Cleaner Credit, Closed Doors: Medical Debt Reporting Bans and Access to Hospital Care

Qianfan Wu*

September 2026

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Abstract

Medical debt appeared on roughly one in six credit reports, and policymakers are increasingly restricting its reporting to credit bureaus to protect indebted households. I study the demand- and supply-side effects of removing medical debt from credit reports, using staggered state-level policy changes and administrative data. On the demand side, household delinquencies increase, raising hospitals' uncollectible medical bills by 8.6%. On the supply side, hospitals restrict access to care and, in less-competitive markets, raise prices by 3.6%. Access falls along two dimensions: hospitals admit fewer patients with less generous insurance and cut essential but unprofitable services. Repeat emergency room visits subsequently increase, consistent with deteriorating patient health. These effects are concentrated among low-income households and among financially fragile and non-profit hospitals. Together, the results suggest that medical debt credit protections trigger supply-side adjustments, with the burden falling heaviest on the very households they aim to protect.

Keywords: Medical Debt, Debt Collection, Credit Reporting, Credit Default, Healthcare Access

*The author is from the Kelley School of Business at Indiana University. I thank Berk Yayvak (discussant) and seminar participants at the 2026 Eastern Finance Association Meeting, the 2025 FMA Doctoral Student Consortium, the Health Policy Workshop, and Indiana University for valuable feedback. Email: qw18@iu.edu.

1 Introduction

As of 2018, 71 million Americans—about one-third of the adult population—had debts in collection on their credit reports (Hassani & McKernan 2018). To protect delinquent borrowers, policymakers have increasingly adopted information deletion policies that remove negative records from credit files, aiming to lower borrowing costs and help households escape persistent debt cycles.

While such policies have well-documented benefits, particularly in improving credit access (Musto 2004), they may also impose unintended costs to consumers. Credit reporting serves as a disciplinary tool that incentivizes repayment in sectors where consumption occurs before payment (Padilla & Pagano 2000).[1] By weakening this discipline, reporting restrictions can create a moral hazard problem: borrowers have less incentive to pay, and providers, anticipating higher default, may adjust the access and pricing of the goods and services. These supply-side responses are central to the net welfare effects of such policies, yet they remain largely unexamined. This paper studies the importance of these responses in the hospital industry, asking how hospitals respond to credit reporting restrictions and the consequences on healthcare access and affordability.

I focus on hospitals for two main reasons. First, healthcare is among the most economically significant sectors that rely on credit reporting, and one where the consequences for consumers are especially high. It provides essential services central to public health and consumer welfare and accounts for 17 percent of GDP as of 2023 (CMS 2023). Because hospitals rarely collect payment up front, they routinely bill patients after care and report unpaid medical bills (medical debt) to credit bureaus through collection agencies. This reliance is extensive: medical debt accounts for 58% of third-party collection records reported to credit bureaus and appears on 17.8% of individuals' credit reports (Kluender et al. 2021,

[1] Healthcare providers, telecommunications firms, utilities, and retailers are among the top sources of delinquent bills reported to credit bureaus through third-party collectors (CFPB 2014).

CFPB 2022). Second, the question carries immediate policy relevance given the rapid proliferation of medical debt reporting restrictions. Since early 2023, the CFPB has banned the reporting of medical debts under $500 and, subsequently, all medical debts, while many states have proposed similar bans since mid-2023. As these protections expand, understanding how hospitals adjust their supply of care is essential for weighing their benefits against their costs.

To study this question, I exploit the staggered adoption of medical debt credit reporting bans in Colorado and New York. I find that hospitals' uncollectible medical bills rise after the bans. Drawing on administrative health insurance claims that track healthcare procedures and their prices for 39.7 million individuals, I show that hospitals respond by restricting admissions for patients with less generous insurance, cutting essential but unprofitable services, and raising prices. These adjustments are costly to patients: repeat emergency room (ER) visits increase, consistent with worse health outcomes from delayed or incomplete care. Hospitals in lower-income areas experience larger increases in defaults and respond more aggressively, suggesting that the policies disproportionately harm the disadvantaged households they are intended to protect.

The fact that hospitals respond along these margins is not obvious ex ante. First, conceptually, it is unclear whether the policy raises medical bill defaults in the first place. Eliminating the credit consequences of medical debt weakens repayment incentives, but it also improves past defaulters' access to credit and liquidity, which may raise their ability to pay future bills. Second, even if collections decline, hospitals may not pass the burden to patients. Unlike profit-maximizing firms, most hospitals are nonprofits that value patient access and may continue providing care even when it is unprofitable (Garthwaite et al. 2018). Modest collection losses may therefore be absorbed rather than passed on to patients, while larger shocks may force hospitals to trade off their charitable objectives against financial sustainability. Whether the policy generates a sufficiently large financial shock to alter hospital

behavior, and which hospitals are most likely to respond, are therefore empirical questions.

One major challenge in assessing the effects of credit reporting bans is the lack of detailed household and patient-level data that captures medical bill payment and hospitals' operational policies over time. To track medical bill payments, I use comprehensive hospital financial information from the Centers for Medicare & Medicaid Services (CMS) Cost Reports alongside the nationally representative, longitudinal Survey of Income and Program Participation (SIPP). To examine hospital responses, I use the Merative™ MarketScan® Research Database, which contains commercial insurance claims for 39.7 million individuals, roughly 14% of the insured population, and is broadly representative of insured Americans (Cornaggia et al. 2024, Ouimet et al. 2025). For each individual, I observe every hospital visit, including all diagnoses, procedures, and negotiated prices charged to insurance and patients. This granularity lets me measure hospital responses on both the extensive margin (changes in admission patterns across patient and service types) and the intensive margin (the prices charged for each procedure).

My empirical strategy exploits the staggered introduction of medical debt credit reporting bans in Colorado and New York, which generates cross-state variation in whether delinquent medical bills can be reported to credit bureaus. Following Wing et al. (2024), I implement a stacked difference-in-differences design that addresses the biases of two-way fixed effects estimators under staggered treatment adoption (Goodman-Bacon 2021, Callaway & Sant'Anna 2021). To address concerns about preexisting differences between adopting and non-adopting states, for each treated hospital and MSA, I construct a cohort of closely matched control observations based on pre-policy hospital characteristics and local socioeconomic conditions. The granularity of the MarketScan® data allows me to extend this matching to the patient level: I further compare individuals with the same diagnosis,[2] age, gender, industry, and

[2]Specifically, individuals within the same Diagnosis-Related Group (DRG), a standardized, rule-based classification based on clinical conditions and expected treatment needs.

employment type (e.g., full-time vs. part-time), differing only in whether they reside in a treated state. After matching, treated and control units exhibit no significant differences across a wide range of hospital and location-based outcomes in the pre-treatment period.

This paper has four main findings. First, credit-reporting bans increase hospitals' uncollectible medical bills by 8.6%, or approximately $1.21 million per hospital-year. This effect is economically meaningful because 41% of hospitals had negative profit margins before the policy. Evidence from the SIPP survey corroborates this interpretation: households become more likely to default on medical bills (extensive margin) and accumulate larger unpaid balances (intensive margin), while overall healthcare utilization remains unchanged. These effects are concentrated among low- and middle-income households, consistent with medical debt falling in repayment priority once the credit penalty is removed, especially for households with tighter liquidity constraints.

Second, hospitals tighten insurance screening after the reporting bans, reflecting concerns about serving patients with less generous coverage and higher out-of-pocket obligations. Using claim-level data, I measure insurance generosity by patients' deductibles, copayments, and coinsurance. Among elective admissions,[3] where hospitals have discretion over patient selection, the share of patients with less generous coverage falls by 4.3 percentage points in treated states, or 16% of the mean. This shift suggests that patients with greater out-of-pocket obligations have reduced access to inpatient care due to higher perceived default risk. By contrast, I find no effect on emergency or urgent admissions, for which hospitals face legal constraints on screening patients based on insurance generosity.

Third, hospitals cut unprofitable services to protect their profit margins. Inpatient admissions for acute medical and behavioral conditions decline by 11% in treated states following

[3]I classify elective admissions as scheduled, non-emergency inpatient surgeries, such as knee replacements, which hospitals may delay or cancel for patients perceived to pose collection risk (e.g., University of Kansas Health System 2022). I classify emergency or urgent admissions as emergency surgeries, acute medical conditions, and behavioral crises; most originate in the emergency department and are subject to EMTALA, while the remainder are generally governed by rules that prioritize clinical need over financial considerations.

the policy. These admissions are often loss-generating, and weaker bill collection makes them even more costly to provide.[4] An alternative explanation is that the decline reflects lower demand: by easing collection pressure, the ban may reduce financial stress and the need for admission,[5] in which case the decline would be a benefit rather than a cost of the policy. Two patterns instead indicate that the decline reflects a supply-side response: hospitals raise inpatient admission thresholds. Outpatient and ER visits leading to these admissions remain unchanged, so the pool of patients seeking care did not shrink, while the decline is concentrated among lower-severity diagnoses, for which hospitals have greater discretion to avoid admission. These adjustments are costly to patients: ER revisits within one month increase by 3%, consistent with delayed or incomplete care when hospitals forgo clinically warranted but unprofitable admissions.

Fourth, hospitals raise insurer-negotiated prices following the reporting bans, but only where they hold bargaining power. Hospital prices are determined through bilateral negotiations, and hospitals with greater bargaining leverage can secure more flexible contract terms, enabling them to more readily pass revenue losses on to insurers (Cooper et al. 2019). In concentrated markets, inpatient admission charges rise by \$749 (3.6%) and patients' out-of-pocket charges by \$34 (2.6%) through patients' cost sharing, while less concentrated markets show no price response. This suggests that hospitals with bargaining leverage thus recoup part of their collection losses from insurers, with patients bearing part of the burden.

An important question is whether hospitals' responses generate distributional effects, particularly given that the policy is intended to protect financially vulnerable households. I show that disadvantaged households bear a disproportionately higher burden of healthcare access through two channels. First, patients with less generous insurance, those most affected

[4]The average margins for behavioral health and acute medical admissions (e.g., infectious disease) are -34.3% and -15.3% (American Hospital Association 2024). In contrast, elective surgeries (e.g., knee replacement and cardiovascular procedures) generate profit margins of 15% to 25% (Tsang 2025).

[5]E.g., Dobbie & Song (2015) and Currie & Tekin (2015) document that financial distress is associated with worse health outcomes.

by tighter insurance screening, tend to be socioeconomically vulnerable. Specifically, I find these patients are more likely to reside in disadvantaged areas with lower incomes, higher uninsured rates, and greater medical debt prevalence.[6] This pattern aligns with evidence that low-income households are less likely to afford generous coverage or work for firms offering comprehensive plans (McMorrow et al. 2011). Second, hospitals in disadvantaged areas respond more aggressively because their patient base is more resource-constrained and thus more prone to default after the policy. Together, these patterns suggest that the policy imposes its highest costs on the very households it was designed to protect.

My findings further reveal that even financially low-risk patients, those with generous insurance and low out-of-pocket exposure, face reduced access to unprofitable admissions and higher prices when residing in disadvantaged areas. Although these patients are unlikely to respond directly to credit reporting bans, they are affected through within-hospital spillovers. Hospitals cannot selectively reduce unprofitable admissions only for financially risky patients, as most of these admissions originate in the emergency room, where screening is limited. Nor are hospitals willing to raise prices only for high-risk patients, as higher prices would likely exacerbate default. As a result, low-risk patients bear higher healthcare burdens not because of their own likelihood of default, but because they are treated by hospitals serving a larger share of financially vulnerable patients. These spillovers broaden the policy's unintended consequences and concentrate them in disadvantaged communities.

Having shown that the policy's costs are concentrated among lower-income patients and communities, I next ask which hospital characteristics determine whether collection losses are absorbed or passed on to patients. This heterogeneity matters for policy because it identifies where additional support may be most effective in preserving access while maintaining consumer debt protections. Financially fragile hospitals, measured by fewer pre-policy days

[6]Pre-shock, a 10% decrease in MSA median income is correlated with $35 increase in inpatient out-of-pocket medical bills, or 2.2% of the mean.

of cash on hand, respond more aggressively, consistent with limited liquidity making revenue losses harder to absorb. Responses are also stronger among non-profit hospitals, which enter the policy with thinner margins and greater exposure to patients in settings where financial screening is limited; they experience larger increases in defaults and respond correspondingly more. For-profit hospitals, by contrast, appear to have already limited these risks and adjust less. Together, these findings show that hospitals' financial capacity and ownership shape the supply-side consequences of reporting bans, and point to targeted support for uncompensated care at financially fragile or non-profit hospitals as the most effective complement to these protections.

Contribution to the literature: First, the paper contributes to the literature evaluating medical debt protection policies. Using randomized experiments, Kluender et al. (2021) find that medical debt relief improves credit access but has no effect on healthcare utilization. Duarte et al. (2025) documents that deleting medical debt under $500 from credit reports has no effect on consumers' non-medical credit outcomes, while Won & Loutskina (2025) shows that frictions in obtaining medical debt information lead lenders to restrict credit access. Existing work has largely evaluated these policies through their effects on consumers in credit markets, holding the healthcare market fixed. This paper instead highlights an important supply-side response: by weakening households' incentives to repay medical bills, these policies increase hospitals' revenue losses, prompting them to restrict access to care and raise prices. Accounting for these endogenous provider responses is therefore essential for assessing the policies' net welfare effects, particularly because the resulting costs fall disproportionately on the households they are intended to protect.

Second, the paper relates more broadly to research on the role of credit information. Removing delinquency or bankruptcy records can improve credit access and labor market outcomes for past defaulters (Musto 2004, Bos & Nakamura 2014, Miller & Soo 2020, Herkenhoff et al. 2021, Jansen et al. 2022), but can also affect lending and hiring when negative

records contain information used to screen consumers (Bos & Nakamura 2014, Liberman et al. 2018, Maturana et al. 2020). This paper highlights a distinct role of credit reporting emphasized by theory: sustaining repayment incentives (Padilla & Pagano 2000, Elul & Gottardi 2015, Chatterjee et al. 2023). Medical debt provides a useful setting for studying this role because hospitals generally do not use credit reports to screen patients, so reporting bans do not mechanically reduce the information available to providers when care is supplied. Instead, they weaken the consequences of nonpayment. I show that this disciplinary role is economically important in equilibrium: when credit-reporting penalties are removed, medical defaults rise and the resulting losses feed back into hospitals' supply of care. Thus, even if medical debt were a noisy signal for subsequent lenders, as argued in support of recent reporting restrictions (CFPB 2024), removing it could still have important consequences by weakening repayment incentives.

Third, this paper contributes to the literature on how hospitals respond to financial shocks. Prior work shows that hospitals adjust investment, service provision, and pricing in response to shocks to investment returns, asset values, liquidity, and debt burdens (Adelino et al. 2015, Dranove et al. 2017, Adelino et al. 2022, Aghamolla et al. 2024, Bourveau et al. 2023, Liu 2022, Aghamolla et al. 2023). Relatedly, the cost-shifting literature documents price responses to reductions in government reimbursement (Dranove 1988, Robinson 2011, Frakt 2013, White & Wu 2014). This paper studies a distinct financial shock arising from the patient side of hospitals' balance sheets: weaker repayment incentives increase losses on patient receivables. I show that hospitals respond not only by raising prices, but also by reallocating care away from patients and services with greater collection risk. Unlike broad balance-sheet shocks, this shock is concentrated among financially vulnerable patients, causing hospitals' responses to shift part of the resulting burden back onto the households whose payment protections generated the shock.

2 Background and Conceptual Framework

2.1 The Role of Credit Reporting in Collecting Medical Bills

In the U.S. health care system, hospitals typically do not collect full payment up front for most services they render (Mahoney 2015). Instead, they provide care first and bill patients (or their insurers) later, creating accounts receivable subject to default risk. This deferred payment model arises in part because many patients lack the liquidity to pay in full at the time of service and because billing must be coordinated with insurers, whose reimbursement schedules are complex and delayed. Moreover, federal law requires hospitals to provide emergency treatment regardless of ability to pay, further limiting hospitals' capacity to secure upfront payment.[7] As a result, providers face substantial uncertainty in revenue realization and rely heavily on post-service billing and collection practices.

Hospitals frequently rely on assigning or selling delinquent medical bills to third-party collectors, often within 90 to 180 days of nonpayment. At that stage, hospitals' ability to recover revenue depends largely on the effectiveness of collection agencies in securing payment. Debt collectors, in turn, make extensive use of credit reporting as a mechanism to pressure repayment. According to the CFPB, medical collections have historically been among the most common types of debts reported to credit bureaus, at one point accounting for over half of all collection tradelines on consumer credit reports (CFPB 2014). More recent data show that while not all medical collections are reported, a substantial share still appear on credit files: The Commonwealth Fund (2025) estimates that, on the lower bound, 42% of individuals with unpaid medical bills also had those debts reported to a credit bureau, reflecting the continued importance of credit reporting as a collection tool.

[7] Recent survey evidence shows that only about 20% of hospitals require insured patients to pay cost-sharing amounts upfront for elective procedures (Randall et al. 2024). Besides, Ippolito & Vabson (2025) shows that only 14.5% of payments from privately insured patients are collected upfront for elective services from 2018 to 2023, underscoring that most medical bills are not paid until well after care is delivered.

Consistent with the disciplinary role of credit information (Padilla & Pagano 2000, Elul & Gottardi 2015, Chatterjee et al. 2023), reporting medical debt to credit bureaus raises the cost of default and strengthens repayment incentives. Medical collection tradelines are used by lenders to allocate and price credit and can materially reduce borrowers' access to credit (Dobkin et al. 2018, Brevoort et al. 2020, CFPB 2022, Kluender et al. 2025). Consumers therefore have strong incentives to avoid medical collections, especially when they expect to borrow in the future (Brown & Zehnder 2007, Liberman 2016). For example, Liberman (2016) finds that consumers are willing to pay up to 11% of monthly income for a cleaner credit report.

Although debt collectors employ a range of collection tactics beyond credit reporting, such as repeatedly contacting consumers by phone, sending collection letters, or pursuing lawsuits, these methods are often less effective or prohibitively costly. Telephone and mail outreach depend on the debtor's willingness to respond and may be ignored or disputed. Litigation, while legally enforceable, entails substantial legal expenses, court fees, and reputational risks. Moreover, regulatory constraints limit the scope of these strategies: the Fair Debt Collection Practices Act (FDCPA) restricts the frequency and timing of collection contacts, while state-level protections further curtail the use of aggressive collection practices (Consumer Financial Protection Bureau 2017, Commonwealth Fund 2025). By contrast, furnishing delinquent accounts to credit bureaus imposes an immediate and lasting credit penalty on patients without the high transaction costs of litigation. This regulatory and economic landscape underscores why credit reporting has historically served as one of the most powerful tools available to hospitals and collection agencies in pressuring repayment of medical debts.

2.2 Hospitals' Responses to Credit Reporting Restrictions

Reduce Collection Risk Exposure: Credit-reporting restrictions operate through patients' out-of-pocket obligations, making patients with higher OOP exposure more finan-

cially risky for hospitals. When the probability of nonpayment rises, hospitals lose more on patients who owe larger amounts out of pocket: for the same increase in default risk, a larger patient balance translates into a larger expected revenue loss. Hospitals may therefore respond by screening more aggressively on insurance generosity and shifting elective admissions toward patients with lower OOP exposure. Although EMTALA limits such screening for emergency care, hospitals retain substantial discretion over elective admissions, where financial screening is common.

