Hospital Mergers, Health Provider Concentration, and Medical Debt

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Abstract

We leverage consumer credit panel data covering the 2011–2019 period to examine how hospital consolidation impacts individual medical debt. In our preferred event study specifications, we find that exposure to hospital mergers causes significant increases in the likelihood of having medical debt in collections, the flow of new debt generated over the previous twelve months, and the total outstanding stock. Furthermore, these effects amplify over time: five years after treatment, the effect on our outcomes of interest translates to increases that range from 11% to 23%. We find evidence suggesting that some medical expenses may be paid through credit cards, with mergers increasing outstanding credit card debt. In a placebo test, we find no effect of merger exposure on analogous outcomes for types of non-medical debt in collections that are plausibly unaffected by hospital mergers. Using comprehensive county-level healthcare spending and utilization data from the IHME Disease Expenditure Project, we investigate the mechanisms underlying these effects and identify three complementary channels: hospital mergers raise the price of commercially insured care, expand the set of healthcare encounters for which patients bear direct out-of-pocket financial responsibility, and shift the local payer mix away from Medicare and toward commercial insurance. Together, these channels increase both the cost and the breadth of patients' financial exposure to healthcare.

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I Introduction

In a multi-payer healthcare system such as that which prevails in the United States, health providers frequently perform services under the assumption that they will be reimbursed from multiple sources: patients themselves, the patient's commercial insurer, a government insurer, or even other patients. Moreover, the price for a healthcare service frequently depends on both the type and the identity of the payer (Cooper et al., 2019). Most commercial insurance plans build patient participation into ongoing care and use that mechanism to steer patients to different healthcare services and to preferred providers as a means of cost-sharing (Aron-Dine, Einav and Finkelstein, 2013; Gowrisankaran, Nevo and Town, 2015).

Medical debt is the logical result: the provider seeks payment from the patient for delivered-but-uncompensated care (Messac, 2024), and patients may be surprised that services they thought were covered are nonetheless being billed (Cooper, Scott Morton and Shekita, 2020). Moreover, healthcare needs are inherently risky (hence the need for insurance in the first place), so necessary care may be delivered to both uninsured and under-insured patients with high-deductible health plans (Tolbert et al., 2025; Brot-Goldberg et al., 2017).

A large literature now demonstrates that a lack of competition in the healthcare industry is responsible, to a considerable degree, for the fact that healthcare prices are higher and actual healthcare provided is lower in the United States, relative to peer countries. A subset of that literature has identified horizontal concentration in the hospital sector as responsible for both higher prices for the care that is delivered and follow-on effects, such as a high marginal cost of employing an additional worker, in the form of inflated premiums for employer-provided health insurance (Brot-Goldberg et al., 2024). However, one outcome of hospital mergers that has not been studied to the same degree is medical debt, likely because individual-level credit records are difficult to obtain, let alone link to individual-level healthcare data, such as insurance claims.

In this paper, we link together these two strands in the literature: merger-induced horizontal consolidation among hospitals, and medical debt. Using a staggered difference-in-differences and event study research design, with treatment assigned by geography, we show that mergers predicted to increase local area hospital concentration correspond to substantial increases in medical debt in the population. In our preferred specification, five years after treatment, hospital mergers increase the likelihood of having medical debt by 11%, the amount of medical debt accumulated over the previous 12 months by 20%, and the total stock of outstanding medical debt by 23%. Moreover, we find that hospital mergers lead to increases in credit card balances, suggesting that some patients carry medical expenses on their credit cards if they cannot immediately pay outright.

We then investigate the mechanisms through which hospital mergers generate medical debt, using comprehensive county-level healthcare spending and utilization data from the IHME Disease Expenditure Project (Dieleman et al., 2025). We identify three complementary channels. First, mergers raise the price of commercially insured care: private insurance spending per beneficiary-episode increases by approximately 4 percent five years after treatment, while utilization remains

stable, indicating that the spending increase is driven by higher unit prices rather than a greater volume of care. Second, mergers expand the set of healthcare encounters for which patients bear direct out-of-pocket financial responsibility. Total out-of-pocket spending rises by roughly 7 percent, but this increase is driven entirely by more encounters involving patient payments, not by higher charges per encounter—a novel finding in the hospital mergers literature, to our knowledge. Third, mergers shift the local payer mix away from Medicare and toward commercial insurance, so that a larger share of healthcare utilization is subject to the higher prices and broader cost-sharing obligations documented in the first two channels. We find no evidence that the medical debt effects are driven by an indirect labor market channel in which merger-induced increases in commercial insurance costs depress local earnings and employment (cf. Brot-Goldberg et al., 2024).

