Avoiding Regulation: Evidence from Nursing Homes

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Abstract

We study a minimum staffing requirement for California nursing homes with at least 100 beds. We document extensive bunching in response to the regulation: facilities in California are 158 times as likely to be just below the regulatory threshold compared to just above. Using a novel method of estimating the effect of crossing a treatment threshold in the presence of manipulation of treatment status, we demonstrate that the regulation has little effect on staffing for those facilities subject to it. Moreover, size distortions induced by the regulation limit access, particularly for disadvantaged patients, illustrating an unintended consequence of threshold-based regulations.

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1 Introduction

Threshold-based regulations are widely used across policy domains, yet their effectiveness depends critically on the extent to which entities can manipulate whether they are subject to the regulation. When treatment status is tied to whether an assignment variable crosses a discrete threshold—as it is for labor regulations (Schivardi and Torrini, 2008; Garicano et al., 2016), tax and benefit rules (Onji, 2009; Dillender et al., 2022), financial regulations (Iliev, 2010; Ewens et al., 2024), antitrust scrutiny (Wollmann, 2019) and many other settings—entities often have strong incentives to bunch just below the threshold. This avoidance behavior can undermine the effectiveness of the regulation, particularly because the entities that select into avoidance are often those that are furthest from compliance. Furthermore, this avoidance behavior may itself have important consequences. The welfare effects of a threshold-based regulation thus depend on two key objects: the effects of avoidance behavior by bunchers, as well as the effects of compliance behavior among entities that do not manipulate their assignment variable.

In this paper, we develop a simple framework to estimate the effects of a threshold-based staffing regulation in nursing homes, a sector with annual spending exceeding $200 billion (Martin et al., 2025). We focus on a California regulation requiring that nursing homes with 100 or more beds maintain a registered nurse on-site 24 hours per day. Higher nursing home staffing is often linked to better patient health outcomes, but hiring more staff is costly to facilities. Because the staffing requirement we study only applies to facilities with 100 or more beds, facilities for which meeting the staffing requirement would be very costly may choose to avoid the requirement by adjusting their scale and capping their bed count below 100.

Estimating the total effect of this threshold-based policy requires identifying the effects of both avoidance and compliance behaviors. First examining avoidance behavior, we document extensive bunching in facility size to avoid the regulation: facilities in California are 158 times as likely to have 99 beds than 100 beds, with no such pattern in other states without this regulation. We also estimate that the probability of bunching is decreasing in the costs of manipulation, as bunchers tend to have the lowest staffing, and their preferred size absent the regulation tends to be smaller than other facilities that choose to comply with the regulation. On average, facilities bunching below the 100-bed threshold would have had 113.8 beds in the absence of the regulation. This shrinkage results in reduced patient access, particularly in ZIP codes with high poverty and Hispanic populations, suggesting that the welfare effects of avoidance behavior are negative.

Identifying the second object of interest—the effect of compliance with the staffing regulation—is more difficult given that the ability for facilities to precisely manipulate their assignment variable invalidates standard research designs such as regression discontinuity (McCrary, 2008). For instance, staffing levels of facilities above the threshold may be high due to the treatment effect of the regulation on staffing or because only facilities that would already have had high staffing without the regulation choose to remain above the threshold. In order to identify the effect of the regulation on staffing levels, we develop a novel, generalizable method for estimating causal effects in the presence of selection in avoidance behavior. Using a simple model of facility utility over size and staffing that formalizes the tradeoff between complying with staffing mandates and operating at one's preferred scale, we partition facilities into four distinct response types: never-regulated, self-regulated, bunchers, and rule-takers. We show that under reasonable assumptions about continuity and smoothness of potential outcomes, we can characterize these response types, both in terms of baseline characteristics as well as counterfactual and realized values of staffing and size. A key insight for estimating the effect on staffing is that while staffing outcomes above the threshold reflect both selection and treatment, the degree to which bunching facilities differ from those below the manipulation region is completely attributable to selection, allowing us to determine how selected the facilities that remain above the threshold are. This allows us to compare staffing at facilities bunching below the threshold to those with slightly fewer beds, identifying the degree to which bunching facilities are selected on observable characteristics and potential outcomes, including staffing. With the degree of selection in hand, the remaining difference in outcomes for facilities just above and below the regulatory threshold can be attributed to the treatment effect of the regulation. In short, we recover counterfactual outcomes for the facilities subject to the regulation net of selection using observed bunchers as a selection-revealing group.

We find that facilities bunching below the threshold have significantly lower staffing per bed than facilities just below the threshold that do not manipulate their size, employing 9.4% fewer RN hours per bed-day. Yet, among the remaining facilities that do comply with the regulation, we detect little change in actual staffing, with nearly the entire observed difference in staffing being attributable to selection. This lack of positive effect on welfare from compliance combined with the negative effect on welfare from the extensive bunching (either due to the private costs incurred by bunching facilities or the social costs of decreased access) indicates that the policy we study reduces social welfare.

In sum, this paper illustrates the limits of regulatory thresholds in contexts where treatment status is manipulable. When regulations impose substantial costs and when avoidance is feasible, entities that would fail to satisfy the regulatory requirements may avoid the regulations altogether. This selective compliance blunts the intended effects of the policy—with only the least affected entities complying with the regulation—and can lead to reductions in access or aggregate service provision. In our context, these welfare costs from avoidance outweigh the negligible effects of the regulation on compliance, demonstrating the empirical relevance of this tradeoff.

This paper contributes to two main strands of research. First, we contribute to the literature on the effectiveness of nursing home staffing regulations. Our paper differs from earlier work in the type of regulation analyzed and consequently, in results. Prior research has focused on altering the financial incentives of nursing homes, either through direct payment or competition (Hackmann, 2019; Gandhi et al., 2024), or through staffing mandates that apply to all facilities (Park and Stearns, 2009; Matsudaira, 2014; Chen and Grabowski, 2015). By contrast, we focus on a threshold-based regulation that may be easier to subvert. Indeed, while prior work finds that staffing regulations tend to have some positive effects on staffing, our work shows that facilities' strategic responses to a threshold-based regulation can fully undermine the intent of the policy while heightening disparities in access.

Methodologically, we contribute to the literature bridging regression discontinuity and bunching designs. Bunching at a discontinuous threshold generally undermines the ability to estimate the effect of crossing the threshold (McCrary, 2008; Imbens and Lemieux, 2008), with such avoidance behavior often being the phenomenon of interest (Saez, 2010; Chetty et al., 2011; Kleven and Waseem, 2013; Benzarti, 2020). By contrast, we estimate the effect of crossing the regulatory threshold even in the presence of bunching by identifying the degree of selection into bunching (Caetano, 2015; Diamond and Persson, 2016; Caetano et al., 2025). While existing research has shown that bounds on the treatment effect of the policy can be obtained under milder assumptions (Gerard et al., 2020; Goff, 2024), we use a simple model and somewhat stronger assumptions to generate transparent reduced-form tools for point identification of causal effects in the presence of bunching.

2 Setting and Data

2.1 Nursing Home Staffing

Nursing homes provide care for roughly 1.2 million Americans (Chidambaram et al., 2025), and more than half of individuals aged 57–61 today are expected to spend some time in a nursing home during their lives (Hurd et al., 2017). Given the vulnerable population served—many of whom are elderly and suffer from physical and cognitive impairments—policymakers have long been concerned with nursing home quality, of which staffing by registered nurses (RNs) is an important component. In particular, RNs are trained to oversee clinical care and respond to emergent health issues, and have been shown to improve quality of care (Lin, 2014), and even extreme patient outcomes such as survival (Friedrich and Hackmann, 2021). At the federal level, regulations require that all Medicare-certified nursing homes ensure that an RN is on duty for at least 8 hours per day,[1] with facilities with more than 120 beds being required to employ a full-time qualified social

[1]See 42 C.F.R. §483.35. In 2024, the Biden administration promulgated a 24-hour RN staffing requirement, but this was repealed by the Trump administration.

worker.[2]

Many states have adopted stricter staffing requirements, with California standing out for its relatively stringent rules. In addition to regulating the minimum number of care hours per patient for all facilities, California also sets higher requirements based on the size of the facility. In particular, while all California nursing homes are required to have an RN or licensed vocational nurse (LPN) on duty 24 hours a day, facilities with 60 or more beds are required to have a separate director of nursing services, while facilities with 100 or more beds are required to have 24-hour RN (rather than LPN) staffing.[3] RN staffing is higher-skill than LPN, but also much more expensive. In our data, the average RN wage for a California facility is just over $42 an hour vs only $31 for LPNs. This means the annual cost of 24 hours of RN coverage is over $100,000 more than the same amount of LPN coverage. Thus, the staffing rule potentially introduces a strong incentive for facilities to manipulate their scale to avoid being subject to the regulation.

Importantly, there are no other discontinuous regulations of California nursing homes at the 100-bed threshold. In addition to the staffing requirements discussed above, the only other size-based regulations imposed by California on nursing homes are at 6, 50, 60, and 200 beds.[4] These regulations generally relate to only facilities with intermediate care units, or are easily met, such as the requirement to have a telephone. Thus, any discontinuity in staffing or bunching at 100 beds can be plausibly attributed to the 24-hour RN staffing requirement.

2.2 Data

Our analysis draws on three complementary administrative data sources that offer detailed, facility-level information on nursing homes across the United States: the Healthcare Cost Report Information System (HCRIS), the Online Survey Certification and Reporting (OSCAR) system, and the payroll-based journal (PBJ) staffing data. HCRIS is maintained by the Centers for Medicare & Medicaid Services (CMS) and includes cost reports submitted annually by all Medicare-certified facilities. These reports contain rich information about facility scale, including the total number of certified beds, and operations, including staff hours and wages for registered nurses and other staff.

The OSCAR system provides inspection and survey data collected by state licensing agencies. These data similarly allow us to validate bed counts and offer an alternative measure of staffing.

Finally, the PBJ staffing data, which are only available starting in 2017, offer further measures of staffing that are thought to be the most accurate of our three data sources. We merge these three data sources using facility identifiers, creating a panel that captures both facility size and staffing levels.

[2] See 42 C.F.R. §483.70(o).

[3] See Cal. Code Regs. Tit. 22, §72329.