Hospitals can discourage or defer elective care for financially risky patients through several mechanisms. For example, the University of Kansas hospital system classifies patients whose upcoming OOP exceeds $5,000 as "underinsured", describing this as a key "financial risk" (University of Kansas Health System 2022). The policy states that when financial risks are identified, "the patient's service may be deferred until financial clearance can be achieved." Hospitals can also implicitly limit access for less-insured patients by denying care to those with existing medical debt or requiring upfront payment. Because less-insured patients are more likely to carry medical debt and to cancel scheduled admissions when faced with upfront costs (U.S. Census Bureau 2021, Kaufman et al. 2006), such policies may disproportionately restrict their access to care. A recent New York Times article documents a nonprofit hospital system that "cuts off patients with medical debt" by embedding restrictions directly into its scheduling system (Kliff & Silver-Greenberg 2023). Similarly, Valley Medical Center's financial policies require patients with high deductibles to "establish a payment arrangement that includes a deposit of at least 50% of the allowed amount" (Valley Medical Center 2025). Hospitals can also reduce exposure to less-insured patients by dropping insurance plans that predominantly cover these individuals.[8] Together, these strategies allow hospitals to tilt their patient mix toward better-insured individuals and buffer the financial impact of weakened

[8] A 2013 survey found that 23.5% of physician practices chose not to participate in ACA exchange plans, and many cited concerns that high deductibles and patient nonpayment risk made these plans financially unattractive (Holan 2014).

medical bill payment incentives.

Enhance Revenue: When legal and administrative constraints limit hospitals' ability to adjust patient mix through stricter insurance screening, hospitals may turn to revenue-enhancing strategies to offset revenue losses caused by rising medical bill defaults. One such strategy is to scale back unprofitable services. In principle, hospitals may be willing to offer low- or negative-margin services when profits from high-margin services can cross-subsidize them, especially if hospitals place some weight on patient welfare rather than solely maximizing profits. However, an increase in unpaid medical bills can disrupt this balance by worsening the losses associated with low-margin services. As a result, hospitals may reduce the provision of these services to remain financially sustainable.

There is substantial variation in hospital profit margins across service lines. In inpatient settings, surgical admissions are often considered profit leaders, with average margins ranging from 15% to 25%. In contrast, admissions related to acute medical conditions, such as sepsis and pneumonia, and behavioral conditions, such as psychiatric disorders and substance use, frequently generate negative margins ranging from -35% to -15% (Melnick et al. 1989, Horwitz et al. 2021, American Hospital Association 2024, Tsang 2025). One reason driving these differences is that medical and behavioral admissions are more likely to occur on an urgent or unplanned basis, making costs less predictable and controllable under relatively inflexible case-based reimbursement. By contrast, surgical admissions are typically more standardized and often rely on specialized technologies and physician expertise, which can significantly reduce costs and strengthen hospitals' bargaining power with private insurers and support higher margins (Horwitz et al. 2021).

One way hospitals may reduce less profitable inpatient care is by raising the threshold for inpatient admission. Instead of admitting patients for a multi-day inpatient stay, hospitals may treat them in outpatient settings or place them in observation beds. Hospitals facing shocks to medical bill collections may be especially likely to adjust along this margin because

patients' out-of-pocket costs are much lower in outpatient settings than in inpatient settings.[9] Prior research suggests that hospitals do have discretion over this threshold. For instance, Shi (2024) shows that hospitals raise admission thresholds when auditing risks increase.

Besides restricting admissions, hospitals may raise insurer-negotiated prices for the broader patient pool to offset losses from financially riskier patients. In the theoretical framework of Dranove (1988), a non-profit hospital values both profit and patient volume, so it sets prices below the profit-maximizing level to attract more patients, deliberately leaving some profit unclaimed. A financial shock raises the marginal value of revenue, and the hospital responds by moving prices toward the profit-maximizing level to restore its financial position (Dranove et al. 2017). These price increases would necessarily focus on privately insured patients, since hospitals have very limited flexibility to adjust prices for Medicare or Medicaid patients. The ability and intensity of such price adjustments depend on a hospital's market power and its bargaining position with insurers.

Why hospitals may not respond: Whether credit-reporting restrictions induce hospitals to change admissions or prices is unclear for two reasons. First, the equilibrium effect on medical bill repayment is ambiguous. Removing medical collections from credit reports may weaken repayment incentives, but it can also restore credit access and liquidity to past defaulters, improving their ability to pay future bills. For example, Kluender et al. (2021) finds improved credit access among patients whose medical debt is relieved and no longer reported to credit bureaus. Patients also remain subject to other collection pressures, such as collection calls and lawsuits, so the extent to which the perceived cost of default falls is uncertain. If improved credit access instead makes repayment more reliable, hospitals may have incentives to expand rather than restrict access. Second, even if defaults rise, hospitals may absorb the resulting losses. Most hospitals are non-profits that balance fi-

[9]Based on the MarketScan® sample, the median OOP for an ER visit is $150 and the $1,686 for an inpatient admission.

nancial sustainability against patient access and may therefore tolerate moderate revenue losses without profit-seeking adjustments (Garthwaite et al. 2018). Only a sufficiently large financial shock may induce profit-restoring. Whether the reporting bans generate a shock large enough to cross this threshold is therefore unclear ex ante. For-profit hospitals, by contrast, may already screen aggressively to limit collection risk, leaving little room for further adjustment. Thus, both whether hospitals respond and how strongly they respond are ultimately empirical questions.

3 Data and Empirical Strategy

3.1 Data

I obtain hospital-level information from CMS cost reports. Medicare-certified hospitals are required to submit annual cost reports to CMS, which are then compiled into a standardized dataset under the Healthcare Cost Report Information System (HCRIS). A key measure in my analysis is hospitals' reported bad debt, which is defined as "the charges for health services for which a hospital determines the patient has a financial responsibility to pay, but the patient does not pay." This measure is reported net of recoveries from all collection efforts, including third-party collections, and therefore captures the amount ultimately deemed uncollectible from patients. I construct a hospital-by-year panel covering the fiscal years 2016 to 2024, with the latest fiscal year ending June, 2025. The sample includes 239,11 hospital-year observations from 3,152 hospitals.

Because reported hospital bad debt may also reflect endogenous hospital-side responses, such as changes in bad debt write-off and recording policies, I supplement the analysis with data from the 2018 to 2024 SIPP. This dataset is well suited to my setting for three reasons. First, it provides rich individual-level information on household balance sheets, including medical debt, demographics, income, and healthcare utilization. Second, its longitudinal

design allows me to track the medical debt status of the same households over time. Third, its nationally representative sample allows me to examine how medical debt evolves across treated and control states in a way that reflects the broader U.S. population.

The sample begins in 2018, the first reference period in which the SIPP reports medical debt, and extends through 2024, the most recent available reference period. I restrict the sample to panelists with complete information on medical debt and income for at least two years, which allows me to exploit within-individual variation over time. The final sample includes 34,769 households and 67,617 individuals nationwide. Of these, 1,580 and 2,937 individuals ever reside in Colorado and New York, respectively.

The dataset I use to study hospitals' admission and pricing policies comes from the Merative™ MarketScan® Research Database from 2022 to 2024, which contains claim-level reimbursement records for privately insured patients. The database is sourced from a large collection of employers and health insurance providers. It provides detailed information on the episodes of hospital visits, including the date and location of each visit at the MSA level, patient demographics, diagnoses at the Diagnosis-Related Group (DRG) level, and medical procedures performed during the hospital stay. In addition, the data include granular financial variables, such as the amounts paid by insurers to hospitals and the portions owed by patients for each claim. Patient obligations are further broken down into copayments, deductibles, and coinsurance.

Speaking of representativeness, Cornaggia et al. (2024) documents that the MarketScan® data covers roughly 20% of the privately insured and 14% of all insured in the U.S. population. Dunn et al. (2013, 2014), Schwartz et al. (2021) and Ouimet et al. (2025) argue that the data is representative of the privately insured population in age, sex, and medical spending. Figure 1 shows the geographical coverage of the MarketScan® data. Appendix Figure A1 benchmarks the monthly trends of the MarketScan®'s inpatient admissions to the population-level State Inpatient Database (SID) from the Healthcare Cost and Utiliza-

tion Project (HCUP). The MarketScan® series closely tracks the SID series, suggesting no serious selection issues across time.

After excluding observations with missing location or patient-related characteristics (approximately 5% of the raw sample), the MarketScan® sample contains 1.45 million inpatient hospital admissions from 1.15 million unique individuals and 332.46 million outpatient visits from 20.18 million unique individuals. To examine household utilization and hospitals' supply-side adjustments, I draw on both inpatient admissions and outpatient visits. For the analysis of hospitals' insurance screening and pricing patterns, however, I focus on inpatient admissions, as these admissions account for the most expensive procedures and constitute hospitals' primary source of revenue (Gao et al. 2025); they are therefore the most salient margin along which hospitals' financial concerns are likely to translate into pricing and selection behavior.

3.2 Credit Reporting Bans in Colorado and New York

Colorado's House Bill 23-1126 took effect on August 7, 2023. The law prohibits consumer reporting agencies from including adverse medical debt information in consumer reports, with a narrow exception for credit transactions exceeding the Federal Housing Finance Agency's conforming loan limit for a one-unit property (effectively preserving the use of medical debt only in jumbo-credit settings). Importantly, the law also requires debt collectors, in their initial written communication, to disclose that "Colorado law prohibits credit bureaus from reporting medical debt or factoring medical debt into a credit score" (Colorado General Assembly 2023). This statutory disclosure ensures that consumers with unpaid medical bills are directly informed of the reduced credit consequences of nonpayment, and industry filings in a federal challenge to the law confirm that collection agencies materially revised their contact campaigns, disclosures, and employee training in response (ACA International and Fresno Credit Bureau 2025).

New York adopted a similar but broader reform through Senate Bill S4907A, signed by Governor Hochul on December 13, 2023, and effective immediately. The law prohibits hospitals, health care professionals, and ambulance services from furnishing any portion of a medical debt to a consumer reporting agency, requires contracts with collection entities to prohibit such reporting, bars consumer reporting agencies from reporting medical debt regardless of when it was incurred, and renders any reported medical debt void. The reform was signed at a publicized ceremony, received extensive national media coverage (Lee 2023), and was accompanied by coordinated public-awareness campaigns from major consumer-advocacy organizations (Community Service Society of New York 2023).

Together, these laws create a useful institutional setting for studying how hospitals respond when credit reporting becomes ineffective in enforcing medical bill repayment. Relative to the recent nationwide credit bureau reforms that exclude only smaller medical debts, these state laws impose much sharper restrictions, and their salience, through statutory disclosure in Colorado and a high-profile rollout in New York, mitigates concerns that the policies' effects could be muted by limited consumer awareness.

3.3 Empirical Setting

Matching: To address concerns that the raw sample is imbalanced and that differences between hospitals and households in treated and control states may generate differential outcome trends, I implement a matching strategy to improve comparability in the pre-shock period. To construct clean control groups, ever-treated observations are excluded from serving as controls in the matching procedure.

As a first-stage analysis of the credit reporting bans, I examine their effect on hospitals' uncollectible medical bills using the hospital sample constructed from the CMS cost reports. I match each treated hospital in Colorado and New York to three hospitals in other states using Euclidean distance based on a set of pre-treatment characteristics. These matching

variables include ownership status (for-profit or nonprofit), number of beds, profit margin (net income divided by net patient revenue), bad debt, and charity care charges. Before computing Euclidean distance, I normalize all matching variables to lie between 0 and 1 so that differences in scale do not disproportionately affect the matching weights. To ensure consistent fiscal year timing within matched cohorts, I restrict control hospitals to have fiscal year ends within $\pm$ 3 months of the treated hospital. I allow control hospitals to be matched to multiple treated hospitals. The final matched hospital sample includes 163 treated hospitals and 332 unique control hospitals.

I use the MarketScan® data to examine hospitals' responses on admission and pricing policies. Because the MarketScan® data do not report hospital identities for confidentiality reasons, I instead match treated and control observations at the metropolitan statistical area (MSA) level, which is the most granular geographic unit available in the data. I exclude MSAs with fewer than 250 inpatient admissions in the pre-treatment period to avoid excessive fluctuations in outcomes driven by small sample sizes. For each treated MSA, I match five control MSAs using nearest-neighbor matching based on Euclidean distance. The matching variables include MSA-level median income, the uninsured share, black population share, and the total number of inpatient admissions. After matching, the sample comprises 351k inpatient admissions from 283k unique individuals and 100.79 million outpatient visits from 5.59 million unique individuals.

Figure 2 presents the matching performance for the hospital sample. Panel B shows that, after matching, the p-values for differences in means between treated and control groups all exceed the 5% threshold, suggesting that the matched groups are no longer statistically different along these dimensions. Because p-values can be sensitive to sample size, I also assess balance using standardized differences, defined as the absolute difference in means divided by the pooled standard deviation. Standardized differences provide a scale-free measure of balance that is not mechanically driven by sample size. Austin (2009) suggests

that covariates are generally well balanced when the standardized difference is below 0.1 or, under a more lenient threshold, below 0.25. Panel A shows that matching substantially improves balance: 12 of the 15 hospital characteristics have standardized differences below 0.1, and 14 of the 15 are below 0.25. The MSA-level matching results in Figure 3 show a similar pattern. Matching substantially increases the p-values for mean differences and reduces standardized differences across a range of MSA characteristics, making treated and control groups much more comparable.

Stacked difference-in-differences: To address the well-documented biases of two-way fixed effects estimators under staggered treatment timing (Goodman-Bacon 2021, Callaway & Sant'Anna 2021), I implement a stacked difference-in-differences design following Wing et al. (2024). For each reporting ban event $e \in \{\text{Colorado}, \text{New York}\}$, I construct a separate event-specific dataset consisting of matched cohorts c, where each cohort contains one treated unit (hospital or MSA) and its matched control units drawn from never-treated states as described in the previous section. The two event datasets are then stacked into a single stacked panel for estimation. As described above, restricting controls to never-treated units ensures that all comparisons are made against "clean controls" that are themselves unaffected by the policy. For example, a Colorado hospital treated in August 2023 is compared only to matched hospitals in states that never adopted a medical debt reporting ban during the sample window, and likewise for New York's December 2023 reform.

To estimate the effects of credit reporting bans in Colorado and New York on hospitals' aggregate bad debt, I use hospital-by-year observations from CMS cost reports based on the matched sample. The pre-treatment period spans fiscal years 2016–2022, and the post-treatment period covers fiscal years 2023–2024. Because cost reports are defined over hospital-specific fiscal years, I map fiscal years to policy exposure separately for each stacked dataset (Colorado and New York).

I define the first treated fiscal year (event time zero) as the fiscal year ending at least

five months after policy implementation. For example, a treated hospital with a fiscal year running from January to December 2023 is assigned event time zero for the Colorado policy (implemented in August 2023), but event time -1 for the New York policy (implemented in December 2023).[10]

Specifically, the following specification examines the effect of the policy on hospital outcomes:

$$Y_{i,t,e,c} = \beta\, Treated_{i,e} \times Post_{i,t,e} + \alpha_{i,c} + \delta_{t,c} + X_i^{\top} \lambda_{t,e} + \varepsilon_{i,t,e,c} \tag{1}$$

where i indexes hospitals, t fiscal years, e reporting ban events, and c matched cohorts. $Treated_{i,e}$ is an indicator for whether hospital i is located in a treated state (Colorado or New York), and $Post_{i,t,e}$ indicates whether the hospital's fiscal year t is exposed to the policy (i.e., the fiscal year ends at least five months after the policy's effective date). The fixed effects $\alpha_{i,c}$ and $\delta_{t,c}$ denote hospital $\times$ matched-cohort and year $\times$ matched-cohort fixed effects, respectively, ensuring that identification comes from within-cohort comparisons between treated and control hospitals over time. The term $X_i^{\top} \lambda_{t,e}$ denotes control-bin $\times$ year $\times$ reporting ban event fixed effects, constructed by separately interacting year $\times$ event effects with indicators for whether hospital i's county has above-median pre-shock household income or uninsured rates. These fixed effects absorb event-specific time trends that are common to hospitals operating in similar local socioeconomic environments, which could otherwise generate differential trends in medical bill payments between treated and control hospitals. The coefficient of interest, β, captures the average change in outcomes for hospitals in treated states relative to matched control hospitals in the same cohort, before versus after the credit reporting ban. Beyond hospital-level aggregate bad debt, I also examine individual-level

[10] I use the five-month cutoff since most hospitals' fiscal year ends in December, exactly about five month Colorado's policy implementation month. Changing the cutoff to four or six months does not impact the result.

unpaid medical bills using the SIPP data. Appendix Section A describes the empirical specification.

Using MarketScan® data, I examine inpatient and outpatient volume, insurance composition, hospitalization prices, and repeated ER visits as outcome variables. I define five-quarter pre- and post-treatment windows around each policy shock, including the implementation quarter in the post-treatment period.[11] To study inpatient admission and outpatient visits volume on the extensive margin, I aggregate encounter data to the MSA-by-quarter level, and use the following specification:

$$Y_{m,t,e,c} = \beta \, Treated_{m,e} \times Post_{t,e} + \alpha_{m,c} + \delta_{t,c} + \varepsilon_{m,t,e,c} \tag{2}$$

where m indexes MSAs, t quarters, e reporting ban events, and c matched cohorts. $Treated_{m,e}$ is an indicator for whether MSA m is located in a treated state (Colorado or New York), and $Post_{t,e}$ indicates whether quarter t occurs after the policy implementation. The fixed effects $\alpha_{m,c}$ and $\delta_{t,c}$ denote MSA $\times$ matched-cohort and quarter $\times$ matched-cohort fixed effects, respectively, ensuring identification from within-cohort comparisons across MSAs over time. The coefficient of interest, β, captures the average change in quarterly admission and visit volumes for treated MSAs relative to matched control MSAs in the same cohort, before versus after the credit reporting ban.

I leverage hospital visit episodes and patient-level information from the MarketScan® data to examine the effects of the policy on insurance screening, prices, and repeated ER visits. This specification allows me to incorporate granular controls for patient characteristics and clinical conditions:

$$Y_{i,m,t,e,c} = \beta \, Treated_{i,m,e} \times Post_{t,e} + \alpha_{m,c} + X_i^{\top} \delta_{t,c} + \varepsilon_{i,m,t,e,c} \tag{3}$$

[11] I use a five-quarter window because it is the longest balanced post-treatment window available for both policy events given that the MarketScan® sample ends in December 2024.

where i indexes hospital visit episodes, m MSAs, t quarters, e reporting ban events, and c matched cohorts. $Treated_{i,m,e}$ indicates whether visit i occurs in a treated state (Colorado or New York), and $Post_{t,e}$ indicates whether the quarter of the visit t occurs after policy implementation. $\alpha_{m,c}$ denotes MSA $\times$ matched cohort fixed effects, absorbing cohort-specific time-invariant heterogeneity across MSAs. $X_i^\top \delta_{t,c}$ denotes several separate sets of quarter $\times$ matched-cohort $\times$ patient-type fixed effects, where X_i contains indicators for patient-type categories: age decile, gender, industry, employment status (full-time, part-time, salaried, or hourly), and Diagnosis-Related Group (DRG). These fixed effects ensure that treated and control hospital visits are compared within the same cohort and quarter, and among patients with similar demographics or clinical conditions. Regarding hospital pricing outcomes, I further control for patients' emergency status and insurance plan type (e.g., High-deductible, PPO, HMO). The purpose of adding these additional controls is to isolate pricing effects by netting out composition shifts across patients' insurance plan generosity and emergency status induced by hospitals' strategic responses.[12] The coefficient of interest, β, captures the average change in hospital visit outcomes in treated MSAs relative to matched control MSAs in the same cohort and patient type, before versus after the credit reporting ban.

