How Do Hospitals Respond to Input Regulation? Evidence from the California Nurse Staffing Mandate

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1 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 minute increase in nursing time per patient day and 9 percent increase in the wage bill per patient day in acute care among treated hospitals. Hospitals responded on several margins: increased their use of lower-licensed and younger nurses, reduced capacity by 16 beds (12 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). I investigate whether the decline in length of stay is indicative of higher care quality or hospitals discharging patients "quicker and sicker" due to capacity constraints. I find no effect on the 30-day readmission rate and conclude there was an increase in care quality per day for AMI.

Keywords: Minimum staffing ratios, Staffing, Nurses, Hospitals, Healthcare quality
JEL Codes: D22, H75, I10, I11, J44, J23

2 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

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and at the federal level.[1] A stated intention of minimum ratios is to increase patient welfare through improved healthcare quality.[2] 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 (Gruber and Kleiner, 2012; Friedrich and Hackmann, 2021).[3]

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 are limited and the production technology is unique to the sector, therefore these questions must be answered empirically.

In this paper, I utilize 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 general acute care hospitals to meet minimum nurse-to-patient ratios established at the hospital unit level (e.g. 0.2 in acute care) 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 several other margins: increased their use of lower-licensed and younger nurses, reduced capacity by 16 beds (12 percent), and increased bed utilization rates by 0.045 points (8 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. Low length of stay is often used as an indicator for high quality of care (Bartel et al., 2014). However, discharging patients "quicker and sicker" may be one way hospitals respond to capacity constraints (Friedman and Pauly, 1981; Hoe, 2022). In light of the substantial capacity reduction that I document, I investigate whether the decline in length of stay is indicative of early discharge or higher care quality. Contrary to the expectations under a "quicker and sicker" hypothesis, I find no effect on the 30-day AMI readmission rate despite the decline

[1] Only California and Massachusetts have mandated minimum 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 would implement minimum nurse-to-patient ratios in hospitals. California voters recently rejected Proposition 29 which would have mandated a minimum number of licensed healthcare professionals in dialysis clinics.

[2] 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."

[3] Descriptive studies of the mandate including Burnes Bolton et al. (2007) and Donaldson et al. (2005) also do not find quality effects of the mandate.

in length of stay. I conclude that there was an increase in care quality per day for AMI.

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 acute care unit of hospitals initially below and above the mandated minimum ratio threshold in acute care. Hospitals below and above the threshold in acute care were located in most of the same geographic markets for hospital services. In a heterogeneity analysis, I exploit the continuity of treatment and show that in line with expectations the treatment effect increases with the gap between the hospital's initial staffing ratio and the threshold.

Second, the mandated ratios were established at the unit level and created variation in the "bite" of the mandate across hospital units within a hospital. In some hospital units (e.g. acute care), the majority of hospitals were initially below the unit-specific mandated threshold whereas in other units (e.g. 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). [4] In a robustness specification, I estimate my model on outcomes from the intensive care unit as a placebo test. I do not find any significant effects on labor, capacity, or output in the intensive care unit in which mandated ratios were already in place.

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 for estimation. 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 (17 percent) for treated hospitals. This implies an additional 58 minutes of nursing time per patient day.[5] I show that 36 minutes came from Registered Nurses (RNs) and 22 minutes from lower-licensed Licensed Vocational Nurses (LVNs). I show that substitution away from other labor (aide, physician) 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 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

[4] 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.

[5] 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 minutes of additional nursing time per patient day.

sources that 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 using 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 129 beds (12 percent) and increased utilization rates by 0.045 points on a mean of 0.547 (8 percent) almost immediately after the mandate. This suggests that hospitals were operating with excess capacity prior to the mandate and reduced excess capacity in response to the rise in costs per staffed bed. I also show descriptive evidence of declines in patient days and length of stay that are not statistically significant in the medium-term.

Finally, using administrative data on AMI discharges, I estimate the effects on risk-adjusted length of stay and risk-adjusted 30-day readmission rates. I find a decline in length of stay of 0.281 days on a mean of 6.153 days (5 percent) which corroborates the descriptive evidence on the decline in length of stay from the hospital financial data. Low length of stay is often used as an indicator for high quality of care (Bartel et al., 2014). However, discharging patients "quicker and sicker" may be one way hospitals respond to capacity constraints. Recent evidence from U.K. hospitals documents that declines in length of stay due to capacity constraints are associated with higher readmission rates Hoe (2022). I investigate this possibility but find that the 30-day AMI readmission rate is stable despite the decline in AMI length of stay. The 30-day readmission rate declines by a statistically insignificant .004 points (2 percent). I conclude that treated hospitals experienced an increase in care quality per patient day for AMI.

My event-study estimates show that the decline in length of stay increases over time. I find a statistically insignificant decline in length of stay of 2.6 percent within one year of the mandate that increases to 6.9 percent and becomes significant three years after the mandate which is qualitatively consistent with quality returns to tenure in nursing.

My event-study results also indicate that the effects on nurse labor, the wage bill, and capacity were not temporary. This is consistent with the mandate remaining in place through the start of the COVID-19 pandemic when it was temporarily suspended.

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[6] 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

[6]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.

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 are larger for hospitals with the lowest initial ratios prior to the mandate.

My paper contributes to several literatures. First, I contribute the literature on the quality returns to nursing. 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 an increase in one year of average tenure across all RNs on the current nursing unit leads to a non-linear, average 1.3 percent decline in length of stay. Their estimates imply that the 36 and 22 minutes of additional RN and LVN time per patient day that I find due to the mandate would lead to an immediate 3.1 percent decline in length of stay and increase over time. My findings are qualitatively consistent with these implications.

My paper complements prior literature that finds no or mixed effects of the mandate on in-hospital quality indicators when estimated over the 2004-2006 post-mandate period (Cook et al., 2012; Mark et al., 2013; Spetz et al., 2013). I focus on length of stay and 30-day readmissions for AMI, as opposed to in-hospital quality indicators, and find an increase in quality. My estimation over a slightly longer 2004-2008 post-mandate period provides suggestive evidence of dynamic quality effects and indicates that findings may depend on the period over which the effects are estimated. My findings qualitatively confirm prior work on minimum ratios in nursing homes (Lin, 2014) and the quality returns to nursing (Gruber and Kleiner, 2012; Friedrich and Hackmann, 2021).

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 that there was limited crowding out of other inputs in response to the mandate. My estimates of the cost effects complement descriptive estimates in prior work (Terasawa, 2016) which are far larger than my causal estimates suggest.[7]

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).

[7] 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).

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). My finding of a decline in the RN wage at treated hospitals is suggestive of hospitals employing younger and more recently licensed RNs. This finding complements Matsudaira (2014), who finds no change in the unlicensed aide wage at long-term care facilities treated by a 1999 California staffing mandate in long-term care. I focus on RNs rather than aides at treated facilities. I uncover relative to Matsudaira (2014) that heterogeneity in nurse composition is important to control for when testing for monopsony.

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

3 Institutional Context

3.1 Nursing in the Hospital Setting

In 2000, California had 351 general acute care (GAC) hospitals providing 24-hour inpatient care. [8] The medical/surgical acute care unit (hereafter "acute care unit") treats patients of lower acuity relative to intensive care.[9] Acute care constituted 46 percent of GAC hospitals' total inpatient days and 57 percent of discharges. Acute care attends to pre- and post-surgical patients and stroke, heart attack, and pneumonia patients, among others. Additionally, all patients are stabilized in acute care prior to hospital discharge. 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.[10]

Nurses are viewed as not only central to the volume of services provided but also to the quality because nurses' tasks limit the occurrence of adverse events. These tasks include "(1) ongoing monitoring and assessment of their patients, and, as necessary, initiating interventions to 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). Prior to the mandate there was wide variation in nurse-to-patient ratios across hospitals which I will exploit to identify the causal effect of the mandate.

[8] Kaiser Permanente hospitals are excluded because they were not required to report hospital level financials to HCAI until the fiscal year ended 12/31/2021.

[9] Acute care encompasses multiple hospital units including medical/surgical acute care, pediatric acute care, psychiatric acute care, obstetrics, and labor and delivery. In this paper, I focus on the medical/surgical acute care given that most hospitals in the sample have this hospital unit.

[10] 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.

3.1.1 Variation in Nursing Quality

Hospitals choose the quality of nursing hours 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 two to four years of training culminating in either a diploma from a nursing program (two years of training), an Associate of Applied Science in Registered Nursing degree (two years), or a Bachelor of Science in Nursing degree (four years). 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 which may partly reflect 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. Evidence of input quality variation within licensing type suggest that in response to an input quantity regulation hospitals may respond by using more lower-skilled inputs. I will show in Sections 5.1 that this is what happened.

3.1.2 Pre-Existing Regulatory Constraints

Prior to the mandate, the hospital's input 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).

Second, legislation passed in the 1976-1977 legislative session established minimum nurse-to-patient ratios in 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. The ratios for these units that were passed in the 1999 mandate were identical to the ones passed in 1976-1977. I will exploit the variation in the "bite" of the mandate between the acute and intensive care units for identification.

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 4.1.2, I discuss my use of a hospital level patient severity index to control for variation in staffing and cost trends over time.

3.2 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.[11] 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 consequent increases in inpatient acuity and declines in nursing staff were often cited as reasons for the perceived low staffing ratios. CNA argued that these ratios 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 ratios according to licensed nurse classification (RN, LVN) and inpatient hospital unit after a public comment period. The June amendment specified that DHS would need to establish 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, hospitals knew that minimum ratios would be announced no earlier than January 2002 for an implementation date down the road. The full timeline of the public comment and implementation periods is shown in Figure 1.

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 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

[11] 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.

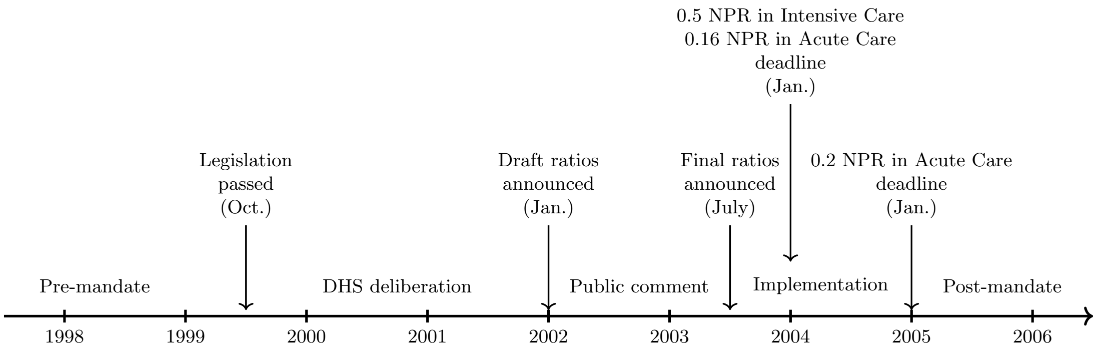


Figure 1: Mandate Timeline in Acute and Intensive Care Units

Notes: Sources are DHS, Los Angeles Times, and California Legislative Information

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.

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. However, 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 medical/surgical intensive care unit and state regulation in place since the late 1970s already specified that LVNs could make up no more than 50 percent of nursing hours in intensive care. As a result, I do not consider the regulation on LVN share to be a relevant constraint to the hospital in either unit.

3.3 Penalties and Allowances for Special Circumstances

During my sample period, there were no specified administrative penalties for non-compliant hospitals.[12] 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".[13] Figure A.4b presents a histogram of the unadjusted nurse-to-patient ratio in 2006. It indicates that seven of 212 hospitals 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.