[4] See Cal. Code Regs. Tit. 22, §72038, §72513, §72329, and §72617.

Our key measure of staffing is RN hours per bed-day. RN hours are the staffing level subject to the regulation, and scaling the facility-wide reported staffing level by bed-days allows for a measure of staffing that is not mechanically related to facility size. While the regulation required 24-hour RN staffing, we observe only total RN hours per day. Thus our outcome measures whether facilities increased RN staffing in response to the regulation and abstracts from any reallocation of RN hours within the day.

We report estimates using staffing data from each data source independently, with the PBJ data serving as our preferred, primary source. The assignment variable is number of beds, which we validate is consistently reported in both the HCRIS and OSCAR data.

We also use geographic information on facility locations combined with data on ZIP code-level demographics from the American Community Survey. These data allow us to investigate the extent to which nursing homes in different areas respond heterogeneously to the staffing requirement, splitting the sample into facilities in ZIP codes that have median incomes, Black population shares, and Hispanic population shares above the median for California ZIP codes.

Appendix Table A.1 reports sample means for the number of beds and staffing level measured using each data source, along with facility characteristics. Our sample is limited to facility-years in which all facility characteristics and the size of the facility are observed. We further exclude facilities in Wisconsin because it is unclear whether it had a similar staffing requirement as California in place during our sample period. In total, we have nearly 250,000 facility-year observations from 2000–2019 with over 20,000 of these for facilities in California.

3 Methodology

3.1 Setup

In this section, we introduce a simple model of a firm's decision to avoid or comply with a regulation that applies only above a given threshold in the assignment variable, which in our context is facility size. Firm i chooses the assignment variable a_i and the level of the regulated variable r_i, which in our setting is staffing. Firm utility is determined by how close it is to its preferred assignment variable level a_i^* and preferred value of the regulated variable r_i^* such that facilities maximize

$$U_i = g_i(a_i - a_i^*, r_i - r_i^*)$$

where $g_i()$ is a firm-specific function that is decreasing in $|a_i - a_i^*|$ and $|r_i - r_i^*|$. Hence, firms will choose $a_i = a_i^*$ and $r_i = r_i^*$ in the absence of regulation ($D_i = 0$). On the other hand, in the presence of regulation ($D_i = 1$), firms with an assignment variable greater than a regulatory threshold A must have a value of the regulated variable of at least R, so (a_i^*, r_i^*) may no longer be feasible.

Note that this model is similar to a regression discontinuity (RD) design because the treatment (the size-based regulation) applies only to entities that exceed some threshold. In contrast to standard treatments of RD (McCrary, 2008; Imbens and Lemieux, 2008), we allow firms to manipulate their treatment status, choosing the assignment variable a_i, which corresponds to the running variable in previous discussions of RD. Unlike in RD settings, because the assignment variable is manipulable, treatment status D_i will refer to the presence of threshold-based regulation, rather than an entity exceeding the regulatory threshold. Thus in our context, D_i is an indicator for the facility i being in California, the state with the staffing regulation.

To succinctly express firms' hypothetical choices under different regulation regimes, we use potential outcomes notation:

$$a_i = a_{1i} \cdot D_i + a_{0i} \cdot (1 - D_i),$$

$$r_i = r_{1i} \cdot D_i + r_{0i} \cdot (1 - D_i).$$

Similarly, let $f_{\tilde{D}_i=d|D_i=d'}(a)$ denote the distribution of the assignment variable among firms with treatment status d' had their treatment status been d instead. Note that when $d = d'$, $f_{\tilde{D}_i=d|D_i=d'}(a)$ is simply the observed distribution of the assignment variable for the relevant subsample, which we will denote by $f_d(a)$.

First, we observe that two types of firms will choose $a_{0i} = a_{1i} = a_i^*$ and $r_{0i} = r_{1i} = r_i^*$ even in the presence of regulation because their preferred assignment and regulated variable amounts remain admissible under the regulation:

- *Never-regulated firms*: firms with a preferred assignment variable value small enough such that they are not subject to the regulation, or $NR \equiv \{i : a_i^* \leq A\}$;
- *Self-regulated firms*: firms that are large enough to be subject to the regulation but with a preferred value of the regulated variable already meeting the requirement, or $SR \equiv \{i : a_i^* > A, r_i^* \geq R\}$.

In addition, under our model, firms will deviate from a_i^* or r_i^*, but never both, given that the utility function is maximized at $r_i = r_i^*$ regardless of the value of a_i and vice versa.[5] Furthermore, because firm utility is decreasing in the distance from the firm's preferred assignment and regulated variable values, firms that deviate in terms of the assignment variable will choose $a_{1i} = A$ and just barely avoid the regulation, and firms that deviate in terms of the regulated variable will choose

[5]This may seem to be a restrictive assumption, given that facilities that downsize may need less staff as a result. However, rather than total staffing, we use staffing hours per bed-day as our staffing measure, which is already normalized and thus less likely to be mechanically related to facility size. We validate that in states not subject to the regulation, our staffing measures do not meaningfully vary by facility size, as shown by Figure 1 and Appendix Figure A.2.

$r_{1i} = R$ and just barely comply with the regulation. This allows us to decompose remaining firms into two types:

- *Bunchers*: firms that reduce their assignment variable value to avoid regulation, or $B \equiv \{i : a_{1i} \leq A < a_{0i} = a_i^*, r_{0i} = r_{1i} = r_i^* < R\}$;
- *Rule-takers*: firms that are large enough to be subject to the regulation and comply with it by increasing their regulated variable value, or $RT \equiv \{i : a_{1i} = a_{0i} = a_i^* > A, r_{1i} \geq R > r_{0i} = r_i^*\}$.

In this model, the welfare effects of the regulation come from the changes in behavior by bunchers and rule-takers. Never-regulated and self-regulated firms do not change their behavior in response to the regulation, so for social welfare calculations, they are irrelevant. Denoting the social value (ignoring the private costs and benefits) of a firm having assignment variable a_i with regulated variable r_i as $w_i(a_i, r_i)$, the welfare effect of the regulation is

$$\mathcal{W} = \sum_{i \in \{RT\}} [w_i(a_i^*, R) - w_i(a_i^*, r_i^*)] - [g_i(0,0) - g_i(0, R - r_i^*)] + \sum_{i \in \{B\}} [w_i(A, r_i^*) - w_i(a_i^*, r_i^*)] - [g_i(0,0) - g_i(A - a_i^*, 0)], \quad (1)$$

where each first bracketed term is the social benefit of the firm complying with the regulation and the second is the private cost to the firm.

Equation (1) makes clear that the welfare effect of the regulation in the presence of avoidance behavior depends on two key objects of interest: the (social and private) effects of avoidance behavior by bunchers, and the effects of compliance by rule-takers. Thus, we will estimate these two objects.

3.2 Bunching Estimation

The manipulation region (the region for which $a_{0i} \neq a_{1i}$ for any i) is given by $\mathcal{M} = [\underline{A}, \overline{A})$. Thus, the model predicts that $f_0(a) = f_1(a)$ for $a < \underline{A}$, with $f_1(A) \geq f_0(A)$ and $f_1(a) \leq f_0(a)$ for $a > A$. That is, there will be excess mass just below the threshold and missing mass in the manipulation region.

A standard bunching estimator is to fit a polynomial using data for treated firms outside the manipulation region, and interpolate based on data (Kleven, 2016). While we employ this standard bunching estimator as a robustness check, our setting provides us with a number of opportunities to overcome well-known limitations of these standard methods (Kleven, 2016). In particular, in our setting, we observe the distribution of facility sizes in states not subject to the staffing requirement. Under the assumption that the distributions of preferred facility size within the manipulation region are the same in California as in other states,

$$f_{\tilde{D}_i=0|D_i=1,a_i\in\mathcal{M}}(a) = f_{\tilde{D}_i=0|D_i=0,a_i\in\mathcal{M}}(a),$$

we can use the size distribution of these untreated firms as a counterfactual for the size distribution of treated firms had they not been subject to regulation.[6] This existence of a control group allows us to relax the functional form assumptions of standard bunching estimators and estimate the counterfactual size distribution non-parametrically, similar to the difference-in-discontinuity designs (Kleven et al., 2011; Brown, 2013; Benzarti, 2020). For this reason, we employ standard bunching estimators fitting a polynomial as a robustness check.

Regardless of how the counterfactual distribution of facility size is obtained, the proportion of bunchers among treated firms in the bunching region is given by the share of mass at the bunching point that is excess mass,

$$\mathbb{P}[B \mid a_{1i} \in [\underline{A}, A]] = \frac{(F_1(A) - F_1(\underline{A})) - (F_0(A) - F_0(\underline{A}))}{F_1(A) - F_1(\underline{A})}, \tag{2}$$

and the proportion of bunchers among untreated firms above the bunching point is given by the share of mass at point a that is missing,

$$\mathbb{P}[B \mid a_{0i} = a > A] = \frac{f_0(a) - f_1(a)}{f_0(a)}, \tag{3}$$

as in standard treatments of bunching (Chetty et al., 2011; Kleven and Waseem, 2013; Kleven, 2016)

While bunching estimators usually emphasize excess mass, the implied counterfactual location of bunchers also provides a measure of the intensive size distortion induced by the regulation. We summarize this distortion by estimating the mean preferred size of bunching facilities $\mathbb{E}[a_{0i} \mid B, D_i = 1]$. While Equation (2) reports *how many* entities bunch, the *degree* to which these entites manipulate their assignment variable is a useful complement for understanding the severity of the avoidance behavior. Although this object follows directly from bunching accounting and is implicitly used in the estimation of structural parameters underlying bunching behavior (Kleven, 2016), to our knowledge, we are the first to report it directly as a transparent, policy-relevant summary of the intensive margin of avoidance.

