# How Do Debt Collection Restrictions Affect Hospitals and Patients?

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**Abstract**

Tighter regulations on consumer debt collectors, although intended to curb predatory practices, may disrupt industries to which such debts are owed. Using a paired-county stacked difference-in-differences design, we show that these regulations adversely affect hospitals. While hospital patient volume remains unchanged, their accounts receivable, liabilities, and profitability deteriorate, with stronger effects in states with ex-ante higher debt collector density. Consequently, treatment capacity, employment, and care quality at hospitals decline, while patterns indicating upcoding in hospitalization emerge. Additionally, non-profit hospitals reduce charity care for uninsured patients. Overall, ignoring spillovers of consumer financial protection laws on non-financial sectors may overstate their benefits.

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Third-party debt collection industry in the U.S. specializes in recovering defaulted unsecured debt from consumers and plays an essential role in the consumer credit ecosystem. In 2020, it recovered approximately $102 billion in unsecured unpaid consumer debt and returned nearly $90 billion to creditors (ACA International (2020)). By helping creditors recover unpaid debt, this industry minimizes creditor losses and potentially allows them to expand credit availability, thereby enhancing efficiency of the consumer credit markets. At the same time, serious concerns have been raised about the prevalence of abusive and deceptive collection practices.[1] In an effort to protect consumers from such practices, states enact stricter debt collection laws that typically raise compliance costs and create entry barriers for the industry, for example by imposing mandatory licensing and bond requirements, raising financial penalties, or classifying certain collection practices as criminal offenses etc. While these laws are generally effective in containing the growth of the industry, they can also reduce recovery rates on defaulted unsecured debts, essentially trading off credit market efficiency against consumer protection. This paper emphasizes this trade-off by examining the implications of these stricter regulations for a specific creditor industry—hospitals, which are the largest users of the debt collection industry, accounting for nearly half of all volumes sold to collectors and significantly surpassing financial institutions, which represent only 11% (Fedaseyeu (2020)).

We focus on hospitals for two reasons. The first is their essential role in healthcare delivery, which is not only crucial for individual well-being and health outcomes but also makes a significant contribution to the economy. The healthcare sector is the largest employer and healthcare expenditure accounted for 17% of the national gross domestic product (BLS (2023), CMS (2023)). The second reason is their substantial usage of the debt collection industry, accounting for 47.2% of the volume sold to this sector in 2013, as noted previously. This dependence on collection

[1] For example, the debt collection industry accounts for the highest number of fraud cases filed with the Federal Trade Commission (FTC (n.d.)) and consistently ranks among the top sources of consumer complaints to the CFPB (CFPB (2015)). Reported abusive practices include attempts to collect debts not owed, threats of illegal actions, inappropriate contact with employers or family members, and repeated and harassing phone calls often involving abusive language.

industry is immediately evident in the widespread prevalence of medical debt, which consistently ranks as the most common category of unpaid debt in collections recorded on consumers' credit reports and affects between one-fifth to one-third of the entire American adult population (CFPB (2014), CFPB (2023)).[2]

The key to our empirical strategy is the staggered adoption of stricter third-party debt collection regulations by states. We hand-collected data on changes in state debt collection laws across all 50 states from 2013 till 2022. For earlier years, we supplemented our data with information originally compiled by Fedaseyeu (2020), which was subsequently used by Fonseca (2023) with some modifications. Our sample period begins in 2010—the first year since when hospital-level variables of interest became consistently available in hospital cost report data.[3] Within our sample period spanning 2010 and 2022, we identify 28 instances of regulatory changes that increased restrictions on third-party collection industries.

To estimate the causal effects of the third-party debt collection restriction regulations on hospitals, we combine the methodology of Dube, Lester, and Reich (2010) to compare outcomes in contiguous (neighboring) counties straddling state borders—where one state implements a stricter debt collection law while the other does not—with the stacked difference-in-differences (DiD) approach for staggered treatments (Gormley and Matsa (2011), Cengiz et al. (2019)). Specifically, we classify the counties located along the borders of the states adopting the regulation as treated counties. We then create treated-control county pairs by repeatedly matching each of the treated counties with all its neighboring counties from across the state border. We perform this pairing for each of the 28 regulatory changes and create a stacked panel aligned with respect to the respective treatment years, with an event window spanning three years before and after each adoption. We include only clean controls in the

[2] The high incidence of unpaid medical bills (accounts receivable) and bad debt in the hospital industry are commonly attributed to several factors, including limited ex ante price transparency, uncertainty regarding the cost of care at the time of patients' admission, the complexity and opacity of medical billing, and reliance on reimbursements from insurers and third-party payers that could be subject to delays, disputes, and denial risks after the care has been rendered.

[3] Constructing a consistent time series for earlier years is infeasible due to the substantial redesign of the cost reports by Centers for Medicare & Medicaid Services (CMS) in 2010.

stacked panel by dropping the years in which control states (and counties) are within their respective post-treatment window, if any. In essence, we compare outcomes in hospitals at the border of states enacting tighter debt collection regulations with those from neighboring counties across state border where there was no change in such laws using two-way fixed effects estimators (TWFE).

Combining contiguous county comparison with stacked approach offers several key advantages. First, since the treated and control counties are contiguous, they are unlikely to experience broadly different local shocks. Second, stacking the events allows us to inclusion event×county-pair×hospital fixed effects (FE), which flexibly account for any time-varying changes across events experienced by the hospitals, counties, and states. This implies that we exploit only the variations within hospitals located within a pair of contiguous counties over a single six-year event window, making our estimates immune to the alternative explanations, such as changes in state laws, demographics, or socio-economic changes. Third, the stacked panel also permits inclusion of event×county-pair×relative-year FE, which flexibly eliminates the effects of geographically varying year-specific shocks by allowing a separate time trend for each county pair across each event. Finally, the stacking approach avoids the negative weighting issue of TWFE (Cengiz et al. (2019)).

We examine this DiD design for two essential features. First, regarding the parallel trends assumption, we find no evidence of diverging pre-trends between treated and control groups across a range of hospitals characteristics, such as size, operating margins, account receivables, and liability. Second, regarding the relevance of the treatment event, county-level regressions show that the growth rate of establishments and wages in the debt collection industry declined after the adoption of stricter laws. Similarly, county-level establishment size, wages, and employment in the healthcare industry also declined, indicating its reliance on debt collection. In contrast, no such decline was observed in non-healthcare industries, suggesting that the adverse effects on the healthcare sector were driven by debt collection restriction laws rather than some

other contemporaneous state laws, as these laws would likely have also affected the non-healthcare industries.

We begin our main empirical analysis by examining the impact on financial health of hospitals using Healthcare Provider Cost Reporting Information System (HCRIS) data made available by Rand Corporation. Hospital-level DiD regressions suggest that treated hospitals experienced 9.4% increase in account receivables and 8.6% rise in unreimbursed cost of care relative to control hospitals following the adoption of stricter debt collection regulations by the state. These findings are consistent with reduced recovery on unpaid medical bills.[4] Additionally, in line with hospitals anticipating declines in the recovery of unpaid medical bills, they increased the amount set aside for allowance for uncollectible receivables as the ratio of total revenues by 12%. Finally, both the ratio of interest expense to revenues and total liability to total assets increased by 18.9% and 8.8%, indicating rising financial liabilities.

Before examining other outcomes, we consider potential spillovers in our geographic DiD design, where neighboring counties serve as controls. Spillovers could arise if patients from control counties sought care in treated counties to benefit from stricter debt collection protections. However, several institutional features of the healthcare system—most notably insurance requirements, referral-based access, and the importance of continuity of care—make cross-state substitution for planned care impractical and uncommon.[5] In addition, because debt collection laws apply to the debtor's residence, hospitals in treated states can still use debt collectors from the control states, where the laws remain unchanged. Accordingly, we find no increase

[4] Note that this reduction does not stem from weaker creditor rights. It arises from restrictions on informal predatory collection practices employed by the collection industry, with whom neither patients nor hospitals have a credit contract prior to the provision of care. Had such a contractual relationship existed, the laws would likely have exerted little impact on hospitals, since the contracting parties could have negotiated the terms to account for stricter collection restrictions—though such adjustments would still have faced limits of incomplete contracting.

[5] For example, many insurance plans restrict access to out-of-state hospitals for non-emergency care. Medicaid and Medicare Advantage health maintenance organization (HMO) plans typically limit planned care to in-state providers, while preferred provider organization (PPO) plans that allow out-of-state access are substantially more expensive. Insurers' pre-certification requirements for planned treatments further limit cross-state care. More generally, healthcare delivery relies on referral networks, established provider relationships, and continuity of medical records, all of which make cross-state provider switching uncommon.

in patient volume at treated hospitals relative to control hospitals. Finally, we test for spillovers using a DiD design based on a normalized outcome difference, defined as the difference between a hospital's outcome and the mean outcome of interior hospitals within the same state, scaled by the standard deviation of the latter. We find no change in this measure across a range of outcomes, indicating an absence of spillover effects.

Another important concern is that treated and control counties may differ in the number of hospitals, potentially generating differential trends. While we find no evidence of pre-trends, we further address this concern by re-estimating our main specifications on subsamples of county pairs in which hospital county in both treated and control counties exceed different thresholds. The estimated effects remain stable across these subsamples, alleviating concerns that pre-existing differences in hospital numbers drive our results.

Three sets of results further link the adverse effects we document to the enactment of stricter third-party debt collection laws. First, treatment effects are stronger for hospitals in states with an above-median number of debt collectors per healthcare establishment, proxying for greater reliance on third-party collectors. Second, using hospital job postings, we find that the share of postings for "collection representatives" increased by 1.8 percentage points, nearly 28% relative to the pre-event-year mean, in treated hospitals compared to controls. Third, using CFPB complaint data, we show that while the growth rate of total complaints against debt collectors declined by 10 percentage points, complaints related to healthcare firms rose by 23 percentage points, whereas complaints related to non-healthcare firms declined by 11 percentage points.

We next examine the effect of tighter restrictions on hospital financial performance. Patient volume remained unchanged, but net patient revenues at treated hospitals declined by 2%, suggesting that hospitals likely lowered charges to increase the probability of payment. Treated hospitals also experienced declines in profit and operating margins of 0.9 to 1 percentage points, reduced overall employment by 2%, wages by 4%, and core medical professionals (physicians and registered nurses) by 7%, leading to a seven-percentage-point decline in core medical professionals per bed-day.

We also observe cuts in care capacity. Treated hospitals reduced overall beds and ICU beds by 1% and 4%, respectively, decreased the number of healthcare subfacilities by 4%, and lowered IT investment (as a share of net fixed assets) by 0.84 percentage points. Using hospital-level DRG data from CMS, we further find a decline in availability of medical procedures. Within the universe of procedures provided by the sample hospitals, the share offered by a typical treated hospital declined by 0.8 percentage points relative to the control group, suggesting adverse implications for access to care.

Regarding impacts on prices of healthcare services, we find no significant changes in average prices for inpatient procedures, while treated hospitals raised charges for outpatient services (the billed amount, which are often inflated) on average by 5% relative to the control. However, the actual increase in payments received by hospitals was only 0.4% across all sources, driven by a slightly higher 0.6% increase in Medicare reimbursements, resulting in a 0.9 percentage point decline in the payment-to-charge ratio. Uninsured and out-of-network privately insured patients bear the full burden of these increased charges, while the impact on insured patients is limited to indirect increases in deductibles and premiums.

We also find evidence suggesting that hospitals engaged in upcoding inpatient treatment procedures following the implementation of tighter debt collection restrictions. While total inpatient numbers remained unchanged, treated hospitals saw a 2.7% reduction in patients billed for low-cost procedures (defined as those below the $25^{\text{th}}$ percentile of procedure costs, annually) and a 3.2% increase in patients billed for high-cost procedures (defined as those above the $75^{\text{th}}$ percentile, annually). Because it is unlikely that the laws affected the distribution of healthcare needs within such a short three-year period, and given patients' limited knowledge and control over diagnosis and medical billing, these patterns are consistent with hospitals upcoding inpatient procedures to maximize reimbursements in response to the policy change.

Standard mortality-based quality-of-care metrics also deteriorated at treated hospitals following the law changes. Using Hospital Compare data, we find that mortality

rates for both pneumonia and acute myocardial infarction increased in treated hospitals relative to controls. In addition, treated hospitals significantly reduced charity care provided to uninsured patients: service charges, cost of care, and uncompensated costs associated with charity care all declined compared to control hospitals, while charity care for insured patients remained unchanged. Together, these patterns suggest that hospitals responded to debt collection restrictions by cutting charity care, particularly for the uninsured population.

Could debt collection restriction laws have such substantial impacts on hospitals? Although our data do not allow for a direct estimate of the decline in debt collections on unpaid medical bills following the policy change, a back-of-the-envelope calculation offers useful insights. Using the increase in the ratio of allowances for uncollectible receivables to revenues, and adjusting for revenue changes, we estimate that a typical treated hospital provisioned for a $2.4 million decline in collections, equivalent to almost 28% of net income. These magnitudes emphasize hospitals' significant reliance on debt collectors. First, debt collectors recover nearly 50% of what hospitals typically earn, collecting approximately 11 cents per dollar of debt face value (Hynes (2008), p. 19), compared to 23 cents per dollar of billed charges (sticker price) reimbursed by Medicare. Second, hospitals are the largest sector selling debt to collectors, with a share four times greater than that of financial lenders. Given that these laws cause financial lenders to reduce retail credit (Fedaseyeu (2020), Fonseca (2023), Romeo and Sandler (2021)), it is unsurprising that they exert a significant impact on hospitals as well.