[12]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.

[13]Statistics on the numbers of out-of-ratio deficiencies vary widely. One source states that there were 235 penalties 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).

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 can observe if hospitals are designated as small and rural hospitals by DHS. I have 62 small and rural general acute care hospitals in my sample 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.

3.4 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 were employed in nursing, and the average RN vacancy rate in California was 20 percent (GAO, 2001). Therefore it's 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. In Figure A.1a, I plot the average number of active nurse (RN and LVN) licenses per 100 persons in California and other states. Figure A.1b 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 and the mandate legislation was passed (October 1999) but ratios were not known. 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). Figure A.2 plots entrants as a share of active licenses and shows that the growth was largely due to an increase in new entrants.

I find that the increase in new entrants was from a combination of nurses being endorsed from out-of-state and nurses being examined in state. California followed and continues to follow 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 if licensed out-of-state (LAO, 2007). [14]

[14] It is notable that in 2000, four states passed a Nursing Licensure Compact (NLC) into law that would allow mutual

In Figure A.3, I use licensing data from the NCSBN to show the numbers of newly-licensed RNs that obtained licenses through examination in California or through endorsement from out-of-state in each year between 1996 and 2014. The figure suggests that the growth in the RN labor force came from a combination of the two channels.

This is consistent with the institutional context. 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 out-of-state and the number of people training as nurses in-state.

4 Data and Empirical Framework

4.1 Data and Variable Construction

4.1.1 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 in which I show longer pre-mandate trends, I utilize years 1990-2016.

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

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.

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 labor inputs by type.

4.1.2 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. [15] As mentioned in Section 3.1.2, 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 acute patients. A higher CMI reflects a case mix that is more resource-intensive. [16] CMS and HCAI both produce hospital level CMI 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 CMI data to account for input use and costs variation in my sample. Most hospitals in my sample attribute less than 15 percent of their patient days to Medicare or Medicaid payors therefore the HCAI CMI data is a far more accurate measure of patient severity than CMS CMI data.[17]

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 input prices) 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. All of the outcome variables are winsorized at the 2 and 98 percentiles.

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 5.1 that my results on nurse labor use are remarkably similar to the previous literature regardless of the measure.

[15] 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.

[16] 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."

[17] 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.

4.1.3 Labor Market Data

I use quadrennial survey data between 1977 and 2018 from the National Sample Survey of Registered Nurses (NSSRN) and licensure data for RNs and LVNs between 1996 and 2014 from the National Council of State Boards of Nursing (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 the state and those whose licenses from another state or territory were endorsed.

4.1.4 Quality Data

Finally, to estimate the effects on quality I use administrative data on patient discharges from the California Department of Health. 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 cause within 30 days of the discharge date. 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 and readmission rates, I follow Grieco and McDevitt (2017) and regress the unadjusted variable on the set of risk-adjusters and obtain the residuals from these regressions.

4.2 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 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 input prices, and logged costs. γ_i and ξ_t denote hospital and year fixed effects. For y_{it} that are input quantities or costs, I scale them by the output quantity. For example, I measure 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-hour 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 at least one point in the year if not on average. 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. 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 Figure 3b, 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 treatment assignment relies on the stability of the nurse-to-patient ratio at hospitals over time. 87 percent of treated hospitals would have been classified as treated based on their 1996, 1997, 1998, or 1999 ratios. A smaller majority of 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 trends in the unadjusted ratio among control hospitals shown in Figure A.10 (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. However, the variation in the ratio is not sufficiently stable to employ a continuous treatment variable. 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 may lead to attenuation bias from measurement error.

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).

4.3 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 being below the threshold across years relative to the omitted year, $t = 2003$. By estimating α_t for $t \in \{1996, 2016\}$ I am able to (1) graphically inspect the identifying parallel trends as-

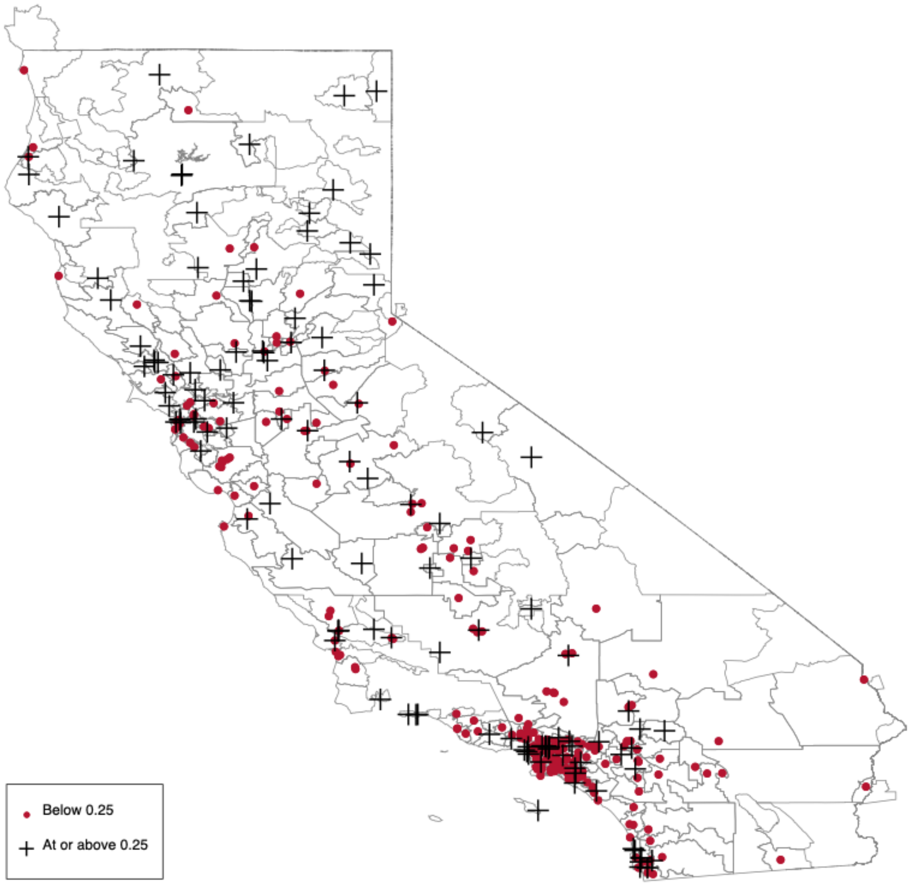


Figure 2: California Hospitals by Initial Nurse-to-Patient Ratio in Acute Care Unit

Notes: This figure shows all hospitals included in my balanced estimation sample and 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. Treated hospitals (below 0.25) and control hospitals (at or above 0.25) are located in most of the same geographic markets for healthcare.

sumption between hospitals that were treated by the mandate and those below the threshold (α_t should be not be statistically different from zero for $t \in \{1996, 2002\}$); and (2) use knowledge of the timing of the mandate and its mechanisms as a robustness check. Estimation of α_t for the 13-year post-period of my data allows me to confirm that the estimated effects are not transitory. We should expect the estimated effects on staffing and costs to be permanent because the mandate remained in place through the start of the COVID-19 pandemic in 2020. The estimation of α_t for the 7-year or 13-year pre-periods of my data allow me to graphically inspect that the estimated effects are not due to mean reversion (Ashenfelter and Card, 1985; Heckman et al., 1999).

4.4 Research Design Validity

The identifying assumption for each of these specifications is that outcomes for hospitals above and below the minimum ratio threshold would have evolved on parallel trends in the absence of the mandate. In Figure 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 in the previous section. These hospitals are present in Figure 2 but 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 nurse staffing is positively correlated with the Case Mix Index, indicating that higher staffing hospitals are on average higher patient acuity, and negatively correlated with patient days, indicating that higher staffing hospitals are also on average 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 admissions and case mix that may lead to higher staffing ratios.

In Figure 3b, 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.

5 Results

5.1 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.

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 acute care. 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.

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 0.241. This corresponds to a 58 minute increase in nursing time per patient day. [18] Columns 4 and 6, Model 2 indicate 0.027 and 0.015 point increases in the RN- and LVN-to-patient ratios, corresponding to 36 and 22 minute increases in RN and LVN time per patient day. [19]

The event-study estimates for the patient severity-adjusted nurse-, RN-, and LVN-to-patient ratios are shown in Figures 3a, 4a, and 5a and raw means for each group with standard error bands in Figures 3b, 4b, and 5b. Figures 3-5 indicate that there are no differential pre-trends in staffing between the two groups prior to the mandate. In Appendix Figure A.5, I present the event-study estimates and raw means for the unadjusted nurse-to-patient ratio. Comparing Figure 3 with Appendix Figure A.5, 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.

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 minute 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 minute or 17 percent increase in nursing time and 5 percent increase in 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 minute 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 minute 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 36 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 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 where hospitals' use of LVNs may see larger increases due to more relaxed scope of practice regulations.

[18]This is 0.025 and 36 minutes based on Column 1. I obtain this figure as follows. 0.035 is the increase in the number of nursing hours per patient hour. I multiply 0.035 by 60 minutes per hour to obtain the increase in the number of nursing minutes per patient hour (2.1 minutes per patient hour). I then multiply this by 24 hours per patient day to obtain the increase in the number of nursing minutes per patient day (50.4 minutes per patient day).

[19]Columns 3 and 5 indicate 0.012 and 0.15 point increases in the unadjusted RN- and LVN-to-patient ratios, corresponding to 17 and 23 minute increases in RN and LVN time per patient day.

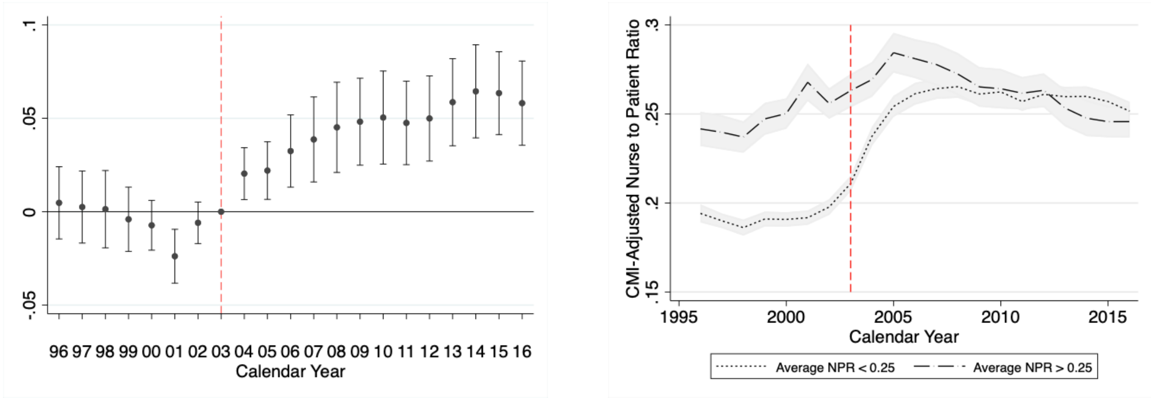
Table 2: Difference-in-Differences Estimates for Nurse Labor in Acute Care

	(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 x Post	0.017***	0.029***	0.011	0.022***	0.012***	0.012***	0.105**
(1996-2006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)	(0.049)
Below 0.25 x Post	0.025***	0.040***	0.012	0.027***	0.015***	0.015***	0.117**
(1996-2010)	(0.007)	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.055)
Below 0.25 x Post	0.035***	0.050***	0.016**	0.033***	0.016***	0.015***	0.092
(1996-2016)	(0.007)	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.063)
Mean	0.285	0.241	0.235	0.197	0.029	0.025	11.848
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.