To obtain the counterfactual average size of bunching facilities, we first estimate the counter-

[6] While the assumption of equal distributions within the manipulation region is a strong assumption, one can test whether it is likely to hold by comparing the size distributions of treated and untreated firms outside this region. As we show in Figure 2, this is the case in our setting. If the size distributions differ outside the manipulation region, one can also weight untreated by observable characteristics firms to ensure that their size distribution matches that of the treated firms outside $\mathcal{M}$.

factual size of all facilities in the bunching region. This is given by the value necessary to equalize the counterfactual mean assignment variable for the treated and control entities, or more generally, the treated entities and the previously identified counterfactual mean of the assignment variable:

$$\mathbb{E}[a_{0i} \mid a_i \in [\underline{A}, A], D_i = 1] = \tag{4}$$

$$\frac{\mathbb{E}[a_i \mid a_i \in \mathcal{M}, D_i = 0] - (1 - \mathbb{P}[a_i \in [\underline{A}, A] \mid a_i \in \mathcal{M}, D_i = 1])\,\mathbb{E}[a_i \mid a_i \in (A, \overline{A}), D_i = 1]}{\mathbb{P}[a_i \in [\underline{A}, A] \mid a_i \in \mathcal{M}, D_i = 1]}.$$

We then scale the counterfactual mean of all entities in the bunching region by the share that are bunchers rather than never-regulated firms from Equation (2) to obtain the counterfactual average size of bunchers:

$$\mathbb{E}[a_{0i} \mid B, D_i = 1] = \tag{5}$$

$$\frac{\mathbb{E}[a_{0i} \mid a_i \in [\underline{A}, A], D_i = 1] - (1 - \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1])\,\mathbb{E}[a_i \mid a_i \in [\underline{A}, A], D_i = 0]}{\mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1]}.$$

3.3 Characterizing Bunchers

Next we characterize bunchers along two dimensions. First, we estimate heterogeneity in the amount of manipulation of the assignment variable among bunchers. To do this, we note that all of the methods discussed in the previous subsection apply conditional on exogenous facility characteristics. Thus, we can use Equation (4) to identify the degree to which facilities with different baseline characteristics manipulate their size.

Second, we estimate heterogeneity in the propensity of facilities with different characteristics to bunch at all. To do this, we make an assumption on continuity of potential outcomes and facility characteristics, specifically, that outcomes for never-regulated firms in the treatment group—$m_{NR}(a) \equiv \mathbb{E}[r_{0i} \mid NR, D_i = 1, a_i = a]$—are smooth throughout for $a \leq A$. This allows us to extrapolate using observations below the manipulation region ($a < \underline{A}$) to obtain $\mathbb{E}[r_{0i} \mid NR, a_i \in [\underline{A}, A], D_i = 1]$.[7] This is reminiscent of the assumption requiring continuity of potential outcomes in RD settings (Imbens and Lemieux, 2008), with a couple of differences. First, we allow extrapolation through a region $[\underline{A}, A]$ rather than to a single point, and second, in the case where $\underline{A} = A$ our assumption is slightly weaker in that it only requires left continuity.

This assumption gives us the characteristics of the never-regulated firms at the bunching point. The intuition is that because there is no discrete change in treatment status, any discontinuity in facility characteristics—including potential outcomes—at the bunching point relative to below the bunching region must come from selection (Caetano, 2015; Caetano et al., 2025). In particular,

[7] Note that the characteristic here is the potential staffing outcome r_{0i}, but the assumption is analogous for any fixed characteristic X_i.

the continuity assumption ensures that any discontinuous change in the observed staffing level for facilities at the bunching point relative to slightly smaller facilities must come from the bunching facilities that in the absence of the regulation would not be at the bunching point. With both the share of bunchers and never-regulated firms and the potential outcomes for the never-regulated firms in hand, we can then back out the potential outcomes for the bunchers as well.

Figure 1 shows this identification graphically. The solid points denote observed staffing as a function of facility size in California. The dip at the threshold can be decomposed into average staffing for bunchers (shown by the open triangle) and non-bunchers (i.e., never-regulated facilities, shown by the open circle), with weights given by the share of bunchers among facilities at the threshold and its complement. Continuity of potential outcomes allows us to identify average staffing for non-bunchers, which allows us to solve for average staffing among bunchers:

$$\mathbb{E}[r_{0i} \mid B, D_i = 1] = \tag{6}$$
$$\frac{\mathbb{E}[r_i \mid a_i \in [\underline{A}, A], D_i = 1] - (1 - \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1])\,\mathbb{E}[r_{0i} \mid NR, a_i \in [\underline{A}, A], D_i = 1]}{\mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1]}.$$

3.4 Estimation of Treatment Effect of Compliance

To identify the treatment effect of compliance with the regulation using our model, we once again decompose counterfactual outcomes for treated firms with preferred sizes above the threshold (shown by the portion of the line above the threshold in Figure 1) into components for bunchers (shown by the open triangle in the same figure) and non-bunchers (shown by the open square), with weights given by the probability of bunching among facilities with preferred size above the threshold and its complement.[8] As shown by the dashed line, we assume that counterfactual outcomes for the combined group can be obtained by smoothly extrapolating outcomes for never-regulated facilities, while counterfactual outcomes for bunchers is given by Equation (6). Hence, we can solve for counterfactual outcomes among non-bunchers (rule-takers and self-regulated firms) above the threshold:

$$\mathbb{E}[r_{0i} \mid a_i \in (A, \overline{A}), D_i = 1] = \tag{7}$$
$$\frac{\mathbb{E}[r_{0i} \mid a_{0i} \in (A, \overline{A}), D_i = 1] - \mathbb{P}[B \mid a_{0i} \in (A, \overline{A}), D_i = 1]\mathbb{E}[r_{0i} \mid B, D_i = 1]}{1 - \mathbb{P}[B \mid a_{0i} \in (A, \overline{A}), D_i = 1]}.$$

[8] Note that it is possible to derive bounds for the effect in a fully reduced form way without using our model, as described in Gerard et al. (2020). Unless manipulation is modest, these bounds tend to be wide, as is the case in our setting. Comparing staffing at the regulatory threshold with the trimmed distributions of staffing just below the threshold, we obtain bounds for the treatment effect on RN hours per bed of $[-0.213, 0.338]$, which is extremely wide given that mean staffing levels in California are 0.317.

The first term in the numerator is the counterfactual mean of the regulated variable among all facilities that would be above the treatment threshold in the absence of the regulation (bunchers, rule-takers, and self-regulated firms), while the second term is the product of the share of this group that is bunchers (from Equation (3)) and the counterfactual mean value of the regulated value among bunchers (from Equation (6)). Thus, this equation formalizes the idea that we are identifying the counterfactual outcomes of those that choose to remain above the regulatory threshold by comparing the (extrapolated) counterfactual outcomes among all the firms that would be above the threshold and "subtracting off" the (already-identified) counterfactual for bunchers.

With the counterfactual outcomes for those that comply with the regulation in hand, the average effect of the regulation on the firms subject to the regulation (those that remain in the treated region, or rule-takers and self-regulated firms), $TOT \equiv \mathbb{E}[r_{1i} - r_{0i} \mid a_i \in (A, \overline{A}), D_i = 1]$, is given by the difference between observed and counterfactual outcomes for these firms:

$$TOT = \mathbb{E}[r_i \mid a_i \in (A, \overline{A}), D_i = 1] - \mathbb{E}[r_{0i} \mid a_i \in (A, \overline{A}), D_i = 1]. \tag{8}$$

Derivations of all formulae are given in Appendix B, and for all estimates in the next section, we report standard errors from 2,000 bootstrap iterations clustered at the facility level.

4 Results

4.1 Extent of Bunching

Figure 2 shows the distribution of nursing home sizes in California $f_1(s)$ (in blue), and in other states $f_0(s)$ (in red). With the exception of excess mass in California just below the threshold above which California facilities are required to have a separate director of nursing, the size distributions in California and other states leading up to the threshold are very similar. This suggests that untreated states may serve as a reasonable counterfactual distribution for California. On the other hand, we observe a striking amount of excess mass at the threshold in California, with no similar spike in other states.[9] In particular, 19% of California facilities have 99 beds, over 16 times the share in other states. This difference is highly statistically significant ($p < 0.001$), with California also having statistically significantly more facilities at 98 beds and 97 beds ($p < 0.05$), although these differences are much more modest.[10] For this reason, we treat 97–99 beds as the bunching region. Using Equation (2), we estimate that the share of nursing homes in California in the

[9] If anything, there is a small spike *above* the threshold at 100 beds in control states, which may be due to round number bias.

[10] California facilities are 45% (37%) more likely to have 97 (98) beds.

bunching region that are bunchers is 89.3%.[11]

Above the threshold, we observe missing mass in California, with $f_1(s) < f_0(s)$ until roughly 135 beds at which point $f_1(s) \approx f_0(s)$ once again, suggesting this is the point at which the manipulation region ends. The amount of missing mass decreases meaningfully after the threshold for the federal requirement to employ a social worker, consistent that few facilities that would otherwise comply with the social worker requirement manipulating their size to avoid the 24-hour RN requirement.

More formally, we can use Equation (3) to compute the share of bunchers as a function of preferred (counterfactual) bed sizes in California in the absence of regulation. We estimate that nearly 80% of facilities with 100–104 beds would prefer to bunch than comply with the staffing requirement, with this percentage remaining above 50% through the 120–125 bed bin. Beyond 135 beds, the estimated share of bunchers is not statistically different from zero.[12]

We reach similar conclusions if we estimate the counterfactual distribution using traditional methods, as shown in Appendix C.

Not only do many facilities choose to bunch to avoid the staffing regulation, but bunching results in large changes in facility size. Using Equation (4) and a manipulation region of $\mathcal{M} = [97, 135)$, we estimate that facilities in the bunching region would counterfactually have had 112.1 beds. For bunchers specifically, the average counterfactual size is 113.8 beds,[13] meaning bunching facilities shrink by over 13%, on average.

4.2 Characteristics of Bunchers

Having demonstrated that there is extensive bunching in this context, we next turn to characterizing bunchers along observable characteristics. Table 1 reports heterogeneity in the propensity of facilities to bunch as well as in the extent to which facilities manipulate their size to avoid the staffing requirement.

Panel A of Table 1 characterizes bunchers and non-bunchers (i.e., never-regulated firms) just below the threshold, using methods from Section 3.3. To extrapolate baseline characteristics of never-regulated firms to the manipulation region below the threshold, we estimate a local linear fit with MSE-optimal bandwidth (Imbens and Kalyanaraman, 2012; Calonico et al., 2014; Gelman and Imbens, 2019). We find that bunchers are somewhat more likely to be for-profit and chain-owned than non-bunchers, but these differences are not statistically significant, with no meaningful difference in the probability the facility has an Alzheimer's special care unit. In terms of the ZIP

[11] The standard error of this estimate is 0.80%. Appendix Figure A.1a reports the share of bunchers at each size from 95 to 99 beds.

