effect heterogeneity



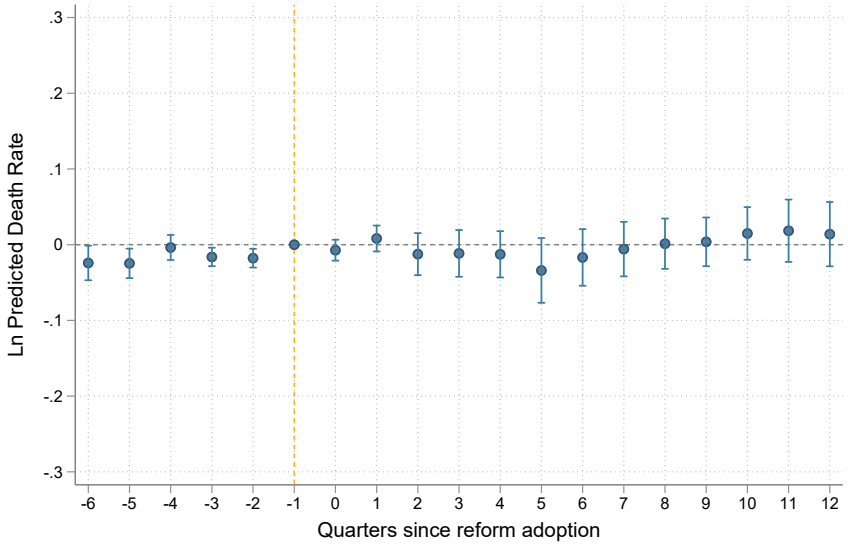
(B) Poisson Model

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 red circle markers); Callaway and Sant'Anna (2021) (in blue diamond markers); and Sun and Abraham (2021) (in green triangle markers), all of which are robust to treatment effect heterogeneity and appropriate in our setting (De Chaisemartin and d'Haultfoeuille, 2023). For comparison, we overlay them to our main results from Figure 3 (labeled as Main TWFE in the figure). 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 the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient.

Figure A.6: Dynamic effects of the reform on risk-adjusted death rate and predicted death rate