Beyond these three quantitative channels, we identify a fourth mechanism that, while not directly testable in our data, provides the institutional foundation for the price and cost-sharing effects we document: anti-competitive provisions in hospital–insurer contracts. Dominant hospital systems impose anti-steering clauses, anti-tiering restrictions, and all-or-nothing requirements on commercial insurers, contractually preventing insurers from directing patients to lower-cost competitors or placing high-cost hospitals in less-preferred network tiers. These provisions are the hospital-sector analog of the Most-Favored-Nations (MFN) clauses at issue in recent antitrust litigation, e.g. Ohio v. American Express Co. (2018): they neutralize downstream price competition by contractually prohibiting the intermediary from channeling demand toward lower-cost alternatives. When a merged hospital system imposes anti-steering language on insurers, it forecloses the primary mechanism through which insurers could discipline the price increases that mergers enable. One of the few remaining instruments available to insurers in this situation is to redesign their plans to include greater patient cost-sharing, thereby shifting the burden of deterring utilization of high-cost providers onto patients themselves. This institutional channel—documented in federal and state antitrust enforcement actions against Atrium Health and Sutter Health, among others—helps explain both the price increases and the expansion of patient financial exposure that we observe in the IHME data (United States v. Charlotte-Mecklenburg Hospital Authority, 2019; UFCW & Employers Benefit Trust v. Sutter Health, 2021).

A recent experimental literature has found that forgiving downstream medical debt—debt that has already been sent to collections—has little measurable effect on recipients' financial wellbeing, mental health, or healthcare utilization (Kluender et al., 2025). This finding might seem to suggest that medical debt is not itself a meaningful source of harm to patients or consumers. Our findings suggest a re-interpretation. The medical debt observable on credit reports represents only the final, residual layer of a much larger transfer of healthcare costs onto patients when healthcare markets become less competitive. Before a bill reaches collections, patients may pay it outright, finance it with a credit card, negotiate a reduced amount, or forgo care altogether to avoid incurring the expense. The fact that the debt which survives this process and lands in collections can be forgiven at pennies on the dollar with little welfare consequence may indicate that the economically significant harm occurs upstream: in the higher prices patients face, the broader set of encounters on which they bear out-of-pocket responsibility, and the less generous effective insurance coverage

that results from provider consolidation. Our contribution is to document those upstream, direct consequences of reduced provider competition and their downstream residuum. The medical debt increases we estimate should be understood not as the totality of the consumer harm from hospital mergers, but as a downstream indicator of a broader shift in the financial burden of healthcare from insurers and providers to patients.

Medical debt can lead to adverse credit consequences for patients and their families (also demonstrated by Kluender et al. 2025), which is why policy surrounding its reportability on credit records has fluctuated considerably since 2019. Along with the advent of the pandemic, this is the key reason why our study concludes in 2019, after which the medical debt observable in our credit panel dwindles to the point of unreliability. But beyond the immediate consequences of the debt that is our main outcome of interest, our findings are informative about access to healthcare in a context in which it is rationed. The mechanisms we study indicate that mergers increase patients' financial exposure to healthcare costs along both intensive and extensive margins: each episode of commercially insured care becomes more expensive, and more episodes generate out-of-pocket obligations in the first place. These findings imply that, in the wake of hospital mergers, commercial health insurance plans effectively become less generous relative to the total cost of care, even absent any explicit change in plan design, although our findings are also consistent with plans being redesigned to include greater patient participation, e.g., through higher deductibles.

This paper proceeds as follows: Section II explains how our analysis sample of hospital mergers and credit records is constructed. Section III explains our empirical approach, including summary statistics and our main estimating equation for the event study. Section IV reports our main results, including several dimensions of heterogeneity, as well as a placebo test. Section V uses aggregated healthcare spending and utilization data to investigate the mechanisms through which hospital mergers increase medical debt. Section VI concludes.

II Data

In this article, we rely primarily on two key datasets. The first is the University of California's Consumer Credit Panel (UCCCP), a longitudinal dataset commencing in 2004. UCCCP provides anonymized credit records for a nationally-representative 2 percent sample of U.S. adults with credit histories. It includes detailed demographic and geographic information, which allows us to identify the location of each individual over time. Specifically, we rely on ZIP code information to geolocate individuals. We first use the yearly US Department of Housing and Urban Development's ZIP-code-to-county crosswalks to assign each individual to a county (U.S. Department of Housing and Urban Development, 2019); then, we use Dorn's county-to-commuting-zone (CZ) crosswalk to assign each individual to a CZ (Autor and Dorn, 2013). The UCCCP data also includes individual-level credit scores, as well as tradeline-level data on every loan or credit record obtained by each individual and every debt that has gone into collections, such as the date the tradeline was opened, the initial amount, the current outstanding balances, and various indicators of the tradeline status at

each point in time. Crucially for this paper, starting in 2011, the UCCCP includes comprehensive tradeline-level data on medical debt in collections. To avoid potential confounding caused by the pandemic and possible biases introduced by changes in the reporting of medical debt by credit bureaus, we restrict the dataset to the 2011-2019 period.[1] Although the UCCCP data is available quarterly, we use only the Q4 archives because our indicator for hospital mergers is defined on an annual basis.