The underlying identification assumption is that, conditional on the control variables and fixed effects included in the above specifications, the outcome variables would have evolved in parallel in the absence of the credit reporting bans. To provide evidence supporting this assumption, I estimate the following dynamic specification:

$$Y_{i,m,t,e,c} = \sum_{\substack{\tau=-K \\ \tau\neq -1}}^{K} \beta_\tau \, Treated_{i,m,e} \times D_{t,\tau,e} + \alpha_{m,c} + X_i^\top \delta_{t,c} + \varepsilon_{i,m,t,e,c} \tag{4}$$

where $Y_{i,m,t,e,c}$ denotes the outcome for unit i in MSA m at time t, within event sample e

[12] Insurance type and emergency status are not included in the insurance-generosity regressions, as the sample is already split along these two margins and the variation they capture is subsumed by the dependent variable.

and matched cohort c, and $D_{t,\tau,e}$ equals one if period t is τ periods from event e's effective date, and zero otherwise. Equation (4) is displayed for the admission episodes level specification; The dynamic analogues of Equations (1) and (2) replace $Treated \times Post$ with $\sum_\tau Treated \times D_{t,\tau,e}$, all else unchanged; event time for Equation (1) is in fiscal years. The period immediately before the policy ($\tau = -1$) is omitted as the reference. The coefficients β_τ trace out the dynamic effects of the reporting ban relative to the period before the policy takes effect, with pre-period coefficients ($\tau < -1$) serving as a diagnostic for parallel trends.

Standard Errors: Across all the above specifications, standard errors are clustered at the state level, which is the level of treatment. In Table A11, I show that the results are robust to clustering at the MSA level and to two-way clustering by MSA and time (year or quarter), which corrects for autocorrelated residuals at the MSA and time levels. Following MacKinnon & Webb (2020), Figure A4 shows that the statistical significance of the baseline results is robust to a randomization permutation test, which addresses the concern of a small number of treated states.

4 Results

4.1 Summary Statistics

Table 1 reports summary statistics for the matched sample in the pre-treatment period. Panel A presents hospital-level characteristics from the CMS Cost Reports, while Panel B summarizes admission and pricing outcomes constructed from the MarketScan® data.

Bad debt refers to uncollectible OOP obligations from patients across inpatient and outpatient facilities. Total patient revenue refers to gross charges for patient care before contractual adjustments and bad debt write-offs, while net patient revenue reflects payments received after these adjustments. The average hospital reports total patient revenue of $1.63 billion and net patient revenue of $426 million, with substantial dispersion across facilities.

Bad debt averages $14.1 million, corresponding to 1.10% of total patient revenue. Net income is modest on average at $21.8 million, with the 25th percentile -$4.5 million, indicating that a sizable fraction of hospitals operate with low or negative margins. Consistent with this, the interquartile range of net income spans from -2.6% to 10.7% of net patient revenue. Hospitals are moderate in size, with an average of 244 beds and approximately 11,700 annual inpatient discharges.

Panel B reports admission and pricing measures at the MSA-by-quarter levels. Following Cornaggia et al. (2025), inpatient admission and outpatient visit volumes are scaled by the number of insured individuals in the MarketScan® data to account for the coverage of the MarketScan® and the fluctuations in insurance enrollment. On average, there are 3.79 inpatient admissions per 1,000 enrollees per quarter per MSA,[13] of which the majority are non-surgical (2.83), followed by elective surgical (0.58) and emergency surgical admissions (0.37). Outpatient visits are substantially higher, with an average of 2,264 visits per 1,000 enrollees per quarter, while emergency room visits average 71 per 1,000 enrollees. I define a repeated ER visit as a return visit by the same patient to the ER within a specified time window following an initial ER visit. In the baseline sample, 8.5 percent of initial ER visits are followed by at least one repeated visit within a 20- to 40-day window, with an average of 0.146 repeated visits per initial visit.

At the admission level, average total charges are $24,080, with a median of $16,810, indicating a right-skewed distribution. Out-of-pocket (OOP) charges average $1,351, representing approximately 10% of total charges on average. The distribution of OOP payments is also skewed, with a median share of 4.8%. Decomposing OOP spending, coinsurance accounts for the largest component ($850 on average), followed by deductibles ($411) and copayments ($71). A substantial share of admissions involve zero copayments or deductibles,

[13]Following Aghamolla et al. (2024), I exclude childbirth inpatient admissions, as these are less manipulable by hospitals. A placebo test shows no change in either their volume or insurance composition.

reflecting substantial heterogeneity in patients' cost-sharing structures.

4.2 Hospitals' Bad Debt

Table 2 reports the regression results examining the effects of the reporting bans on hospitals' bad debt using Equation (1). In Column (1), the dependent variable is bad debt scaled by total revenue. The denominator proxies for hospital size and the volume of services exposed to medical bill collection risk. Column (2) uses the same measure scaled by lagged total revenue to account for delays in bad debt write-offs, which can occur several months after service delivery. Because hospitals are reimbursed based on negotiated prices rather than charges, Column (3) uses an alternative denominator—net patient revenue plus bad debt—to proxy for the total amount hospitals expect to collect prior to write-offs. Columns (4) and (5) use the inverse hyperbolic sine (IHS) transformation of bad debt and bad debt per inpatient discharge, respectively, providing alternative specifications based on the unscaled level of bad debt and adjusting for differences in patient volume. The IHS transformation accommodates zero and highly skewed values while retaining an interpretation similar to a log transformation (Burbidge et al. 1988).

Across specifications, the estimates indicate that credit reporting bans lead to a statistically significant increase in hospitals' bad debt. In Column (1), bad debt as a share of total revenue increases by 0.11 percentage points relative to a baseline mean of 1.10, corresponding to roughly a 10 percent increase. The results are similar in statistical significance and magnitude across Columns (2) and (3). Consistent with these findings, Column (4) shows that the unscaled bad debt amount increased by approximately 8.6% following the policy change, while the amount of bad debt per discharge increased by 9.9%.

The 8.6% increase in bad debt reported in Column (4) translates into approximately $1.21 million in additional financial burden for the average hospital per year. This magnitude is economically meaningful, as 33.7% of hospital-year observations in the pre-treatment period

(2016–2022) report negative net income, and 41.4% of hospitals have negative net income in the year immediately preceding the policy shock.

A concern with using hospital bad debt as a proxy for household medical bill default is that it may be confounded by changes in hospitals' write-off policies or accounting practices that could be correlated with credit reporting bans. For example, hospitals may write off unpaid bills more aggressively even if collection efforts are not exhausted, leading to an overstatement of default. To address this concern, I use individual-level SIPP panel data, which directly measure households' year-end unpaid medical bill balances.

Table A1 reports difference-in-differences regressions under SIPP, examining both the extensive and intensive margins of medical debt. Columns (1)–(3) study the likelihood of default, defined as having a positive year-end medical debt balance, while Columns (4)–(6) examine the level of outstanding debt. I classify individuals as low-income if their pre-treatment income is below the median.

Column (2) shows that the probability of default increases by 1.9 percentage points for low-income individuals in the treated state, a result statistically significant at the 5% level. This effect is economically meaningful, corresponding to 18% of the sample average in the pre-treatment period. Column (5) further shows that low-income individuals in the treated state hold significantly larger debt balances at year-end, with average balances rising by roughly 21% relative to the sample mean, significant at the 1% level. In contrast, while the coefficients for high-income individuals are negative (Columns (3) and (6)), they are statistically indistinguishable from zero, suggesting no meaningful impact.

The heterogeneous effects indicate that the credit reporting ban has larger impacts on payment behavior among low-income individuals who are typically more liquidity- and resource-constrained. Conceptually, stronger responses among low-income individuals arise for two main reasons. First, they are more often covered by less generous insurance plans with higher out-of-pocket costs. Because credit penalties tend to increase with the size of

unpaid bills, the gains from defaulting on medical debt are correspondingly larger when reporting is removed. Second, tighter liquidity constraints make the cash preserved by delaying or avoiding repayment more valuable, especially for households using scarce resources to repay other debts or finance essential consumption.

Low-income individuals could, in principle, respond less if weaker credit histories make an additional medical collection less consequential for their access to credit. The empirical results suggest that this force is dominated by the effects of higher out-of-pocket exposure and tighter liquidity constraints, leading to a larger reduction in repayment among low-income individuals.

An alternative explanation is that the increase in unpaid balances reflects higher medical utilization rather than reduced repayment. To test this, Panel B of Table A1 examines healthcare utilization and health outcomes, including physician visits, nights in the hospital, and self-reported health status. The estimates are small and statistically insignificant, indicating that the rise in unpaid balances reflects lower repayment of bills incurred rather than greater consumption of care. One plausible reason is that patients seldom know their out-of-pocket cost at the point of care, given the complexity and limited transparency of medical billing, so a lower cost of default has little influence on whether they seek care.

Figure 4 and Figure A2 separately report dynamic difference-in-differences estimates of hospitals' bad debt and medical bill outcomes for low-income individuals. The figure shows that pre-treatment trends in default outcomes are parallel across treated and control states, supporting the parallel trend assumption.

Overall, these results indicate that credit reporting bans increase unpaid medical debt, consistent with weakened repayment incentives, particularly among low-income households, and thereby increase the financial burden borne by hospitals.

4.3 Insurance Screening

Table 3 examines whether hospitals adjust patient composition based on insurance generosity using Equation (3). I classify inpatient admissions (excluding childbirth) into two groups: elective and emergency/urgent. Elective admissions comprise non-emergency surgeries scheduled in advance, affording hospitals sufficient lead time and discretion to assess patients' financial risk prior to admission. Emergency/urgent admissions, including emergency surgeries, acute medical conditions, and behavioral health cases, are predominantly unscheduled and originate through the emergency department, leaving hospitals with limited opportunity for financial screening. These admissions are further constrained by EMTALA, which prohibits hospitals from conditioning emergency treatment on insurance status or ability to pay, and by state involuntary commitment statutes that define behavioral health admission criteria in exclusively clinical terms.

Insurance generosity is measured by an indicator for whether a patient's out-of-pocket (OOP) charge exceeds the 75th percentile of the pre-treatment distribution, conditional on state, year of the month, emergency status, and admission type. This measure captures patients with higher OOP obligations and thus greater financial risk from the hospital's perspective.

The specification in Table 3 includes detailed DRG fixed effects and patient characteristics, so identification comes from comparing treated and control patients within similar clinical conditions and backgrounds. Column (1) shows that, for elective admissions, the share of patients with less generous insurance declines by 4.3 percentage points (16% of the mean), with the estimate significant at the 1% level. This pattern suggests a shift in patient composition toward individuals with more generous coverage, consistent with hospitals responding to heightened bill collection risk associated with high OOP obligations.

In contrast, Columns (2)–(4) show no significant changes in patient composition for admissions with limited scope for financial screening due to administrative and legal constraints.

As a robustness check, Table A2 shows that the results are robust to alternative measures of insurance generosity, including different cutoff thresholds at the 70th percentile and the OOP-to-total-charges ratio. The robustness effect based on the OOP-to-total-charges ratio mitigates concerns that changes in OOP costs are mechanically driven by price changes. Figure 5 presents the dynamic effects around the credit reporting bans and supports the parallel trends assumption.

The estimates thus far indicate that hospitals begin reducing elective admissions once patients' OOP obligations exceed approximately the 70th percentile of the pre-treatment distribution. This naturally raises the question of where the effective cutoff for admission reductions lies, which is important for understanding both the aggressiveness of hospitals' responses and the level of insurance generosity they perceive as financially risky. To examine this, I re-estimate Equation (3) using alternative OOP percentile bins in Table A3, ranging from 0–20th to 80–100th percentiles. Because the outcome is each bin's share of admissions, a decline in share, particularly one that is large relative to the bin mean, indicates a reduction in admissions. Columns (1)–(5) show that the composition of admitted patients shifts away from higher-OOP patients above the 60th percentile (OOP $\geq$ \$1,000) and toward lower-OOP patients below the 40th percentile (OOP $\leq$ \$100), while the middle of the distribution (40th–60th percentile, \$100–\$1,000) remains largely unchanged. Columns (6)–(10) show a similar pattern using the OOP-to-total-charges ratio. Taken together, the results suggest that insurance-based screening begins around the 60th percentile: patients above this threshold appear to be viewed as relatively high financial risk, whereas those below it are treated as lower risk.

Given the evidence of admission cuts for patients with less generous insurance coverage, it is important to assess the socioeconomic status of these patients to better understand the distributional effects of the credit reporting ban. Two competing hypotheses frame this question. On one hand, disadvantaged individuals with fewer resources may be more likely

to hold less generous insurance plans because of higher premiums or because their employers are financially constrained to provide high-coverage options. On the other hand, consistent with adverse selection, healthier and wealthier individuals may opt for less generous coverage if their expected utilization is low. Table A4 tests these hypotheses by regressing various measures of insurance generosity on MSA-level socioeconomic characteristics. The dependent variables include OOP dollars, the OOP-to-total-charges ratio, and indicators of high OOP charges. The estimates show that individuals in disadvantaged areas are systematically more likely to face less generous coverage. A 10% decline in MSA median income is associated with a $35 increase in OOP costs (2.2% of the mean), while a 10 percentage point increase in the fraction of individuals with medical debt raises OOP costs by $118 (7.4% of the mean). Similarly, areas with higher uninsured rates exhibit higher OOP burdens. These results hold across all measures. Figure A3 shows the binned scatter plot of these correlations. While not entirely ruling out adverse selection, they provide evidence that less generous coverage is concentrated among lower socioeconomic groups.

4.4 Admission Volume

Profitability-based admission cuts: Table 4 presents regression results on inpatient admission volumes using Equation (2). Panel A reveals an asymmetric response across admission types. Profitable admissions, including elective and emergency surgical admissions, show no statistically significant changes in volume, although elective surgical admissions decline slightly. In contrast, non-surgical admissions, comprising acute medical and behavioral cases, decline by approximately 11% in Column (3). Because non-surgical admissions are generally characterized as loss-generating, this pattern is consistent with the hypothesis that rising medical bill defaults exacerbate unprofitable margins on these services, creating incentives for hospitals to curtail them to offset revenue losses.

The natural question is what mechanism drives this reduction. I provide evidence con-

sistent with hospitals raising the effective admission threshold for patients arriving through outpatient visits or the emergency department. If this is indeed the primary mechanism, the decline in admissions should be concentrated among lower-severity DRGs — marginal cases where hospitals retain greater discretion over whether to admit or discharge a patient as an outpatient. To test this, I construct an MSA × Quarter × DRG panel on admission volumes to examine heterogeneous effects across DRGs of varying severity.[14] Panel B of Table 4 supports this hypothesis: the observed decline in non-surgical admissions is concentrated among lower-severity DRGs, as measured by below-median ICU utilization rates, abnormal discharge transfers, and length of stay. Together, these patterns suggest that hospitals tighten admission thresholds for marginal cases, where clinical discretion is greatest, while continuing to admit more severe cases where the decision to hospitalize is constrained.

An alternative explanation for the decline in non-surgical inpatient admissions is that it reflects improved individual health outcomes rather than a supply-side response by hospitals. Under this hypothesis, the policy-induced expansion of credit access and liquidity, combined with reduced concern over the adverse credit consequences of medical debt, may have improved patients' financial well-being and, consequently, their health, leading to a reduction in the demand for acute care. If so, admission volumes could fall even in the absence of any supply-side adjustments.

To evaluate this competing mechanism, I examine two outcomes in Table 5: the volume of outpatient and ER visits, and the conversion rate from these encounters into inpatient admissions. If the decline in admissions were driven purely by reduced demand, one would expect outpatient and ER visits to fall as well, since healthier individuals would be less likely to seek care at any entry point. However, Panel A shows that outpatient and ER visit volumes

[14] I restrict the sample to MSA–quarter–DRG cells with at least three admissions to reduce volatility in outcomes driven by small cell counts due to the granularity of DRG. This ensures that estimated effects reflect systematic changes in admissions rather than idiosyncratic fluctuations. In Table A5, results are robust to including all cells.

remain stable, indicating that overall demand for medical services is largely unchanged by the policy. Panel B further shows that the ratio of non-surgical inpatient admissions to outpatient visits declines by 0.033 percentage points (15%), and the ratio relative to ER visits declines by 0.5 percentage points (8%), consistent with a lower likelihood that a given visit results in an admission. Taken together, these findings point to hospitals raising admission thresholds rather than a broad reduction in care-seeking driven by improved health.

Figure 6 illustrates the dynamic effects on inpatient admission volumes, revealing a clear asymmetry: high-margin surgical admission volumes, both elective and emergency, remain stable throughout, while non-surgical admissions decline sharply one quarter after the policy shock. Figure 7 corroborates this pattern by showing the dynamic effects: outpatient and ER visit volumes remain stable, while the ER-to-outpatient admission ratio declines following the policy. Across all figures, no discernible pre-trends are observed in the pre-shock period, supporting the validity of the parallel trends assumption.

A potential concern is that hospitals may not benefit from reducing unprofitable inpatient admissions if they cannot effectively reduce costs. For instance, the revenue loss from forgone admissions may exceed any achievable cost savings. To evaluate this, Table A6 examines heterogeneity by hospitals' cost structure. I show that the reduction in unprofitable admissions is concentrated among hospitals with higher occupancy rates and a greater reliance on contract labor.[15] These hospitals tend to face higher variable costs, as they are more likely to rely on costly contract staff (e.g., travel nurses) or overtime shifts to accommodate patient volume. As a result, they stand to realize larger cost savings from reducing unprofitable admissions. This evidence supports the interpretation that hospitals cut admissions precisely

[15]Hospitals facing high occupancy rates rely more heavily on contract labor to accommodate patient volume, but at substantially higher cost. For example, travel nurses are typically paid 2–4 times more than permanent staff (Visa Solutions Healthcare 2023, DirectShifts 2023, Center for Economic and Policy Research 2022). Consistent with this, using the pre-shock sample, Panel A of Table A6 shows that a hospital's share of contract labor costs is positively correlated with occupancy rate (% bed-days utilized) and operating cost per discharge.

in settings where costs are more variable and can be effectively reduced.

While MarketScan® provides the granular diagnosis codes needed to examine service provision by profitability, it covers only privately insured patients. To show the cut in inpatient admissions generalizes to the full patient pool, I re-estimate Equation (1) on CMS cost report data in Table A7.[16] Because the cost reports do not break discharges out by DRG and thus profitability, I analyze total discharges separately for general facilities (childbirth, surgical, and acute medical admissions) and inpatient psychiatric facilities (IPFs, covering mental health and behavioral cases), Columns (1)–(3) show that general admissions fall by 3.7%, with a larger decline among publicly insured patients (5.4%) than privately insured patients (3.4%). Columns (4)–(6) show that IPF admissions, services that are generally unprofitable, drop by 9.4%, again with a stronger effect for the publicly insured (13.1%). Hospitals thus cut admissions more aggressively for publicly insured patients, whose lower reimbursements likely make them less profitable.