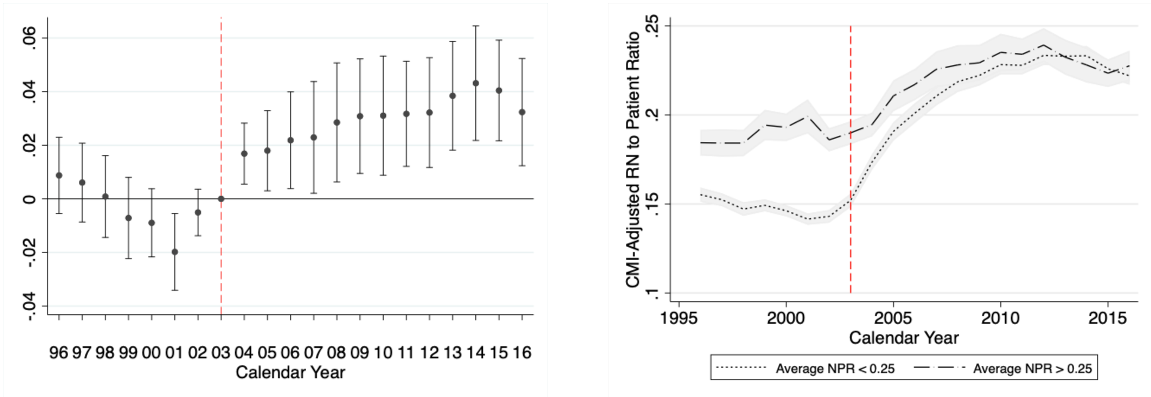
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 across all groups. 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 0.241 (17 percent). The increase is robust to the length of the sample period.



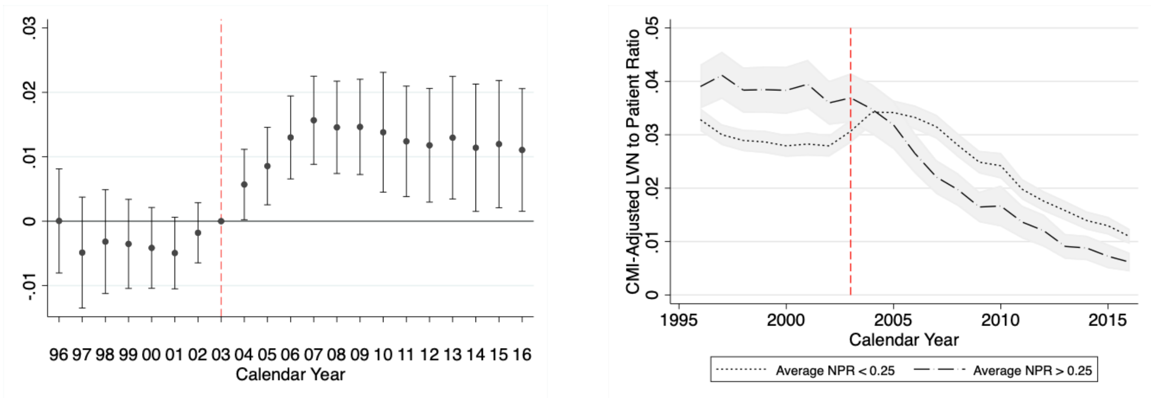
(a) Event-Study Estimates (b) Raw Means

Figure 3: Nurse-to-Patient Ratio Adj.



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

Figure 4: RN-to-Patient Ratio Adj.



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

Figure 5: LVN-to-Patient Ratio Adj.

Notes: In Figures 3-5 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the nurse-, RN-, or LVN-to-patient ratio as dependent variable. Standard errors are clustered at the hospital level. In Figures 3-5 Panel (b), this figure plots average values and standard error bands of the nurse-, RN-, or LVN-to-patient ratio by group.

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. [20] In Columns 1, 3, and 5, I present the unadjusted outcomes.

Column 2, Model 2 and 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 (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 4, Model 2 indicates that the productive staff-to-patient ratio increased by 0.036 points or 52 minutes per patient day. This is less than the 58 minute 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. Column 6 indicates that there was no significant effect on the expenditures on physicians per patient day but there was a decline in expenditures by 3.6 percent in the medium-term. These findings broadly suggest that nurse and non-nurse labor are not highly substitutable in healthcare production at the observed wages.

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 caveat that scope of practice regulations vary widely from state to state therefore the substitution patterns in response to a mandate of this sort in other states are also likely to vary.[21]

5.2 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 Figures 6a and 6b. 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.

[20] 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.

[21] 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).

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 x 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 x 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 x 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	0.101	0.085	0.418	0.354	2.267	2.130
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 across all groups. 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 control group mean of 0.354 (10 percent). The increase is robust to the length of the sample period. Standard errors are clustered at the hospital level.

LVN wages also declined but by a smaller magnitude (2.4 percent) and the decline is not statistically significant. Non-nurse wages appear to be unaffected.

I mention that 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 this wage effect. This is a distinct channel from a general equilibrium wage effect on California hospitals (both treated and control) driven by a shift in the labor demand curve. These channels will have competing effects on the average wage if nurses at treated hospitals become younger or more recently licensed. The general equilibrium wage effects of the mandate have been estimated by several papers (Mark et al., 2009; Munnich, 2014; Harless, 2019). In Appendix Section 9.1, 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 size of the labor demand shock.

5.2.1 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, a decline in experience is the explanation that requires fewest assumptions on labor market structure (it is consistent with both perfectly competitive and oligopsonistic labor markets) and is consistent with the aggregate evidence that I show from union contracts, survey data on RNs, and licensing data.

Figure 6b shows that RN wages at the two groups of hospitals were for the most part statistically indistinguishable prior to the mandate. First, in Appendix Section 9.2, I show that if the 5.1 percent wage decline (from the long-term model) were due to differences in worker composition between incumbent nurses (hired before the mandate) and new hire nurses (hired after the mandate) and not by differences in wages between the treatment and control groups for the same worker type, then the incumbent wage must have been 34 percent higher than the new hire wage.

Second, I show that within-hospital RN wage range of this magnitude is plausible using data from RN union contracts in the early 2000s. I find that the average range of within-hospital RN wages among the union contracts I analyze is 52 percent within position and education level (e.g. RN with the position of "charge nurse" and holds an Associate's degree) between the entry-level nurse and nurse with 20+ years of experience. The RN wages that I analyze from HCAI are not delineated by education or position, however, and I find even wider ranges for within-hospital variation in the union contracts when I do not control for education and position. In the contracts I analyzed, a 34 percent higher wage among incumbents corresponds to 13 additional years of experience.

Third, I analyze data from the National Sample Survey of Registered Nurses (NSSRN) to see how the characteristics of RNs employed at hospitals in California changed relative to other states following the mandate. In Table 5, I present the shares of hospital RNs by

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 x Post (1996-2006)	-0.017 (0.015)	-0.015 (0.018)	-0.011 (0.019)
Below 0.25 x Post (1996-2010)	-0.033** (0.016)	-0.024 (0.023)	-0.008 (0.019)
Below 0.25 x Post (1996-2016)	-0.051*** (0.018)	-0.014 (0.026)	-0.021 (0.022)
Mean	3.229	2.678	2.489
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 $\times$ 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 across all groups. 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 doesn't appear to be an effect on non-nurse wages (Column 3, Model 2).

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.

Finally, I link these changes in characteristics to average hourly wages from survey data rather than union contracts. In Table 6, I present the average hourly wage of California hospital RNs surveyed in either 2000, 2004, or 2008 by age-education bin. The range of RN wages within education bin is between 35 and 80 percent which is consistent with the ranges found in the union contracts that I analyzed from the same period. Taken together, Tables 5 and 6 provide suggestive evidence that a change in composition towards younger, more recently licensed nurses could be driving the wage decline.

Evidence from the licensing data is supportive of these patterns. In Figures 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. Furthermore, in Section 3.3, I discussed that the institutional context made it unlikely that the growth in hospital nurses was drawn from trained nurses that 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.

The quality implications of these changes in nurse composition are unclear. Lower wages do not necessarily imply lower marginal product of labor with respect to care quality, particularly in a setting with high unionization rates. I find that the unionization rate among RNs employed in California hospitals in the NSSRN sample in 2004 is 44 percent. Prior literature is inconclusive about the quality returns to experience in nursing. Bartel et al. (2014) find significant hospital unit-specific quality returns to tenure in nursing but find that a nurse's experience outside of the specific practice setting (in any health care delivery organization or in the same hospital but a different unit) has no relevance for quality. In the mandate setting where hospitals are likely hiring nurses new to the hospital and unit, nurses will not have that specific human capital regardless of age. Evidence from the nursing field is inconclusive about the relationship between years of experience and patient health outcomes with some studies finding a positive correlation (Dunton et al., 2007) and others finding no relationship (Aiken et al., 2003).

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. The quality implications may vary based on the composition of the labor supplied and vary over time due to dynamic treatment effects.

Table 5: Changes in the Composition of RNs Employed at Hospitals

	California		Other States	
Share of RNs	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 6: 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.

5.2.2 Alternate Mechanism - Compensating Wage Differentials

The evidence that I presented in the previous section is consistent with a change in composition driving the wage decline at treated hospitals. However, I consider an alternate mechanism based on the theory of compensating wage differentials. 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). We might therefore think that there are compensating differentials in the labor market whereby RNs employed at treated hospitals were offered higher wages prior to the mandate, controlling for marginal product of labor, to compensate for poorer working conditions. 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. [22]

In an oligopsonistic labor market, we would expect that if this mechanism is at play then we should see a higher wage effect for control hospitals that have more treated hospitals nearby. To evaluate this possibility, I estimate 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

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

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 with no treated hospitals within 5 or 10 miles.

In Table A.2, I present the results from the estimation of Specification (3). I find that the coefficient of interest, β_2, is estimated to be close to zero and statistically insignificant implying that the wage effects do not vary with the degree of labor market competition. This finding is not supportive of a compensating wage differential mechanism in which we would expect the effect to increase in number of competitors.

5.3 Non-Labor Inputs and Costs

In Section 5.1, I documented that hospitals increased nurse labor per patient day due to the mandate and showed that decline in any other labor inputs did not completely offset this increase. Considering nurse labor to be a variable rather than fixed input, we should observe an increase in marginal costs due to the mandate. [23]

[22] 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.

[23] 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.

In Table 7, I present the effects on hospital costs 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 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 is unsurprising if we consider that having more nurse labor per patient day may increase use of supplies such as medication administered to the patient or reapplication of bandages. However, the magnitude is relatively large.

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. This is consistent with expectations since there are only a few allocated cost components that are directly linked to the mandate: employee benefits and health insurance, nursing administration, inservice education for nurses, and licensed vocational nurse programs to train LVNs. The wage bill comprised on average 50 percent of average total costs in acute care prior to the mandate. Column 6, Model 2 indicates that average total costs 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 perhaps due to other factors such as the decline in patient days in the long-term.

5.4 Capacity and Output

In Figures 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.[24] Columns 3, 4, 5, and 6 shows 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 128.6 beds (12 percent decline). Column 2, Model 2 indicates a reduction in staffed beds by 13.2 beds on a mean of 101.9 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 76.0 patients per day (6 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 57 to 62 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 Models 1, 2, and 3 within each column, we find that the reduction in capacity was immediate and remained relatively stable over time. It appears unlikely that hospitals were reducing capacity because they were unable to hire their desired number of nursing hours. We would expect labor supply to become more elastic over time and if the observed capacity reductions were due to short-term labor supply inelasticity then they should be temporary.