The second key dataset is data on hospital mergers from Cooper et al. (2024). This covers the period from 2000 to 2024 but, due to the UCCCP data restrictions mentioned above, we focus on mergers through 2019. These data include the universe of hospitals, their locations, their number of beds, and the hospital systems to which they belong, allowing us to identify, for each year and commuting zone, the mergers predicted to result in an increase in local sectoral concentration relative to the previous year. As we outline in section III.1, this is central to our methodology.

We supplement our primary datasets with information on the timing of the ACA Medicaid expansion across states (Kaiser Family Foundation, 2025), the share of uninsured people at the commuting zone level (from the Small Area Health Insurance Estimates program), and whether a ZIP code is predominantly rural (US Census Bureau, 2023). In Section V we consider impacts on local labor demand as a possible mechanism, utilizing data on hospital-specific employee counts by year from the American Hospital Association, as well as industry-by-county-by-year employee counts and average earnings from the U.S. Census Bureau's Quarterly Workforce Indicators (QWI). In that section, we also introduce the aggregated county-level health insurance claims data from the Institute for Health Metrics and Evaluation that we use for the bulk of the mechanisms analysis.

Appendix A describes the data cleaning process and its underlying assumptions in detail. However, there is a particularity of credit reporting data that is especially relevant in this context and merits discussion in the main text: approximately 18% of medical debt in collections tradelines in UCCCP do not appear on credit reports in the year when they are first created. For example, a collections tradeline for medical debt might report a creation date of January 2012, but only appears in UCCCP data for the first time in 2014. Given the impact that this could have on the levels and trends of the key outcomes of interest that we define in Section III.1 and in Appendix A, we take the tradeline creation date as accurate, create observations for the missing years, and estimate the corresponding balances through linear interpolation. For our outcomes of interest, we construct both a "raw/unadjusted" measure where we take the data in UCCCP at face value, and an "adjusted" measure that corrects for issues such as the one described above. All of our main results concern the adjusted measure; we report the same results with the unadjusted measure in Appendix B.

[1] For example, starting in 2023, medical debt in collections of less than $500 stopped being included in credit reports.

III Empirical approach

III.1 Empirical specification

We examine the impact of hospital consolidation and merger exposure on medical debt in collections using a dynamic difference-in-differences ("event study") design. This approach relies on the differential exposure to hospital mergers and variations in their timing to identify their dynamic effects on medical debt. Equation 1 outlines our specification:

$$y_{i,t} = \sum_{e=-3}^{E_i} \beta_e \mathbb{1}\{\ell_{i,z,c}\}\mathbb{1}\{t - c_i = e\} + \alpha_i + \lambda_t + \gamma_z + \phi x_{i,t} + \varepsilon_{i,t} \tag{1}$$

$y_{i,t}$ represents the outcome of interest for individual i in year t. We focus on three outcomes of interest: (i) whether an individual has medical debt in collections; (ii) the flow of new medical debt in collections generated over the previous 12 months; and (iii) the total amount of outstanding medical debt in collections.

c_i indicates the treatment cohort to which each individual i belongs: either 2014, 2015, 2016, 2017, or 2018, with $c_i = \infty$ for untreated individuals. $\mathbb{1}\{\ell_{i,z,c}\}$ indicates that i lived in a treated commuting zone z in treatment year c. $\mathbb{1}\{t - c_i = e\}$ indicates that $\{i,t\}$ corresponds to an observation of i in relative time period (event year) e, up to a limit of E_i for all individuals in cohort c_i. $x_{i,t}$ is a binary control that takes the value of one if individual i is in a Medicaid expansion state in year t, and takes the value of zero otherwise. Finally, α_i, λ_t, and γ_z represent individual, year, and commuting zone fixed effects, respectively, and standard errors are clustered at the commuting zone level.