[12] These estimates are reported in Figure A.1b.

[13] This estimate has a standard error of 0.57 beds.

code of the facility, bunchers are much more likely than non-bunching facilities to be in areas with below-median income and an above-median Hispanic population share. In Appendix Table A.2, we show that we get broadly similar results estimating Equation (2) using the conditional size distribution for facilities with each characteristic in other states as the counterfactual.

Panel B of Table 1 shows that there is limited heterogeneity in the *degree* of size manipulation by bunching facilities by facility characteristics, with none of the differences being statistically significant. Given the similar degree of bunching and the greater propensity to bunch, these results indicate that not only does the staffing requirement reduce facility size, but this reduction in access is most acute in disadvantaged areas.

4.3 Effect on Staffing

Next, we estimate the effect of regulation on staffing following subsection 3.4.[14] Figure 3 shows registered nurse hours per bed-day as a function of facility size. Notice the similarity with Figure 1, where staffing among facilities at the bunching point is significantly below the average among those below the bunching region. At the same time, above the threshold, staffing is higher in the manipulation region before falling back to the pre-manipulation region mean. These patterns suggests, as predicted by the model, that facilities with low staffing (and thus the highest necessary change in staffing) are disproportionately bunching below the 100-bed threshold. We observe no such patterns in states other than California.[15]

In addition, we observe in Figure 3 that staffing is roughly constant as a function of facility size outside the manipulation region. Hence, we assume that staffing is mean-independent of preferred size conditional on treatment status ($\mathbb{E}[r_{0i} \mid a_{0i} \in \mathcal{M}, D_i = 1] = \mathbb{E}[r_i \mid a_i < \underline{A}, D_i = 1]$), which simplifies treatment effect estimation. Thus, we assume the counterfactual mean staffing at all (counterfactual) facility sizes is the same as that of the never-regulated, which we estimate using facilities with between 60 and 96 beds: 0.309 RN hours per bed-day.

Using Equation (6), we find that mean staffing among bunchers $\mathbb{E}[r_{0i} \mid B, D_i = 1]$ is 0.280, 9.4% lower than among never-regulated facilities, indicating that bunchers are negatively selected on staffing level.

Having in hand the overall counterfactual mean staffing level, the share of facilities that bunch, and the staffing level of bunching facilities, we can then calculate the counterfactual staffing level of facilities subject to the regulation that do not bunch (rule-takers and self-regulated firms) using Equation (7). We find that the counterfactual mean staffing among compliant facilities in the manipulation region $\mathbb{E}[r_{0i} \mid a_i \in (A, \overline{A}), D_i = 1]$ is 0.353 RN hours per bed-day. This compares with

[14] Appendix Figure A.2 and Table A.3 show we get very similar results using staffing data from OSCAR or HCRIS, rather than PBJ.

[15] See Appendix Figure A.3.

mean staffing among the treated facilities of 0.380. Therefore, the estimated treatment effect on the treated $TOT \equiv \mathbb{E}[r_{1i} - r_{0i} \mid a_i \in (A, \overline{A}), D_i = 1]$ is only 0.026 and not statistically different from zero. This is also much less than the 0.071 difference between facilities subject to the regulation and those with between 60 and 96 beds.

Table 2 summarizes these results. We find that bunching facilities have lower counterfactual staffing levels than non-bunching facilities, such that nearly the entire observed difference in staffing levels among facilities subject to the staffing requirement is due to selection rather than treatment. Thus, while the staffing regulation causes facilities to manipulate their size to avoid it, it has only a negligible effect on staffing.

4.4 Welfare Effects

Finally, with the key objects of interest of the effect of avoidance behavior by bunchers and compliance by rule-takers in hand, we return to Equation (1) to assess the overall welfare effect of the staffing policy. To do this, we first note that because we estimate that the policy has a treatment effect on the treated of zero, we can conclude that the welfare effects of rule-takers' compliance behavior are negligible, either because there are very few rule-takers, compliance requires very little change in staffing for rule-takers, or both. Thus, the welfare effects of the policy depend entirely on the welfare effects of avoidance behavior by bunchers.

The welfare effect of bunching in our context is likely to be negative and, we argue, significantly so. This is for two main reasons. First, the model presented in Section 3.1 guarantees that bunching imposes private costs on bunching firms: avoidance behavior requires deviating from firms' preferred number of beds, and is thus costly to these firms.

Second, the avoidance behavior likely has negative social consequences given the degree to which it restricts access to nursing homes in disadvantaged areas. Given the well-established preference of nursing home patients for facilities very close to their homes (Hackmann, 2019; Gandhi, 2023; Einav et al., 2025), the reduction in access in low-income, high-Hispanic ZIP codes we estimate is likely very costly to patients, with its incidence on disadvantaged communities only heightening this cost.

5 Conclusion

This paper highlights how threshold-based regulations can be rendered ineffective when regulated entities can manipulate their treatment status. Using a simple but flexible empirical framework, we document how California's 100-bed staffing rule has led to widespread bunching of nursing homes at 99 beds, with this reduction in access being most severe in disadvantaged areas. Furthermore, because facilities that would otherwise have the lowest staffing manipulate their size to avoid the regulation, the requirement led to virtually no increase in average staffing, illustrating how avoidance behavior can undermine the effectiveness of threshold-based regulations.

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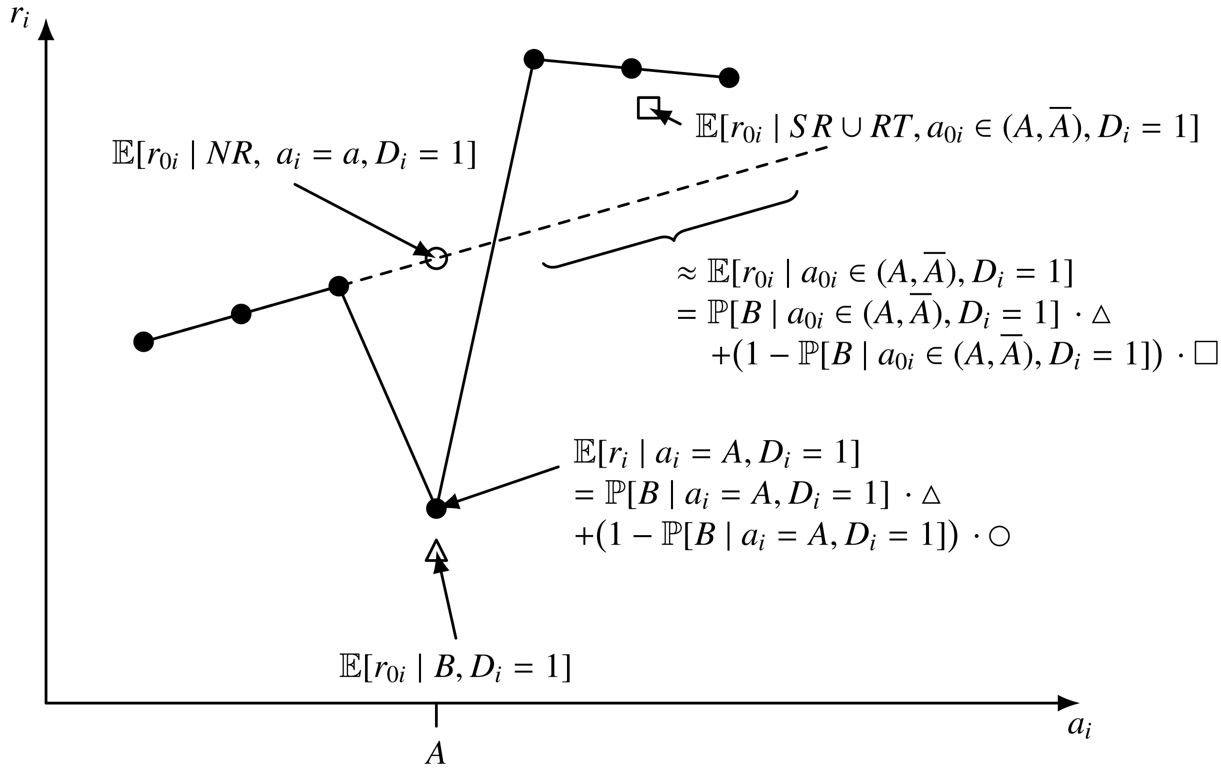
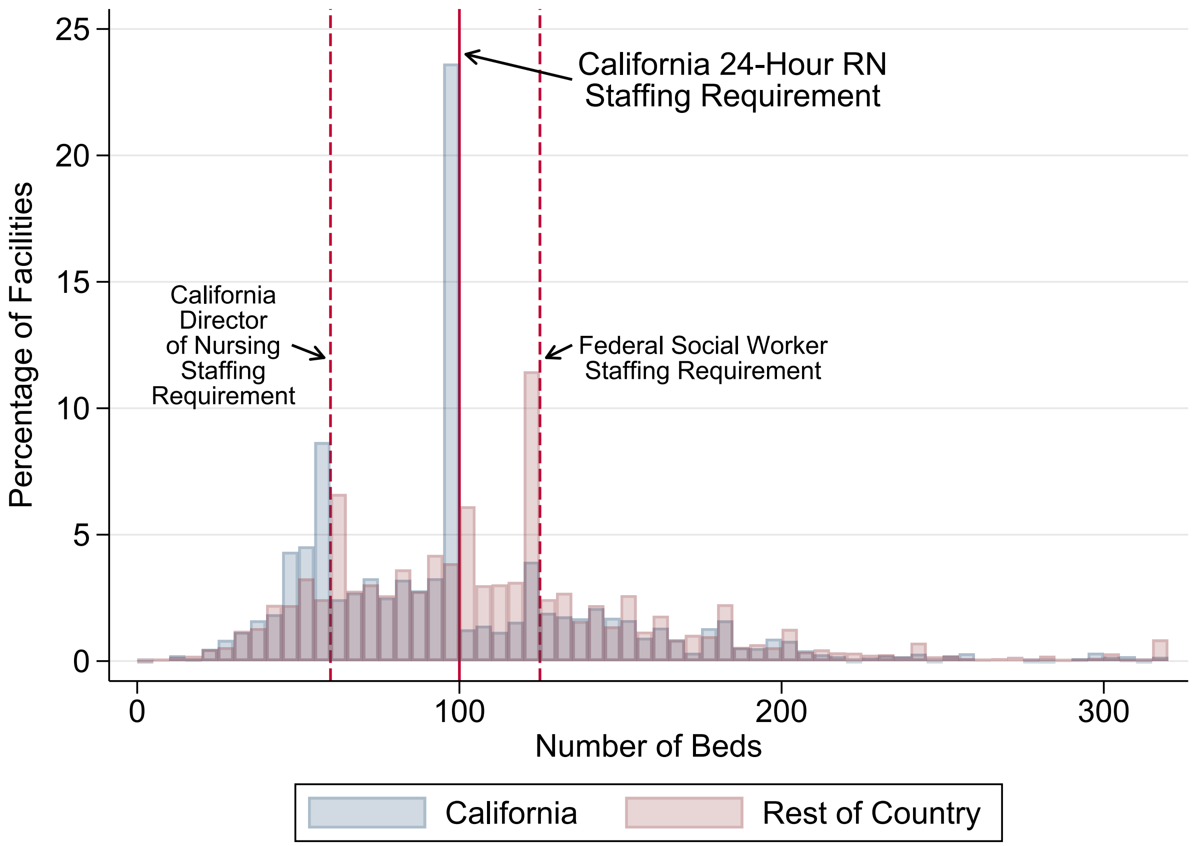
Figure 1. Identification of Potential Outcomes