**Contribution and Related Literature:** To the best of our knowledge, this is the first study to examine the impact of debt collection restriction on hospitals—a sector that is central to healthcare delivery. Hospitals have unique characteristics that set them apart: the cost of services is often uncertain at the time of admission, they cannot geographically diversify their patient base, and as discussed previously, they rely heavily on third-party debt collection agencies for recovery of unpaid unsecured medical debt.

Our findings highlight that consumer financial protection laws can have significant effects on non-financial sectors. Specifically, we show that stricter third-party debt collection regulations affect hospitals through the informal supply chain of defaulted consumer debt. We argue that these regulations reduce the recoverability of hospitals earned revenues, representing a persistent financial shock to which hospitals respond in ways that deteriorate employment, care capacity and quality, investment, the availability of treatment procedures, and access to healthcare for uninsured populations.

Through these findings, we contribute to the emerging literature at the intersection of healthcare and finance. Prior research in this area has typically examined the effects of shocks originating from financial markets—that tend to be non-permanent—on hospitals, with the focus in many cases on specific organizational forms, such as non-profit institutions, or on narrow quality-of-care outcomes. For example, Adelino, Lewellen, and Sundaram (2015) examine how returns on financial securities influence investment decisions of nonprofit hospitals; Aghamolla et al. (2024) study the effects of attenuated bank credit—caused by supervisory stress testing at 26 banks—to 505 borrower hospitals; and Adelino, Lewellen, and McCartney (2022) analyze the impact of hospitals' lost endowment during the financial crisis on treatment quality for heart attack and C-section patients.[6] Building on these studies, we focus on a regulatory change that directly and permanently reshapes the financial relationship between hospitals and their patients by reducing the recoverability of their earned revenues. Through this unique context, we highlight a trade-off between consumer financial protection laws and access to healthcare arising from the informal supply chain that links defaulted consumers to hospitals through third-party debt collection agencies. Our results on the reduction in availability of treatment procedures and the decline in charity care for uninsured patients highlight the unintended implications of stricter debt collection laws for access to healthcare.

[6] Other notable studies on hospitals unrelated to financial shocks include: the differential effects of telehealth adoption on urban and rural hospitals (Cornaggia, Li, and Ye (2024)); the impact of private equity investments on hospital operations (Gao, Kim, and Sevilir (2025), Richards, Shi, and Whaley (2024), Zeller (2023)); and the effects of financial liquidity disclosure mandates (Bourveau et al. (2024)).

This paper also speaks to a nascent but growing literature on consumer debt collection laws. Prior literature has highlighted their implications on access to credit (Fedaseyeu (2020), Fonseca (2023), Romeo and Sandler (2021)). Relative to these studies, we make two distinct contributions. First, we are the first to show that the effect of these consumer financial protection laws is not limited to financial institutions but rather extends to hospitals—a non-financial industry—due to the informal supply chain of defaulted consumer credit. This is however not surprising, since the share of non-financial industry is significantly larger than financial industry, as discussed previously. Second, we also contribute a hand-collected dataset on state-level debt collection restriction laws from 2010 to 2022, extending previous academic efforts that compiled this data until 2015.

The rest of this paper is organized as follows. Section I provides institutional information, and Section II describes research design and data. Section IV discusses results and Section VI concludes.

## I. Institutional Details

### *A. Debt Collection Industry Regulations*

Debt collection practices in the US are regulated federally under the Fair Debt Collection Practices Act of 1977 (FDCPA). While FDCPA sets a baseline for consumer protection against unfair, deceptive, or abusive actions by third-party debt collectors, it allows states the flexibility to enact their own laws to provide more stringent protection to state residents. By 2022, forty-three states had adopted legislation to further regulate third-party debt collection. These restrictions include mandatory licensing requirements, increased penalties for violations, and classification of certain offenses as criminal.

To systematically collect information on state laws governing third-party debt collection, we followed a two-step procedure. First, we relied on the dataset compiled by Fedaseyeu (2020), which covers the laws through 2015. For each state, we verified whether a debt collection law was in effect from 2010 onward, then manually examined the statutory text available on the Justia platform for each year through

2022. Whenever we observed a change in the regulatory text, we assessed whether it represented a substantive tightening of the law and recorded the year of tightening as an event. Through this manual examination, we ensured to exclude clerical updates and technical corrections to the statutory text. Second, we drew on the 2023 compilation of state debt collection laws by (LexisNexis (2023)) and manually back-traced all laws active in 2023 to 2010, applying the same procedure to identify law changes.

Through this forward- and backward-tracing strategy we identified 28 instances of regulatory tightening across 17 states between 2010 and 2022. The changes identified through this process include Illinois increasing fines for unlicensed practices from $5000 to $10,000 in 2015; Iowa enacting Debt Collection Practices Act in the same year, which codified the list of prohibited practices for debt collection agencies, defined penalties and granted state residents the private right of action; and Oregon introducing additional licensing, pleading, and substantive requirements for debt buyers in 2018.

### *B. Conceptual Framework: Credit Market Efficiency vs Consumer Protection*

As discussed in the introduction, regulating the third-party debt collection industry involves balancing a trade-off between efficiency in unsecured credit markets and consumer protection against abusive collection practices. The efficiency rationale for third-party debt collection rests on several benefits. First, outsourcing collections to specialized firms often provides cost advantages to creditors relative to in-house collection. These advantages arise from economies of scale, as such firms pool collection costs across large number of consumers, and from their specialized expertise—such as generating new information on consumers' ability and willingness to pay through communication and technology (Fedaseyeu and Hunt (2018)). These benefits are especially valuable for non-financial creditors, such as hospitals, whose core competency lies in healthcare provision rather than debt recovery. Second, outsourcing collections shields hospitals from reputational harm, particularly when recovery efforts are not conducted in their own name. Third, using these agencies allows hospitals to

hedge potential litigation risks that could arise if excessively aggressive practices are employed against consumers.

On the other side of the trade-off lies the protection of consumers from abusive collection practices. Such practices impose significant non-pecuniary disutility in the form of harassment through repeated and excessive contacts, the use of abusive language, and reputational damage caused by these agencies contacting employers and family members. More importantly, the abusive collection practices could also have serious financial consequences, most notably through the practice known as the "rocket docket." In these proceedings, collection agencies obtain default judgments in "judgeless courtrooms" against thousands of consumers, thereby gaining the legal authority to garnish wages and bank accounts (Stifler (2017)). Collectors exploit the fact that defaulted consumers typically lack the resources to secure legal representation, allowing these proceedings to move forward with minimal judicial scrutiny, if any.

Our results indicate that stricter third-party debt collection laws adversely affect hospitals, prompting them to adopt a range of measures, including more aggressive in-house collection practices, upcoding of inpatient procedures, and reductions in employment, care capacity, investments, and charity care for uninsured patients.

## II. Empirical Research Design

### *A. Empirical Strategy*

We exploit the variation in state-level regulations that tightens debt collection practices over time to estimate its effect on hospitals. Estimating a state-level staggered difference-in-differences (DiD) design comparing outcomes before and after such law changes across states faces two key identification challenges. First, the staggered timing of restrictions raises concerns about treatment-effect heterogeneity across events and issues of bad comparisons (Goodman-Bacon (2021)). Second, hospitals in states tightening debt collection laws may systematically differ from those in states without such changes.

In an effort to address these concerns, we combine two complementary empirical approaches. Specifically, we implement a stacked DiD strategy following Cengiz et al. (2019) and augment it with the paired-bordering county comparison design widely used in empirical labor studies, which is motivated by the approach first used in **card1994** and developed later by Huang (2008) and Dube, Lester, and Reich (2010), among others. First, while stacked designs do not perfectly weight and sum event-specific treatment effects, they ensure that there are no problematic comparisons and thus avoid the associated issues such as negative weights in TWFE (Goodman-Bacon (2021)).

Second, our identification strategy compares hospitals located in contiguous counties on either side of a state border, leveraging only the variation in debt collection laws of one state while holding local geography and broader economic conditions relatively constant. The geographic proximity of the border counties ensures that the treated and control units being compared are not subject to vastly different state policies or local socioeconomic trends. Moreover, in Section IV, we show that the treated and control groups exhibit no diverging pre-trends in a range of hospital outcomes and county characteristics, alleviating concerns about potential violations of the parallel trends assumption.

As mentioned previously, we identify 28 events of regulation tightening across 17 states between 2010 and 2022. The calendar year in which a given regulation is adopted is an "event" year. The counties located along the border of a state enacting the law constitute the treated counties for a given event. All counties that share border with the treated counties but are located across the state lines are potential control counties. From these, we select the "clean" control counties—those located in such states that itself did not enact a tighter debt collection regulation over a seven-year event window (three years before and after the event year). For states with multiple treatment events during the sample period, we exclude subsequent events that occur within six years of the initial one. Applying these criteria gives us 20 distinct law changes across 16 states over the sample period.

Having identified the treated and clean control counties, we pair each treated counties repeatedly with control counties, allowing each treated county to appear as many times in the sample as the number of neighboring clean control counties.[7] We then create a seven-year panel around the treated-control pairs corresponding to each event and stack these event-specific datasets by aligning them with respect to the event year. This gives us the stacked panel of paired treated and control counties.

We merge the county panel with hospital data. Naturally, all hospitals located in treated counties comprise the treatment group, and those in control counties comprise the control group. We exclude government-owned hospitals, as their operations are directly supported by government financial assistance and are subject to different incentive structures than non-governmental hospitals. The baseline sample covers 16,230 hospital-year observations from 574 treated hospitals and 655 control hospitals, located in 178 treated and 205 control counties, respectively. Panel A of Figure 1 shows all treated and control counties on the map of contiguous US. Panel B indicates the mean number of hospitals over the sample period in these counties.

For our main specification, we utilize the following hospital-level stacked DiD regression to explore the effect of debt collection restrictions on hospital outcomes:

$$Y_{ickp\tau} = \alpha + \beta \times \text{Treat}_{ickp} \times \text{Post}_{kp\tau} + \text{Controls} + \gamma_{ickp} + \eta_{kp\tau} + \epsilon_{ickp\tau}, \tag{1}$$

where $Y_{ickp\tau}$ is the outcome variable of hospital $i$, located in county $c$ within county-pair $p$, in relative year $\tau$ with respect to event $k$. Outcomes include measures of hospital financial, operational, and care quality information. $Treat_{ickp}$ equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. $Post_{kp\tau}$ is one for $\tau \geq 0$—i.e., the year of treatment and the subsequent three years—in event $k$ and zero otherwise. This definition implies that $\tau = 0$ represents the year of the regulation change, and $\tau = -1$ is the year prior

[7] In 2013, California introduced a policy requiring debt collectors to obtain the original debt agreement before attempting collection and granted consumers a private right of action. The corresponding event window is from 2010 to 2016. As an example, the treated county of San Bernardino borders three adjacent control counties across state lines: La Paz and Mohave in Arizona, and Clark in Nevada, resulting in three county pairs. To avoid overlap between closely timed events, we exclude California's 2015 regulatory change from the analysis, as it falls within the event window of the 2013 policy and cannot be separately identified.

to treatment. $\beta$ is the coefficient of interest capturing the average change in outcomes of hospitals before and after the event in the treated counties relative to the control. *Controls* vary across specifications and consist of a host of hospital and county-level socioeconomic characteristics. $\gamma_{ickp}$ and $\eta_{kp\tau}$ denote event×county-pair×hospital and event×county-pair×relative year fixed effects, respectively.

To analyze outcomes measured at county and state levels, we adapt the above equation and discuss in Section IV, where we present the results.

*B. Examining Pre-Trends*

To examine pre-trends in the hospital outcomes discussed above, we estimate the following the dynamic version of equation (1) as follows:

$$Y_{ickp\tau} = \alpha + \sum_{\tau=-3,\, \tau\neq -1}^{3} \beta_\tau \text{Treat}_{ickp} \times \mathbb{1}(\text{R.Year} = \tau) + \text{Controls} + \gamma_{ickp} + \eta_{kp\tau} + \epsilon_{ickp\tau}, \quad (2)$$

where $\mathbb{1}(\text{R.Year} = \tau)$ is equal to one when the relative year to the event year is $\tau$. Panels A to H of Figure 2 plot $\beta_\tau$s and the corresponding 95% confidence intervals based on standard errors clustered by state for eight hospital-level variables: allowance for uncollectible receivables to revenues, accounts receivable (in days), total liability to total assets, operating margin, and the natural logarithm of net patient revenues, employment, wages, and number of ICU beds. We do not observe any statistical difference or diverging trends between the treated and control group prior to the event for these key hospital outcomes, indicating an absence of pre-trends. Overall, the absence of diverging trends in the key hospital and county characteristics does not raise concerns about a violation of parallel-tends assumption in our DiD design.

## III. Data and Summary Statistics

We utilize data from a variety of sources. The primary data is the state-level debt collection regulation changes from 2010 to 2022, which we described earlier. Our second main data is about hospitals. We source the data from the Healthcare Cost Report Information System (HCRIS) maintained by CMS, which contains detailed information on Medicare-certified hospitals. We use the version processed and standardized by the RAND Corporation, which consolidates approximately 2,000

variables from over 16,000 raw items in the original HCRIS reports. The Rand hospital data offer a consistent time series of variables capturing comprehensive information on hospital characteristics, utilization, costs, charges, wages, employment, ownership type (for-profit vs. non-profit), operational information such as total occupied bed days, inpatient discharges, and financial information such as detailed assets, liabilities, net income, and costs of charity care.