We take commuting zones (CZs) to be the relevant geographic market, and therefore treatment is likewise defined at the CZ level. The treated CZs are those that, between 2014 and 2018, were exposed to hospital mergers that were predicted to increase the local Herfindahl-Hirschman Index (HHI) for the hospital industry (for simplicity, hereafter we refer to these simply as "hospital mergers").[2] The HHI is computed based on the share of hospital beds owned by each hospital system within each CZ. Independent hospitals that are not part of a hospital system are counted for the purposes of this computation as a 'system' composed of only one hospital. For example, a merger between hospital A in CZ X and hospital B in CZ Y would not be predicted to lead to a merger-induced increase in local HHI; therefore, those CZs would not be considered treated. The treatment

[2] Following the DOJ and FTC Merger Guidelines (U.S. Department of Justice and Federal Trade Commission, 2023) we define treatment as a "change in HHI", $2ab$, resulting from a merger between two incumbent local firms with pre-merger shares a and b. This definition means the local level of HHI can decrease in the year following an HHI-increasing merger, even if the merger itself is predicted to increase HHI. Nonetheless, the HHI-increasing mergers we consider as treatment generally induce post-merger increases in concentration and, as evident in Figure 1, they in fact amplify those increases in the years after the merger.

cohorts are defined based on the year in which each CZ is first exposed to a merger-induced increase in HHI (in the 2014-2018 period); thus, a CZ can be treated at most once in our research design.

The control group consists of never-treated CZs, i.e., those that were not exposed to a merger that increased HHI during 2014-2019. Our study period covers a time of intense merger activity in the hospital sector; however, another such period occurred in the 1990s to early 2000s, so our control CZs are, by-and-large, "already" concentrated due to earlier mergers.

We do not consider treatment cohorts before 2014 to ensure that every cohort has at least three years in their pre-treatment period; to prevent large imbalances in the pre-treatment period, we restrict it to a maximum of five years for every treated cohort. As unpaid medical debt takes at least six months to go into collections, we impose that every treatment cohort must have a minimum of one year in their post-treatment period, resulting in 2018 being the last cohort. The CZs that, during the 2014-2019 period, were first exposed to a merger that induced an HHI increase in 2019 are dropped from our sample. As is typical of staggered designs, the post-treatment period varies by cohort, from a minimum of one year (for the 2018 cohort) to a maximum of five years (for the 2014 cohort).[3] β_e are our key parameters of interest for each relative time period and measure the effect of merger exposure on our outcomes of interest.

In recent years, a large body of literature has developed on the biases that can potentially result from two-way fixed effects estimation (e.g., Goodman-Bacon 2021). To address this, we estimate equation 1 using the interaction-weighted estimator of Sun and Abraham (2021), which accounts for heterogeneity in treatment timing and dynamic treatment effects (see also Callaway and Sant'Anna 2021; Borusyak, Jaravel and Spiess 2024).[4]

III.2 Identification assumptions and the effect of hospital mergers on the CZ-level HHI

The empirical specification described above relies on the traditional identification assumptions for difference-in-differences approaches, particularly that there is no anticipation of treatment and that there are common trends in potential outcomes between the treated and control cohorts. We find no evidence suggesting that the treatment (hospital merger that raises HHI in the CZ where patients live) is anticipated in our medical debt data. Similarly, while we cannot test the common trends assumption directly, Section IV will show that there are parallel trends between the treatment and control cohorts in the pre-treatment period, making it plausible to assume that such trends would have continued in the absence of the treatment.

[3] E_i therefore takes a maximum value of 5 and is defined as $E_i = min\{5, 2019 - c_i\}$, with $c_i \in \{2014, 2015, 2016, 2017, 2018\}$.

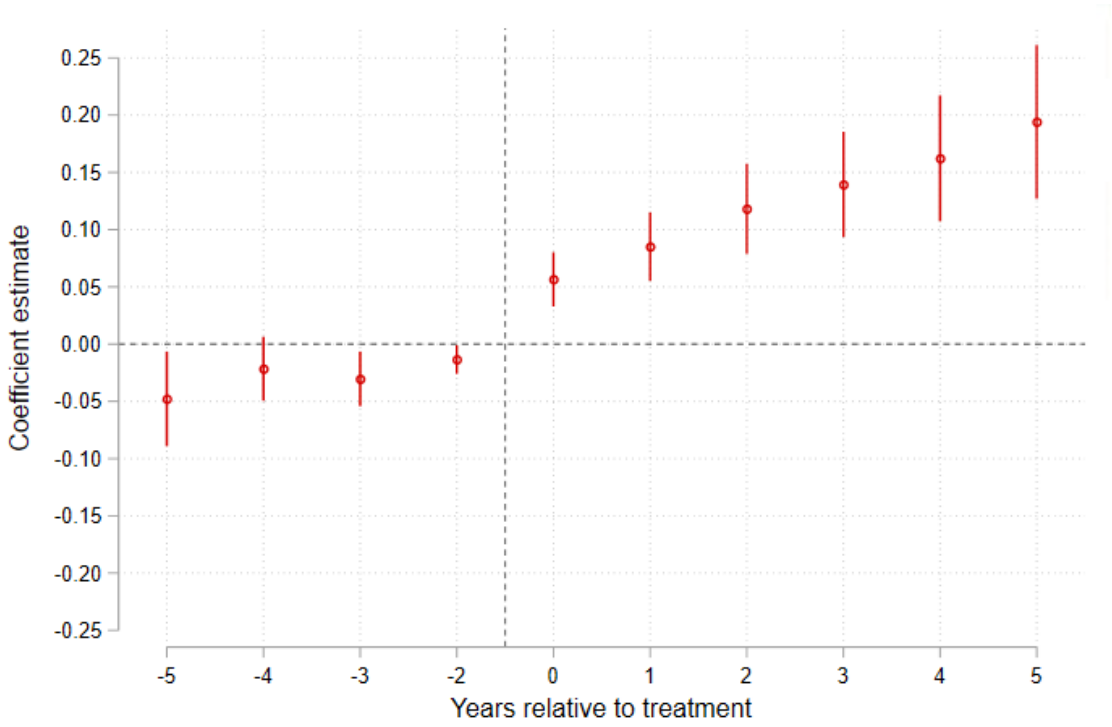
[4] Imputation-based alternatives (e.g., Borusyak, Jaravel and Spiess 2024) may offer efficiency gains over Sun and Abraham (2021) while relying more heavily on correct specification of the counterfactual model across all untreated observations.

