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Managers and Public Hospital Performance†

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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 percent. 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* D24, G34, I11, I18, J45, M54, O15)

Global government spending on publicly provided goods and services more than doubled between 1980 and 2019 and accounts for approximately 30 percent of world GDP (Gethin 2025). 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

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selection processes and objective and verifiable performance outcomes in the public sector are rare (Besley et al. 2022; Best, Fenizia, and Khan 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 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 health care 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. This procedure allows us to deal with concerns regarding treatment effect heterogeneity that might bias the estimates in settings

[1] In Supplemental Appendix Table A.1, we present a nonexhaustive 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] Health care represents almost 20 percent of government expenditures in the average OECD country. Between 2000 and 2019, health care costs increased by 15 percent as a share of GDP on average in OECD countries. In the OECD, public hospitals account for an average of 72 percent of all medical beds (see Supplemental Appendix Figure A.1).

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, Graves, and Gruber 2019).

We find that the reform reduced in-hospital death rates by approximately 8 percent in the 3 years following the adoption of the new system, from a 2.6 percent death rate average prereform. 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 nondeferrable admissions (Card, Dobkin, and Maestas 2009); and (iii) risk-adjusted mortality (Ash et al. 2012). We also estimate an event-study model using Poisson quasimaximum likelihood estimation (QMLE), with death rates in levels rather than logs (Wooldridge 2023; Chen and Roth 2024; Mullahy and Norton 2024), 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.

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

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

[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)

effects when we focus exclusively on lower-income patients who cannot easily access health care 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 US 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, Kramarz, and Margolis 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 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, Friedman, and Rockoff 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, Heining, and Kline 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 5.5 percent 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 percent 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 nondoctor CEOs with management training by more than 20 percentage points. This shift largely resulted from replacing doctor CEOs with

[5] Since the standard deviation of our empirical-Bayes adjusted CEO fixed effects is 0.22, the impact on the CEO fixed effects is -0.055 (i.e., -0.25×0.22), which implies a 5.5 percent 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 III.

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 two 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 postreform CEOs with managerial qualifications.[7] The effects are not statistically different from zero for postreform managers without managerial training, and the average effect is largely driven by postreform 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 postreform CEOs earning approximately 25 percent more than prereform 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, Weber, and Díaz 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 prereform 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

[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).

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, Rachkovski, and Boiko 2023); public R&D labs (Choudhury, Khanna, and Makridis 2019); social security claims, (Fenizia 2022); and public procurement (Best, Hjort, and Szakonyi 2023). Closest to our work, Janke, Propper, and Sadun (2024) study the impact of CEOs in NHS hospitals in the United Kingdom 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 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, He, and le Maire (2025) 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 US wartime industrial facilities and finds significant performance improvements. Relative to these papers, we focus on public sector organizations and document that postreform 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, Moreno-Serra, and Propper 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; Xu 2018; Colonnelli, Prem, and Teso 2020; Martinez-Bravo et al. 2022; Voth and Xu 2022) and personnel selection and civil service recruitment (Dal Bó, Finan, and Rossi 2013; Estrada 2019; Ashraf et al. 2020; Moreira and Pérez 2022; Dahis, Schiavon, and Scot 2025; Aneja and

[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, Propper, and Sadun 2018), which is similar to the average in our setting before selection reform adoption. The reform in Chile increased the share of CEOs with management qualifications by more than 35 percentage points the quarter 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, Sollaci, and Syverson 2023 for a discussion).

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 health care. 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 I describes the setting and data. Section II presents the main effects of the reform on hospital mortality and discusses the validity of the results and potential mechanisms. Section III examines the extent to which the identity and characteristics of managers matter for performance. Section IV examines the effects of the financial incentives included in the reform, and Section V concludes.

I. Setting, Data, and Descriptive Evidence

A. *The Health Care System in Chile*

Chile's health care 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 percent of the population is covered by a public health scheme, 15 percent 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 health care provider within the public network.[12] Individuals must register with the health care 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 2004). In Supplemental 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 health care 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 health care establishments in

[11] This is similar, for example, to the public health care 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 health care providers. In the universe of admissions, 97 percent 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 percent of inpatients in private hospitals have this coverage.

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 of the hospital into the health network and the community.

B. *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 (HR) 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. Supplemental 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 fixed monthly supplement.[14] In the case of public hospital CEOs, we document that the reform increased the position's pay around 25 percent. The financial package also includes a small performance pay component, under which the yearly wage

[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 wage supplement. First, it cannot be larger than 100 percent of the base wage. Second, the total wage cannot be higher than that of the undersecretary of the ministry in which the position is based.

is slightly reduced if the manager does not meet certain performance thresholds. In Section IV, we provide further details on the changes to the pay schedule and argue that this incentive 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 IIA. 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 Supplemental 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 2004, 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.

