ONLINE APPENDIX

Managers and Public Hospital Performance

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A Description of the Referral and Counter-Referral System

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

Table A.7 shows an example of referral guidelines from different primary care centers to public hospitals in two Health Services. Primary care centers in columns (1)-(2) and (3)-(4) are in two different Health Services: Metropolitano Norte and Metropolitano Oriente, respectively. The numbers in the table are the hospitals to which patients are referred. The example shows how referrals vary depending on the primary care center and the patient's diagnosis and demographics. For example, a medical oncology patient older than 15 in CESFAM Colina is referred to "Instituto Nacional del Cáncer Dr. Caupolicán Pardo Correa."

To empirically assess compliance with the referral guidelines, we focus on a sample of patients with public insurance who were discharged (dead or alive) at any point during the year 2005 and who were not admitted to the hospital via ER. In this sample, we classify patients into cells defined by the patient's municipality of residence, age group (less than 1, between 1 and 15, and more than 15), and diagnosis (as reported by the hospital from which they are discharged). If we observed the catchment areas and guidelines were strictly followed, all patients within a cell should attend the same hospital. To visually evaluate this, Figure A.16 plots a histogram with the share of patients in each cell who are discharged exclusively from one hospital; around 75% of patients within a cell are discharged from the same hospital. The fact that patients within a cell are being discharged from different hospitals is likely explained by the fact that we only observe the patient's municipality of residence, and catchment areas do not map one-to-one with municipalities, or by the fact that patients can also use their work address to register with the health system.

B CEO Turnovers Do Not Have an Automatic Impact on Performance

A potential explanation for the effects of the reform could be attributed to mechanical or Hawthorne effects, which might occur as a result of the appointment of a new CEO under the reform. A direct impact on death rates could arise from the CEO appointment itself. To explore this mechanism, we leverage CEO turnovers in never-treated or yet-to-be-treated hospitals in any quarter between

2005 and 2019.

To deal with multiple events and the lack of clean controls, we perform a stacked event study in which for each turnover event, we define a time window around it and a control group of hospitals with no turnovers in the time window.[1] Next, we define a set of valid events as those that do not overlap with another turnover in the pre-period within the time window. Finally, we append the data for all valid events and estimate the following stacked event study regression:

$$y_{hte} = \alpha_{he} + \gamma_t + \sum_{\tau=-4}^{8} \beta_\tau D_{hte}^{\tau} + \epsilon_{hte}, \tag{A.1}$$

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

Appendix Figure A.17 presents the effect of a CEO turnover on death rates on never-treated and yet-to-be-treated hospitals. The effect is a precisely estimated zero and confirms that a CEO turnover before the reform has no significant effect on hospital quality. This evidence suggests that the impacts of the recruitment reform reported so far are not explained by a mechanical effect driven by the CEO appointment itself.

C CEO Selection Reform in the Context of Other Policies

In this Appendix, we benchmark our results to the effects of other policies studied in the literature. One of the advantages of our data is that we can check the impact of the policy on different samples of patients, which allows us to match some of the characteristics in the sample with those of patients studied elsewhere. For each comparison, we present the average death rate in different samples used in the literature and our sample after we match them according to patients' characteristics. Note, however, that although we can match the sample of patients in some dimensions, such as age bracket and type of admission, patient composition will still differ across settings. Comparisons should thus serve as a benchmark and not as a horse race competition between policies. The results are summarized in Table A.8.

We first compare the effect of the CEO selection reform with the impact of increasing health spending. **?** examine the effect in the U.S. of receiving higher payments from Medicare. They find that a 10% increase in Medicare reimbursement per capita decreases death rates by 6%. Their sample of patients includes emergency admissions arriving by ambulance, over 65 years old, and with non-deferrable medical conditions. Since we do not have data on whether a patient arrives by ambulance, we only compute the effect of our policy on the sample of patients over 65 admitted

[1] Note that there is a trade-off between the length of the window and the number of events and controls. We use 4 quarters prior to the turnover and 8 quarters after the turnover, although the results are robust to other time windows.

via the ER. We find that the reform we study has a slightly larger effect (11%) over a slightly lower average death rate in the sample.

Second, we focus on policies related to the impact of increasing competition in the health sector. **?** examine the effect of adding competition between health providers in the UK. They find that adding one extra hospital in the neighborhood decreases the in-hospital 28-day death rate by 10% following emergency admissions for AMI. The policy we study in this paper finds a similar effect over a very similar death rate in the same sample group (emergency admissions for AMI). Previous work by **?** also reports that increasing competition by 10%, as measured by a decrease in the Herfindahl-Hirschman Index (HHI), reduces the 28-day in-hospital death rate by 1%. In this regard, improving CEO selection has a larger effect over a slightly larger sample mean.

D Additional Test on Intensive Margin Effects of Financial Incentives

As an additional test to examine the potential effects of higher wages on CEO performance on the intensive margin, we exploit a 2016 amendment to the recruitment reform (**?**). Before the amendment, all CEOs were paid according to the public employees' pay grade, regardless of their profession. After the modification, CEOs appointed after November 2016 can choose to be paid according to the medical pay laws instead of the public employees' pay grade, but *only if* they are medical doctors.[2] The medical pay law is more generous than the public employees' pay law. Therefore, the amendment implied an increase in remuneration for doctor CEOs but not for CEOs with other educational backgrounds.

If the efficiency wage hypothesis is at play in this setting, we should expect that a wage increase is followed by an improvement in performance in hospitals in which new managers are doctors *and* receive a pay boost. To answer this question, we perform a stacked event study in which an event is a turnover after November 2016 that uses the new selection system and the incoming CEO is a doctor. For each event, we define a time window around the turnover and determine an event-specific control group that excludes hospitals that experienced an event as described above, append the data for each event, and estimate the following stacked event study regression:

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

where e is an event. An observation is at hospital-by-time-by-event level and includes hospital-by-

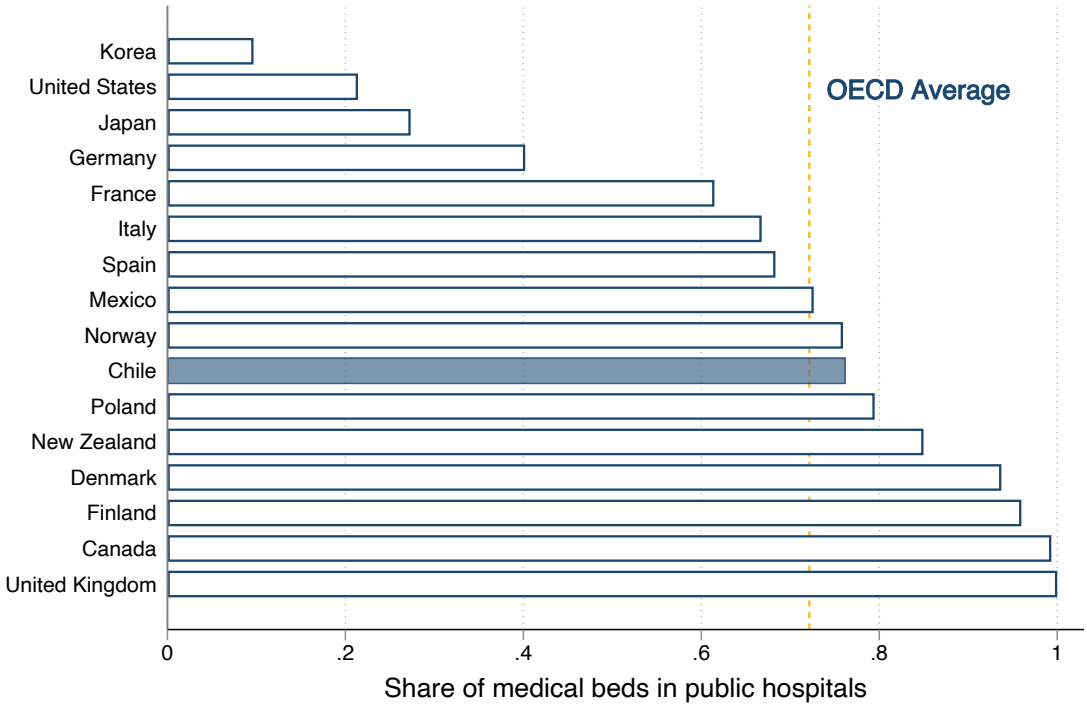
[2]More precisely, doctors can choose to be paid according to Law 19,664 instead of Law 18,834.

event and time fixed effects. We cluster standard errors at hospital level.[3]

Panels A and B in Appendix Figure A.18 present the impact of the 2016 amendment on doctor CEO wages and hospital performance, respectively. As expected, the change in the regulation increased wages for incoming doctor CEOs. The effect is an approximately 15% quarterly wage increase. However, we do not observe any effect on death rates. In other words, the wage increase was not followed by an improvement in CEO performance. This finding provides further evidence that the efficiency wage hypothesis is unlikely to play a substantial role in this context.