To measure hospital care quality, we use risk-standardized 30-day mortality data from the CMS Hospital Compare program. Mortality is defined as patient death within 30 days of discharge from a hospital admission, including deaths occurring during hospitalization. We focus on mortality rates for three key acute conditions—acute myocardial infarction (AMI or heart attack), heart failure, and pneumonia.

For diagnosis-related analyses, we use the Medicare Inpatient Hospitals by Provider and Service dataset from CMS. Available from 2013 onward, this dataset reports service utilization, total and Medicare-specific payments, and submitted charges at the hospital–MS-DRG level. MS-DRGs are a classification system that groups clinically similar conditions and the associated procedures performed during a hospital stay. Each hospital discharge is assigned to one MS-DRG. This dataset aggregates the patient-level data in a given year to hospital-by-MS-DRG level. This dataset is particularly well suited for our analysis because it consistently captures the actual services and treatment procedures performed by hospitals, allowing us to assess the changes in the scope and menu of available treatment procedures.

Finally, we complement our analysis with a range of county-level socio-economic indicators drawn from publicly available sources. Population counts come from the Census Bureau's annual county resident population estimates. Information on establishments and payroll at the county–industry level is drawn from the County Business Patterns dataset. To capture local labor market conditions, we use the number of active employment and rate of unemployment from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS). In addition, we incorporate county-level gross domestic product (GDP) data from the Bureau of Economic Analysis.

Table I shows the summary statistics for the key variables. Panel A shows the hospital level variables from HCRIS data. The mean profit margin and operating margin are 3.26% and 0.76%, respectively. On average, the natural logarithm of the number of inpatients is 7.62, and the natural logarithm of hospital beds is 4.39. A typical hospital in our sample takes approximately 42 days to collect payments. Panel B presents county-level data on healthcare establishment size, unemployment rate, employment, GDP, and population.

[Insert Table I Here]

## IV. Results

### *A. Debt Collection Restrictions Laws and the Collection Industry*

Before examining the link between stricter debt collection regulations and hospitals, we first set out to assess the validity of the natural experiment by assessing whether enactment of such laws constrains the debt collection industry. Using NAICS codes 561440 and 561491 in county business patterns (CBP) data, we identify the establishments operating in this industry and estimate effects of the laws on this industry at state-level using the following equation:[8]

$$Y_{skp\tau} = \alpha + \beta\, \text{Treat}_{sk} \times \text{Post}_{kp\tau} + \text{Controls} + \gamma_{skp} + \eta_{kp\tau} + \epsilon_{skp\tau}. \tag{3}$$

Here $Y_{skp\tau}$ denotes a state-aggregate measure of debt collection industry in state $s$, within state-pair $p$, in relative year $\tau$ of event $k$. $Treat_{skp}$ equals one if a state $s$ tightened its debt collection regulations during event $k$ and zero otherwise. $Post_{kp\tau}$ is defined separately for each event $k$ as follows: it is one if $0 \leq \tau \leq 3$—that is, for the year of treatment and the subsequent three years—and zero otherwise. $\beta$ is the coefficient of interest capturing the average change in outcome variable before and after the event in the treated states relative to the control. $\gamma_{skp}$ and $\eta_{kp\tau}$ denote event×state-pair

[8] This regression is conducted at the state level because (i) the industry is regulated at that level, allowing debt collectors to operate anywhere within the state as long as they have physical presence in the state; and (ii) many border counties do not have any registered establishments in this industry, precluding a county-level estimation. We employ more granular county- and hospital-level regressions for the empirical analysis in later sections.

and event×state-pair×relative year fixed effects, respectively. Controls are state-level unemployment rate and the natural logarithms of employment, GDP, and population.

Table B.1 in Online Appendix presents the results from regressing the growth rate of the number of establishments (column (1)), state-aggregated total wages (column (2)), average wages per employee (column (3)), and average wages per establishment (column (4)).[9] We see that all four outcomes declined in the treated states relative to the control, indicating that the restriction laws constrained the growth of the collection industry and validating the natural experiment.

### *B. Debt Collection Restriction Laws and Healthcare Industry*

Our examination of the effects of the restriction laws on hospitals proceeds in two steps. We first use county-level measures to examine the effects on the whole healthcare industry, which includes hospitals as well as other healthcare establishments. This provides context to the hospital-level findings by generalizing the results to the broader industry. More importantly, this also allows us to conduct placebo tests focusing on the effects on non-healthcare industries, strengthening the causal interpretation of the granular hospital-specific results obtained using more stringent specifications.

We identify the establishments in healthcare industry in CBP data using 3-digit NAICS codes 621 and 622 and use the following county-level regression equation:

$$Y_{ckp\tau} = \alpha + \beta\, \text{Treat}_{ckp} \times \text{Post}_{kp\tau} + \text{Controls} + \gamma_{ckp} + \eta_{kp\tau} + \epsilon_{ckp\tau}, \tag{4}$$

where $Y_{ckp\tau}$ denotes the healthcare industry outcome in county $c$, within county-pair $p$, in relative year $\tau$ of event $k$. $Treat_{ckp}$, $Post_{kp\tau}$, $Controls$, and the coefficient of interest $\beta$ are the county-level analogues of the corresponding state-level definitions in equation (3). $\gamma_{ckp}$ and $\eta_{kp\tau}$ denote event×county-pair×county and event×county-pair×relative year fixed effects, respectively.

[Insert Table II Here]

Table II reports the results. In column (1), we see that the average establishment size in healthcare industry, defined as the average number of employees per establishment,

[9] The annual payroll amounts recorded in the CBP data is referred here as wages, for brevity.

declined by 6% in treated counties relative to the control. Regarding wages, we find in columns (2) to (4) that total wages, average wages per employee, and average wages per establishment declined by eight, three, and ten percent, respectively. Collectively, these findings indicate that the stricter debt collection laws affected the employment and wages in healthcare industry.

[Insert Table B.2 Here]

Table B.2 in Online Appendix presents regression results for the same outcomes for establishments in non-healthcare industries. Specifically, columns (1) to (4) present the results for grocery, restaurants, and fitness industry; (5) to (8) for all non-tradable industries excluding healthcare; and (9) to (12) for all industries excluding healthcare. The definition for non-tradable industries is from Mian and Sufi (2014). We see that none of the coefficients are significant, indicating that the laws did not lead to declines in employment or wages in non-healthcare industries.

The lack of adverse effects in the first two groups of industries is particularly notable, as they, like healthcare, belong to the non-tradable sector, where both production and consumption are local. Even though these non-healthcare industries operate in states that enacted the restriction laws, they are placebo industries rather than treated ones, because they typically do not generate unpaid consumer bills that subsequently become debt subject to collection. The absence of effects on these placebo industries also suggests that the adverse effects on the healthcare industry discussed earlier were not driven by contemporaneous enactment of other state laws, as such laws would likely have affected these industries as well.

### *C. Hospital-Level Outcomes*

Building on the previous results, which show that stricter debt collection restrictions negatively affected the healthcare sector while leaving other local industries unaffected, we now use hospital-level data and more rigorous specifications to examine the effects on hospitals and explore the underlying mechanisms.

### *C.1. Rising Bad-Debt Concerns and Financial Obligations of Hospitals*

We begin by examining the effect on hospitals' ability to collect unpaid bills from patients. We estimate regression equation (1) for accounts receivable collection periods (in days) using Rand Hospital data. The result in column (1) of Table III shows that, following the adoption of stricter debt collection laws, the receivable period increased by 3.6 days relative to control hospitals in neighboring counties, a 9.4% rise over the pre-event-year ($\tau = -1$) mean of 38 days for treated hospitals. This suggests that the laws weakened hospitals' ability to collect earned revenues.

While the increase in receivables is statistically significant, hospitals could offset its impact on their finances by securing higher reimbursements from insurance payers. To explore this, we next examine the effect on unreimbursed costs, which represents the portion of expenses incurred for providing care to patients covered by public insurance programs but not reimbursed. We obtain total unreimbursed costs for Medicaid, the State Children's Health Insurance Program (SCHIP), and state and local indigent care programs from Rand Hospital data.[10] The outcome variable is the ratio of total unreimbursed costs to gross patient revenues. The coefficient in column (2) suggests that affected hospitals experienced an increase of 0.15 percentage points in this ratio relative to the control hospitals, equivalent to an 8.6% increase over pre-event-year mean of 1.74 across the treated hospitals. This implies that affected hospitals did not offset the increase in receivable by securing increased reimbursements from public insurance programs.

[Insert Table III Here]

Facing both rising receivables and unreimbursed costs, standard accounting practices may require hospitals to increase their allowances for uncollectible receivables. We assess this by regressing the ratio of allowances for uncollectible receivables to gross patient revenues. The result, presented in column (3), shows that the ratio increased

[10] Unreimbursed cost is distinct from charity care, which refers to free or discounted healthcare to people who cannot afford to pay for their medical bills, regardless of insurance status. This cost pertains to insured patients for whom payments and reimbursements from public insurance programs are inadequate. It is calculated as the difference between the total cost of providing care to patients in these programs and the total payments and reimbursements received. Note that this cost does not cover information on unreimbursed costs from private insurance programs.

by 0.69 percentage points for affected hospitals relative to the control, equivalent to an increase of 12% relative to the pre-event-year mean of 5.75 for treated hospitals. This increase is consistent with hospitals anticipating a rise in bad debt following the regulations. They likely expected this due to both intensive-margin effects, such as selling unpaid bills at higher discounts to collectors, and extensive-margin effects, including an inability to sell some of the debts or longer delays in selling them. However, the hospital cost report data are not sufficiently detailed to enable separate estimation of these two effects.

The increase in the ratio of uncollectible receivables to gross revenues allows us to estimate the reduction in dollar amounts hospitals expected from the policy change. A 12% increase in this ratio, coupled with a 2% reduction in patient revenues (Table III, column (1)), implies a 9.8% increase in receivables. Given that treated hospitals held a total of $17 billion in receivables in the pre-treatment year, this increase suggests that the laws reduced collections by $1.66 billion in total, or $2.4 million per hospital, which is equivalent to nearly 28% of net income.

With several financial metrics deteriorating, we next examine the effects on interest burden (interest expenses to total revenues) and liability (total liability to total assets) for affected hospitals. Relative to control hospitals, the interest burden of affected hospitals increased by 7 basis points (bps) in column (4), and liability rose by 4.4 percentage points in column (5). These increases are notable, with the two metrics rising by 18.9% and 8.8%, respectively, relative to the pre-event-year means of 0.37 and 50.07% across treated hospitals.

To further examine the effect of the restriction laws on hospital borrowings, we use an alternate empirical strategy. We link hospitals to Dealscan borrowers and loan-level information using data from Aghamolla et al. (2024). Since a Dealscan borrower often represents multiple hospitals operating across several states, we calculate annual borrower-level exposure to debt collection restriction laws, *DCR Exposure*, as the sum of the state-level debt collection restriction index weighted by the net fixed assets of

hospitals operating in each state.[11] We then use the following regression equation:

$$Y_{lit} = \alpha + \beta \times \text{DCR Exposure}_{it} + \text{Controls} + \gamma_i + \eta_t + \theta_s + \epsilon_{lit}. \quad (5)$$

Here the outcome is loan $l$ of a Dealscan hospital borrower $i$ in calendar year $t$. $\gamma_i$, $\eta_t$, and $\theta_s$ respectively denote fixed effects for Dealscan borrower, calendar year, and the state of the borrower as recorded in Dealscan. The sample period spans from 2010 to 2020, the last year for which loan data are available in the legacy Dealscan dataset. Table B.3 in Online Appendix presents the results. Column (1) shows the effect on loan spreads and fees, while column (2) reports the effect on the natural logarithm of outstanding loan amounts. A unit increase in exposure to these regulations is associated with an increase in spreads and fees of about 85 bps and a decline in outstanding loan amounts of about 20% ($e^{-0.22} - 1$). In terms of magnitudes, the increase in loan spreads is at least 15% larger than that induced by bank stress testing (68–74 bps), while the decline in loan amounts is 33% smaller (Aghamolla et al. (2024)). Overall, these findings suggest that hospitals borrowings from syndicated loan market was adversely affected following the adoption of the restriction laws.

**Robustness of Baseline Results:** Before examining other outcomes, we undertake a series of tests motivated by the paired-county literature to assess the robustness of baseline results. First, a common concern with a paired-county design using contiguous counties is the potential for spillover effects between treated and control areas. Specifically, regression estimates could be biased if population movement across state lines occurs in search of healthcare services. While several institutional features of healthcare services limit individuals' ability to easily switch to out-of-state providers, as discussed in the introduction (including footnote 5), we now directly assess whether the restriction laws led to spillovers. Specifically, we examine the normalized difference of an outcome, defined as the difference in the outcome of a hospital in a bordering county and the mean outcome for same-state hospitals in inner counties, scaled by the standard deviation of the outcome across inner hospitals. If the event leads to significant spillovers, this bordering-to-inner difference would change, as

[11] Our debt collection index is a continuation of that in Fedaseyeu (2020) for our sample period.

spillovers are likely to affect bordering counties more than inner counties. The results, presented in Table B.4 in the Online Appendix, show no statistically significant change in the normalized difference for any of the outcomes, consistent with the absence of significant spillovers.