C. *Data*

For this paper, we build a novel dataset that identifies the CEO in every public hospital in the country, spanning every month between January 2005 and December 2019. Because these data were not available in a systematic way, we filed several hundred Freedom of Information Act (FOIA) requests to local hospitals and health authorities—who, in many cases, had to collect archived data. We complement these data with background and performance records. For background characteristics, we collect date of birth, gender, and educational attainment. We gather this information from several sources, including a national registry of all medical personnel in the country, curriculum vitae requested by the Civil Service, LinkedIn

[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 HR 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.

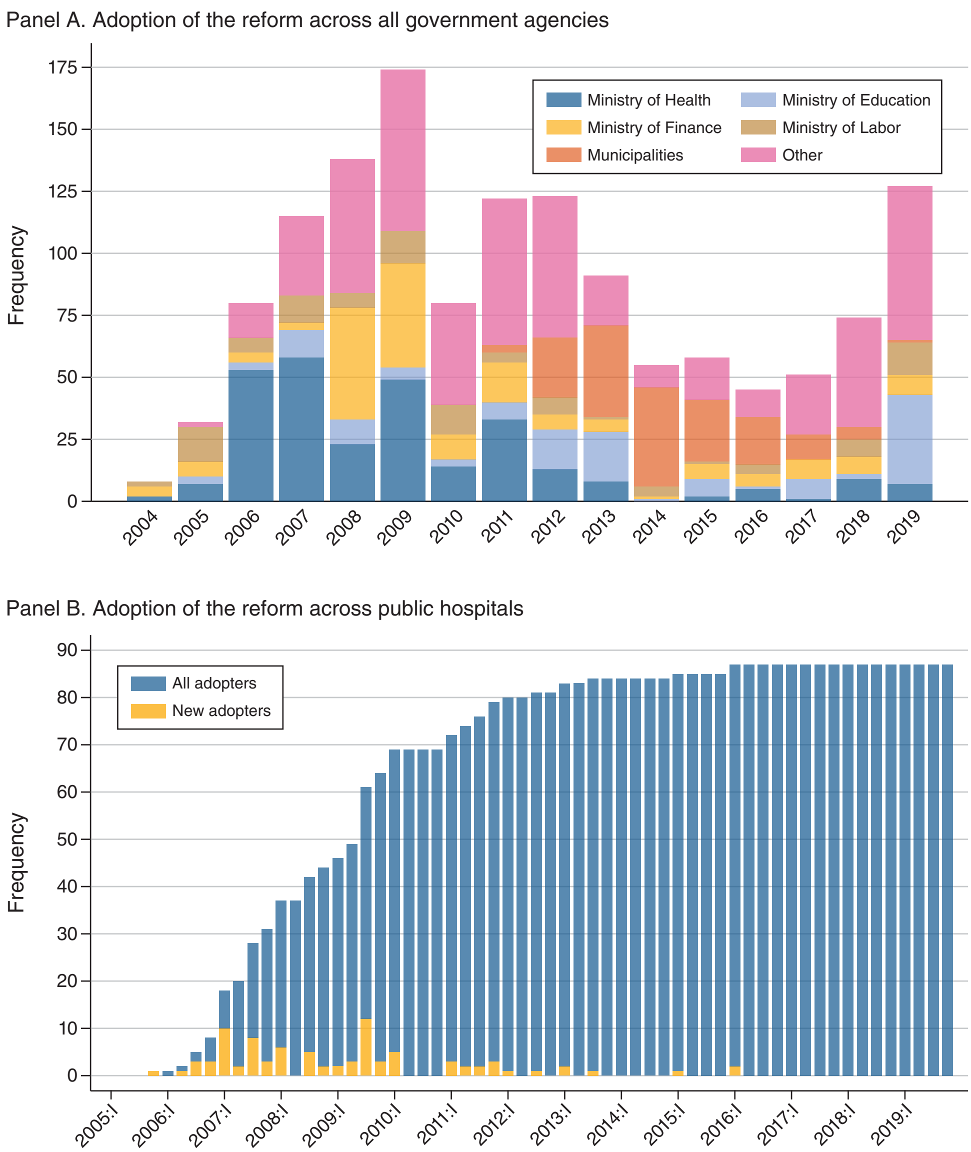

FIGURE 1. ADOPTION OF THE NEW RECRUITMENT PROCESS

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.