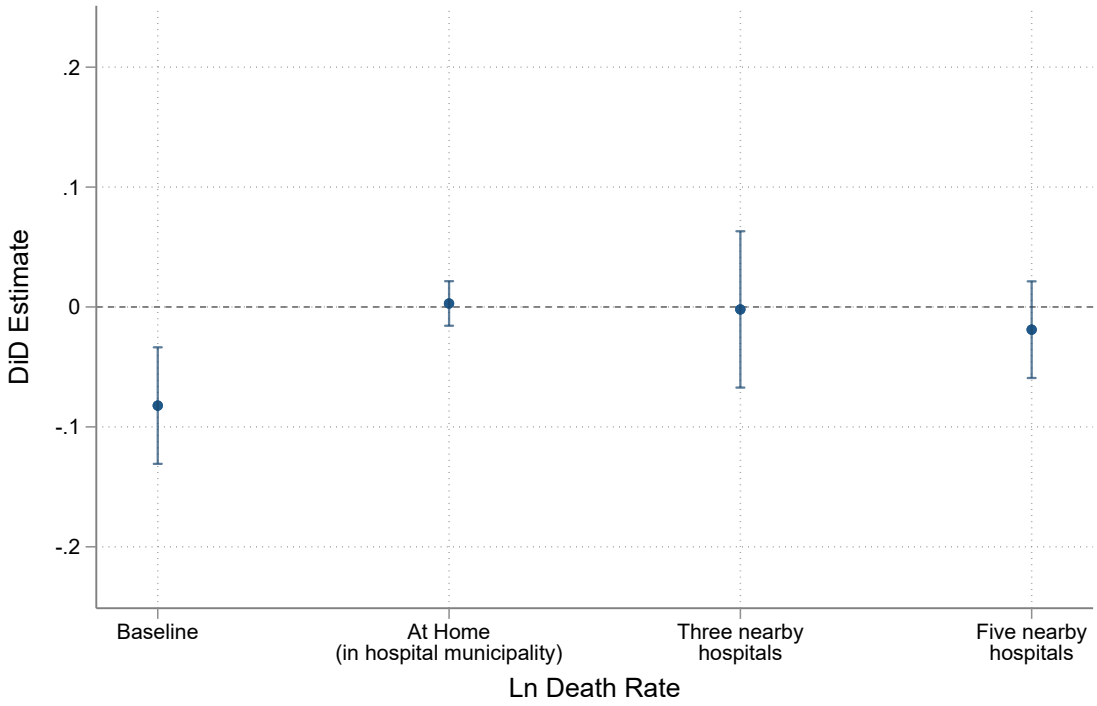
(A) Risk-adjusted mortality



(B) Predicted death rate

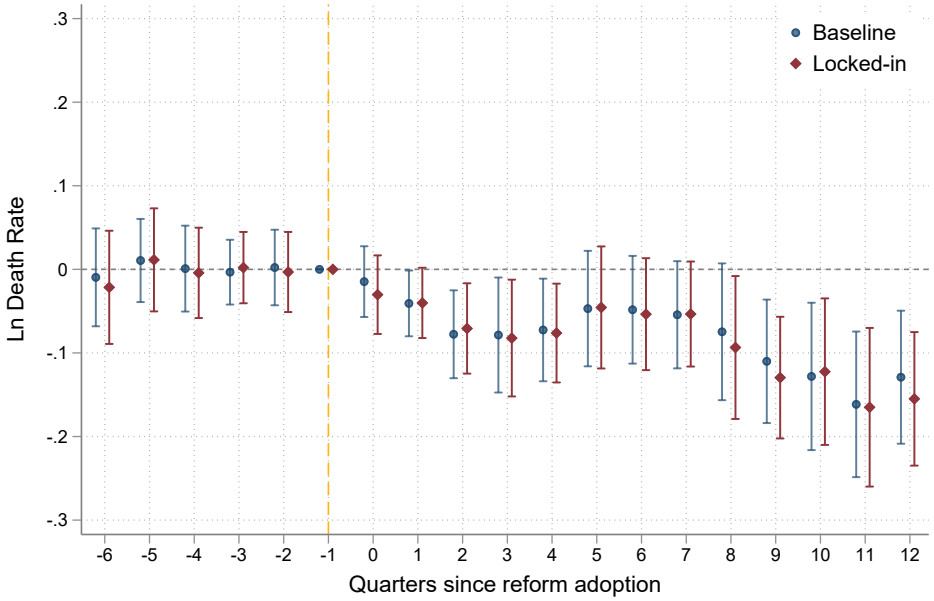
Notes: This figure presents event study evidence, following Equation 1, on the impact of the selection reform on the risk-adjusted death rate and on the predicted death rate. Panel A reports estimates for the logged risk-adjusted death rate, and Panel B reports estimates for the logged predicted death rate. For this exercise, we use 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 hospital-by-event and time fixed effects, as well as case-mix controls. Results are weighted by the pre-policy number of inpatients. Each dot corresponds to an estimated coefficient, and vertical lines indicate the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. Standard errors are clustered at hospital level.

Figure A.7: Testing for patient selection: Supply side

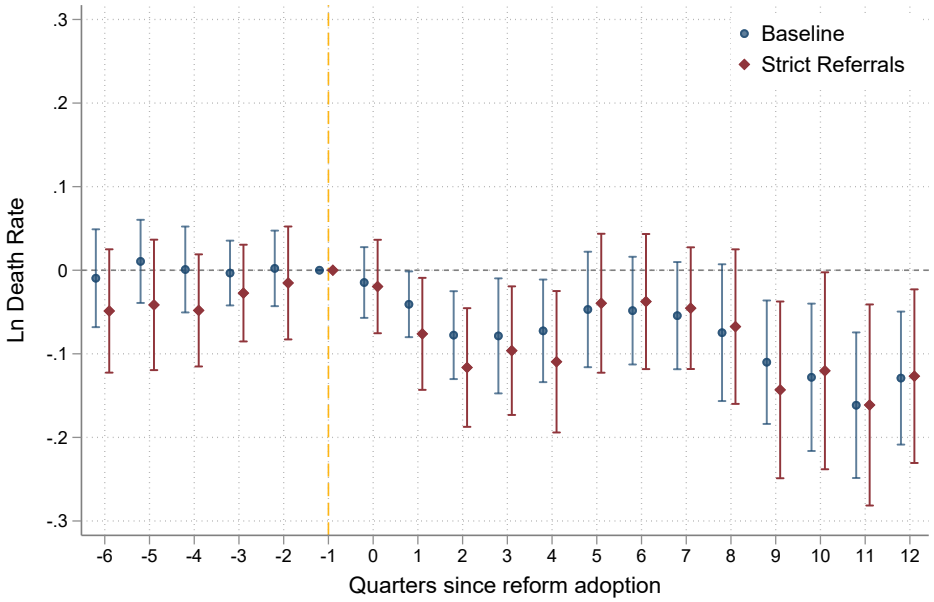


Notes: This figure presents evidence to assess patients' selection as a confounder of our main results. We plot the estimates and confidence intervals obtained by estimating Equation 2 for the logged at-home death rate and for logged death rates at nearby hospitals. All regressions include hospital-by-event and time fixed effects as well as case-mix controls. Standard errors are clustered at hospital level.