Because general admissions pool profitable surgical cases with unprofitable acute medical cases, the aggregate decline alone cannot reveal which type hospitals cut. Since the data do not identify discharges by group, I use the ratio of operating room expenses to total expenses as a proxy for the share of profitable surgical procedures in a hospital's service mix. Column (7) shows this ratio rises by 0.3 pp (3.7% of the mean) after the policy, consistent with hospitals tilting their case mix toward profitable surgical procedures. Overall, this evidence corroborates the MarketScan® results: hospitals respond to the credit reporting ban by curtailing unprofitable services.

Costs to patients: The preceding evidence shows that hospitals scale back marginal, unprofitable admissions—but whether patients bear a cost is ex ante ambiguous. If these

[16] A publicly available alternative is the CMS Medicare Inpatient Hospitals by Provider and Service dataset, which reports fee-for-service Medicare discharges at the hospital-by-DRG level. However, it is less suitable because it excludes Medicare Advantage enrollees, who account for roughly 54% of Medicare beneficiaries in 2025 (Ochieng et al. 2025), and suppresses hospital-DRG cells with ten or fewer discharges, raising selection concerns.

marginal admissions were low-value—cases that could be safely managed in outpatient settings—cutting them reduces wasteful care without harming patients (Shi 2024). If instead they carry immediate clinical value, or precautionary value in the longer term—an inpatient stay allows monitoring and early intervention that prevent a manageable condition from deteriorating—then patients sent home without such care are more likely to see their conditions worsen over time and return for care, most visibly through the ER. To distinguish these hypotheses, Table 6 estimates the effect of the policy on repeated ER visits across three time windows following the initial visit: 5–10 days, 10–20 days, and 20–40 days. Specifically, I estimate Equation (3) using all outpatient ER visits at the individual level, controlling for outpatient Major Diagnostic Category (MDC) codes provided by MarketScan®, as well as patient demographics and insurance type.[17] For each ER visit, I construct two outcomes: an indicator for whether the patient has at least one repeat visit within a given time window, and the total number of repeat visits within that window.

The results show no significant change in either the probability or frequency of return visits within 5–20 days of the initial visit (Columns 1–2 and 4-5). However, Columns (3) and (6) reveal a statistically significant increase in both measures in the 20–40 day window. Specifically, following the policy, patients in treated states are 0.33 percentage points more likely to revisit the ER relative to those in control states, and experience an increase of 0.008 visits on average within 20–40 days after the initial visit. These estimates correspond to 3.8% and 5.8% of the pre-policy sample mean, respectively. The delayed emergence of these effects is consistent with patients experiencing gradual health deterioration after being discharged without inpatient admission, suggesting that tighter admission thresholds may have meaningful downstream consequences for patient care.

[17] I focus on ER visits rather than all outpatient visits because ER visits more directly reflect acute health needs and are more sensitive to insufficient or delayed care, whereas other outpatient visits (e.g., routine office visits) include substantial noise unrelated to severity.

4.5 Inpatient Prices

I examine the effect of the medical debt credit reporting ban on hospital pricing using Equation (3). The central question is whether hospitals raise negotiated prices with insurers in order to offset revenue losses from increased medical bill defaults. Beyond baseline controls for patient demographics, DRGs, and MSA characteristics, I include insurance plan type (e.g., high-deductible, PPO, HMO) and emergency status to account for composition shifts that may confound price comparisons. Identification relies on a set of interacted fixed effects — diagnosis-group by quarter-shock, plan-type by quarter-shock, and emergency-status by quarter-shock — so that estimated effects reflect within-group price changes over time. This design ensures that the results are not driven by shifts in case mix, plan composition, or emergency caseloads, and rests on the assumption that hospitals have limited ability to differentially price services within these groups. Following Cooper et al. (2019), total charges to the insurer and patient are aggregated across all claims associated with each inpatient admission.

A key mechanism governing hospitals' ability to raise prices is their bargaining leverage with insurers (Liu 2022). Hospitals with greater market power can more credibly threaten to exit insurer networks or invoke the risk of financial distress during contract negotiations, enabling them to extract higher reimbursement rates in response to intensified default-driven revenue losses. Concentration also shapes how flexible prices can move. Hospitals with greater leverage disproportionately hold contracts paid as a share of billed charges rather than Medicare-indexed prospective rates (Cooper et al. 2019). Because charge-linked payments are a discount off the hospital's own chargemaster—an internal price list that hospitals set and update unilaterally (Tompkins et al. 2006)—raising list prices raises reimbursement without reopening the contract (Whaley et al. 2020). Under the Medicare-indexed contracts prevalent in competitive markets, by contrast, repricing requires renegotiating the base multiplier with the insurer. Both channels yield the same prediction: hospitals in concentrated markets can

raise commercial prices to restore lost collection revenue, while hospitals in competitive markets lack the contractual means to do so. To test this hypothesis, I conduct a subsample analysis based on local market concentration, measured by the Herfindahl–Hirschman Index (HHI) constructed from hospital bed counts in the CMS cost report data, where a higher HHI reflects greater concentration and stronger bargaining power.

Table 7 reports the results, separately for hospitals in high- and low-concentration markets. Columns (1) and (2) show that hospitals in high-concentration markets raise total charges by \$749 (3.6% of the sample mean) and out-of-pocket charges by \$34 (2.7%), both statistically significant at conventional levels. In contrast, Columns (3) and (4) show no significant price response among hospitals operating in low-concentration markets. The differences in coefficients across the two subsamples are statistically significant, with p-values of 0.098 and 0.022 for total and OOP charges, respectively. Taken together, these findings are consistent with the hypothesis that bargaining leverage is the operative mechanism: hospitals with greater market power are better positioned to negotiate higher reimbursement rates in response to increased medical bill collection pressure, while those in more competitive markets lack the leverage to do so.

A potential concern with interpreting the results is that hospitals simultaneously adjust the patient mix based on insurance generosity (the extensive margin) and inpatient prices (the intensive margin). Because more generously insured patients face lower OOP charges by construction, a compositional shift toward such patients could mechanically reduce observed OOP charges even in the absence of any price change, complicating interpretation. To address this concern, Table A8 reports the same analysis after excluding elective admissions, the type of admissions along which Section 4.3 documents hospitals adjusting their patient mix based on insurance coverage. The results are quantitatively similar: for this cleaner sample, free of insurance-generosity-driven composition shifts, high-market-power hospitals raise inpatient prices by 4.0%, and OOP charges rise by 3.4%. This indicates that hospitals

raise prices broadly, and that patient mix adjustments on elective admissions contribute little to the overall effect.

Figure 8 shows the dynamic effects of the bans on hospitals' total charges and OOP charges to patients. Pre-period coefficients are close to zero and statistically insignificant, supporting the parallel trends assumption.

4.6 Heterogeneity on Local Socioeconomic Characteristics

The baseline results establish that hospitals respond to the credit reporting ban by adjusting insurance composition and curtailing low-margin admissions, leading to repeated ER visits. A natural follow-up question is whether these responses, and their consequences for patients, are distributed uniformly across communities, or whether they fall disproportionately on already-disadvantaged populations. This section addresses that question by estimating heterogeneous effects across three MSA-level socioeconomic dimensions: median income, the uninsured rate, and the pre-shock fraction of the population with medical debt in collection.

The direction of heterogeneity is not obvious ex ante. On one hand, hospitals in disadvantaged areas serve a higher share of liquidity-constrained patients who are more likely to default once credit reporting penalties are removed. These hospitals also tend to operate on thinner margins, and may therefore face more severe revenue shocks and respond more aggressively to stay financially sustainable. On the other hand, if credit reporting was not the binding constraint on repayment behavior in these areas, possibly because default rates were already high prior to the policy, the ban may generate little incremental change in collections, muting the shock and the hospital response. The empirical results decisively favor the former hypothesis.

Table 8 reports the triple-difference estimates across four outcome categories, where the third interaction term (e.g., Treated $\times$ Post $\times$ D(High Income)) captures how baseline effects

vary with local socioeconomic conditions. Columns (1)–(3) establish that the policy-induced revenue shock falls disproportionately on hospitals in disadvantaged areas. The increase in bad debt as a share of total revenue is significantly amplified in MSAs with high uninsured rates (0.395^{***}) and high pre-existing medical debt burdens (0.192^{**}), while it is significantly attenuated in high-income areas (-0.096^{*}), confirming that financial exposure to the policy is heavily concentrated in already-strained hospital markets.

Columns (4)–(6) reveal a corresponding intensification of insurance screening in disadvantaged areas. The policy-induced reduction in the probability of high OOP elective admissions — reflecting tighter restrictions on patients with less generous insurance coverage — is significantly stronger in high-uninsured MSAs (-0.037^{*}) and significantly weaker in high-income areas (0.065^{**}), consistent with hospitals facing greater revenue pressure selectively restricting admissions for patients who pose elevated bill collection risk.

Columns (7)–(9) show that the retrenchment in low-margin non-surgical admissions follows the same geographic gradient. The decline in non-surgical admission volumes is significantly more pronounced in high-uninsured MSAs (-0.137^{*}) and significantly attenuated in high-income areas (0.170^{*}), suggesting that hospitals exposed to the largest revenue shocks respond most aggressively by curtailing services with the thinnest margins.

Columns (10)–(12) show that patient-level consequences are similarly concentrated among disadvantaged populations. Repeated ER visits increase significantly more in high-medical-debt MSAs (0.023^{*}) and attenuate in high-income MSAs (-0.019^{***}), consistent with patients in these communities bearing a disproportionate burden of the incomplete or delayed care resulting from tighter admission thresholds.

Table A9 presents the same heterogeneity analysis for inpatient prices among hospitals in high market power MSAs. Column (1) shows that the increase in total charges is attenuated by $353 in high-income MSAs, statistically significant at the 10% level. Column (2) indicates a similar attenuation of $35 for OOP charges, but the estimate is not statistically

significant at the 10% level, likely reflecting reduced statistical power due to the smaller sample restricted to high market power hospitals.

To assess whether hospitals' responses extend beyond financially risky patients, I examine MSA-level heterogeneity within the subsample of low-risk patients in Table A10, defined as those with OOP obligations below the 60th percentile of the pre-treatment distribution.[18] Table A10 focuses on two revenue-enhancing margins: reductions in unprofitable inpatient admissions and increases in prices. If hospitals could precisely target financially risky patients, these outcomes should remain unchanged for this low-risk group. However, such targeting is limited in practice. Many unprofitable admissions (e.g., sepsis, pneumonia, psychiatric cases) originate in the emergency room, where screening based on ability to pay is constrained by EMTALA. At the same time, selectively raising prices for higher-risk patients is unlikely to be optimal, as it would further increase their default probability. As a result, hospitals may adjust both admission and pricing margins more broadly across the patient pool, generating spillover effects even for low-risk patients.

Table A10 shows that low-risk patients are nonetheless affected. Columns (1) and (2) indicate that the reduction in unprofitable inpatient admissions, along with the associated increase in repeated ER visits, is significantly attenuated in high-income MSAs. Columns (3) and (4) show that the increases in inpatient total charges and OOP charges are also attenuated in high-income areas, although only the effect on total charges is statistically significant. Overall, the results indicate that hospitals adjust these two types of revenue-enhancing margins broadly rather than targeting only high-risk patients, generating spillover effects that disproportionately burden even low-risk patients in disadvantaged areas.

Taken together, these findings reveal a concerning gradient: the magnitude of the revenue shock, the aggressiveness of hospitals' supply-side responses, and the severity of patient-level

[18] Section 4.3 and Table A3 suggest no admission cuts below the 60th percentile of the pre-treatment OOP distribution, indicating these patients are at lower risk of default.

consequences all increase with local socioeconomic disadvantage, with effects spilling over even to patients unlikely to default in response to the credit reporting ban. While the policy was intended to protect financially vulnerable individuals from the consequences of unpaid medical bills, the results point to an unintended supply-side response: hospitals in the most disadvantaged communities adjust most aggressively, potentially undermining the policy's intended benefits for the very populations it was designed to protect.

4.7 Heterogeneity by Hospital Characteristics

The previous section shows that the effects of credit reporting bans are concentrated in disadvantaged communities, where hospitals experience larger increases in uncollectible bills. I next examine whether hospitals' own characteristics shape how these financial pressures translate into supply-side responses. I focus on two dimensions: financial fragility and ownership type.

Financial Fragility: Table 9 examines heterogeneity by hospitals' pre-policy days of cash on hand. Column (1) provides an important benchmark: the increase in bad debt is similar across hospitals with high and low cash reserves, as the interaction with high days of cash is small and statistically insignificant. Thus, the stronger responses of financially fragile hospitals do not appear to reflect a larger initial collection shock.

Instead, hospitals with fewer financial resources respond much more aggressively to a similar shock. Because MarketScan® does not report hospital identifiers, I measure financial fragility for these outcomes using the MSA-level weighted average of hospitals' pre-policy days of cash on hand. In markets characterized by more financially fragile hospitals, the share of high-OOP elective admissions falls by 5.5 percentage points, while unprofitable admissions decline by approximately 16%. Both effects are substantially attenuated where hospitals are financially stronger, with implied declines of only 1.1 percentage points and roughly 4%, respectively. Repeated ER visits likewise increase by 0.015 visits in markets with

more financially fragile hospitals, with an implied effect of essentially zero (-0.001) where hospitals have stronger financial buffers. In concentrated markets, inpatient payments also rise by $1,710 when hospitals are more financially fragile, compared with an implied increase of only about $357 when they are financially stronger. These patterns suggest that limited financial capacity makes collection losses harder to absorb, leading financially constrained hospitals to restrict care and seek additional revenue more aggressively.

Hospital Ownership: Ownership provides a complementary dimension of heterogeneity because non-profit and for-profit hospitals differ in both their financial position and their exposure to patients whose bills may be difficult to collect. Table 10 examines these differences. Because New York does not permit for-profit hospitals, the analysis focuses on Colorado's credit reporting ban.

Panel A documents two important pre-policy differences. First, non-profit hospitals operate with substantially thinner profit margins than for-profit hospitals, 6.5% versus 24.2%. While profit margins are not a direct measure of liquidity, they capture the earnings cushion available to absorb revenue losses: a given decline in collections places greater pressure on a hospital when its pre-existing margin is small. Second, non-profit hospitals devote more resources to service settings where financial screening is limited. Their inpatient psychiatric facility expense share is 2.5%, compared with 0.8% among for-profit hospitals, while their ER expense share is 5.4%, compared with 4.3%. In emergency care, EMTALA requires hospitals to stabilize patients regardless of ability to pay; similarly, Colorado law (C.R.S. §27-65) limits hospitals' discretion over psychiatric admissions for patients meeting involuntary commitment criteria. These constraints make it more difficult for hospitals to avoid patients with elevated collection risk. Non-profit hospitals therefore enter the policy with both thinner earnings cushions and greater exposure to settings in which collection risk is difficult to mitigate.

Panel B provides complementary evidence on ex-ante patient selection. I focus on elective

surgical admissions, where hospitals have greater discretion to screen patients based on financial risk. Because MarketScan® does not identify hospital ownership, I use the MSA-level share of for-profit hospitals to capture local ownership composition. After controlling for patient characteristics, detailed DRGs, and local income, patients admitted in markets with greater for-profit presence have systematically lower OOP obligations. A 10 percentage point increase in the for-profit share is associated with approximately $55 lower OOP charges and a 0.4 percentage point lower OOP-to-charge ratio. These patterns are consistent with more intensive ex-ante financial screening in markets with greater for-profit presence, leaving markets dominated by non-profit hospitals more exposed to patients with higher collection risk.

Panel C shows that these pre-policy differences translate into heterogeneous responses to the ban. Column (1) shows that bad debt increases by 0.107 percentage points among non-profit hospitals, while the effect is attenuated by 0.092 percentage points for for-profit hospitals. Consistent with this greater exposure, column (2) shows that insurance screening also intensifies more strongly in markets with lower for-profit presence: the share of high-OOP elective admissions falls by 9.1 percentage points, compared with an implied decline of only 2.2 percentage points in markets with greater for-profit presence. Column (3) shows no statistically significant ownership heterogeneity in unprofitable admissions. Column (4), however, shows that repeated ER visits increase more strongly in markets with lower for-profit presence, indicating that the downstream consequences for patients are also concentrated in these markets.

Taken together, Tables 9 and 10 point to two distinct but related determinants of hospitals' responses. Financially fragile hospitals respond more aggressively even when they experience similar increases in bad debt, highlighting the importance of hospitals' capacity to absorb revenue losses. Non-profit hospitals, meanwhile, enter the policy with thinner earnings cushions and greater ex-ante exposure to collection risk, experience larger increases

in bad debt, and subsequently exhibit stronger adjustments in patient screening and downstream patient outcomes. This heterogeneity is important for policy design because a uniform consumer protection can impose very different burdens across providers. The results suggest that complementary policies aimed at preserving access may be most effective when targeted toward hospitals with limited financial capacity or substantial uncompensated-care exposure, rather than applied uniformly across hospitals.

5 Conclusion

This paper shows that medical debt credit reporting bans generate important supply-side responses in healthcare markets. Exploiting state-level bans in Colorado and New York, I find that removing the credit consequences of medical debt weakens repayment incentives and increases hospitals' uncollectible medical bills. Hospitals respond by restricting elective admissions for patients with less generous insurance, reducing unprofitable inpatient care, and, where they have greater bargaining power, raising negotiated prices. These adjustments have real consequences for patients: repeat emergency room visits increase, consistent with delayed or incomplete care. The responses are strongest in disadvantaged communities and among financially fragile and non-profit hospitals, concentrating the costs among patients the policies are intended to protect.

The broader implication is that consumer credit protections cannot be evaluated by their effects on borrowers alone. When goods or services are provided before payment is secured, weakening the consequences of default can alter firms' willingness to provide them and shift costs back to consumers through reduced access and higher prices. Medical debt reporting restrictions may protect households from the financial consequences of illness, but their benefits must therefore be weighed against these equilibrium supply-side responses. Complementary support for uncompensated care, particularly at financially fragile hospitals

serving disadvantaged communities, may help preserve the protections of medical debt relief without undermining access to care.

Table 1:
Summary Statistics

This table reports summary statistics for the matched sample in the pre-treatment period. Panel A presents hospital characteristics from the CMS Cost Reports based on hospital-by-year observations. Panel B reports summary statistics for admissions, visits, and prices using the MarketScan® data. The admissions, visits, and repeated-visit variables are measured at the MSA-by-quarter level. The price variables are measured at the inpatient admission episode level.

