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Publication Date
2023-12-01

DOI
10.1016/j.jhealeco.2023.102826

Peer reviewed

HHS Public Access
Author manuscript
J Health Econ. Author manuscript; available in PMC 2023 December 11.

Published in final edited form as:
J Health Econ. 2023 December ; 92: 102826. doi:10.1016/j.jhealeco.2023.102826.

How do hospitals respond to input regulation? Evidence from the California nurse staffing mandate

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Abstract

Mandated minimum nurse-to-patient ratios have been the subject of active debate in the U.S. for over twenty years and are under legislative consideration today in several states and at the federal level. This paper uses the 1999 California nurse staffing mandate as an empirical setting to estimate the causal effects of minimum ratios on hospitals. Minimum ratios led to a 58 min increase in nursing time per patient day and 9 percent increase in the wage bill per patient day in the general medical/surgical acute care unit among treated hospitals. Hospitals responded on several margins: increased use of lower-licensed and younger nurses, reduced capacity by 16 beds (14 percent), and increased bed utilization rates by 0.045 points (8 percent). Using administrative data on discharges for acute myocardial infarction (AMI), I find a significant reduction in length of stay (5 percent) and no effect on the 30-day all-cause readmission rate. The null effect on readmissions suggests that length of stay declined not because hospitals were discharging AMI patients "quicker and sicker", rather, AMI patients recovered more quickly due to an improvement in care quality per day.

Keywords

D22; H75; I10; I11; J44; J23; Minimum staffing ratios; Staffing; Nurses; Hospitals; Healthcare quality

1. Introduction

Mandated minimum nurse-to-patient ratios have been the subject of active debate in the U.S. for over twenty years and are under legislative consideration today in several states and at the federal level.[2] A stated intention of minimum ratios is to increase patient welfare through

[1]I am grateful for the encouragement and support of my dissertation committee: John Asker, Martin Hackmann, Will Rafey, and Till von Wachter. I thank Maya Ayoub, Jesper Böjeryd, Naomi Crowther, Domenico Fabrizi, Andrew Heinzman, David Henning, Jonathan Kowarski, Adriana Lleras-Muney, Daniel Perez, Huihuang Zhu and seminar participants for helpful comments. I thank Kyle Rowert, Tim Pasco, Harry Dhami, and Merry Holliday-Hanson at the California Department of Healthcare Access and Information (HCAI) for answering my questions regarding the financial reporting and patient severity data. This work benefited from secure data storage facilities and support provided by the California Center for Population Research, United States (P2CHD041022). The content is solely the responsibility of the author and does not necessarily represent the views of any organizations listed. All errors are mine.

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[2]Only California and Massachusetts currently have minimum nurse-to-patient ratios in hospitals. Massachusetts mandates minimum ratios only in the intensive care unit. Active bills S. 1567 in the US Senate, SB 240 in the Pennsylvania Senate, and S6855 in the New York Senate are among proposed legislation would implement minimum nurse-to-patient ratios in hospitals. Several other pieces of legislation have focused on staffing in non-hospital healthcare settings. For example, many states including California have mandated

improved healthcare quality.[3] Notably, however, most studies have found no or mixed effects of minimum ratios on healthcare quality in hospitals (Cook et al., 2012; Mark et al., 2013; Spetz et al., 2013) which is puzzling given the evidence of large, positive quality returns to nursing time per patient day (Gruber and Kleiner, 2012; Friedrich and Hackmann, 2021).[4]

The apparent contradiction between the null quality effect of minimum ratios and the large returns to nursing time raises several questions: Do minimum ratio policies lead to crowding out of other inputs due to factor substitution? An increased use of low-skilled nurses? Reductions in length of stay? Hospitals may substitute away from unregulated inputs, hire low-skilled nurses, or discharge patients “quicker and sicker” in response to minimum ratios. Each of these responses may, depending on the production technology, have adverse implications for healthcare quality. Prior literature on factor substitution and the quantity-quality tradeoff in healthcare is limited and the production technology is unique to the sector, therefore these questions must be answered empirically.

In this paper, I use the 1999 California nurse staffing mandate as an empirical setting to study the effects of minimum ratios on input use, capacity, output, costs, and healthcare quality. The mandate required hospitals to meet minimum nurse-to-patient ratios established for each hospital unit by the California Department of Health Services. I combine hospital financial reporting data and administrative patient discharge data with a difference-in-differences research design.

I find that the mandate significantly increased hospitals’ nurse-to-patient ratios and led to limited crowding out of other inputs. However, hospitals responded on other margins: increased use of lower-licensed and younger nurses, reduced capacity by 16 beds (14 percent), and increased bed utilization rates by 0.045 points (8 percent) to 64 percent. The increase in utilization suggests that hospitals were operating with excess bed capacity prior to the mandate and reduced capacity in response to a rise in costs per staffed bed.

Using administrative data on discharges for acute myocardial infarction (AMI), I find that the mandate led to a 5 percent decline in length of stay. Shorter length of stay is used as an indicator for high quality of care because delays and errors in the delivery of care increase length of stay. However, discharging patients “quicker and sicker” may be one way hospitals respond to financial incentives (Morrisey et al., 1988) or capacity constraints (Hoe, 2022). In light of the substantial capacity reduction that I document, I investigate whether the decline in length of stay is indicative of premature discharge or higher care quality. Contrary to the expectations under a “quicker and sicker” hypothesis, I find no effect on the 30-day all-cause readmission rate despite the decline in length of stay. My findings indicate that AMI patients at treated hospitals recovered more quickly following the mandate due to an improvement in care quality per day.

minimum staffing ratios for nursing homes. California voters recently rejected a proposition which would have mandated a minimum number of licensed healthcare professionals in dialysis clinics.

[3]The text of the 1999 California nurse staffing legislation (AB 394) which mandated minimum nurse-to-patient ratios in hospitals states that “quality of patient care is jeopardized because of staffing changes implemented in response to managed care” and staffing regulation is consequently enacted to “ensure the adequate protection of patients in acute care settings”.

[4]Descriptive studies of the mandate including Burnes Bolton et al. (2007) and Donaldson et al. (2005) also find null quality effects of the mandate.

I exploit two institutional features for identification. First, variation in nurse-to-patient ratios across hospitals prior to the mandate created variation in the "bite" of the mandate across hospitals. Hospitals below the mandated threshold were treated. In my main specification, I estimate a difference-in-differences model comparing the outcomes in the general medical/surgical acute care unit (hereafter "acute care unit") of hospitals initially below and above the mandated minimum ratio threshold in acute care. [5] In a heterogeneity analysis, I exploit the continuity of treatment and show that in line with expectations the treatment effect on labor increases with the gap between the hospital's initial staffing ratio and the threshold.

Second, the mandated ratios were established at the hospital unit level and created variation in the "bite" of the mandate across hospital units within a hospital. In some hospital units (e.g. general medical/surgical acute care), the majority of hospitals were initially below the unit-specific threshold whereas in other units (e.g. general medical/surgical intensive care), the majority of hospitals were initially above. In California, intensive care units were already subject to minimum nurse-to-patient ratios under state law beginning in the 1976–1977 fiscal year (Spetz et al., 2000).[6] In a robustness specification, I estimate my model on outcomes from the intensive care unit as a placebo test and show that there were no significant effects of the mandate in intensive care.

For estimation, I use annual financial data reported for each hospital unit in each hospital between 1990–2016 from the Department of Health Care Access and Information (HCAI) in conjunction with administrative patient discharge data between 1995–2008. The long time frame and granularity of the data allow me to validate my difference-in-differences research design and show several robustness specifications.

The analysis proceeds as follows. First, I find that the mandate had its intended effect on understaffed hospitals' nurse-to-patient ratios in the acute care unit. I estimate a significant, 0.040 point increase in the nurse-to-patient ratio on a mean of 0.241 (21 percent) for treated hospitals. This implies an additional 58 min of nursing time per patient day.[7] I show that roughly 39 min came from Registered Nurses (RNs) and 22 min from lower-licensed Licensed Vocational Nurses (LVNs). I show that substitution away from other labor (aides, physicians) and non-labor (capital, intermediate inputs) inputs was limited. The limited substitutability between nurse and non-nurse labor is consistent with strict scope of practice regulations in California that specify the tasks that each licensed healthcare professional is allowed to perform in the hospital setting. I consequently find that treated hospitals faced a 9 percent increase in the wage bill due to the mandate.

Second, I estimate that the average wage of RNs at treated hospitals declined by 3.3 percent relative to control hospitals. I provide descriptive evidence from several data sources that

[5]For the remainder of this paper, I refer to the general medical/surgical acute care unit as the "acute care unit" and care provided in this unit as "acute care" with the acknowledgment that the term may encompass care from a broader set of hospital units when used in contexts outside of this paper.

[6]Beginning in the 1976–1977 fiscal year, hospitals were required to staff 0.5 nurse-to-patient ratio in the intensive and coronary intensive care units (Title 22 of California Code of Regulations). These ratios were unchanged by the mandate.

[7]My main specification adjusts the patient days by patient severity using the Case Mix Index calculated by the California Department of Health Services. If the outcome is not adjusted for patient severity I find a significant, 0.025 point increase in the nurse-to-patient ratio and corresponding 36 min of additional nursing time per patient day.

the wage decline was plausibly due to changes in RN composition towards younger and more recently licensed RNs. I use the National Sample Survey of Registered Nurses to show that RNs employed in California hospitals became younger and more recently licensed than RNs employed at hospitals in other states after the mandate. I use licensing data from the National Council of State Boards of Nursing to show that the changes in composition are consistent with a large growth of new entrants into the California nursing labor market at the time of the mandate. These new entrants came from both the “examined in-state” and “endorsed from out-of-state” channels.

Third, I estimate the effects on capacity, output, and utilization and find that treated hospitals reduced capacity by 16 beds on a mean of 118 beds (14 percent) and increased utilization rates by 0.045 points on a mean of 0.556 (8 percent) almost immediately after the mandate. The increase in utilization to 64 percent among treated hospitals suggests that hospitals were operating with significant excess capacity prior to the mandate.

Finally, I use administrative data on AMI discharges to estimate the effects on the risk-adjusted length of stay, 30-day all-cause readmission, and in-hospital mortality. I find no effect on the in-hospital mortality rate. However, I find a decline in length of stay of 0.281 days on a mean of 6.153 days (5 percent) consistent with descriptive evidence I show from the hospital financial data covering all discharges. I investigate whether the shorter length of stay is indicative of premature discharge or higher care quality. I find that the 30-day all-cause readmission rate was stable despite the decline in length of stay. I conclude that AMI patients at treated hospitals experienced increases in care quality per day which led to quicker recovery times. Importantly, I show that the increase in quality in the long-run is consistent with prior work on the returns to tenure in nursing (Bartel et al., 2014).

I show three robustness checks. First, I extend the pre-period by an additional six years for which I lack data on hospital-level patient severity[8] allowing for graphical inspection of pre-trends over a longer period. Second, I repeat the main specification using the intensive care rather than acute care unit of the same sample of hospitals as a placebo test of my findings and estimate null effects for the majority of outcomes. Third, I use a heterogeneity analysis to show that in accordance with expectations the treatment effects on labor are larger for hospitals with the lowest initial ratios prior to the mandate.

My paper relates to several literatures. First, my paper attempts to bridge the gap between prior work on the effects of minimum ratio policies on quality and on the quality returns to nursing. I find that the reduction in length of stay increases over time from 2.6 percent and statistically insignificant within one year of the mandate to 6.9 percent and significant three years after the mandate. These dynamic effects are consistent with estimates of the returns to tenure in nursing measured in length of stay (Bartel et al., 2014) and suggest that the magnitude and significance of the estimated treatment effects depend on the length of the post-mandate estimation period. Prior work on the mandate estimates no or mixed

[8]These additional six years are not included in the main specification because I lack data on the hospital-level Case Mix Index (CMI) prior to 1996. Patient severity as measured by CMI is a key determinant of nurse staffing levels for a hospital. For example, the CMI constructed by Centers for Medicare and Medicaid (CMS) is used to adjust reimbursements for the severity of admitted patients and the expected costs of caring for more acute patients.

quality effects using a 2004–2006 post-mandate period (Cook et al., 2012; Mark et al., 2013; Spetz et al., 2013). I complement this literature by using a longer post-mandate period from 2004–2008 over which I find positive quality effects.

My findings are therefore consistent with prior evidence on positive quality returns to nursing measured as a decline in length of stay (Bartel et al., 2014) or a decline in readmission with stable length of stay (Friedrich and Hackmann, 2021). At the same time, I find a null effect on in-hospital mortality consistent with Friedrich and Hackmann (2021), who find no effect of a decline in nurse staffing on AMI in-hospital mortality, but distinct from Gruber and Kleiner (2012), who find large increases in in-hospital mortality across conditions. I posit that estimates vary across papers due to differences in the staffing shocks and quality indicators used. In my setting, the incidence of the staffing shock fell on the acute care unit therefore we should expect to observe effects on indicators that are sensitive to acute care staffing. In-hospital mortality is an unlikely candidate because mortality is far more likely to take place in the intensive care unit, where patients in critical condition are stabilized prior to being transferred to acute care.

Second, I contribute more broadly to the literature on the effects of the minimum staffing mandate. As far as I am aware, I provide novel evidence of several responses: the decline in capacity, increase in bed utilization rates, increase in use of younger and more recently licensed RNs, and the limited crowding out of other inputs in response to the mandate. I estimate the cost effects of the mandate to be far smaller than estimated in prior descriptive work (Terasawa, 2016).[9]

Notably, my identification approach represents an improvement on prior work which has shown little evidence to support research design validity. I provide up to thirteen years of pre-mandate data to allow for graphical inspection of pre-trends, utilize difference-in-differences and event study estimates, and provide several robustness checks.

Third, I contribute to a long literature on hospital production. My finding that hospitals reduced excess capacity in response to an exogenous shock to costs per staffed bed illustrates the hospital’s tradeoff between healthcare access (having a lower probability of turning patients away) and profits (having a lower cost of unused, staffed beds) as modeled in early theoretical literature (Newhouse, 1970). In models of the hospital’s capacity choice, hospitals operate with excess capacity to target a desired probability of turning patients away rather than due to inefficiency (Gaynor and Anderson, 1995).

I corroborate findings that nurse and non-nurse labor have limited substitutability in hospital production (Friedrich and Hackmann, 2021) and complement evidence that hospitals substitute between nurses of different skill levels (Acemoglu and Finkelstein, 2008). The latter finding complements and uncovers relative to prior work (Matsudaira, 2014)

[9]My findings confirm the magnitudes of the increases in nurse- and RN-to-patient ratios (Cook et al., 2012; Mark et al., 2013; Spetz et al., 2013; Terasawa, 2016; Munnich, 2014) and the LVN-to-patient ratio (Mark et al., 2013; Spetz et al., 2013; Cook et al., 2012) and decline in the aide-to-patient ratio (Chapman et al., 2009; Cook et al., 2012) documented in earlier studies. Similar to the prior literature, I do not find conclusive evidence of general equilibrium effects on wages (Harless, 2019; Munnich, 2014).

that heterogeneity in workforce composition is important to control for when testing for monopsony using labor quantity regulation.

Relatedly, my findings contribute to a broader literature in labor economics on the firm's responses to labor market regulation. The mandate represents a labor quantity floor which is conceptually similar to minimum wage policies that represent labor price floors. My finding that hospitals hire lower wage nurses (lower-licensed, younger nurses) in response to the mandate is therefore related to prior empirical work that has found changes in workforce composition towards higher-skilled workers following minimum wage policies (Clemens et al., 2021; Gopalan et al., 2021). However, the extent to which my findings are generalizable to other industries is an open question given that substitution patterns across inputs depend on production technology specific to the sector.

The remainder of the paper proceeds as follows. Section 2 describes the institutional context of nursing in the hospital setting and the nurse staffing mandate. In Section 3, I discuss the data and empirical framework. I present the results in Section 4, heterogeneity results in Section 5, and robustness checks in Section 6. Section 7 concludes.

2. Institutional context

2.1. Nursing in the hospital setting

Hospitals consist of several inpatient hospital units including general medical/surgical acute care, general medical/surgical intensive care (also referred to as "critical care"), obstetrics, definitive observation, and coronary care, among others. The general medical/surgical acute care unit treats patients of lower acuity relative to general medical/surgical intensive care. In 2000, 357 hospitals in California reported providing inpatient care in a general medical/surgical acute care unit (hereafter "acute care unit").[10]

Acute care constituted 46 percent of total inpatient days and 59 percent of total hospital discharges at non-psychiatric, non-specialty hospitals. The majority of patients spend some time in acute care during their inpatient stay and are discharged from acute care. Acute care attends to pre- and post-surgical patients and stroke, heart attack, and pneumonia patients, among others.

Licensed nurses are a central input into the production of healthcare services for these patients. Nurses' salaries constituted 80 percent of the non-physician wage bill, 73 percent of the wage bill including physicians, and 28 percent of total costs in the acute care unit prior to the mandate.[11]

Nurses are viewed as not only central to the volume of services provided but also to the quality of care per patient day. Nurses have a variety of tasks including "(1) ongoing monitoring and assessment of their patients, and, as necessary, initiating interventions to

[10]Statistic includes hospitals reporting nonzero and nonmissing patient days and nursing hours in the medical/surgical acute care unit in 2000. Statistic excludes Kaiser Permanente hospitals which were not required to report hospital-level financials to HCAI until the fiscal year ended 12/31/2021.

[11]These statistics are consistent with hospital-wide figures from other sources, for example Welton (2011) who finds that nurses' labor costs constitute 30.1 percent of total costs.

address complications or reduce risk; (2) coordinating care delivered by other providers; and (3) educating patients and family members for discharge, which can reduce the risk of posthospital complications and readmission” (Needleman and Hassmiller, 2009). How nurses affect quality of care therefore depends on the hospital unit in which they work and the measure of quality that is used. Nurses in intensive care, where patients are stabilized prior to being transferred to acute care and where serious complications are more likely to occur, may play an outsized role in addressing complications. On the other hand, nurses in acute care, the point of discharge from the hospital for most patients, may play a larger role in educating patients for discharge and preventing unplanned readmission. As I discuss in Section 4.5, these institutional details are important for the contextualization of the results.

2.1.1. Variation in nursing skill—Hospitals choose the skill of nursing hours to employ in addition to the quantity. There are two types of licensed nurses in the U.S. RNs are the higher-licensed, higher-skilled nurse and receive at minimum one to four years of training culminating in either a diploma from a nursing program (one to three years of training), an Associate of Applied Science in Registered Nursing degree (two years), or a Bachelor of Science in Nursing degree (four years). 87 percent of RNs employed in a California hospital in the early 2000s reported having an Associate’s degree or higher. LVNs, also known as Licensed Practical Nurses (LPNs) in other states, are the lower-licensed nurse and receive at minimum one year of training leading to a diploma or certificate in practical nursing. Each type of licensed nurse is required to pass a separate national licensing exam and is subject to different scope of practice regulations that restrict their tasks within the hospital setting.

In 2000, the average acute care RN hourly wage in my data was 63 percent higher than the average acute care LVN hourly wage within the same unit in the same hospital reflecting in part variation in skill. Evidence from the economics (Bartel et al., 2014) and nursing literatures (Needleman et al., 2006; Lankshear et al., 2005) indicates that LVNs are less productive than RNs when it comes to patient health outcomes.

2.1.2. Pre-existing regulatory constraints—Prior to the mandate, the hospital’s staffing choices were already constrained in a few ways. First, state-level scope of practice regulations by licensing type formally limit the degree of substitution between RNs and LVNs. LVNs must be supervised by a physician, RN, or Advanced Practice RN whereas RNs are considered independent practitioners meaning they do not need to be supervised if they are within their scope of practice. For LVNs, scope of practice consists of the following tasks: direct services related to daily living activities (e.g. provide baths to or feed patients), administer medication including injections and immunizations, conduct skin tests, and draw blood. RN scope of practice includes all of the tasks listed for LVNs and additional tasks (NursingExplorer, 2000).

Second, state-level legislation passed during the 1976–1977 legislative session established minimum nurse-to-patient ratios for intensive care, operating room, and neonatal nurseries (Dilcher, 1999). These ratios additionally specified that RNs should comprise at least 50 percent of the mandated licensed nursing hours. For these hospital units, the ratios and RN share specifications legislated under the 1999 mandate were identical to the ones passed in

1976–1977. Therefore, I exploit the variation in the “bite” of the mandate between the acute and intensive care units for identification under the assumption that the intensive care unit should be unaffected by the mandate barring spillover effects across hospital units.

Third, revisions to Title 22 of the California Code in 1996 required hospitals to submit staffing plans to the state that would specify the number of licensed nurses and unlicensed aides that would be allocated to a unit based on the patient severity in the unit at any given time (Title 22, Division 5, Ch 1, Section 70053.1, p. 761). These staffing plans are known as patient classification systems (PCS). Descriptive evidence suggests that PCS reporting did not constitute a legitimate constraint to the hospital’s staffing choice (Spetz et al., 2000) in part because each hospital established its own staffing plan by which it had to abide. However, the design of the PCS is indicative that hospital staffing and costs generally increase in patient severity. In Section 3.1.2, I discuss my use of a hospital-level patient severity index to control for variation in staffing and cost trends over time.

2.2. 1999 California nurse staffing mandate

The 1999 California nurse staffing mandate (AB 394) was passed after several unsuccessful attempts at state-level healthcare staffing legislation in the 1990s. These unsuccessful attempts include AB 1445, Proposition 216, and AB 695 all of which would have established minimum nurse-to-patient ratios in hospitals.[12] These bills were spearheaded by California’s primary nurse union, the California Nurses Association (CNA). The rise of managed care insurers in the 1980s and 1990s and subsequent increases in inpatient acuity and declines in nursing staff were often cited as reasons for the perceived low staffing ratios. CNA argued that low staffing created unsafe environments for patients and that the state should mandate minimum ratios (Purdum, 1999; Spetz et al., 2000).