Rather, 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).[25] My evidence is inconclusive as to whether hospitals actually had to turn patients away due to capacity constraints. The event-study estimates in Figure 9 indicate a slight decline in discharges after the mandate, however, Table 8 indicates that the decline in discharges in the medium-term, despite being large (11 percent), is not statistically significant.

The increase in the utilization rate declines and becomes statistically insignificant

[24]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.

[25]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.

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 x Post	0.143	0.139	0.069***	0.049	-0.012	0.025
(1996-2006)	(0.147)	(0.279)	(0.026)	(0.025)	(0.038)	(0.032)
Below 0.25 x Post	0.190	0.065	0.087**	0.078**	0.051	0.071
(1996-2010)	(0.150)	(0.297)	(0.034)	(0.034)	(0.040)	(0.037)
Below 0.25 x Post	0.256	0.255	0.095**	0.098**	0.080	0.089**
(1996-2016)	(0.153)	(0.292)	(0.039)	(0.039)	(0.044)	(0.041)
Mean	1.333	-1.632	5.414	5.547	5.241	6.137
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 across all groups. 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).

in the long-term due to the long-term decline in patient days. 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. On the other hand, Figure 10 suggests that part of the immediate decline in patient days is due to a decline in length of stay (also statistically insignificant). I show in the next section that the decline in length of stay found in these data is confirmed by and statistically significant in the administrative data on AMI discharges.

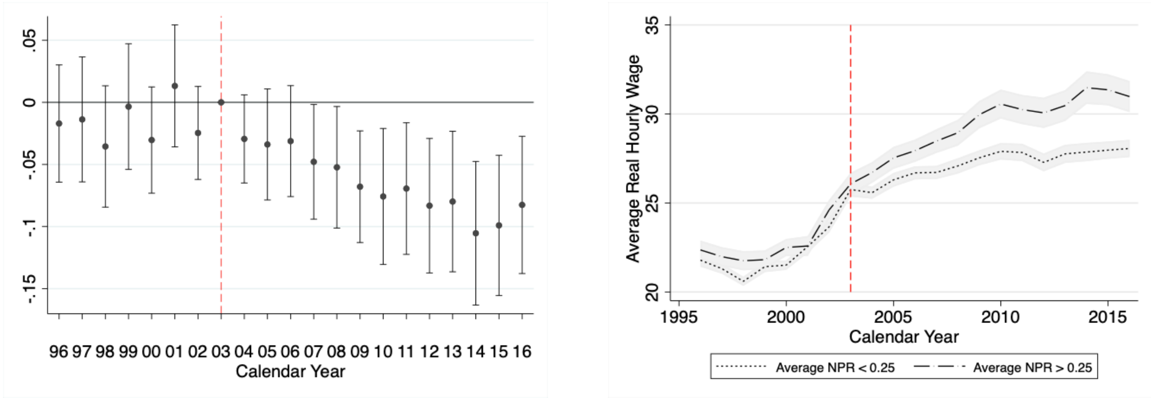
Table 8: Difference-in-Differences Estimates for Output in Acute Care

	(1) Available Beds	(2) Staffed Beds	(3) Patient Days	(4) Utilization Rate	(5) Discharges	(6) Length of Stay
Below 0.25 x Post	-12.266***	-11.192**	-1147.245	0.045***	-754.344	-0.323
(1996-2006)	(4.552)	(4.477)	(1048.506)	(0.017)	(512.026)	(0.377)
Below 0.25 x Post	-15.625***	-13.195***	-1769.781	0.045**	-910.698	-0.507
(1996-2010)	(5.132)	(4.973)	(1273.792)	(0.019)	(538.776)	(0.413)
Below 0.25 x Post	-16.527***	-15.455***	-3521.592**	0.010	-1209.133**	-0.784
(1996-2016)	(5.860)	(5.085)	(1558.710)	(0.021)	(577.594)	(0.428)
Mean	128.586	101.918	27729.455	0.574	8163.512	4.622
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

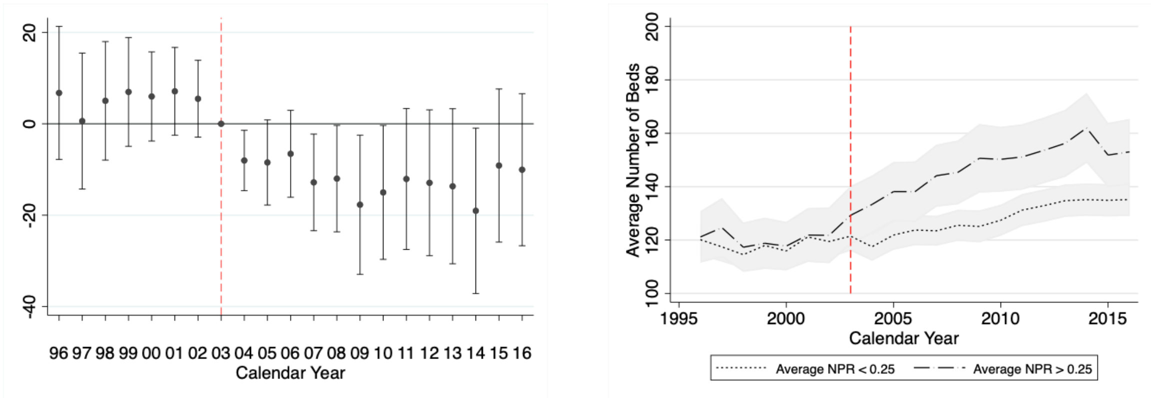
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows difference-in-differences estimates of the treatment effect (Below 0.25 x 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 across all groups. 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 (12 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).



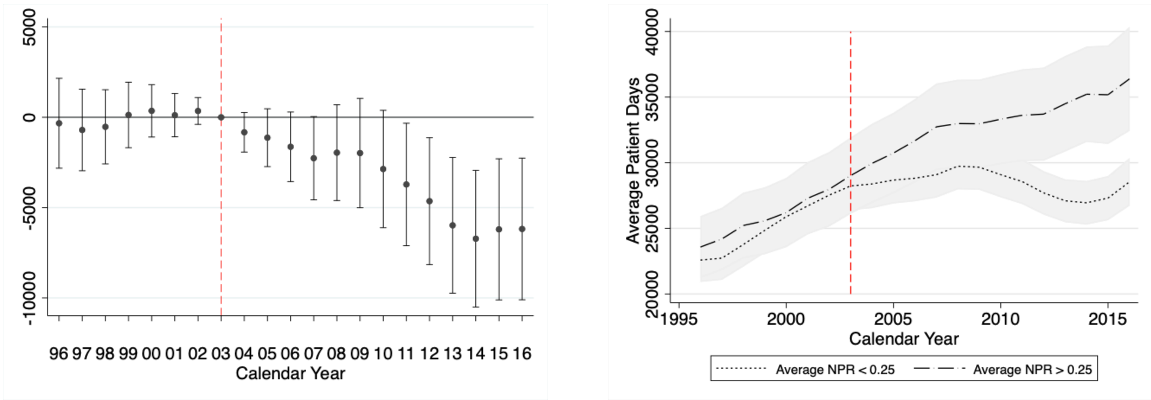
(a) Event-Study Estimates (b) Raw Means

Figure 6: RN Real Hourly Wage



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

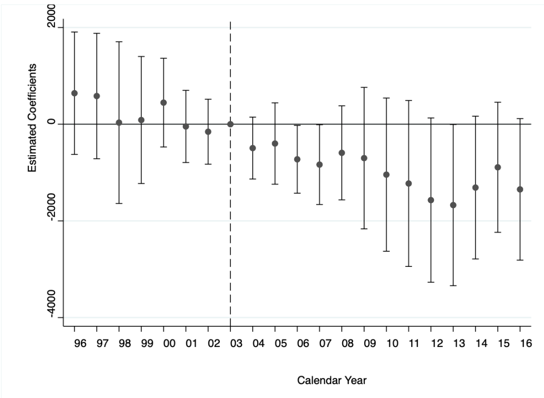
Figure 7: Acute Care Available Beds



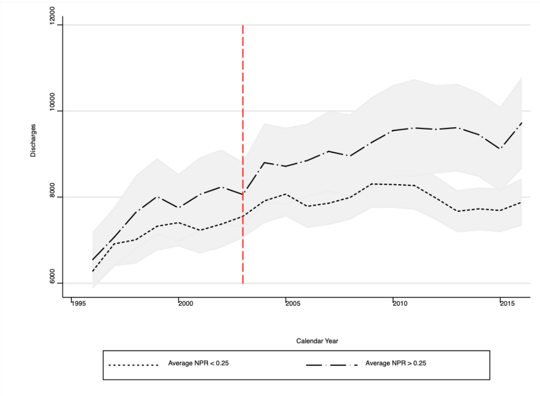
(a) Event-Study Estimates (b) Raw Means

Figure 8: Acute Care Patient Days

Notes: In Figures 6-8 Panel (a), this figure plots coefficients α_t and 95 percent confidence intervals from Specification (2) with the 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. In Panel (b), this figure plots average values and standard error bands of the RN real hourly wage in USD (not logged), acute care available beds, or acute care patient days.

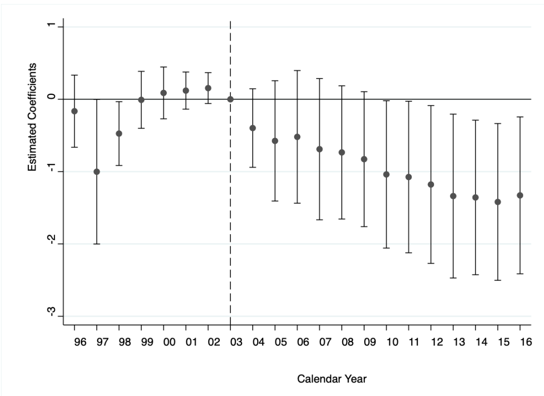


(a) Event-Study Estimates

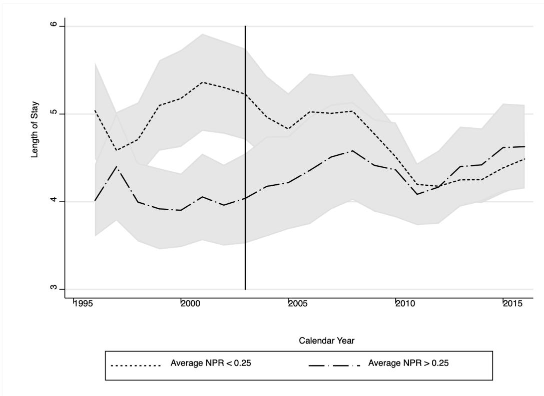


(b) Raw Means

Figure 9: Acute Care Discharges



(a) Event-Study Estimates



(b) Raw Means

Figure 10: Length of Stay

Notes: In panel (a) of Figures 9 and 10, I plot the coefficients α_t and 95 percent confidence intervals from Specification (1) with the acute care discharges and length of stay as dependent variables. In panel (b) of Figures 9 and 10, I plot the average values and standard error bands of the discharges and length of stay by group.

5.5 Healthcare Quality

In this section, I estimate the effects of the mandate on care quality focusing jointly on length of stay and 30-day readmission rates for acute myocardial infarction (AMI).