Figure A.8: Testing for patient selection: Demand side



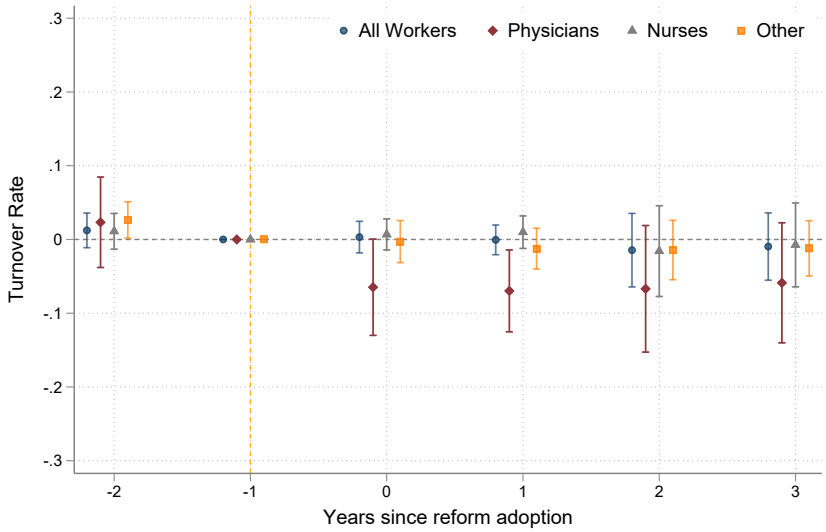
(A) Locked-in patients



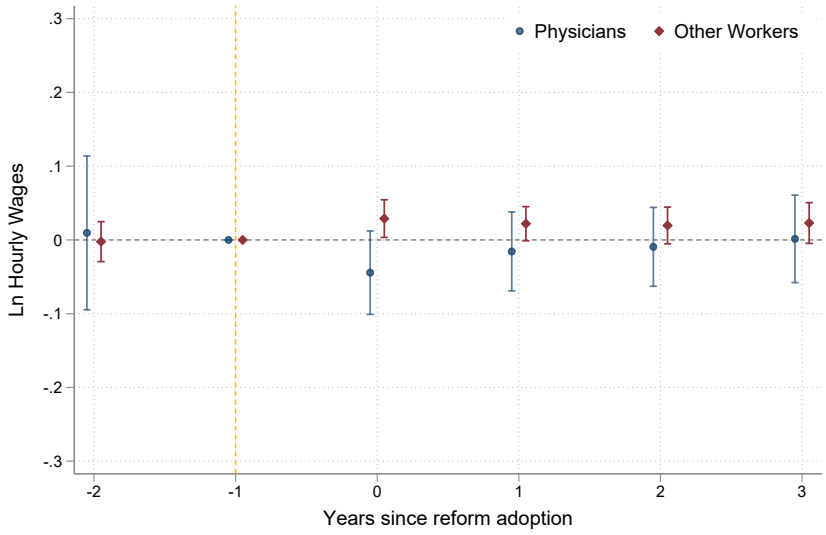
(B) Strict-referrals patients

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 1, 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 1, but on a restricted sample of patients who followed the referrals mandated by the health system. These figures also include the baseline estimates for comparison. All regressions include hospital-by-event and time fixed effects as well as case-mix controls. Standard errors are clustered at hospital level.

Figure A.9: Effect of the reform on hospital personnel outcomes



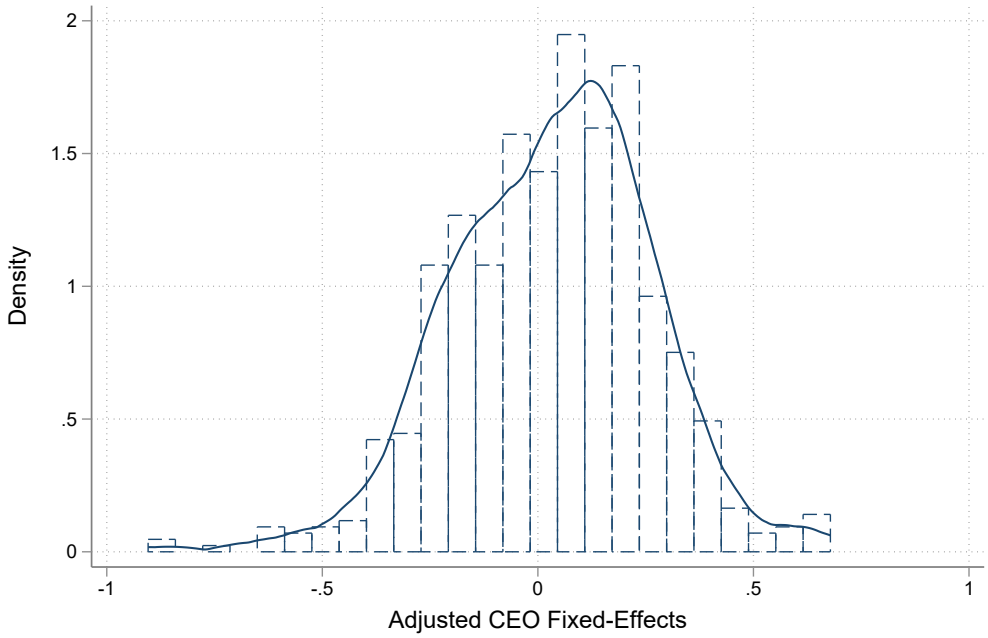
(A) Turnover of health workers



(B) Ln hourly wages

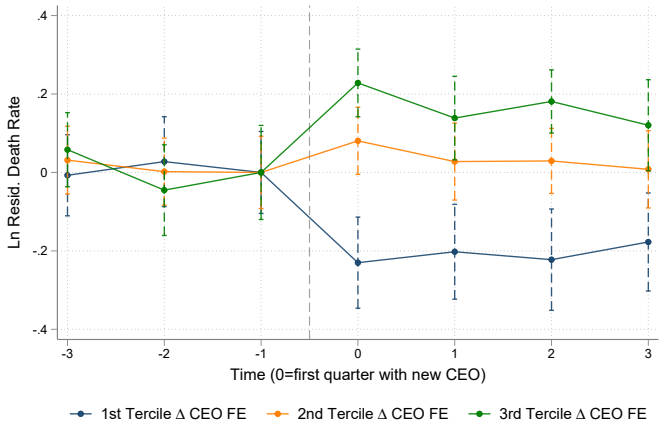
Notes: This figure presents event study evidence on the reform's effect healthcare workers' outcomes, following Equation 1. Panel A uses data at yearly level on the turnover of health personnel. Turnover is defined as the number of workers in group j who are leaving hospital h in $t+1$ (job-to-job or job-to-unemployment transitions) over the number of workers in group j working in h at time t. Nurses encompass registered nurses and licensed practical nurses. Other includes administrative and support personnel and other professionals (such as dentists, pharmacists, among others). In Panel B, hourly wages correspond to the hospital's wage bill (in real terms) divided by the number of hours on workers' contracts. Estimates are weighted by the number of workers in each category in 2011 (first year of personnel data available). Each dot corresponds to an estimated coefficient, and vertical lines indicate the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient. All regressions include hospital-by-event and time fixed effects. Standard errors are clustered at hospital level.