AB 394 was introduced in the legislature on February 11, 1999. The original version of the bill specified within the text the minimum nurse-to-patient ratios that hospitals would need to adhere to. However, the bill was amended in June 1999 to instead direct the Department of Health Services (DHS) to establish the ratios by licensed nurse classification (RN, LVN) and hospital unit after a public comment period. The June amendment specified that DHS would need to establish the ratios by March 1, 2000 but another amendment in August pushed the deadline back to January 1, 2001. The bill was signed into law by Gov. Gray Davis on October 10, 1999 but only under the agreement that the measure’s sponsor in the State Assembly would delay the DHS deadline further by at least one year to January 1, 2002 (Purdum, 1999). The implementation date was not specified in the original or final versions of the bill. Therefore at the time of its passage in October 1999, hospitals knew that draft minimum ratios would be announced no earlier than January 2002 for an implementation date down the road.

Gov. Davis announced draft ratios created by DHS on January 22, 2002. At the time of his announcement, it was publicly known that the draft ratios could be changed following the public comment period and that the final ratios would not be implemented until January

[12]AB 1445 failed in committee in 1992–1993 legislative session. Proposition 216 was rejected by voters in 1996. AB 695 was approved by the legislature but vetoed by Gov. Pete Wilson in 1997–1998 legislative session.

2004 (Ellis and Warren, 2002). Final ratios were announced more than one year later in July 2003. Implementation deadlines were staggered by hospital unit and began on January 1, 2004. In acute care, hospitals had to implement a 0.16 nurse-to-patient ratio by January 1, 2004 and a 0.2 ratio by January 1, 2005. In intensive care, hospitals had to implement a 0.5 ratio by January 1, 2004. However, as previously mentioned, state regulation in place since the late 1970s already required a 0.5 ratio in intensive care. The full timeline of the public comment and implementation periods is shown in Fig. 1.

One additional feature of the mandate was that it specified that LVNs could only make up 50 percent of the mandated nursing hours and LVNs would not count towards mandated nursing hours in some intensive care units, for example the neonatal intensive care unit. In practice, the LVN share mandate was not binding for the vast majority of hospitals. In my sample, the 10th, 50th, and 90th percentiles of hospitals by LVN share in acute care in 2000 were 0.009, 0.130, and 0.323, respectively. LVNs were used even less frequently in the intensive care unit consistent with existing state regulation. As a result, I do not consider the regulation on LVN share to be a binding constraint to the hospital in either unit.

2.2.1. Penalties and allowances for special circumstances—During my sample period, there were no specified administrative penalties for non-compliant hospitals.[13] However, nurses were encouraged by nursing unions to report out-of-ratio deficiencies to the California Department of Public Health (CDPH) which would issue citations to the non-compliant hospital and issue penalties if the deficiency put patients in "immediate jeopardy".[14] Fig. A.4(b) presents a histogram of the unadjusted nurse-to-patient ratio in 2006. It indicates that seven of 212 hospitals in my balanced sample were non-compliant on average. One can think of this as a lower bound on the number of cases of non-compliance given that hospitals were required to be in compliance 24/7. Nonetheless, it suggests that hospitals were for the most part complying with the policy, perhaps due to the reputational harm associated with public disclosure of out-of-ratio deficiencies.

The mandate made allowances for special circumstances for university hospitals, rural hospitals, and county hospitals. Rural general acute care hospitals meeting Section 70059.1 of Title 22 of the California Code of Regulations were eligible to request for and obtain waivers (text of AB 394). Terasawa (2016) states that 38 rural hospitals were granted waivers. In my sample, I observe 62 hospitals designated by DHS as small and rural, which suggests that the majority of these hospitals obtained waivers. University of California teaching hospitals were mentioned to ensure that the staffing ratios were "consistent with Board of Registered Nursing approved nursing education requirements" but, as far as I am aware, were not exempt from the policy. Finally, county hospitals were accorded a one year phase-in-process beyond the general deadline.

[13]The California Department of Public Health (CDPH) began issuing administrative penalties for non-compliance with the ratios only beginning January 1, 2020 following the passage of SB 227. The financial penalties associated with SB 227 are $15,000 for the first violation and $30,000 for every subsequent violation.

[14]Statistics on the numbers of out-of-ratio deficiencies vary widely. One source states that there were 235 deficiencies reported to CDPH between January 2007 and October 2012 of which five were related to staffing (NurseRecruiter, 2012). Another states that between 2008 and 2017 there were 634 out-of-ratio deficiencies reported to CDPH (Larson, 2019).

2.3. Nursing labor supply

The mandated ratios were announced a few years after the Government Accountability Office declared a nationwide RN labor shortage to which California was no exception. Several facts about the RN labor market in 2000 are indicative of a shortage: the nationwide RN unemployment rate declined to one percent (its lowest point in over a decade), 82 percent of licensed RNs nationwide were employed in nursing, and the average RN vacancy rate in California was 20 percent (GAO, 2001). Therefore it is unlikely that the growth in hospital nursing hours that I will show were drawn from trained nurses that were either unemployed, out of the labor force, or employed in non-nursing settings.

However, the California nursing labor force grew significantly in the 2000s after the announcement of the shortage. I use licensing data from the National Council of State Boards of Nursing (NCSBN) to plot the numbers of licensed nurses by year. In Fig. A.1(a), I plot the average number of active nurse (RN and LVN) licenses per 100 persons in California and other states. Fig. A.1(b) plots same measure for each group normalized to that group's 1996 value. The dashed red line at 2003 represents the event date used in my main analysis. The dashed blue line at 2000 represents the date that a nursing shortage was announced. These figures show a rapid growth in active licenses per capita in California compared to other states between 2000 and 2010.

The growth in active licenses per capita could be coming from an increase in the rate of renewals (nurses choosing to stay in the nursing labor force) or an increase in the rate of new entrants (nurses choosing to enter) where each may have different implications for the skill level of the resulting labor force. In Fig. A.2, I plot entrants as a share of active licenses. I show that the labor force growth was largely due to an increase in new entrants.

In Fig. A.3, I plot the numbers of newly-licensed RNs that obtained licenses through examination in California or endorsement from out-of-state for each year between 1996 and 2014. Fig. A.3 suggests the increase in new entrants was from a combination of nurses being endorsed from out-of-state and nurses being examined in-state.[15] These facts are consistent with other descriptive evidence indicating a growth in nurses educated in the state. Between 2000 and 2007, California added 26 public or private nursing programs (25 percent increase) and total enrollment at these and existing institutions increased by around 25 percent. State funding increased significantly for the University of California, California State University, and California Community College systems to expand enrollment in their nursing programs (LAO, 2007).

Taken together, the descriptive evidence suggests that the nurse expansion that I will show was plausibly driven by the expansion in the labor force from both nurses entering from new entrants rather than a reallocation of nurses across hospitals.

[15]California follows a single-state licensing format in which RNs and LVNs with licenses in other states must pass the national licensing examination, pass a background check, and show proof of completion for a nursing program that meets state requirements in order to be endorsed to practice in California (LAO, 2007). It is notable that in 2000, four states passed a Nursing Licensure Compact (NLC) into law that would allow mutual recognition of nursing licenses across states. This increased mobility of the nursing labor force across states, however, California was not and still does not participate in the NLC.

3. Data and empirical framework

3.1. Data and variable construction

3.1.1. Hospital financial data—I utilize data on input quantity, output quantity, cost, and hospital characteristics publicly available from HCAI's Hospital Annual Financial Disclosure Reports and Pivot Tables. The Hospital Annual Financial Disclosure Reports that I use contain financial data reported for each hospital unit, hospital, and fiscal year. I convert the data from fiscal to calendar year using the beginning and end dates of the fiscal year reporting period specified by each hospital. My sample consists of calendar years 1990–2016. In my main specification, I restrict to years 1996–2016 for which I can link my sample of hospitals to publicly available data from HCAI on patient severity for each hospital and year. In a robustness check, I utilize years 1990–2016 to show longer pre-mandate trends for outcomes unadjusted for patient severity. All of the outcome variables are winsorized at the 2 and 98 percentiles by year.

In all specifications, I restrict my sample to hospitals with nonmissing, nonzero patient days and nursing hours in acute care for every year of my sample period. My sample necessarily excludes hospitals that enter or exit. I remove hospitals labeled as small and rural by DHS because I do not observe which of these hospitals were granted waivers. Additionally, low-volume hospitals face well-documented variance in admissions and case mix (Dalton et al., 2003) that imply differential staffing trends. I remove all Kaiser hospitals because they were not required to report hospital-level statistics to HCAI until fiscal year end 12/31/2021 due to legislative exemption. My final sample for my main specification after these exclusions consists of 212 hospitals which comprise 74 percent of the acute care patient days over my sample period. The sample offers broad coverage.

The HCAI financial data are notable in a few respects. First, the data are reported separately for each hospital unit within a hospital which allows me to use hospital units that should be unaffected by the mandate due to pre-existing regulation as a placebo test. Second, labor quantities are reported in hours rather than number of full-time equivalent employees and labor quantities are reported for RNs separately from LVNs and registry nurses. This allows me to precisely measure nursing labor by skill level.

3.1.2. Patient severity data—I link these data to publicly available data on patient severity from HCAI between 1996–2016 to control for differential staffing and cost trends.[16] As mentioned in Section 2.1.2, hospital staffing and costs generally increase in patient severity. The reimbursement system for hospitals used by government payors reflects this. Centers for Medicare and Medicaid Services (CMS) uses the CMI to increase Medicare reimbursement rates for hospitals with more severe patients. A higher CMI reflects a case mix that is more resource-intensive.[17] CMS and HCAI both produce hospital-level CMI

[16]Data for 1996–2007 are at the hospital-calendar year level and data from 2008–2016 are at the hospital-federal fiscal year level. Regardless, the latter data are linked to the hospital financial data by calendar year.

[17]According to HCAI: "CMI is the average relative DRG weight of a hospital's inpatient discharges, calculated by summing the Medicare Severity-Diagnosis Related Group (MS-DRG) weight for each discharge and dividing the total by the number of discharges. The CMI reflects the diversity, clinical complexity, and resource needs of all the patients in the hospital. A higher CMI indicates a more complex and resource-intensive case load".

data for California hospitals, however, HCAI uses all payor claims while CMS uses only Medicare claims to produce the index. In this paper, I use the HCAI index to control for differential trends. Most hospitals in my sample attribute less than 15 percent of their patient days to Medicare or Medicaid payors therefore the HCAI index is a far more accurate measure of patient severity than the CMS index.[18]

I show patient severity-adjusted and unadjusted outcomes for labor inputs (nurses, RNs, LVNs, aides, productive staff, and physicians) and patient severity-adjusted outcomes for costs. I construct adjusted ratios by dividing the number of hours reported for the occupation by (24*number of patient days*CMI). The logged patient severity-adjusted costs per patient day are calculated by dividing the costs by (number of patient days*CMI) and logging the fraction. The other outcome variables (output quantity, logged wages) are unadjusted for patient severity. In the robustness check in Section 6.1, all outcomes are unadjusted because patient severity data is unavailable prior to 1996. Prior literature documents the role of patient severity in hospital costs (Hornbrook and Monheit, 1985; Martin et al., 1984) and adjusts for patient severity whether with CMI (Jensen and Morrisey, 1986; McHugh et al., 2011) or other measures (Spetz et al., 2013; Mark et al., 2013). I show in Section 4.1 that my results on nurse labor use are very similar to those estimated in prior work regardless of the patient severity measure used.

3.1.3. Labor market data—I use quadrennial survey data between 1977 and 2018 from the National Sample Survey of Registered Nurses (NSSRN) and annual licensure data for RNs and LVNs between 1996 and 2014 from the NCSBN to investigate changes in nurse composition in California hospitals as a consequence of the mandate. The NSSRN surveys active RN license holders in the U.S. on their employment, wage, and socioeconomic and demographic characteristics and contains geographic identifiers at the county-level through 2008 and at the state-level through 2018. To study changes in composition of hospital RNs across states, I restrict the sample to respondents that reported being employed as a nurse in a hospital setting at the time of the survey.

The NCSBN publishes annual state-level statistics on the numbers of newly-licensed and active licenses for RNs and LVNs. The newly licensed are delineated into those examined in-state and those whose out-of-state licenses were endorsed.

3.1.4. Quality data—Finally, to estimate the effects on quality I use administrative data on patient discharges from HCAI. These data contain the date of admission, date of discharge, hospital, primary and secondary diagnoses, primary and secondary procedures, patient characteristics including a patient identifier, and status on discharge for each discharge at a California general acute care hospital between 1995 and 2008.

I identify index admissions for acute myocardial infarction (AMI) following the procedures in CMS (2008) and Chandra et al. (2016b) and for each of these admissions obtain the length of stay and whether or not the patient was readmitted to the hospital for any

[18]Most hospitals are not designated Disproportionate Share Hospitals (DSH). DSH have over 15 percent of patient days paid for my Medicare or Medicaid. In my California sample in 2000, below 30 percent of hospitals were DSH.

cause within 30 days of the discharge date. For the in-hospital mortality measure, I use a larger sample of AMI admissions that include non-index admissions and admissions where the patient died in hospital (both of which are excluded from the sample of admissions used to measure length of stay and 30-day readmissions). For each of these admissions, I obtain whether or not the patient died in hospital. I follow Chandra et al. (2016a) in constructing risk-adjusters: a series of gender, race, and age group interacted indicators and indicators for whether the patient was admitted to a hospital in the year prior to the index admission for each of 25 conditions. To obtain risk-adjusted length of stay, readmission rates, and in-hospital mortality rates, I follow Grieco and McDevitt (2017) and estimate patient admission-level regressions of the unadjusted variable on the set of risk-adjusters and obtain the residuals from these regressions as the risk-adjusted outcomes.

3.2. Difference-in-differences

My analytic framework is centered around a difference-in-differences model comparing outcomes in the acute care unit of hospitals that were below and above the mandated nurse-to-patient ratio threshold in acute care. My basic estimating equation for a hospital i in year t is

$$y_{it} = \beta_0 + \beta_1 BELOW_i * POST_t + \gamma_i + \xi_t + \epsilon_{it} \tag{1}$$

where the outcomes (y_{it}) are measures of the hospital's input quantities, output quantities, logged wages, and logged costs. γ_i and ξ_t denote hospital and year fixed effects. I scale the input quantities and costs by patient days. Therefore, I use as outcomes the nurse-to-patient, RN-to-patient, LVN-to-patient, aide-to-patient, and productive staff-to-patient ratios and costs per patient day.

The indicator variable $BELOW_i$ takes the value of one if the hospital's average, unadjusted nurse-to-patient ratio from 2000–2002 was below the mandated threshold for the acute care unit. Hospitals with $BELOW_i = 1$ are treated. The observed ratios are annual averages, however, the ratios had to be adhered to on a 24/7 continuous basis. I therefore use a threshold of 0.25 rather than 0.2 (as mandated) to be inclusive of hospitals that could have found the mandate binding at least one point in the year if not on average.[19] The same two groups of hospitals are tracked over time using a balanced sample.

The indicator variable $POST_t$ takes the value of one if the observation is in or after calendar year 2004. I consider the event to take place in 2003 because it is when hospitals learned the final mandated ratios from DHS.[20]

My treatment assignment is intended to capture whether the hospital would find the mandated constraint binding or not. It therefore relies on the stability of the nurse-to-patient ratio at a hospital over time. I find that 87 percent of my treated hospitals would have been classified as treated based on their 1996, 1997, 1998, or 1999 ratios. A smaller majority of

[19]My findings are robust to using a threshold of 0.2 rather than 0.25.

[20]Prior to 2003, several sources including SEIU, CNA, and California Hospital Association published proposed ratios and DHS published draft ratios. However, hospitals knew these could be changed. In Fig. 3(b), I show that there is no anticipation of the final ratios by hospitals below the threshold which do not increase their staffing until the final ratios were announced.

my control hospitals, 60 percent, would have been classified as control based on their earlier ratios. That control hospitals were less likely to be classified in the control group based on their ratios in earlier years is unsurprising given the upward staffing trend in the unadjusted ratio among control hospitals shown in Fig. A.11 (the upward trend from 1996 to 2003 is largely explained by an increase in patient severity). On the other hand, the ratio at treated hospitals was stable between 1990 and 2003.

The ratio is not sufficiently stable, however, to employ a kinked treatment variable with continuous treatment below the threshold. Only 62 percent of the variation in the unadjusted nurse-to-patient ratio prior to 2003 is due to time-invariant differences across hospitals and the use of a continuous treatment variable in this setting would likely lead to attenuation bias from measurement error in the independent variable.

I estimate Specification (1) on the sample of 212 hospitals over three time periods: short-term (1996–2006), medium-term (1996–2010), and long-term (1996–2016).

3.3. Event study

I also estimate the following event-study specification

$$y_{it} = \alpha_0 + \sum_{t \neq 2003} \alpha_t \{YEAR_t = t\} * BELOW_i + \gamma_i + \xi_t + \epsilon_{it} \tag{2}$$

The coefficients α_t reflect the relationship between the outcome and the treatment across years relative to the omitted year, $t = 2003$. The event-study estimates α_t for $t \in \{1996, 2016\}$ will allow me to graphically inspect the identifying parallel trends assumption (α_t should be not be statistically different from zero for $t \in \{1996, 2002\}$) and any evidence of mean reversion in the treated group (Ashenfelter and Card, 1985; Heckman et al., 1999). Additionally, estimation of α_t for the 13-year post-period of my data allows me to confirm that the estimated effects on staffing are permanent as we would expect given that the policy remained in place through the end of the sample period. Finally, the event-study estimates provide evidence of dynamic treatment effects which I show to be important for healthcare quality in particular.

3.4. Research design validity

The identifying assumption for my empirical strategy is that the outcomes that I analyze would have evolved on parallel trends for hospitals above and below the minimum ratio threshold in the absence of the mandate. In Fig. 2, I show that the hospitals in the two groups are often found in the same geographic markets which limits confounding variation from shocks to institutions or market structure. The notable exceptions are small and rural hospitals which are more likely to be above the threshold for reasons mentioned earlier. I include these hospitals in Fig. 2 but they are excluded from my analysis.

In Table 1, I show a balance test of hospital characteristics by group for my balanced sample. Hospitals above the threshold are significantly more likely to be church or non-profit owned, higher cost, higher revenue, and lower profit than hospitals below the threshold. They are more likely to have third-party payors (any payors that are not Medicare, MediCal, County Indigent, or charity payors). These correlations between nurse staffing, non-profit status, cost, and payor share at the provider level have been noted in several studies (Jha et al., 2009; Seago et al., 2004; Mark and Harless, 2007).

In Table A.1, I separate the sample of hospitals with initial nurse-to-patient ratio below 0.25 into three groups to study heterogeneity among treated hospitals. Table A.1 indicates that when we break up the hospitals below 0.25 into three groups, the individual comparisons between each of these groups and the above 0.25 group along ownership, revenue, cost, and profit dimensions (Columns 5–7 of Table A.1) largely confirm the findings from Table 1. The lowest staffing hospitals are for-profit, lowest cost, and lowest revenue though they are not necessarily higher profit. They have a larger share of patient days coming from MediCal and a smaller share coming from third-party payors.

It is also notable that higher staffing hospitals are on average higher patient acuity and lower volume. These differences are not statistically significant in Table 1 or Table A.1 but neither are they precisely estimated to be zero. The correlation between patient severity and staffing motivates the use of the Case Mix Index to control for differential trends in severity between groups. Lower volume hospitals have higher variance in both admissions and patient severity that may lead to higher staffing ratios on average.

In Fig. 3(b), I show that any level differences in nurse staffing are not linked to trend differences. The event-study coefficients and raw means that I present in the remaining figures in this paper allow for graphical inspection of the identifying assumption.

4. Results

4.1. Labor inputs

Table 2 presents the effects of the mandate on nurse labor. In each column I show estimates of the coefficient of interest, $\hat{\beta}_1$, from the estimation of Specification (1) over three time periods: short-term (1996–2006), medium-term (1996–2010), and long-term (1996–2016) (hereafter “Model 1”, “Model 2”, and “Model 3”, respectively). In Columns 1, 3, and 5, I present the unadjusted nurse-, RN-, and LVN-to-patient ratios as outcomes. In Columns 2, 4, and 6, I present the patient severity-adjusted ratios.

My preferred models utilize the medium-term sample (Model 2) and the patient severity adjusted outcomes (Columns 2, 4, and 6). Column 2, Model 2 indicates that the mandate led to a 0.040 point increase in the adjusted nurse-to-patient ratio of treated hospitals on a mean of 0.192. This corresponds to a 58 min increase in nursing time per patient day.[21] Columns 4 and 6, Model 2 indicate 0.027 and 0.015 point increases in the RN- and LVN-to-patient ratios, corresponding to 39 and 22 min increases in RN and LVN time per patient day.[22]

[21]This is 0.025 and 36 min based on Column 1. I obtain this figure as follows. 0.025 is the increase in the number of nursing hours per patient hour. I multiply 0.025 by 60 min per hour to obtain the increase in the number of nursing minutes per patient hour (1.5 min per patient hour). I then multiply this by 24 h per patient day to obtain the increase in the number of nursing minutes per patient day (36 min per patient day).

[22]Columns 3 and 5 indicate 0.012 and 0.015 point increases in the unadjusted RN- and LVN-to-patient ratios, corresponding to 17 and 22 min increases in RN and LVN time per patient day.