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 postreform 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 (ICD-10); 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 (DEIS 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 2022). 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, Gowrisankaran, and Town 2003; Gaynor, Moreno-Serra, and Propper 2013; Bloom et al. 2015; Doyle et al. 2015; Chandra et al. 2016; Hull 2020; Gupta 2021; Chan, Card, and Taylor 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 Supplemental Appendix A.

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, Moreno-Serra, and Propper 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

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

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 nondeferrable diagnoses, who are more likely to need urgent medical attention (Card, Dobkin, and Maestas 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 the 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 10,326 hospital-by-quarter observations. Supplemental Appendix Table A.2 presents descriptive statistics of this sample. The average hospital in our sample discharges 1,546 patients per quarter, while the median hospital discharges 606 patients. On average, 59 percent of these discharges correspond to female inpatients and 34 percent to inpatients younger than 29 years old. 97 percent of patients discharged from public hospitals have public insurance. Regarding hospital outcomes, the average hospital experiences 42 deaths per quarter, with a corresponding in-hospital death rate of 2.91 percent. The 28-day death rate—which considers both in- and out-of-hospital deaths—is larger and corresponds to 4.67 percent.

II. 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.

A. *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 nonadopters at that point in time. For each treated hospital, we define a control group that excludes already treated units. We consider a set of variables related to inpatient characteristics, hospital outcomes, and political outcomes at the municipality level where the hospital

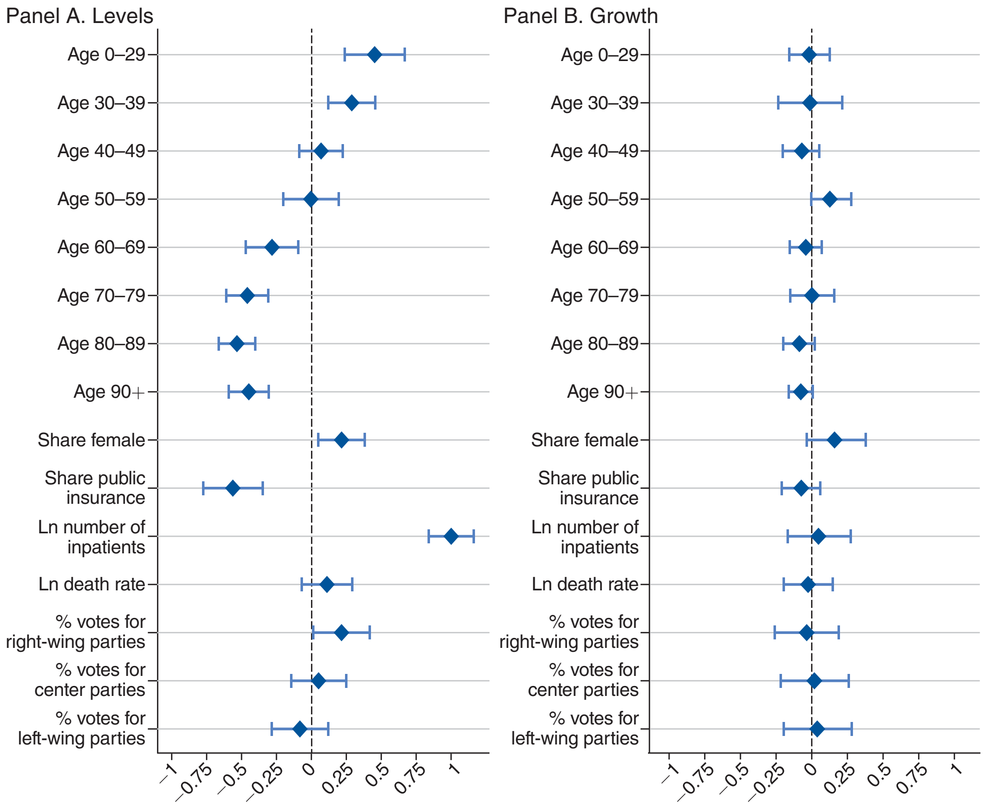

Figure 2. Balance in Observable Characteristics

Notes: This figure studies differences between treated units and their control group in an array of observable characteristics, prior to adoption. For each treated hospital, we define a control group that excludes already treated units. 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 preadoption 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.

is located. To focus on the preadoption period, we consider the quarter prior to each hospital's reform adoption.