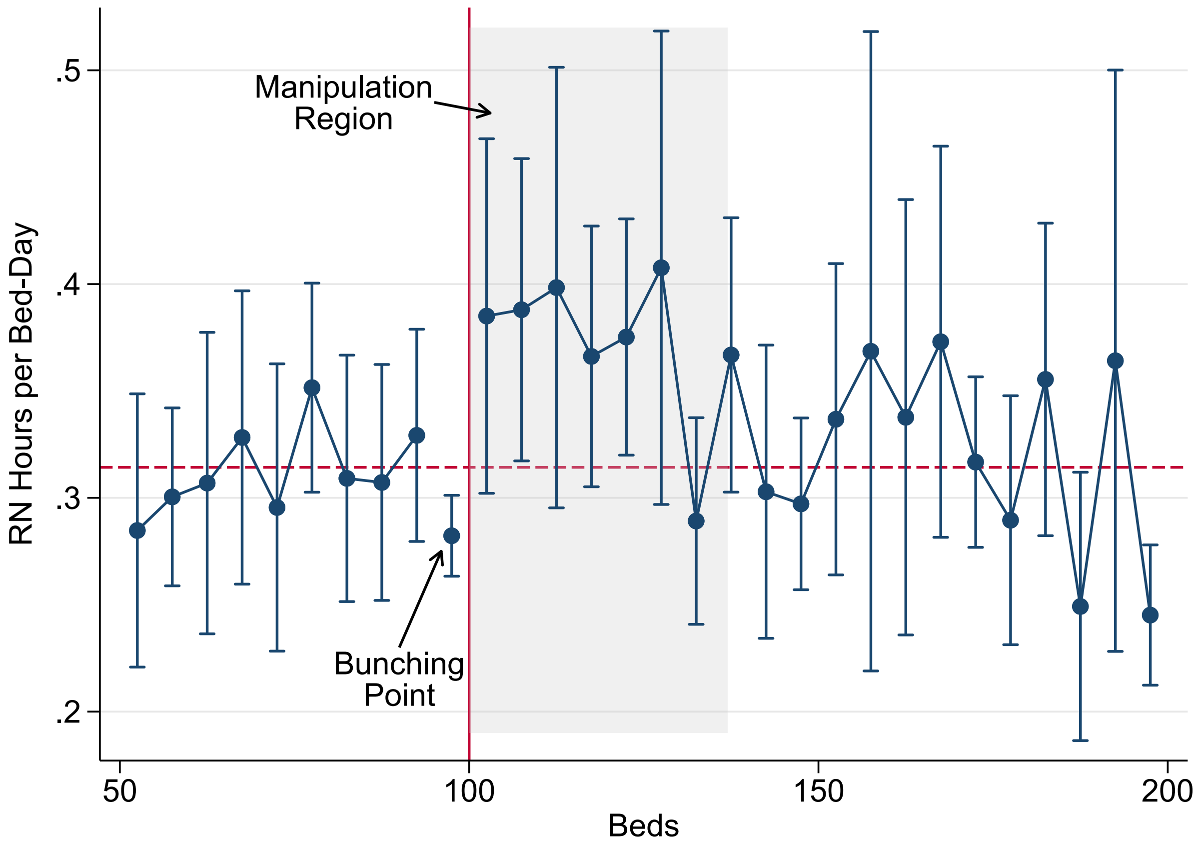
Notes: Figure is a stylized illustration of the identification strategy described in Sections 3.3 and 3.4. Solid points denote hypothetical observed staffing $\mathbb{E}[r_i \mid a_i, D_i = 1]$ as a function of facility size for California. The open shapes represent hypothetical counterfactual means.

Figure 2. Distribution of Nursing Home Sizes in California and in Other States



Notes: Distribution of nursing home bed counts for California (blue) and all other states (red) in 5-bed bins. Sample is restricted to facility-years with non-missing bed count, facility characteristics, and ZIP characteristics for 2000–2019, excluding facilities in Wisconsin. The vertical solid line marks the regulatory threshold *A*, and the vertical dashed lines denote the thresholds above which California facilities are required to have a separate director of nursing services and above which facilities are federally required to employ a full-time social worker.

Figure 3. Staffing Level by California Facility Size



Notes: Figure plots binned-scatter means of RN hours per bed-day from the Payroll-Based Journal (PBJ) data, against facility size in beds (5-bed bins). The vertical red line denotes the regulatory threshold, while the gray shaded area denotes the manipulation region above this threshold. The horizontal dashed line denotes the mean staffing level for facilities with between 60 and 96 beds. The sample is limited to facility-years with non-missing staffing, size, facility characteristics, and ZIP characteristics for 2017–2019. The sample is further limited to facilities in California with between 50 and 200 beds.

Table 1. Characterizing Bunchers and Non-Bunchers

Panel A: Share of Facilities				
Characteristic	Bunchers	Non-Bunchers	Difference	SE of Diff.
Facility Characteristics				
For-Profit	0.918	0.825	0.093	0.064
Chain Ownership	0.530	0.496	0.034	0.091
Has Alz. Unit	0.056	0.064	-0.008	0.030
ZIP Characteristics				
Low-Income	0.663	0.352	0.310***	0.120
High-Black	0.532	0.481	0.051	0.102
High-Hispanic	0.635	0.282	0.353***	0.111

Panel B: Counterfactual Size of Bunchers				
Characteristic	Has Characteristic	Does Not Have Characteristic	Difference	SE of Diff.
Facility Characteristics				
For-Profit	113.7	114.3	-0.53	1.28
Chain Ownership	114.2	113.3	-0.91	0.85
Has Alz. Unit	112.4	113.8	-1.33	2.51
ZIP Characteristics				
Low Income	113.5	114.8	-1.34	1.22
High Black	113.4	114.0	-0.65	1.12
High Hispanic	115.6	113.7	1.96	1.27

Notes: Panel A reports the share of bunchers and non-bunchers (never-regulated firms) in the bunching region (97–99 beds) with each characteristic, estimated by applying Equation (6) with the characteristic X_i in place of staffing r_i. Panel B reports counterfactual size in beds for bunchers in each characteristic group, estimated using Equation (5). ZIP characteristics "Low-Income," "High-Black," and "High-Hispanic" indicate that the facility's ZIP code is below or above the median for California ZIP codes. The sample is limited to facility-years with non-missing size, facility characteristics, and ZIP characteristics for 2000–2019, excluding facilities in Wisconsin. The sample in Panel B is further limited to facilities with between 96 and 135 beds. Standard errors are from 2,000 bootstrap iterations at the facility level. * p<0.1, ** p<0.05, *** p<0.01

Table 2. Effect on Staffing

Parameter	Expression	Estimate
Counterfactual Staffing of All Facilities	$\mathbb{E}[r_{0i} \mid D_i = 1]$	0.309
		(0.0148)
Observed Staffing of Treated Facilities	$\mathbb{E}[r_{1i} \mid RT \text{ or } SR, D_i = 1]$	0.380
		(0.0166)
Staffing of Bunchers	$\mathbb{E}[r_{0i} \mid B, D_i = 1]$	0.280
		(0.0115)
Counterfactual Staffing of Treated Facilities	$\mathbb{E}[r_{0i} \mid RT \text{ or } SR, D_i = 1]$	0.353
		(0.0436)
Treatment Effect on the Treated	$\mathbb{E}[r_{1i} - r_{0i} \mid RT \text{ or } SR, D_i = 1]$	0.026
		(0.0457)

Notes: Estimates of treatment-effect parameters from Section 3.4, using manipulation region $[97, 135)$ and bunching region $[97, 99]$. Staffing is RN hours per bed-day from the PBJ data. The counterfactual mean staffing of all facilities is identified by the mean-independence assumption stated in Section 4.3; bunchers' counterfactual staffing is from Equation (6); the counterfactual staffing of treated facilities is from Equation (7); the treatment effect on the treated (TOT) is from Equation (8). The sample is limited to facility-years with non-missing staffing, size, facility characteristics, and ZIP characteristics for 2017–2019, excluding facilities in Wisconsin. The sample is further limited to facilities with between 60 and 135 beds. Standard errors from 2,000 bootstrap iterations at the facility level are reported in parentheses.