In the previous section, I showed descriptive evidence that average length of stay converged between the treatment and control groups following the mandate. Low length of stay is often used as an indicator for high quality of care (Bartel et al., 2014). However, medical literature has not reached a consensus on how length of stay affects or is reflective of healthcare quality (Kossovsky et al., 2002; Brasel et al., 2007; Spetz et al., 2013). At the same time, recent evidence in economics has shown that patients with lower length of stay when hospitals are overcrowded are readmitted to the hospital at higher rates, indicating low care quality in the first instance (Hoe, 2022).

This is a salient concern in my setting given my finding that the mandate leads to significant capacity reductions. Do hospitals discharge patients "quicker and sicker" after the mandate because they are more capacity constrained or do patients become healthier quicker due to the increase in nursing time per patient day? To address these concerns, I focus jointly on length of stay and 30-day readmission rates 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 construct and risk-adjust the length of stay and 30-day readmission measures from the administrative patient discharge data following CMS (2008) and (Chandra et al., 2016a). The difference-in-differences and event-study regressions for length of stay and readmission rates are weighted at the hospital-year level by the hospital's share of total AMI discharges in the given year. In other words, hospitals with more AMI discharges in a given year are assigned higher weights. The event-study estimates are presented in Figures 11 and 12 and the difference-in-difference estimates are presented in Table 9.

Table 9 shows a decline in length of stay of 0.281 points (5 percent) and a statistically insignificant decline in the 30-day readmission rate by 0.004 points (2 percent). Contrary to the expectations under a "quicker and sicker" hypothesis, I find no statistically significant effect on the 30-day AMI readmission rate despite the decline in length of stay. I conclude that there was an increase in care quality per day for AMI patients who appear to be becoming healthier quicker after the mandate.

The 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. I investigate whether the 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 an increase in one year of average tenure across all RNs on the current nursing unit leads to a 1.3 percent decline. However, returns to tenure are non-linear. The implied treatment effects of the mandate on length of stay based on Bartel et al. (2014)'s estimates are 3.1 percent (one year after mandate), 3.8 percent (two years), 4.2 percent (three years), 4.2 percent (four years), and 4.2 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. [26] My event-study estimates in the post-mandate period are qualitatively consistent with these approximations.

My findings complement prior literature on the quality effects of the mandate. Prior literature has used a 2004-2006 post-mandate period for estimation and focused on in-hospital metrics including inpatient failure to rescue (Cook et al., 2012; Mark et al., 2013; Spetz et al., 2013) and decubitus ulcer rates (Cook et al., 2012; Spetz et al., 2013). I focus on length of stay and 30-day readmissions for AMI, as opposed to in-hospital quality indicators, and find an increase in quality. My estimation over a slightly longer 2004-2008 post-mandate period provides suggestive evidence of dynamic quality effects and indicates that findings may depend on the period over which the effects are estimated. My findings qualitatively confirm prior work on minimum ratios in nursing homes (Lin, 2014) and the quality returns to nursing (Gruber and Kleiner, 2012; Friedrich and Hackmann, 2021).

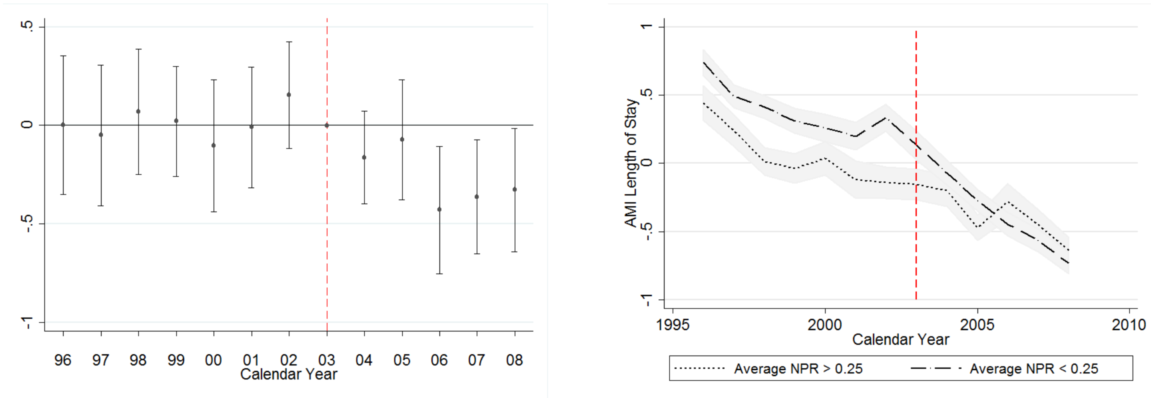
Table 9: Difference-in-Differences Estimates for Length of Stay and 30-Day Readmission Rate for AMI

	(1) Readmission Rate	(2) Risk-Adjusted Rate	(3) Length of Stay	(4) Risk-Adjusted Length of Stay
Below 0.25 x Post	-0.002	-0.004	-0.273***	-0.281***
	(0.006)	(0.005)	(0.102)	(0.098)
Observations	2,080	2,080	2,080	2,080
R^2	0.604	0.519	0.695	0.677
Mean	0.239	0.028	6.153	0.281
Hospital FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

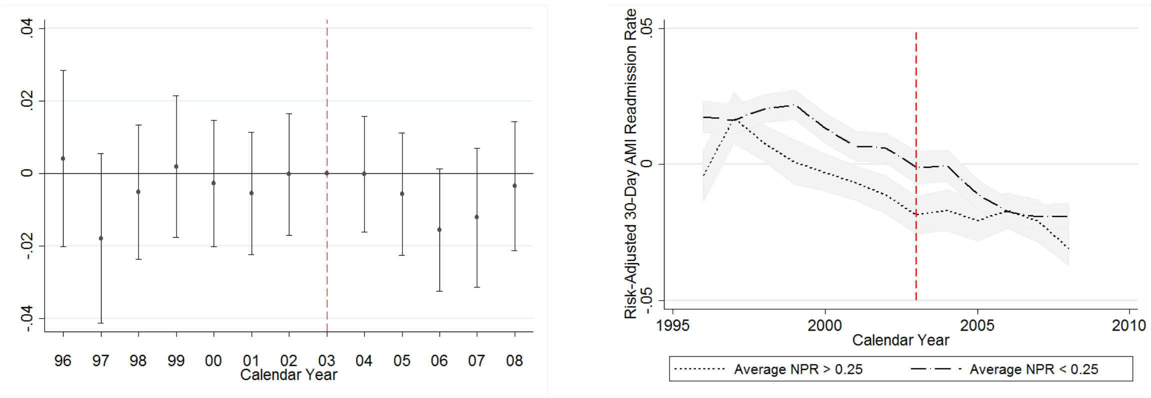
[26] I obtain these calculations as follows. My estimated treatment effects on RN and LVN time per patient day are 36 and 22 minutes, respectively. The implied decline in length of stay for the combination of these increases in nursing time is 3.1 percent. The returns to tenure are approximated based 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.



(a) Event-Study Estimates

(b) Raw Means

Figure 11: Risk-Adjusted AMI Length of Stay



(a) Event-Study Estimates

(b) Raw Means

Figure 12: Risk-Adjusted 30-Day AMI Readmission Rate

Notes: Notes: In panel (a) this figure plots the coefficients α_t and 95 percent confidence intervals from Specification (1) with the risk-adjusted readmission rate and risk-adjusted length of stay as dependent variables. In panel (b) this figure plots the average values and standard error bands of the risk-adjusted readmission rate and 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.

6 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

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

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

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

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 Figure 13.

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 patient severity differences across hospitals not fully captured by the Case Mix Index. Hospitals whose patient mix becomes more severe over this period may be less able to apportion licensed nurses to perform the tasks of aides.

The results in Table 12 also 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. 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 Figure 13 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 Figure 13 shows that for the most part there aren't 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.

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***	0.008	0.096***	0.048***	-0.019***	-0.021***	0.248***
	(0.007)	(0.009)	(0.008)	(0.008)	(0.004)	(0.003)	(0.054)
Between 0.22 and 0.25 x Post	0.017**	0.033***	0.011	0.025***	0.011**	0.012***	0.065
	(0.008)	(0.009)	(0.009)	(0.009)	(0.005)	(0.004)	(0.067)
Between 0.19 and 0.22 x Post	0.026***	0.037***	0.011	0.022**	0.015***	0.015***	0.092
	(0.008)	(0.009)	(0.009)	(0.009)	(0.005)	(0.004)	(0.057)
Below 0.19 x Post	0.033***	0.051***	0.016	0.034***	0.020***	0.019***	0.193***
	(0.009)	(0.010)	(0.010)	(0.011)	(0.005)	(0.004)	(0.069)
Mean	0.264	0.235	0.208	0.184	0.033	0.030	11.756
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 across all groups. 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	-0.005	0.091***	0.022	-1.444***	-1.613***
	(0.006)	(0.005)	(0.011)	(0.012)	(0.175)	(0.173)
Between 0.22 and 0.25 x Post	-0.016**	-0.010	0.005	0.025**	-0.141	-0.075
	(0.007)	(0.006)	(0.011)	(0.013)	(0.184)	(0.182)
Between 0.19 and 0.22 x Post	-0.013	-0.008	0.013	0.029**	0.024	0.067
	(0.007)	(0.006)	(0.011)	(0.013)	(0.207)	(0.206)
Below 0.19 x Post	-0.011	-0.005	0.030**	0.053***	0.163	0.237
	(0.007)	(0.006)	(0.013)	(0.014)	(0.185)	(0.183)
Mean	0.096	0.085	0.389	0.346	2.342	2.205
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 across all groups. 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 0.026 points for hospitals between 0.22 and 0.25 initial ratio, by 0.043 points for hospitals between 0.19 and 0.22 initial ratio, and by 0.062 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***	0.016	0.088***
	(0.018)	(0.029)	(0.019)
Between 0.22 and 0.25 x Post	-0.014	0.002	0.014
	(0.020)	(0.027)	(0.025)
Between 0.19 and 0.22 x Post	-0.034	-0.039	-0.022
	(0.018)	(0.027)	(0.025)
Below 0.19 x Post	-0.051**	-0.034	-0.015
	(0.021)	(0.032)	(0.023)
Mean	3.188	2.669	2.457
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 across all groups. 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 5.2 percent for hospitals between 0.19 and 0.22 initial ratio and by 7.3 percent 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 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	0.104	0.281***	0.173***	0.163***	0.167***
	(0.116)	(0.288)	(0.036)	(0.035)	(0.042)	(0.038)
Between 0.22 and 0.25 x Post	0.291	-0.212	0.088**	0.074**	0.065	0.076
	(0.191)	(0.359)	(0.038)	(0.037)	(0.048)	(0.041)
Between 0.19 and 0.22 x Post	0.194	0.274	0.065	0.071	0.051	0.083**
	(0.182)	(0.406)	(0.036)	(0.037)	(0.046)	(0.041)
Below 0.19 x Post	0.089	0.126	0.107***	0.090**	0.039	0.054
	(0.211)	(0.374)	(0.041)	(0.040)	(0.047)	(0.042)
Mean	1.052	-1.783	5.325	5.474	5.159	6.059
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 across all groups. 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 3 and 4 indicate that the treatment effects on salary and direct costs per patient day scale as we would expect, however, average total costs in Column (6) indicate that the decline in patient days rather than the increase in salary expenditures may be driving average total costs up. 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***	4.096	3754.413***	-0.042**	1636.050***	-0.096
	(5.116)	(5.076)	(1236.756)	(0.018)	(541.546)	(0.429)
Between 0.22 and 0.25 x Post	-16.119**	-15.314**	-2045.264	0.045	-561.667	-0.961
	(6.264)	(6.357)	(1536.527)	(0.024)	(721.656)	(0.738)
Between 0.19 and 0.22 x Post	-15.578**	-10.688	-1988.956	0.039	-1473.177**	-0.460
	(6.096)	(5.866)	(1398.827)	(0.022)	(670.650)	(0.451)
Below 0.19 x Post	-15.191**	-13.683**	-1277.384	0.050	-677.706	-0.622
	(6.001)	(5.779)	(1541.469)	(0.029)	(542.337)	(0.495)
Mean	123.978	107.017	27003.562	0.584	8016.275	4.965
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 across all groups. 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 coefficients in Columns 3 and 4 indicate that hospitals may have taken different approaches to meeting mandated ratios (reducing patient days or hiring nurse hours) with hospitals nearer to the threshold (betwen 0.19 and 0.22 initial ratio) choosing to reduce patient days and hospitals further from the threshold (below 0.19 initial ratio) choosing to increase staffing. Hospitals below 0.25 are considered treated in the main specification with binary treatment.