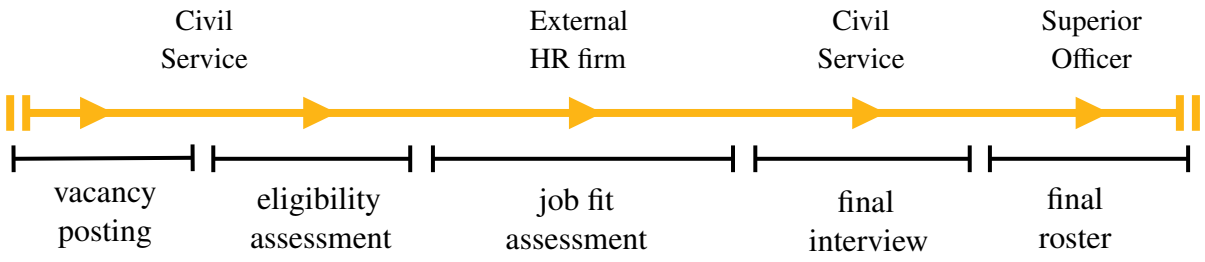
[3]There is a trade-off between the length of the window and the number of events. We consider 2 periods before treatment and 4 periods post-treatment, for which we have 17 events.

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



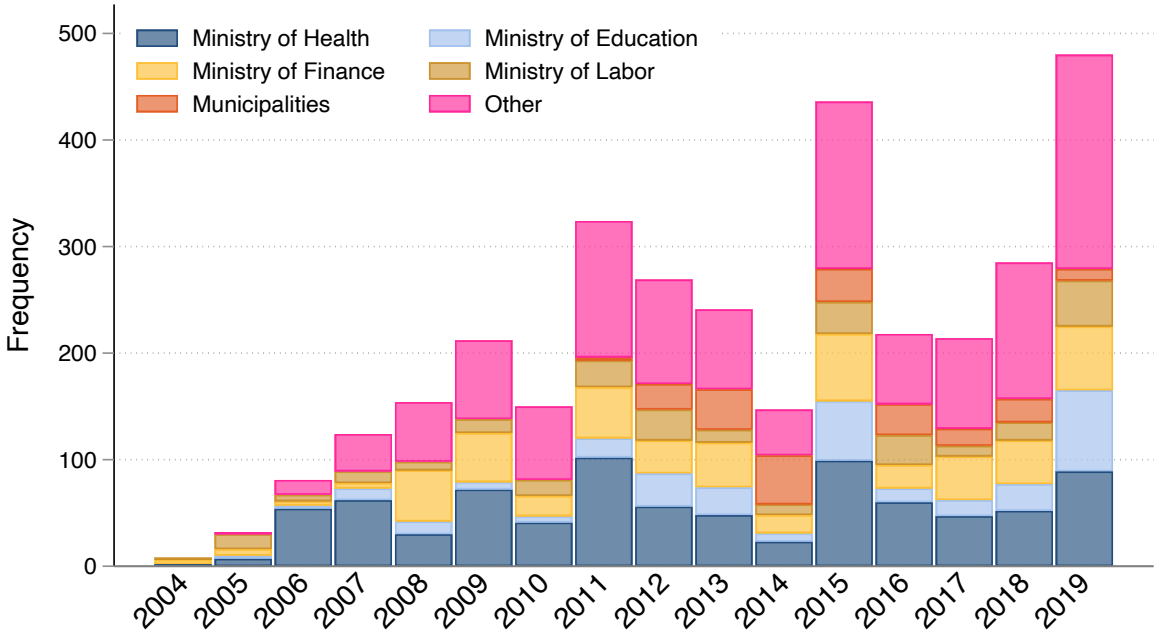
Notes: This figure displays the share of medical beds provided by public hospitals in a set of selected OECD countries in 2019. The dashed red line represents the average share in all OECD countries. The share is computed as the ratio between the total number of hospital beds in publicly owned hospitals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

Figure A.2: Selection process after the recruitment reform



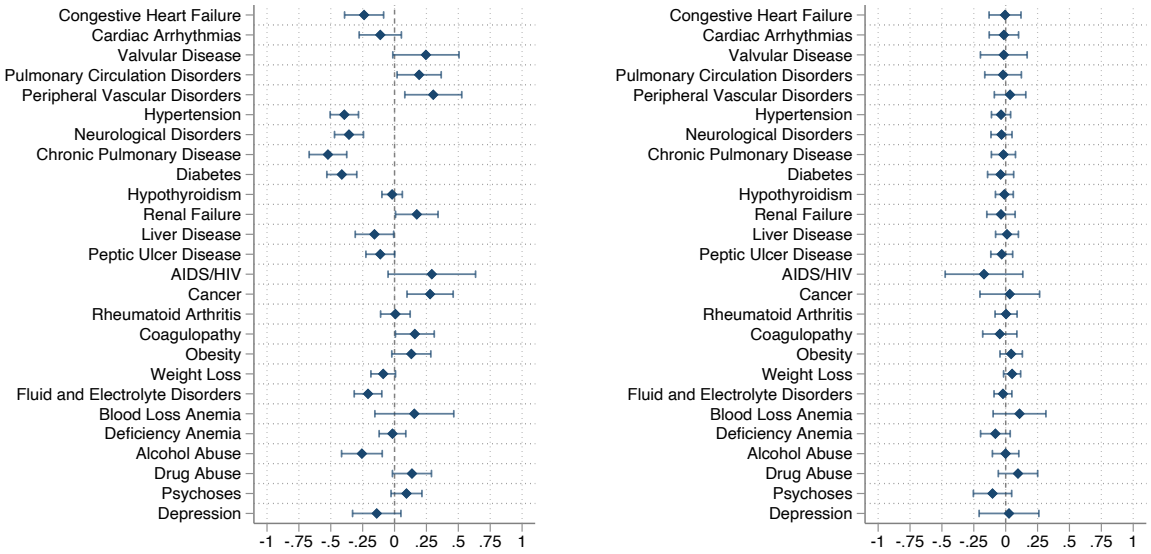
Notes: This figure illustrates the selection process for senior executive positions when the selection reform has been adopted. The job call starts with the position posted online on the Civil Service's website and in a newspaper with national circulation. After the job posting closes, an external HR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

Figure A.3: Yearly recruitment processes overseen by the Civil Service



Notes: This figure displays the number of selection processes the Civil Service oversees every year. We use the ending date of the process to allocate the process to a given year. Yearly observations include positions using the selection system for the first time and positions that had already adopted it in the past and are selecting a new manager. The spikes observed in 2011, 2015, and 2019 are evidence of substantial senior executive transitions after a new government is in place.