In panel A of Figure 2, we present OLS estimates of a dummy variable that takes a value of 1 for adopters and 0 for other hospitals in the control group. The regression includes fixed effects for each treated unit and its event-specific control group, so that the contrast is between each treated hospital and its control group. We also standardize each variable for ease of comparison. We find that adopters have a larger number of inpatients, consistent with 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 Supplemental 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.[17] 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 coefficients from the same regressions as above, but using the difference in each characteristic between the quarter before reform adoption and one year prior. 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 Supplemental 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):

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

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 denote time fixed effects that capture unobservable time shocks. D_{hte}^{τ} 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).[18] 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

[17] Support for right- and left-wing candidates is measured using official municipality-level electoral records (SERVEL 2017).

[18] 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.

Reenen 2010; Gaynor, Moreno-Serra, and Propper 2013; Doyle et al. 2015). 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, we also include the share of inpatients for each of six categories of health insurance.[19] 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.

B. *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 percent. These effects are statistically significant and economically meaningful. An 8 percent effect implies a decrease in the death rate toward 2.37, over a sample mean of 2.58 deaths per 100 patients (i.e., 2 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 prereform 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 Supplemental 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. An additional concern with our main specification is that using logged death rates as the outcome mechanically excludes observations in which hospitals report zero deaths in a given quarter.[20] As a robustness check, we instead compute the outcome in levels and estimate the model using Poisson QMLE, which accommodates zeros. This approach is recommended by Chen and Roth (2024) and Mullahy and Norton (2024) as an alternative to log-like transformations[21] and recovers a consistent estimate of the average proportional treatment effect on the treated in settings with staggered adoption (Wooldridge 2023). Panel B of Supplemental Appendix Figure A.5 presents

[19] 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.

[20] In our hospital-by-quarter data, around 6 percent of observations are 0, primarily because small hospitals occasionally report no deaths due to idiosyncratic variation.

[21] Chen and Roth (2024) and Mullahy and Norton (2024) show that under log-like transformations, estimated treatment effects can be sensitive to the scaling of the dependent variable when the extensive margin effect is nonnegligible.

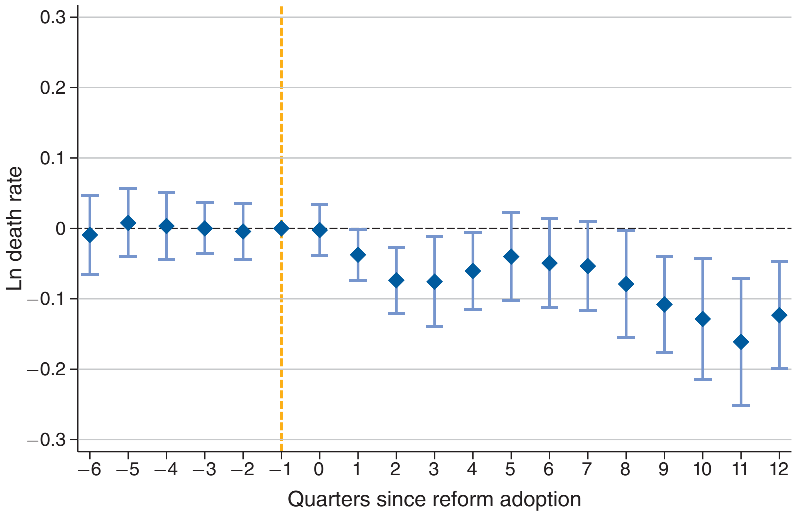

FIGURE 3. DYNAMIC EFFECTS OF THE REFORM ON HOSPITAL QUALITY

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 marker corresponds to an estimated coefficient, and vertical lines indicate the corresponding 95 percent confidence intervals. Estimates are weighted by the prepolicy number of inpatients. The dashed yellow line represents the omitted coefficient. Standard errors are clustered at hospital level.

event-study estimates from this model, which closely mirror our main results.[22] Finally, in Supplemental Appendix B, we 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-difference 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.[23] In Table 1, we report the results. Column 1 shows that reform adoption led to an average 8.1 percent 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 5.4 percent after reform adoption, from a sample mean of 3.87 to 3.66 deaths per 100 patients.

[22] In Supplemental Appendix Table A.3, we also show that the extensive margin treatment effect is negligible and that restricting the sample to hospitals with strictly positive deaths yields virtually identical results.

[23] 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.