The event-study estimates for the patient severity-adjusted nurse-, RN-, and LVN-to-patient ratios are shown in Figs. 3(a), 4(a), and 5(a) and raw means for each group with standard error bands in Figs. 3(b), 4(b), and 5(b). Figs. 3–5 show that there are no differential pre-trends in staffing between the two groups prior to the mandate. In Appendix Fig. A.5, I present a version of Fig. 3 that estimates my model on a restricted sample of hospitals with initial ratios between 0.2 and 0.3 to alleviate concerns that the treated and control groups differ from one another in levels, particularly on the margins. My main results are robust to estimation on this restricted sample of hospitals.[23]

My estimates are similar in magnitude to prior papers with causal estimates on nurse labor (Spetz et al., 2013; Cook et al., 2012; Mark et al., 2013; Munnich, 2014). Spetz et al. (2013) find a 69 min increase in nursing time per severity adjusted patient day among the bottom quartile of hospitals by initial staffing level. Mark et al. (2013) find a 15 percent increase in nursing time per adjusted patient day among the bottom quartile. Munnich (2014) finds a 5.3 percent increase in RN time per unadjusted patient day among the bottom quartile. I find a corresponding 58 min or 21 percent increase in adjusted nursing time and 8 percent increase in unadjusted RN time among my sample of treated hospitals which includes but is not limited to the bottom quartile. Cook et al. (2012) find a 58 min increase in nursing time per unadjusted patient day for a hospital with an initial nurse-to-patient ratio of 0.15. I find a 48 min increase per unadjusted patient day for hospitals with an initial ratio below 0.19 (average initial ratio of 0.18) as indicated in Table 10.

My findings indicate that 33 percent of the increase in nursing time came from lower-licensed LVNs confirming findings in prior work (Mark et al., 2013; Spetz et al., 2013; Cook et al., 2012). I posit that this has implications for care quality. Evidence from the economics (Bartel et al., 2014) and nursing literatures (Needleman et al., 2006; Lankshear et al., 2005) indicates that LVNs are less productive than RNs when it comes to improving patient health outcomes. It is important to keep this margin of adjustment in mind when thinking about the quality implications of minimum ratios, particularly in settings outside of California where hospitals' use of LVNs may see larger increases under more relaxed scope of practice regulations.

Table 3 presents the effects on aides (Columns 1–2), all productive staff including nurses but excluding physicians (Columns 3–4), and physicians (Columns 5–6). In Columns 2, 4, and 6, I present patient severity-adjusted aide-to-patient ratio, productive staff-to-patient ratio, and logged physician expenditures per adjusted patient day.[24] In Columns 1, 3, and 5, I present the unadjusted outcomes.

Column 2, Model 2 and Column 4, Model 2 indicate that the mandate led to a statistically insignificant decline in the aide-to-patient ratio and an increase in the productive staff-to-patient ratio. The estimated effects on the patient severity adjusted aide-to-patient ratio

[23]Furthermore, in Appendix Fig. A.6, I present the event-study estimates and raw means for the unadjusted nurse-to-patient ratio. Comparing Fig. 3 with Appendix Fig. A.6, we see that controlling for patient severity addresses differential staffing trends in the pre-mandate period as we would expect given the linkage between patient acuity and staffing.

[24]Physicians are most often contracted rather than directly employed by the hospital. Therefore HCAI requires that their wage bill be recorded under professional fees but does not require that their total hours are reported.

(Column 2) are not significant while the estimated effects on the unadjusted ratio (Column 1) are significant in the short- and medium-terms. Taken together, the results in Columns 1 and 2 suggest that licensed nurse labor and unlicensed aide labor appear to be partly substitutable in production. The positive shock to nurse labor may have led to nurses taking on some of the tasks of unlicensed aides after the mandate which is consistent with prior work (Cook et al., 2012; Chapman et al., 2009).

Column 6, Model 2 indicates a statistically insignificant decline in physician expenditures per patient day of 3.6 percent with the effects reversed in the long-term. These findings broadly suggest that nurse and non-nurse labor (aides, physicians) have limited substitutability in healthcare production at the observed wages.

Column 4, Model 2 indicates that the productive staff-to-patient ratio increased by 0.036 points or 52 min per patient day. This is less than the 58 min increase in nursing time which reflects some small substitution away from other labor inputs. Importantly, Munnich (2014) finds evidence that RNs employed in management roles were reclassified into clinical roles following the mandate which could explain part of this substitution in addition to the evidence that I documented above.

The limited substitutability between nurse and non-nurse labor is consistent with strict scope of practice regulations in California that specify the tasks that each licensed healthcare professional is allowed to perform in the hospital setting. Given that scope of practice regulations vary widely from state to state, the substitution patterns in response to minimum ratios in other states are also likely to vary.[25]

4.2. Wages and nurse composition

Table 4 presents the effects on the RN, LVN, and non-nurse real hourly wages in acute care. The event-study coefficients and raw means for RN wages are presented in Figs. 6(a) and 6(b). RN wages at treated hospitals saw a significant decline due to the mandate. Column 1, Model 2 indicates that in the medium-term RN wages at treated hospitals declined by 3.3 percent. If we compare across Models 1, 2, and 3 in Column 1 we see that the wage gap widens over time from 1.7 percent and insignificant in the short-term to 5.1 percent and significant in the long-term. LVN wages also declined but by a smaller magnitude (2.4 percent in the medium-term) and the decline is not statistically significant. Non-nurse wages appear to be unaffected.

My research design focuses on identifying the wage effect on treated hospitals. In the next section, I posit that changes in nurse composition may be driving the observed decline in RN wages at treated hospitals. The wage effects of the mandate on treated hospitals driven by this channel (changes in nurse composition) are distinct from any general equilibrium

[25]Anecdotal evidence has shown that the scope of practice for unlicensed aides varies widely across states and healthcare settings with some states and settings, including acute care hospitals in California, limiting unlicensed aides to performing nonnursing functions whereas in other cases aides perform nursing functions including the administration of medications (Huston, 2013). Similarly, the scope of practice for LVNs also varies across states. Most states require that LVNs or LPNs work under the supervision of an RN, physician, or other health care practitioner. While some states (Louisiana, Montana, Maine, Nevada) include lists of tasks LPNs cannot perform in their nurse practice acts, other states (Alabama, Georgia, Alaska, Kentucky, Oklahoma) use “decision trees” to guide LPN practice. Some states (Michigan, Texas) have not defined LPN scope of practice at all (Seago et al., 2006).

wage effects on all California hospitals (treated and control) driven by an outward shift in the labor demand curve. The general equilibrium wage effects of the mandate have been estimated by several papers (Mark et al., 2009; Munnich, 2014; Harless, 2019) but are not the focus of this work. In Appendix A.1, I find inconclusive results on the general equilibrium wage effects of the mandate and show that these effects, if any, were small in magnitude and consistent with the relatively small size of the labor demand shock.

4.2.1. Mechanism — changes in RN composition—In this section, I posit that the wage decline at treated hospitals may be due to a change in composition towards less experienced RNs. Unfortunately I do not observe the RN wage distribution within hospitals in the HCAI financial data, which limits my ability to comment on the mechanisms. However, I use a number of alternate data sources including union contracts, survey data from the NSSRN, and licensing data from the NCSBN to illustrate that changes in RN composition plausibly explain the wage decline. Additionally, I show that a plausible alternate mechanism (an increase in the amenity value of working at treated hospitals following the mandate) is not supported by empirical facts.

Fig. 6(b) shows that RN wages at the two groups of hospitals were for the most part statistically indistinguishable prior to the mandate. First, in Appendix A.2, I compute that if the entirety wage decline were due to differences in worker composition between incumbent nurses (hired before the mandate) and new hire nurses (hired after the mandate) rather than differences in wages between the treatment and control groups for the same worker type (incumbent or new) then the incumbent wage must have been 42 percent higher than the new hire wage.

Second, I show that within-hospital RN wage range of this magnitude is plausible using data from union contracts from the early 2000s and using data from the NSSRN. The average range of within-hospital RN wages in the union contracts I analyze is 52 percent within position and education level (e.g. an RN with the title of “charge nurse” and holding an Associate’s degree) between the entry-level nurse and nurse with 20+ years of experience. I find even wider ranges for within-hospital RN wage variation in the union contracts when I do not control for education and position. This wage range implies that a 42 percent higher wage among incumbents corresponds to roughly 16 additional years of experience.

Data from the NSSRN is consistent with the wage variation found in the union contracts. In Table 5, I present the average hourly wage of California hospital RNs surveyed in 2000, 2004, or 2008 by age-education bin. The range of RN wages within education bin is between 35 and 80 percent.

Third, I find using data from the NSSRN that there were aggregate changes in RN composition at California hospitals towards younger and more recently licensed nurses. In Table 6, I present the shares of hospital RNs by age, experience, and education in California and averaged across other states. It is notable that the share of hospital RNs under 35 declines from 34 to 28 percent in California (18 percent decline) but from 39 to 26 percent in other states (33 percent decline) between the pre- and post-mandate periods. The California workforce shifted towards younger RNs relative to other states. Also notable is the relative increase in the share of RNs licensed in the past 10 years where the share remained constant in California but declined from 38 to 31 percent (18 percent decline) in other states. Taken together, Tables 5 and 6 provide suggestive evidence that there was a change in composition towards younger, more recently licensed nurses at California hospitals after the mandate.

Fourth, evidence from the licensing data is supportive of this hypothesis. In Figs. A.1 and A.2, I showed that the California nursing labor force grew significantly in the 2000s due to an increase in new entrants. I discussed in Section 2.3 that given the labor shortage it is unlikely that the growth in hospital nurses was drawn from trained nurses who were unemployed, out of labor force, or employed in non-nursing settings. I additionally find that both control hospitals in my balanced sample and hospitals outside of my sample increased their aggregate nursing labor demand over this period making it unlikely that nurse labor was simply reallocated across hospitals within the state.

In Section 4.2, I discussed the quality implications of changes in nurse composition towards lower-licensed LVNs. The quality implications of changes in RN composition towards younger and more recently licensed RNs, however, are unclear. The lower wage of these workers does not necessarily imply lower marginal product with respect to care quality in settings where hospitals are not incentivized to improve quality or in settings with high unionization rates. Based on the NSSRN data, I find that the unionization rate among RNs employed in California hospitals is 44 percent.

Furthermore, prior work in the nursing literature is inconclusive about the quality returns to experience in nursing (Dunton et al., 2007; Aiken et al., 2003). In the economics literature, Bartel et al. (2014) find positive hospital unit-specific quality returns to tenure but no returns to tenure outside of the specific hospital and hospital unit of employment. In the mandate setting where hospitals are hiring nurses new to the hospital and unit, nurses will not have that specific human capital regardless of age.

Nonetheless, my findings suggest that if there is a quality return to experience or age in nursing then labor heterogeneity should be taken into account when minimum ratio policies are implemented. Any estimated quality effects may vary based on the composition of the labor supplied and over time as nurses gain human capital.

4.2.2. Alternate mechanism — increase in amenity value of working at treated hospitals—

In this section, I consider an alternate mechanism for the wage decline and whether there is empirical evidence to support it. There is a wealth of descriptive work in nursing on the amenity value to nurses of higher staffing ratios (Lu et al., 2019; Cheung and Ching, 2014). In this labor market, it is plausible that there are compensating differentials whereby prior to the mandate RNs employed at treated hospitals earned higher wages than RNs at control hospitals to compensate for poorer working conditions resulting from lower staffing. The mandate represented a positive shock to the amenity value of treated hospitals and in response, control hospitals would need to increase wages (or other amenities) to provide the same level of compensation as treated hospitals.[26]

If this mechanism is at play in an oligopsonistic labor market, we would expect to see a larger wage increase at control hospitals that are in proximity to treated hospitals with whom they compete for workers. I test this possibility by estimating the following specification for hospital i in year t where C is the number of treated hospitals within five or ten miles of hospital i and the indicator variable $ABOVE$ takes on a value of 1 if i is a control hospital

$$y_{it} = \beta_0 + \beta_1 ABOVE_i * POST_t + \beta_2 ABOVE_i * POST_t * C_i + \gamma_i + \xi_t + \epsilon_{it} \quad (3)$$

I use the same balanced sample as in my main analysis, however, all hospitals regardless of whether they are in the balanced sample are included in the number of treated hospitals nearby as long as they have an average nurse-to-patient ratio below 0.25 between 2000–2002. The coefficient β_1 represents the treatment effect for control hospitals without any treated hospitals within 5 or 10 miles and β_2 represents the additional wage effect associated with one additional treated hospital in the proximity.

In Table A.2, I present the results from the estimation of Specification (3). I estimate the coefficient of interest, β_2, to be close to zero and statistically insignificant implying that the wage effects do not vary with the number of nearby competitors. This is contrary to what we would expect if control hospitals raised their wages in response to an improvement in working conditions at treated hospitals after the mandate.

4.3. Non-labor inputs and costs

In Section 4.1, I documented increases in nursing time and productive staff time per patient day due to the mandate. If we consider nurse labor to be a variable rather than fixed input, we should observe an increase in marginal and average costs per patient day due to the mandate.[27]

In Table 7, I present the effects on average costs per patient day in acute care. Columns 1 and 2 show the expenditures on supplies and leases per adjusted patient day. Supply expenditures include medical inputs (surgical supplies, pharmaceuticals, radiology films) as well as non-medical inputs (linen and bedding, cleaning supplies, food). Capital expenditures include lease costs for buildings and equipment. Columns 3, 4, 5, and 6 show salaries, direct costs (salary plus non-salary expenditures), allocated costs, and total costs (direct plus allocated costs) per adjusted patient day.

Direct costs accrue directly to the hospital unit whereas allocated costs accrue to the hospital and are then allocated to each unit during financial reporting based on the unit's usage levels. For example, non-payroll employee benefits accrue to the hospital and are allocated to each unit based on the number of hospital FTEs employed by the unit. Lease and

[26]One could also think that treated hospitals needed to adjust their wages downward but I show that real wages at treated hospitals continued to grow, albeit at a slower rate than at control hospitals, after the mandate. Wages are "sticky" and difficult to adjust downwards particularly in settings such as this one where a large share of workers are unionized.

[27]My sample of hospitals excludes small and rural hospitals that face relatively inelastic labor supply curves. Therefore it is reasonable to assume that nurse labor is variable rather than fixed.

insurance costs accrue to the hospital and are allocated to each unit based on the square footage of the unit. Direct costs and allocated costs comprise total costs.

Each of the outcomes in Table 7 is adjusted for patient severity because hospitals with higher severity are expected to have higher costs for at least some components included in each cost category. As I mention in a previous section, accounting for patient severity controls for differential trends in pre-mandate staffing and costs that one might expect to vary as a function of severity. This includes salaries, patient care costs, and direct costs to the hospital unit. It may also include allocated costs to the hospital unit such as the provision of health insurance to employees of the hospital unit.

The results in Columns 1 and 2 indicate positive but statistically insignificant effects on the use of non-labor inputs (intermediate inputs, capital). The positive coefficient on supplies in the first column may be reconciled by the consideration that increasing nurse labor per patient day may increase use of supplies such as medication administered to the patient or reapplication of bandages. The magnitude is large, however, it is not statistically significant.

Unlike intermediate inputs, nurse labor and capital are difficult to reconcile as substitutes or complements in the production of patient days or care quality. It is possible that the long-term effects on leases, statistically insignificant but large, are driven by the declines in patient volumes that I document in the next section and the inability of fixed inputs to adjust downwards immediately.

Column 3, Model 2 indicates that the mandate led to a 8.7 percent increase in the wage bill of treated hospitals. The wage bill and non-salary expenditures comprise direct costs on the hospital unit. Column 4, Model 2 indicates that direct costs increased by 7.8 percent. Column 5 indicates that the increase in allocated costs is not significant in the short-, medium-, or long-terms. There are a few allocated cost components that are directly linked to the mandate: employee benefits and health insurance, nursing administration, in-service education for nurses, and licensed vocational nurse programs to train LVNs. Therefore we might have expected significant increases in allocated costs. Column 6, Model 2 indicates that average total costs, of which the wage bill comprises on average 50 percent, increased by a statistically insignificant 7.1 percent.

If we compare Models 1, 2, and 3 within each column, we see that the increase in the wage bill was immediate and remained relatively stable over time whereas the increase in average total costs becomes larger and significant in the long-term. In the next section, I show that patient days decline in the long-term and it is possible that fixed costs are unable to adjust downwards in response. These long-term movements are likely due to non-mandate factors.

4.4. Capacity and output

In Figs. 7, 8, 9, and 10, I present the event-study coefficients and raw means for available beds, patient days, discharges, and length of stay. In Table 8, I present the difference-in-differences results. Columns 1 and 2 show the results for capacity in terms of available and staffed beds.[28] Columns 3, 4, 5, and 6 show the number of patient days, bed utilization rate, number of discharges, and length of stay per discharge in days.

Column 1, Model 2 indicates a reduction in available beds by 15.6 beds on a mean of 118.2 beds (14 percent decline). Column 2, Model 2 indicates a reduction in staffed beds by 13.2 beds on a mean of 104.3 beds (13 percent decline). Column 3, Model 2 indicates that patient days declined by 1,769 patients days per year or 4.8 patients per day on a mean of 65.6 patients per day (7 percent decline) but it was not statistically significant in the medium-term. Consequently, the bed utilization rate increased by 0.045 points (8 percent). Average utilization increased from 56 to 64 percent. The increase in utilization suggests that hospitals were operating with excess bed capacity prior to the mandate and reduced capacity in response to a rise in costs per staffed bed.

If we compare Column 1, Models 1, 2, and 3, we find that the reduction in capacity was immediate and remained relatively stable over time. It therefore appears unlikely that hospitals were reducing capacity because they were unable to hire the desired nursing hours. We expect labor supply to be more elastic in the long-term therefore if the observed capacity reductions were due to short-term inelasticity of labor supply then the reductions should be temporary. Rather, the capacity reduction in response to a rise in costs per staffed bed illustrates the hospital's tradeoff between healthcare access (having a lower probability of turning patients away) and profits (having a lower cost of unused, staffed beds) as modeled in early theoretical literature (Newhouse, 1970).[29]

The capacity reductions that I document raised the average utilization rate to 64 percent. This rate remains below the optimal bed occupancy rate prescribed by policymakers (National Institute for Health and Care Excellence, 2018) and the mandated ratios were rolled back during the COVID-19 pandemic to address the surge in hospital demand (NPR, 2020). My evidence is furthermore inconclusive as to whether hospitals actually had to turn patients away due to capacity constraints. The event-study estimates in Fig. 9 indicate a decline in discharges after the mandate. However, Table 8, Column 5, Model 2 indicates that the decline in discharges in the medium-term, despite being large (11 percent), is not statistically significant. Furthermore, given the inconsistent timing of what appears to be an additional shock to discharges after 2010, my results are inconclusive as to whether the long-term effects on discharges and patient days are due to the mandate.

4.5. Healthcare quality

In this section, I estimate the effects of the mandate on care quality by analyzing length of stay, 30-day readmission, and in-hospital mortality for acute myocardial infarction (AMI).

In the event-study for acute care length of stay in Fig. 10, I show a decline in acute care length of stay following the mandate that was statistically insignificant according to the results in Table 8, Column 6, Model 2. The relationship between length of stay and care quality is ambiguous based on the medical literature (Kossovsky et al., 2002; Brasel et al.,

[28]The HCAI hospital financial reporting manual mentions that, "hospitals typically staff for those beds currently occupied by inpatients, plus an increment for unanticipated admissions". The increment for unanticipated admissions is usually a larger share of beds at low-volume or rural hospitals where there is greater variance in admissions.

[29]I show in Appendix Table A.3 that results from a heterogeneity analysis suggest that hospitals with more nearby substitutes reduced capacity by slightly more than those with fewer substitutes. The treatment effect on available and staffed beds increases as the number of substitutes for the treated hospital increases that can take on additional patients if needed (number of hospitals within five and ten miles of the treated hospital). The pattern also holds for discharges though it is not statistically significant.

2007; Spetz et al., 2013), however, prior work in economics has used shorter length of stay as an indicator of high care quality in settings where hospitals do not have incentives for premature discharge (Bartel et al., 2014). Shorter length of stay is used as an indicator because delays and errors in the delivery of care increase length of stay. At the same time, accounting for premature discharge (an indicator of low care quality during the initial inpatient stay) is important given hospitals' incentives under Medicare Prospective Payment System (PPS) (Morrisey et al., 1988) or under capacity constraints (Hoe, 2022) to discharge patients "quicker and sicker".

Premature discharge is a salient concern in my setting given my finding that the mandate leads to significant capacity reductions. To address this concern, I focus jointly on length of stay and 30-day readmission rates in addition to in-hospital mortality for AMI.

AMI is an important discharge diagnosis from regulatory and policy persectives. In the mid- to late-2000s, AMI was among the most common principal hospital discharge diagnoses for Medicare patients and the fourth most expensive condition billed to Medicare (CMS, 2008). Furthermore, AMI patients had been cited to have high all-cause 30-day readmission rates up to 28 percent (CMS, 2008) which is both costly and signals low quality of care during the initial inpatient stay. The 30-day readmission rate for AMI is also widely used as a quality indicator by researchers in health economics (Chandra et al., 2016a; Friedrich and Hackmann, 2021; Gupta et al., 2021).

I identify index admissions and construct and risk-adjust the length of stay and 30-day readmission measures for these admissions following CMS (2008) and Chandra et al. (2016a). I construct and risk-adjust the in-hospital mortality measure for a larger sample of admissions that includes admissions where the patient died in hospital (to measure in-hospital mortality) and non-index admissions. The difference-in-differences and event-study regressions for each of the measures are weighted at the hospital-year level by the hospital's share of total AMI admissions in the given year. In other words, hospitals with more AMI admissions in a given year are assigned higher weights. The event-study estimates are presented in Figs. 11, 12, and 13 and the difference-in-difference estimates are presented in Table 9.

Table 9, Columns 4 and 2 show a decline in risk-adjusted length of stay of 0.281 points (5 percent) and a statistically insignificant decline in the risk-adjusted 30-day readmission rate by 0.004 points (2 percent). The null effect on readmissions suggests that length of stay declined not because hospitals were discharging AMI patients "quicker and sicker", rather, AMI patients recovered more quickly due to an improvement in care quality per day. Table 9, Column 6 shows a statistically insignificant decline in the risk-adjusted in-hospital mortality rate by 0.003 points (4 percent).

I highlight a few important points about these findings. First, as we would expect the effects on care quality depend on the staffing shock and quality indicator used. I find a decline in length of stay with a stable readmission rate and no effect on in-hospital mortality. Understanding the average AMI patient's pathway through different hospital units during an inpatient stay suggests that this finding is reasonable. In-hospital mortality, among AMI or other patients, is far more likely to take place in intensive care than in acute care, where patients are transferred after they are stabilized. On the other hand, educating patients and families for discharge to reduce readmission risk is more likely to take place in acute care, which is the point of discharge from the hospital for most patients. The incidence of the staffing shock in my setting falls on acute care and the institutional context suggests that some quality indicators (length of stay, readmissions) are more sensitive to acute care staffing than others (in-hospital mortality).