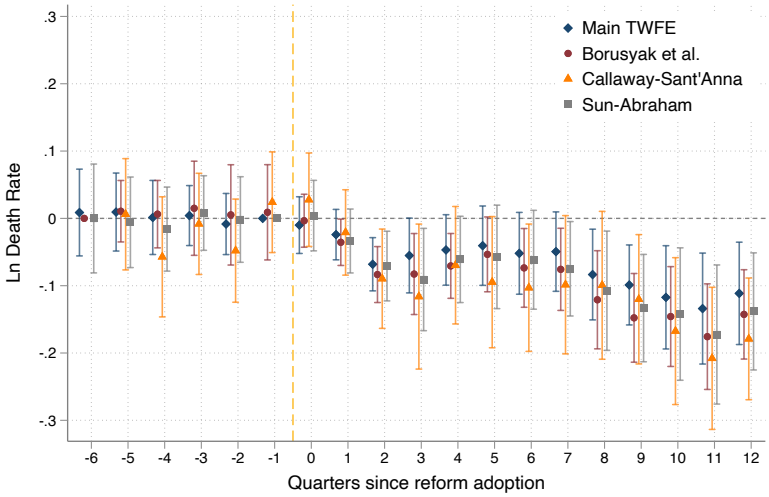
Figure A.4: Balance in observable characteristics: Elixhauser categories



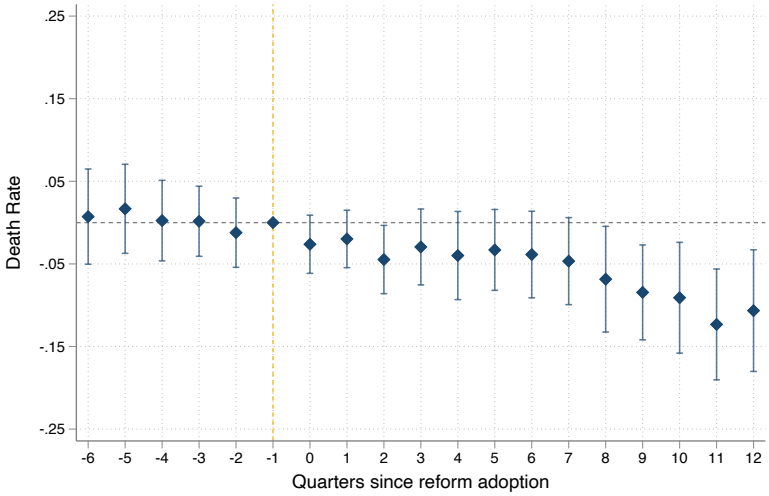
(A) Levels (B) Growth

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

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



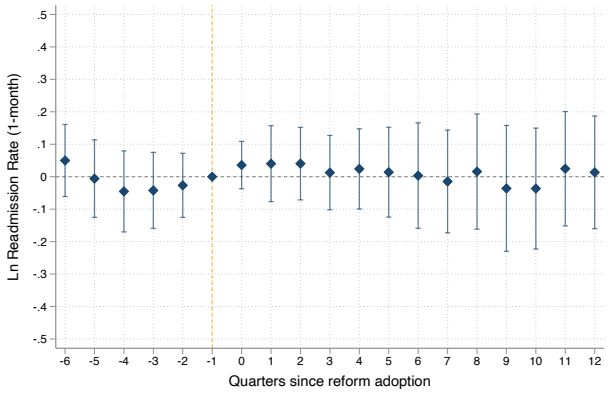
(A) Robustness to treatment effect heterogeneity



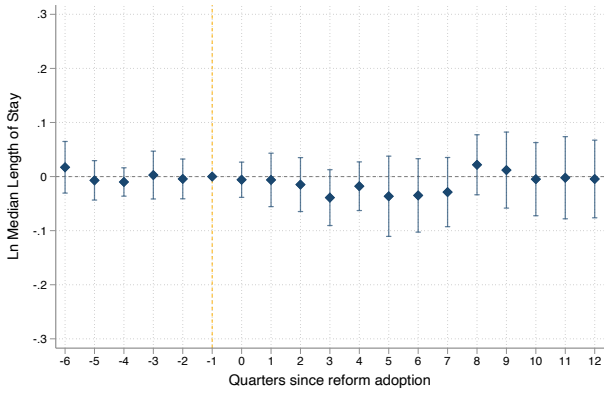
(B) Poisson QMLE

Notes: This figure plots the estimates and confidence intervals obtained using different event study models and estimation methods. Panel A presents results obtained using the model suggested by **?** (in red circle markers); **?** (in orange triangle markers); and **?** (in gray square markers), all of which are robust to treatment effect heterogeneity and appropriate in our setting (**?**). For comparison, we overlay them to our main results from Figure **??** (labeled as Main TWFE in the figure). Panel B presents event study evidence of the reform's effect on hospital deaths, using death rates in levels as the dependent variable in a dynamic Poisson QMLE regression. The estimation sample is at hospital-by-quarter and observations are weighted by the pre-reform number of inpatients in each hospital. Specifications in both panels include case-mix controls. Markers represent an estimated coefficient, and vertical lines indicate the corresponding 95% confidence intervals. Dashed yellow lines represent the omitted coefficient.

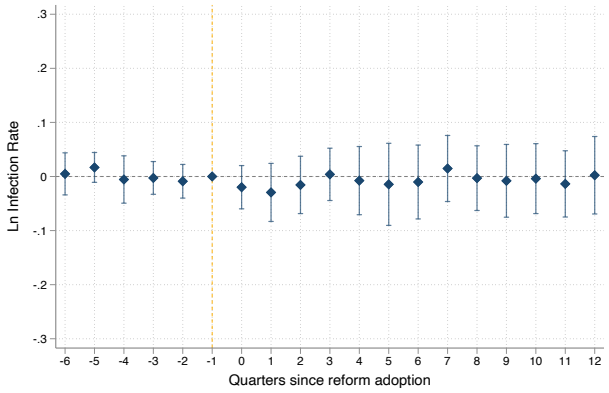
Figure A.6: Impact of the reform on other outcomes



(A) Ln Readmission Rate (1-month)



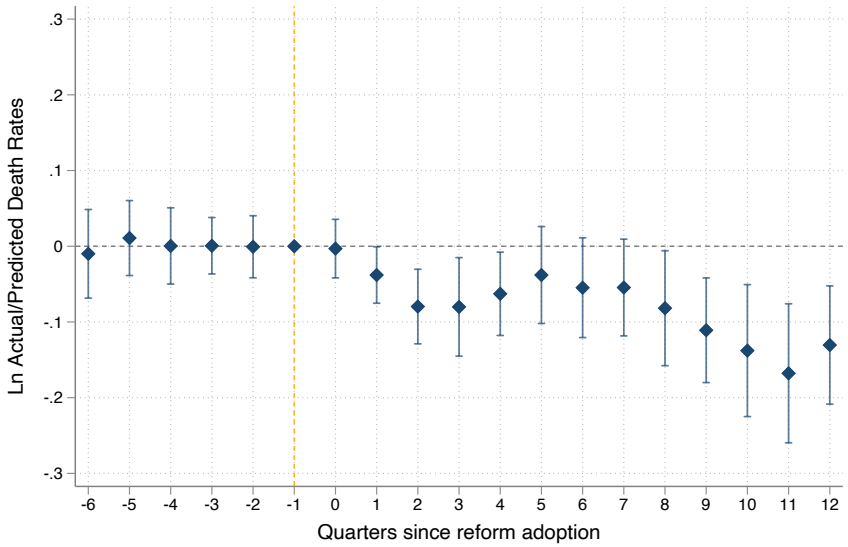
(B) Ln Median Length of Stay



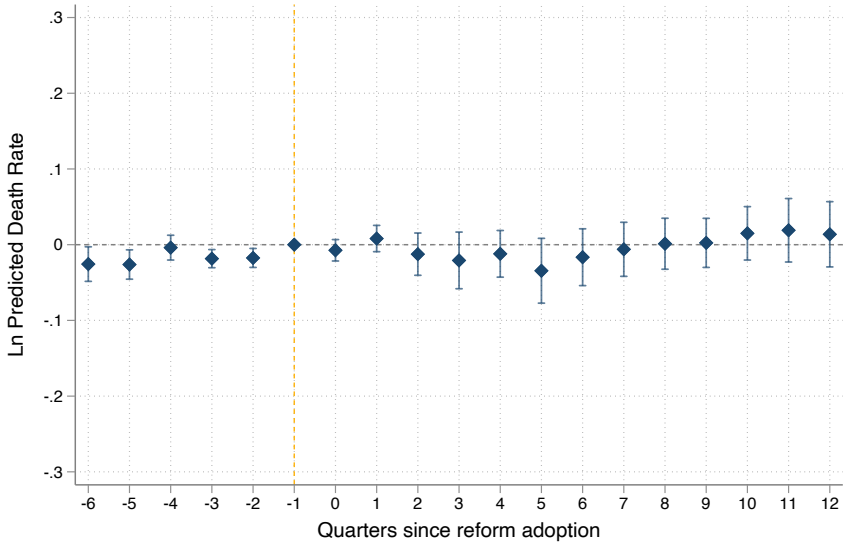
(C) Ln Infection Rate

Notes: This figure provides event study evidence, following Equation **??**, of the results presented in Appendix Table A.4. Panel (A) focuses on 1-month readmission rates in any hospital for emergency care. Panel (B) focuses on the median length of stay since admission. Panel (C) examines complication rates, defined as the proportion of inpatients diagnosed with conditions explicitly described as infections, sepsis, hemorrhage, or other complications in their diagnostic gloss.