Additionally, our research design relies on the assumption that the CZs affected by HHI-raising mergers are not subject to other changes, such as the entry of new hospitals, that would fully offset the HHI increase caused by the mergers in our sample. We test this assumption by estimating a version of Equation 1 in which the individual-level outcome is the log-transformed HHI in that individual's CZ.

Figure 1 displays the resulting event study plot. It shows a large and persistent increase in HHI immediately after the treatment takes place. This was to be expected given that the definition of treatment requires the mergers to be predicted to increase HHI in the exposed CZs. However, the figure also rules out the possibility that a factor like new hospital entries subsequently brought the HHI of affected CZs back down. In fact, it implies precisely the opposite: the treatment effect amplifies over time such that, five years post-treatment, the affected CZs have an HHI that is 21.4% higher than if they had not been exposed to a hospital merger.[5] It also rules out violations of the parallel trends assumption for CZ-level hospital HHI.

[5]This percentage increase is based on the fact that the point estimate for $t = 5$ is 0.194 and $exp(0.194) - 1 \approx 21.4\%$. It is important to highlight that the coefficient at $t = 5$ is estimated solely from the 2014 cohort, but the increasing trend in the point estimates is present in every preceding year, when it is estimated from a larger number of cohorts.

FIGURE 1: Event study plots for the effect of hospital merger exposure on the HHI faced by individuals.



Note: This figure shows the point estimates and 95% confidence intervals for β_e, obtained from implementing a version of equation 1 that uses the log-transformed HHI level faced by individuals as the outcome of interest. The estimates include year, CZ, and individual fixed effects, as well as a binary control taking the value of 1 when a state joins the ACA Medicaid expansion. The horizontal axis denotes years relative to the merger event; event time $t = -1$ is omitted as the reference period, with the dashed vertical line indicating the period just before the merger took place.

III.3 Summary statistics

Table 1 provides an overview of the basic demographic characteristics, the key outcomes of interest related to medical debt in collections, and indicators related to other types of debt, across the treatment and control cohorts. All the values in the table refer to 2013, before the treatment period for each cohort.

The gender composition of individuals in treated and control CZs is very similar, while those in control CZs are slightly older than those in treated CZs. It also shows that the control CZs have a substantially larger HHI level than the treated cohorts, except for the 2018 cohort. Likewise, control CZs typically had a higher share of individuals with medical debt in collections, new medical

debt in collections generated over the previous 12 months, and total outstanding medical debt in collections, than most or all of the treated CZs. As stated earlier, control CZs were "treated" by the earlier wave of hospital mergers in the 1990s-2000s. Table 1 also shows that the 2013 levels of the "raw" and "adjusted" measures for the outcomes of interest were very similar for each cohort.

We emphasize that difference-in-differences and event study approaches do not require identical baseline levels between the treatment and control groups. Instead, the key identification assumptions are those previously discussed in Section III.2, i.e., no anticipation of treatment and common trends in potential outcomes. If those hold, as we have argued is the case, the control cohort provides an appropriate counterfactual and allows us to assess the causal impact of hospital merger exposure on medical debt in collections, even with differences in baseline characteristics and levels.

TABLE 1 Summary statistics table for key demographics and outcomes, by cohort, in 2013 (last year before the first treatment year).