Second, I find dynamic treatment effects on length of stay indicating that the returns to quality increase over time. My event-study estimates indicate an initial decline in the length of stay by 2.6 percent that increases to 6.9 percent and becomes significant three years after the mandate. These estimated treatment effects are consistent with a story about nurses learning on-the-job. Bartel et al. (2014) find that a 60-minute increase in RN or LVN time per patient day leads to a 3.4 or 2.9 percent decline in length of stay, respectively, and find positive, non-linear returns to tenure. Bartel et al. (2014)'s estimates imply the following declines in length of stay for the staffing increases that I document in my setting: 3.3 percent (one year after mandate), 4.0 percent (two years), 4.4 percent (three years), 4.4 percent (four years), and 4.4 percent (five years). The treatment effects are stable between years three and five because the returns to tenure level off between three and seven years before increasing again.[30] My event-study estimates from one to five years post-mandate are consistent with these approximations.

My findings complement prior literature both on the effects of minimum ratios and on the quality returns to nursing. Prior work on the mandate has used a 2004–2006 post-mandate period for estimation and found no or mixed effects on quality (Cook et al., 2012; Mark et al., 2013; Spetz et al., 2013) using failure to rescue, respiratory failure, infections due to medical care, pressure ulcer, pulmonary embolism, and postoperative sepsis as outcome measures. I focus on length of stay, 30-day readmissions, and in-hospital mortality for AMI and estimate my model over a longer time horizon that allows documented returns to tenure (Bartel et al., 2014) to kick in. My findings are therefore qualitatively consistent with prior work on the returns to nursing (Friedrich and Hackmann, 2021; Bartel et al., 2014; Gruber and Kleiner, 2012; Lin, 2014).

5. Heterogeneity analysis

In this section, I exploit heterogeneity in the treatment intensity and show that treatment effects on nurse labor scale in magnitude as we would expect if these effects were driven by the "bite" of the mandate. My basic estimating equation for a hospital i in year t is

$$y_{it} = \beta_0 + \sum_{g \in 1, 2, 3, 4} \beta_g B_i^g * POST_t + \gamma_i + \xi_t + \epsilon_{it} \tag{4}$$

[30]I obtain these calculations as follows. My estimated treatment effects on RN and LVN time per patient day are 39 and 22 min, respectively. The implied decline in length of stay for the combination of these increases in nursing time is 3.3 percent. The returns to tenure are approximated based on a 10 percentage point increase in nursing staff who are new to the unit at the time of the mandate and their progression into nursing staff with 1–2 years, 3–4 years, etc... of experience.

where B_g for $g \in \{1, 2, 3, 4\}$ are indicator variables for whether the hospital is below 0.19 $(B_1 = 1)$ between 0.19 and 0.22 $(B_2 = 1)$, between 0.22 and 0.25 $(B_3 = 1)$, or above 0.25 $(B_4 = 1)$ in initial ratio in acute care. The outcomes y_{it} are measured in acute care and γ_i and ξ_t are hospital and year fixed effects as in the main specification. I only estimate Specification (4) over the medium-term (1996–2010).

I additionally estimate the following event study specification

$$y_{it} = \beta_0 + \sum_{t \neq 2003} \sum_{g \in 1, 2, 3, 4} \alpha_{gt} B_i^g * \{YEAR_t = t\} + \gamma_i + \xi_t + \epsilon_{it} \quad (5)$$

Results from the estimation of Specification (4) are presented in Tables 10, 11, 12, 13, and 14. Event-study results from the estimation of Specification (5) for my main outcome variables (nurse-, RN-, and LVN-to-patient ratios, RN hourly wage, available beds, patient days) are presented in Fig. 14.

The results on nurse labor in Table 10, Columns 1–6 indicate that treatment effects of the mandate on nurse staffing ratios scale as we would expect. Hospitals with the lowest initial staffing ratios (below 0.19) had the largest treatment effects including a 19.3 percent increase in nurse hours. Table 11 shows similarly that the treatment effects on the productive staff-to-patient ratio increase with the distance from the threshold. The substitution away from aides, however, is decreasing in the distance from the threshold which is surprising but may reflect heterogeneity in responses.

The results in Table 12 scale as we would expect. Hospitals that hired the most nurse hours after the mandate had larger declines in the RN hourly wage as we would expect if the wage effects were driven by changes in nurse composition. The results in Tables 13 and 14 on the other hand do not scale as clearly as the results in the previous tables and indicate heterogeneity in responses. The initially lowest staffing hospitals have the largest increases in the wage bill and direct costs per patient day but the two other treated groups do not “fall in line” in terms of the magnitude of treatment effects. It is particularly the “Between 0.19 and 0.22” group that diverges unexpectedly from expectations. The same is true for the capacity and output results in Table 14 which do not appear to be larger for initially lower staffing hospitals. In fact, capacity reductions are uniform across the three treated groups.

The event-study estimates that I present in Fig. 14 show whether the parallel trends assumptions are valid for each of the treated groups with respect to the control group and whether in fact the initially highest staffing hospitals are a good control group for the initially lowest. A visual inspection of the pre-trends in Fig. 14 shows that for the most part there are not any differential pre-trends for the three treated groups relative to the control group. The exception is a shock to the nurse-to-patient ratio in 2001 that hits the initially lowest staffing hospitals harder than others.

6. Robustness checks

I conduct two robustness checks of my findings. First, I present estimated event-study coefficients from Specification (2) and raw means that utilize the full sample from 1990–2016 to show six additional years of pre-mandate trends for my main outcomes (nurse-to-patient ratio, RN hourly wage, available beds, and patient days). Second, I estimate the difference-in-differences model in Specification (1) on the intensive care unit of the hospitals in my sample as a placebo test of my findings. The results for these robustness checks are presented in the Appendix.

6.1. Longer pre-mandate trends

I do not utilize data prior to 1996 in my main specification because I do not have data on the HCAI Case Mix Index to adjust staffing and cost outcomes for patient severity. In this section, I include data from 1990–1995 in the estimation to allow for graphical inspection of the pre-trends over a longer period. For all outcomes with the exception of the nurse-to-patient ratio, I estimate Specification (2) on a balanced panel of 203 hospitals that I observe in the data over the extended estimation period (1990–2016).

In the main specification, the nurse-to-patient ratio is adjusted for patient severity. I showed in my comparison between Fig. 3 and Appendix Fig. A.6 that accounting for patient severity is important in controlling for differential pre-trends in nurse staffing. Absent these controls for 1990–1995, I instead include group-specific linear time trends following the two-step strategy in Goodman-Bacon (2021). First, I estimate linear time trends in the nurse-to-patient ratio separately for the treated and control groups using the pre-treatment years (1990–1999). Next, I subtract the time trend terms from the full panel before the estimation of Specification (2) on the full panel.

Appendix Figs. A.11, A.12, A.13, and A.14 show estimated event-study coefficients and raw means for my four main outcomes (nurse-to-patient ratio, RN hourly wage, available beds, and patient days). Fig. A.11(b) indicates the existence of differential pre-trends in the unadjusted staffing ratio. However, Fig. A.11(a) indicates that the pre-trends are well-approximated by and controlled for using linear time trends. Figs. A.12, A.13, and A.14 do not show differential pre-trends for any of the other outcomes.

Figs. A.11, A.12, A.13, and A.14 confirm the findings from my main specification. The mandate led to an increase in the nurse-to-patient ratio and declines in the RN hourly wage, available beds in acute care, and patient days in acute care among treated hospitals.

6.2. Placebo test using intensive care unit

In this section, I utilize the intensive care unit as a placebo test of my findings by estimating Specification (1) on the outcomes in intensive care units of the hospitals in my sample. Given that the intensive care unit was already subject to minimum ratios prior to the mandate, I expect that to find null effects on my main outcomes in the intensive care unit absent any spillover effects of the mandate within the hospital. If there are hospital-level shocks that confound identification, they should be observed in the intensive care unit.

My placebo test relies on the assumption that there was little to no “bite” of the mandate on the intensive care unit for hospitals that were treated in acute care. Therefore the treated and control assignments remain the same as in the main specification. In my sample of 212 hospitals used in the main specification, seven did not have an intensive care unit for at least part of the sample period. Of the 205 hospitals with an intensive care unit, 197 hospitals (96 percent) had an 2000–2002 average nurse-to-patient ratio in intensive care greater than the mandated minimum of 0.5. Of the remaining eight, four had ratios greater than 0.48 and the lowest of the group was 0.31.

I present the results for nurse labor, all labor, wages, costs, and output in intensive care in Appendix Tables A.4, A.5, A.6, A.7, and A.8. With the exception of the adjusted productive staff-to-patient ratio and the RN real hourly wage in the long-term, none of the results show statistically significant coefficients. However, the patient severity-adjusted ratios increase after the mandate despite not being statistically significant. This may be because the average Case Mix Index at treated hospitals continued to decline relative to control hospitals after the mandate and labor was not adjusted downwards in response.

Table A.6, Column 1, Model 2 indicates that RN wages declined by a statistically insignificant 2.8 percent in intensive care. This is notable because the intensive care unit did not add additional nursing time due to the mandate and it raises the question of whether the wage decline in acute care is in fact driven by changes in composition. A possibility is that the nurses hired due to the mandate were distributed across hospital units. Chapman et al. (2009) report from interviews with hospital leaders that following the mandate, nurses were hired for float pools in which they would work across multiple units and, consequently, required cross-training.[31] These newly hired nurses would be generalists rather than specialists with higher skills in a specific unit. My discussions with practitioners suggest that in settings with labor demand shocks (e.g. mandate or COVID-19), senior nurse administrators believe one of the largest issues is the inflow of inexperienced nurses. They indicated that more experienced nurses are often required to supervise. This is legally true of LVNs, who must be supervised by RNs or physicians. The increase in staffing may have required a reassignment of nurses across hospital units leading to changes in composition in intensive care as well. However, I cannot conclusively speak to this hypothesis in my setting. This may be an area for future research given more granular data on labor flows within the hospital.

The results from my placebo test broadly suggest that hospital-level shocks coincident with the mandate cannot be driving my main results.

7. Conclusion

In this paper, I use the 1999 California nurse staffing mandate as an empirical setting to study the causal effects of minimum ratios on hospitals’ input use, capacity, output, costs, and healthcare quality. The mandate required hospitals to meet minimum nurse-to-patient

[31] Float pool nurses are recorded separately by the hospital in the hospital reporting forms, however, at the time of cost allocation to the individual units these float pool nurse hours are allocated to the unit. Therefore I cannot observe how the float pool hours changed using my data.

ratios established for each hospital unit by the California Department of Health Services. I combine hospital financial reporting data and administrative patient discharge data with a difference-in-differences research design.

I find that the mandate had its intended effect on understaffed hospitals' nurse-to-patient ratios in the general medical/surgical acute care unit and led to limited crowding out of other labor and non-labor inputs. This finding is consistent with strict scope of practice regulations in California that limit substitution across different types of labor and may therefore differ in contexts with less stringent regulation. However, I provide suggestive evidence that hospitals increased use of lower-licensed and younger nurses. My findings suggest that labor heterogeneity should be taken into account when minimum ratios are implemented and that any estimated quality effects may vary based on the composition of the labor supplied and over time as nurses gain human capital. Importantly, I quantify the costs to be a 9 percent increase in the wage bill of the acute care unit.

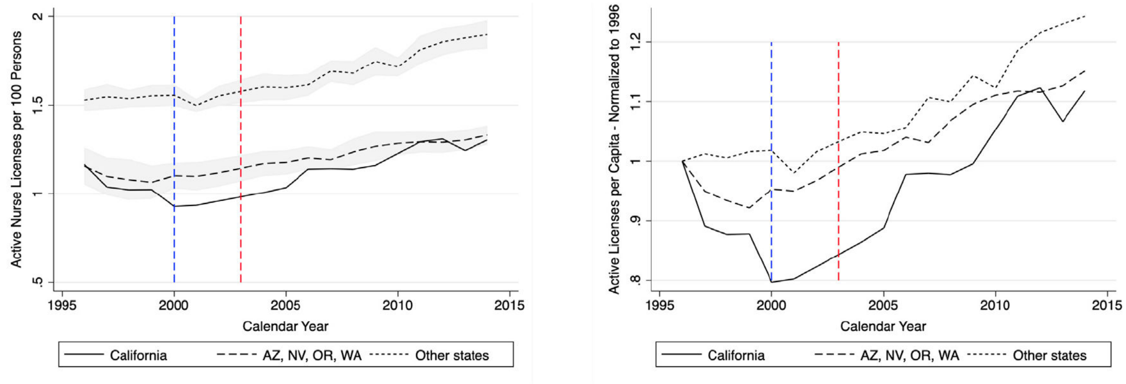
Furthermore, I find that hospitals reduced capacity by 16 beds (14 percent) and increased bed utilization rates by 0.045 points (8 percent). The increase in utilization to 64 percent suggests that hospitals were operating with excess bed capacity prior to the mandate and reduced capacity in response to a rise in costs per staffed bed.

Using administrative data on discharges for AMI, I estimate the effects of the mandate on quality using risk-adjusted in-hospital mortality, 30-day all-cause readmission rate, and length of stay as quality indicators. I find a null effect on in-hospital mortality which is consistent with the incidence of the staffing shock in my setting on the general medical/surgical acute care unit. In-hospital mortality is far more likely to take place in the intensive care unit, where patients in critical condition are stabilized prior to being transferred to acute care. However, I find that the mandate led to a 5 percent decline in length of stay and no effect on the 30-day all-cause readmission rate indicating that AMI patients at treated hospitals recovered more quickly following the mandate due to an improvement in care quality per day.

Appendix

A.1. General equilibrium wage effects

Several papers have focused on estimating the general equilibrium wage effects of the mandate (Mark et al., 2009; Munnich, 2014; Harless, 2019). Mark et al. (2009) use three different survey data sources and find a 7.8 percent increase in annual earnings (unadjusted for hours worked per year) using the National Sample Survey for Registered Nurses (NSSRN) and 5 percent and 6.5 percent increases in wages using the Current Population Survey and National Compensation Survey, respectively. Munnich (2014) uses two different survey data sources and finds a 4.3 percent increase in wages using the American Community Survey and no significant change using the CPS Merged Outgoing Rotation Group. Harless (2019) finds a 4.33 percent growth in RN wages relative to other occupations and metro areas outside of California.



(a) Active Nursing Licenses Per 100 Persons (b) Licenses Per Capita Normalized to 1996

Fig. A.1.

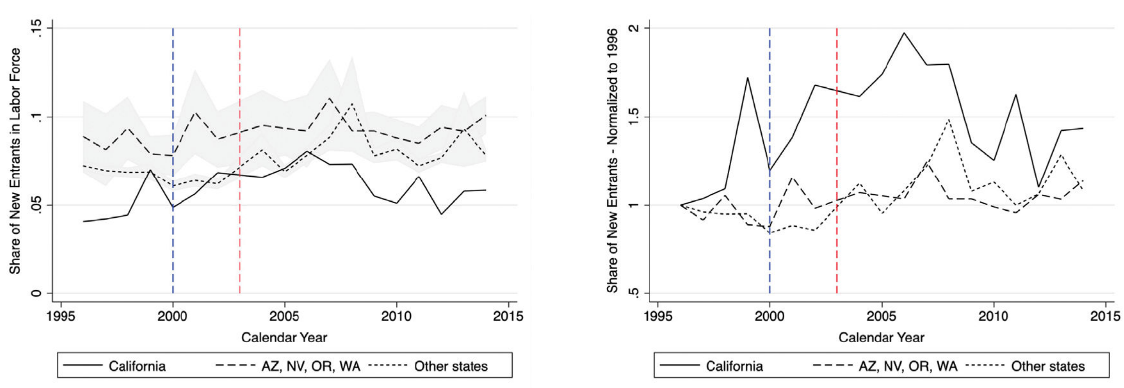
Growth in the nursing labor force in California vs. Other states.

Notes: In Panel (a), I plot the average number of active LVN and RN licenses per 100 persons by group. Standard error bands for the averages across states are shown in gray for neighbors and other states. In Panel (b), I plot the same measure for each group normalized to the average number of nurse licenses per 100 persons for that group in 1996. The dashed red line marks the treatment year (2003) and the dashed blue line marks the year in which both the policy and nurse shortage were announced. Data are not available for 2003.

I use data from the NSSRN to estimate the general equilibrium wage effects of the mandate. I utilize a difference-in-difference research design comparing the average annual salary and hourly wage of RNs employed at California hospitals and RNs employed at hospitals in other states. I estimate the following event-study regression for a state s at time t where CALIFORNIA is an indicator variable that takes on the value 1 if the state is California and the value 0 if not:

$$y_{st} = \alpha_0 + \sum_{t \neq 2000} \alpha_t \{YEAR_t = t\} * CALIFORNIA_s + \gamma_s + \xi_t + \epsilon_{st} \quad (6)$$

Relative to Mark et al. (2009) who use the same data but focus exclusively on annual salary, I utilize the estimate of hours worked per year to construct an hourly wage measure.



(a) New Entrants' Share of Active Licenses (b) New Entrants' Share Normalized to 1996

Fig. A.2.

Growth in the new entrants' share of labor force.

Notes: In Panel (a), I plot the average new entrants as a share of active nurse license holders. Standard error bands for the averages across states are shown in gray for neighbors and other states. In Panel (b), I plot the same measure for each group normalized to the average new entrants' share for that group in 1996. The dashed red line marks the treatment year (2003) and the dashed blue line marks the year in which both the policy and nurse shortage were announced. Data are not available for 2003.

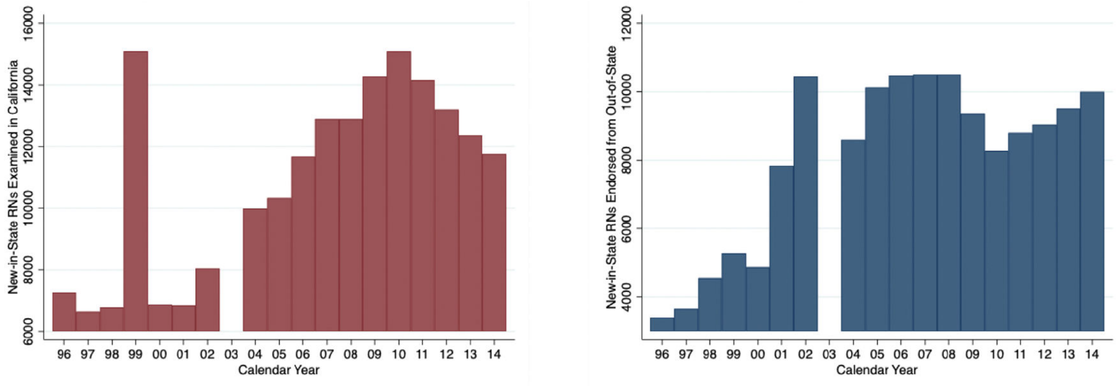
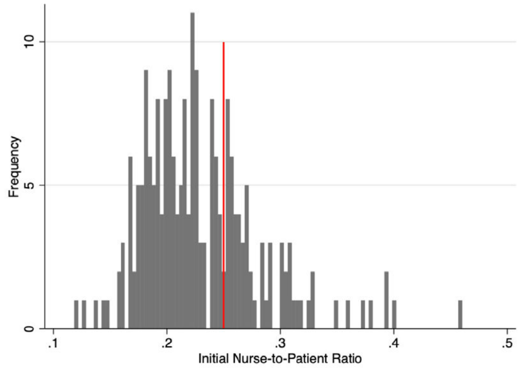


Fig. A.3.

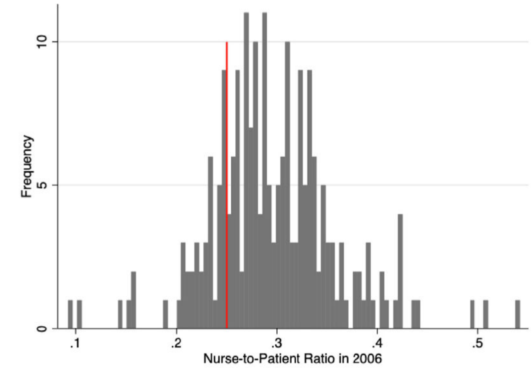
New-in-State RN licenses, 1996–2014.

Notes: This figure shows the numbers of new-in-state RN licenses for RNs examined in California and RNs endorsed from out of state. Data for 2003 is not available. The figure shows that the growth in RN licenses between 2000 and 2010 came from a combination of the two channels.

In Fig. A.7(a), I present the event-study estimates of α_1 from a regression of the log RN real annual salary in Specification (6). In Fig. A.7(b), I present the raw means of the RN real annual salary in 1996 USD. The real annual salary is denominated in 1996 USD to be consistent with the wages reported in Fig. 6 of my main analysis. Fig. A.7 confirms within ballpark the finding in Mark et al. (2009) of an increase in earnings in California relative to other states between 2000 and 2004. Mark et al. (2009) find a 7.8 percent increase whereas I find a 7.1 percent increase.



(a) Initial Nurse-to-Patient Ratio in 2000-2002



(b) Nurse-to-Patient Ratio in 2006

Fig. A.4.

Histograms of the nurse-to-patient ratio.

Notes: This figure shows the distributions of the initial nurse-to-patient ratio and nurse-to-patient ratio in 2006 for my balanced sample. The red solid line marks the 0.25 threshold used to delineate my sample into treatment and control hospitals. Seven of the 212 hospitals in my sample reported a ratio of below 0.2 in 2006 indicating that they were not compliant with the policy.

Table A.1

Descriptive statistics on California hospitals by initial nurse-to-patient ratio, four groups.