Figure A.7: Dynamic effects of the reform on risk-adjusted death rate and predicted death rate



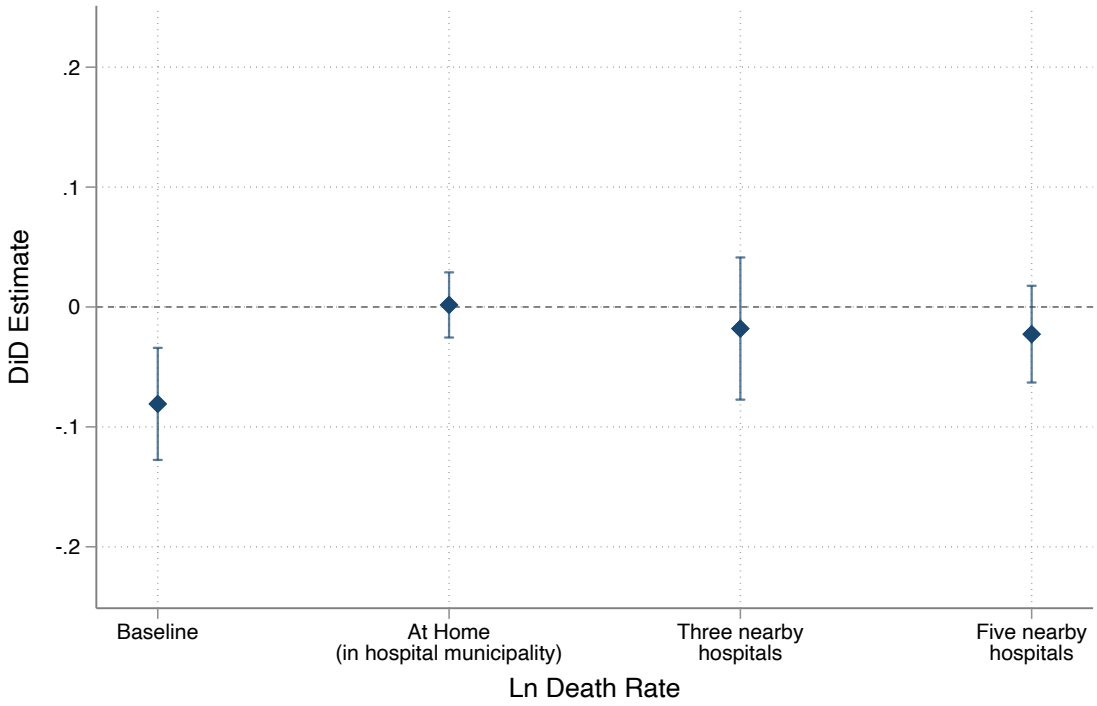
(A) Risk-adjusted mortality



(B) Predicted death rate

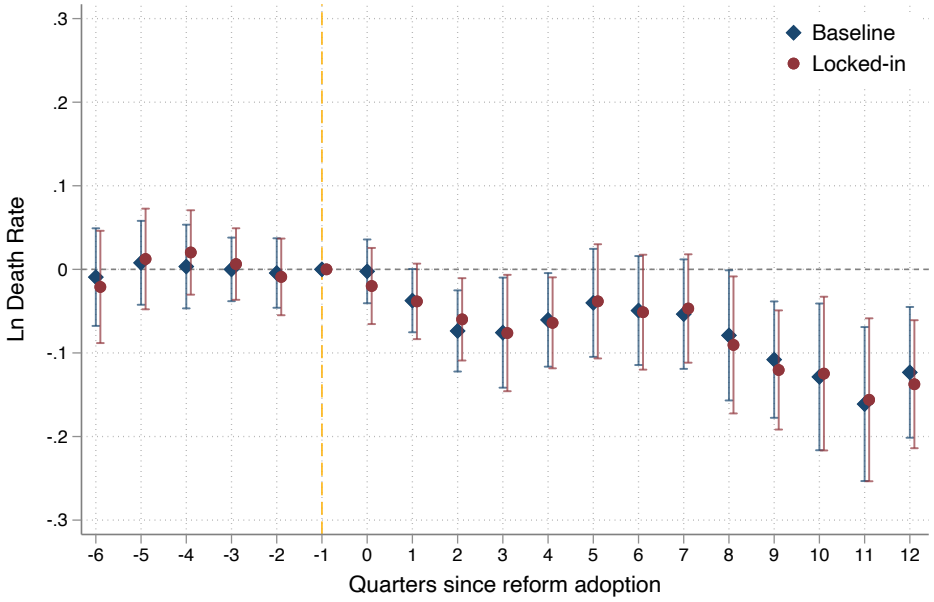
Notes: This figure presents event study evidence, following Equation **??**, on the impact of the selection reform on the risk-adjusted death rate and on the predicted death rate. Panel A reports estimates for the logged risk-adjusted death rate, and Panel B reports estimates for the logged predicted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

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

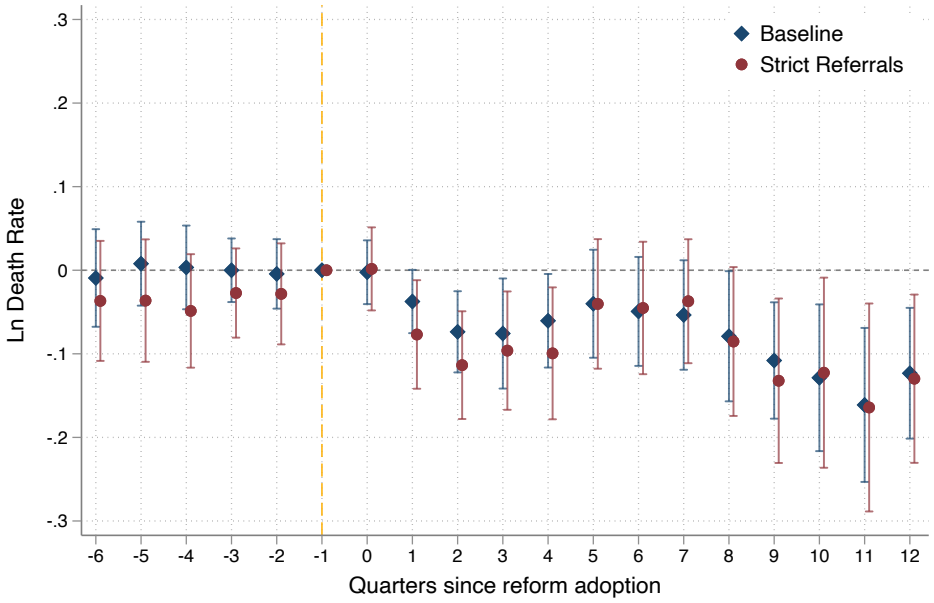


Notes: This figure presents evidence to assess patients' selection as a confounder of our main results. We plot the estimates and confidence intervals obtained by estimating Equation **??** for the logged at-home death rate and for logged death rates at nearby hospitals. All regressions include hospital-by-event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