7 Robustness Checks

I conduct two robustness specifications 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.

7.1 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).

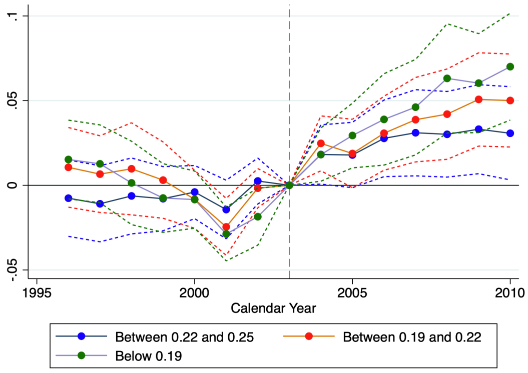
In the main specification, the nurse-to-patient ratio is adjusted for patient severity. I showed in my comparison between Figure 3 and Appendix Figure A.5 that accounting for patient severity is important in controlling for differential pre-trends in nurse staffing. Absent these controls for 1990-1995, 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 Figures A.10, A.11, A.12, and A.13 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). Figure A.10b indicates the existence of differential pre-trends in the unadjusted staffing ratio. However, Figure A.10a indicates that the pre-trends are well-approximated by and controlled for using linear time trends. Figures A.11, A.12, and A.13 do not show differential pre-trends for any of the other outcomes.

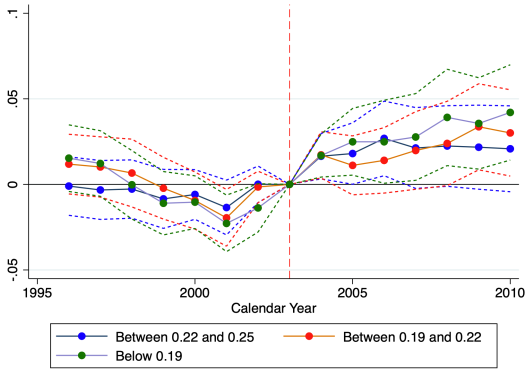
Figures A.10, A.11, A.12, and A.13 confirm the findings from my main specification. The mandate led to an increase in the nurse-to-patient ratio and declines in the RN hourly wage, available beds in acute care, and patient days in acute care among treated hospitals.

7.2 Placebo Test Using Intensive Care Unit

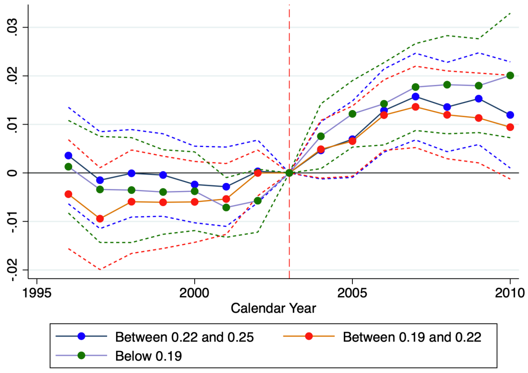
In this section, I utilize the intensive care unit as a placebo test of my findings by estimating Specification (1) on the outcomes in intensive care units of the hospitals in my sample. Given that the intensive care unit was already subject to minimum ratios prior to the mandate, I expect that to find null effects on my main outcomes in the intensive care unit



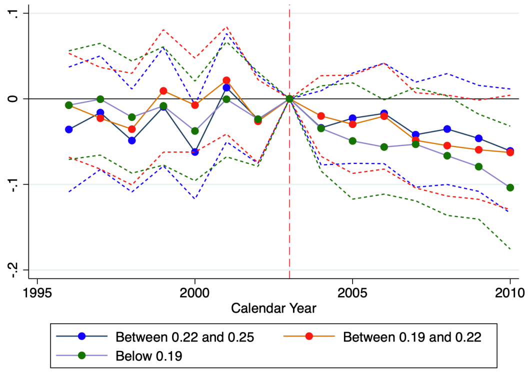
(a) Nurse-to-Patient Ratio Adj.



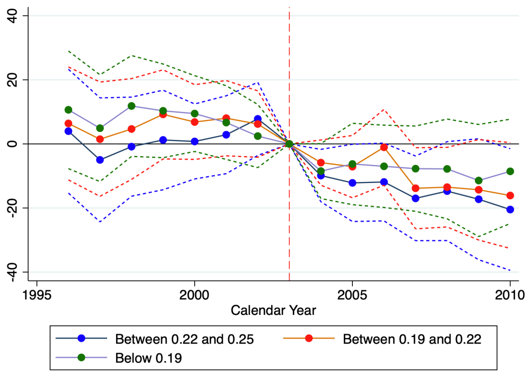
(b) RN-to-Patient Ratio Adj.



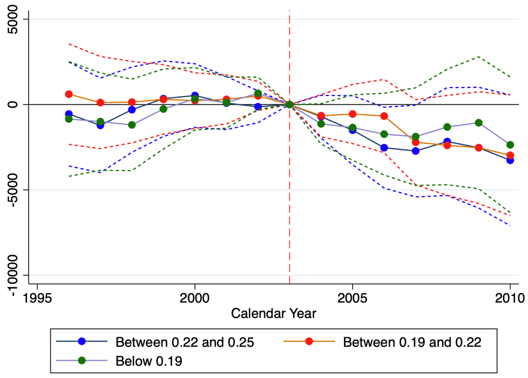
(c) LVN-to-Patient Ratio Adj.



(d) RN Real Hourly Wage



(e) Acute Care Beds



(f) Acute Care Patient Days

Figure 13: 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.

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 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.[27] 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.

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

[27]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.

8 Conclusion

In this paper, I utilize 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 general acute care hospitals to meet minimum nurse-to-patient ratios established at the hospital unit level (e.g. 0.2 in acute care) 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 several other margins: increased their use of lower-licensed and younger nurses, reduced capacity by 16 beds (12 percent), and increased bed utilization rates by 0.045 points (8 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. Low length of stay is often used as an indicator for high quality of care (Bartel et al., 2014). However, discharging patients "quicker and sicker" may be one way hospitals respond to capacity constraints (Friedman and Pauly, 1981; Hoe, 2022). In light of the substantial capacity reduction that I document, I investigate whether the decline in length of stay is indicative of early discharge or higher care quality. Contrary to the expectations under a "quicker and sicker" hypothesis, I find no effect on the 30-day AMI readmission rate despite the decline in length of stay. I conclude there was an increase in care quality per day for AMI patients.

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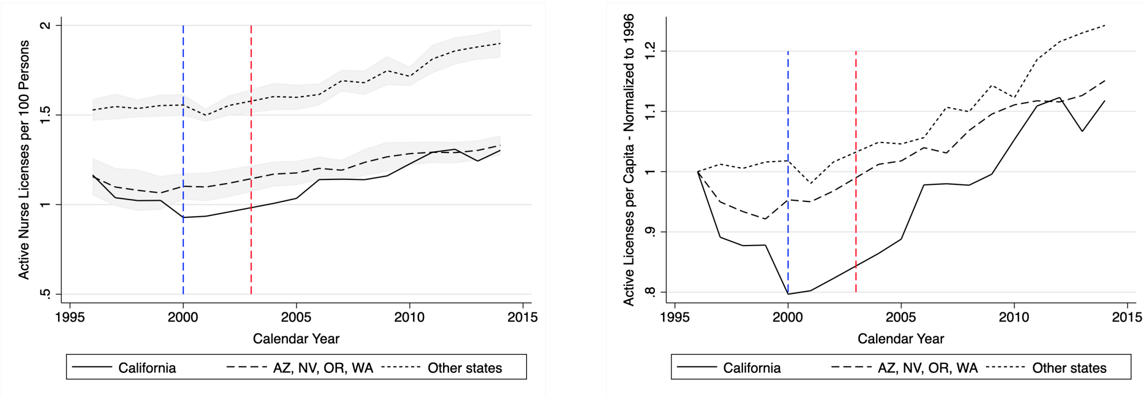
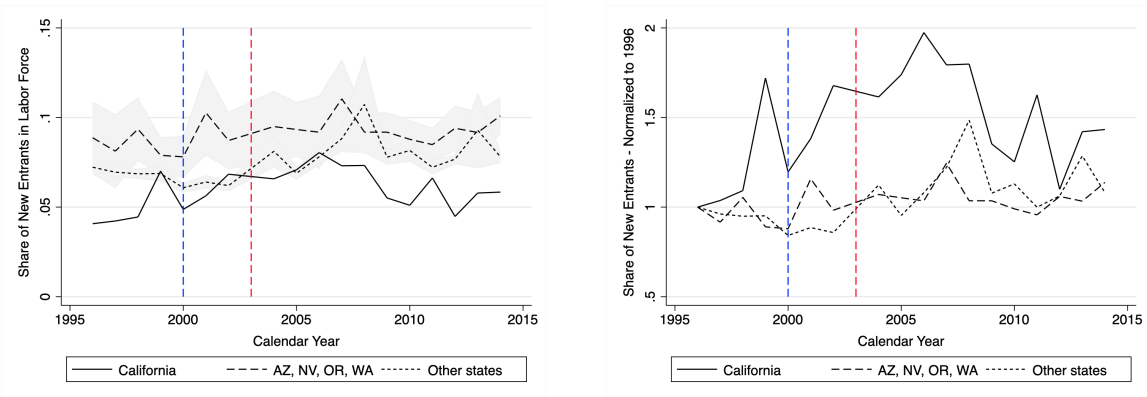
9 Appendix



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

Figure A.1: Growth in the Nursing Labor Force in California vs. Other States

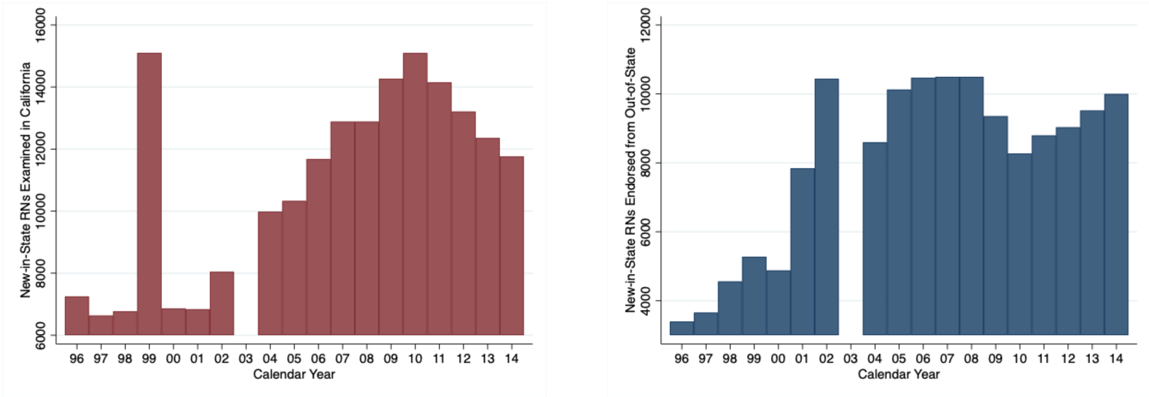
Notes: In Panel (a), I plot the average number of active LVN and RN licenses per 100 persons by group. Standard error bands for the averages across states are shown in gray for neighbors and other states. In Panel (b), I plot the same measure for each group normalized to the average number of nurse licenses 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.