Figure A.10: Empirical distribution of CEO fixed-effects

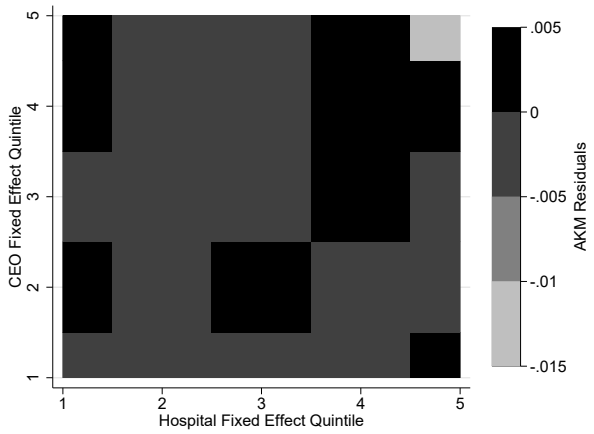


Notes: This figure plots kernel densities of the empirical distribution of CEO fixed-effects adjusted by their reliability using empirical Bayes shrinkage.

Figure A.11: Threats to the identification of managerial talent



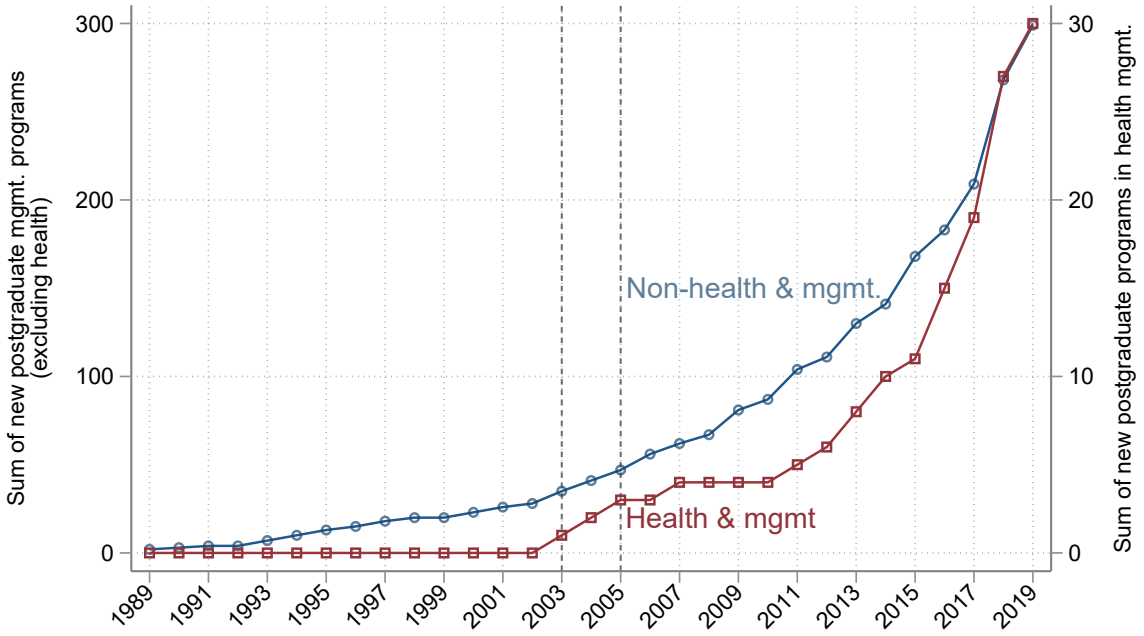
(A) Turnover by Δ fixed-effect



(B) Residuals

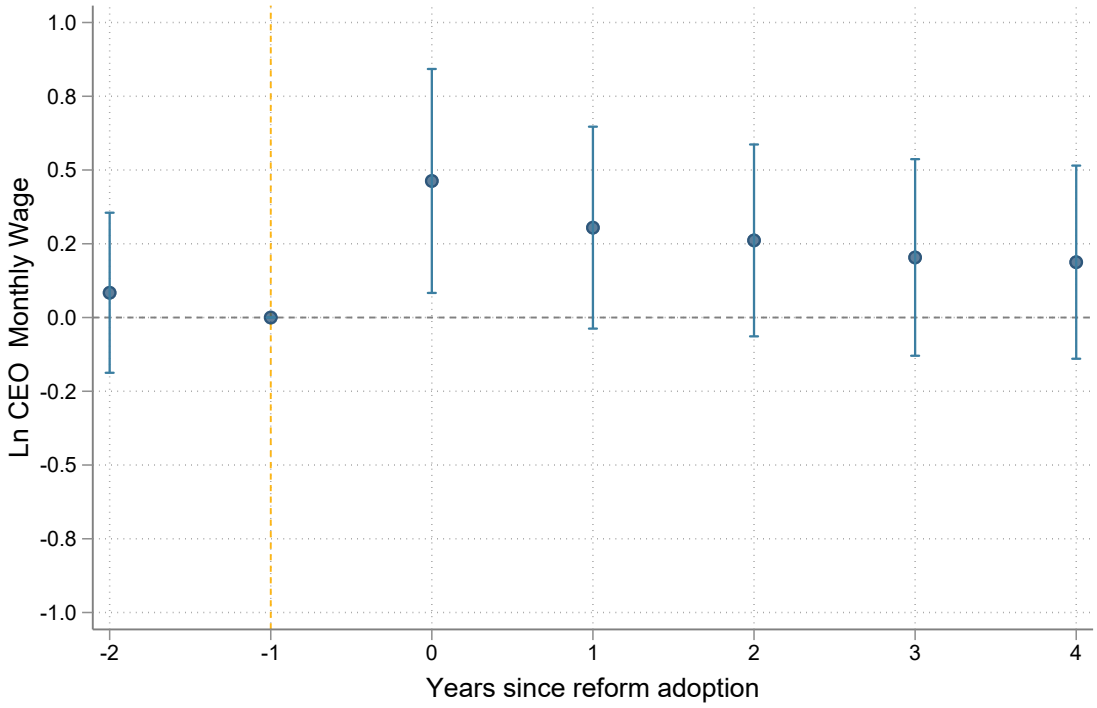
Notes: These figures assess threats to the identification of CEO fixed effects. Panel A plots three types of leadership transitions, classified by terciles of the change in managerial ability: (1) an overall increase (in green), (2) an overall decrease (in blue), and (3) no significant change (in orange). Each dot corresponds to the mean residualized logged death rate, and vertical lines indicate the corresponding 95% confidence intervals constructed using the standard errors of the means. Panel B shows mean residuals from model 3 with cells defined by quintiles of estimated manager effect interacted with quintiles of estimated hospital effect.