Table 1—Impact of the Reform on Death Rates

	Ln death rate			
	In-hospital (1)	28 days (2)	Nondeferrable (3)	Emergency (4)
1 if reform adopted	−0.081	−0.054	−0.070	−0.090
	(0.024)	(0.020)	(0.025)	(0.032)
Mean dependent variable	2.58	3.87	4.81	3.15
Observations	205,453	205,453	193,848	185,279

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-difference 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 nondeferrable and emergency admissions. Results are weighted by the prepolicy number of inpatients. The mean dependent variable is computed for ever adopters in the quarter before adoption and is presented in levels instead of logs. Standard errors are displayed in parentheses and clustered at hospital level.

We next focus on patients for whom immediate medical attention is critical and selection is unlikely: emergent patients and patients with nondeferrable conditions.[24] Column 3 shows that reform adoption led to a 7 percent decrease in the death rate of patients with nondeferrable diagnoses, while column 4 shows that the reform led to a 9 percent decrease in the death rate among emergency admissions. In Supplemental 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 infection rates (the share of inpatients with diagnoses related to infections, hemorrhage, or other complications). As shown in Supplemental 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.[25]

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

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

[25] Supplemental Appendix Figure A.6 presents event-study evidence for each outcome.

severity of their illness.[26] 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.[27] 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 prereform 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 United Kingdom's NHS (NHS Digital 2016), is defined as the ratio between the actual hospital-level death rate and the predicted death rate—that is, an increase from one indicates more deaths than predicted and a decrease from one 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 percent, which implies that hospitals' death rates decreased by more than what can be predicted based on their patients' case mix.[28] This is reassuring, because—according to recent research that leverages quasirandom variation in death rates—risk-adjusted mortality measures are reliable and valid indicators of hospital quality in the United States, where the institutional setting is more prone to patient selection (Doyle, Graves, and Gruber 2019). Finally, column 4 in Table 2 presents difference-in-difference estimates of the impact of the reform on log aggregate mortality rates predicted based on patients' risk scores.[29] 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 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.

[26] 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).

[27] 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 counterreferral guidelines.

[28] In Supplemental Appendix Figure A.7, 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.

[29] In Supplemental Appendix Figure A.7, panel B, we present the corresponding event-study evidence.

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	−0.084	−0.085	−0.085	0.010
	(0.023)	(0.023)	(0.024)	(0.014)
Mean dependent variable	0.98	0.98	0.98	0.64
Observations	205,453	205,453	205,453	205,453
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 (prereform) 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-difference 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 prepolicy number of inpatients. The mean dependent variable is computed for ever adopters in the quarter before adoption and is presented in levels instead of logs. Standard errors are displayed in parentheses and clustered at hospital level.

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 Supplemental Appendix Figure A.8 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 health care in public hospitals; some cannot buy private health care 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 in Supplemental Appendix A.[30] For this analysis, we estimate equation (1) using smaller samples consisting of low socioeconomic status 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 Supplemental Appendix Figure A.9. 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

[30] Cases not classified as strict compliers do not necessarily imply noncompliance with the established referral and counterreferral guidelines; this could be due to data limitations. For details, see Supplemental Appendix A.

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, Moreira, and Trucco 2022) and emphasizes staff retention as a key challenge, particularly in public health care 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 health care personnel collected by the Ministry of Health for HR purposes (SIRH 2019).[31] 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 Supplemental Appendix Figure A.10 shows that the reform reduced the turnover of doctors 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.

III. 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.

A. *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 0.70 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.

[31] 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.

Table 3—Explanatory Power of Managerial Talent to Account for Hospital Performance

	Ln death rate			
	(1)	(2)	(3)	(4)
R^2	0.46	0.71	0.78	0.78
Adjusted R^2	0.46	0.70	0.75	0.75
Observations	10,326	10,326	10,216	10,216
Hospital FE	No	Yes	Yes	No
Manager FE	No	No	Yes	No
Hospital-manager FE	No	No	No	Yes
F-statistic for manager FEs	–	–	6.95	–
F-statistic for hospital manager FEs	–	–	–	10.66

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.

Motivated by this finding, we compute individual measures of CEO talent following the approach pioneered by Abowd, Kramarz, and Margolis (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

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

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, Kramarz, and Margolis 1999; Card, Heining, and Kline 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). Supplemental Appendix Figure A.11 shows the distribution of the adjusted CEO fixed effect estimates, with a standard deviation of 0.22.