Panel A: Hospital Characteristics

Variable	*N*	*Mean*	*Median*	*p25*	*p75*
Total Patient Revenue (mil.$)	3,825	1,625.23	1,003.71	397.46	2,006.82
Net Patient Revenue (mil.$)	3,825	426.12	275.43	123.45	496.81
Bad Debt (mil.$)	3,825	14.12	7.63	3.93	15.34
Net Income (mil.$)	3,825	21.76	7.48	-4.50	32.83
Bad Debt/Total Patient Revenue (%)	3,825	1.10	0.87	0.54	1.37
Net Income/Net Patient Revenue (%)	3,825	3.83	3.66	-2.59	10.65
Bad Debt/# Inpatient Discharges ($)	3,825	1,348.17	1,053.91	675.70	1,651.63
D(For-profit)	3,825	0.06	0.00	0.00	0.00
# Beds	3,825	244.26	183.00	95.00	327.00
# Inpatient Discharges	3,825	11,707.43	9,063.00	3,553.00	16,080.50

Panel B: Admissions, Visits, and Prices

Variable	*N*	*Mean*	*Median*	*p25*	*p75*
Quarterly Admissions or Visits per 1,000 Enrollees:					
All Admissions	405	3.79	3.75	3.12	4.31
Elective Surgical Admissions	405	0.58	0.57	0.43	0.73
Emergency Surgical Admissions	405	0.37	0.33	0.24	0.45
Non-Surgical Admissions	405	2.83	2.80	2.35	3.21
Outpatient Visits	405	2,264.00	2,218.00	2,108.00	2,395.00
ER Visits	405	71.36	69.09	60.89	78.84
Repeated Visits:					
D(Repeated ER Visits [20–40 days])	1,605,304	0.085	0.000	0.000	0.000
# Repeated ER Visits [20–40 days]	1,605,304	0.146	0.000	0.000	0.000
Prices:					
Total Charges ($)	164,474	24,080.32	16,809.66	8,975.68	27,811.20
OOP Charges ($)	164,474	1,350.54	700.10	20.00	2,108.93
OOP Charges (%)	164,474	9.99	4.81	0.09	15.50
Copay ($)	164,474	70.98	0.00	0.00	0.00
Deductible ($)	164,474	411.46	0.00	0.00	364.34
Coinsurance ($)	164,474	849.77	259.31	0.00	1,441.47

Table 2:
Hospitals' Bad Debt

This table reports regression results examining the effects of medical debt credit reporting bans on hospitals' bad debt, i.e., uncollectible medical bills from patients. The outcome variables in Columns (1), (2), and (3) are measured in percentage points. The outcome variables in Columns (4) and (5) are measured in the inverse hyperbolic sine transformation. Hospital and year indicators are interacted with cohorts of matched hospitals as fixed effects. Controls include county pre-policy below/above-median income and uninsured rate, each interacted with year × reporting-ban event indicators as fixed effects. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Bad Debt / Total Revenue	Bad Debt / Lagged Total Revenue	Bad Debt / (Net Revenue + Bad Debt)	IHS (Bad Debt)	IHS (Bad Debt / Discharge)
	(1)	(2)	(3)	(4)	(5)
Treated × Post	0.110**	0.099**	0.383***	0.086*	0.099**
	(0.043)	(0.042)	(0.111)	(0.048)	(0.037)
Dep Var Mean:	1.10	1.17	3.70	14.12 (mil.$)	1,348.2 ($)
Dep Var SD:	0.879	0.936	2.77	19.06 (mil.$)	1,041.9 ($)
Hospital × Match FE	Yes	Yes	Yes	Yes	Yes
Year × Match FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	4,610	4,590	4,610	4,612	4,612
Adjusted R^2	0.697	0.698	0.722	0.846	0.668

Table 3:
Insurance Screening

This table reports regression results examining the effects of medical debt credit reporting bans on the composition of patients by insurance generosity. The dependent variable, D(High-OOP), is an indicator equal to one if an admission's total OOP charges (co-pays, deductibles, and co-insurance) exceed the 75th percentile of the pre-treatment OOP distribution, and zero otherwise. The cutoffs are calculated separately within groups defined by state, month of year, admission type (i.e., surgical, acute medical, and behavioral), and emergency status. Elective admissions include non-emergency surgical admissions (Column (1)). Emergency and urgent admissions include emergency surgical admissions (Column (2)) and non-surgical admissions (Column (3)), where non-surgical admissions include acute medical and behavioral admissions. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, and employment status. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	D(High-OOP)			
	Elective	Emergency & Urgent		
Admission Types:	Elective Surgical	Emergency Surgical	Non-Surgical	Column (2) & (3)
	(1)	(2)	(3)	(4)
Treated × Post	-0.043***	0.016	0.004	0.008
	(0.007)	(0.026)	(0.006)	(0.006)
Dep Var Mean :	0.259	0.259	0.264	0.263
MSA × Match FE	Yes	Yes	Yes	Yes
Qrt. × Match × Patient-Types FE	Yes	Yes	Yes	Yes
Observations	32,609	25,654	163,502	184,901
Adjusted R^2	0.059	0.069	0.054	0.057

Table 4:
Inpatient Admission Volume

This table reports the regression results examining the effects of medical debt credit reporting bans on hospitals' inpatient admission volumes. Panel A uses MSA-by-quarter observations, with outcomes measured as the inverse hyperbolic sine of admissions per 1,000 insured enrollees. Profitable admissions are elective and emergency surgical admissions; unprofitable admissions are non-surgical (acute medical and behavioral) admissions. In Panel B, the observations are at the MSA-by-Quarter-by-DRG level for unprofitable admissions. The regressions include indicators for whether the DRG-level share of ICU stays, the share of non-routine discharge transfers, and the average length of stay (LOS), measured in the pre-treatment period, exceed the median. Panel A includes matched-cohort-by-MSA and matched-cohort-by-quarter fixed effects; Panel B further interacts these fixed effects with DRG. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Admission Volume

Dependent Variables:	IHS(Admissions per 1,000 Enrollee)		
Profitability:	Profitable		Unprofitable
Admission Types:	Elective Surgical	Emergency Surgical	Non-Surgical
	(1)	(2)	(3)
Treated × Post	-0.027	0.024	-0.116**
	(0.024)	(0.030)	(0.051)
Dep Var Mean:	0.578	0.367	2.83
MSA × Match FE	Yes	Yes	Yes
Quarter × Match FE	Yes	Yes	Yes
Observations	798	798	798
Adjusted R^2	0.231	0.335	0.602

Panel B: Admissions Volume by DRG

Dependent Variable:	IHS(Admission per 1,000 Enrollee)			
Profitablility:	Unprofitable			
	(1)	(2)	(3)	(4)
Treated × Post	-0.011*	-0.019**	-0.017**	-0.017**
	(0.006)	(0.008)	(0.007)	(0.007)
Treated × Post × D(High-ICU DRG)		0.016***		
		(0.005)		
Treated × Post × D(High-Transfer DRG)			0.013**	
			(0.005)	
Treated × Post × D(High-LOS DRG)				0.012**
				(0.005)
Dep Var Mean:	0.095	0.095	0.095	0.095
MSA × Match × DRG FE	Yes	Yes	Yes	Yes
Quarter × Match × DRG FE	Yes	Yes	Yes	Yes
Observations	12,789	12,789	12,789	12,789
Adjusted R^2	0.861	0.861	0.861	0.861

Table 5:
Outpatient Volume and Inpatient Conversion

This table reports regression estimates of the effect of medical debt credit reporting bans on outpatient visit volume and the outpatient- and ER-to-inpatient conversion rates. In Panel A, the outcome variables are the number of all outpatient visits and ER visits per 1,000 insured enrollees. In Panel B, the outcome variables are the ratios of the number of unprofitable inpatient admissions to the number of ER visits and all outpatient visits, scaled into percentage points. Unprofitable admissions are non-surgical (acute medical and behavioral) admissions. MSA and quarter indicators are interacted with cohorts of matched MSAs as fixed effects. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Outpatient Visits Volume

Dependent Variables:	IHS(Outpatient Visits per 1,000 Enrollee) (1)	IHS(ER Visits per 1,000 Enrollee) (2)
Treated × Post	0.0006	0.009
	(0.013)	(0.040)
Dep Var Mean:	2,264	71.4
MSA × Match FE	Yes	Yes
Quarter × Match FE	Yes	Yes
Observations	798	798
Adjusted R^2	0.947	0.879

Panel B: Outpatient to Inpatient Conversion for Unprofitable Admissions

Dependent Variables:	Conversion Rate	
	Inpatient / ER Visit (1)	Inpatient / Outpatient Visit (2)
Treated × Post	-0.522*	-0.033***
	(0.248)	(0.008)
Dep Var Mean:	6.83	0.213
MSA × Match FE	Yes	Yes
Quarter × Match FE	Yes	Yes
Observations	798	798
Adjusted R^2	0.456	0.524

Table 6:
Repeated ER Visits

This table reports regression results examining the effects of medical debt credit reporting bans on patients' repeated ER visits. The dependent variable in Columns (1) to (3) is an indicator equal to one if a patient has at least one repeat ER visit within the specified time window after the initial ER visit. The dependent variable in Columns (4) to (6) is the number of repeated visits. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for outpatient Major Diagnostic Category (MDC) codes, age deciles, gender, industry, employment status, and insurance plan types (e.g., High-Deductible, PPO). Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	D(Repeated ER Visits)			# Repeated ER Visits		
Following Initial ER Visit:	[5–10 Days]	[10–20 Days]	[20–40 Days]	[5–10 Days]	[10–20 Days]	[20–40 Days]
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	−0.001	0.0011	0.0033**	0.0024	0.0023	0.0085*
	(0.0021)	(0.0013)	(0.0014)	(0.0022)	(0.0017)	(0.0044)
Dep. Var. Mean:	0.0526	0.0632	0.0852	0.0702	0.0938	0.1464
MSA × Match FE	Yes	Yes	Yes	Yes	Yes	Yes
Qrt. × Match × Patient-Types FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,299,583	3,299,583	3,299,583	3,299,583	3,299,583	3,299,583
Adjusted R^2	0.023	0.031	0.038	0.028	0.035	0.048

Table 7:
Inpatient Prices

This table reports the regression results examining the effects of medical debt credit reporting bans on hospitals' inpatient prices. Hospital market concentration at the MSA level is measured by the Herfindahl–Hirschman Index (HHI) based on available hospital bed-days. Columns (1)–(2) include on MSAs in the top tercile of concentration, and Columns (3)–(4) on those in the bottom two terciles. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, employment status, emergency status, and insurance plan types (e.g., High-Deductible, PPO). Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Total Charges($)	OOP Charges($)	Total Charges($)	OOP Charges($)
Market Concentration:	High		Low	
	(1)	(2)	(3)	(4)
Treated × Post	749.4***	34.3**	121.6	-15.4
	(192.2)	(14.7)	(328.1)	(16.0)
Dep Var Mean:	20,367	1,281	24,862	1,361
% of Mean:	3.68%	2.68%	0.49%	-1.13%
MSA × Match FE	Yes	Yes	Yes	Yes
Qrt. × Match × Patient-Types FE	Yes	Yes	Yes	Yes
p-value (*Coef* Column (1) = (3)):	0.098*			
p-value (*Coef* Column (2) = (4)):		0.022**		
Observations	59,088	59,088	290,361	290,361
Adjusted R^2	0.697	0.552	0.623	0.525

Table 8:
Heterogeneity on MSA Characteristics

This table shows the heterogeneity effects of the baseline results based on MSA-level characteristics. The independent variables include interaction terms with indicators for whether the MSA-level median income, % uninsured population, and % population with medical debt in collection is above the median. The dependent variable is: in columns (1)–(3), hospital bad debt as a share of total patient revenue (percentage points); in columns (4)–(6), an indicator for less generous insurance coverage: OOP for elective admissions above the 75th percentile of the pre-shock distribution; in columns (7)–(9), the inverse hyperbolic sine of unprofitable admissions per 1,000 enrollees; and in columns (10)–(12), the number of repeated ER visits 20–40 days after the initial visit. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Bad Debt / Total Revenue			D(High-OOP: Elective)			IHS(Unprofitable Admissions)			# Repeated ER Visits		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated × Post	0.182***	-0.026	0.060*	-0.059*	-0.008	-0.046***	-0.179**	-0.077	-0.123	0.023***	0.012**	0.003
	(0.045)	(0.029)	(0.032)	(0.031)	(0.019)	(0.009)	(0.074)	(0.099)	(0.082)	(0.007)	(0.005)	(0.004)
Post × D(High Income)	0.045			0.080			-0.090			0.002		
	(0.044)			(0.053)			(0.122)			(0.007)		
Treated × Post × D(High Income)	-0.096*			0.065**			0.170*			-0.019***		
	(0.051)			(0.030)			(0.086)			(0.006)		
Post × D(High % Uninsured)		-0.074			0.089***			-0.143			-0.015**	
		(0.051)			(0.024)			(0.133)			(0.007)	
Treated × Post × D(High % Uninsured)		0.395***			-0.037*			-0.137*			-0.006	
		(0.071)			(0.018)			(0.073)			(0.005)	
Post × D(High % Medical Debt)			-0.026			-0.019**			-0.091			-0.001
			(0.038)			(0.009)			(0.106)			(0.008)
Treated × Post × D(High % Medical Debt)			0.192**			0.007			-0.042			0.023*
			(0.084)			(0.015)			(0.197)			(0.012)
Dep Var Mean:	1.10	1.10	1.10	0.259	0.259	0.259	2.83	2.83	2.83	0.146	0.146	0.146
Baseline FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,547	4,547	4,547	32,609	32,609	32,609	798	798	798	3,299,583	3,299,583	3,299,583
Adjusted R^2	0.702	0.714	0.728	0.059	0.059	0.059	0.575	0.560	0.564	0.048	0.048	0.048

Table 9:
Heterogeneity on Financial Fragility

This table shows the heterogeneity effects of the baseline results based on hospitals' financial fragility. The independent variables include interaction terms with an indicator for whether hospitals' days of cash on hand, measured relative to operating expenses, is above the median. Fragility is measured at the hospital level in Column (1) and by the MSA-level weighted average of hospitals' pre-policy days of cash on hand in Columns (2)–(6). The dependent variable is: in Column (1), hospital bad debt as a share of total patient revenue (percentage points); in Column (2), an indicator for less generous insurance coverage: OOP for elective admissions above the 75th percentile of the pre-shock distribution; in Column (3), the inverse hyperbolic sine of unprofitable admissions per 1,000 enrollees; in Column (4), the number of repeated ER visits 20–40 days after the initial visit; and in Columns (5)–(6), inpatient total charges and OOP charges in MSAs in the top tercile of hospital market concentration. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Bad Debt / Total Revenue	D(High-OOP: Elective)	IHS(Unprofitable Admissions)	# Repeated ER Visits	Inpatient Prices ($)	
					Total Charges	OOP Charges
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.110**	-0.055***	-0.166**	0.015**	1,710.0***	34.2*
	(0.044)	(0.008)	(0.062)	(0.006)	(452.6)	(17.7)
Post × D(High Days of Cash)	0.024	-0.024	-0.047	0.013	411.9	-18.2
	(0.048)	(0.030)	(0.055)	(0.008)	(514.0)	(24.5)
Treated × Post × D(High Days of Cash)	0.011	0.044**	0.123*	-0.016**	-1,353.3*	-20.0
	(0.125)	(0.016)	(0.069)	(0.007)	(670.7)	(38.0)
Dep Var Mean:	1.10	0.259	2.83	0.146	20,367	1,281
Baseline FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,555	32,609	798	3,299,583	59,088	59,088
Adjusted R^2	0.711	0.071	0.661	0.048	0.698	0.566

Table 10: Heterogeneity on Ownership Types

This table presents pre-shock hospital characteristics and heterogeneous effects of the baseline results by hospital ownership, using the Colorado sample. Panel A reports pre-shock means of profit margins (net income over net patient revenue) and of inpatient psychiatric facility (IPF) and emergency room expenses as shares of total expenses, proxying for exposure to each service line. Panel B relates the MSA-level share of for-profit hospitals, scaled into percentage points, to patients' OOP charges in the pre-shock period, using the sample of elective surgical inpatient admissions; the dependent variable is the dollar amount of OOP in Columns (1) and (3) and the ratio of OOP to total charges in Columns (2) and (4), and Columns (3) and (4) additionally control for polynomials of MSA median income. Panel C presents triple-difference estimates of the baseline specification, where the dependent variable is: in Column (1), hospital bad debt as a share of total patient revenue (percentage points); in Column (2), an indicator for less generous insurance coverage: OOP for elective admissions above the 75th percentile of the pre-shock distribution; in Column (3), the inverse hyperbolic sine of unprofitable admissions per 1,000 enrollees; and in Column (4), the number of repeated ER visits 20–40 days after the initial visit. Column (1) interacts the treatment with an indicator for for-profit hospitals; Columns (2)–(4) use an indicator for whether the MSA-level share of for-profit hospitals is above the median. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ER and IPF Exposure (Pre-shock)

Variable Mean:	Non-Profit	For-Profit	p-value
Net Income / Net Patient Revenue	0.065	0.242	0.001***
IPF Expense Share	0.025	0.008	0.001***
Emergency Room Expense Share	0.054	0.043	0.027**

Panel B: Pre-shock Insurance Screening

Dependent Variables:	OOP ($) (1)	OOP (%) (2)	OOP ($) (3)	OOP (%) (4)
% For-Profit	-2.81**	-0.0002*	-5.53*	-0.0004***
	(0.821)	(0.00009)	(2.30)	(0.00009)
Dep Var Mean:	1,481.4	0.049	1,481.4	0.049
% For-profit Mean:	24.7			
Month FE	Yes	Yes	Yes	Yes
Patient Types FE	Yes	Yes	Yes	Yes
DRG FE	Yes	Yes	Yes	Yes
Income Polynomials			Yes	Yes
Observations	1,257	1,257	1,257	1,257
Adjusted R^2	0.118	0.337	0.116	0.337

Panel C: Heterogeneity for For-Profit Hospitals

Dependent Variable:	Bad Debt/ Total Revenue	D(High-OOP: Elective)	IHS(Unprofitable Admission)	# Repeated ER Visits
	(1)	(2)	(3)	(4)
Treated × Post	0.107***	-0.091**	-0.196***	0.031***
	(0.037)	(0.030)	(0.044)	(0.009)
Treated × Post × D(For-Profit)	-0.092*			
	(0.049)			
Post × D(High % For-Profit)		0.067**	-0.045	0.015
		(0.025)	(0.084)	(0.013)
Treated × Post × D(High % For-Profit)		0.069*	0.108	-0.026*
		(0.031)	(0.065)	(0.012)
Hospital/MSA × Match FE	Yes	Yes	Yes	Yes
Year/Qrt. × Match FE	Yes		Yes	
Qrt. × Match × Patient-types FE		Yes		Yes
Observations	1,251	8,366	310	914,817
Adjusted R^2	0.697	0.063	0.641	0.069