A Appendix Figures and Tables

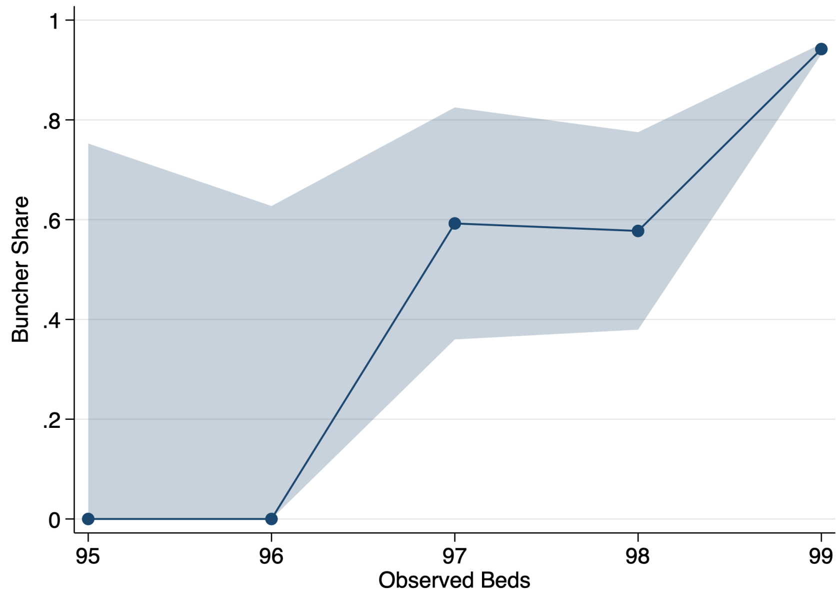
Table A.1. Summary Statistics

	California	Other States
Facility Size		
Beds	100.5	111.4
Staffing Measures		
RNs per bed (PBJ)	0.317	0.329
RNs per bed (OSCAR)	0.397	0.354
RNs per bed (HCRIS)	0.453	0.442
Facility Characteristics		
For-Profit	0.840	0.692
Chain-Owned	0.522	0.564
Has Alzheimer's Unit	0.063	0.182
ZIP Characteristics		
Median Income (thousands)	29.12	26.26
Black Share	0.061	0.120
Hispanic Share	0.359	0.098
Observations	20,494	227,980

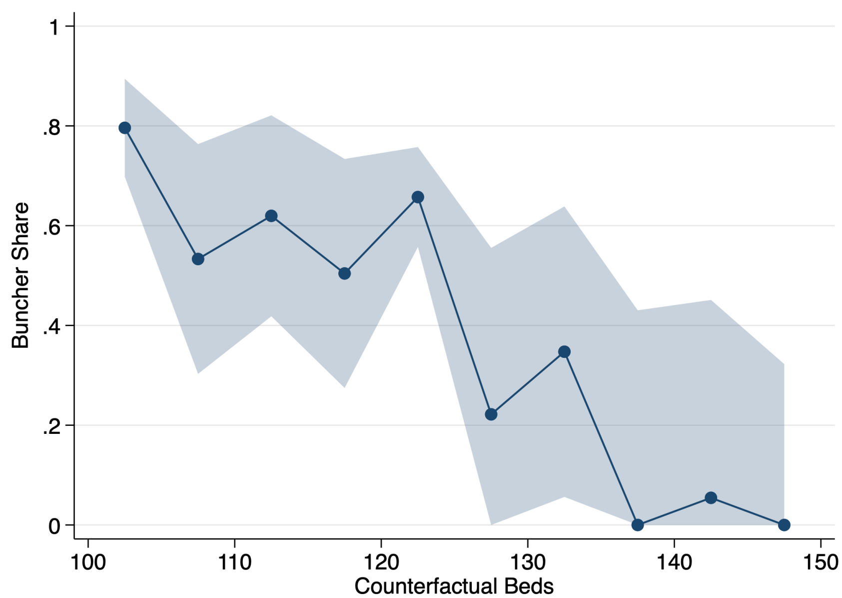
Notes: Sample means of facility size, staffing measures, facility characteristics, and ZIP characteristics for California vs. all other states except Wisconsin, 2000–2019. An observation is a facility-year. The sample is limited to facility-years with non-missing size, facility characteristics, and ZIP characteristics for 2000–2019, excluding facilities in Wisconsin. PBJ staffing data is available only for 2017–2019.

Figure A.1. Share of Facilities that are Bunchers

(a) By Observed Size Below Threshold



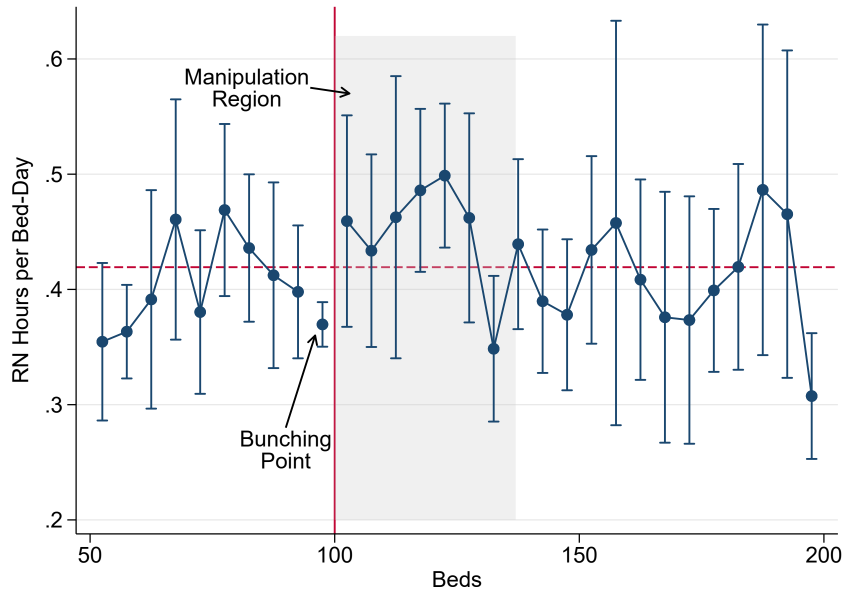
(b) By Counterfactual Size Above Threshold



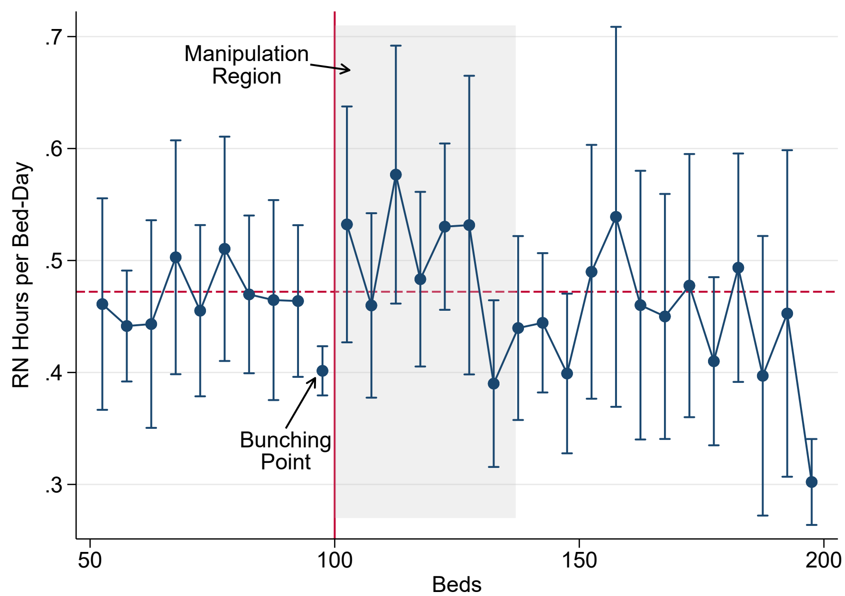
Notes: Panel A reports the estimated share of California nursing homes at each observed bed count that are bunchers, computed as the difference between the density in California and other states, divided by the density in California following Equation (2). Shares are reported for 95–99 beds. Panel B reports the estimated share of California nursing homes at each counterfactual bed count that are bunchers, computed following Equation (3), in 5-bed bins. Shares are reported for 100–149 beds. The sample is limited to facility-years with non-missing size, facility characteristics, and ZIP characteristics for 2000–2019, excluding facilities in Wisconsin. 95% confidence intervals are from standard errors clustered at the facility level.

Figure A.2. Staffing Level in Other Sources by California Facility Size

(a) OSCAR



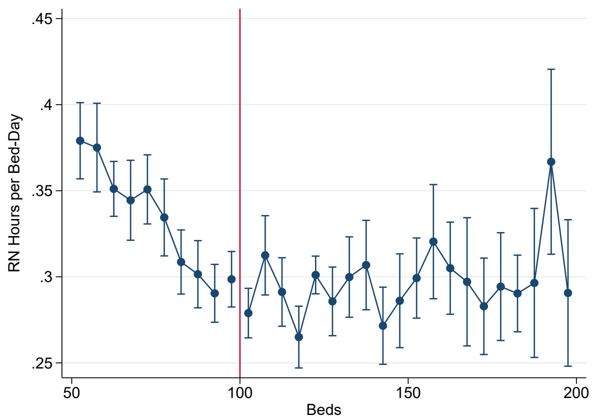
(b) HCRIS



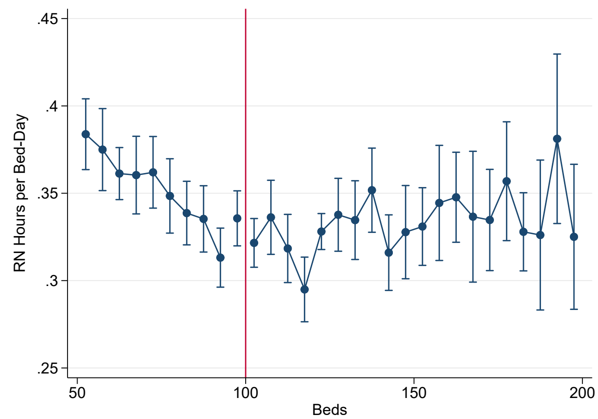
Notes: Panel A plots binned-scatter means of RN hours per bed-day from the OSCAR data, against facility size in beds (5-bed bins). Panel B reports RN hours per bed-day from the HCRIS data. The vertical red line denotes the regulatory threshold, while the gray shaded area denotes the manipulation region above this threshold. The horizontal dashed line denotes the mean staffing level for facilities with between 60 and 96 beds. The sample is limited to facility-years with non-missing staffing, size, facility characteristics, and ZIP characteristics for 2017–2019. The sample is further limited to facilities in California with between 50 and 200 beds.