Variable	Below 0.19	0.19 to 0.22	0.22 to 0.25	Above 0.25	Diff. 1 vs. 4	Diff. 2 vs. 4	Diff 3 vs. 4
Share church or non-profit	0.50	0.59	0.59	0.74	−0.24***	−0.15*	−0.15*
Share investor-owned	0.38	0.24	0.33	0.18	0.20**	0.06	0.15*
Share government-owned	0.12	0.18	0.08	0.08	0.04	0.10	0.00
Share teaching hospitals	0.06	0.08	0.12	0.15	−0.09	−0.07	−0.02
Share DSH hospitals	0.30	0.25	0.22	0.23	0.07	0.03	−0.00
Share with psychiatric unit	0.42	0.53	0.47	0.34	0.08	0.19**	0.13
Share with chem. dependency unit	0.04	0.04	0.02	0.11	−0.07	−0.07	−0.09*
Share with rehab. unit	0.34	0.27	0.31	0.32	0.02	−0.05	−0.02
Share with LT care unit	0.54	0.59	0.55	0.45	0.09	0.14	0.10
Share with other units	0.16	0.16	0.04	0.16	−0.00	−0.00	−0.12**
HHI using acute patient days	1,721	1,580	2,301	2,361	−640*	−781***	−60
HHI using acute discharges	1,918	1,734	2,503	2,521	−603*	−787***	−17
MSA patient days per year	23,051	25,825	28,939	26,202	−3,150	−376	2,737
Total patient days per year	53,549	59,993	65,909	58,686	−5,137	1,308	7,224

Variable	**Below 0.19**	**0.19 to 0.22**	**0.22 to 0.25**	**Above 0.25**	**Diff. 1 vs. 4**	**Diff. 2 vs. 4**	**Diff 3 vs. 4**
MSA available beds	109	121	127	118	−10	3	9
MSA length of stay	5.62	5.21	5.81	3.89	1.73*	1.33	1.92
MSA utilization rate	0.59	0.56	0.58	0.55	0.04	0.01	0.03
Case Mix Index	1.14	1.12	1.14	1.18	−0.05	−0.06	−0.04
Revenues per patient day	268	291	288	351	−83***	−60*	−63**
Expenses per patient day	347	358	385	486	−139**	−127**	−100*
Profits per patient day	−98	−91	−116	−177	79*	86**	61
Medicare share of days	0.37	0.38	0.36	0.35	0.02	0.02	0.01
MediCal share of days	0.22	0.16	0.18	0.14	0.07**	0.01	0.03
County Indigent programs share of days	0.02	0.03	0.01	0.02	−0.00	0.00	−0.01*
Other third-party payor share of days	0.35	0.41	0.42	0.45	−0.10***	−0.04	−0.03
Other payor share of days	0.04	0.03	0.04	0.03	0.00	−0.00	0.00
Observations	50	51	49	62	112	113	111

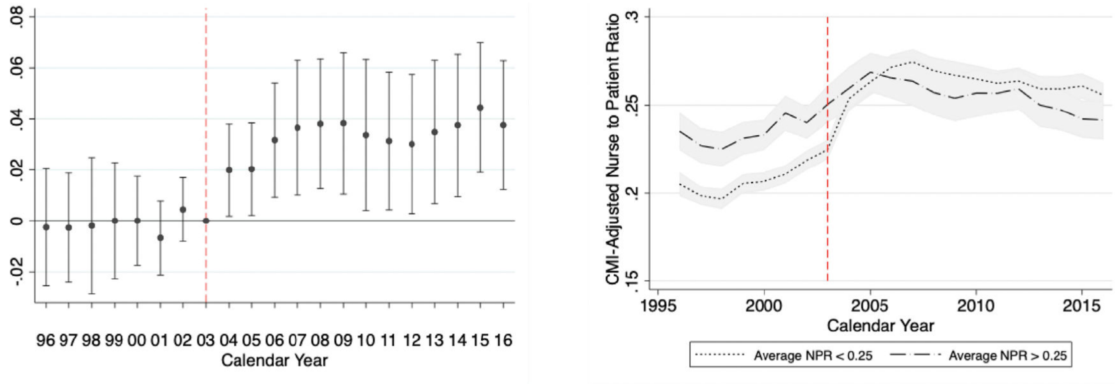
Notes: This figure shows all hospitals included in my balanced estimation sample as in Table 1 except the first column in Table 1 (Below 0.25) is separated into three columns. Columns 5–7 in this table are results from regressions of the dependent variable on indicator variables for whether the hospital is in the specified group versus the “Above 0.25” group (control group).

In Fig. A.8, I present the event-study estimates and raw means using the log RN real hourly wage and the RN real hourly wage, respectively. These results are not shown in Mark et al. (2009). Fig. A.8(a) shows a statistically insignificant 1.4 percent increase in the hourly wage in California relative to other states between 2000 and 2004. The coefficient increases to 3.1 percent in 2008.

In Section 4.2, I motivated that the shift in the labor demand and changes in nurse composition might have competing effects on the wage if nurses in California become younger and more recently licensed relative to other states. To account for these compositional changes, I estimate the following specification that includes time-variant age and education controls for the share of RNs employed in hospitals within each age group $a \in A$ and the share within each education level $e \in E$

$$y_{st} = \alpha_0 + \sum_{t \neq 2000} \alpha_t \{YEAR_t = t\} * CALIFORNIA_s + \sum_{a \in A} \beta_a A_{st} + \sum_{e \in E} \beta_e E_{st} + \gamma_s + \xi_t + \epsilon_{st} \tag{7}$$

The results from the estimation of Specification (3) are presented in Fig. A.9. Comparing Figs. A.9 and A.8(a), the inclusion of age and education controls in estimation does not change the results very much.

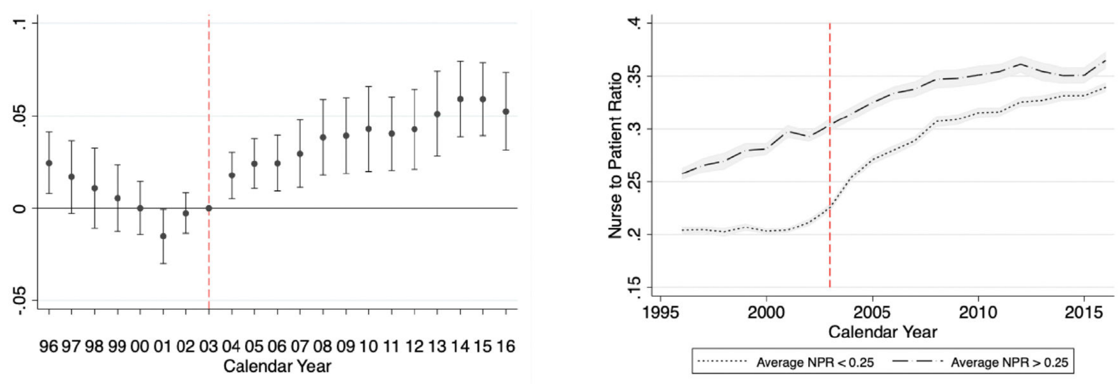


(a) Event-Study Estimates (b) Raw Means

Fig. A.5.

Nurse-to-patient ratio, adjusted — restricted sample of hospitals.

Notes: Panel (a) plots coefficients and Panel (b) plots averages for the adjusted nurse to patient ratio estimated on a restricted sample of 124 hospitals with an initial ratio between 0.2 and 0.3. This figure illustrates that my findings are robust to concerns that the treated and control groups in my main analysis differ from one another in levels, particularly on the margins.



(a) Event-Study Estimates (b) Raw Means

Fig. A.6.

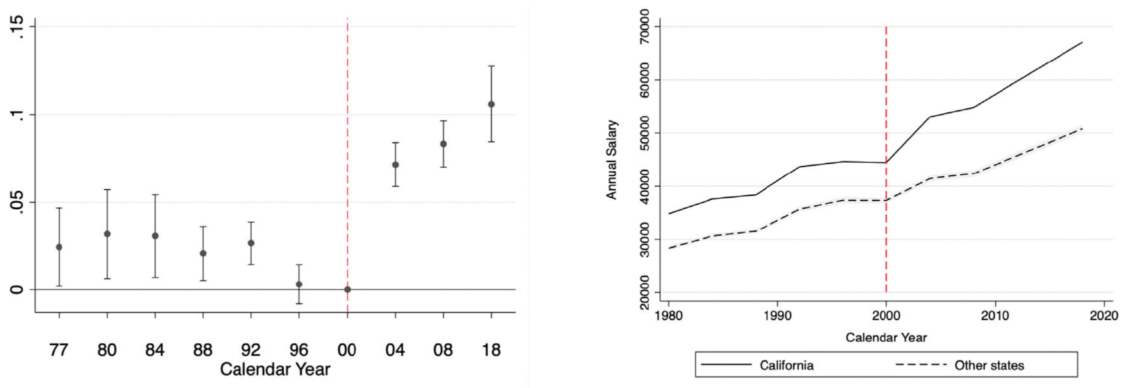
Nurse-to-patient ratio, unadjusted.

Notes: Panel (a) plots coefficients and Panel (b) plots averages for the nurse to patient ratio as in Figure 3 with two modifications. First, it utilizes a longer sample period (1990–2016) and a balanced panel of 203 hospitals. Second, the outcome is not adjusted for patient severity and as a result the model being estimated (and estimates shown in Panel (b)) includes group-specific linear time trends (Goodman-Bacon, 2021).

The event-study estimates for the pre-mandate years indicate that the research design is flawed due to differential pre-trends. If we were to interpret the results barring the flaws in the research design, we can see that there are small general equilibrium effects on wages. My estimate of the effect is 1.9 percent and statistically insignificant in 2004. This estimate is a lower bound on prior estimates when compared to 4.3 percent based on ACS in Munnich

(2014), −3.9 percent and insignificant based on CPS MORG in Munnich (2014), 4.33 percent in Harless (2019), 5 percent based on NCS in Mark et al. (2009), 6.5 percent based on NCS in Mark et al. (2009), and 7.8 percent based on earnings rather than wages in the NSSRN in Mark et al. (2009).

I show that my findings can be consistent with the magnitude of the shock to aggregate RN labor demand under estimated RN labor supply elasticities. First, I estimate the magnitude of the shock to be 2.8 percent of the California hospital RN labor force (it would be even smaller if I defined the market beyond hospital RNs).[32] Estimated RN labor supply elasticities vary widely based on whether they are estimated over the short- or long-run, whether they include extensive margin labor supply decisions, and how widely the market is defined. For labor supply elasticities ranging from 0.1 to 2, the implied wage effects vary widely in magnitude from 28 to 1.4 percent. My preferred estimate of the labor supply elasticity of 1.3 includes the extensive margin decision (Hanel et al., 2014) and implies an increase in average wages of 2.2 percent, all else equal. That the general equilibrium effects are small in magnitude is not surprising from this perspective. However, I caveat that estimating these effects is not a strength of this paper given my reliance on aggregate data and a cross-state research design.



(a) Event-Study Estimates

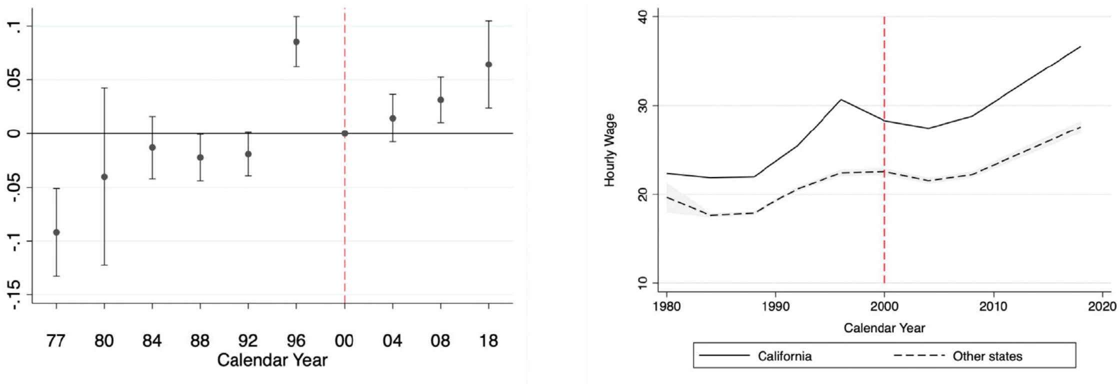
(b) Raw Means

Fig. A.7.

Hospital RN real annual salary.

Notes: In Fig. A.7 panel (a) this figure plots the coefficients α_{τ} and 95 percent confidence intervals from Specification (6) with the log real annual salary as dependent variables. In Figs. A.7 panel (b) this figure plots the average values and standard error bands of the real annual salary in 1996 USD by group. These findings are consistent with prior work.

[32] I take the estimated effect of the mandate on RN hours (distinct from the RN-to-patient ratio) as a measure of the increase in RN labor demand by treated hospitals. First, I estimate an equivalent of Appendix Table 12, Column 7 for RN hours instead of nursing hours. I find that the effect of the mandate on RN hours is not statistically significant but positive and increasing in magnitude with the hospital's distance from the threshold. The 50 hospitals in the "Below 0.19" treated group saw a 13.2 percent increase in RN hours due to the mandate. These 50 hospitals hired an average of 67,745 RN hours in 2000 prior to the mandate. This implies an increase in RN labor demand from these hospitals of 50*67,745*0.132 = 447,117 RN hours or 215 RNs working 2080 h per year. Adding this number to the estimates for the 51 hospitals "Between 0.19 and 0.22" (4.9 percent) and the 49 hospitals "Between 0.22 and 0.25" (3.7 percent), we obtain an increase in labor demand of 476 RNs. 476 RNs represent 2.8 percent of the California hospital RN labor force in 2000 which will represent the magnitude of the shift in the labor demand curve.



(a) Event-Study Estimates (b) Raw Means

Fig. A.8.

Hospital RN real hourly wage.

Notes: In Fig. A.8 panel (a) this figure plots the coefficients α_t and 95 percent confidence intervals from Specification (6) with the log real hourly wage as dependent variables. In Figs. A.8 panel (b) this figure plots the average values and standard error bands of the real hourly wage in 1996 USD by group. This figure indicates that the estimated increase in the log real annual salary shown in Fig Fig. A.7 was largely driven differential changes in hours worked.

Table A.2

Difference-in-differences estimates for RN wages by number of nearby hospitals.

	(1) ln(RN real hrly wage)	(2) ln(RN real hrly wage)
Above 0.25 × Post	0.048 ** (0.021)	0.069 *** (0.021)
Above 0.25 × Post × Number Treated Hospitals Within 5 Mi	0.004 (0.016)	
Above 0.25 × Post × Number Treated Hospitals Within 10 Mi		−0.006 (0.006)
Observations	4,412	4,412
R^2	0.525	0.526
Hospital FE	✓	✓
Year FE	✓	✓

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

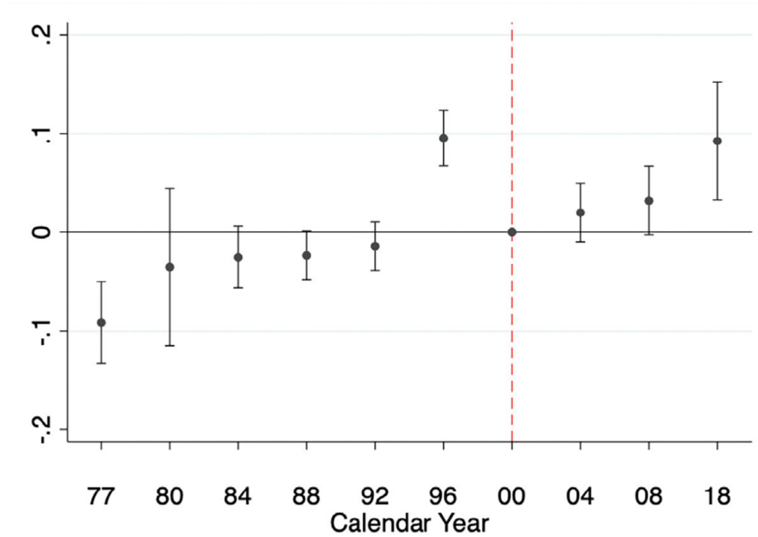


Fig. A.9.

Hospital RN real hourly wage with age and education controls.

Notes: In Figure Fig. A.9, I plot the coefficients α_t and 95 percent confidence intervals from Specification (7). Taken together, I find small, if any, general equilibrium wage effects consistent with the magnitude of the shock to aggregate RN labor demand.

STAFF NURSE SALARY									
Length of Service	June 1, 2004			June 1, 2005			June 1, 2006		
	AD & D	Bacc.	Masters	AD & D	Bacc.	Masters	AD & D	Bacc.	Masters
Start	23.42	24.24	25.06	24.36	25.21	26.07	25.33	26.22	27.10
1 year	24.89	25.76	26.63	25.89	26.80	27.70	26.93	27.87	28.82
2 years	25.92	26.83	27.73	26.96	27.90	28.85	28.04	29.02	30.00
3 years	26.93	27.87	28.82	28.01	28.99	29.97	29.13	30.15	31.17
4 years	27.94	28.92	29.90	29.06	30.08	31.09	30.22	31.28	32.34
5 years	28.81	29.82	30.83	29.96	31.01	32.06	31.16	32.25	33.34
6 years	29.66	30.70	31.74	30.85	31.93	33.01	32.08	33.20	34.33
7 years	30.85	31.93	33.01	32.08	33.20	34.33	33.36	34.53	35.70
8 years	31.15	32.24	33.33	32.40	33.53	34.67	33.70	34.88	36.06
9 years	32.36	33.49	34.63	33.65	34.83	36.01	35.00	36.23	37.45
10 years	33.21	34.37	35.53	34.54	35.75	36.96	35.92	37.18	38.43
12 years	33.87	35.06	36.24	35.22	36.45	37.69	36.63	37.91	39.19
15 years	34.80	36.02	37.24	36.19	37.46	38.72	37.64	38.96	40.27
20 years	35.48	36.72	37.96	36.90	38.19	39.48	38.38	39.72	41.07

Fig. A.10.

Sample of RN wage scale in Minnesota union contract, 2004.

Notes: This figure shows an example of a wage scale in a union contract between the Minnesota Nurses Association and Allina Health System/United Hospital in 2004. The range for RNs with a diploma or associate's degree, calculated using the June 1, 2004 scale, is 51 percent.

Source: Collective Bargaining Agreements Digital Collections@ILR Cornell

Table A.3

Difference-in-differences estimates for capacity and discharges by number of nearby hospitals.

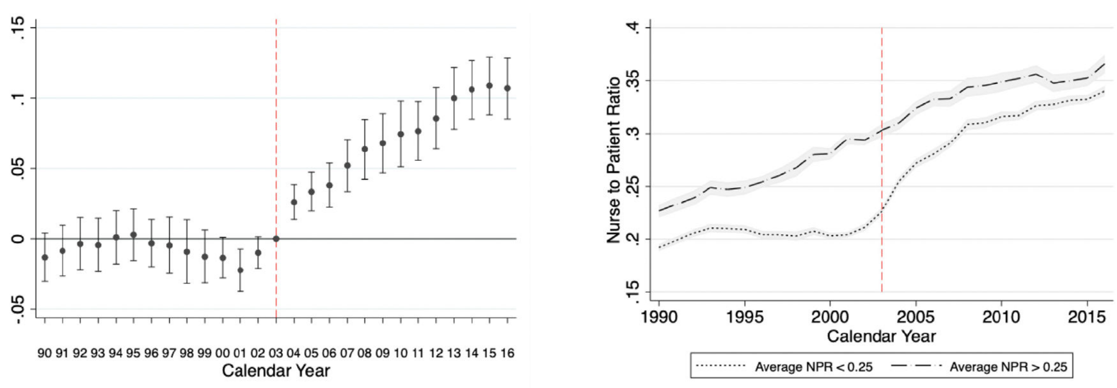
	(1) Available beds	(2) Available beds	(3) Staffed beds	(4) Staffed beds	(5) Discharges	(6) Discharges
Below 0.25 × Post	−13.774** (6.115)	−11.475* (6.315)	−13.298** (5.330)	−11.956** (5.482)	−878.364 (621.146)	−819.885 (658.569)
Below 0.25 × Post × Num. Hospitals Within 5 Mi	−2.009** (1.013)		−1.669* (0.924)		−249.881 (231.937)	
Below 0.25 × Post × Num. Hospitals Within 10 Mi		−1.011** (0.395)		−0.733* (0.372)		−82.776 (85.120)
Observations	4402	4402	4402	4402	4402	4402
R^2	0.096	0.098	0.110	0.110	0.045	0.043
Hospital FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

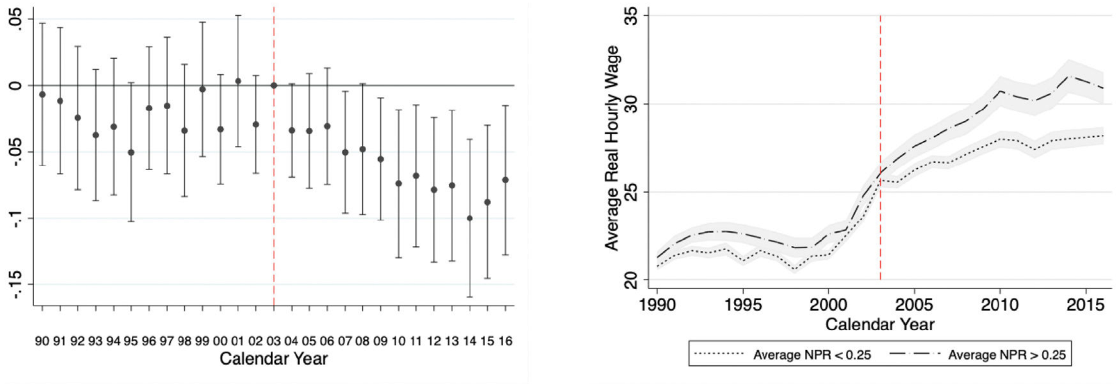


(a) Event-Study Estimates (b) Raw Means

Fig. A.11.

Nurse-to-patient ratio, unadjusted, longer pre-mandate trends.