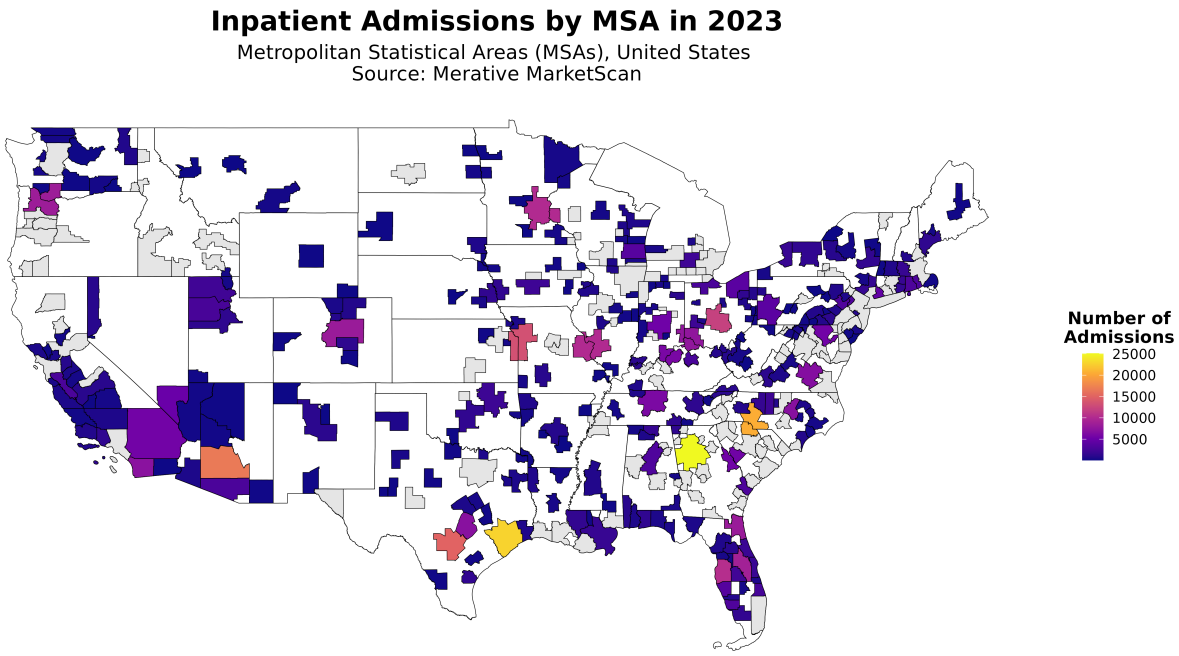
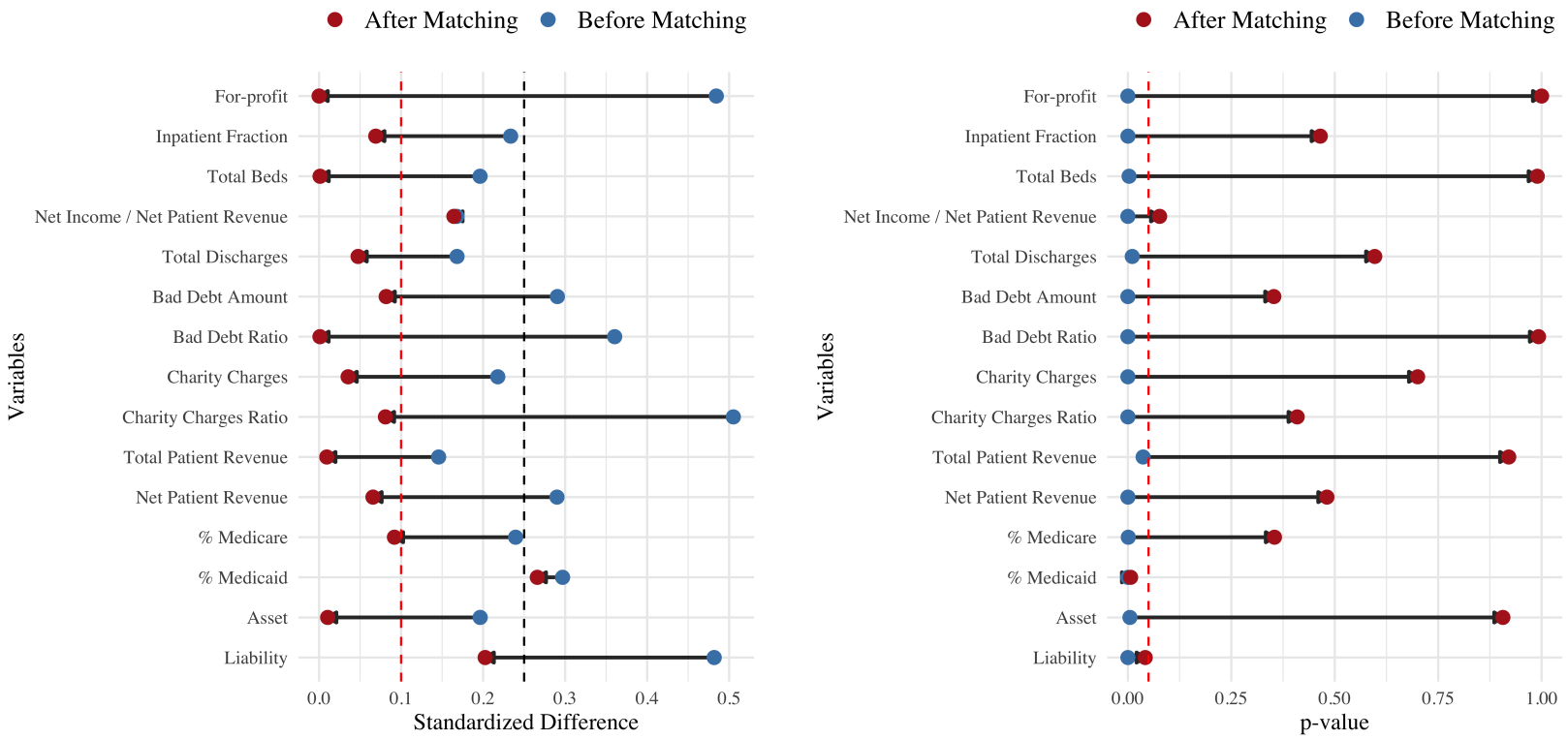


Figure 1:
Inpatient Admissions by Metropolitan Statistical Area (MSA) in 2023
Source: Merative™ MarketScan® Research Database

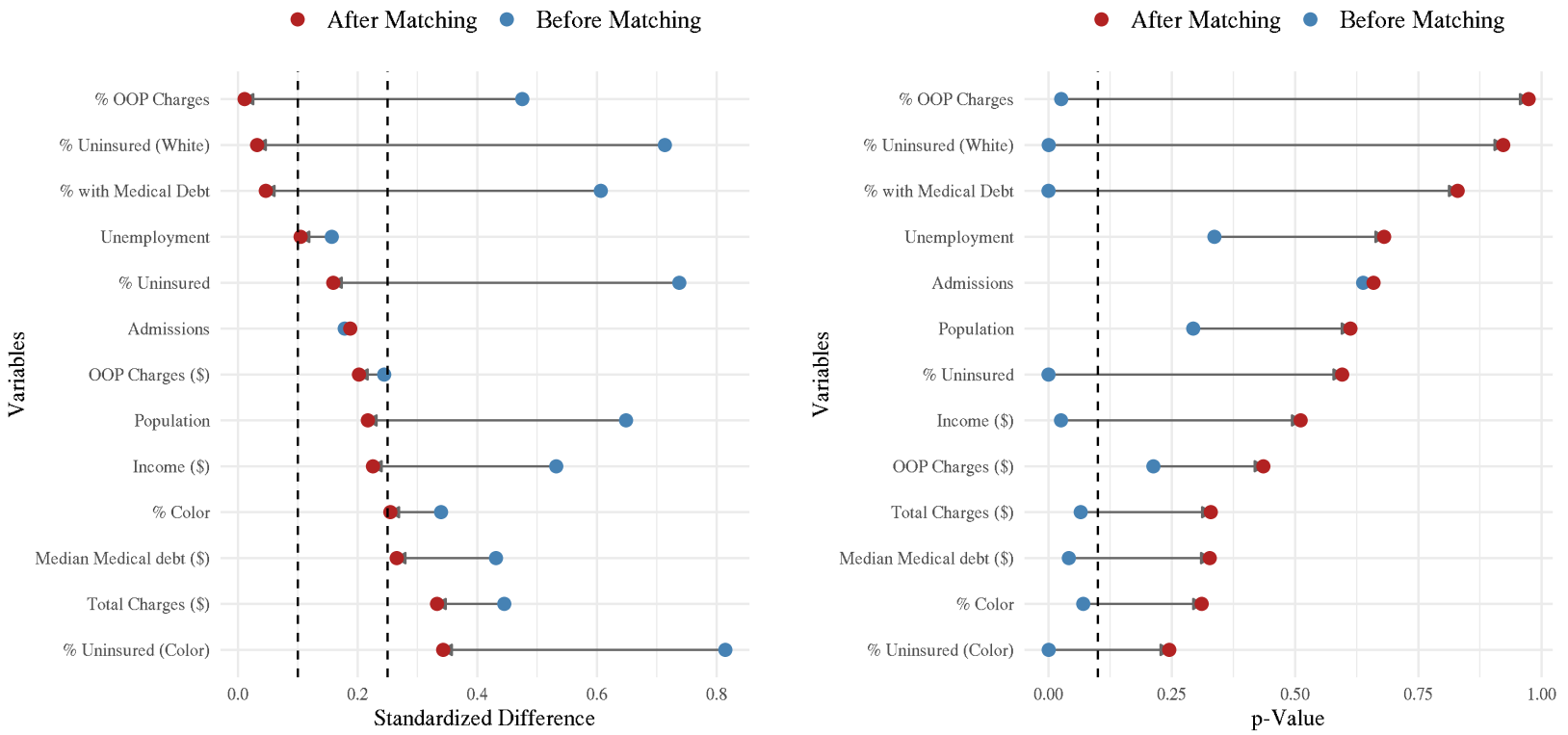


(a) Standardized Difference

(b) p-Value

Figure 2:
Matching Hospitals

This figure shows the standardized differences and p-values between treated and control hospitals in the year before the policy, before and after matching. Each treated hospital is matched to three control hospitals using nearest-neighbor matching based on the Euclidean distance across the matching variables. Matching variables include for-profit status, the number of beds, profit margins, bad debt amount, and charity charges amounts, measured in the year before the policy. Standardized difference is defined as the absolute value of the average difference between matched treated and control observations, divided by the standard deviation computed over the full sample.



(a) Standardized Difference **(b)** p-value

Figure 3:
Matching MSAs

This figure shows the standardized differences and p-values between treated and control MSAs in the year before the policy, before and after matching. Each treated MSA is matched to five control MSAs using nearest-neighbor matching based on the Euclidean distance across the matching variables. Matching variables include median-income, uninsured rate, the number of inpatient admissions, population, and % black population, measured in the year before the policy. Standardized difference is defined as the absolute value of the average difference between matched treated and control observations, divided by the standard deviation computed over the full sample.

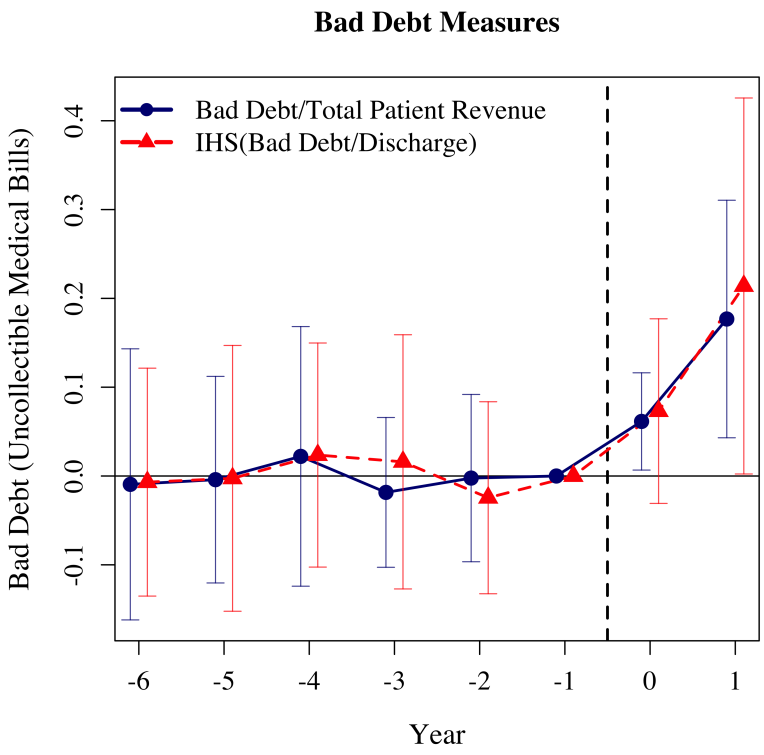
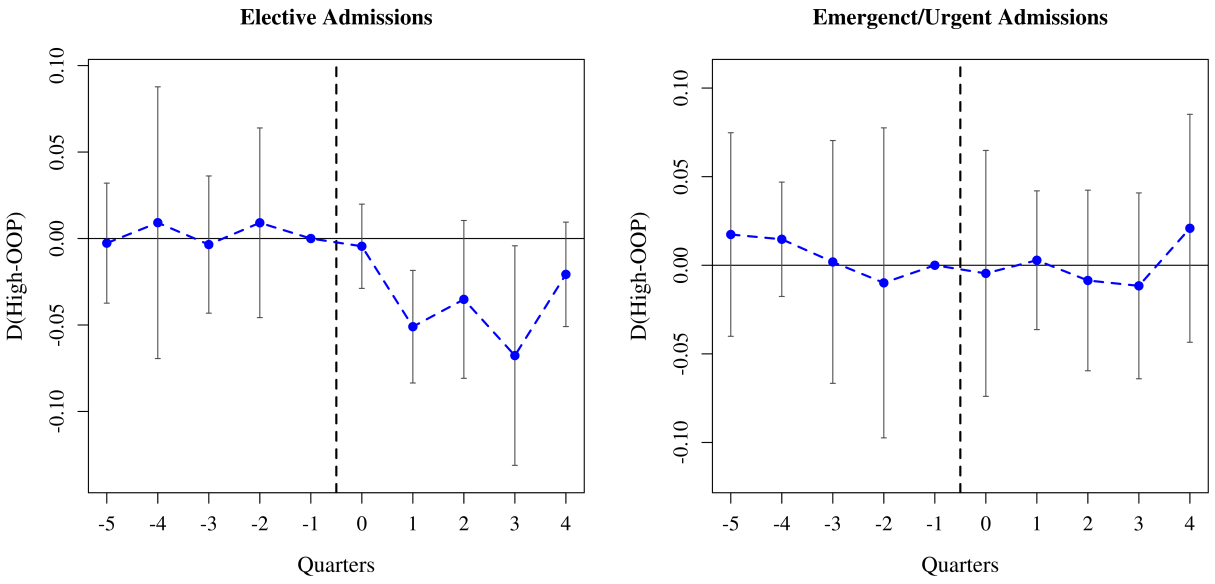


Figure 4:
Effect Timing on Bad Debt

This figure shows the timing of the effect of the medical debt credit reporting ban on hospitals' bad debt (uncollectible medical bills) relative to total patient revenue (blue line), and the inverse hyperbolic sine transformation of bad debt per inpatient discharge (red line). This figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-in-differences estimation. Hospital and year indicators are interacted with cohorts of matched hospitals as fixed effects. Controls include county pre-policy below/above-median income and uninsured rate, each interacted with year × reporting-ban event indicators as fixed effects. Standard errors are clustered at the state level.



(a) Elective

(b) Emergency & Urgent

Figure 5:
Effect Timing on Insurance Screening

This figure shows the timing of the effect of the medical debt credit reporting ban on the composition of patients by insurance generosity, separately for elective admissions (Panel (a)) and emergency and urgent admissions (Panel (b)). This figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-in-differences estimation. Elective admissions include non-emergency surgical admissions. Emergency and urgent admissions include emergency surgical and non-surgical (acute medical and behavioral) admissions. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, and employment status. Standard errors are clustered at state level.

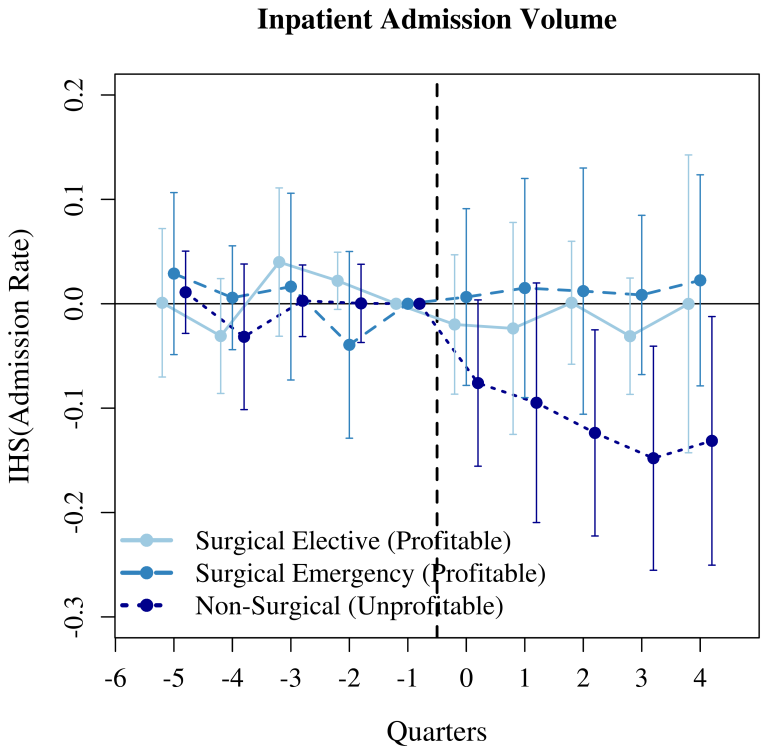
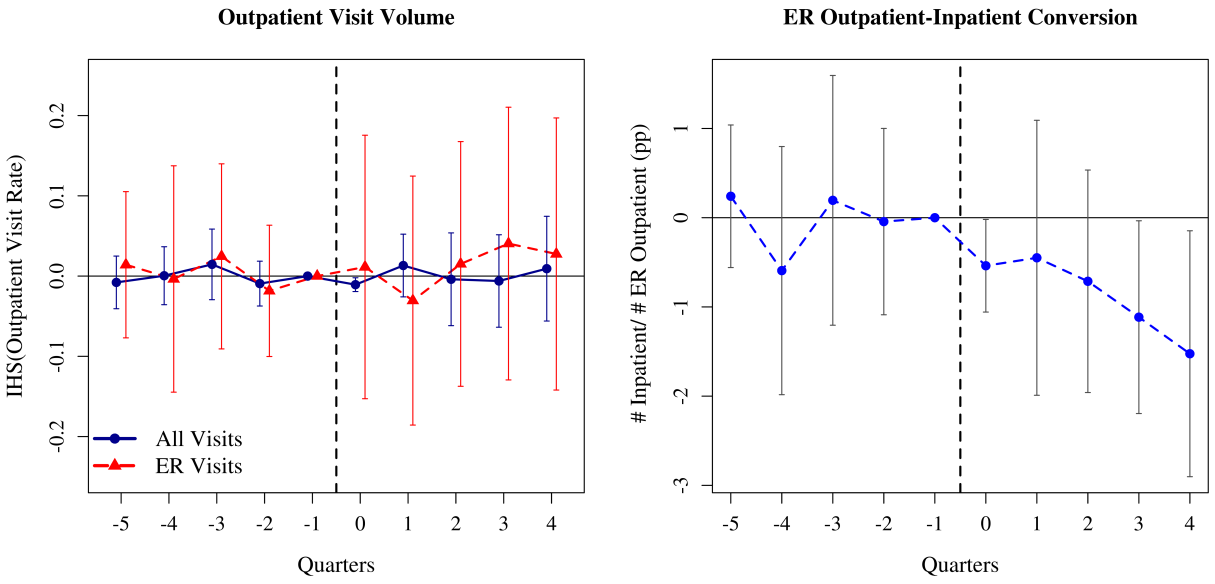


Figure 6:
Effect Timing on Inpatient Admission Volume

This figure shows the timing of the effect of the medical debt credit reporting ban on the volume of inpatient admissions. This figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-in-differences estimation. Profitable admissions are elective and emergency surgical admissions; unprofitable admissions are non-surgical (acute medical and behavioral) admissions. MSA and quarter indicators are interacted with cohorts of matched MSAs as fixed effects. Standard errors are clustered at the state level.