Figure A.12: Creation of postgraduate programs in health management



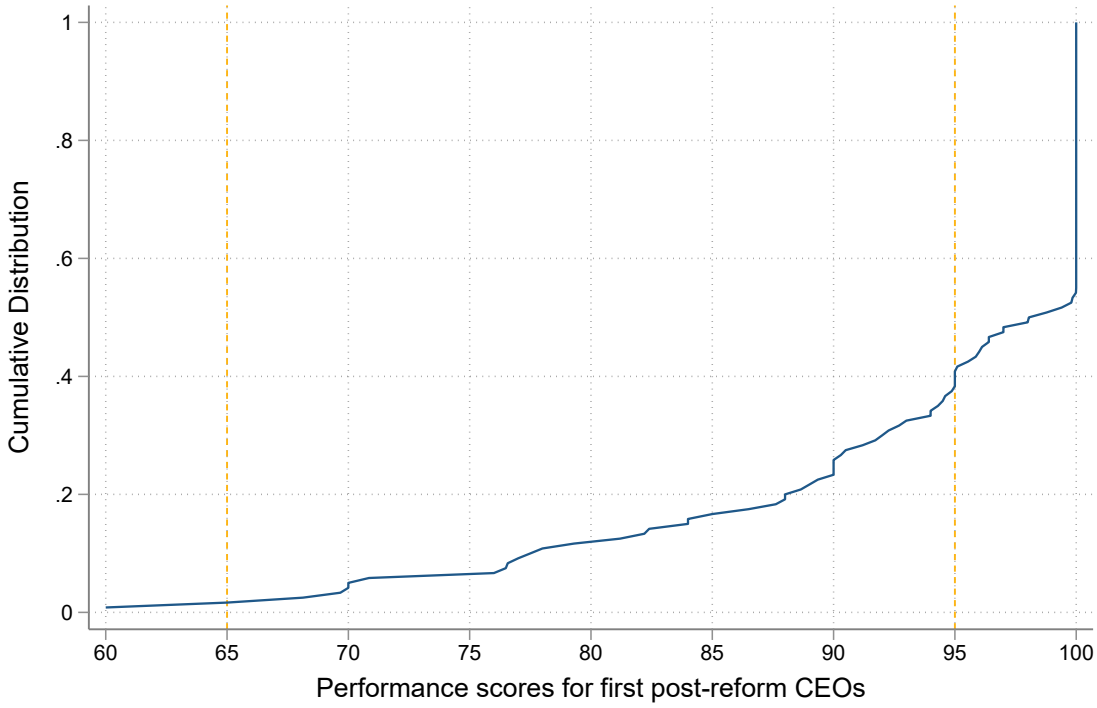
Notes: This figure shows the cumulative number of postgraduate management programs (diplomas and master's) by date of creation. Blue circles depict all management postgraduate degrees, excluding those related to health; corresponding frequencies are displayed on the left y-axis. Red squares depict new 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.13: Effect of the reform on CEOs' wages



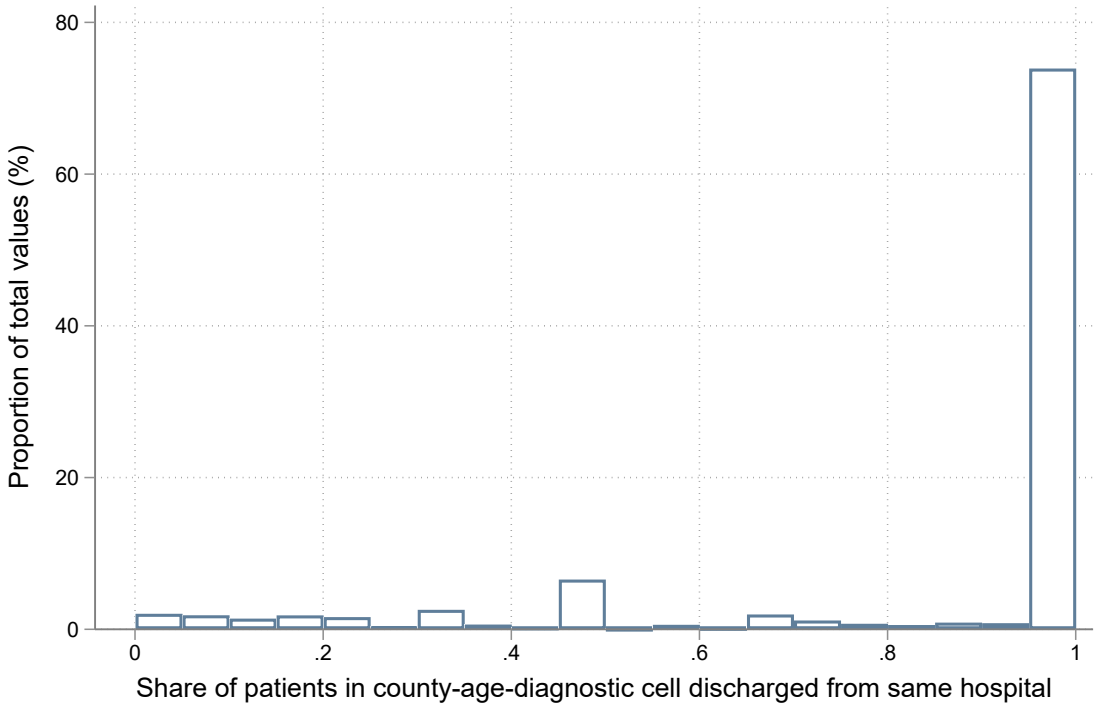
Notes: This figure presents the impact of the reform on hospital CEO wages. The empirical design leverages the gradual adoption of the selection reform across hospitals on an event study design. Regression controls include a quadratic polynomial of age and a dummy that indicates whether the individual is a doctor, which affects pay in the public sector. Dots indicate estimated coefficients, and vertical lines indicate the corresponding 95% confidence intervals. The regression includes hospital-by-event and time fixed effects. Standard errors are clustered at hospital level.