Notes: Panel (a) plots coefficients and Panel (b) plots averages for the nurse to patient ratio as in Figure 3 with two modifications. First, it utilizes a longer sample period (1990–2016) and a balanced panel of 203 hospitals. Second, the outcome is not adjusted for patient severity and as a result the model being estimated includes group-specific linear time trends (Goodman-Bacon, 2021).



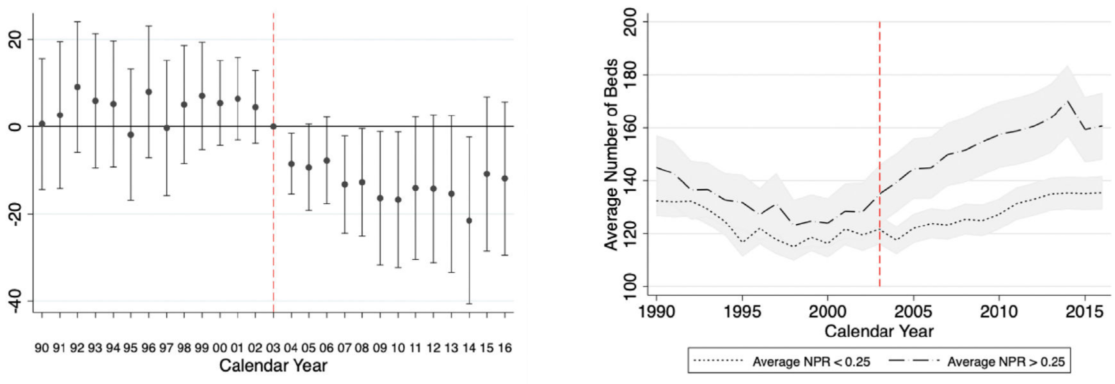
(a) Event-Study Estimates

(b) Raw Means

Fig. A.12.

RN real hourly wage, longer pre-mandate trends.

Notes: Fig. A.12 Panel (a) plot coefficients and Panel (b) plot averages for the log RN real hourly wage (raw means are not logged) as in Fig. 6 except here I utilize a longer sample period (1990–2016) and a balanced panel of 203 hospitals.



(a) Event-Study Estimates

(b) Raw Means

Fig. A.13.

Acute care available beds, longer pre-mandate trends.

Notes: Fig. A.13 Panel (a) plot coefficients and Panel (b) plot averages for acute care available beds as in Fig. 7 except I utilize a longer sample period (1990–2016) and a balanced panel of 203 hospitals.

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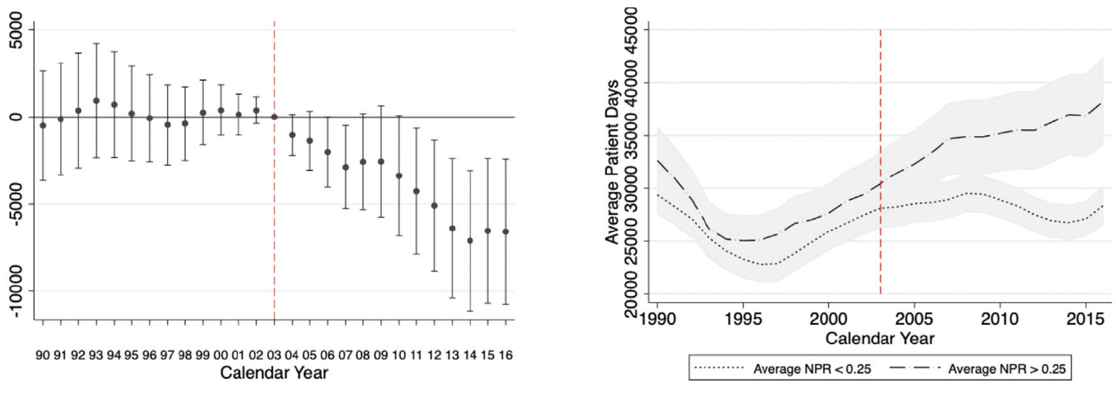

(a) Event-Study Estimates (b) Raw Means

Fig. A.14.

Acute care patient days, longer pre-mandate trends.

Notes: Figs. A.13 Panel (a) plot coefficients and Panel (b) plot averages for acute care patient days as in Figs. 8 except I utilize a longer sample period (1990–2016) and a balanced panel of 203 hospitals.

Table A.4

Difference-in-differences estimates for nurse labor in intensive care.

	(1) Nurse-Patient	**(2) Nurse-Patient Adj.**	**(3) RN-Patient**	**(4) RN-Patient Adj.**	**(5) LVN-Patient**	**(6) LVN-Patient Adj.**	**(7) ln(nurse hours)**
Below 0.25 × Post (1996–2006)	0.009 (0.020)	0.037 (0.022)	0.007 (0.020)	0.031 (0.021)	0.005 (0.003)	0.006 (0.003)	−0.002 (0.037)
Below 0.25 × Post (1996–2010)	0.007 (0.021)	0.038 (0.025)	0.001 (0.021)	0.029 (0.023)	0.004 (0.003)	0.004 (0.003)	−0.022 (0.044)
Below 0.25 × Post (1996–2016)	0.014 (0.021)	0.046 (0.024)	0.009 (0.020)	0.038 (0.023)	0.003 (0.004)	0.003 (0.004)	−0.028 (0.051)
Mean	0.666	0.627	0.595	0.560	0.017	0.016	10.874
R^2	0.080	0.048	0.162	0.034	0.116	0.120	0.260
Observations	4,116	4,116	4,116	4,116	4,116	4,116	4,116
Hospital FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the nurse to patient ratio, the Case Mix Index (CMI) adjusted nurse to patient ratio, RN-to-patient ratio, adjusted RN-to-patient ratio, LVN-to-patient ratio, adjusted LVN-to-patient ratio, and log of nurse hours employed.

Table A.5

Difference-in-differences estimates for non-nurse labor in intensive care.

	(1) Aide-Patient	(2) Aide-Patient Adj.	(3) Productive-Patient	(4) Productive-Patient Adj.	(5) ln(physician exp. ppd)	(6) ln(physician exp. ppd adj.)
Below 0.25 × Post (1996–2006)	0.004 (0.005)	0.007 (0.004)	0.026 (0.026)	0.053** (0.026)	0.214 (0.166)	0.248 (0.168)
Below 0.25 × Post (1996–2010)	0.003 (0.006)	0.006 (0.005)	0.020 (0.029)	0.053 (0.030)	0.222 (0.174)	0.254 (0.176)
Below 0.25 × Post (1996–2016)	0.003 (0.006)	0.006 (0.005)	0.027 (0.027)	0.059** (0.029)	0.258 (0.191)	0.289 (0.192)
Mean	0.023	0.022	0.732	0.690	3.045	2.995
R^2	0.038	0.017	0.118	0.036	0.195	0.210
Observations	4,116	4,116	4,116	4,116	3,789	3,789
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the aide to patient ratio, the Case Mix Index (CMI) adjusted aide to patient ratio, productive staff to patient ratio, adjusted productive staff to patient ratio, log of expenditures on physicians per patient day, and log of expenditures on physicians per adjusted patient day.

A.2. Evidence on the RN wage distribution within hospitals from union contracts

As an exercise, I produce a back-of-envelope calculation for the range of RN wages within hospital and hospital unit (i.e. between the least and most skilled RN in the acute care unit of each hospital) that must exist if the decline in wages at treated hospitals was entirely due to a decline in skill. I then compare the range that I find from this exercise with the range observed in publicly available union contracts from the early 2000s from the Collective Bargaining Agreements Digital Collections@ILR Cornell. I find that the wage range in the union contracts is far larger than the wage range required to support the decline in skill hypothesis. I outline the exercise below.

Table A.6

Difference-in-differences estimates for wages in intensive care.

	(1) ln(RN real hrly wage)	(2) ln(LVN real hrly wage)	(3) ln(non-nurse real hrly wage)
Below 0.25 × Post (1996–2006)	−0.015 (0.019)	0.059 (0.052)	−0.037 (0.036)
Below 0.25 × Post (1996–2010)	−0.028 (0.019)	−0.033 (0.046)	−0.032 (0.036)
Below 0.25 × Post (1996–2016)	−0.044** (0.020)	−0.064 (0.045)	−0.026 (0.038)
Mean	3.143	2.547	2.690
R^2	0.515	0.027	0.086
Observations	4,112	2,509	4,049
Hospital FE	✓	✓	✓
Year FE	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the RN real hourly wage, LVN real hourly wage, and real hourly wage of all directly employed workers excluding RNs, LVNs, and registry nurses. The latter group includes staff in the categories: management and supervision, technicians and specialists, aides and orderlies, clerical and other administrative, environmental and food service, salaried physicians, and non-physician medical practitioners. Physicians are normally employed as contractors whose hours are not reported to the health department, which is why expenditures on physicians are reported separately in Columns 5 and 6 of Table 8.

Table A.7

Difference-in-differences estimates for average costs in intensive care.

	(1) ln(supplies ppd adj.)	(2) ln(leases ppd adj.)	(3) ln(salaries ppd adj.)	(4) ln(dir. costs ppd adj.)	(5) ln(alloc. costs ppd adj.)	(6) ln(costs ppd adj.)
Below 0.25 × Post (1996–2006)	0.149 (0.169)	0.778*** (0.285)	0.035 (0.035)	0.035 (0.031)	−0.012 (0.054)	0.011 (0.040)
Below 0.25 × Post (1996–2010)	0.201 (0.163)	0.735** (0.290)	0.028 (0.039)	0.039 (0.038)	0.022 (0.050)	0.027 (0.042)
Below 0.25 × Post (1996–2016)	0.219 (0.158)	0.480 (0.285)	0.017 (0.041)	0.032 (0.041)	0.041 (0.055)	0.038 (0.044)
Mean	0.922	−0.742	6.159	6.339	5.716	6.798
R^2	0.618	0.024	0.297	0.267	0.168	0.238
Observations	4,089	2,963	4,116	4,116	4,116	4,116
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the log of expenditures on supplies, log of expenditures on capital leases, log of expenditures on salaries, log of total direct expenditures, log of total allocated expenditures (expenditures that accrue to the hospital and that are allocated back to the hospital unit based on usage), and log of total costs (sum of direct and allocated costs). All costs are per adjusted patient day.

I assume that there are two types of RNs: incumbents and new hires. New hires are RNs hired after the mandate. I assume that an RN in the workforce of either group (treated or control) prior to the mandate remains employed in the same group after the mandate. In other words, hospitals retain their existing incumbent RN workforce and add new hire RNs after the mandate. I refer to the average RN real hourly wage of RNs prior to the mandate as the “incumbent RN wage”. The 1999 incumbent RN wage was \$21.81 at control hospitals and \$21.42 at treated hospitals. As Fig. 6(b) shows, there was no statistically significant difference in RN wages between the two groups.

If the divergence in average RN wage between the two groups is due solely to differences in composition and there is no wage variation across groups (treated or control) for either type (new hire or incumbent) then we can use the following expression for each group $g \in \{t, c\}$ to solve for the difference between incumbent and new hire wages:

$$avewage_g^{post} * (incumbenthrs_g^{post} + newhirehrs_g^{post}) = incumbenthrs_g^{post} * incumbentwage^{post} + newhirehrs_g^{post} * newhirewage^{post}$$

Table A.8

Difference-in-differences estimates for output in intensive care.

	(1) Available beds	(2) Staffed beds	(3) Patient days	(4) Discharges	(5) Length of stay
Below 0.25 × Post (1996–2006)	−0.136 (0.928)	−0.138 (0.896)	−44.704 (232.748)	−228.616 (222.836)	−0.038 (0.863)
Below 0.25 × Post (1996–2010)	−0.720 (1.160)	0.690 (1.032)	−199.434 (285.859)	−329.095 (307.523)	−0.020 (0.828)
Below 0.25 × Post (1996–2016)	−0.947 (1.351)	1.113 (1.351)	−326.349 (299.063)	−211.165 (223.453)	0.017 (0.773)
Mean	18.150	16.350	4104.420	1160.477	5.983
R^2	0.116	0.657	0.159	0.012	0.064
Observations	4,116	4,116	4,116	4,116	4,116
Hospital FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the number of available beds, number of staffed beds, number of patient days, number of discharges, and length of stay in days.

The average wage for each group in the post-mandate period is known: $avewage_g^{post}$. Based on the assumption that I make about retention of the existing workforce, the following objects are known: $incumbenthrs_g^{post}$. I can solve for $newhirehrs_g^{post}$ using my estimated causal effect of the mandate on RN hours. I take the causal effect of the mandate on the adjusted RN-to-patient ratio (0.027) and multiply it by 24 h per patient day to obtain the additional number of new hire hours per patient day at the treated hospitals (0.648) relative to the hospitals above the threshold. I have a system of two equations with two unknowns: $newhirewage^{post}$ and $incumbentwage^{post}$ which do not differ between groups. I find that if the magnitude of the wage divergence is determined entirely by composition, incumbents must have 42 percent higher wages than new hires.

I assess the plausibility of wage variation of this magnitude using data from RN union contracts in the early 2000s. Union contracts specify the RN wage structure within the hospital by experience and education levels. In Appendix Fig. A.10, I provide a sample of a wage structure within a contract. I analyzed 28 RN union contracts executed between 2001 and 2006. These contracts cover 7 states, 11 unions, and 24 hospital systems or counties. On average, the most experienced nurse is paid 52 percent more than the entry-level nurse with the same level of education. At the median, the most experienced nurse is paid 51 percent more.[33] Given the average length of service differential between the most experienced nurse and entry-level nurse in these contracts (20 years), a 42 percent wage differential corresponds to roughly 16 additional years of experience.

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[33]These percentages are conservatively estimated and represent a lower bound for the range of wage variation. RN hours reported in HCAI financial data include staff nurses, charge nurses, head nurses, and nurse practitioners. Charge nurses, head nurses, and nurse practitioners hold leadership roles within the hospital and are paid higher salaries than staff nurses based on my assessment of union contracts. However, to provide a conservative estimate, I report the range of wage variation within staff nurses.

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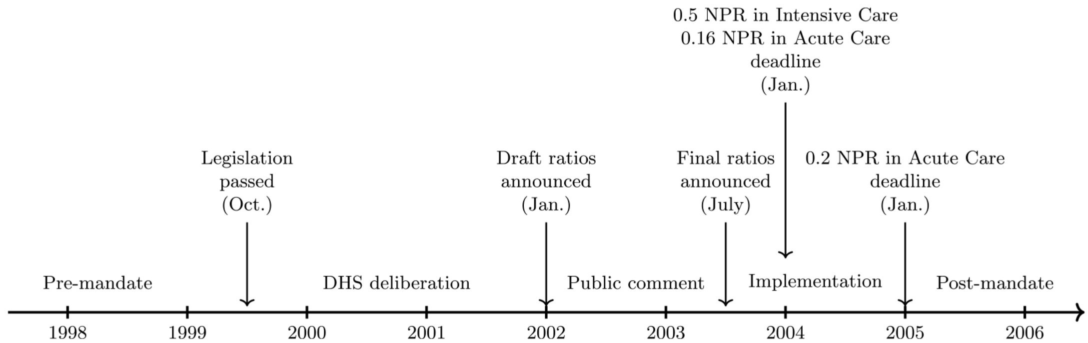


Fig. 1.

Mandate timeline in acute and intensive care units. Notes: Sources are DHS, Los Angeles Times, and California Legislative Information.

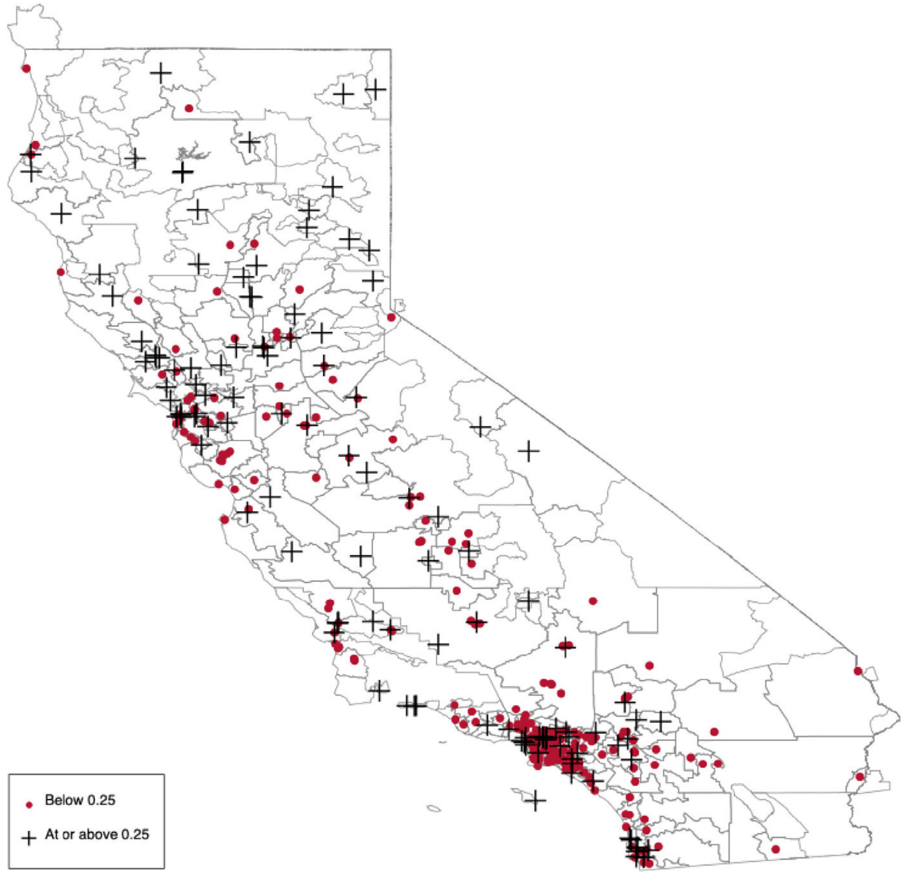
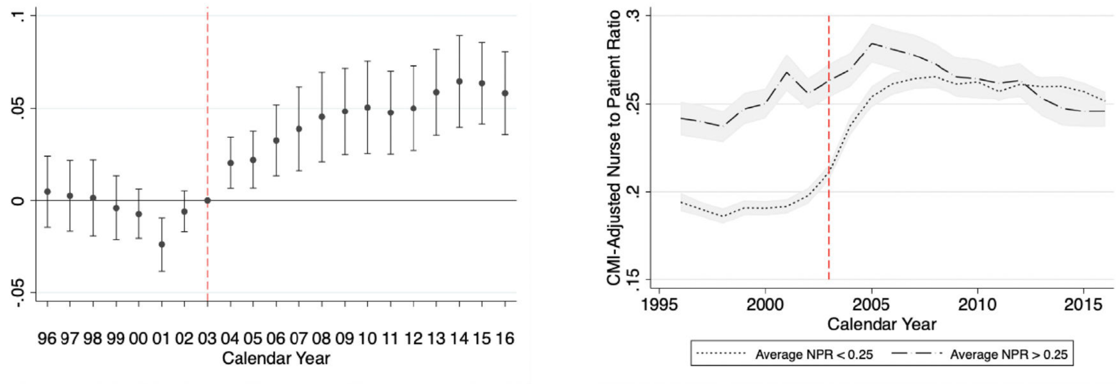


Fig. 2.

California hospitals by initial nurse-to-patient ratio in acute care unit.

Notes: This figure shows all hospitals included in my balanced estimation sample in addition to small and rural hospitals that are excluded from my estimation sample. Hospitals are classified by average nurse-to-patient ratio between 2000 and 2002. Gray lines indicate boundaries of hospital service areas from Dartmouth Atlas of Healthcare and can cross state lines. The map shows that treated hospitals (below 0.25) and control hospitals (at or above 0.25) are located in most of the same geographic markets for healthcare.



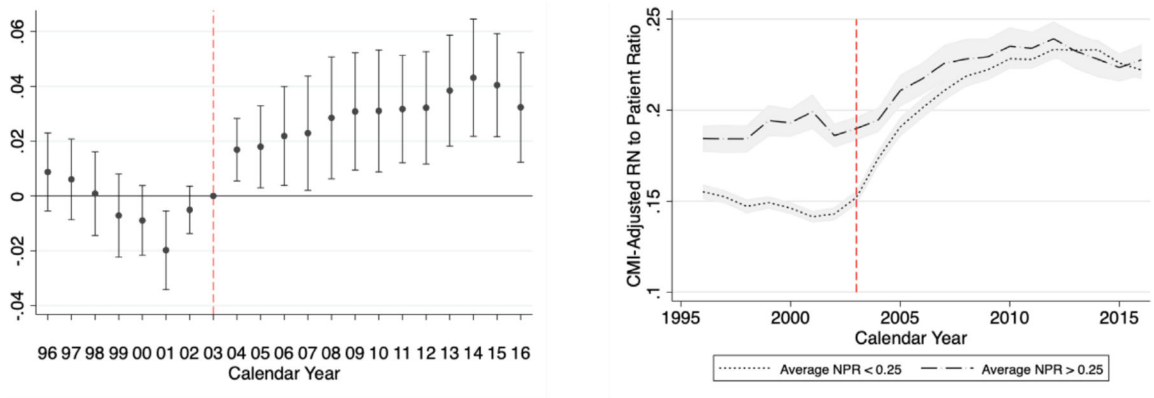
(a) Event-Study Estimates

(b) Raw Means

Fig. 3.

Nurse-to-Patient Ratio Adj.

Notes: In Fig. 3 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the nurse-to-patient ratio as dependent variable. Standard errors are clustered at the hospital level. In Fig. 3 Panel (b), this figure plots average values and standard error bands of the nurse-to-patient ratio by group.