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

Figure 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.



(a) New-in-State RNs Examined in California (b) New-in-State RNs Endorsed from Out-of-State

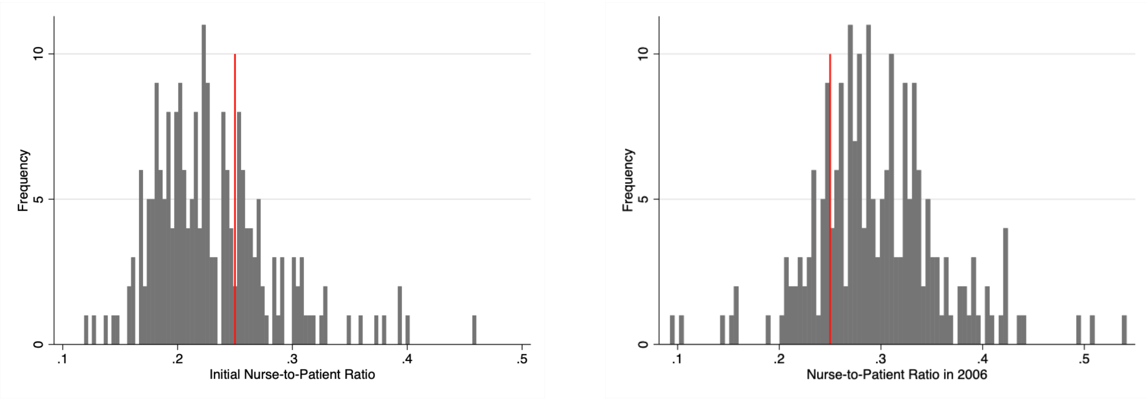
Figure A.3: New-in-State RN Licenses, 1996-2014

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.

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
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).

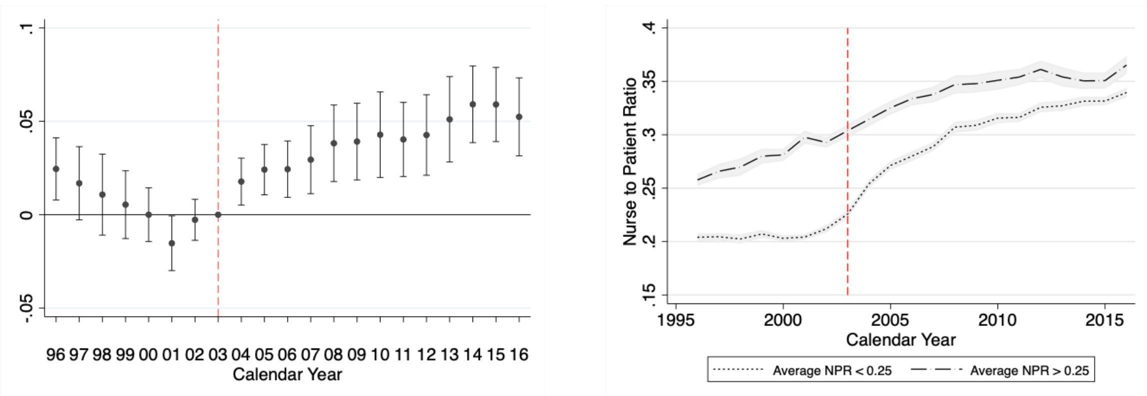


(a) Initial Nurse-to-Patient Ratio in 2000-2002

(b) Nurse-to-Patient Ratio in 2006

Figure 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.



(a) Event-Study Estimates

(b) Raw Means

Figure A.5: Nurse-to-Patient Ratio, Unadjusted

Notes: Panel (a) plots coefficients and Panel (b) plots averages for the unadjusted nurse to patient ratio. Comparison between this figure and Figure 3 shows that the patient severity adjustment of the outcome variable in Figure 3 controls for differential staffing trends in the pre-mandate period.

9.1 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.

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:

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

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.

In Figure A.6a, I present the event-study estimates of α_t from a regression of the log RN real annual salary in Specification (6). In Figure A.6b, 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 Figure 6 of my main analysis. Figure A.6 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.

In Figure A.7, 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). Figure A.7a 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 5.2, 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 $e \in E$

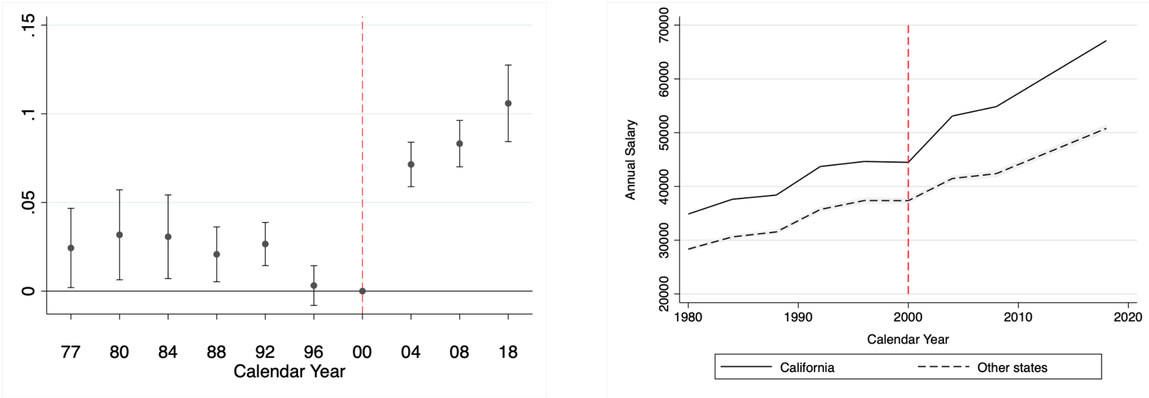
$$y_{st} = \alpha_0 + \sum_{t \neq 2000} \alpha_t \{YEAR_t = t\} * CALIFORNIA_s + \sum_{a \in A} \beta_a A_{st} + \sum_{e \in E} \beta_e E_{st} + \gamma_s + \xi_t + \epsilon_{st} \tag{7}$$

The results from the estimation of Specification (3) are presented in Figure A.8. Comparing Figures A.7a and A.8, the inclusion of age and education controls in estimation does not change the results very much.

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).[28] 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.

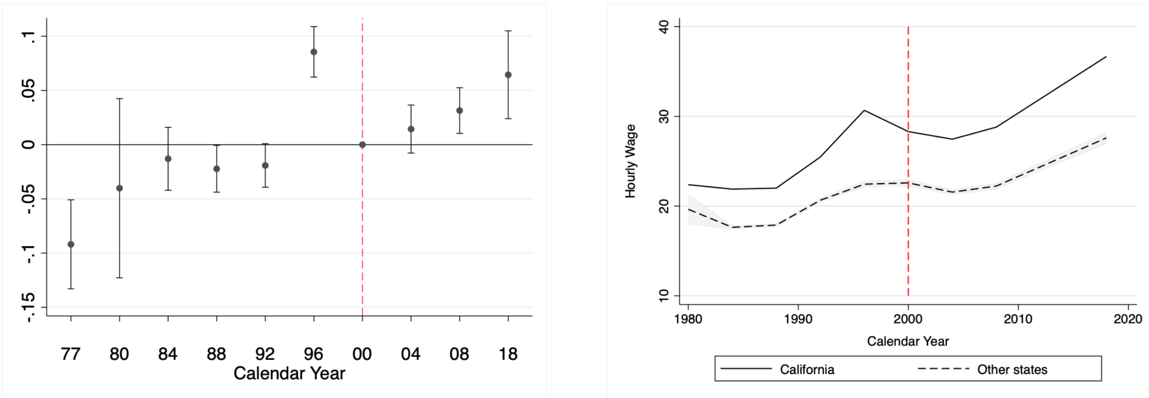
[28] I take the estimated effect of the mandate on RN hours (distinct from the RN-to-patient ratio) as a measure of the increase in RN labor demand by treated hospitals. First, I estimate an equivalent of Appendix Table 12, Column 7 for RN hours instead of nursing hours. I find that the effect of the 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 2,080 hours 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

Figure A.6: Hospital RN Real Annual Salary



(a) Event-Study Estimates

(b) Raw Means

Figure A.7: Hospital RN Real Hourly Wage

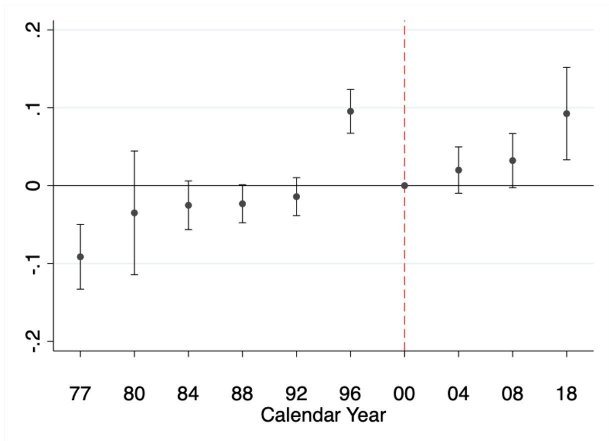


Figure A.8: Hospital RN Real Hourly Wage with Age and Education Controls

Notes: In Figures A.6 and A.7 panel (a) this figure plots the coefficients α_t and 95 percent confidence intervals from Specification (2) with the log annual salary and log hourly wage as dependent variables. In Figures A.6 and A.7 panel (b) this figure plots the average values and standard error bands of the real annual salary and real hourly wage in 1996 USD by group. In Figure A.8, I plot the coefficients α_t and 95 percent confidence intervals from Specification (3). Taken together, I find small, if any, general equilibrium wage effects consistent with the magnitude of the shock to aggregate RN labor demand.

9.2 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.

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 Figure 6b 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:

$$\begin{aligned} avewage_g^{post} * (incumbenthrs_g^{post} + newhirehrs_g^{post}) = \\ incumbenthrs_g^{post} * incumbentwage^{post} + newhirehrs_g^{post} * newhirewage^{post} \end{aligned}$$

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 (0.033) and multiply it by 24 hours per patient day to obtain the additional number of new hire hours per patient day at the treated hospitals (0.792) 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 34 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 Figure A.9, 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.[29] Given the average length of service differential between the most experienced nurse and entry-level nurse in these contracts (20 years), a 34 percent wage differential corresponds to 13 additional years of experience.

The range of wage variation observed in these contracts indicates that attributing a 34 percent wage differential to skill differences is more than plausible.