Figure A.9: Testing for patient selection: Demand side



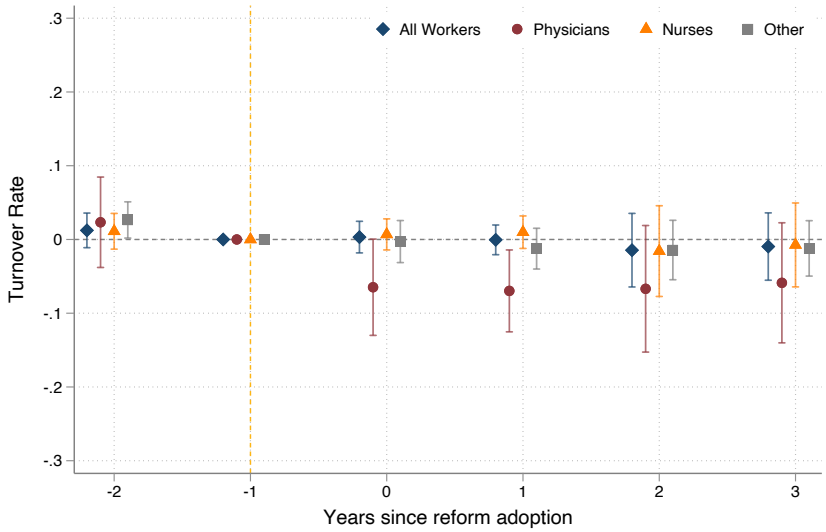
(A) Locked-in patients



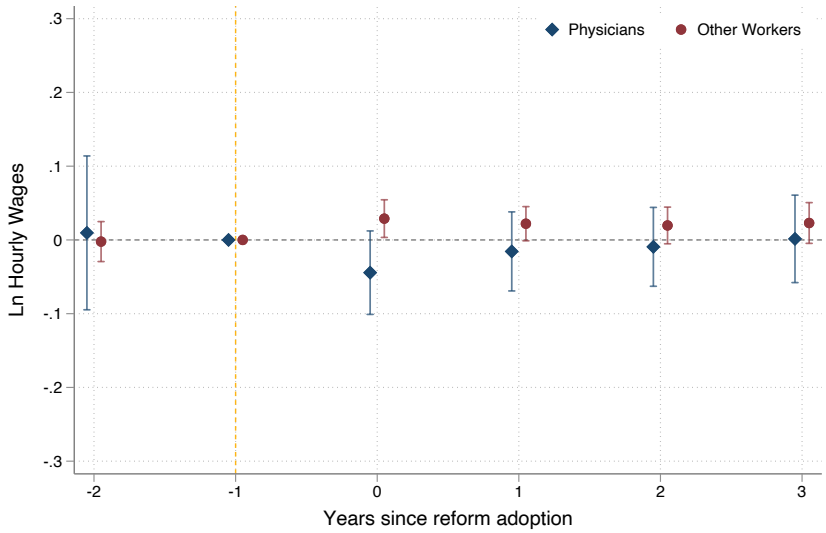
(B) Strict-referrals patients

Notes: This figure presents evidence to assess patients' selection as a confounder of our main results. Panel A presents event study evidence on the reform's effect on hospital deaths, following Equation **??**, but on a restricted sample of locked-in patients only. Panel B presents event study evidence on the reform's effect on hospital deaths, following Equation **??**, but on a restricted sample of patients who followed the referrals mandated by the health system. These figures also include the baseline estimates for comparison. All regressions include hospital-by-event and time fixed effects as well as case-mix controls. Standard errors are clustered at hospital level.

Figure A.10: Effect of the reform on hospital personnel outcomes



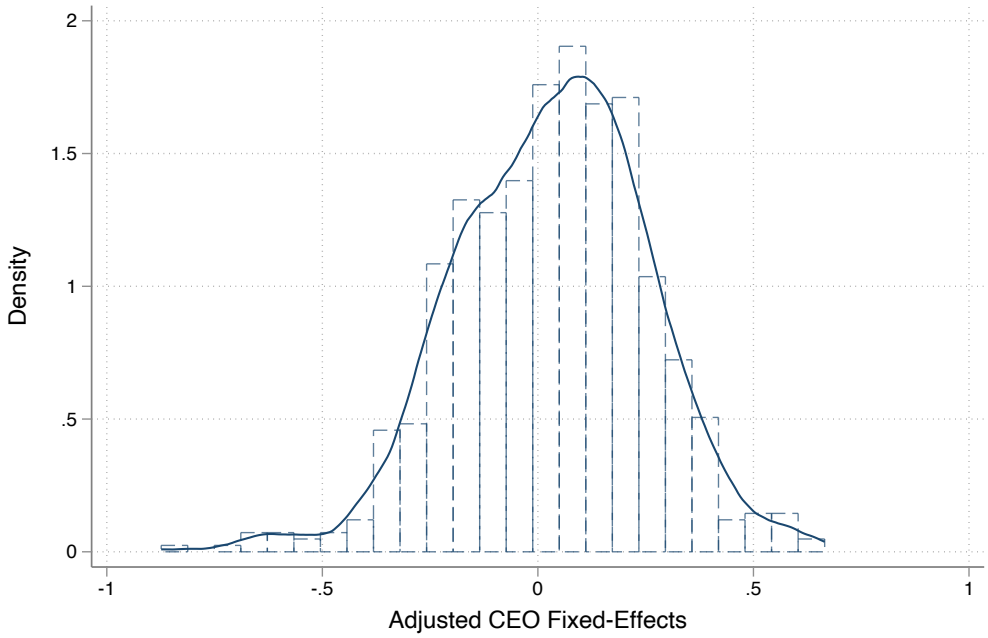
(A) Turnover of health workers



(B) Ln hourly wages

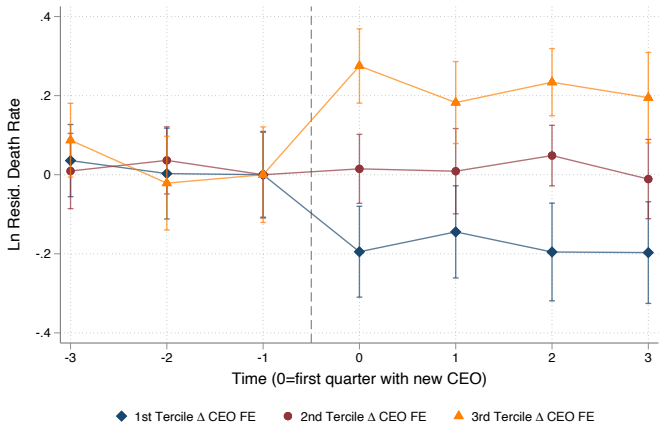
Notes: This figure presents event study evidence on the reform's effect healthcare workers' outcomes, following Equation **??**. Panel A uses data at yearly level on the turnover of health personnel. Turnover is defined as the number of workers in group j who are leaving hospital h in $t+1$ (job-to-job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

Figure A.11: Empirical distribution of CEO fixed-effects

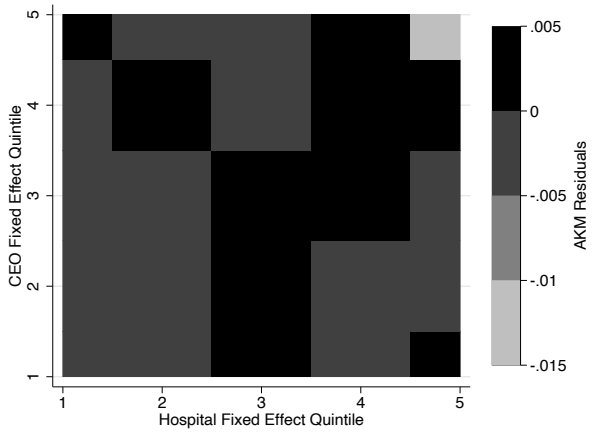


Notes: This figure plots kernel densities of the empirical distribution of CEO fixed-effects adjusted by their reliability using empirical Bayes shrinkage.