To validate the use of CEO fixed effects as a proxy for CEO quality, we implement the quasi-experimental methodology developed in Chetty, Friedman, and

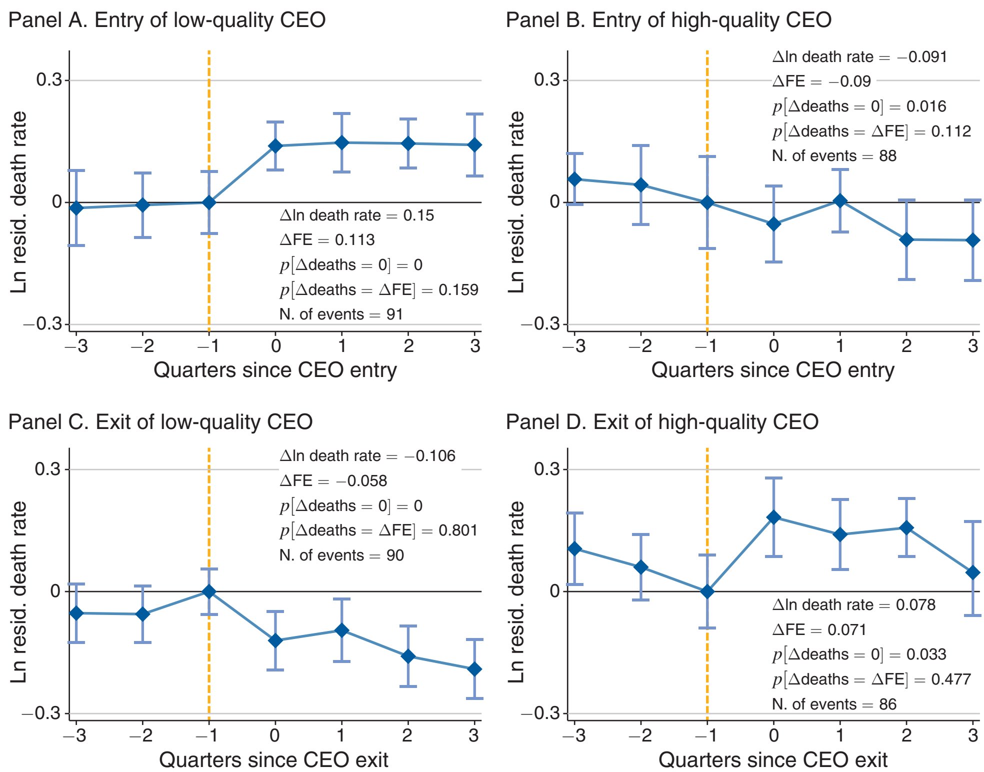

Figure 4. Impact of CEOs Entry and Exit on Mortality

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 marker corresponds to the mean residualized logged death rate, and vertical lines indicate the corresponding 95 percent confidence intervals constructed using the standard errors of the means.

Rockoff (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—that is, 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, Heining, and Kline (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 Supplemental Appendix Figure A.12 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 quintiles 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.[32] Panel B of Supplemental Appendix Figure A.12 presents this results.

B. *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.

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 0.22, the impact on CEO fixed effects is -0.055 (i.e., -0.25×0.22), which implies a 5.5 percent decrease in death rates.[33]

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 nonmedical professionals from being selected as CEOs, in 2004—the year before the first hospital adopted the selection reform—99 percent 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 percent. The de facto exclusion of individuals with nonmedical 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

[32] 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.

[33] 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.

TABLE 4—EFFECT OF THE REFORM ON MANAGERS' SKILLS AND DEMOGRAPHICS

	CEO fixed effect (1)	Has management studies (2)	Age (3)	Female (4)
1 if reform adopted	−0.25	0.36	−1.80	−0.03
	(0.08)	(0.05)	(1.07)	(0.05)
Mean dep. variable	0.32	0.28	49.94	0.23
Observations	113,212	203,807	200,840	203,807

Notes: This table presents the impact of the selection reform on public hospital CEOs' skills and demographics. Estimates are from the stacked difference-in-difference 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 prepolicy 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.

organizational performance (Huber, Lindenthal, and Waldinger 2021) and, more broadly, that talent misallocation reduces aggregate output (Hsieh et al. 2019).

Figure 5 presents the results of stacked difference-in-difference regressions, as specified in equation (2), on various variables that capture the educational background of hospital CEOs.[34] 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 percent 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 percent, while substantially decreasing the number of doctor CEOs without management training by 30 percentage points.