We conduct a second test to address the concern that county pairs with large geographic areas may experience differing local economic trends despite bordering each other. Following the approach in literature (Dube, Lester, and Reich (2016)), we estimate the effect only within pairs whose population centroids are within 75 kilometers (46 miles) of each other. The estimates in this sample for each of the baseline outcomes remain essentially the same, as shown in Online Appendix Table B.5.

A third concern arises from pre-existing differences in the number of hospitals operating in treated and control counties within each pair. While the DiD setting accounts for level differences in variables across the units being compared, there remains a concern that the counties may be on different trends due to these differences. The absence of pre-trends, as shown in Figure 2, mitigates this concern. Nevertheless, we estimate the effects on each of the baseline outcomes for the subsamples of county pairs where the number of hospitals in both treated and control counties exceeds $n(n = 2,3,4)$ and present the results in Online Appendix Table B.6. We obtain largely similar estimates despite drastic changes in sample sizes, corroborating the robustness of the baseline results.

Overall, the findings collectively suggest that the restrictions on third-party debt collection negatively impacted hospitals' ability to recover receivables, as well as their interest burdens and financial obligations.

**Heterogeneity by Debt Collector Density:** We conduct a heterogeneity test to further support the proposed mechanism: that stricter debt collection laws affect hospitals by reducing the recoverability of unpaid medical bills, thereby impacting their financial positions. If this holds, the effects should be more pronounced for hospitals that rely more heavily on third-party debt collectors. Although a direct measure of reliance is unavailable, we assume that hospitals in areas with higher debt collector density

are more dependent on debt collectors. This assumption is grounded in the fact that healthcare is the largest sector supplying debt to the collection industry, suggesting that more widespread local debt collection reflects greater reliance by local healthcare establishments on these services. To test this, we classify states based on the number of debt collection industry employees per healthcare establishment. Hospitals in states above the sample median in the pre-event-year (i.e., $\tau = -1$) are assumed to be more reliant on debt collection than those below the median, with this classification held fixed for each respective event.

[Insert Table IV Here]

Table IV reports the results of the same five outcomes from baseline analysis now estimated separately for high and low group in odd and even columns respectively. Coefficients in the odd-numbered columns indicate that the enactment of stricter debt collection laws in states with higher ex ante debt collector density led to greater increases in receivable days, unreimbursed costs, allowances, interest burden, and total liabilities (scaled by assets) relative to control states, compared to the relative increase in states with lower ex ante debt collector density (in even-numbered columns).[12] The stronger adverse effects for states with higher ex ante collector density further support the proposed mechanism that the restriction laws affect the hospital finances adversely through their dependence on the collection industry.

### *C.2. Hospital In-House Debt-Collection Efforts*

We now turn to evidence on the underlying mechanism driving the adverse effects on hospitals. Specifically, we aim to demonstrate that these effects were caused by restrictions on third-party debt collection rather than by other contemporaneous factors. If the laws constrained third-party collectors and thereby negatively affected hospitals, one would expect hospitals to respond by expanding their own in-house collection efforts, as the restrictions did not apply to creditors collecting on their own

[12] The opposite treatment effects on receivables and the liability ratio for the low collector-density group in columns (2) and (10) likely reflect successful cost-saving and revenue-maximizing measures taken by hospitals, which offset the adverse impacts of the laws, likely because these hospitals were not deeply reliant on debt collectors to begin with. Later sections examine several margins of adjustments undertaken by hospitals.

behalf. Consistent with this idea, we examine whether hospitals increased hiring of collection specialists and intensified in-house collection practices following the law changes.

To analyze changes in hospital hiring practices, we first merge CMS hospital provider numbers with Lightcast company data using text-matching. We then identify 30 unique 5-digit Standard Occupational Classification (SOC) codes associated with debt collection, detailed in Section A.1 in Online Appendix. Using these codes, we classify hospital job postings into collection-related and non-collection-related positions.[13] We calculate the ratio of collection-related job postings to total job postings at hospital-year level and estimate regression equation (1). Column (1) of Table V shows that the affected hospitals significantly increased their efforts to hire collection specialist relative to the control group, with the ratio rising by 1.78 percentage points—a sizable 28.5% increase over the pre-event-year mean of 6.25 across treated hospitals.

[Insert Table V Here]

To gauge changes in aggressiveness of in-house collection efforts of hospitals, we draw on CFPB's consumer complaint narrative data, available from 2013 onward. The idea is that, because hospitals faced greater difficulty recovering payments through third-party collection agencies due to the restriction laws, they may respond by increasing the aggressiveness of in-house collection efforts, which in turn would generate more consumer complaints related to hospital debt collection. We test this by first classifying consumer complaints as related to hospital care if they contain one or more of 53 keywords associated with hospital care and as related to non-hospital firms otherwise. The keywords are listed in Section A.2 in Online Appendix. Due to limited geographic variation in the complaints data within our sample counties, we examine this at the state level using regression equation (3), with the same set of controls. Columns (2) to (4) of Table V report the results. We find that while the

[13] Here are two examples of debt-collection–related hospital job postings identified using our method. Ascension Health, part of Amita Health, posted a full-time job in Arlington Heights, IL, whose description included: "Locates and notifies responsible parties of delinquent accounts to collect payment." Similarly, Temecula Valley Hospital posted a Patient Access Coordinator position with responsibilities that included payment processing and adherence to cash collection practices.

growth rate of total consumer complaints regarding debt collection declined by 10% after the adoption of third-party debt collection restrictions (Column (1)), complaints against hospitals increased by 23%, whereas those against non-hospital firms declined by 11%. These diverging patterns strongly suggest that, although the restriction laws improved the overall debt collection environment by reducing consumer complaints overall, they likely shifted the abusive practices from third-party agencies to in-house collectors—at least for hospitals, the largest users of third-party collection services.

### *C.3. Financial Performance*

We continue our analysis by now focusing specifically on the effects of the stricter collection laws on financial performance of hospitals. We continue to use regression equation (1). The first outcome of interest is net patient revenues, measured as the natural logarithm of patient charges minus contractual allowances and discounts. Column (1) of Table VI shows that affected hospitals experienced a 2% revenue decline relative to the control group, equivalent to an annual revenue loss of $3.72 million based on the pre-event-year average of $186.52 million for treated hospitals.

This revenue decline could arise either from lower inpatient volume or from higher discounts offered by hospitals in an effort to increase the likelihood of timely payments from patients and to prevent billed revenues from being converted into accounts receivable. To disentangle these channels, we regress the natural logarithm of the number of inpatients and find statistically insignificant coefficient, as reported in column (2). This suggests that the revenue decline was not driven by a reduction in inpatient volume, but likely by larger discounts on patient charges.

The absence of change in inpatient volume has two important implications. One, because the restriction laws do not alter the underlying healthcare needs of the population, patient volume is expected to remain unchanged. Two, it helps rule out the concern that patients in adjacent control counties might have strategically switched to treated states to receive care, default on medical debt, and benefit from stricter protections against debt collection practices. Three factors support this. First, such switching would have increased the patient volume at treated hospitals, which we do

not evidence. Second, institutional provisions in the healthcare industry make strategic changes of providers highly unlikely, as noted in the introduction and footnote 5. Third, debt collection laws apply based on the defaulter's state of residence rather than the state where the debt originated, so collection practices of treated hospitals for out-of-state patients remain unaffected from the tightening of collection laws of their state of operation.

[Insert Table VI Here]

Finally, we investigate hospital profitability. In column (3), we find that affected hospitals' profit margin decreased by 0.95 percentage points, equivalent to a stark decline of almost 48% over pre-event-year mean value of 1.97 for treated hospitals. Similarly, column (4) indicates that operating margin declined by 1 percentage point, while column (5) and (6) show that the declines are robust when net income and operating income are scaled by net fixed assets, instead of revenues.

### *C.4. Employment Decisions*

We next test whether hospitals respond to stricter debt collection laws by adjusting their employment decisions amid deteriorating operational and financial performance. We assess a series of employment-related outcomes using regression equation 1, and report the results in Table VII. Columns (1) and (2) show that the affected hospitals reduced overall employment and core medical workers by 2% and 7% respectively relative to the control group.[14] These percentages correspond to declines of 27 total jobs and 7.3 core medical jobs, based on the respective pre-event-year sample means. Columns (3) and (4) further suggest that both hospital-aggregated wage and average wage per employee declined by 4% and 1% respectively. Given that the number of inpatients did not decline, the cuts in employment indicate a reduction in medical professionals per patient. We confirm this interpretation in column (5), which shows that the ratio of core workers to inpatient bed-days declined by 7%. The decline in this ratio indicates a deterioration in patient care quality. Overall, these findings suggest that hospitals scaled back their core physician workforce more than the patient occupancy. Given

[14] These variables are defined in Section A.3 of Online Appendix.

the crucial role these employees play in healthcare delivery, administrating treatment procedures, and managing medication and other care, these cuts in employment and wages have serious adverse implications for quality of patient care.

[Insert Table VII Here]

### *C.5. Care Capacity and Asset Divestitures*

We now show that hospitals also curtailed back care capacity and engaged in asset divestitures as cost-saving measures following the adoption of stricter laws. We use regression equation (1) and report the results in Table VIII. Columns (1) and (2) show that, relative to the control group, affected hospitals reduced the total number of beds by 1% and functioning ICU beds by 4%, whereas column (3) suggests that the number of non-ICU beds remained unchanged. Taken together, these findings are consistent with hospitals curtailing ICU beds to achieve cost savings, since maintaining ICU capacity entails substantially higher per-day costs due to the need for adequate critical care and medical staff—which, as shown in our previous results, declined—and essential equipment. Along the same lines, we see in column (4) that they reduced investment in information technology infrastructure by 0.84 percentage points relative to control group. Investment in health IT, particularly electronic medical records, not only generates productivity gains through complementary investments by other healthcare providers (Dranove et al. (2014)) but is also highly elastic, making it sensitive to market fluctuations (Richards, Shi, and Whaley (2025)). Consequently, these reductions may limit hospitals' ability to coordinate care delivery and, ultimately, affect care quality.

[Insert Table VIII Here]

For asset divestitures, we estimate changes in the number of hospital sub-providers or sub-units, which are distinct functional divisions located within hospital providing specialized medical and operational services.[15] The coefficient in column (5) suggests

[15] Hospitals sub-units include specialty certified ambulatory surgical centers, renal dialysis providers, inpatient rehabilitation and psychiatric facilities, hospice providers, skilled nursing facilities etc.

that affected hospitals reduced the number of sub-units by 4% relative to control group, indicating a potential narrowing of scope of healthcare delivery.

Lastly, we directly examine changes in scope of care by using data on Diagnosis-Related Group (DRG) procedures performed by hospitals. A DRG classifies patients based on diagnosis similarity, severity of the illness, complications, procedures performed etc. For each hospital, we calculate its unique number of DRGs relative to the total number of unique DRGs performed by all sample hospitals in the same state-year. This measure captures the breadth of treatment options or procedural coverage at a hospital relative to its state peers in the same year. The coefficient in column (6) indicates that affected hospitals' DRG share declined by 0.8 percentage points relative to the control group, corresponding to a 3.5% reduction in coverage. Since debt collection restriction laws are unlikely to directly influence patients' medical needs, the reduction in coverage of procedures likely reflects hospitals' decision to eliminate certain treatment procedures—potentially contributed by the loss of specialized core medical workers, which we documented previously—thereby limiting the menu of care.

### *C.6. Prices of Healthcare Services*

Our analysis thus far has examined the effects of stricter debt collection laws on non-price outcomes. A key margin of adjustment and an important policy question is whether hospitals responded by increasing prices. Given that a substantial share of hospital revenues comes from insurers, who may resist price hikes, the effectiveness of a price-raising strategy is limited. Simultaneously, hospitals may have incentives to reduce prices (say, by offering larger discounts) to encourage timely payments and limit the accumulation of receivables and bad debt, particularly for patient-paid services.[16] Whether hospitals raised or lowered prices is therefore an empirical question. We address this by examining procedure-level prices for all inpatient

[16] While price increases by hospitals adversely affect all patients, the effective impact varies significantly across patient groups. For those covered by public or in-network private insurance, the impact is largely indirect and limited, through higher premiums and deductibles. In contrast, uninsured patients, out-of-network privately insured patients, and workers' compensation insurers bear the full burden of any price increase (Zander et al. (2024)).

and outpatient services rendered by hospitals, as recorded in CMS data. Inpatient services are categorized by Medicare Severity Diagnosis Related Groups (DRGs), while outpatient procedures are classified by Ambulatory Payment Classifications (APCs). The data include 677 unique DRGs and 71 unique APCs, with 577 and 70 unique categories, respectively, in our merged dataset. We expand our hospital-level stacked panel to hospital-by-healthcare-service-level stacked panel and estimate the effect on procedure-level pricing using the following equation:

$$Y_{isckp\tau} = \alpha + \beta \text{Treat}_{isckp} \times \text{Post}_{kp\tau} + \text{Controls} + \gamma_{isckp} + \eta_{kp\tau} + \epsilon_{isckp\tau}. \tag{6}$$

Here $Y_{isckp\tau}$ is a price outcome for hospital $i$ providing healthcare service $s$ (DRG or APC) in relative year $\tau$ of event $k$, where hospital $i$ is located in county $c$ within county pair $p$. The controls are the same as in equation (1). $\gamma_{isckp}$ and $\eta_{kp\tau}$ respectively denote fixed effects for event×county-pair×hospital×service and event×county-pair×relative year. The former fixed effects limit comparisons within hospital-service combinations, addressing potential concerns that certain DRGs dominate the sample, that hospitals may vary in specialization, or that shifts toward higher-cost DRGs could reflect substitution of older, less expensive procedures with newer, costlier ones.