Figure A.12: Threats to the identification of managerial talent



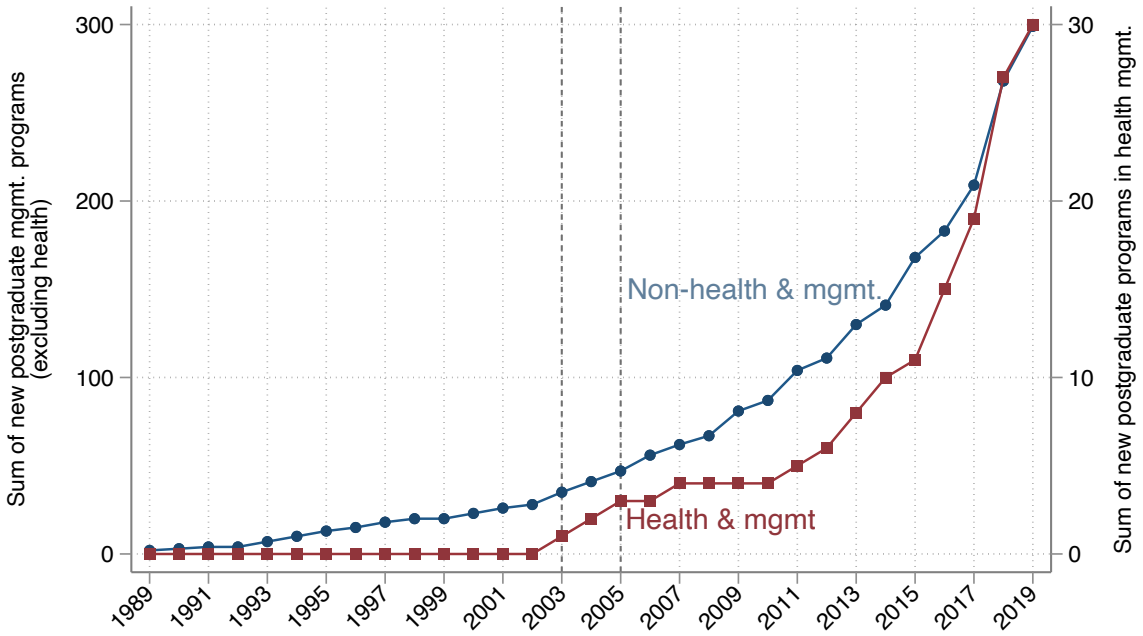
(A) Turnover by Δ fixed-effect



(B) Residuals

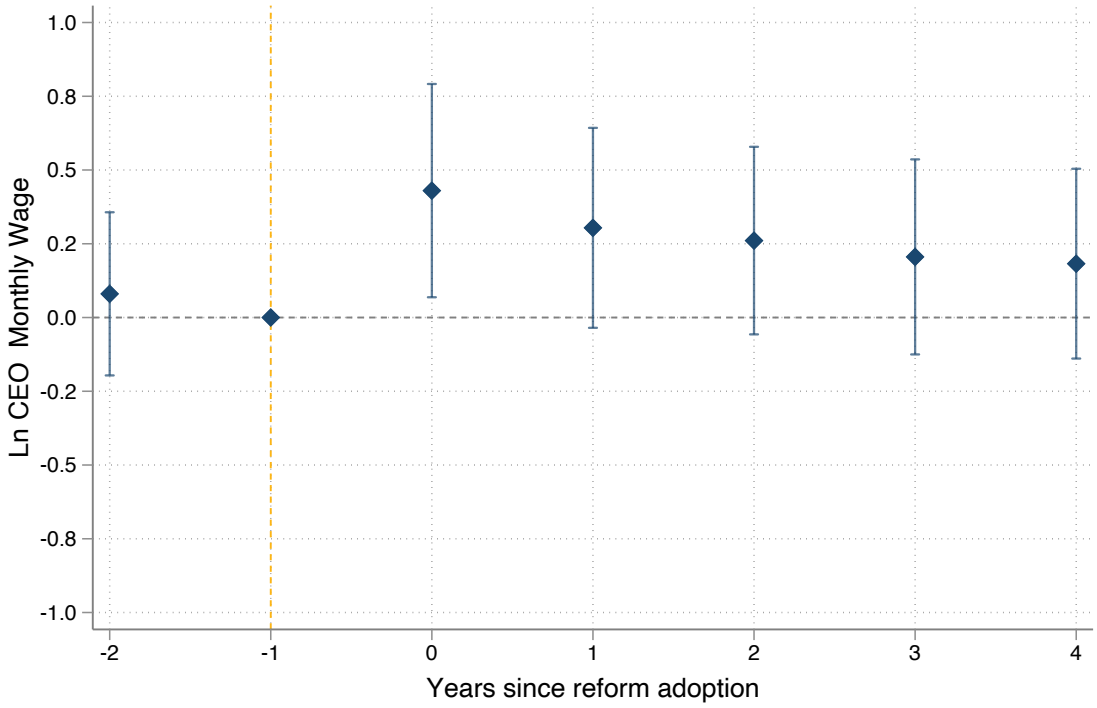
Notes: These figures assess threats to the identification of CEO fixed effects. Panel A plots three types of leadership transitions, classified by terciles of the change in managerial ability: (1) an overall increase (in orange triangle markers), (2) an overall decrease (in blue diamond markers), and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

Figure A.13: Creation of postgraduate programs in health management



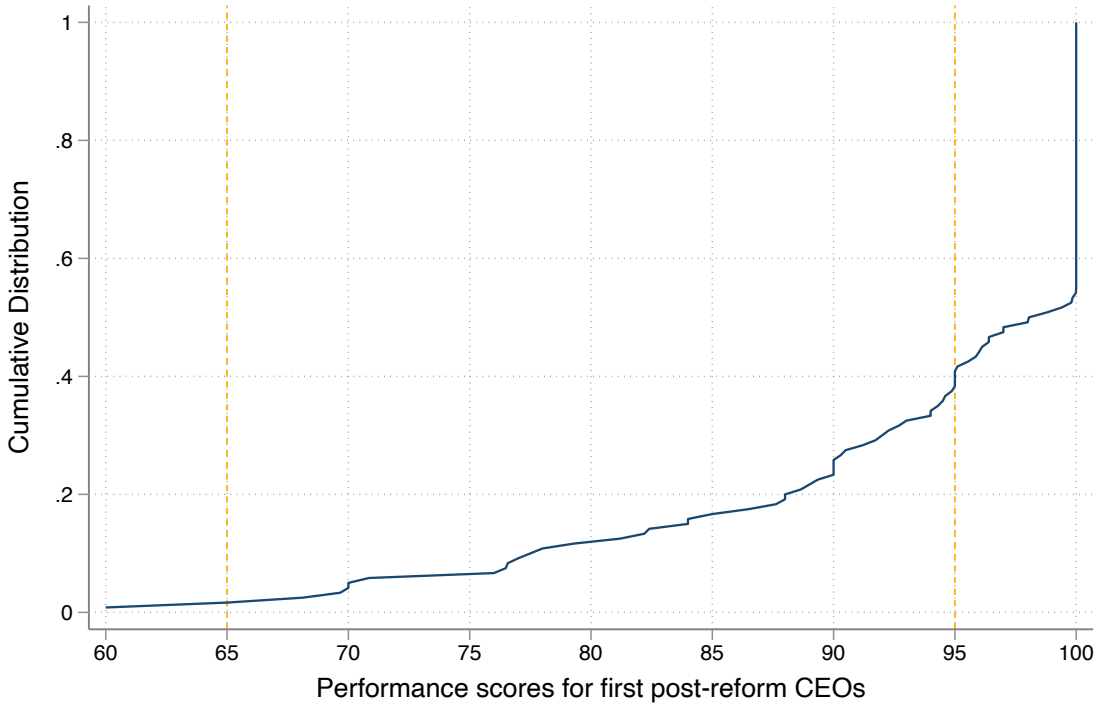
Notes: This figure shows the cumulative number of postgraduate management programs (diplomas and master's) by date of creation. Blue circles depict all management postgraduate degrees, excluding those related to health; corresponding frequencies are displayed on the left y-axis. Red squares depict new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