Column 2 in Table 4 summarizes the impact of the policy on the CEO's management training status—regardless of whether the CEO is a doctor. 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

[34] 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 United States

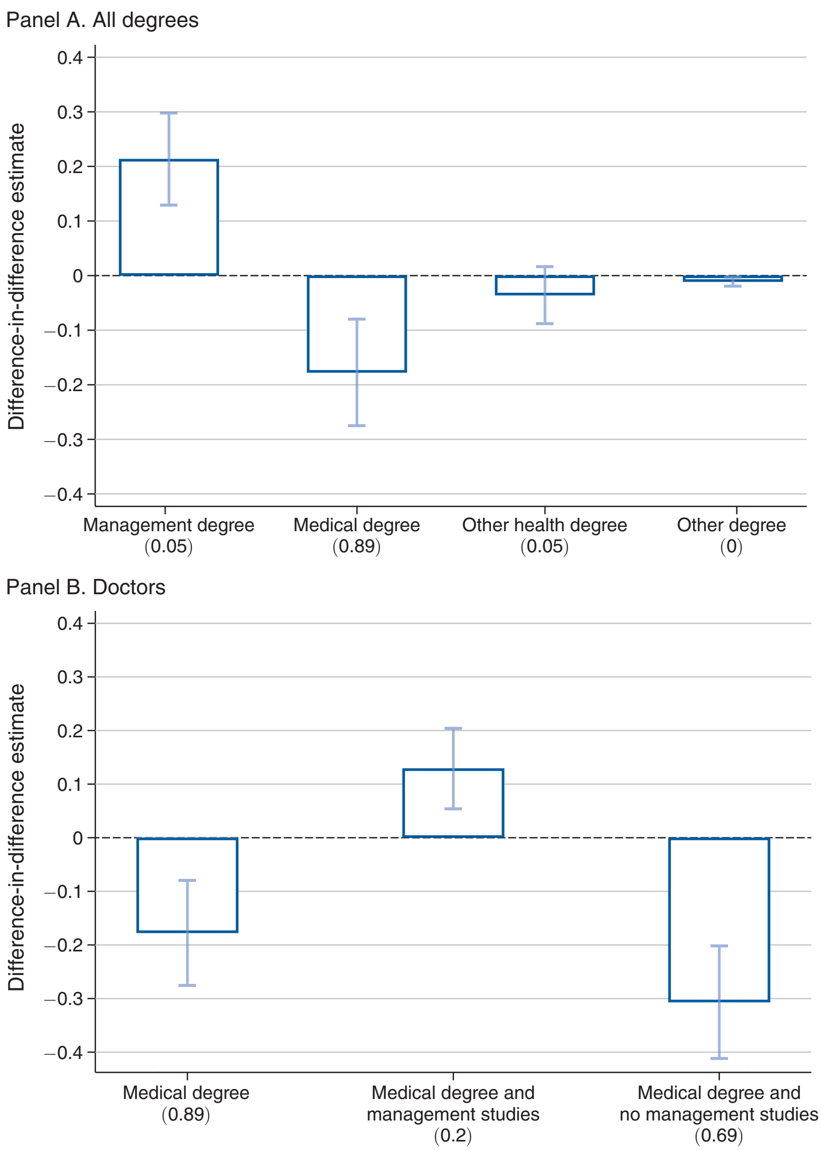

Figure 5. The Policy Displaced Doctor CEOs with No Management Training

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 percent confidence intervals. Standard errors are clustered at hospital level.

management training being appointed to CEO positions after the reform.[35] The average across ever-adopters the period prior adoption was 28 percent. As a point of comparison, in NHS hospitals, Janke, Propper, and Sadun (2018) report that 26 percent 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.

Finally, columns 3 and 4 of Table 4 focus on managers' demographics. We find that managers are almost two 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 prepolicy share of female CEOs was less than 25 percent.

C. *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, postreform, 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 percent 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 12 percent, 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 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.[36] Second, it is the characteristic that most drastically changed postreform. Third, the prereform 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

[35] This finding is consistent with the surge of health management postgraduate degrees in the country. Supplemental Appendix Figure A.13, constructed from official records on tertiary enrollment (CNED 2022), shows that the opening of the first health management postgraduate programs coincides with the timing of the reform. In contrast, 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.latercera.com/noticia/mba-en-salud-para-que-medicos-chilenos-entren-al-mundo-del-management/.

[36] Supplemental 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 0.28 standard deviations below that of CEOs without management studies. Also, we find a negative association between CEOs' age and talent; similar to findings in Fenizia (2022), we observe that female CEOs are associated with higher levels of managerial talent.