Figure A.14: Distribution of performance scores for post-reform CEOs



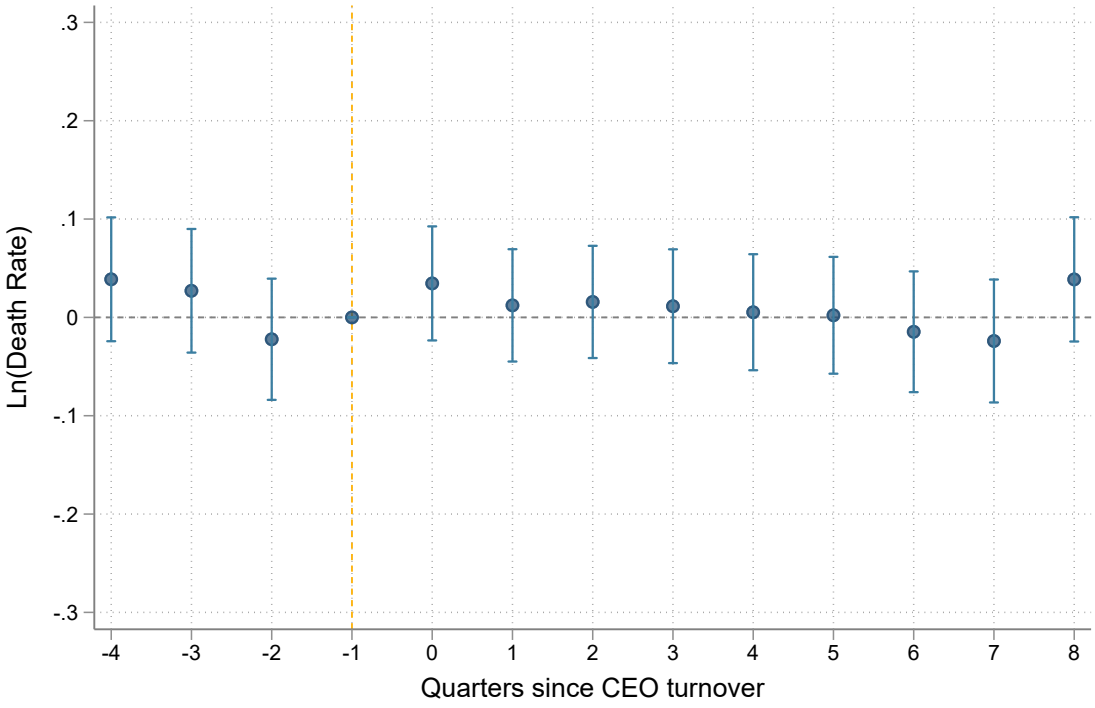
Notes: This figure displays the cumulative distribution of performance scores for the first post-reform CEOs. Before the reform, performance did not affect the wage schedule. After the reform, CEOs face wage penalties if they perform below specific performance thresholds. We accessed all available performance 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 Section 5. 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.

Figure A.15: Empirical test of patient selection



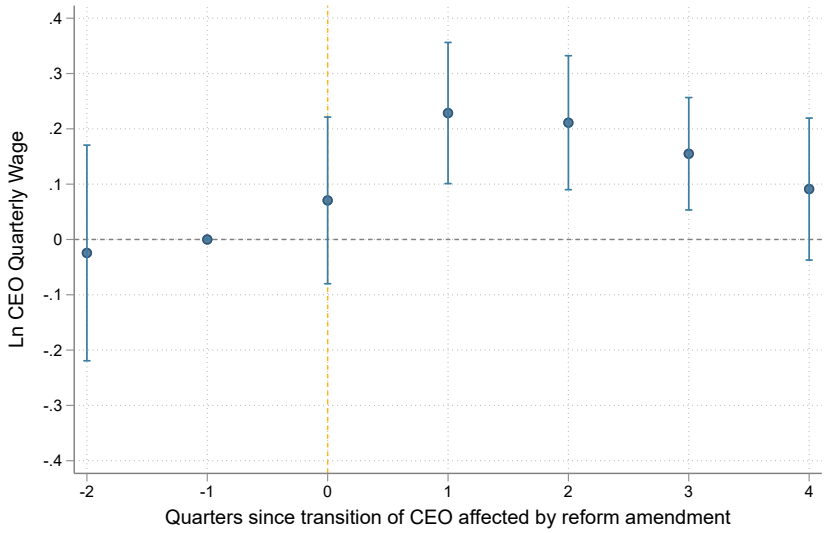
Notes: This figure plots a histogram with the share of patients in each cell who are discharged exclusively from one hospital. A cell is defined by the patient's municipality of residence, age group (less than 1 year, between 1 and 15 years, and more than 15 years), and diagnosis (as reported by the 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.16: Effect of CEO turnover on death rates