Figure A.14: Effect of the reform on CEOs' wages



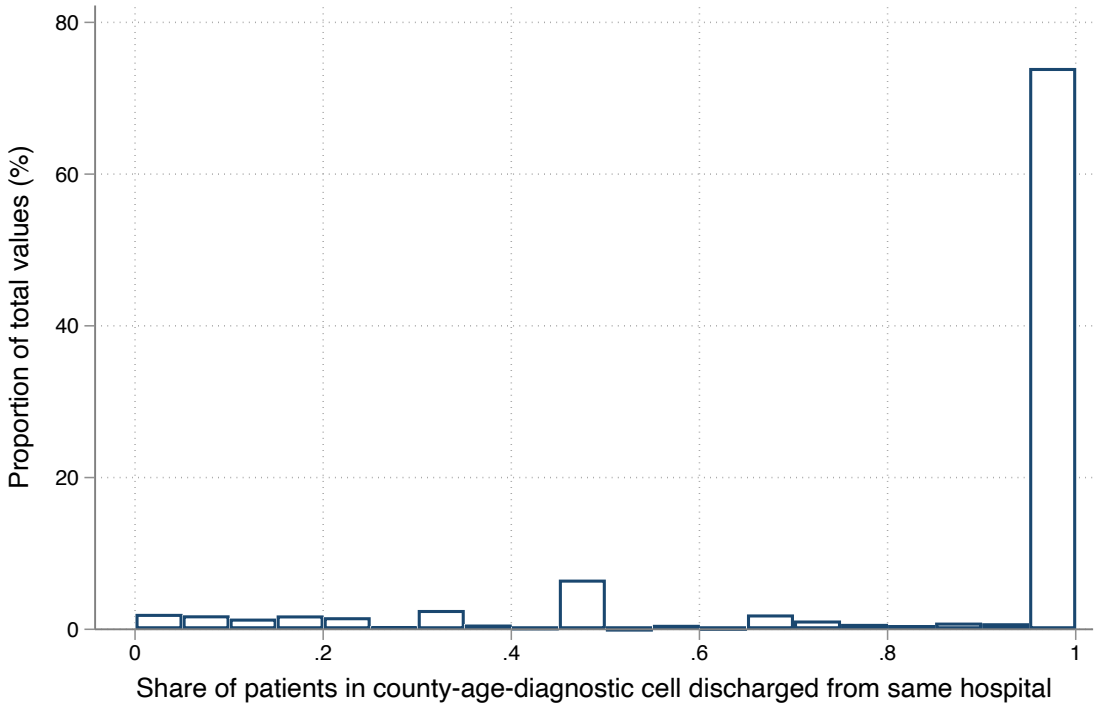
Notes: This figure presents the impact of the reform on hospital CEO wages. The empirical design leverages the gradual adoption of the selection reform across hospitals on an event study design. Regression controls include a quadratic polynomial of age and a dummy that indicates whether the individual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

Figure A.15: Distribution of performance scores for post-reform CEOs



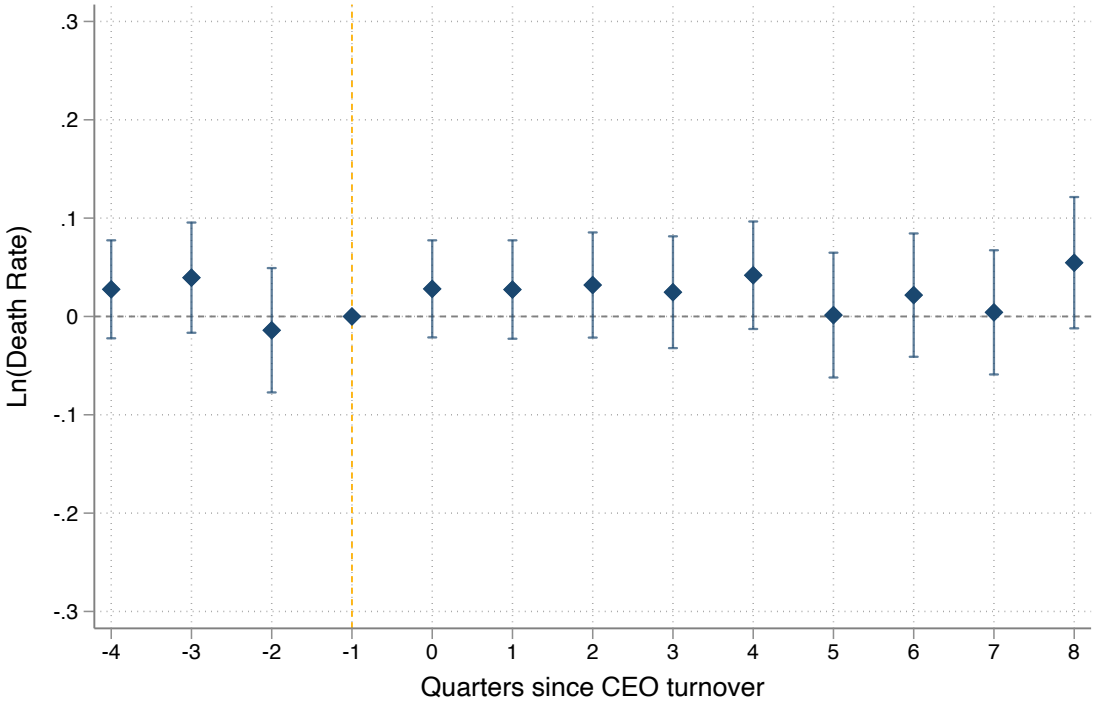
Notes: This figure displays the cumulative distribution of performance scores for the first post-reform CEOs. Before the reform, performance did not affect the wage schedule. After the reform, CEOs face wage penalties if they perform below specific performance thresholds. We accessed all available performance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

Figure A.16: Empirical test of patient selection



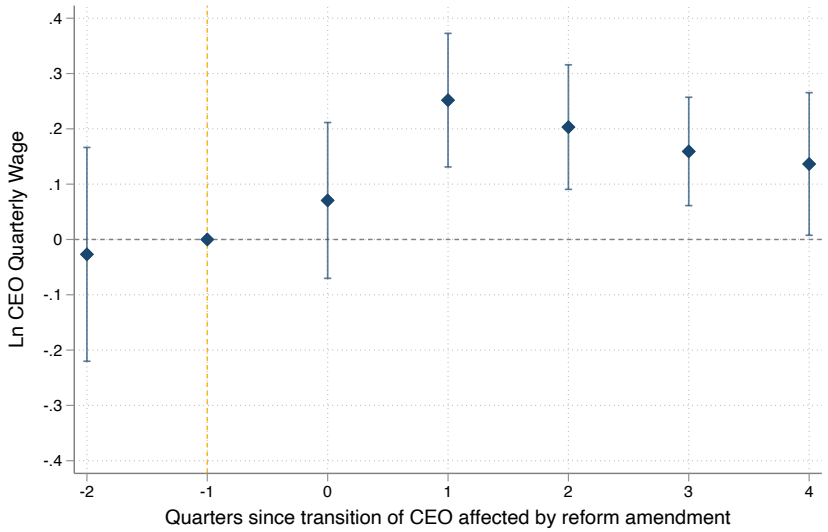
Notes: This figure plots a histogram with the share of patients in each cell who are discharged exclusively from one hospital. A cell is defined by the patient's municipality of residence, age group (less than 1 year, between 1 and 15 years, and more than 15 years), and diagnosis (as reported by the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

Figure A.17: Effect of CEO turnover on death rates

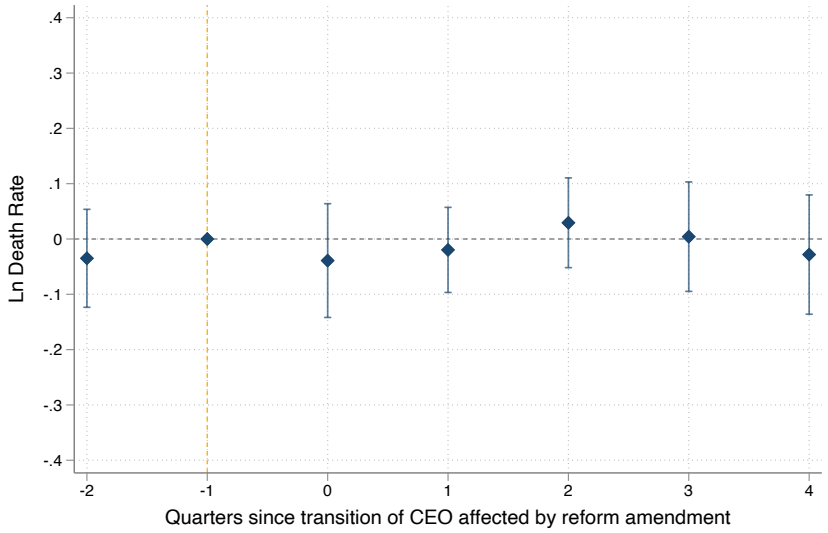


Notes: This figure presents the coefficients of the stacked event study specification in Equation A.1. An event is a CEO turnover in a hospital that never adopts the reform or in an adopter before the reform. For each turnover event, we define a time window around it and a control group of hospitals with no turnovers in the time window. We define a set of valid events as those that do not overlap with another turnover in the pre-period within the time window. The dependent variable is the death rate at hospital level in a given quarter. The regression includes case-mix controls. Markers indicate estimated coefficients and vertical lines corresponding 95% confidence intervals. Standard errors are clustered at hospital level.