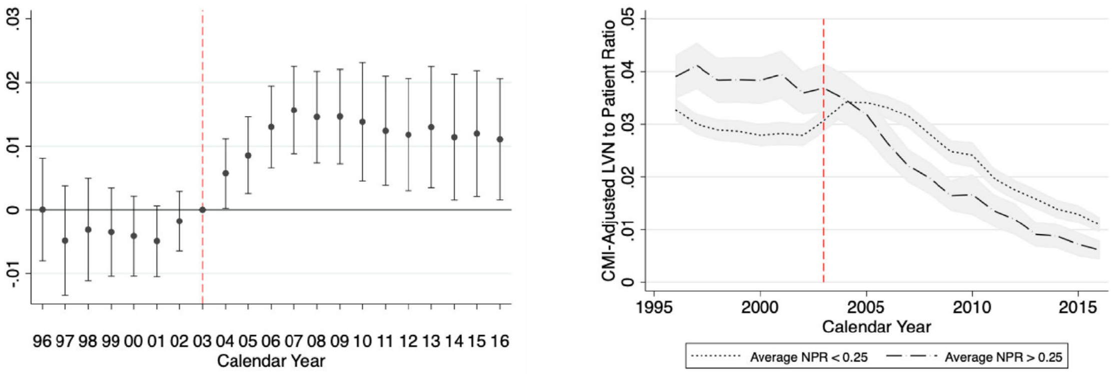


(a) Event-Study Estimates (b) Raw Means

Fig. 4.

RN-to-Patient Ratio Adj.

Notes: In Fig. 4 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the RN-to-patient ratio as dependent variable. Standard errors are clustered at the hospital level. In Fig. 4 Panel (b), this figure plots average values and standard error bands of the RN-to-patient ratio by group.

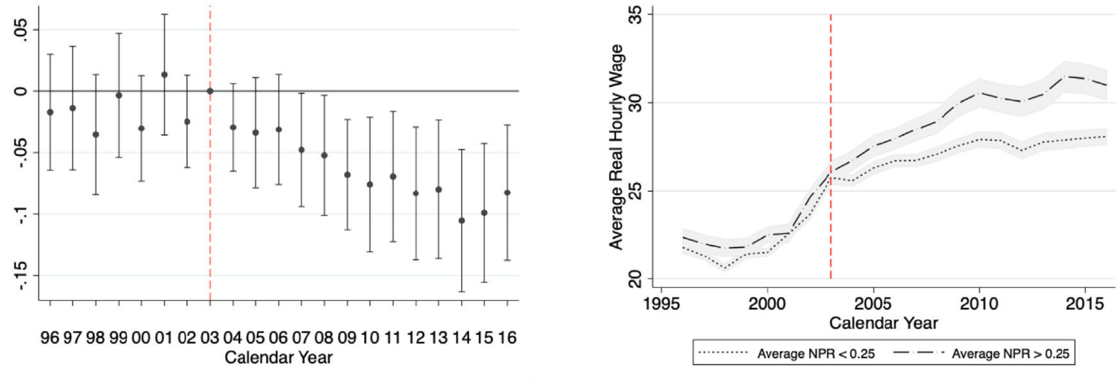


(a) Event-Study Estimates (b) Raw Means

Fig. 5.

LVN-to-Patient Ratio Adj.

Notes: In Fig. 5 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the LVN-to-patient ratio as dependent variable. Standard errors are clustered at the hospital level. In Fig. 5 Panel (b), this figure plots average values and standard error bands of the LVN-to-patient ratio by group.

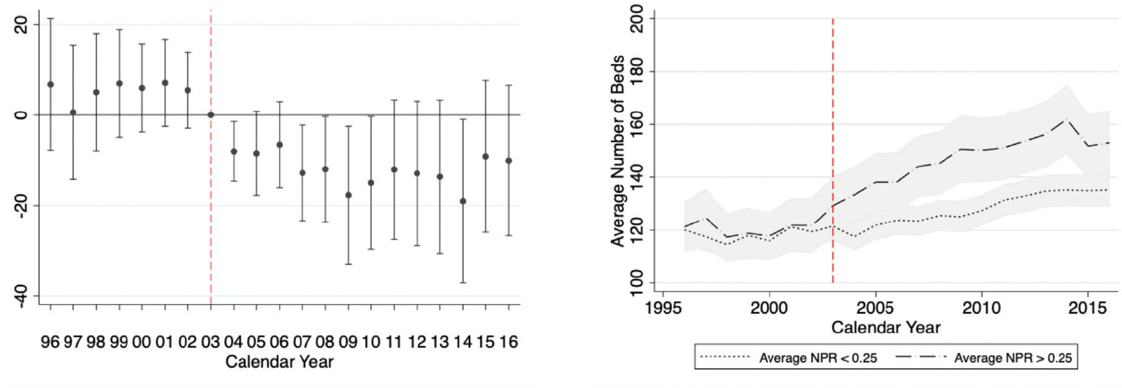


(a) Event-Study Estimates (b) Raw Means

Fig. 6.

RN real hourly wage.

Notes: In Fig. 6 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the logged RN real hourly wage as dependent variable. Standard errors are clustered at the hospital level. In Fig. 6 Panel (b), this figure plots average values and standard error bands of the RN real hourly wage by group.

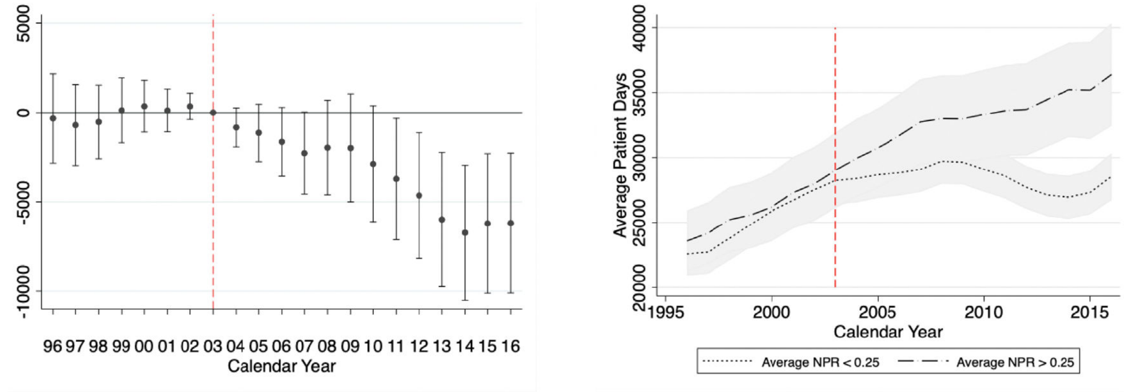


(a) Event-Study Estimates (b) Raw Means

Fig. 7.

Acute care available beds.

Notes: In Fig. 7 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the acute care available beds as dependent variable. Standard errors are clustered at the hospital level. In Fig. 7 Panel (b), this figure plots average values and standard error bands of the available beds by group.

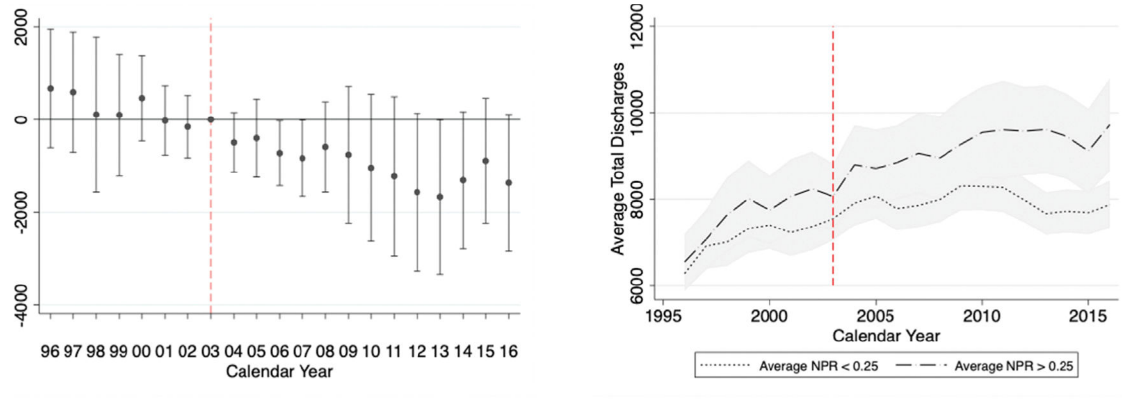


(a) Event-Study Estimates (b) Raw Means

Fig. 8.

Acute care patient days.

Notes: In Fig. 8 Panel (a), this figure plots coefficients α_i and 95 percent confidence intervals from Specification (2) with the acute care patient days as dependent variable. Standard errors are clustered at the hospital level. In Fig. 8 Panel (b), this figure plots average values and standard error bands of the patient days by group.

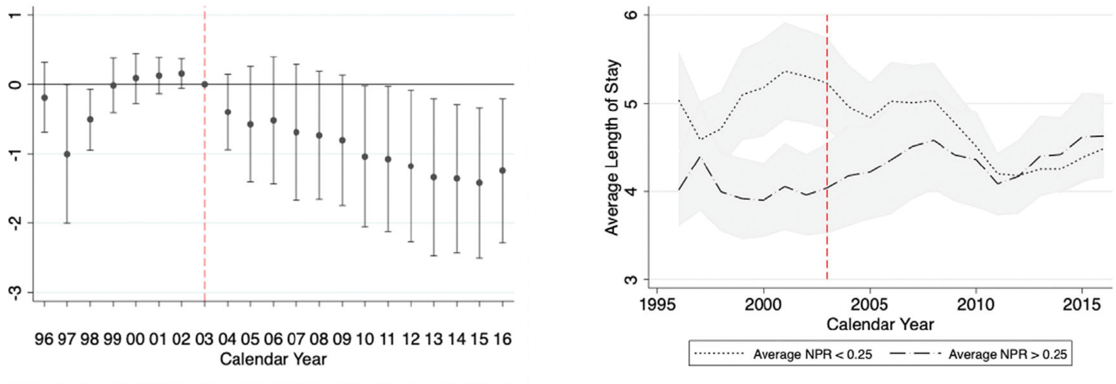


(a) Event-Study Estimates (b) Raw Means

Fig. 9.

Acute care discharges.

Notes: In Fig. 9 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the acute care discharges as dependent variable. Standard errors are clustered at the hospital level. In Fig. 9 Panel (b), this figure plots average values and standard error bands of the discharges by group.



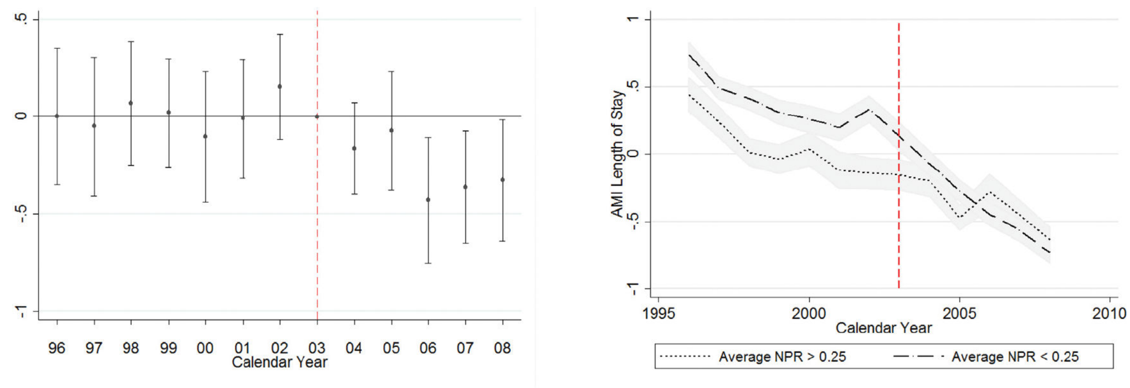
(a) Event-Study Estimates

(b) Raw Means

Fig. 10.

Length of stay.

Notes: In Fig. 10 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the length of stay as dependent variable. Standard errors are clustered at the hospital level. In Fig. 10 Panel (b), this figure plots average values and standard error bands of the length of stay by group.



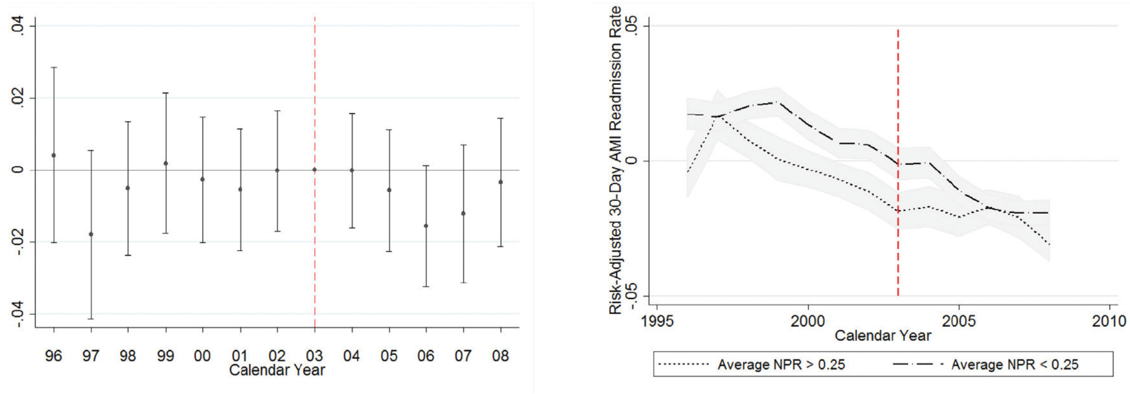
(a) Event-Study Estimates

(b) Raw Means

Fig. 11.

Risk-adjusted AMI length of stay.

Notes: In Fig. 11 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the risk-adjusted AMI length of stay as dependent variable. In Fig. 11 Panel (b), this figure plots average values and standard error bands of the AMI length of stay by group. The regressions and raw means are weighted at the hospital-level by the share of AMI discharges that were treated at the hospital in the calendar year.

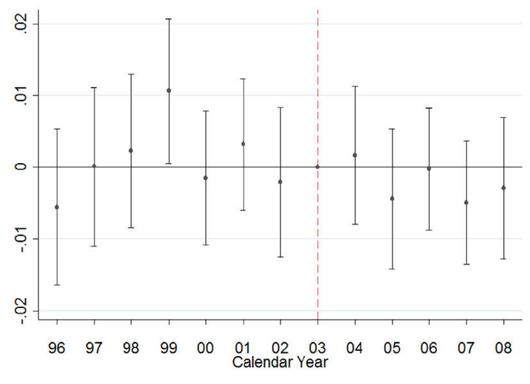


(a) Event-Study Estimates (b) Raw Means

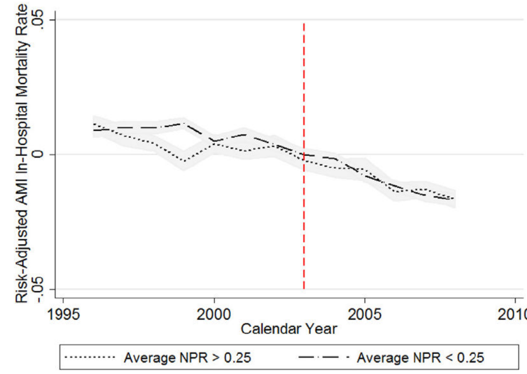
Fig. 12.

Risk-adjusted AMI 30-day all-cause readmission rate.

Notes: In Fig. 12 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the risk-adjusted AMI 30-day all-cause readmission rate as dependent variable. In Fig. 12 Panel (b), this figure plots average values and standard error bands of the AMI readmission rate by group. The regressions and raw means are weighted at the hospital-level by the share of AMI discharges that were treated at the hospital in the calendar year.



(a) Event-Study Estimates

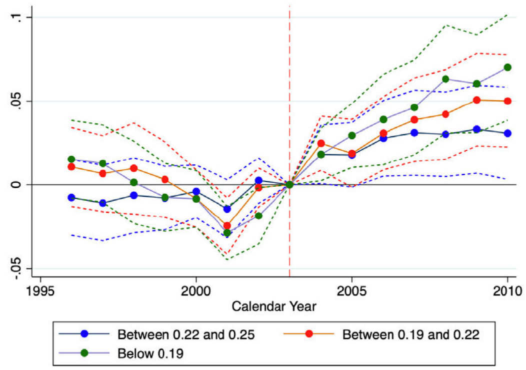


(b) Raw Means

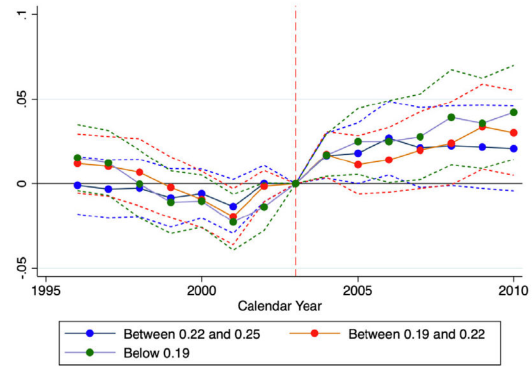
Fig. 13.

Risk-adjusted AMI in-hospital mortality rate.

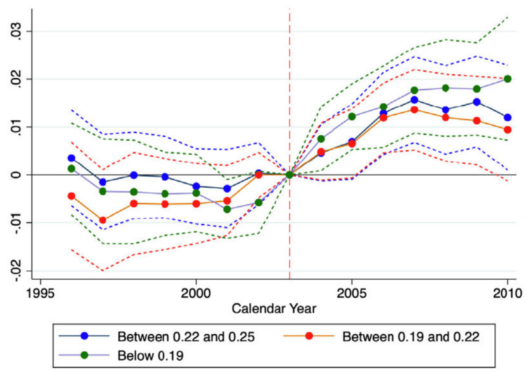
Notes: In Fig. 13 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the risk-adjusted AMI in-hospital mortality rate as dependent variable. In Fig. 13 Panel (b), this figure plots average values and standard error bands of the AMI in-hospital mortality rate by group. The regressions and raw means are weighted at the hospital-level by the share of AMI discharges that were treated at the hospital in the calendar year.



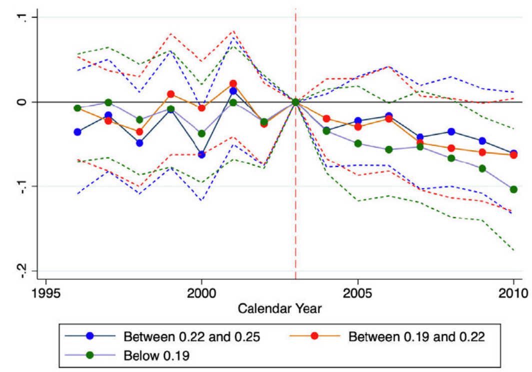
(a) Nurse-to-Patient Ratio Adj.



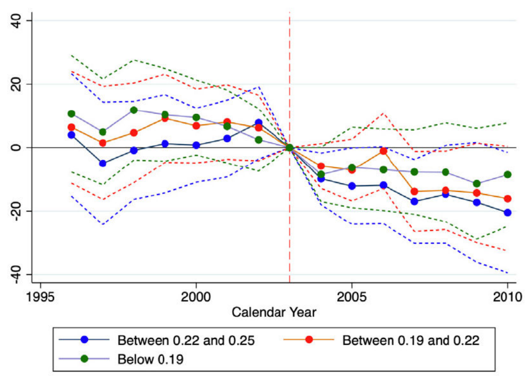
(b) RN-to-Patient Ratio Adj.



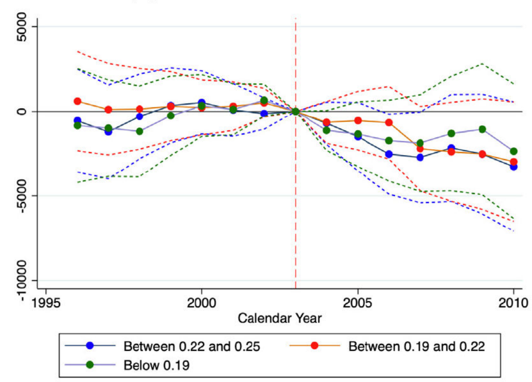
(c) LVN-to-Patient Ratio Adj.



(d) RN Real Hourly Wage



(e) Acute Care Beds



(f) Acute Care Patient Days

Fig. 14.

Heterogeneous treatment effects.

Notes: This figure plots coefficients α_{gt} and 95 percent confidence intervals from Specification (5) with the nurse-, RN-, or LVN-to-patient ratio, log of RN real hourly wage, acute care available beds, or acute care patient days as dependent variable. Standard errors are clustered at the hospital level.

Table 1

Descriptive statistics on California hospitals by initial nurse-to-patient ratio.

Variable	Below 0.25	Above 0.25	Difference
Share church or non-profit	0.56	0.74	0.18**
Share investor-owned	0.31	0.18	−0.14**
Share government-owned	0.13	0.08	−0.05
Share teaching hospitals	0.09	0.15	0.06
Share DSH hospitals	0.26	0.23	−0.03
HHI using acute patient days	1,862	2,361	498*
HHI using acute discharges	2,046	2,521	474*
Share with psychiatric unit	0.47	0.34	−0.13*
Share with chem. dependency unit	0.03	0.11	0.08**
Share with rehab. unit	0.31	0.32	0.02
Share with LT care unit	0.56	0.45	−0.11
Share with other units	0.12	0.16	0.04
Acute care patient days per year	25,918	26,202	284
Total patient days per year	59,778	58,686	−1,092
Acute care available beds	119	118	−1
Acute care length of stay	5.54	3.89	−1.66
Acute care utilization rate	0.58	0.55	−0.03
Case Mix Index	1.13	1.18	0.05
Revenues per patient day	282	351	68***
Expenses per patient day	363	486	122***
Profits per patient day	−102	−177	−76***
Medicare share of days	0.37	0.35	−0.02
MediCal share of days	0.18	0.14	−0.04*
County Indigent programs share of days	0.02	0.02	0.00
Other third-party payor share of days	0.39	0.45	0.06**
Other payor share of days	0.03	0.03	−0.00
Observations	150	62	212

Notes: Statistics are shown for 2000. This figure shows all hospitals included in my balanced estimation sample. Herfindahl–Hirschman Index (HHI) is calculated based on the acute care unit. The hospital’s ownership is measured by the health system recorded by HCAI and the healthcare market is defined as the hospital referral region by the Dartmouth Atlas of Healthcare. All financial variables are denoted in USD.