[Insert Table IX Here]

We examine four key price metrics: the natural logarithm of the average charge (what a hospital bill for a given procedure, often inflated), average payment it actually receives for a given procedure from all sources, average payment received from Medicare, and the payment-to-charge ratio. We regress the natural logarithm of these outcomes and present results in Table IX. Columns (1) to (4) show results for inpatient services, while columns (5) to (8) report results for outpatient services. For inpatient services, we observe no significant price increases in treated hospitals relative to the control group. However, for outpatient services, hospitals raised charges by about 5% (column (5)) but could increase the realized payment only by 0.4% across all payers (column (6)), with a marginally larger increase of 0.6% in payment from Medicare (column (7)). As a result, the payment-to-charge ratio for outpatient services declined by 0.9 percentage points (column (8)).

Overall, these findings suggest that hospitals raised prices for outpatient services but were able to capture only about one-tenth of the increased payments on average, with Medicare contributing more than other sources. However, the uninsured and out-of-network privately insured patients bear the full burden of these increased charges.

### *C.7. Upcoding Inpatient Procedures*

Another policy-relevant margin of adjustment for hospitals in response to changes in debt collection restriction policies is upcoding inpatient procedures—that is, billing for procedures at a higher level of complexity than those actually provided or documented. Upcoding-related overpayments not only cost taxpayers but also distort consumer choices and result in welfare loss (Geruso and Layton (2020)). High-cost DRGs billed to Medicare offer hospitals a particularly attractive upcoding opportunity, as they involve complex, resource-intensive procedures. Given that patients have limited influence over the recommended diagnosis and medical billing, and that a significant portion of the revenue from high-cost DRGs is virtually guaranteed by Medicare (except for small deductibles), hospitals have incentives to engage in upcoding.[17]

We therefore test for patterns of upcoding following the adoption of debt collection restriction policies. Our test for upcoding builds on the assumption that the enactment of debt collection restriction laws is unlikely to alter the distribution of healthcare needs towards higher-cost DRGs in a short three-year post-enactment period. Using Medicare Inpatient Hospital data by Provider and Service (available from 2014), we collect all DRGs performed by the sample hospitals. Each year, we classify DRGs into low- and high-cost groups based on submitted charges: DRGs below the 25$^{th}$ percentile are categorized as low-cost, and those above the 75$^{th}$ percentile as high-cost.[18] Therefore, if the share of patients billed for high-cost DRGs increases significantly after the policy change, it may reflect either upcoding by hospitals or the steering of patients towards more complex procedures.

[17] In 2026, the Original Medicare Part A deductible is $1,736 per benefit period, with a copayment of $0 for the first 60 days of inpatient care.

[18] Examples of high-cost DRGs include chimeric antigen receptor (CAR) T-cell therapy and other immunotherapies, while low-cost DRGs include childbirth and ear, nose, and throat diagnoses.

[Insert Table X Here]

We expand the previous hospital-level stacked panel to hospital-by-DRG-level stacked panel and examine the share of Medicare inpatients billed across the two DRG groups using regression equation (6). Table X reports the results. Column (1) shows that, relative to the control group, the affected hospitals did not experience a change in overall inpatient volume (consistent with the previous results in Table VI). However, the number of inpatients billed in low-cost DRGs declined by 2.7% (column (2)), while those billed for high-cost DRGs increased by 3.2% (column (3)) following the policy change. These differential patterns are consistent with hospitals either upcoding inpatient procedures or recommending more complex procedures to patient.[19] While the data do not allow us to disentangle whether these patterns arise from upcoding, shifts in actual diagnoses, or a combination of both, each suggests important policy implications for patient welfare.

### *C.8. Quality of Care: Patient Mortality*

Our final quality of care analysis focuses on the standard health-based mortality measures. Using data from CMS Hospital Compare, we investigate whether mortality associated with the following three common conditions deteriorated at the treated hospitals: pneumonia (PN), acute myocardial infarction (AMI), and heart failure (HF). A practical issue in using these data is that mortality rates are reported using a 36-month rolling window. For example, the rate reported on June 30, 2013, reflects data from July 1, 2010, to June 30, 2013, making a clean separation of pre- and post-event outcomes infeasible in our DiD setting, which spans six years. To address this issue, we construct annual mortality rates as follows: for each pre-period year $t$, we use the rate based on data ending in year $t$; for each post-period year $t$, we use the rate based on data beginning in year $t$. For instance, the pre-period 2013 rate is based on data from July 1, 2010, to June 30, 2013, while the post-period 2013 rate uses data from

[19] Note that these patterns do not imply a crude substitution of low- to high-cost DRGs (e.g., a patient with an ear condition receiving immunotherapy), but rather reflect a shift in the distribution, with a greater proportion of patients being either billed for more complex procedures or assigned more complex diagnoses (and billed appropriately).

July 1, 2013, to June 30, 2016. This scheme ensures that pre-period mortality rates rely solely on pre-event data, while post-period rates rely solely on post-event data.

[Insert Table XI Here]

We regress these mortality rates using equation (1) and present the results in Table XI. Columns (1) and (2) show that mortality rates for pneumonia and heart failure increased in affected hospitals relative to the control group. We do not detect an increase in AMI-related mortality, which may be because deterioration in care quality for AMI does not readily translate into higher mortality rates due to high autocorrelation in outcomes (Aghamolla et al. (2024)). In economic terms, the increases imply additional 136 pneumonia-related deaths and 21 HF-related deaths per year among the 716 affected hospitals in our sample. Taken together, these results reaffirm that care quality at affected hospitals deteriorated following the adoption of stricter debt collection laws.

### *D. Charity Care by Non-profit Hospitals*

A crucial dimension of healthcare provision at non-profit hospitals is charity care. To maintain their tax-exempt status, non-profit hospitals are required to provide free or reduced-cost care to low-income, underinsured, and uninsured patients who cannot afford to pay for healthcare services. However, insufficient charity care spending remains a longstanding policy concern, as how much to allocate to charity care is unregulated and left to hospitals' discretion (Zare, Eisenberg, and Anderson (2022), Jenkins and Ho (2023)).

[Insert Table XII Here]

We test whether non-profit hospitals reduced charity care in response to changes in debt collection restriction policies using equation (1) and report the results in Table XII. Columns (1) to (3) report results for charity care at non-profit hospitals provided to uninsured patients, scaled by their net fixed assets. Compared to control non-profit hospitals, the affected non-profits devoted 1.07 percentage points less of their total charges (service revenues) to charity care and curtailed their estimated and incurred uncompensated costs of providing charity care by 0.36 and 0.32 percentage points,

respectively. These cuts are economically large and non-trivial, as these correspond to a 16.8% reduction in total charity care charges, a 19.3% reduction in estimated cost, and a 17.8% reduction in uncompensated cost, relative to respective pre-event year means across treated non-profit hospitals. In a stark contrast, we do not find any decline in any of these charity care measures for insured patients, as indicated by the coefficients in column (4) to (6).

These findings indicate that hospitals selectively reduced charity care for uninsured patients as a cost-saving strategy following the adoption of stricter debt collection laws. This has important policy and public health implications, as the laws appear to have reduced access to healthcare for the uninsured—ironically, the group vulnerable to incurring unpaid medical debt (CFPB (2022), p.10) and central to the motivation behind the stricter debt collection laws.

## V. Additional Results

### *A. Shifts in Healthcare Utilization*

Our findings thus far reveal two main effects: the broader healthcare sector faced declines in employment and wages (Section IV.B), and hospitals experienced negative financial impacts and deteriorating care quality, which likely harmed the populations reliant on them (Section IV.C). However, these results do not address whether healthcare utilization by the overall population changed after the debt collection restriction laws. While the lack of increased inpatient numbers in Table VI (column 2) suggests that populations in treated states did not increase hospital utilization anticipating fewer abusive collection practices, we now examine the impacts on healthcare utilization by populations at the county level. Using CMS Medicare Geographic Variation data and the Dartmouth Atlas of Healthcare, we calculate three measures of hospital utilization—ED visits, outpatient visits, and inpatient stays—and three measures of other providers' utilization—primary care visits, ambulatory surgery center (ASC) events, and home health (HH) visits. Table B.7 in Online Appendix presents the results of regressing the natural logarithm of these variables using equation (4). Column (1) indicates that ED visits rose by 3 percentage points

in treated hospitals relative to control, while utilization of other hospital services remained unchanged (columns (2) and (3)). Because hospitals are legally required to provide ED services regardless of the patients' ability to pay, these indicate that some patients may have increased ED utilization in response to higher outpatient prices. Higher ED utilization also likely contributes to the decline in overall hospital care quality discussed earlier. Finally, the utilization of primary care, ASCs, or HH did not change significantly, implying that the collection restriction laws primarily affected hospitals.

*B. Effect Heterogeneity by Type of Debt Collection Restriction Law*

Our estimates thus far represent the average effect of laws that increase restrictions on debt collection activities. However, these laws are not identical and vary in their specific provisions. Given the limited number of distinct law changes in our merged panel (20), we classify them into two categories: (i) compliance-based laws that introduce or raise licensing, bonding, and other requirements for debt collectors, and prohibit specific collection practices, and (ii) punitive laws that impose civil or administrative penalties for violations. We then interact dummies for the two types with $\text{Treat}_{ckp} \times \text{Post}_{kp\tau}$ in equation (1), estimating the treatment effects of each group relative to the control. Online Appendix Table B.8 presents the results. We find that compliance laws, which directly raise operational costs for all debt collectors, yield effects similar to the overall effects.

## VI. Conclusion

Consumer financial protection policies can have unintended consequences for non-financial sectors. We show that stricter third-party debt collection laws, intended to protect consumers from predatory practices, negatively impact hospitals, a sector that relies more heavily on debt collection than even the financial industry to recover unpaid, unsecured medical debt. We hand-collected data on state-level regulations on debt collection restrictions until 2022 and supplemented it with similar data previously compiled by Fedaseyeu (2020) until 2014 and used in Fonseca (2023). Employing

this data in a methodology that combines contiguous-county-pair comparisons with stacked difference-in-differences approach, we estimate the causal effects of these laws on hospitals.

We find that tighter debt collection regulations significantly increase hospitals' accounts receivable, unreimbursed costs, and liabilities, while reducing profitability and raising borrowing costs. In response, hospitals hire more in-house debt collectors, reduce employment, care capacity, and charity care for uninsured patients, engage in upcoding inpatient procedures, and raise outpatient prices. Moreover, mortality rates-based quality of care measures for major acute conditions decline. These laws therefore may inadvertently harm the very population they aim to protect: uninsured patients, who are more likely to incur medical debt, face reduced access to charity care after the enactment of the law. In summary, consumer financial protection policies can have unintended adverse effects even on non-financial sectors, ignoring which may overstate the net benefits of such policies.

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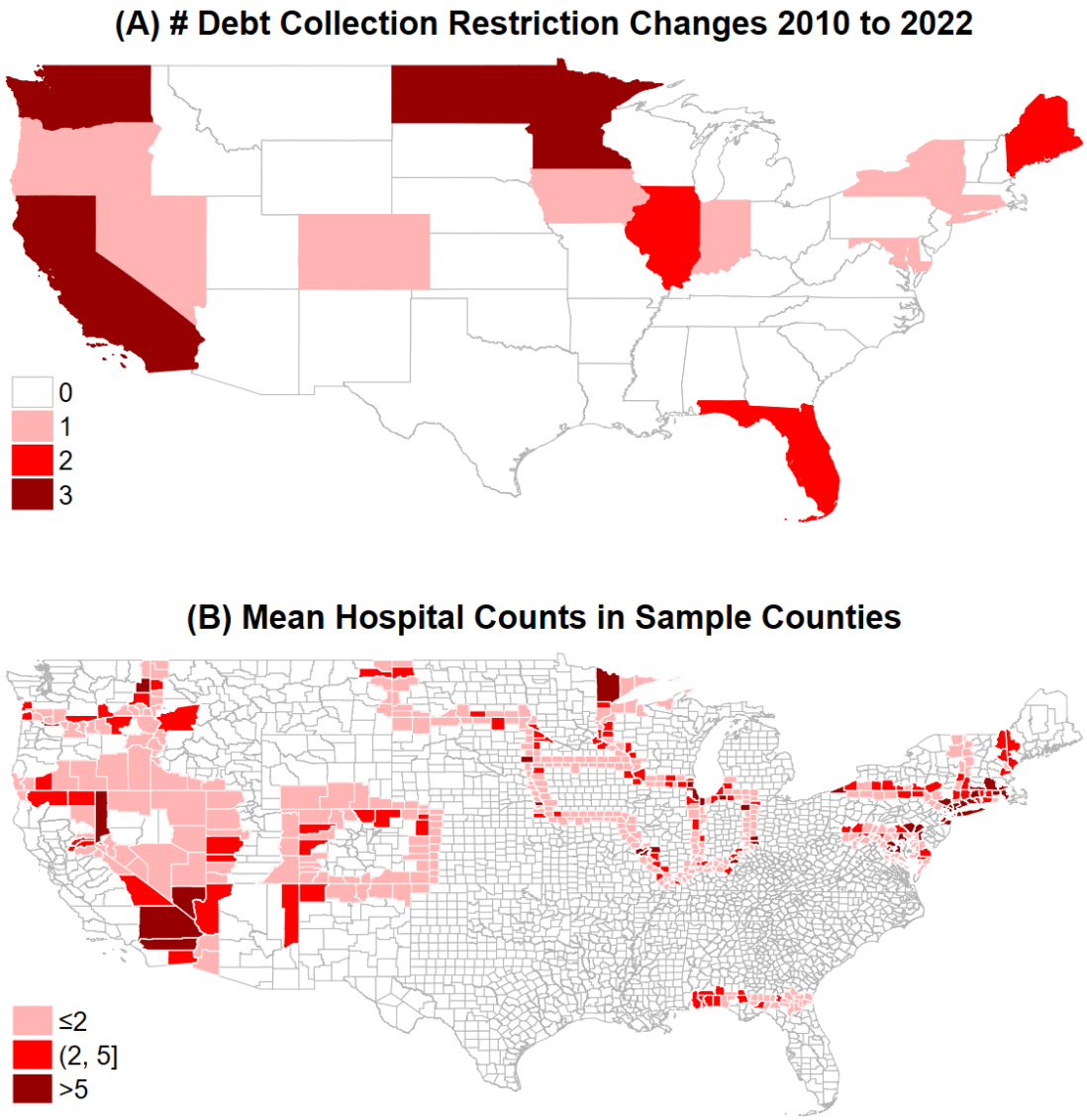



**Figure 1. Debt Collection Policy Changes and Hospital Distribution.** Panel A shows the number of state-level changes in debt collection restrictions between 2010 and 2022. Panel B shows the treatment and control counties plotted on the map of the contiguous U.S., along with the average number of hospitals located within them. These counties are located at the border between the treatment states and their neighboring control states.