Figure A.3. Staffing Level by Facility Size in Other States

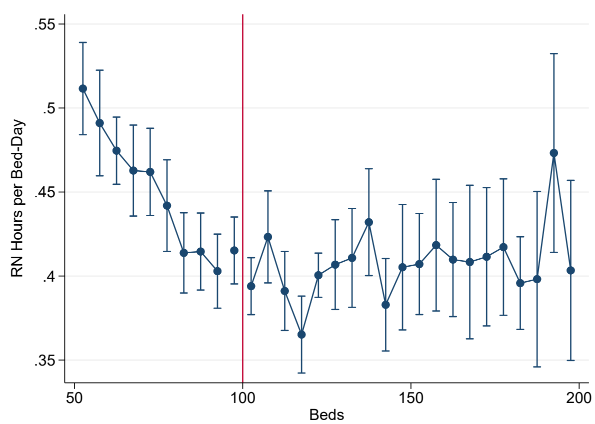
(a) PBJ



(b) OSCAR



(c) HCRIS



Notes: Panel A plots binned-scatter means of RN hours per bed-day from the PBJ data, against facility size in beds (5-bed bins) for facilities in states other than California. Panel B reports RN hours per bed-day from the OSCAR data. Panel C reports RN hours per bed-day from the HCRIS data. The vertical red line denotes the regulatory threshold in California. The sample is limited to facility-years with non-missing staffing, size, facility characteristics, and ZIP characteristics for 2017–2019. The sample is further limited to facilities in states other than California and Wisconsin with between 50 and 200 beds.

Table A.2. Share of Bunchers in the Bunching Region by Characteristics

Facility Characteristic	Has Characteristic	Does Not Have Characteristic	Difference	SE of Difference
For-Profit	0.896	0.864	0.032**	0.016
Chain Ownership	0.885	0.897	-0.013	0.010
Has Alz. Unit	0.886	0.889	-0.003	0.022
Low Income	0.881	0.913	-0.032**	0.014
High Black	0.904	0.869	0.034**	0.014
High Hispanic	0.937	0.881	0.057***	0.017

Notes: Table reports the share of bunchers in the bunching region (97–99 beds) for each characteristic group, estimated using Equation (2) with the sample limited to facilities with or without the relevant characteristic. ZIP characteristics "Low-Income," "High-Black," and "High-Hispanic" indicate that the facility's ZIP code is below or above the median for California ZIP codes. The sample is limited to facility-years with non-missing size, facility characteristics, and ZIP characteristics for 2000–2019, excluding facilities in Wisconsin. The sample is further limited to facilities with between 97 and 135 beds. Standard errors are clustered at the facility level. * p<0.1, ** p<0.05, *** p<0.01

Table A.3. Effect on Staffing, Alternative Staffing Data

Parameter	Estimate	Standard Error
Panel A: Staffing from OSCAR		
Counterfactual Mean Staffing Among All Facilities	0.408	0.0186
Observed Mean Staffing Among Treated Facilities	0.477	0.0194
Mean Staffing Among Bunchers	0.366	0.0115
Counterfactual Mean Staffing Among Treated Facilities	0.471	0.0535
Treatment Effect on the Treated	0.005	0.0560
Panel B: Staffing from HCRIS		
Counterfactual Mean Staffing Among All Facilities	0.459	0.0213
Observed Mean Staffing Among Treated Facilities	0.519	0.0221
Mean Staffing Among Bunchers	0.394	0.0128
Counterfactual Mean Staffing Among Treated Facilities	0.560	0.0612
Treatment Effect on the Treated	-0.042	0.0634

Notes: Estimates of treatment-effect parameters from Section 3.4, using manipulation region [97, 135) and bunching region [97, 99]. Panel A reports RN hours per bed-day from the OSCAR data. Panel B reports RN hours per bed-day from the HCRIS data. The counterfactual mean staffing of all facilities is identified by the mean-independence assumption stated in Section 4.3; bunchers' counterfactual staffing is from Equation (6); the counterfactual staffing of treated facilities is from Equation (7); the treatment effect on the treated (TOT) is from Equation (8). The sample is limited to facility-years with non-missing staffing, size, facility characteristics, and ZIP characteristics for 2000–2019, excluding facilities in Wisconsin. The sample is further limited to facilities with between 60 and 135 beds. Standard errors from 2,000 bootstrap iterations at the facility level are reported in parentheses.

B Derivations

The following steps show the derivation of Equation (4).

$$
\begin{aligned}
\mathbb{E}[a_i \mid a_i \in \mathcal{M}, D_i = 0] &= \mathbb{E}[a_{0i} \mid a_i \in \mathcal{M}, D_i = 0] \\
&= \mathbb{E}[a_{0i} \mid a_{0i} \in \mathcal{M}, D_i = 1] \\
&= \mathbb{E}[a_{0i} \mid a_i \in \mathcal{M}, D_i = 1] \\
&= \mathbb{P}[a_i \in [\underline{A}, A] \mid a_i \in \mathcal{M}, D_i = 1]\mathbb{E}[a_{0i} \mid a_i \in [\underline{A}, A], D_i = 1] \\
&\quad + (1 - \mathbb{P}[a_i \in [\underline{A}, A] \mid a_i \in \mathcal{M}, D_i = 1])\, \mathbb{E}[a_{0i} \mid a_i \in (A, \overline{A}), D_i = 1] \\
&= \mathbb{P}[a_i \in [\underline{A}, A] \mid a_i \in \mathcal{M}, D_i = 1]\mathbb{E}[a_{0i} \mid a_i \in [\underline{A}, A], D_i = 1] \\
&\quad + (1 - \mathbb{P}[a_i \in [\underline{A}, A] \mid a_i \in \mathcal{M}, D_i = 1])\, \mathbb{E}[a_i \mid a_i \in (A, \overline{A}), D_i = 1]
\end{aligned}
$$

Rearranging then yields Equation (4). The first equality comes from the definition of potential outcomes. The second equality comes from the assumption that the distribution of a_i in the manipulation region among untreated units serves as a valid counterfactual for the distribution of a_i absent the regulation among regulated units. The third equality comes from the definition of the manipulation region. The fourth equality follows by the law of total expectation, and the fifth equality follows from the model's prediction that units do not manipulate a_i upwards in response to the regulation.

The following steps show the derivation of Equation (5).

$$
\begin{aligned}
\mathbb{E}[a_{0i} \mid a_i \in [\underline{A}, A], D_i = 1] &= \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1]\mathbb{E}[a_{0i} \mid B, a_i \in [\underline{A}, A], D_i = 1] \\
&\quad + (1 - \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1])\, \mathbb{E}[a_{0i} \mid \neg B, a_i \in [\underline{A}, A], D_i = 1] \\
&= \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1]\mathbb{E}[a_{0i} \mid B, D_i = 1] \\
&\quad + (1 - \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1])\, \mathbb{E}[a_{0i} \mid a_{0i} \in [\underline{A}, A], D_i = 1] \\
&= \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1]\mathbb{E}[a_{0i} \mid B, D_i = 1] \\
&\quad + (1 - \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1])\, \mathbb{E}[a_i \mid a_i \in [\underline{A}, A], D_i = 0]
\end{aligned}
$$

Rearranging then yields Equation (5). The first equality comes from the law of total expectation. The second comes from the definition of bunchers. The third comes from the assumption that the distribution of a_i in the manipulation region among untreated units serves as a valid counterfactual for the distribution of a_i absent the regulation among regulated units and the definition of potential outcomes. Note that $\mathbb{E}[a_{0i} \mid a_i \in [\underline{A}, A], D_i = 1]$ is identified by Equation (4) and $\mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1]$ is identified by Equation (2).

The following steps show the derivation of Equation (6).

$$
\begin{aligned}
\mathbb{E}[r_i \mid a_i \in [\underline{A}, A], D_i = 1] &= \mathbb{E}[r_{0i} \mid a_i \in [\underline{A}, A], D_i = 1] \\
&= \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1]\, \mathbb{E}[r_{0i} \mid B, a_i \in [\underline{A}, A], D_i = 1] \\
&\quad + (1 - \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1])\, \mathbb{E}[r_{0i} \mid \neg B, a_i \in [\underline{A}, A], D_i = 1] \\
&= \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1]\, \mathbb{E}[r_{0i} \mid B, D_i = 1] \\
&\quad + (1 - \mathbb{P}[B \mid a_i \in [\underline{A}, A], D_i = 1])\, \mathbb{E}[r_{0i} \mid NR, a_i \in [\underline{A}, A], D_i = 1].
\end{aligned}
$$

Rearranging yields Equation (6). The first equality follows from the assumption that bunchers do not manipulate r_i and the definition of potential outcomes for the never-regulated firms. The second equality comes from the law of total expectation. The third equality follows by the definition of bunchers and never-regulated firms. Note that the derivation holds for any characteristic X_i (i.e., not just r_i) that is not affected by bunching.

The following steps show the derivation of Equation (7).

$$\begin{aligned}
\mathbb{E}[r_{0i} \mid a_{0i} \in (A, \overline{A}), D_i = 1] &= \mathbb{P}[B \mid a_{0i} \in (A, \overline{A}), D_i = 1]\, \mathbb{E}[r_{0i} \mid B, a_{0i} \in (A, \overline{A}), D_i = 1] \\
&\quad + \Big(1 - \mathbb{P}[B \mid a_{0i} \in (A, \overline{A}), D_i = 1]\Big)\, \mathbb{E}[r_{0i} \mid \neg B, a_{0i} \in (A, \overline{A}), D_i = 1] \\
&= \mathbb{P}[B \mid a_{0i} \in (A, \overline{A}), D_i = 1]\, \mathbb{E}[r_{0i} \mid B, D_i = 1] \\
&\quad + \Big(1 - \mathbb{P}[B \mid a_{0i} \in (A, \overline{A}), D_i = 1]\Big)\, \mathbb{E}[r_{0i} \mid a_i \in (A, \overline{A}), D_i = 1].
\end{aligned}$$

Rearranging yields Equation (7). The first equality follows from the law of total expectation. The second equality follows from the definition of bunchers, rule-takers, and self-regulated firms. Note that the buncher share $\mathbb{P}[B \mid a_{0i} \in (A, \overline{A}), D_i = 1]$ is identified by Equation (3); the bunchers' counterfactual staffing $\mathbb{E}[r_{0i} \mid B, D_i = 1]$ is identified by Equation (6). Thus, having in hand the mean level of the outcome among non-bunchers in the bunching region (In our setting, this is equal to the average staffing below the manipulation region, but in other contexts it may be an extrapolation or the average in non-treated jurisdictions or time periods), delivers the mean counterfactual outcome among the treated.