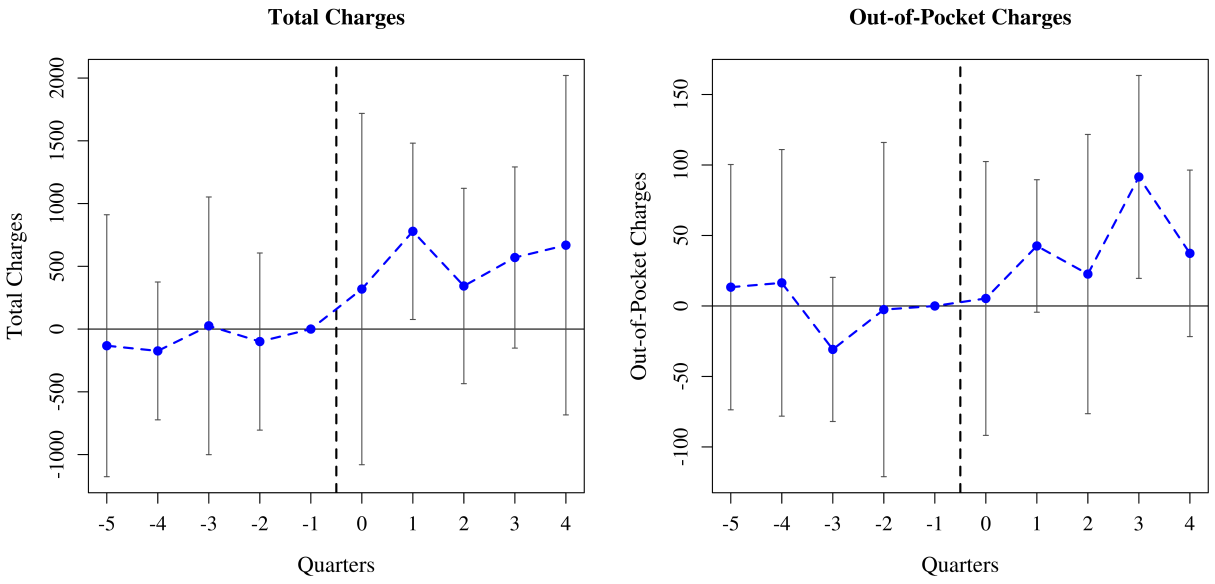


(a) Outpatient Visit Volume

(b) ER to Inpatient Conversion

Figure 7:
Effect Timing on Outpatient Volume and Inpatient Conversion

This figure shows the timing of the effect of medical debt credit reporting bans on outpatient visit volume (Panel (a)) and the ER-to-inpatient conversion rate for unprofitable inpatient admissions (Panel (b)). Unprofitable inpatient admissions include acute medical admissions and behavioral admissions. This figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-in-differences estimation. MSA and quarter indicators are interacted with cohorts of matched MSAs as fixed effects. Standard errors are clustered at the state level.



(a) Total Charges

(b) OOP Charges

Figure 8:
Effect Timing on Hospital Charges in High-Concentration Markets

This figure shows the timing of the effect of the medical debt credit reporting ban on hospitals' total charges (Panel (a)), and on OOP charges (Panel (b)), in MSAs in the top tercile of hospital market concentration. This figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-in-differences estimation. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, employment status, emergency status, and insurance plan types (e.g., High-Deductible, PPO). Standard errors are clustered at the state level.

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Appendix for Online Publication

A SIPP Empirical Specification

This section describes the empirical strategy used to examine the effects of medical debt credit reporting bans on year-end medical debt, healthcare utilization, and health outcomes using the SIPP data introduced in Section 3.1. I estimate:

$$Y_{i,e,t} = \beta \, Treated_{i,e} \times Post_{t,e} + \alpha_{i,e} + X_i^{\top} \lambda_{t,e} + \varepsilon_{i,e,t}, \tag{5}$$

where $Y_{i,e,t}$ denotes an outcome for individual i in event sample e and year t. $Treated_{i,e}$ indicates residence in the state treated in event e, and $Post_{t,e}$ indicates the post-policy period. $\alpha_{i,e}$ denotes individual $\times$ event fixed effects, allowing individuals in never-treated states to serve as controls in multiple event samples. $X_i^{\top} \lambda_{t,e}$ denotes separate control-bin $\times$ year $\times$ event fixed effects based on age, race, educational attainment, insurance type, and lagged income and home-loan-balance deciles. The coefficient β captures the average post-policy change for treated individuals relative to comparable controls.

To avoid changes in treatment status due to migration, I restrict the sample to individuals who do not move across states during the sample period. Movers account for only 1.03% of the full sample.

A limitation of the SIPP analysis is its annual frequency and short post-treatment window. Because outcomes are observed only at year-end and the sample ends in 2024, Colorado has two post-policy observations, year-end 2023 and 2024, whereas New York has only one meaningful post-policy observation, year-end 2024. As a result, the pooled estimate is informed more heavily by the Colorado policy and should be interpreted with this difference in post-treatment exposure in mind.

Table A1:
Household Medical Bill Defaults and Healthcare Outcomes

This table reports stacked difference-in-differences estimates using annual year-end individual-level SIPP data. Treated individuals are those residing in Colorado or New York. Post is an indicator equal to one if the state's credit reporting ban is in effect as of year-end. Individuals are classified as low-income if their pre-treatment income falls below the median. In Panel A, Columns (1)–(3) use an indicator for having a positive unpaid medical bill balance, while Columns (4)–(6) use the inverse hyperbolic sine (IHS) of unpaid medical bill balances. Panel B reports healthcare utilization and health status outcomes. All specifications include individual-by-event and year-by-event-by-control fixed effects. Controls include age, race, education, insurance type, and lagged income and home loan balance deciles. Events are defined separately for the credit reporting bans in Colorado and New York. Standard errors are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Medical Bill Defaults

Dependent Variables:	D(Medical Debt)			IHS (Medical Debt Amount)		
	Whole Sample (1)	Low-Income (2)	High-Income (3)	Whole Sample (4)	Low-Income (5)	High-Income (6)
Treated × Post	-0.009	0.019**	-0.008	-0.033	0.210***	-0.070
	(0.007)	(0.008)	(0.006)	(0.060)	(0.063)	(0.054)
Dep Var Mean:	0.088	0.101	0.073	1,183	1,634	756
Individual × Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Event × Control FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	218,954	109,642	109,312	218,954	109,642	109,312
Adjusted R^2	0.455	0.479	0.488	0.483	0.510	0.507

Panel B: Healthcare Utilization and Health Status

Dependent Variables:	IHS (# Physician Visits)		IHS (# Hospital Nights)		D(Poor Health Status)	
	Low-Income (1)	High-Income (2)	Low-Income (3)	High-Income (4)	Low-Income (5)	High-Income (6)
Treated × Post	-0.018	0.029	0.044	0.025	0.017	-0.007
	(0.036)	(0.053)	(0.038)	(0.028)	(0.012)	(0.005)
Dep. Var. Mean	5.936	4.970	1.118	0.522	0.261	0.118
Individual × Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Event × Control FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	108,857	108,832	108,857	108,832	109,642	109,312
Adjusted R^2	0.557	0.563	0.339	0.313	0.540	0.509

Table A2:
Insurance Screening: Robust Measaures

This table reports regression estimates examining the effects of medical debt credit reporting bans on the composition of patients by insurance generosity. The dependent variable, D(High-OOP), is an indicator equal to one if an admission's total out-of-pocket payment (co-pays, deductibles, and co-insurance) exceeds the specified percentile of the pre-treatment out-of-pocket payment distribution, and zero otherwise. Columns (1)–(4) use indicator variables for whether total OOP exceeds the 70th or 80th percentile of the pre-treatment distribution, while Columns (5)–(6) use an indicator for whether the ratio of OOP to total charges (insurer plus OOP) exceeds the 75th percentile. The cutoffs are calculated separately within groups defined by state, month of year, admission type (i.e., surgical, acute medical, and behavioral), and emergency status. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, and employment status. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	D(High-OOP)					
Measures:	OOP ($) ≥ 70th Cutoff		OOP ($) ≥ 80th Cutoff		% OOP ≥ 75th Cutoff	
Admission Types:	Elective	Emergency & Urgent	Elective	Emergency & Urgent	Elective	Emergency & Urgent
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.045***	0.003	-0.044***	0.009	-0.027***	-0.002
	(0.008)	(0.007)	(0.005)	(0.006)	(0.008)	(0.008)
Dep Var Mean :	0.311	0.313	0.211	0.212	0.252	0.262
MSA × Match FE	Yes	Yes	Yes	Yes	Yes	Yes
Qrt. × Match × Patient Types FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,609	184,901	32,609	184,901	32,609	184,901
Adjusted R^2	0.070	0.061	0.053	0.051	0.047	0.094

Table A3:
Insurance Screening: OOP Cutoffs

This table reports regression results examining the effects of medical debt credit reporting bans on the composition of patients by insurance generosity across percentile groups for elective inpatient admissions. In Columns (1) to (5), the dependent variables are indicators equal to one if an admission's total OOP charges (co-pays, deductibles, and co-insurance) fall within a given percentile range of the pre-treatment OOP distribution, and zero otherwise. In Columns (6) to (10), the dependent variables are indicators based on the ratio of OOP payments to total charges (insurer plus OOP). The cutoffs are calculated separately within groups defined by state, month of year, admission type, and emergency status. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, and employment status. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	D(High OOP: Elective)									
Measure:	OOP ($)					% OOP				
Percentiles:	[0–20]	[20–40]	[40–60]	[60–80]	[80–100]	[0–20]	[20–40]	[40–60]	[60–80]	[80–100]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated × Post	0.031	0.062***	0.009	-0.021	-0.044***	0.040	0.059**	-0.009	-0.001	-0.037***
	(0.027)	(0.018)	(0.016)	(0.015)	(0.005)	(0.027)	(0.023)	(0.016)	(0.008)	(0.008)
Dep Var Mean:	0.353	0.400	0.294	0.201	0.206	0.350	0.393	0.287	0.194	0.194
OOP Amount Mean:	7.85	65.0	333.6	1,204.2	3,697	14.9	91.9	490.7	1,621.3	3,230
OOP Fraction Mean:	0.0003	0.002	0.012	0.049	0.128	0.0001	0.001	0.007	0.034	0.164
MSA × Match FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Qrt. × Match × Patient Types FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,609	32,609	32,609	32,609	32,609	32,609	32,609	32,609	32,609	32,609
Adjusted R^2	0.103	0.085	0.123	0.036	0.053	0.112	0.105	0.113	0.036	0.142

Table A4:
Are Patients Facing High Out-of-Pocket Costs Disadvantaged?

This table shows the correlations between inpatient out-of-pocket (OOP) charges and MSA-level characteristics, using the sample from the pre-treatment period (Jan 2022 to July 2023). MSA-level uninsured rate and the fraction of population with medical debt are scaled into percentage points. The dependent variable is the dollar amount of OOP charges in Columns (1) and (5); the ratio of OOP to total charges in Columns (2) and (6); an indicator for whether an admission's total OOP charges (co-pays, deductibles, and co-insurance) exceed the 75th percentile of the pre-treatment OOP distribution in Column (3); and an analogous indicator based on the OOP ratio in Column (4). The cutoffs are calculated separately within groups defined by state, month of year, admission type (i.e., surgical, acute medical, and behavioral), and emergency status. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, and employment status. Standard errors, clustered at the MSA level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	OOP ($)	OOP (%)	D(High-OOP)	D(High % OOP)	OOP ($)	OOP (%)
	(1)	(2)	(3)	(4)	(5)	(6)
log(Income)	-787.4***	-0.122***	-0.035**	-0.100***	-346.2***	-0.068***
	(141.4)	(0.015)	(0.017)	(0.020)	(124.1)	(0.011)
% Uninsured					26.7***	0.002***
					(6.78)	(0.0004)
% with Medical Debt					11.8**	0.002***
					(5.00)	(0.0004)
Dep Var Mean:	1,601	0.124	0.253	0.255	1,601	0.124
Income Mean:	81,110					
% Uninsured Mean:	9.26					
% with Medical Debt Mean:	13.5					
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Patient Types FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	679,814	679,814	679,814	679,814	679,814	679,814
Adjusted R^2	0.136	0.181	0.070	0.075	0.143	0.191

Table A5:
Admission Volume by DRG: Alternative Sample Construction

This table reports the regression results examining the effects of medical debt credit reporting bans on the volumes of hospitals' unprofitable inpatient admissions. Unprofitable admissions are non-surgical (acute medical and behavioral) admissions. In contrast to Table 4, the sample includes all MSA–DRG–quarter cells, assigning a value of zero when a DRG does not appear in a given MSA–quarter. The regressions include indicators for whether the DRG-level share of ICU stays, the share of non-routine discharge transfers, and the average length of stay (LOS), measured in the pre-treatment period, exceed the admission-type-specific median. The observations are at the MSA-by-Quarter-by-DRG level. Controls include above/below-median indicators for MSA-level median income and uninsured rate, measured in the year before the policy. MSA and quarter indicators are interacted with cohorts of matched MSAs and DRGs as fixed effects. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	IHS(Admission per 1,000 Enrollee)			
Profitability:	Unprofitable			
	(1)	(2)	(3)	(4)
Treated × Post	-0.0049**	-0.0068***	-0.0066**	-0.0072***
	(0.0018)	(0.0022)	(0.0024)	(0.0024)
Treated × Post × D(High-ICU DRG)		0.0038**		
		(0.0016)		
Treated × Post × D(High-Transfer DRG)			0.0031*	
			(0.0015)	
Treated × Post × D(High-LOS DRG)				0.0037*
				(0.0020)
Dep Var Mean:	0.0340	0.0340	0.0340	0.0340
MSA × Match × DRG FE	Yes	Yes	Yes	Yes
Qrt. × Match × DRG FE	Yes	Yes	Yes	Yes
Observations	77,406	77,406	77,406	77,406
Adjusted R^2	0.532	0.568	0.593	0.529

Table A6:
Heterogeneity on Occupancy and Contract Labor

This table examines the relationship between hospitals' occupancy, contract labor share, and operating expenses, and explores heterogeneity based on these variables. Contract labor share is defined as contract labor costs divided by total labor costs, and occupancy is defined as the share of available bed-days that are utilized. Panel A presents correlations estimated using the pre-treatment sample. Panel B examines heterogeneity in the effects of credit reporting bans on the volume of unprofitable admissions, based on whether the MSA-level average occupancy rate or contract labor intensity is above the median. Unprofitable admissions are non-surgical (acute medical and behavioral) admissions. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Pre-Shock Occupancy, Contract Labor, and Operating Cost

Dependent Variables:	Contract Labor Share	IHS(Operation Expense per Discharge)
	(1)	(2)
Utilized Bed-Days Fraction	0.055**	
	(0.023)	
Contract Labor Share		0.643***
		(0.070)
Dep Var Mean	0.058	15,957
Hospital FE	Yes	Yes
State × Year FE	Yes	Yes
Observations	2,679	2,677
Adjusted R^2	0.667	0.944

Panel B: Heterogeneity on Occupancy and Contract Labor Share

Dependent Variable:	IHS(Admission per 1,000 Enrollee)	
Profitability:	Unprofitable	
	(1)	(2)
Treated × Post	-0.018	-0.017
	(0.064)	(0.065)
Post × D(High-Occupancy)	0.204***	
	(0.053)	
Treated × Post × D(High-Occupancy)	-0.229*	
	(0.122)	
Post × D(High-Contract-Labor)		0.104*
		(0.053)
Treated × Post × D(High-Contract-Labor)		-0.140**
		(0.050)
MSA × Match FE	Yes	Yes
Qrt. × Match FE	Yes	Yes
Observations	798	798
Adjusted R^2	0.600	0.633

Table A7:
External Validity of Admission Volumes

This table reports regression results assessing the external validity of the baseline findings using CMS cost report data. Columns (1) to (6) examine inpatient discharges. The dependent variables in these columns are inverse hyperbolic sine transformations of the number of inpatient discharges. Columns (1) to (3) report results for general inpatient discharges, including childbirth, surgical, and acute medical discharges. Columns (4) to (6) report results for discharges in hospitals' inpatient psychiatric facilities. Within each discharge category, outcomes are reported separately for all discharges, discharges covered by public insurance (Medicare and Medicaid), and discharges covered by private insurance. Column (7) examines the operating room expense ratio, defined as operating room expenses divided by total operating expenses. Hospital and year indicators are interacted with cohorts of matched hospitals as fixed effects. Controls include county pre-policy below/above-median income and uninsured rate, each interacted with year × reporting-ban event indicators as fixed effects. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	IHS(Inpatient Discharges)						Operating Room Expense Ratio
Facilities:	General			Inpatient Psychiatric			
Insurance Types:	All	Public	Private	All	Public	Private	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × Post	-0.037**	-0.054***	-0.034*	-0.094**	-0.131*	-0.081*	0.003*
	(0.015)	(0.018)	(0.018)	(0.044)	(0.071)	(0.046)	(0.002)
Dep Var Mean:	11,700	7,634	4,008	1,070	543	525	0.081
Hospital × Match FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Match FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,529	4,529	4,529	1,292	1,292	1,292	4,564
Adjusted R^2	0.985	0.969	0.915	0.689	0.401	0.403	0.879

Table A8:
Inpatient Prices: Excluding Elective Admissions

This table reports the regression results examining the effects of medical debt credit reporting bans on hospitals' inpatient prices, excluding elective admissions. Hospital market concentration at the MSA level is measured by the Herfindahl–Hirschman Index (HHI) based on available hospital bed-days. Columns (1)–(2) include on MSAs in the top tercile of concentration, and Columns (3)–(4) on those in the bottom two terciles. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, employment status, emergency status, and insurance plan types (e.g., High-Deductible, PPO). Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Total Charges($)	OOP Charges($)	Total Charges($)	OOP Charges($)
Market Concentration:	High		Low	
	(1)	(2)	(3)	(4)
Treated × Post	721.6***	45.0*	113.4	-13.7
	(136.2)	(24.1)	(300.6)	(16.1)
Dep Var Mean:	17,905	1,294	22,347	1,377
% of Mean:	4.03%	3.48%	0.51%	-0.99%
p-value (*Coef* Column (1) = (3)):	0.043**			
p-value (*Coef* Column (2) = (4)):		0.042**		
MSA × Match FE	Yes	Yes	Yes	Yes
Qrt. × Match × Patient-Types FE	Yes	Yes	Yes	Yes
Observations	54,828	54,828	269,956	269,956
Adjusted R^2	0.681	0.569	0.609	0.525

Table A9:
Heterogeneity on MSA Characteristics: Inpatient Prices

This table shows heterogeneous effects in inpatient prices (total charges and OOP charges) by whether the MSA-level median income is above the median. The sample is confined to MSAs with high hospital market concentration, defined as those in the top tercile of the Herfindahl–Hirschman Index (HHI) based on available hospital bed-days. MSA and quarter indicators are interacted with cohorts of matched MSAs and patient types as fixed effects. Patient types include indicators for diagnosis-related group (DRG), age deciles, gender, industry, employment status, emergency status, and insurance plan types (e.g., High-Deductible, PPO). Standard errors are clustered at the state level.