	2014 cohort	2015 cohort	2016 cohort	2017 cohort	2018 cohort	Control CZs
Age	47.3	47.7	48.3	47.6	47.8	48.5
Female	44%	44%	45%	44%	44%	45%
Male	44%	44%	44%	44%	44%	45%
HHI level	1,035	1,444	1,508	1,554	3,208	3,145
Has medical debt (adj)	18%	22%	23%	20%	26%	23%
Has medical debt (raw)	17%	21%	22%	20%	25%	22%
New medical debt, last 12m (adj)	159	182	216	187	201	219
New medical debt, last 12m (raw)	151	179	222	186	189	208
Total medical debt (adj)	472	558	693	535	756	708
Total medical debt (raw)	419	499	637	482	690	628
Has non-med debt (adj)	31%	34%	35%	32%	36%	34%
Has non-med debt (raw)	30%	33%	34%	31%	35%	33%
New non-med debt, last 12m (adj)	382	388	474	394	401	407
New non-med debt, last 12m (raw)	375	390	467	392	396	402
Total non-med debt (adj)	1310	1291	1665	1279	1507	1408
Total non-med debt (raw)	1228	1208	1555	1188	1429	1305
Has utility debt (adj)	13%	14%	14%	13%	16%	14%
Has utility debt (raw)	12%	13%	13%	13%	15%	13%
New utility debt, last 12m (adj)	48	52	56	45	55	47
New utility debt, last 12m (raw)	45	48	52	42	49	43
Total utility debt (adj)	108	113	120	109	123	102
Total utility debt (raw)	98	104	106	98	112	94
Has credit card debt (raw)	66%	65%	66%	66%	61%	62%
New credit card debt, last 12m (raw)	271	271	281	269	251	231
Total credit card debt (raw)	3954	3942	4069	3950	3573	3378
Observations	1,727,441	659,747	267,208	320,688	62,693	1,394,104

Note: This table displays demographic characteristics, HHI, and outcomes of interest across treatment and control cohorts in 2013, one year before the treatment start of the treatment period. For the outcomes related to medical debt in collections, non-medical debt in collections, and utility/cable/cellular debt in collections, both the raw and adjusted amounts are shown.

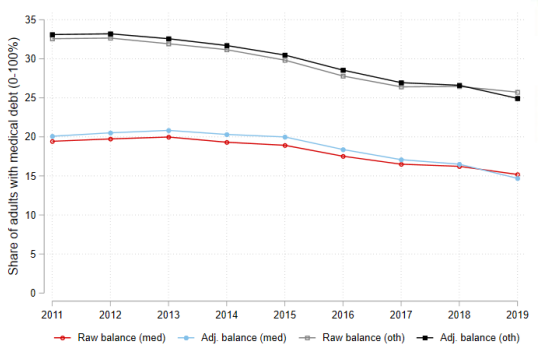
Figure 2 plots the levels of the outcomes of interest for medical debt in collections and for non-medical debt in collections for the 2011-2019 period, showing both the "raw" and "adjusted" measures in each case. For medical debt in collections, we observe the trend previously documented by Kluender et al. (2021): increasing levels until 2013/2014 and large, sustained declines thereafter,

which they demonstrate to be at least partly due to the ACA Medicaid expansion adopted in 2014 and subsequent years by a large number of states.

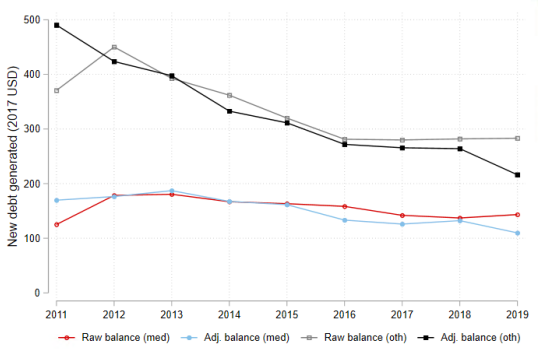
The total amount of debt in collections and the flows of new debt in collections over the previous 12 months are also broadly similar to those reported by Kluender et al. (2021). However, the breakdown between medical and non-medical debt differs significantly from theirs. There are plausible explanations for this, including the fact that Kluender et al. (2021) rely on data from a different credit reporting agency than ours and, as they point out, the measures of medical debt in collections may vary across agencies. Likewise, the specific criteria based on which a tradeline is deemed to correspond to medical debt in collections, as well as the variables present in the data made available by each credit reporting agency, are not necessarily identical.

FIGURE 2: Evolution of key outcomes for medical debt and non-medical debt in collections (adjusted and non-adjusted) over the 2011-2019 time period.

(A) Share with medical debt



(B) New medical debt (last 12 months)



(C) Total medical debt

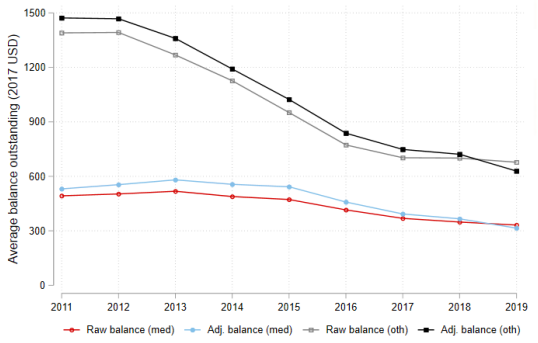


Figure 2 also shows that the "raw" and "adjusted" values for the outcomes of interest regarding medical debt in collections, in addition to having similar levels, also exhibit similar trends. Therefore, unless explicitly stated otherwise, the remainder of this paper will consider only the outcomes based on the adjusted amounts, our preferred definition for the outcomes of interest. Equivalent figures using the raw data are shown in Appendix B.