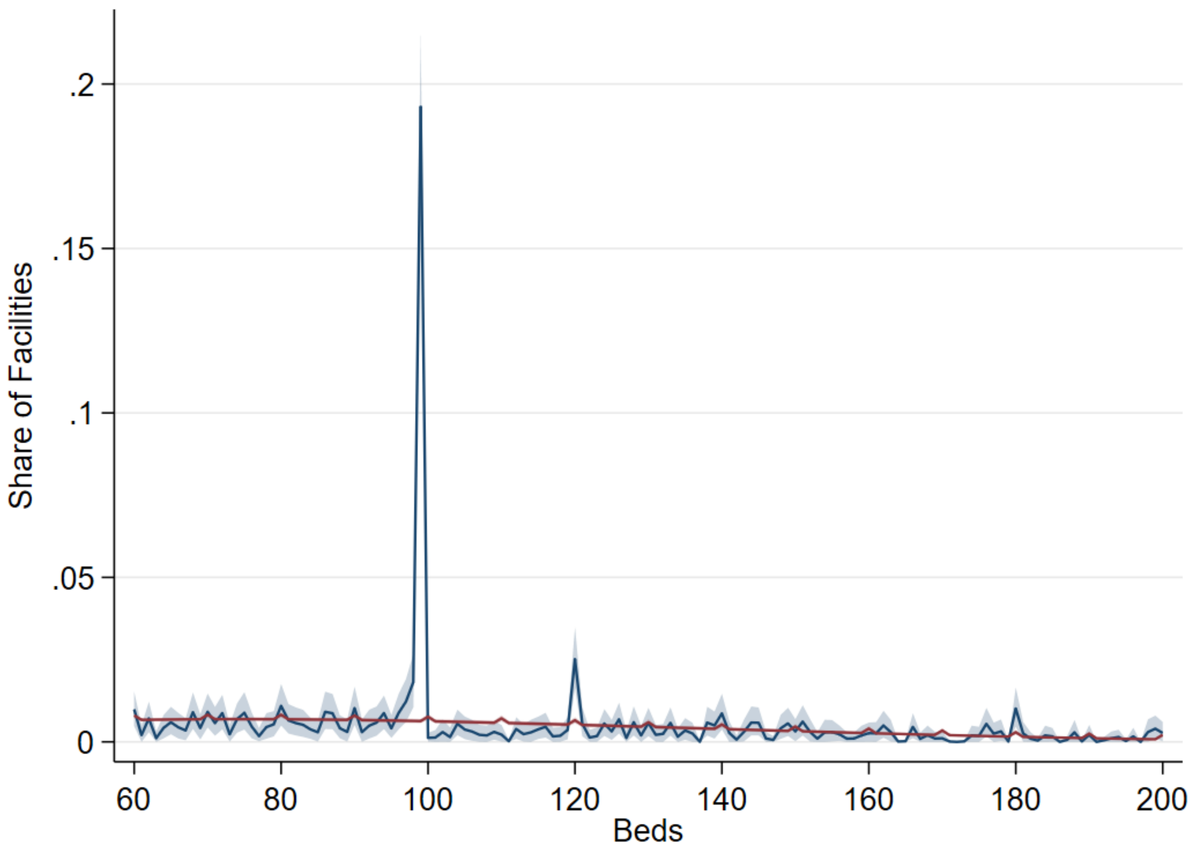
C Results Using Traditional Interpolation Methods

We use data outside a conservative manipulation window of $[90, 150)$ in California to estimate a Kth-order polynomial allowing for spikes at round numbers (specifically, multiples of 10), and interpolate between 90 to 150 beds to obtain the counterfactual distribution in California in the absence of regulation. We obtain similar results for a range of values for $K = 1, \ldots, 10$, although confidence intervals for the interpolated region in the counterfactual distribution tend to get large for $K \geq 10$. So, for brevity, we show results for $K = 5$ here. Appendix Figure C.4 shows that the counterfactual density of beds tends to be greater than the observed distribution above the threshold (other than the spike in the observed distribution around 120) before eventually converging with the observed distribution again.

Appendix Figure C.5 reproduces Appendix Figure A.1, showing the share of bunchers at various observed and counterfactual sizes. Appendix Figure C.5a shows that below the threshold, the fraction of facilities that are bunchers increases as we approach the threshold, and that over 90% of facilities at the threshold of 99 beds are bunchers, supporting our main results using other states as the counterfactual and supporting decision to treat 97–99 beds as the bunching region.

Appendix Figure C.5b reports the share of facilities that would counterfactually have bed counts above the threshold that instead bunch. Like our main results, we estimate roughly 60% of facilities with preferred sizes of 100 to 119 beds are bunchers using these alternative methods. However, the alternative methods result in an estimate of no facilities bunching from 120–124 beds, while our main estimate is over 60%. This is because the traditional interpolation methods cannot account for the bunching at 120 beds corresponding to the federal social worker staffing requirement. This difference highlights the advantage we have in our setting of being able to rely on the distribution of facility sizes in other states, rather than having to rely on traditional interpolation methods. Nonetheless, these traditional methods also result in the estimate manipulation region ending around 135 to 140 beds.

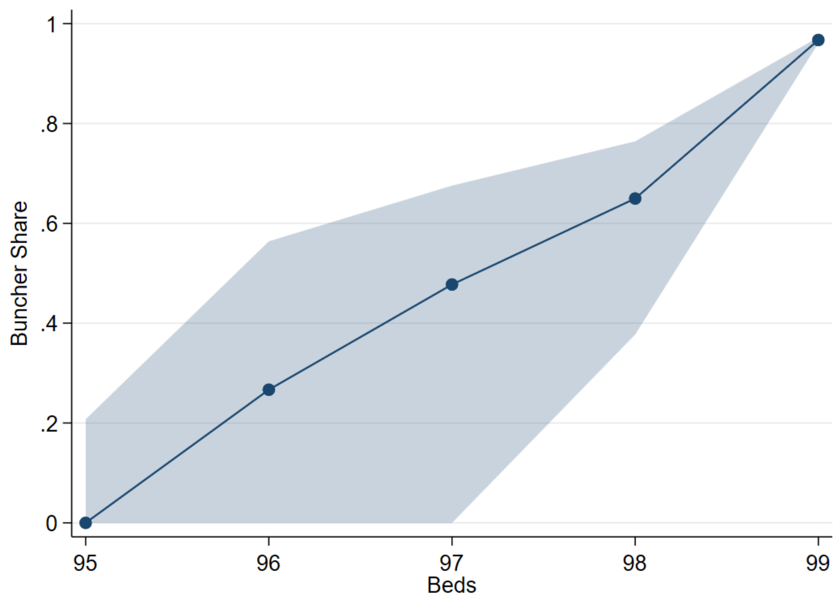
Figure C.4. Observed and Counterfactual Distribution of Beds in California (Based on Polynomial Interpolation)



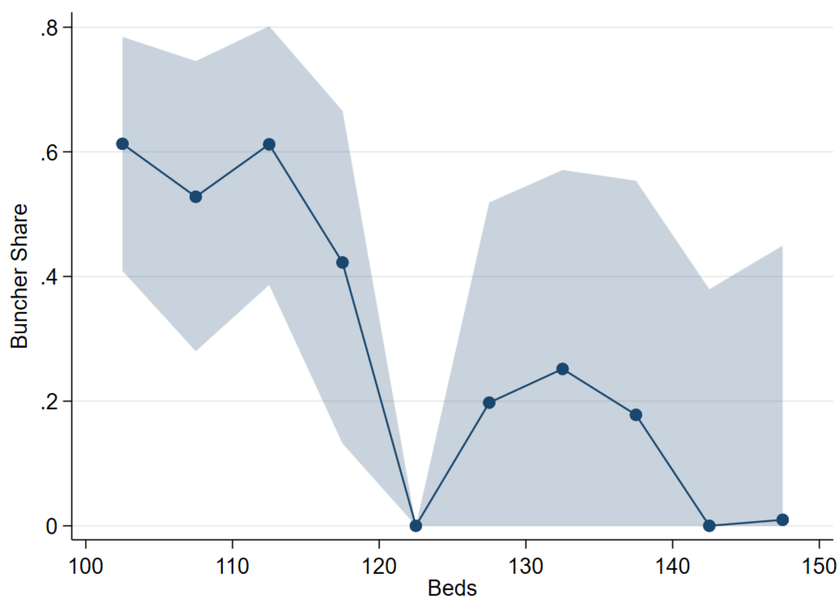
Notes: Observed (solid) and counterfactual (dashed) density of bed counts for California nursing homes using a fifth-order polynomial fit to facility-year counts at bed values outside the manipulation window [90, 150], with separate intercepts allowed at multiples of ten, and interpolated through the window. The sample is limited to facility-years for facilities in California with non-missing size, facility characteristics, and ZIP characteristics for 2000–2019. 95% confidence intervals are from 2,000 facility-level bootstrap iterations.

Figure C.5. Share of Facilities that are Bunchers

(a) By Observed Size Below Threshold



(b) By Counterfactual Size Above Threshold



Notes: Panel A reports the estimated share of California nursing homes at each observed bed count that are bunchers, computed as the difference between the observed density in California and the counterfactual density implied by a polynomial interpolation, divided by the observed density following Equation (2). Shares are reported for 95–99 beds. Panel B reports the estimated share of California nursing homes at each counterfactual bed count that are bunchers, computed following Equation (3), in 5-bed bins. Shares are reported for 100–149 beds. The sample is limited to facility-years for facilities in California with non-missing size, facility characteristics, and ZIP characteristics for 2000–2019. 95% confidence intervals are from standard errors clustered at the facility level.