Dependent Variables:	Total Charges ($)	OOP Charges ($)
	(1)	(2)
Treated × Post	967.7***	47.9**
	(245.3)	(16.9)
Post × D(High Income)	185.8	11.1
	(817.6)	(4.19)
Treated × Post × D(High Income)	-353.1*	-35.0
	(178.3)	(44.9)
Dep Var Mean:	20,948	1,291
MSA × Match FE	Yes	Yes
Qrt. × Match × Patient-Types FE	Yes	Yes
Observations	59,088	59,088
Adjusted R^2	0.698	0.552

Table A10:
Heterogeneity on MSA Characteristics: Low-risk Patients

This table presents heterogeneity in the baseline results by MSA-level characteristics for the subsample of patients with low financial risk of defaulting on out-of-pocket (OOP) bills, defined as those with total OOP payments (co-pays, deductibles, and co-insurance) below the 60th percentile of the pre-treatment OOP distribution. The cutoffs are calculated separately within groups defined by state, month of year, admission type, and emergency status. The independent variables include interaction terms with indicators for whether the MSA-level median income is above the median. The dependent variable is: in Column (1), the inverse hyperbolic sine of unprofitable admissions per 1,000 enrollees; in Column (2), the number of repeated ER visits 20–40 days after the initial visit; in Column (3) and (4), the total and OOP charges for inpatient admissions. Standard errors, clustered at the state level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	IHS(Unprofitable Admissions)	# Repeated ER Visits	Inpatient Prices	
	(1)	(2)	(3) Total Charges	(4) OOP Charges
Treated × Post	-0.142**	0.0116*	1456.0***	9.87**
	(0.063)	(0.0063)	(469.6)	(3.43)
Post × D(High Income)	-0.078	0.0009	-668.6	0.236
	(0.075)	(0.0113)	(786.1)	(4.38)
Treated × Post × D(High Income)	0.170**	-0.0225**	-1640.2*	-7.97
	(0.079)	(0.0099)	(902.9)	(6.64)
Dep Var Mean:	1.56	0.1898	20,050	324.8
MSA × Match FE	Yes	Yes	Yes	Yes
Qrt. × Match FE	Yes			
Qrt. × Match × Patient-Types FE		Yes	Yes	Yes
Observations	798	1,982,415	33,575	33,575
Adjusted R^2	0.683	0.059	0.639	0.548

Table A11:
Robustness: Standard Error Clusters

This table reports the baseline results using alternative standard-error clustering specifications: clustering at the MSA level and two-way clustering at the MSA and year/quarter levels. The dependent variable is: in columns (1)–(2), hospital bad debt as a share of total patient revenue (percentage points); in columns (3)–(4), an indicator for less generous insurance coverage: OOP for elective admissions above the 75th percentile of the pre-shock distribution; in columns (5)–(6), the inverse hyperbolic sine of unprofitable admissions per 1,000 enrollees; and in columns (7)–(10), inpatient total charges and OOP charges. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Bad Debt / Total Revenue		D(High-OOP: Elective)		IHS(Unprofitable Admissions)		Inpatient Prices			
	(1)	(2)	(3)	(4)	(5)	(6)	(7) Total Charges	(8) OOP Charges	(9) Total Charges	(10) OOP Charges
Treated × Post	0.110**	0.110***	-0.043**	-0.043*	-0.116**	-0.116**	749.4**	34.3**	749.4*	34.3**
	(0.048)	(0.030)	(0.018)	(0.022)	(0.057)	(0.051)	(305.6)	(15.0)	(347.6)	(12.2)
Baseline FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	MSA	MSA + Year	MSA	MSA + Qtr.	MSA	MSA + Qtr.	MSA	MSA	MSA + Qtr.	MSA + Qtr.
Observations	4,610	4,610	32,609	32,609	798	798	59,088	59,088	59,088	59,088
Adjusted R^2	0.697	0.697	0.059	0.059	0.602	0.602	0.698	0.553	0.698	0.553

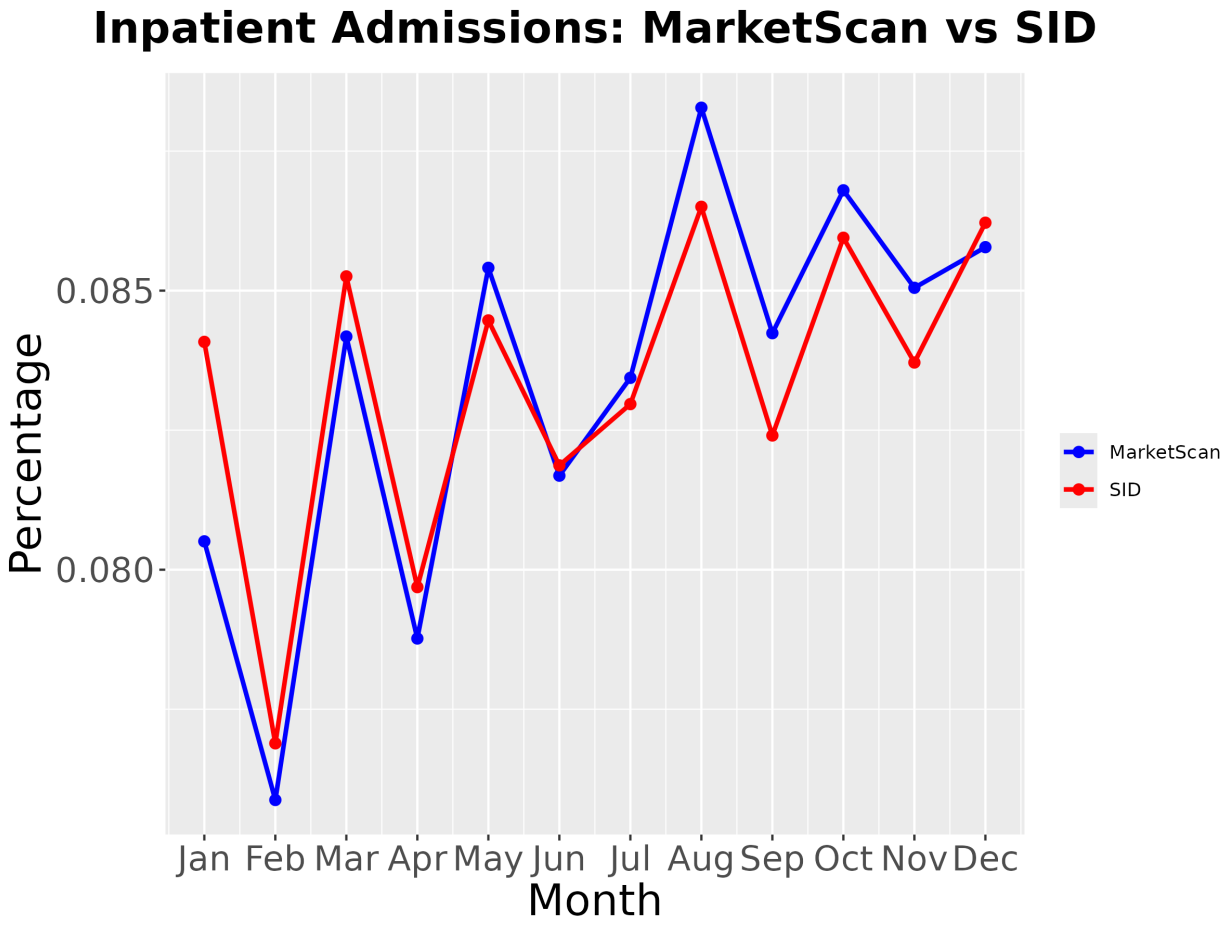
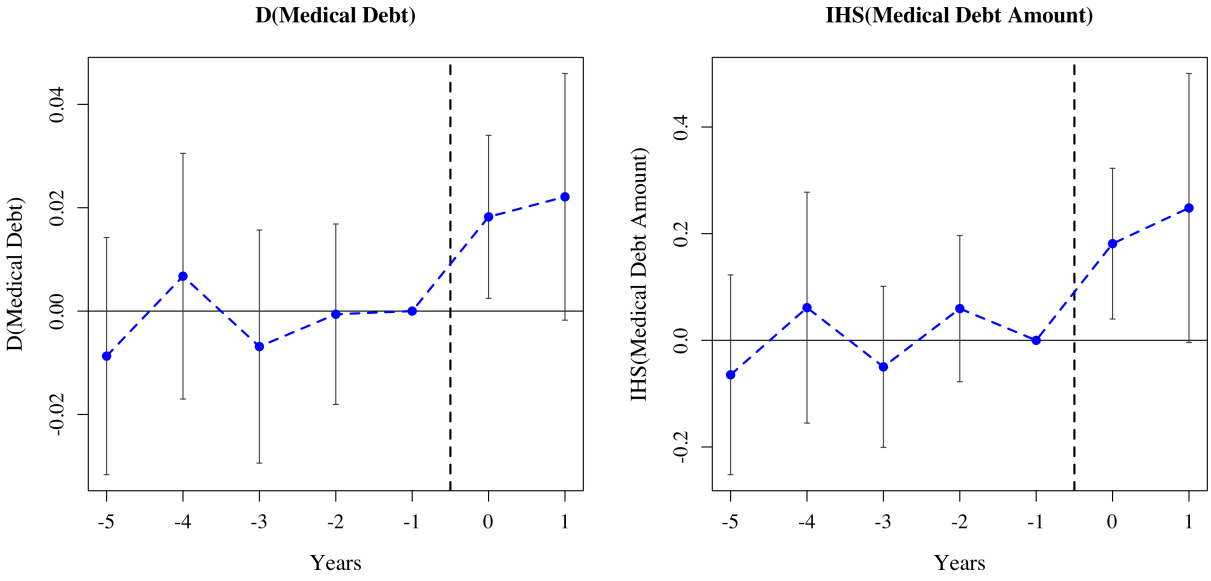


Figure A1: Monthly Trends of Inpatient Admission

The figure shows the monthly fraction of inpatient admissions over total inpatient admissions in 2023 for the MarketScan® database and the HCUP State Inpatient Database (SID). The aggregated monthly admission of SID is obtained from HCUP-US Central Distributor.



(a) D(Medical Debt)

(b) IHS(Medical Debt Amount)

Figure A2: Effect Timing on Medical Bill Defaults

This figure shows the timing of the effect of the medical debt credit reporting bans on whether individuals default on medical debt (Panel (a)) and on the inverse hyperbolic sine transformation of medical debt amounts (Panel (b)). The sample is confined to low-income individuals. Observations are at the individual-year level. This figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-in-differences estimation. The specification includes individual-by-event and year-by-event-by-control fixed effects. Controls include indicators for age, race, education, and insurance type, as well as lagged income and home loan balance deciles. Standard errors are clustered at the state level.

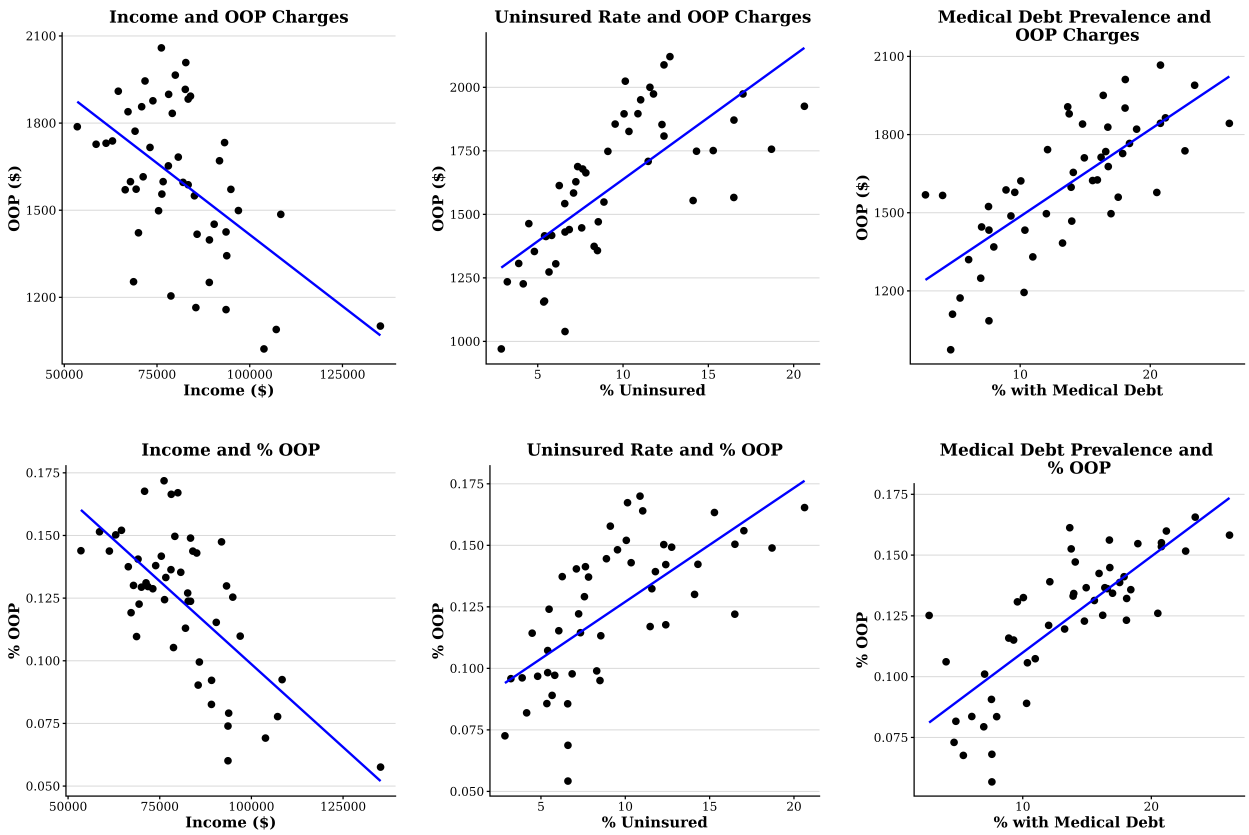


Figure A3: Binned Scatter Plot: Are Patients Facing High Out-of-Pocket Costs Disadvantaged?

This figure shows the correlations between inpatient out-of-pocket (OOP) charges and MSA-level characteristics, using the sample from the pre-treatment period (Jan 2022 to July 2023). MSA-level % uninsured rate and % population with medical debt are scaled into percentage points. The top row reports the dollar amount of OOP charges, while the bottom row reports OOP charges as a fraction of total charges (insurer plus OOP).

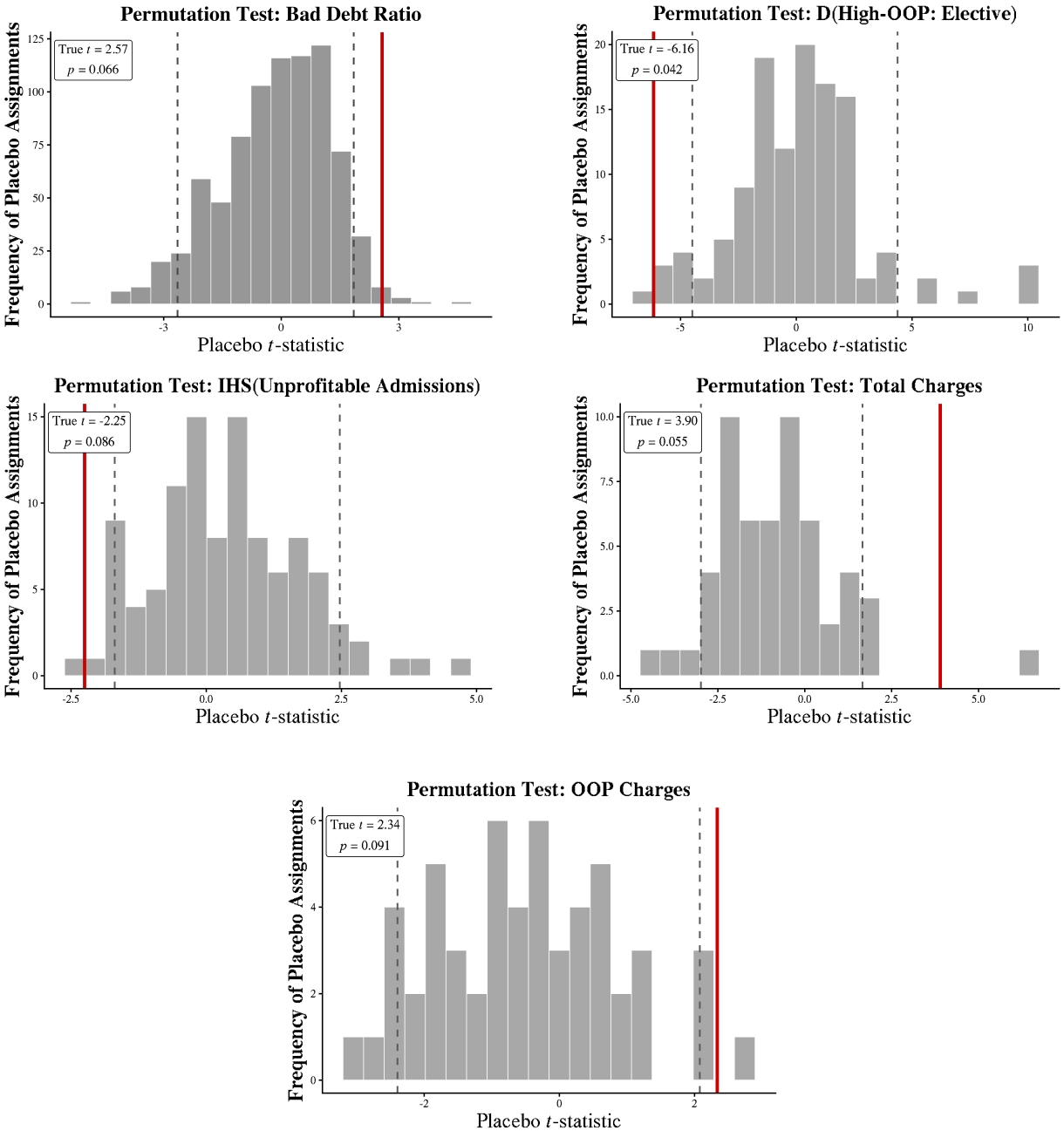


Figure A4: Permutation Tests: Randomization Inference for Main Outcomes
Each panel plots the distribution of placebo t-statistics for the baseline difference-in-differences coefficient, obtained by assigning treatment to all possible pairs of control states and re-estimating the baseline specification for each placebo assignment (MacKinnon & Webb 2020). The red vertical line denotes the t-statistic from the actual treated states, and the black dotted lines denote the 5th and 95th percentiles of the placebo distribution. The reported p-value equals the fraction of placebo assignments with an absolute t-statistic larger than the actual absolute t-statistic.