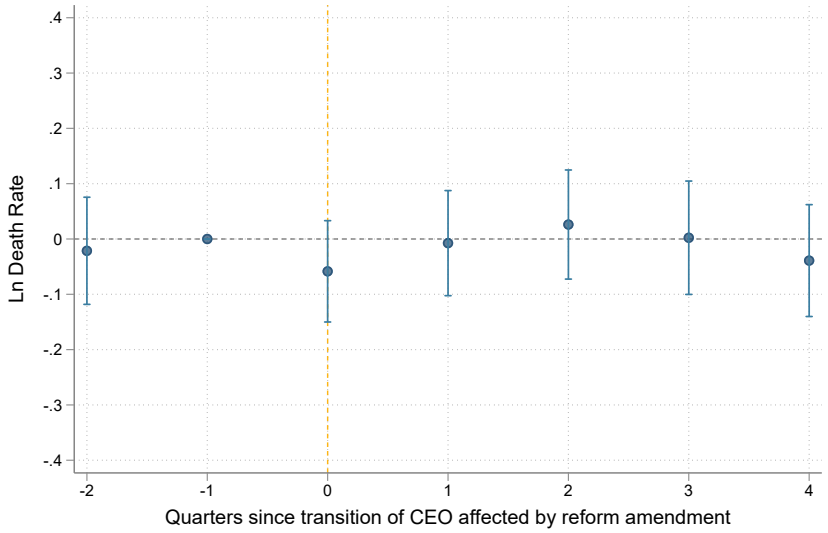


Notes: This figure presents the coefficients of the stacked event study specification in Equation A.1. An event is a CEO turnover in a hospital that never adopts the reform or in an adopter before the reform. For each turnover event, we define a time window around it and a control group of hospitals 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 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.17: 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. There are a total of 17 valid events. We append the data for all valid events e and estimate an event study following Equation A.2. 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 the corresponding 95% confidence intervals.

Table A.1: Civil service reforms

Country	Year(s)	Reform	Competitive Recruitment	Financial Incentives
Argentina	1991	Decree 993 (SINAPA)	Yes	Yes
Australia	1999	Public Service Act	Yes	Yes
Brazil	1995-1998	Civil Service Reform (PDRAE)	Yes	Yes
Canada	2003	Public Service Modernization Act (PSMA)	Yes	Yes
Chile	2003	Law 19,882	Yes	Yes
China	2006	Civil Service Law 2006	Yes	Yes
Colombia	2004	Law 909	Yes	Yes
France	1983-1986	Civil Service Reform	Yes	No
France	2007	RGPP	Yes	Yes
Germany	1997	Civil Service Law Reform	Yes	Yes
Ghana	1987-1993	Civil Service Reform Programme (CSRP)	Yes	Yes
Italy	2009	Brunetta Reform	Yes	Yes
New Zealand	1988	State Sector Act	Yes	Yes
Peru	2013	Law 30,057	Yes	Yes
Sierra Leone	2002	Civil Service Reform Programme (CSRP)	Yes	Yes
Singapore	1995	Public Service for the 21st Century	Yes	Yes
South Africa	1994	Public Service Act	Yes	Yes
South Korea	1998	Administrative Reform under NPM	Yes	Yes
Spain	2007	Law 7, on the Basic Statute of Public Employees (EBEP)	Yes	Yes
Uganda	1993-2003	Civil Service Reform Programme (CSRP)	Yes	Yes
United Kingdom	1988	Next Steps Initiative	Yes	Yes
United States	1978	Civil Service Reform Act	Yes	Yes
Uruguay	1990	Law 16,127	Yes	No
Uruguay	1996-1999	Administrative Reform of the State	Yes	Yes

Notes: This table reports civil service reforms across different countries since the late 1970s, indicating whether they included competitive recruitment and financial incentives. In some cases, the reforms established a framework that later enabled the introduction of performance pay. The table was compiled using official legislative documents, government policy reports, and relevant scholarly analyses. Details on specific countries are available upon request.

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	10,310
% Age < 29	0.34	0.15	0.36	10,310
% Age ∈ (30,29)	0.11	0.04	0.11	10,310
% Age ∈ (40,49)	0.10	0.03	0.10	10,310
% Age ∈ (50,59)	0.10	0.04	0.10	10,310
% Age ∈ (60,69)	0.11	0.04	0.11	10,310
% Age ∈ (70,79)	0.13	0.06	0.11	10,310
% Age ∈ (80,89)	0.09	0.06	0.08	10,310
% Age > 89	0.02	0.02	0.02	10,310
% Public Insurance	0.97	0.04	0.98	10,310
Hospital Characteristics:				
Total Number of Patients	1,555	2,032	611	10,310
Physicians per 100 patients	6.15	7.53	4.74	6,177
Nurses per 100 patients	5.64	6.42	4.65	6,177
Number of Deaths	41.36	63.12	15.00	10,310
Death Rate	2.88	1.96	2.48	10,310
Death Rate 28-days	4.47	2.82	3.82	10,310
Actual over Predicted Death Rate	0.91	0.46	0.87	10,310

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 2011 and 2019, which is collected by the Ministry of Health for HR purposes.