Table 2

Difference-in-differences estimates for nurse labor in acute care.

	(1) Nurse-Patient	(2) Nurse-Patient Adj.	(3) RN-Patient	(4) RN-Patient Adj.	(5) LVN-Patient	(6) LVN-Patient Adj.	(7) ln(nurse hours)
Below 0.25 × Post (1996–2006)	0.017*** (0.006)	0.029*** (0.007)	0.011 (0.007)	0.022*** (0.007)	0.012*** (0.004)	0.012*** (0.004)	0.105** (0.049)
Below 0.25 × Post (1996–2010)	0.025*** (0.007)	0.040*** (0.008)	0.012 (0.008)	0.027*** (0.008)	0.015*** (0.004)	0.015*** (0.004)	0.117** (0.055)
Below 0.25 × Post (1996–2016)	0.035*** (0.007)	0.050*** (0.008)	0.016** (0.008)	0.033*** (0.008)	0.016*** (0.004)	0.015*** (0.004)	0.092 (0.063)
Mean	0.206	0.192	0.159	0.148	0.031	0.029	11.445
R^2	0.567	0.276	0.641	0.387	0.154	0.215	0.443
Observations	4,440	4,440	4,440	4,440	4,440	4,440	4,440
Hospital FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

*$p < 0.10$

**$p < 0.05$

***$p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the nurse to patient ratio, the Case Mix Index (CMI) adjusted nurse to patient ratio, RN-to-patient ratio, adjusted RN-to-patient ratio, LVN-to-patient ratio, adjusted LVN-to-patient ratio, and log of nurse hours employed. Column 2, Model 2 indicates that the mandate significantly increased the nurse-to-patient ratio by 0.040 points relative to the mean of 0.192 (21 percent). The increase is robust to the length of the sample period.

Table 3

Difference-in-differences estimates for non-nurse labor in acute care.

	(1) Aide-Patient	(2) Aide-Patient Adj.	(3) Productive-Patient	(4) Productive-Patient Adj.	(5) ln(physician exp. ppd)	(6) ln(physician exp. ppd adj.)
Below 0.25 × Post (1996–2006)	−0.011** (0.005)	−0.006 (0.005)	0.010 (0.009)	0.026*** (0.009)	−0.152 (0.143)	−0.108 (0.140)
Below 0.25 × Post (1996–2010)	−0.013** (0.006)	−0.008 (0.005)	0.016 (0.010)	0.036*** (0.011)	−0.088 (0.128)	−0.036 (0.125)
Below 0.25 × Post (1996–2016)	−0.012 (0.007)	−0.007 (0.005)	0.023** (0.011)	0.043*** (0.012)	0.096 (0.126)	0.143 (0.121)
Mean	0.097	0.089	0.334	0.312	1.908	1.849
R^2	0.110	0.040	0.490	0.165	0.257	0.270
Observations	4,440	4,440	4,440	4,440	4,440	4,440
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the aide to patient ratio, the Case Mix Index (CMI) adjusted aide to patient ratio, productive staff to patient ratio, adjusted productive staff to patient ratio, log of expenditures on physicians per patient day, and log of expenditures on physicians per adjusted patient day. The declines in the aide-to-patient ratios in Columns 1 and 2 indicate some substitution between aides and licensed nurses. However, the substitution was minimal and Column 4, Model 2 finds an increase in the productive staff to patient ratio by 0.036 points relative to the pre-event treatment group mean (12 percent). The increase is robust to the length of the sample period. Standard errors are clustered at the hospital level.

Table 4

Difference-in-differences estimates for wages in acute care.

	(1) ln(RN real hrly wage)	(2) ln(LVN real hrly wage)	(3) ln(non-nurse real hrly wage)
Below 0.25 × Post (1996–2006)	−0.017 (0.015)	−0.015 (0.018)	−0.011 (0.019)
Below 0.25 × Post (1996–2010)	-0.033^{**} (0.016)	−0.024 (0.023)	−0.008 (0.019)
Below 0.25 × Post (1996–2016)	-0.051^{***} (0.018)	−0.014 (0.026)	−0.021 (0.022)
Mean	3.069	2.598	2.357
R^2	0.527	0.099	0.249
Observations	4,438	3,991	4,440
Hospital FE	✓	✓	✓
Year FE	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

$^{*}p < 0.10$

$^{**}p < 0.05$

$^{***}p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the RN real hourly wage, LVN real hourly wage, and real hourly wage of all directly employed workers excluding RNs, LVNs, and registry nurses. The latter group includes staff in the categories: management and supervision, technicians and specialists, aides and orderlies, clerical and other administrative, environmental and food service, salaried physicians, and non-physician medical practitioners. Physicians are normally employed as contractors whose hours are not reported to the health department, which is why expenditures on physicians are reported separately in Columns 5 and 6 of Table 2. Column 1, Model 2 indicates a decline in the RN real hourly wage by 3.3 percent. LVNs experienced a wage decline as well but the estimate is imprecisely estimated and statistically insignificant (Column 2, Model 2). There does not appear to be an effect on non-nurse wages (Column 3, Model 2).

Table 5

Average hourly wage of California hospital RNs, by age and education.

	Diploma or associate's	Bachelor's	Master's or PhD
Under 25	19.96	25.99	.
Aged 25–29	26.69	26.31	28.28
Aged 30–34	28.36	30.18	30.63
Aged 35–39	29.29	30.18	34.53
Aged 40–44	28.46	32.40	35.72
Aged 45–49	31.34	34.20	36.63
Aged 50–54	32.22	36.55	35.89
Aged 55–59	32.14	34.35	38.05
Aged 60–64	32.31	31.74	36.27
Over 65	27.98	46.82	33.74

Notes: Sample consists of all RNs employed as nurses in a hospital setting in California at the time of the survey for survey years 2000, 2004, and 2008. The average hourly wage is denominated in 2000 USD. This table shows that within education bin, older nurses earn higher wages on average than younger ones. This trend flips around age 60 for diploma and graduate degree holders.

Table 6

Changes in the composition of RNs employed at hospitals.

Share of RNs	California		Other states	
	Pre	Post	Pre	Post
Age				
Under 25	0.03	0.04	0.05	0.05
Aged 25–29	0.13	0.11	0.15	0.10
Aged 30–34	0.18	0.13	0.19	0.11
Aged 35–39	0.16	0.11	0.17	0.12
Aged 40–44	0.16	0.12	0.15	0.13
Aged 45–49	0.13	0.15	0.11	0.15
Aged 50–54	0.09	0.15	0.08	0.15
Aged 55–59	0.07	0.12	0.06	0.11
Aged 60–64	0.04	0.06	0.03	0.05
Over 65	0.01	0.02	0.01	0.02
Experience				
Employed in nursing last year	0.95	0.94	0.95	0.96
Licensed in past 5 years	0.20	0.15	0.21	0.15
Licensed in past 10 years	0.34	0.34	0.38	0.31
Education				
Diploma or associate’s degree	0.53	0.38	0.56	0.41
Bachelor’s degree	0.32	0.38	0.30	0.37
Master’s or PhD degree	0.06	0.24	0.05	0.22

Notes: Sample consists of all RNs employed as nurses in a hospital setting at the time of the survey. Survey years for the pre and post periods are 1977, 1980, 1984, 1988, 1992, 1996, and 2000 and 2004, 2008, and 2018, respectively. The “Other States” average is constructed as follows: first, in each data year I construct a weighted average across non-California states. Then, I construct an unweighted average across data years for the pre and post periods. This table shows a relative growth in RNs under age 35 and licensed in the past ten years in California compared to other states.

Table 7

Difference-in-differences estimates for average costs in acute care.

	(1) ln(supplies ppd adj.)	(2) ln(leases ppd adj.)	(3) ln(salaries ppd adj.)	(4) ln(dir. costs ppd adj.)	(5) ln(alloc. costs ppd adj.)	(6) ln(costs ppd adj.)
Below 0.25 × Post (1996–2006)	0.143 (0.147)	0.139 (0.279)	0.069*** (0.026)	0.049 (0.025)	−0.012 (0.038)	0.025 (0.032)
Below 0.25 × Post (1996–2010)	0.190 (0.150)	0.065 (0.297)	0.087** (0.034)	0.078** (0.034)	0.051 (0.040)	0.071 (0.037)
Below 0.25 × Post (1996–2016)	0.256 (0.153)	0.255 (0.292)	0.095** (0.039)	0.098** (0.039)	0.080 (0.044)	0.089** (0.041)
Mean	−0.282	−2.173	5.060	5.210	5.052	5.865
R^2	0.617	0.043	0.662	0.603	0.276	0.448
Observations	4,434	3,375	4,440	4,440	4,440	4,440
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses. Standard errors are clustered at the hospital level.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on Model 3 in each column that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the log of expenditures on supplies, log of expenditures on capital leases, log of expenditures on salaries, log of total direct expenditures, log of total allocated expenditures (expenditures that accrue to the hospital and that are allocated back to the hospital unit based on usage), and log of total costs (sum of direct and allocated costs). All costs are per adjusted patient day. The coefficients in Columns 1 and 2 indicate increases, albeit statistically insignificant, in the expenditures on intermediate inputs and leases. In the medium-term these expenditures increase by 19 and 6.5 percent, respectively. Salary expenditures increased by 8.7 percent (Column 3, Model 2), direct expenditures by 7.8 percent (Column 4, Model 2), and total costs by a statistically insignificant 7.1 percent (Column 6, Model 2).

Table 8

Difference-in-differences estimates for output in acute care.

	(1) Available beds	(2) Staffed beds	(3) Patient days	(4) Utilization rate	(5) Discharges	(6) Length of stay
Below 0.25 × Post (1996–2006)	−12.266*** (4.552)	−11.192** (4.477)	−1147.245 (1048.506)	0.045*** (0.017)	−754.344 (512.026)	−0.323 (0.377)
Below 0.25 × Post (1996–2010)	−15.625*** (5.132)	−13.195*** (4.973)	−1769.781 (1273.792)	0.045** (0.019)	−910.698 (538.776)	−0.507 (0.413)
Below 0.25 × Post (1996–2016)	−16.527*** (5.860)	−15.455*** (5.085)	−3521.592** (1558.710)	0.010 (0.021)	−1209.133** (577.594)	−0.784 (0.428)
Mean	118.220	104.305	23954.818	0.556	7430.942	5.048
R^2	0.093	0.103	0.162	0.076	0.039	0.023
Observations	4,440	4,440	4,440	4,440	4,440	4,440
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 × Post) over the short (1996–2006), medium (1996–2010), and long terms (1996–2016). The reported observations and R^2 shown are based on the preferred, long term model that exploits the full sample period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the number of available beds, number of staffed beds, number of patient days, bed utilization rate, number of discharges, and length of stay in days. Column 1, Model 2 indicates a reduction in capacity (14 percent) that was immediate and stable in the long-term. In the medium-term, there was a decline in patient days that was statistically insignificant and due to both declines in discharges and length of stay (also statistically insignificant). Consequently, there was an increase in utilization rates (8 percent).

Table 9

Difference-in-differences estimates for AMI length of stay, 30-day readmission rate, and in-hospital mortality rate.

	(1) Readmission rate	(2) Risk-Adjusted readmission	(3) Length of stay	(4) Risk-Adjusted LoS	(5) In-Hospital mortality rate	(6) Risk-Adjusted mortality
Below 0.25 × Post	−0.002 (0.006)	−0.004 (0.005)	−0.273*** (0.102)	−0.281*** (0.098)	−0.002 (0.002)	−0.003 (0.002)
Observations	2,080	2,080	2,080	2,080	2,431	2,431
R^2	0.604	0.519	0.695	0.677	0.473	0.376
Mean	0.244	0.033	6.233	0.355	0.086	0.012
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of the treatment effect over the 1996–2008 period. Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. Admissions from the last month of 2008 are excluded because readmission rates cannot be measured for these admissions. Columns 1–4 use a sample that excludes any admissions for AMI that are within 365 days prior to another AMI admission and excludes admissions for AMI where the patient died in hospital. Columns 5–6 include both of these sets of admissions in addition to the sample used in Columns 1–4. Hospital-year observations are dropped if the hospital has fewer than 10 resulting AMI discharges per calendar year. The regressions are weighted at the hospital-level by the share of AMI discharges that were treated at the hospital in the calendar year. The dependent variables are the 30-day all cause readmission rate, risk-adjusted 30-day all cause readmission rate, length of stay, risk-adjusted length of stay, in-hospital mortality rate, and risk-adjusted in-hospital mortality rate. Column 4 indicates that AMI patients at treated hospitals experienced a decline in length of stay by 0.281 days on a mean of 6.233 days (5 percent) with no statistically significant effect on the risk-adjusted readmission rate (Column 2) or the risk-adjusted in-hospital mortality rate (Column 6).

Table 10

Difference-in-differences estimates for nurse labor in acute care by initial ratio level, 1996–2010.

	(1) Nurse-Patient	(2) Nurse-Patient Adj.	(3) RN-Patient	(4) RN-Patient Adj.	(5) LVN-Patient	(6) LVN-Patient Adj.	(7) ln (nurse hours)
Post	0.059*** (0.007)	0.008 (0.009)	0.096*** (0.008)	0.048*** (0.008)	−0.019*** (0.004)	−0.021*** (0.003)	0.248*** (0.054)
Between 0.22 and 0.25 × Post	0.017** (0.008)	0.033*** (0.009)	0.011 (0.009)	0.025*** (0.009)	0.011** (0.005)	0.012*** (0.004)	0.065 (0.067)
Between 0.19 and 0.22 × Post	0.026*** (0.008)	0.037*** (0.009)	0.011 (0.009)	0.022** (0.009)	0.015*** (0.005)	0.015*** (0.004)	0.092 (0.057)
Below 0.19 × Post	0.033*** (0.009)	0.051*** (0.010)	0.016 (0.010)	0.034*** (0.011)	0.020*** (0.005)	0.019*** (0.004)	0.193*** (0.069)
Mean	0.278	0.248	0.212	0.189	0.042	0.039	11.693
R^2	0.509	0.314	0.535	0.354	0.066	0.092	0.487
Observations	3,169	3,169	3,169	3,169	3,169	3,169	3,169
Hospital FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of categorical treatment effects from the estimation of a single model in each column using the medium term sample (1996–2010). Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the nurse to patient ratio, the Case Mix Index (CMI) adjusted nurse to patient ratio, RN-to-patient ratio, adjusted RN-to-patient ratio, LVN-to-patient ratio, adjusted LVN-to-patient ratio, and log of nurse hours employed. The coefficients in Column 2 indicate that the mandate significantly increased the nurse-to-patient ratio by 0.036 points for hospitals between 0.22 and 0.25 initial ratio, by 0.050 points for hospitals between 0.19 and 0.22 initial ratio, and by 0.064 points for hospitals below 0.19 initial ratio. Hospitals below 0.25 are considered treated in the main specification with binary treatment. The magnitude of the treatment effect scales based on initial ratio as we would expect.

Table 11

Difference-in-differences estimates for non-nurse labor in acute care by initial ratio level, 1996–2010.

	(1) Aide-Patient	(2) Aide-Patient Adj.	(3) Productive-Patient	(4) Productive-Patient Adj.	(5) ln(physician exp. ppd)	(6) ln(physician exp. ppd adj.)
Post	0.009 (0.006)	−0.005 (0.005)	0.091*** (0.011)	0.022 (0.012)	−1.444*** (0.175)	−1.613*** (0.173)
Between 0.22 and 0.25 × Post	−0.016** (0.007)	−0.010 (0.006)	0.005 (0.011)	0.025** (0.013)	−0.141 (0.184)	−0.075 (0.182)
Between 0.19 and 0.22 × Post	−0.013 (0.007)	−0.008 (0.006)	0.013 (0.011)	0.029** (0.013)	0.024 (0.207)	0.067 (0.206)
Below 0.19 × Post	−0.011 (0.007)	−0.005 (0.006)	0.030** (0.013)	0.053*** (0.014)	0.163 (0.185)	0.237 (0.183)
Mean	0.076	0.067	0.383	0.340	2.205	2.070
R^2	0.087	0.060	0.406	0.193	0.282	0.284
Observations	3,169	3,169	3,169	3,169	2,940	2,940
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of categorical treatment effects from the estimation of a single model in each column using the medium term sample (1996–2010). Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the aide to patient ratio, the Case Mix Index (CMI) adjusted aide to patient ratio, productive staff to patient ratio, adjusted productive staff to patient ratio, log of expenditures on physicians per patient day, and log of expenditures on physicians per adjusted patient day. The coefficients in Column 4 indicate that the mandate significantly increased the productive staff-to-patient ratio by 0.025 points for hospitals between 0.22 and 0.25 initial ratio, by 0.029 points for hospitals between 0.19 and 0.22 initial ratio, and by 0.053 points for hospitals below 0.19 initial ratio. Hospitals below 0.25 are considered treated in the main specification with binary treatment. The magnitude of the treatment effect scales based on initial ratio as we would expect.

Table 12

Difference-in-differences estimates for wages in acute care by initial ratio level, 1996–2010.

	(1) ln(RN real hrly wage)	(2) ln(LVN real hrly wage)	(3) ln(non-nurse real hrly wage)
Post	0.124*** (0.018)	0.016 (0.029)	0.088*** (0.019)
Between 0.22 and 0.25 × Post	−0.014 (0.020)	0.002 (0.027)	0.014 (0.025)
Between 0.19 and 0.22 × Post	−0.034 (0.018)	−0.039 (0.027)	−0.022 (0.025)
Below 0.19 × Post	−0.051** (0.021)	−0.034 (0.032)	−0.015 (0.023)
Mean	3.097	2.622	2.455
R^2	0.529	0.158	0.236
Observations	3,168	3,027	3,169
Hospital FE	✓	✓	✓
Year FE	✓	✓	✓

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of categorical treatment effects from the estimation of a single model in each column using the medium term sample (1996–2010). Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the RN real hourly wage, LVN real hourly wage, and real hourly wage of all directly employed workers excluding RNs, LVNs, and registry nurses. The latter group includes staff in the categories: management and supervision, technicians and specialists, aides and orderlies, clerical and other administrative, environmental and food service, salaried physicians, and non-physician medical practitioners. Physicians are normally employed as contractors whose hours are not reported to the health department, which is why expenditures on physicians are reported separately. The coefficients in Column 4 indicate that the mandate significantly decreased the RN hourly wage by 5.1 percent for hospitals below a 0.19 initial ratio. Hospitals below 0.25 are considered treated in the main specification with binary treatment. The magnitude of the treatment effect scales based on initial ratio as we would expect.

Table 13

Difference-in-differences estimates for average costs in acute care by initial ratio level, 1996–2010.

	(1) ln(supplies ppd adj.)	(2) ln(leases ppd adj.)	(3) ln(salaries ppd adj.)	(4) ln(dir. costs ppd adj.)	(5) ln(alloc. costs ppd adj.)	(6) ln(costs ppd adj.)
Post	0.050 (0.116)	0.104 (0.288)	0.281*** (0.036)	0.173*** (0.035)	0.163*** (0.042)	0.167*** (0.038)
Between 0.22 and 0.25 × Post	0.291 (0.191)	−0.212 (0.359)	0.088** (0.038)	0.074** (0.037)	0.065 (0.048)	0.076 (0.041)
Between 0.19 and 0.22 × Post	0.194 (0.182)	0.274 (0.406)	0.065 (0.036)	0.071 (0.037)	0.051 (0.046)	0.083** (0.041)
Below 0.19 × Post	0.089 (0.211)	0.126 (0.374)	0.107*** (0.041)	0.090** (0.040)	0.039 (0.047)	0.054 (0.042)
Mean	−0.091	−2.065	5.246	5.401	5.195	6.039
R^2	0.632	0.037	0.674	0.656	0.098	0.383
Observations	3,163	2,447	3,169	3,169	3,169	3,169
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of categorical treatment effects from the estimation of a single model in each column using the medium term sample (1996–2010). Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the log of expenditures on supplies, log of expenditures on capital leases, log of expenditures on salaries, log of total direct expenditures, log of total allocated expenditures (expenditures that accrue to the hospital and that are allocated back to the hospital unit based on usage), and log of total costs (sum of direct and allocated costs). All costs are per adjusted patient day. Hospitals below 0.25 are considered treated in the main specification with binary treatment.

Table 14

Difference-in-differences estimates for output in acute care by initial ratio level, 1996–2010.

	(1) Available beds	(2) Staffed beds	(3) Patient days	(4) Utilization rate	(5) Discharges	(6) Length of stay
Post	21.406*** (5.116)	4.096 (5.076)	3754.413*** (1236.756)	−0.042** (0.018)	1636.050*** (541.546)	−0.096 (0.429)
Between 0.22 and 0.25 × Post	−16.119** (6.264)	−15.314** (6.357)	−2045.264 (1536.527)	0.045 (0.024)	−561.667 (721.656)	−0.961 (0.738)
Between 0.19 and 0.22 × Post	−15.578** (6.096)	−10.688 (5.866)	−1988.956 (1398.827)	0.039 (0.022)	−1473.177** (670.650)	−0.460 (0.451)
Below 0.19 × Post	−15.191** (6.001)	−13.683** (5.779)	−1277.384 (1541.469)	0.050 (0.029)	−677.706 (542.337)	−0.622 (0.495)
Mean	120.695	105.970	25509.792	0.545	7657.399	4.049
R^2	0.058	0.028	0.235	0.093	0.052	0.014
Observations	3,169	3,169	3,169	3,169	3,169	3,169
Hospital FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses.

* $p < 0.10$

** $p < 0.05$

*** $p < 0.01$.

Notes: This table shows difference-in-differences estimates of categorical treatment effects from the estimation of a single model in each column using the medium term sample (1996–2010). Mean shown is the mean for the treatment group prior to the event year given the level differences in outcomes between the treatment and control groups prior to the event. The dependent variables are the number of available beds, number of staffed beds, number of patient days, number of discharges, and length of stay in days. Hospitals below 0.25 are considered treated in the main specification with binary treatment.