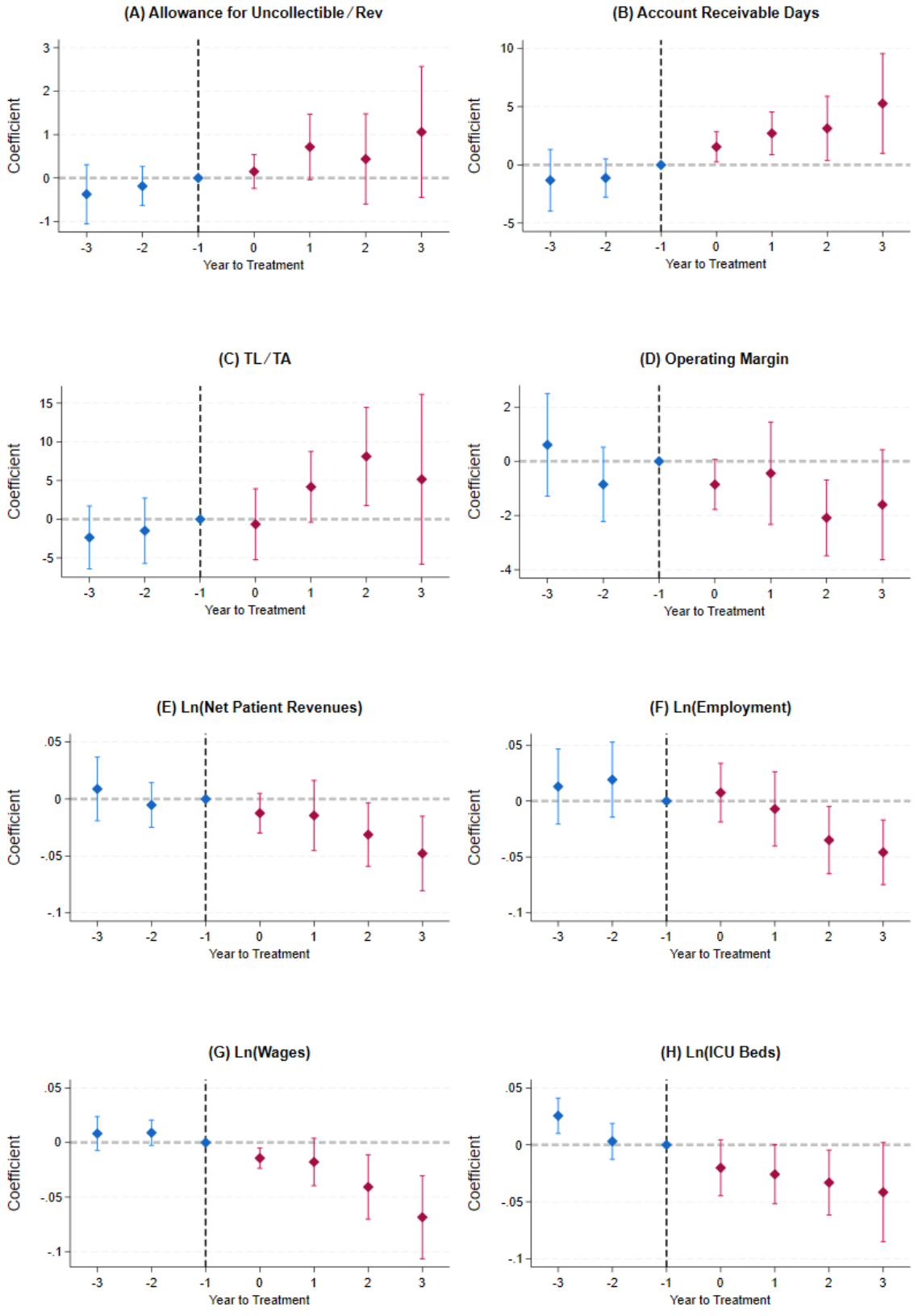



**Figure 2. Pre-Trends for Hospital-level Variables.** This figure shows the coefficients $\beta_\tau$s and 95% confidence intervals obtained from regression equation (2) for eight hospital-level variables, which are indicated in the respective panels. The horizontal axis shows years relative to the event year, with $\tau = 0$ indicating the first treated year.

## Table I
## Summary Statistics

This table reports summary statistics. *Ln*(*Net Pat Rev*) is the natural logarithm of net patient revenue. *Profit Margin* is profit margin, defined as net income over total income. *Oprt Margin* is operating margin, defined as operating earnings over operating income. *Ln*(*# Inpat*) is the natural logarithm of the number of inpatients. *Ln*(*Beds*) is the natural logarithm of the total number of hospital beds. *Ln*(*Wages*) is the natural logarithm of annualized total salaries and employee benefits. *Ln*(*Emp*) is the natural logarithm of total employees. *AR Days* is days in gross accounts receivable. *Allowance Uncollectible/Rev* is the ratio of allowances for uncollectible notes and accounts receivable to gross patient revenue. *Oprt Inc/NFA* is the ratio of operating income to net fixed assets. *Cash/NFA* is the ratio of cash to net fixed assets. *Ln*(*HC Est Size*) is the natural logarithm of establishment size of the healthcare industry (3-digit NAICS codes 621 and 622), calculated as the number of employees divided by the number of establishments. *Ln*(*HC Tot Wage*) is the natural logarithm of total annual payroll in the healthcare industry. *Controls* in regressions include one-year lagged hospital-level *Ln*(*Beds*), *Oprt Inc/NFA*, and *Cash/NFA*, as well as county-level unemployment rate and the natural logarithms of employment, GDP, and population.

| | N | Mean | P25 | Median | P75 | SD |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *A. Hospital×Year-Level Variables* | | | | | | |
| Ln(Net Pat Rev) | 8,791 | 4.30 | 3.20 | 4.28 | 5.45 | 1.42 |
| Profit Margin (%) | 8,782 | 3.26 | -1.05 | 4.05 | 9.73 | 15.31 |
| Oprt Margin (%) | 8,727 | 0.76 | -2.93 | 2.65 | 8.59 | 19.82 |
| Ln(# Inpat) | 9,102 | 7.62 | 6.29 | 7.80 | 9.11 | 1.75 |
| Ln(Beds) | 9,108 | 4.39 | 3.22 | 4.49 | 5.28 | 1.12 |
| Ln(Wages) | 9,059 | 3.59 | 2.54 | 3.54 | 4.61 | 1.32 |
| Ln(Emp) | 5,975 | 6.61 | 5.91 | 6.70 | 7.38 | 1.14 |
| AR Days | 8,790 | 41.80 | 19.39 | 33.67 | 53.52 | 36.08 |
| AllowUncoll/Rev (%) | 6,087 | 7.16 | 1.83 | 5.12 | 9.13 | 9.25 |
| Oprt Inc/NFA (%) | 8,724 | 8.78 | -2.81 | 3.38 | 12.49 | 107.74 |
| Cash/NFA (%) | 8,517 | 16.67 | 0.24 | 5.02 | 15.88 | 68.13 |
| *B. County×Year-Level Variables* | | | | | | |
| Ln(HC Est Size) | 2,890 | 2.05 | 1.55 | 2.12 | 2.64 | 0.85 |
| Ln(HC Tot Wage) | 2,985 | 10.27 | 8.20 | 10.18 | 12.11 | 2.51 |
| Unemployment Rate (%) | 3,908 | 5.67 | 3.50 | 5.00 | 7.20 | 3.00 |
| Ln(Employment) | 3,842 | 8.22 | 6.62 | 8.18 | 9.97 | 2.48 |
| Ln(GDP) | 3,894 | 7.27 | 5.98 | 7.07 | 8.40 | 1.79 |
| Ln(Population) | 3,894 | 10.37 | 9.18 | 10.26 | 11.54 | 1.70 |

## Table II
## Effect on Healthcare Industry

All columns report estimates from the regression model in equation (4). All outcome variables are in natural logarithm, and observations are at the county-year level. *Est Size* is the number of employees divided by the number of establishments in healthcare industry (identified using 3-digit NAICS codes 621 and 622 in CBP data). *Tot Wage* is total annual payroll in the healthcare industry. *Avg Emp Wage* is average annual payroll per employee in the healthcare industry. *Avg Est Wage* is average annual payroll per establishment in the healthcare industry. *Treat* equals one if the county is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are county-level unemployment rate and the natural logarithms of employment, GDP, and population listed and defined in Table I. All models include event×county-pair×county and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Outcome (in Ln): | Est Size | Tot Wage | Avg Emp Wage | Avg Est Wage |
| Treat×Post | -0.06* | -0.08** | -0.03** | -0.10*** |
| | (0.03) | (0.03) | (0.02) | (0.03) |
| Event×Pair×R.Year FE | Y | Y | Y | Y |
| Event×Pair×County FE | Y | Y | Y | Y |
| Controls | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.88 | 0.98 | 0.59 | 0.90 |
| Observations | 4,748 | 5,000 | 4,748 | 5,000 |

## Table III
## Bad Debt Concerns and Financial Obligations of Hospitals

All columns report estimates from the regression model in equation (1). *AR Days* is days in gross accounts receivable. *Unreimb/Rev* is the ratio of total unreimbursed cost for Medicaid, SCHIP and state and local indigent care programs to gross patient revenue, expressed as a percentage. *Allowance Uncollectible/Rev* is the ratio of allowances for uncollectible notes and accounts receivable to gross patient revenue, expressed as a percentage. *Int Exp/Rev* is the ratio of total interest expenses to gross patient revenues, expressed as a percentage. *TL/TA* is the debt-to-assets ratio, defined as total liability over total assets and expressed as a percentage. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| | Bad Debt Concerns | | | Financial Obligations | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Outcome: | AR Days | $\frac{\text{Unreimb}}{\text{Rev}}$ % | $\frac{\text{AllowUncoll}}{\text{Rev}}$ % | $\frac{\text{Int Exp}}{\text{Rev}}$ % | $\frac{\text{TL}}{\text{TA}}$ % |
| Treat×Post | 3.57*** | 0.15* | 0.69** | 0.07*** | 4.40* |
| | (0.88) | (0.09) | (0.33) | (0.02) | (2.50) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.81 | 0.68 | 0.71 | 0.82 | 0.78 |
| Observations | 14,106 | 8,691 | 9,160 | 14,109 | 14,105 |

## Table IV
## Effect Heterogeneity

All columns report estimates from the regression model in equation (1). *AR Days* is days in gross accounts receivable. *Unreimb/Rev* is the ratio of total unreimbursed cost for Medicaid, SCHIP and state and local indigent care programs to gross patient revenue, expressed as a percentage. *Allowance Uncollectible/Rev* is the ratio of allowances for uncollectible notes and accounts receivable to gross patient revenue, expressed as a percentage. *Int Exp/Rev* is the ratio of total interest expenses to gross patient revenues, expressed as a percentage. *TL/TA* is the debt-to-assets ratio, defined as total liability over total assets and expressed as a percentage. # *Debt Collectors Per HC* is the number of debt collectors in a state per healthcare establishment. It is *High* if this value exceeds the respective event-specific pre-event-year median, and *Low* otherwise, with the classification held fixed over the event. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

| Outcome: | AR Days | | $\frac{\text{Unreimb}}{\text{Rev}}$ % | | $\frac{\text{AllowUncoll}}{\text{Rev}}$ % | | $\frac{\text{Int Exp}}{\text{Rev}}$ % | | $\frac{\text{TL}}{\text{TA}}$ % | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| # Debt Collectors Per HC | High | Low | High | Low | High | Low | High | Low | High | Low |
| Treat×Post | 5.79*** | -4.01*** | 0.41*** | 0.18 | 1.02* | 0.61 | 0.13** | 0.01 | 17.33** | -21.34*** |
| | (1.46) | (1.32) | (0.11) | (0.14) | (0.52) | (0.38) | (0.05) | (0.08) | (8.35) | (3.07) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.82 | 0.81 | 0.69 | 0.60 | 0.69 | 0.73 | 0.82 | 0.79 | 0.75 | 0.79 |
| Observations | 8,904 | 4,156 | 5,395 | 2,425 | 5,465 | 2,874 | 8,907 | 4,156 | 8,899 | 4,159 |