Table A.3: Robustness to zeros on the dependent variable

Panel A: Log Death Rate Regressions

	Ln(death rate)		
	(1)	(2)	(3)
1 if reform adopted	-0.082	-0.081	-0.078
	(0.025)	(0.025)	(0.025)
Sample	All obs	Exclude hospital if 4+ obs are zero	Exclude hospital if 1 obs is zero
# of Hospitals	182	172	160
Mean Dep. Variable	2.524	2.556	2.548
Observations	204,466	190,395	165,654

Panel B: Death Rate Transformations

	Asinh(death rate)	Ln(death rate + 0.1)	Ln(death rate + 0.01)
	(1)	(2)	(3)
1 if reform adopted	-0.056	-0.069	-0.071
	(0.020)	(0.024)	(0.027)
# of Hospitals	183	183	183
Mean Dep. Variable	2.409	2.409	2.409
Observations	219,741	219,741	219,741

Notes: This table presents robustness checks of the impact of the selection reform on public hospital outcomes. Estimates are from the stacked difference-in-differences specification in Equation 2. 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. For each treated hospital, we determine an event-specific control group that excludes already-treated units. In Panel A, Column (1) replicates the main result in the main text. Column (2) excludes hospitals that have 4 or more quarters with no deaths. Colum (3) includes only hospitals with a positive number of deaths in every period. Estimates in Columns (1)-(3) in Panel (B) replicate the main specification under different transformations of the dependent variable.

Table A.4: Impact of the reform on other outcomes

	Ln Readmission Rate (1-month)	Ln Median Length of Stay	Ln Infection Rate
	(1)	(2)	(3)
1 if reform adopted	0.022	-0.014	-0.005
	(0.070)	(0.020)	(0.024)
Mean of Dep. Variable	8.725	3.744	10.590
Observations	203,253	204,466	201,828

Notes: This table presents the impact of the selection reform on public hospital outcomes. Estimates are from the stacked difference-in-differences specification in Equation 2. 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. For each treated hospital, we determine an event-specific control group that excludes already-treated units. Column (1) focuses on 1-month readmission rates in any hospital for emergency care. Column (2) focuses on the median length of stay since admission. Column (3) examines complication rates, defined as the proportion of inpatients diagnosed with conditions explicitly described as infections, sepsis, hemorrhage, or other complications in their diagnostic gloss. All specifications include hospital-by-event and time-effects as well as case-mix controls. Results are weighted by the pre-policy number of inpatients and the mean dependent variable is computed for ever adopters in the quarter before adoption. Standard errors are displayed in parentheses and clustered at the hospital level.

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

	CEO Fixed Effect	
	(1)	(2)
Female	-0.117	-0.154
	(0.028)	(0.028)
Age	0.089	0.100
	(0.007)	(0.007)
Age^2	-0.001	-0.001
	(0.000)	(0.000)
Mgmt. Studies		-0.223
		(0.034)
Observations	4,887	4,887

Notes: This table presents the correlation between the CEO fixed effects estimated from Equation 3 and manager characteristics. These characteristics include gender, age, age^2, and the indicator for educational background "Mgmt. Studies", which encompasses undergraduate studies in management or postgraduate studies related to management. We consider CEOs characteristics at the end of our estimation window (12 quarters after adoption of the new recruitment system). All specifications include fixed effects for each event and connected set. Robust standard errors are in parentheses.

Table A.6: Testing financial incentive effects on the intensive margin

	Ln Death Rate		CEO Fixed-effect	
	(1)	(2)	(3)	(4)
Reform	-0.052		-0.048	
	(0.023)		(0.027)	
Reform × CEO Turnover		-0.074		-0.071
		(0.024)		(0.034)
Reform × No CEO Turnover		0.027		-0.016
		(0.039)		(0.037)
Mean of Dep. Variable	2.524	2.524	0.087	0.087
Observations	123,308	123,308	9,429	9,429

Notes: This table presents the heterogeneous effects of the reform depending on whether the incumbent manager was reappointed through the new selection process. Estimates in columns (1) and (3) replicate the main stacked difference-in-differences specification in Equation 2 but restrict the sample to 4 quarters before and 6 quarters after reform adoption. Columns (2) and (4) interact the reform indicator with whether the incumbent was observed in the pre- and post-reform periods. Columns (1) and (2) consider death rates as the dependent variable. Columns (3) and (4) take CEO fixed effects as the dependent variable. To estimate these effects, we compute period-specific CEO fixed effects by grouping managers' identities with an indicator for whether their hospital has implemented the reform. All specifications include hospital-by-event and time-effects as well as case-mix controls. Specifications (3) and (4) also include connected set indicators. Results in are weighted by the pre-policy number of inpatients and the mean dependent variable is computed for ever adopters in the quarter before adoption. Standard errors are displayed in parentheses and clustered at the hospital level.

Table A.7: 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 public primary 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.8: 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		27%	↓ 10%	ER + ≥ 65
Competition					
+1 hospital in neighborhood	Bloom et al. *ReStud* '15	In-hospital, 28-day	15%	↓ 10%	ER + AMI
	Ours		12%	↓ 11%	ER + AMI
↓ 10% HHI	Gaynor et al. *AEJ EP* '13	In-hospital, 28-day	1.6%	↓ 1%	All patients
	Ours		1.9%	↓ 5%	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 2 for the different dependent variables—reported in column (3)—and in different samples of patients reported in column (6). In the case of AMI deaths, we use a Poisson difference-in-differences design. For more details, see Appendix C. 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.

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