Table 5—Heterogeneity in CEO Performance by Manager Characteristics

	Ln death rate				
	(1)	(2)	(3)	(4)	(5)
Reform (1 if reform adopted)	−0.065 (0.033)		−0.069 (0.022)		
Reform × high-quality CEO		−0.124 (0.053)			
Reform × low-quality CEO		−0.038 (0.032)			
Reform × CEO w/ mgmt. studies				−0.092 (0.026)	
Reform × CEO w/o mgmt. studies				−0.029 (0.022)	−0.029 (0.022)
Reform × nondoctor CEO w/ mgmt. studies					−0.085 (0.033)
Reform × doctor CEO w/ mgmt. studies					−0.096 (0.032)
Mean dependent variable	2.30	2.30	2.58	2.58	2.58
Observations	113,212	113,212	203,807	203,807	203,807
p-value high-quality CEO = low-quality CEO	–	0.082	–	–	–
p-value w/ mgmt. studies = w/o mgmt. studies	–	–	–	0.012	–
p-value nondoctor w/ mgmt. = doctor w/ mgmt.	–	–	–	–	0.792

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 prepolicy 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 and is presented in levels instead of logs. Standard errors are displayed in parentheses and clustered at hospital level.

replacing doctor CEOs with professionals holding management degrees and encouraging doctors who aspired to hospital CEO roles to invest in management education.[37]

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 around 9 percent, while the effect in hospitals with postreform CEOs without management background is not statistically different from zero. Finally, in column 5, we compute

[37] 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, Persson, and Rooth 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).

the differential effects between CEOs with no management training, doctor CEOs with management training, and nondoctor 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 nondoctor 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, He, and le Maire (2025), who show that managers with a business degree do not improve firm performance and reduce employees' wages by means of rent-sharing practices.[38] 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, Olken, and Pande 2017; Ashraf and Bandiera 2018).

Why Only Doctor CEOs Prereform?.—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 nonmedical CEO, 'You don't understand this, you can't comment,' and that's it" (Servicio Civil 2014, p. 11).[39]

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.[40]

IV. 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

[38] Panel B of Supplemental Appendix Figure A.10 shows that the reform did not impact the hospital wages of employees other than the CEO.

[39] 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.

[40] 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.

Ministry of Finance. We document the reform wage increase relative to prereform pay in Supplemental Appendix Figure A.14, 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 percent increase in pay relative to the period before the reform.[41]

Performance Pay.—In our setting, the head of the health service jointly drafts a performance contract with the hospital CEO for a three-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 percent or higher, the yearly wage remains at 100 percent. If the performance is between 65 percent and 95 percent, the yearly wage is reduced by 1.5 percent. If the performance is below 65 percent, the yearly wage is reduced by 7 percent. Two points are worth noting about this schedule: First, the first-year wage remains unaffected since it is based on the previous year's performance, with penalties applying only in years two and three. Second, the incentive structure introduces a modest penalty, with no opportunity for a wage increase, and a maximum penalty of only 7 percent of the yearly wage for very poor performers.

A. *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, Finan, and Rossi (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 health care 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.

[41] 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.

Intensive Margin.—Theoretically, both performance-based incentives and higher base wages can drive CEOs to exert greater effort and achieve better results.[42] 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 postreform 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 postreform managers.[43] Supplemental Appendix Figure A.15 shows the cumulative distribution of performance scores for the first postreform CEO in each adopting hospital. Around 70 percent of scores are at or above the 95 percent threshold—and thus avoid wage penalties—and most of the remainder falls between 95 percent and 65 percent, for which CEOs only face a 1.5 percent wage penalty. Almost no CEO scores below 65 percent, which would imply a 7 percent 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 percent of government employees under the recruitment system scored below 80 percent on their performance evaluations (CPPUC 2013), and by 2016, over 90 percent 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, Weber, and Díaz 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.[44] Within this window, 13 out of 88 CEOs remain the same. Column 1 in Supplemental 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

[42] Empirically, recent studies show that financial incentives can boost employee performance in the public sector (Muralidharan and Sundararaman 2011; Khan, Khwaja, and Olken 2015; Burgess et al. 2017; Deserranno et al. 2025), including top managers in public hospitals (Lippi Bruni and Verzulli 2022).

[43] Some of the oldest contracts and performance scores were lost, and the Civil Service has no available records. We have performance score data for 57 postreform CEOs.

[44] There is a trade-off between the length of the time window and the number of managers who remain in place throughout. We use four pre- and six postreform quarters, which provide a reasonable number of stable manager spells and still allow enough pre- and postreform periods to analyze the effect of the reform. The results are robust to alternative window lengths.

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 Supplemental 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 Supplemental Appendix D, we leverage a 2016 amendment to the reform that increased the pay by around 15 percent 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 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.

V. 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 percent. 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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