[29]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.

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	28.02	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

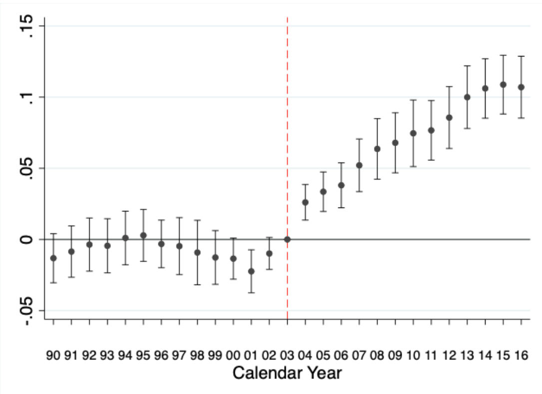
Figure A.9: Sample of RN Wage Scale in Minnesota Union Contract, 2004

Source: Collective Bargaining Agreements Digital Collections@ILR Cornell
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.

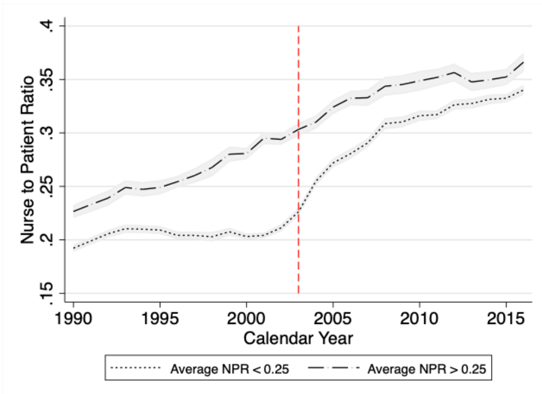
Table A.2: Difference-in-Differences Estimates for RN Wages by Number of Nearby Hospitals

	(1) ln(RN real hrly wage)	(2) ln(RN real hrly wage)
Above 0.25 x Post	0.048**	0.069***
	(0.021)	(0.021)
Above 0.25 x Post x Number Treated Hospitals Within 5 Mi	0.004	
	(0.016)	
Above 0.25 x Post x 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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



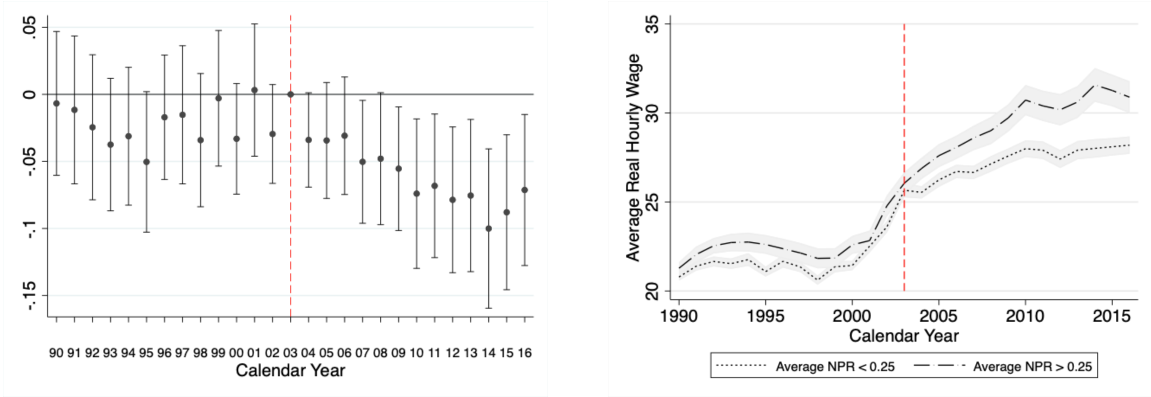
(a) Event-Study Estimates



(b) Raw Means

Figure A.10: Nurse-to-Patient Ratio, Unadjusted, Longer Pre-Mandate Trends

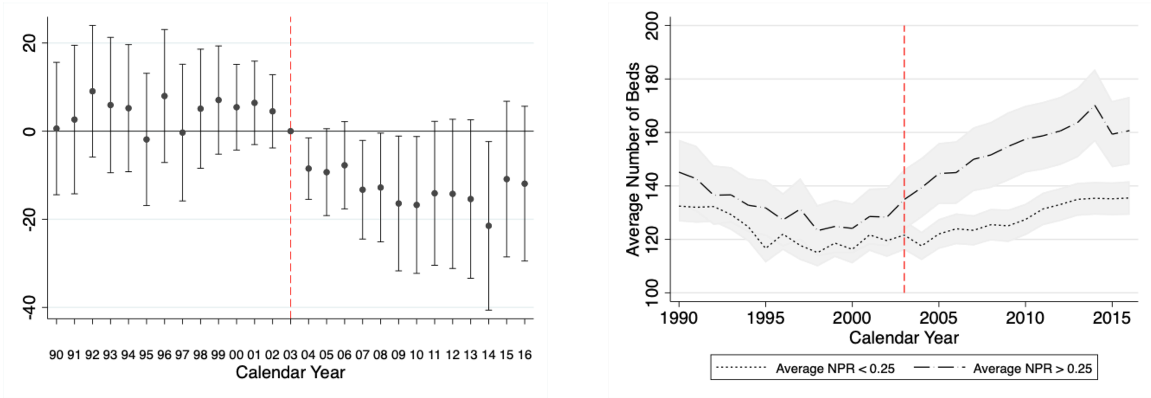
Notes: Panel (a) plots coefficients and Panel (b) plots averages for the nurse to patient ratio as in Figure 3 with two modifications. First, it utilizes a longer sample period (1990-2016) and a balanced panel of 203 hospitals. Second, the outcome is not adjusted for patient severity and as a result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



(a) Event-Study Estimates

(b) Raw Means

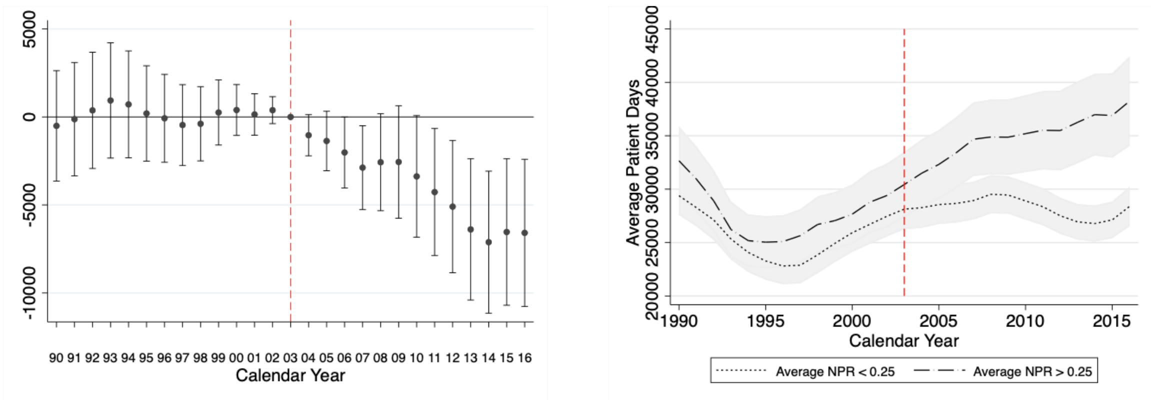
Figure A.11: RN Real Hourly Wage, Longer Pre-Mandate Trends



(a) Event-Study Estimates

(b) Raw Means

Figure A.12: Acute Care Available Beds, Longer Pre-Mandate Trends



(a) Event-Study Estimates

(b) Raw Means

Figure A.13: Acute Care Patient Days, Longer Pre-Mandate Trends

Notes: Figures A.11-A.13 Panel (a) plot coefficients and Panel (b) plot averages for the log RN real hourly wage (raw means are not logged), acute care available beds, and acute care patient days as in Figures 6-8 except they utilize a longer sample period (1990-2016) and a balanced panel of 203 hospitals.

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 x Post	-13.774**	-11.475*	-13.298**	-11.956**	-878.364	-819.885
	(6.115)	(6.315)	(5.330)	(5.482)	(621.146)	(658.569)
Below 0.25 x Post x Num. Hospitals Within 5 Mi	-2.009**		-1.669*		-249.881	
	(1.013)		(0.924)		(231.937)	
Below 0.25 x Post x Num. Hospitals Within 10 Mi		-1.011**		-0.733*		-82.776
		(0.395)		(0.372)		(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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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	0.009	0.037	0.007	0.031	0.005	0.006	-0.002
(1996-2006)	(0.020)	(0.022)	(0.020)	(0.021)	(0.003)	(0.003)	(0.037)
Below 0.25 × Post	0.007	0.038	0.001	0.029	0.004	0.004	-0.022
(1996-2010)	(0.021)	(0.025)	(0.021)	(0.023)	(0.003)	(0.003)	(0.044)
Below 0.25 × Post	0.014	0.046	0.009	0.038	0.003	0.003	-0.028
(1996-2016)	(0.021)	(0.024)	(0.020)	(0.023)	(0.004)	(0.004)	(0.051)
Mean	0.727	0.625	0.653	0.560	0.010	0.009	11.094
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 across all groups. 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	0.004	0.007	0.026	0.053**	0.214	0.248
(1996-2006)	(0.005)	(0.004)	(0.026)	(0.026)	(0.166)	(0.168)
Below 0.25 × Post	0.003	0.006	0.020	0.053	0.222	0.254
(1996-2010)	(0.006)	(0.005)	(0.029)	(0.030)	(0.174)	(0.176)
Below 0.25 × Post	0.003	0.006	0.027	0.059**	0.258	0.289
(1996-2016)	(0.006)	(0.005)	(0.027)	(0.029)	(0.191)	(0.192)
Mean	0.032	0.026	0.799	0.686	3.581	3.415
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 across all groups. 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.

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	-0.015	0.059	-0.037
(1996-2006)	(0.019)	(0.052)	(0.036)
Below 0.25 × Post	-0.028	-0.033	-0.032
(1996-2010)	(0.019)	(0.046)	(0.036)
Below 0.25 × Post	-0.044**	-0.064	-0.026
(1996-2016)	(0.020)	(0.045)	(0.038)
Mean	3.307	2.598	2.793
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 across all groups. 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	0.149	0.778***	0.035	0.035	-0.012	0.011
(1996-2006)	(0.169)	(0.285)	(0.035)	(0.031)	(0.054)	(0.040)
Below 0.25 × Post	0.201	0.735**	0.028	0.039	0.022	0.027
(1996-2010)	(0.163)	(0.290)	(0.039)	(0.038)	(0.050)	(0.042)
Below 0.25 × Post	0.219	0.480	0.017	0.032	0.041	0.038
(1996-2016)	(0.158)	(0.285)	(0.041)	(0.041)	(0.055)	(0.044)
Mean	2.434	-0.437	6.341	6.515	5.865	6.974
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 across all groups. 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.

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	-0.138	-44.704	-228.616	-0.038
	(0.928)	(0.896)	(232.748)	(222.836)	(0.863)
Below 0.25 × Post (1996-2010)	-0.720	0.690	-199.434	-329.095	-0.020
	(1.160)	(1.032)	(285.859)	(307.523)	(0.828)
Below 0.25 × Post (1996-2016)	-0.947	1.113	-326.349	-211.165	0.017
	(1.351)	(1.351)	(299.063)	(223.453)	(0.773)
Mean	20.916	9.703	4930.575	1141.681	7.461
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 across all groups. 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.