## Table V
## Aggressive Hospital Debt Collection Activities

Column (1) report estimates from regression model in equation (1), and the remaining use that in equation (3). *Job Postings %* is the percentage of a hospital's job postings related to debt collection. Δ *Debt Collection Complaints* is the growth rate of the number of debt collection-related consumer complaints reported to CFPB. The complaint categories corresponding to columns (2) to (4) are: all firms, hospitals, and non-hospitals, respectively. In column (1), *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. In columns (2) to (4), *Treat* equals one if the state tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Column (1) includes the seven hospital- and county-level control variables listed and defined in Table I, and columns (2) to (4) include state-level counterparts for four control variables—the unemployment rate and the natural logarithms of employment, GDP, and population. Column (1) includes event×county-pair×hospital and event×county-pair×relative year fixed effects. Columns (2) to (4) include event×state-pair×state and event×state-pair×relative year fixed effects. State pairs are constructed similarly to the county pairs described in Section II, pairing each treated state with appropriate control states. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| Outcome: | Job Postings % | Δ Debt Collection Complaints | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Collection Rep. | All Firms | Hospitals | Non-Hospitals |
| Treat×Post | 1.78** | -0.10** | 0.23** | -0.11** |
| | (0.86) | (0.04) | (0.11) | (0.04) |
| Panel | Hosp | State | State | State |
| Event×State Pair×R.Year FE | N | Y | Y | Y |
| Event×State Pair×State FE | N | Y | Y | Y |
| Event×Pair×R.Year FE | Y | N | N | N |
| Event×Pair×Hosp FE | Y | N | N | N |
| Adjusted $R^2$ | 0.03 | 0.89 | -0.67 | 0.89 |
| Observations | 5,734 | 522 | 398 | 522 |

## Table VI
## Financial Performance

All columns report estimates from the regression model in equation (1). *Ln(Net Pat Rev)* is the natural logarithm of net patient revenue. *Ln(# Inpat)* is the natural logarithm of the number of inpatients. *Profit Margin* is net income over total income. *Oprt Margin* is operating margin, defined as operating earnings over operating income. *NI/NFA* is the ratio of net income to net fixed assets, expressed as a percentage. *Oprt Inc/NFA* is the ratio of operating income to net fixed assets, expressed as a percentage. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Outcome: | Ln(Net Pat Rev) | Ln(# Inpat) | Profit Margin % | Oprt Margin % | $\frac{\text{NI}}{\text{NFA}}$ % | $\frac{\text{Oprt Inc}}{\text{NFA}}$ % |
| Treat×Post | -0.02** | -0.01 | -0.95** | -1.02** | -10.51** | -9.66** |
| | (0.01) | (0.01) | (0.46) | (0.46) | (5.10) | (4.57) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.98 | 0.99 | 0.57 | 0.61 | 0.33 | 0.35 |
| Observations | 14,109 | 14,120 | 14,102 | 14,076 | 14,081 | 14,088 |

## Table VII
## Employment Decisions

All columns report estimates from the regression model in equation (1). *Ln*(*Emp*) is the natural logarithm of total employees. *Ln*(*# Core*) is the natural logarithm of the number of physicians, nurses, and pharmacists, as defined in Gao, Kim, and Sevilir (2025). *Ln*(*Wages*) is the natural logarithm of annualized total salaries and employee benefits. *Ln*(*Avg Wages*) is the natural logarithm of average salary and employee benefits per employee. *# Core/Bed-Days* is the ratio of the number of nurses, physicians, and pharmacists to bed-days available (in '000s). *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Outcome: | Ln(Emp) | Ln(# Core) | Ln(Wages) | Ln(Avg Wages) | $\frac{\text{\# Core}}{\text{Bed-Days}}$ |
| Treat×Post | -0.02* | -0.07** | -0.04*** | -0.01* | -0.07* |
| | (0.01) | (0.03) | (0.01) | (0.01) | (0.04) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.97 | 0.94 | 0.99 | 0.86 | 0.92 |
| Observations | 9,933 | 9,863 | 14,084 | 14,073 | 14,118 |

## Table VIII
## Investments in Care Capacity

All columns report estimates from the regression model in equation (1). *Ln*(*Beds*) is the natural logarithm of the total number of hospital beds. *Ln*(*ICU Beds*) is the natural logarithm of the number of intensive care unit beds. *Ln*(*Non-ICU Beds*) is the natural logarithm of the number of non-intensive care unit beds, calculated as the difference between total beds and ICU beds. *IT*/*NFA* is the ratio of a hospital's investments in health information technology to its net fixed assets, expressed as a percentage. *Ln*(*# Sub-Units*) is the natural logarithm of the number of unique sub-providers for each hospital. *DRG Share* is the ratio of the number of unique DRGs a hospital handles to the total number of unique DRGs performed by all sample hospitals from the same state-year, expressed as a percentage. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Outcome: | Ln(Beds) | Ln(ICU Beds) | Ln(Non-ICU Beds) | $\frac{\text{IT}}{\text{NFA}}$ % | Ln(# Sub-Units) | DRG Share % |
| Treat×Post | -0.01** | -0.04** | -0.01 | -0.84** | -0.04** | -0.80** |
| | (0.00) | (0.02) | (0.01) | (0.32) | (0.02) | (0.33) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.99 | 0.97 | 0.99 | 0.17 | 0.92 | 0.99 |
| Observations | 14,118 | 8,428 | 14,118 | 1,125 | 8,593 | 5,268 |

## Table IX
## Prices of Healthcare Services

All columns report estimates from the regression model in equation (1). The outcome variables are the natural logarithm of the average charge (*Avg Chrg*), average payment received from all sources (*Avg pmt* [*All Src*]), average payment received from Medicare (*Avg pmt* [*Mdcr*]), and the payment-to-charge ratio (*Pmt/Chrg*), with the average calculated across patients of a given hospital for a given procedure. Columns (1) to (4) show results for inpatient healthcare services, and (4) to (8) for outpatient services. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. Fixed effects for each specification are noted in the table. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| Care Type: | Inpatient | | | | Outpatient | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Outcomes in Ln: | Avg Chrg | Avg Pmt [*All Src*] | Avg Pmt [*Mdcr*] | Pmt/Chrg | Avg Chrg | Avg Pmt [*All Src*] | Avg Pmt [*Mdcr*] | Pmt/Chrg |
| Treat×Post | -0.007 | 0.012 | 0.017 | 0.006 | 0.050*** | 0.004* | 0.006* | -0.009** |
| | (0.02) | (0.01) | (0.01) | (0.01) | (0.01) | (0.00) | (0.00) | (0.00) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp×DRG | Y | Y | Y | Y | N | N | N | N |
| FE: Event×Pair×Hosp×APC | N | N | N | N | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.97 | 0.98 | 0.98 | 0.96 | 0.97 | 0.99 | 0.99 | 0.90 |
| Observations | 512,264 | 512,264 | 512,264 | 512,264 | 91,099 | 91,099 | 91,099 | 91,099 |

## Table X
## Upcoding Inpatient Procedures

This table reports estimates from the regression model in equation (6). *Ln(# Inpat)* is the natural logarithm of the number of inpatients. *Low-Cost* DRGs are those with average submitted charges to Medicare below the 25$^{th}$ percentile value across all DRGs submitted by sample hospitals in a year. *High-Cost* DRGs are those with the average charges above 75$^{th}$ percentile. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All specifications include event×county-pair×hospital×DRG and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

| Outcome: | Ln (# Inpat) | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| DRG Type: | All | Low-Cost | High-Cost |
| Treat×Post | -0.011 | -0.027$^{*}$ | 0.032$^{***}$ |
| | (0.012) | (0.015) | (0.011) |
| FE: Event×Pair×R.Year | Y | Y | Y |
| FE: Event×Pair×Hosp×DRG | Y | Y | Y |
| Adjusted $R^2$ | 0.85 | 0.85 | 0.79 |
| Observations | 386,880 | 160,004 | 45,417 |

## Table XI
## Patient Mortality

All columns report estimates from the regression model in equation (1). The outcome variable is the 30-day mortality rate, expressed as a percentage. A mortality is defined as a patient death within 30 days of discharge from a hospital admission. Column header denotes the diagnostic groups: pneumonia, AMI, and heart failure. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| | (1) | (2) | (3) |
|---|---|---|---|
| Mortality Rate %: | Pneumonia | AMI | Heart Failure |
| Treat×Post | 0.38** | 0.14* | -0.16 |
| | (0.17) | (0.07) | (0.16) |
| FE: Event×Pair×R.Year | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y |
| Adjusted $R^2$ | 0.77 | 0.64 | 0.62 |
| Observations | 7,703 | 5,613 | 7,166 |

## Table XII
## Charity Care at Non-Profit Hospitals

All columns report estimates from the regression model in equation (1). The outcome variables, *Chrg Apv*, *Cost Est*, and *Cost Uncomp*, are all scaled by net fixed assets and expressed as a percentage. *Chrg Apv* is the gross charges for services provided to patients approved for a discount under the hospital's charity care policy, *Cost Est* is the estimated cost to the hospital of providing these services, and *Cost Uncomp* is the net cost to the hospital of providing these services, defined as the difference between the estimated cost and the partial payments made by the patients. Columns (1) to (3) present results for uninsured patients, and columns (4) to (6) present results for insured patients. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

| Patient Type: | Uninsured | | | Insured | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Outcome (%): scaled by NFA | Chrg Apv | Cost Est | Cost Uncomp | Chrg Apv | Cost Est | Cost Uncomp |
| Treat×Post | -1.07* | -0.36** | -0.32** | -0.05 | -0.02 | -0.05 |
| | (0.54) | (0.16) | (0.16) | (0.14) | (0.08) | (0.08) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.58 | 0.56 | 0.56 | 0.58 | 0.58 | 0.56 |
| Observations | 8,889 | 8,889 | 8,843 | 7,278 | 7,286 | 7,257 |

# Internet Appendix to

# "How Do Debt Collection Restrictions Affect Hospitals and Patients?"

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# A. Data Appendix

*1. SOC Codes associated with debt collection activities*

| SOC Code | Occupation | SOC Code | Occupation |
|---|---|---|---|
| 11-1021 | General and Operations Managers | 15-2011 | Actuaries |
| 11-3012 | Administrative Services Managers | 23-1011 | Lawyers |
| 11-3031 | Financial Managers | 23-2093 | Title Examiners, Abstractors, and Searchers |
| 13-1031 | Claims Adjusters, Examiners, and Investigators | 23-2099 | Legal Support Workers, All Other |
| 13-1051 | Cost Estimators | 31-9092 | Medical Assistants |
| 13-2011 | Accountants and Auditors | 43-3011 | Bill and Account Collectors |
| 13-2031 | Budget Analysts | 43-3021 | Billing and Posting Clerks |
| 13-2041 | Credit Analysts | 43-3031 | Bookkeeping, Accounting, and Auditing Clerks |
| 13-2051 | Financial and Investment Analysts | 43-4021 | Correspondence Clerks |
| 13-2052 | Personal Financial Advisors | 43-4041 | Credit Authorizers, Checkers, and Clerks |
| 13-2053 | Insurance Underwriters | 43-4131 | Loan Interviewers and Clerks |
| 13-2061 | Financial Examiners | 43-4141 | New Accounts Clerks |
| 13-2071 | Credit Counselors | 43-6012 | Legal Secretaries and Administrative Assistants |
| 13-2072 | Loan Officers | 43-6013 | Medical Secretaries and Administrative Assistants |
| 13-2099 | Financial Specialists, All Other | 43-9061 | Office Clerks, General |

*2. Keywords to identify debt collection complaints related to hospitals*

ER, ICU, hospital, medical, doctor, clinic, emergency room, emergencies, abortion, health center, obgyn, ob-gyn, ob gyn, orthopedic, pediatric, cardiology, oncology, neurology, dermatology, radiology, anesthesiology, urology, pharmacy, pharmacies, pharmacist, drugstore, gastroenterology, endocrinology, psychiatry, dental, dentist, optometry, primary care, urgent care, healthcare, surgery, surgical center, nursing home, prescription, prescriptions, drug, drugs, physician, lab test, therapy, ambulance, ambulatory care, in network, in-network, out-of-network, out of network, co-pay, copay, co pay, no surprises act

*3. Constructing Employment and Wage Variables from Rand Hospital Data and HCRIS*

- $Ln(Emp)$: The natural logarithm of total employees, measured in full-time equivalent employees based on paid hours. This information is sourced from HCRIS Worksheet S-3, Part II, Line 1.

- *Ln*(*# Core*): The natural logarithm of the number of nurses, physicians, and pharmacists, measured in full-time equivalent employees based on paid hours. This information is sourced from HCRIS Worksheet S-3, Part II. Core workers include Non-physician anesthetist Part A (Line 2), Non-physician anesthetist Part B (Line 3), Physician - Part A - Administrative (Line 4), Physician - Part A - Teaching (Line 4.01), Physician and Non Physician-Part B (Line 5), Interns & residents (in an approved program) (Line 7), Contracted interns & residents (in an approved program) (Line 7.01), Contract labor: Direct Patient Care (Line 11), Contract labor: Physician - Part A - Administrative (Line 13), Home office: Physician Part A - Administrative (Line 15), Home office & Contract Physician Part A - Teaching (Line 16), Nursing Administration (Line 38), and Pharmacy (Line 40). This definition is based on Gao, Kim, and Sevilir (2025).