IV Results

IV.1 Baseline results: medical debt in collections

Figure 3 shows the event study estimates for the three outcomes of interest discussed in section III.1.[6] Here, we focus only on our preferred specification that (i) uses the full sample, (ii) includes year, CZ, and individual fixed effects, and (iii) includes a binary control that identifies when states expanded Medicaid.

Panels 3A through 3B suggest that the effect hospital mergers have on medical debt amplifies over time. Five years after the treatment, hospital mergers have a large and significant effect on the likelihood of having medical debt, the amount of new medical debt generated over the previous 12 months, and the total amount of outstanding medical debt. The effects shown in the event study plots translate to increases that correspond to 11%, 20%, and 23%, respectively.[7] While the estimates five years post-treatment are based solely on the 2014 treatment cohort—the earliest-treated one—Panels 3A through 3B show that, even just two years post-treatment, the effects were already positive and significant: an increase of 2.4% in the likelihood of having medical debt, 8.7% in the amount of new medical debt generated over the previous 12 months, and 6.2% in the total amount of outstanding medical debt.[8]

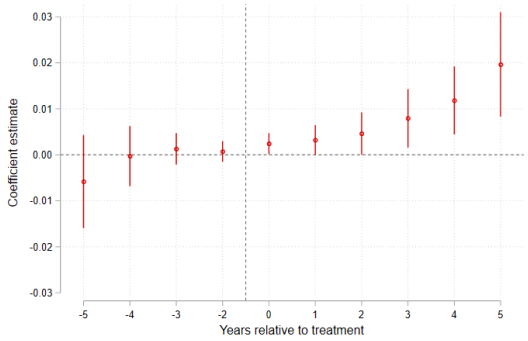
[6] In Appendix B, we re-estimate the baseline specification using the "raw/unadjusted" version of the outcomes for medical debt in collections. Figure B.1 shows that the resulting estimates are generally very similar, with the more significant difference arising from the outcome for new medical debt in collections generated over the previous 12 months. This is to be expected, given that, as outlined briefly in section II and in more detail in Appendix A.1, the data corrections we implement have a larger effect on both the amounts and the timing of this outcome variable.

[7] We computed these by dividing, for each outcome of interest, the corresponding point estimates at $t = 5$ by the baseline pre-treatment value for the 2014 cohort shown in Table 1. The point estimates are detailed in the event study regression tables in Appendix C; the preferred specification illustrated by Figure 3 corresponds to the results in column (3).

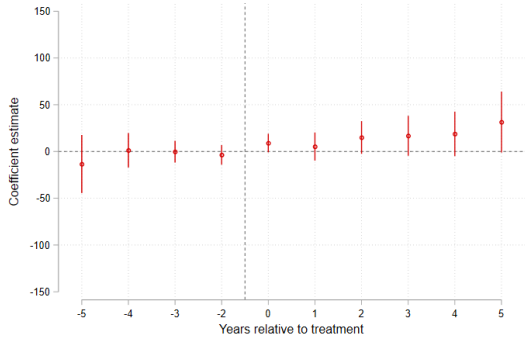
[8] We followed an analogous procedure to the one described in footnote 7. In this case, at $t = 2$, the estimate is based on the 2014, 2015, 2016, and 2017 cohorts; therefore, the denominator used the average of the baseline values for each cohort in 2013, as detailed in Table 1, weighted by the number of individuals in the cohort.

FIGURE 3: Event study plots for the effect of hospital merger exposure on medical debt.

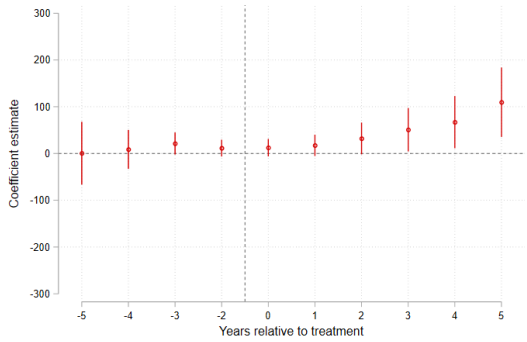
(A) Likelihood of having medical debt



(B) New medical debt (last 12 months)