Figure A.18: Do efficiency wages impact death rates?



(A) Effect on CEO wages



(B) Effect on death rates

Notes: This figure examines the impact of higher hospital CEO wages on hospital performance. The empirical design exploits an amendment to the recruitment reform, which increased wages for CEOs *only if* they were doctors *and* were appointed using the selection reform after November 2016. For each event, we define a time window around the event and determine an event-specific control group that excludes otherwise treated hospitals. There are a total of 17 valid events. We append the data for all valid events e and estimate an event study following Equation A.2. Panel A presents estimates of the amendment's effect on CEO wages, and Panel B displays the impacts on death rates. The regression of wages includes a quadratic polynomial of age and a dummy that indicates whether the individual is a doctor. The regression of death rates includes case-mix controls, and estimates are weighted by the pre-policy number of inpatients. Markers indicate estimated coefficients, and vertical lines indicate the corresponding 95% confidence intervals.

Table A.1: Civil service reforms

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

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

Table A.2: Descriptive statistics

	Mean	Std. Dev.	Median (p50)	# of Obs.
	(1)	(2)	(3)	(4)
Patient Characteristics:				
% Female	0.59	0.08	0.60	10,326
% Age < 29	0.34	0.15	0.36	10,326
% Age ∈ (30,39)	0.11	0.04	0.12	10,326
% Age ∈ (40,49)	0.10	0.03	0.10	10,326
% Age ∈ (50,59)	0.10	0.04	0.10	10,326
% Age ∈ (60,69)	0.11	0.04	0.11	10,326
% Age ∈ (70,79)	0.12	0.06	0.11	10,326
% Age ∈ (80,89)	0.09	0.06	0.08	10,326
% Age > 89	0.02	0.02	0.02	10,326
% Public Insurance	0.97	0.04	0.98	10,326
Hospital Characteristics:				
Total Number of Patients	1,546	2,024	606	10,326
Physicians per 100 patients.	6.16	7.48	4.75	6,173
Nurses per 100 patients.	5.66	6.40	4.67	6,173
Number of Deaths	41.58	63.46	15.00	10,326
Death Rate	2.91	1.96	2.50	10,326
Death Rate 28-days	4.66	2.85	4.00	10,326
Actual over Predicted Death Rate	0.88	0.45	0.85	10,326

Notes: This table presents descriptive statistics for the universe of public hospitals included in our sample. Patient characteristics and hospital outcomes are from individual-level inpatient records collected by the Ministry of Health and encompass more than 16.5 million hospital events (**?**). Hospital characteristics are from hospital-level public records and restricted-use administrative data covering the universe of employees in all public hospitals between 2011 and 2019, which is collected by the Ministry of Health for HR purposes.

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

	$\mathbb{1}$[death rate > 0]	Ln(death rate)		
	(1)	(2)	(3)	(4)
1 if reform adopted	0.002	-0.081	-0.080	-0.077
	(0.002)	(0.024)	(0.024)	(0.024)
Sample	All	All	Exclude hospital if 4+ obs are zero	Exclude hospital if 1 obs is zero
# of Hospitals	183	182	172	162
Mean Dep. Variable	2.46	2.58	2.61	2.60
Observations	219,611	205,453	191,834	164,711

Notes: This table presents robustness checks of the impact of the selection reform on public hospital outcomes. Estimates are from the stacked difference-in-differences specification in Equation **??**. The empirical analysis uses quarterly panel data for public hospitals and a time window comprising 6 quarters before and 12 quarters after the reform was adopted by each hospital and exploits the gradual adoption of the selection reform in public hospitals during that period. For each treated hospital, we determine an event-specific control group that excludes already-treated units. Column (1) uses $\mathbb{1}$[death rate > 0] as the outcome. Column (2) replicates the main result in the main text. Column (3) excludes hospitals that have 4 or more quarters with no deaths. Colum (4) includes only hospitals with a positive number of deaths in every period.

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

	Ln Readmission Rate (1-month)	Ln Median Length of Stay	Ln Infection Rate
	(1)	(2)	(3)
1 if reform adopted	0.014	-0.012	-0.005
	(0.071)	(0.019)	(0.024)
Mean of Dep. Variable	8.68	4.52	10.59
Observations	204,584	205,453	202,790

Notes: This table presents the impact of the selection reform on public hospital outcomes. Estimates are from the stacked difference-in-differences specification in Equation **??**. The empirical analysis uses quarterly panel data for public hospitals and a time window comprising 6 quarters before and 12 quarters after the reform was adopted by each hospital and exploits the gradual adoption of the selection reform in public hospitals during that period. For each treated hospital, we determine an event-specific control group that excludes already-treated units. Column (1) focuses on 1-month readmission rates in any hospital for emergency care. Column (2) focuses on the median length of stay since admission. Column (3) examines complication rates, defined as the proportion of inpatients diagnosed with conditions explicitly described as infections, sepsis, hemorrhage, or other complications in their diagnostic gloss. All specifications include hospital-by-event and time-effects as well as case-mix controls. Results are weighted by the pre-policy number of inpatients. 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 the hospital level.

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

	CEO Fixed Effect	
	(1)	(2)
Female	-0.18	-0.228
	(0.028)	(0.028)
Age	0.087	0.120
	(0.010)	(0.010)
Age^2	-0.001	-0.001
	(0.000)	(0.000)
Mgmt. Studies		-0.277
		(0.035)
Observations	4,860	4,860

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

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

	Ln Death Rate		CEO Fixed-effect	
	(1)	(2)	(3)	(4)
Reform	-0.049		-0.041	
	(0.021)		(0.027)	
Reform × CEO Turnover		-0.067		-0.067
		(0.022)		(0.034)
Reform × No CEO Turnover		0.016		-0.006
		(0.039)		(0.040)
Mean of Dep. Variable	2.58	2.58	0.08	0.08
Observations	123,921	123,921	9,429	9,429

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

Table A.7: Referral guidelines example

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

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

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

Table A.8: CEO selection reform v. other policies

Policy	Paper	Death-rate definition	Average death rate	Impact on death rate	Sample of patients
(1)	(2)	(3)	(4)	(5)	(6)
Spending					
↑ spending by 10%	Doyle et al. JPE '15	All, 1-year	37%	-6%	ER + Amb. + ≥65*
	Ours		33%	-11%	ER + ≥65
Competition					
+1 hospital in neighborhood	Bloom et al. ReStud '15	In-hospital, 28-day	15%	-10%	ER + AMI
	Ours		16%	-13%	ER + AMI
↓10% HHI	Gaynor et al. AEJ EP '13	In-hospital, 28-day	1.6%	-1%	All patients
	Ours		2 %	-8%	All patients

Notes: This table compares the impact of the CEO selection reform we study with the impact of other policies previously studied in the literature. To construct this table, we estimate our main Equation **??** for the different dependent variables—reported in column (3)—and in different samples of patients reported in column (6). In the case of AMI deaths, we use a Poisson difference-in-differences design. For more details, see Appendix C. Acronyms used in the table: ER: Emergency Room; AMI: Acute myocardial infarction; Amb: arriving by ambulance; *: non-deferrable medical condition; **: sample of ambulance rides with no prior ride within a year.