- *Ln*(*Wages*): The natural logarithm of annualized total salaries and employee benefits. Total salaries and employee benefits are sourced from HCRIS Worksheet A, Line 4 and 200. This variable is normalized to an annualized basis by the RAND Corporation.

- *Ln*(*Avg Wages*): The natural logarithm of average salary and employee benefits per employee. This variable is calculated by RAND Corporation as total annualized salaries and employee benefits divided by the number of full-time equivalent employees.

# B. Additional Regression Results

## Table B.1
## The Restriction Laws and Debt Collection Industry

All columns report estimates from the regression model in equation (3). All outcome variables are in natural logarithm, and observations are at the state-year level. # *Est* is the number of establishments in the debt collection industry (NAICS codes 561440 and 561491). *Tot Wage* is total annual payroll in the debt collection industry. *Avg Emp Wage* is average annual payroll per employee in the debt collection industry. *Avg Est Wage* is average annual payroll per establishment in the debt collection industry. *Treat* equals one if the state tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are state-level counterparts for four county-level control variables listed and defined in Table I—the unemployment rate and the natural logarithms of employment, GDP, and population. All models event×state-pair×state and event×state-pair×relative year fixed effects. State pairs are constructed similarly to the county pairs described in Section II, pairing each treated state with appropriate control states. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Outcome: Growth Rate | # Est | Tot Wage | Avg Emp Wage | Avg Est Wage |
| Treat×Post | -0.03* | -0.15*** | -0.08*** | -0.12** |
| | (0.02) | (0.05) | (0.02) | (0.06) |
| FE: Event×State Pair×R.Year | Y | Y | Y | Y |
| FE: Event×State Pair×State | Y | Y | Y | Y |
| Controls | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.20 | -0.26 | -0.38 | 0.11 |
| Observations | 702 | 670 | 644 | 670 |

## Table B.2
## Debt Collection Restriction Laws and Non-Healthcare Industries

| Industry: | Grocery, Restaurant, & Fitness | | | | Non-Tradable Excl. Healthcare | | | | All Industries Excl. Healthcare | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Outcome (in Ln): | Est Size | Tot Wage | Avg Emp Wage | Avg Est Wage | Est Size | Tot Wage | Avg Emp Wage | Avg Est Wage | Est Size | Tot Wage | Avg Emp Wage | Avg Est Wage |
| Treat×Post | 0.02 | 0.04 | 0.01 | 0.02 | 0.01 | 0.03 | 0.01 | 0.01 | -0.02 | 0.01 | 0.01 | -0.01 |
| | (0.02) | (0.02) | (0.01) | (0.02) | (0.02) | (0.02) | (0.01) | (0.02) | (0.01) | (0.01) | (0.01) | (0.02) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| FE: Event×Pair×County | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.85 | 0.99 | 0.80 | 0.92 | 0.82 | 0.99 | 0.76 | 0.90 | 0.97 | 1.00 | 0.84 | 0.97 |
| Observations | 6,964 | 7,052 | 6,964 | 7,052 | 6,750 | 6,910 | 6,750 | 6,910 | 7,610 | 7,610 | 7,610 | 7,610 |

All columns report estimates from the regression model in equation (4). All outcome variables are in natural logarithm, and observations are at the county-year level. Columns (1) to (4) show results for grocery, restaurants, and fitness; (5) to (8) for all non-tradable industries excluding healthcare; and (9) to (12) for all industries excluding healthcare. *Est Size* is the number of employees per establishment. *Tot Wage* is total annual payroll. *Avg Emp Wage* is average annual payroll per employee. *Avg Est Wage* is average annual payroll per establishment. *Treat* equals one if the county is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are county-level unemployment rate and the natural logarithms of employment, GDP, and population listed and defined in Table I. All models include event×county-pair×county and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

## Table B.3
## Effect on Hospital Loans

All columns report estimates from the regression model in equation (5). The unit of observation is a loan facility $l$, borrowed by hospital borrower $i$ in year $t$. *DCR Exposure* is the net fixed asset-weighted exposure to the debt collection restriction tightening. *Spread & Fee* is the interest rate spread (in basis points) over LIBOR, plus fees on the drawn portion of the loan. *Ln(Loan Amt)* is the natural logarithm of the loan facility amount. Controls are borrower $i$'s natural logarithm of net fixed assets, operating income over net fixed assets, total liabilities over total assets, and cash over net fixed assets. All models include hospital borrower, year, and state fixed effects. Standard errors are clustered by hospital borrower and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| | (1) | (2) |
|---|---|---|
| Outcome: | Spread&Fee | Ln(Loan Amt) |
| DCR Exposure | 85.39* | -0.22* |
| | (42.29) | (0.13) |
| FE: Hosp, Loan Year, State | Y | Y |
| Adjusted $R^2$ | 0.54 | 0.78 |
| Observations | 258 | 359 |

## Table B.4
## Cross-Border Spillover Effects from Debt Collection Restriction Laws

All columns report estimates from the regression model in equation (1). All outcomes are measured as normalized difference, defined as the difference in the outcome of a hospital in a bordering county and the mean outcome for same-state hospitals in inner counties, scaled by the standard deviation of the outcome across inner hospitals. *AR Days* is days in gross accounts receivable. *Unreimb/Rev* is the ratio of total unreimbursed cost for Medicaid, SCHIP and state and local indigent care programs to gross patient revenue, expressed as a percentage. *Allowance Uncollectible/Rev* is the ratio of allowances for uncollectible notes and accounts receivable to gross patient revenue, expressed as a percentage. *Int Exp/Rev* is the ratio of total interest expenses to gross patient revenues, expressed as a percentage. *TL/TA* is the debt-to-assets ratio, defined as total liability over total assets and expressed as a percentage. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

| | Bad Debt Concerns | | | Financial Obligations | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Outcome: Norm diff [border - inner] | AR Days | $\frac{\text{Unreimb}}{\text{Rev}}$ | $\frac{\text{AllowUncoll}}{\text{Rev}}$ | $\frac{\text{Int Exp}}{\text{Rev}}$ | $\frac{\text{TL}}{\text{TA}}$ |
| Treat×Post | 0.02 | 0.01 | -0.03 | 7.41 | 0.01 |
| | (0.05) | (0.07) | (0.10) | (6.04) | (0.07) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.76 | 0.57 | 0.74 | 0.22 | 0.70 |
| Observations | 12,056 | 7,405 | 7,659 | 12,059 | 12,055 |

## Table B.5
## Bad Debt Concerns and Financial Obligations of Hospitals [County Pairs ≤ 75km]

All columns report estimates from the regression model in equation (1). The sample includes only the county pairs where the population centroids of the two counties are less than 75 kilometers (46 miles) apart, following Dube, Lester, and Reich (2016). *AR Days* is days in gross accounts receivable. *Unreimb/Rev* is the ratio of total unreimbursed cost for Medicaid, SCHIP and state and local indigent care programs to gross patient revenue, expressed as a percentage. *Allowance Uncollectible/Rev* is the ratio of allowances for uncollectible notes and accounts receivable to gross patient revenue, expressed as a percentage. *Int Exp/Rev* is the ratio of total interest expenses to gross patient revenues, expressed as a percentage. *TL/TA* is the debt-to-assets ratio, defined as total liability over total assets and expressed as a percentage. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| | Bad Debt Concerns | | | Financial Obligations | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Outcome: | AR Days | $\frac{\text{Unreimb}}{\text{Rev}}$ % | $\frac{\text{AllowUncoll}}{\text{Rev}}$ % | $\frac{\text{Int Exp}}{\text{Rev}}$ % | $\frac{\text{TL}}{\text{TA}}$ % |
| Treat×Post | 4.01*** | 0.18** | 1.04*** | 0.07** | 5.52** |
| | (0.95) | (0.08) | (0.31) | (0.03) | (2.59) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.79 | 0.69 | 0.67 | 0.82 | 0.73 |
| Observations | 10,646 | 6,516 | 6,555 | 10,649 | 10,645 |

## Table B.6
## Treatment Effects by Hospital Count in Treated and Control Counties

The columns report coefficient $\beta$ associated with $\text{Treat}_{ickp} \times \text{Post}_{kp\tau}$ and the number of observations $N$ from the regression model in equation (1) for the subsamples of county pairs where the number of hospitals in both treated and control counties exceeds $n (n = 2, 3, 4)$. The outcomes variables are noted in rows. *AR Days* is days in gross accounts receivable. *Unreimb/Rev* is the ratio of total unreimbursed cost for Medicaid, SCHIP and state and local indigent care programs to gross patient revenue, expressed as a percentage. *Allowance Uncollectible/Rev* is the ratio of allowances for uncollectible notes and accounts receivable to gross patient revenue, expressed as a percentage. *Int Exp/Rev* is the ratio of total interest expenses to gross patient revenues, expressed as a percentage. *TL/TA* is the debt-to-assets ratio, defined as total liability over total assets and expressed as a percentage. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| # Hosp in each county of a pair: | n ≥ 2 | | n ≥ 3 | | n ≥ 4 | |
|---|---|---|---|---|---|---|
| Outcome Variable | $\beta$ | N | $\beta$ | N | $\beta$ | N |
| AR Days | 2.56** | 10,424 | 4.49*** | 6,317 | 4.80*** | 4,481 |
| $\frac{\text{Unreimb}}{\text{Rev}}$ % | 0.16** | 6,305 | 0.09 | 3,708 | 0.10 | 2,639 |
| $\frac{\text{AllowUncoll}}{\text{Rev}}$ % | 0.45 | 6,999 | 1.06*** | 4,281 | 1.38*** | 2,994 |
| $\frac{\text{Int Exp}}{\text{Rev}}$ % | 0.06*** | 10,427 | 0.04*** | 6,319 | 0.03* | 4,483 |
| $\frac{\text{TL}}{\text{TA}}$ % | 2.60 | 10,423 | 5.74** | 6,318 | 6.81** | 4,481 |

## Table B.7
## Healthcare Utilization

All columns report estimates from the regression model in equation (4). All outcome variables are in natural logarithm, with observations at the county-year level. The outcomes are respectively the natural logarithm of the number of hospital emergency department visits, hospital outpatient visits, hospital inpatient stays, physician visits, ambulatory surgery center events, and home health visits. *Treat* equals one if the county is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are county-level unemployment rate and the natural logarithms of employment, GDP, and population listed and defined in Table I. All models include event×county-pair×county and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| Facility Type: | Hospital | | | Non-Hospital | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Outcome (in Ln): | # ED Visits | # OP Visits | # IP Stays | # PC Visits | # ASC Events | # HH Visits |
| Treat×Post | 0.03*** | -0.00 | 0.01 | -0.01 | 0.02 | -0.02 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.02) | (0.04) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y | Y |
| FE: Event×Pair×County | Y | Y | Y | Y | Y | Y |
| Controls | Y | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 1.00 | 1.00 | 1.00 | 0.99 | 0.99 | 0.99 |
| Observations | 5,408 | 5,408 | 5,408 | 5,590 | 5,266 | 4,944 |

## Table B.8
## Treatment Effects by Legislation Characteristics

All columns report estimates from the interacting the regression model in equation (1) with indicators for laws that focus on compliance and laws that focus on punitive action. *AR Days* is days in gross accounts receivable. *Unreimb/Rev* is the ratio of total unreimbursed cost for Medicaid, SCHIP and state and local indigent care programs to gross patient revenue, expressed as a percentage. *Allowance Uncollectible/Rev* is the ratio of allowances for uncollectible notes and accounts receivable to gross patient revenue, expressed as a percentage. *Int Exp/Rev* is the ratio of total interest expenses to gross patient revenues, expressed as a percentage. *TL/TA* is the debt-to-assets ratio, defined as total liability over total assets and expressed as a percentage. *Treat* equals one if the hospital is located in a state that tightened its debt collection regulations during event $k$, and zero otherwise. *Post* is one for the year of treatment and the subsequent three years in event $k$ and zero otherwise. Controls are the seven hospital- and county-level variables listed and defined in Table I. All models include event×county-pair×hospital and event×county-pair×relative year fixed effects. Standard errors are clustered by state and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

| | Bad Debt Concerns | | | Financial Obligations | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Outcome: | AR Days | $\frac{\text{Unreimb}}{\text{Rev}}$ % | $\frac{\text{AllowUncoll}}{\text{Rev}}$ % | $\frac{\text{Int Exp}}{\text{Rev}}$ % | $\frac{\text{TL}}{\text{TA}}$ % |
| Treat×Post×Compliance | 4.59*** | 0.22* | 0.90 | 0.09** | 7.05 |
| | (1.44) | (0.11) | (0.56) | (0.04) | (4.90) |
| Treat×Post×Punitive | 0.91 | 0.15* | 0.21 | 0.06 | -0.50 |
| | (1.59) | (0.09) | (0.39) | (0.04) | (3.46) |
| FE: Event×Pair×R.Year | Y | Y | Y | Y | Y |
| FE: Event×Pair×Hosp | Y | Y | Y | Y | Y |
| Adjusted $R^2$ | 0.81 | 0.67 | 0.70 | 0.82 | 0.76 |
| Observations | 11,379 | 6,940 | 7,366 | 11,382 | 11,378 |