(C) Total medical debt



Note: Each panel shows the point estimates and 95% confidence intervals for β_e, obtained from implementing equation 1, and illustrates the effect of exposure to hospital mergers on medical debt outcomes. The estimates included year, CZ, and individual fixed effects, as well as a binary control that accounts for the timing of state-level Medicaid expansion, as outlined in section III.1; standard errors are clustered at the CZ level. The outcome for Panel 3A is a binary variable that takes a value of 1 if an individual has medical debt in collections, and 0 otherwise. The outcome for Panel 3B is the total amount of new medical debt in collections (measured in constant 2017 USD) generated in the previous 12 months, regardless of whether it was subsequently paid. The outcome for Panel 3C is the total outstanding amount of medical debt in collections (measured in constant 2017 USD). The horizontal axis denotes years relative to the merger event; event time $t = -1$ is omitted as the reference period, with the dashed vertical line indicating the period just before the merger took place. The full regression results for each panel are shown in column (3) of Tables C.1, C.2, and C.3, respectively.

Appendix C shows the event study regression tables for each of the three outcomes depicted in Figure 3, in Tables C.1, C.2, and C.3, respectively. All three tables follow the same format: columns

(1) to (3) use the full sample and sequentially add fixed effects and a binary control for the state-level Medicaid expansion that occurred from 2014 onward; column (3) corresponds to our preferred specification, plotted in Figure 3. Column (4) restricts the sample to never movers, and column (5) further restricts it to never movers under the age of 65. Column (6) returns to the full sample as the starting point and then restricts the treatment group to CZs where the merger-induced HHI increase exceeded 150; column (7) further restricts the treatment group to CZs where the post-merger HHI exceeded 1,800, and column (8) increases the post-merger HHI threshold to 2,500.

Overall, Tables C.1-C.3 show that the baseline results are robust to various sample and treatment group definitions. Column (5) shows that restricting the sample to never-movers under 65 results in similar point estimates for all three outcomes. Restricting the treatment group based on merger-induced HHI increases and post-merger HHI levels results in larger and more significant point estimates for the outcomes related to the flow of new medical debt over the previous 12 months and the total outstanding amount. This occurs simultaneously with lower magnitudes and significance regarding the effect on the likelihood of having medical debt, suggesting an effect along the intensive margin for medical debtors in CZs where the hospital mergers result in higher levels of concentration.

One potential concern with our baseline results is that they may suffer from dynamic bias as a result of our baseline model (equation 1) disregarding dynamic feedback in the outcome variable (Klosin, 2024). However, the inclusion of lagged outcomes to address this concern may alternatively induce Nickell bias (Nickell, 1981; de Chaisemartin and D'Haultfoeuille, 2023). In Appendix D we replicate our baseline event study estimates but include the lagged dependent variable as an additional control in every specification. Across all outcomes, the estimates remain similar in magnitude, well within the confidence interval of the baseline results, indicating no evidence of either type of bias in the relevant estimates.[9] In the remainder of this paper, our specifications exclude a lagged dependent variable.

We also explore several dimensions of heterogeneity. Splitting the sample by gender yields nearly identical estimates for female and male individuals (Appendix Figure E.1). Estimating separately for states that did and did not expand Medicaid during the study period, we find no notable difference in the effects across the two groups; if anything, the effects are driven by individuals in expansion states (Appendix Figure E.2). Finally, restricting the sample to individuals who already had medical debt in collections in 2013 produces estimates very similar to the full-sample baseline, suggesting that the effect of merger exposure operates similarly across both the intensive and extensive margins of medical indebtedness (Appendix Figure E.3).

[9] We note that this comparison would be uninformative if both biases were similar in magnitude and direction, though we view this as unlikely given their distinct sources.

IV.2 Results for other outcomes

IV.2.1 Credit card debt

One potential limitation of the approach followed so far, also highlighted by Kluender et al. (2021), is the possibility that patients sometimes pay for out-of-pocket medical expenses with a credit card. That type of medical debt would not be captured by the outcome measures we have used so far; consequently, the baseline estimates might underestimate the total effect of mergers on medical debt outcomes. To address this, we adapt our specification in this section to include new outcomes of interest based on credit card debt.

It is important to stipulate one important difference between credit card debt and medical debt as they each appear on credit reports: credit card tradelines are opened with the express purpose of obtaining access to credit, whereas medical debt is in effect an unpaid bill that has been sent to collections. Hence, for our analysis of credit card debt, we do not stipulate the tradelines in question need to be in collections.

Figure 4 displays the resulting event study plots, suggesting that exposure to hospital mergers affects credit card outcomes as well. The first three outcomes, in Panels 4A-4C, are constructed analogously to the baseline outcomes. However, in contrast to the baseline results, the effects on credit card debt operate solely through the intensive margin. Panel 4A shows no effect of merger exposure on the probability of having credit card debt, relative to individuals in non-exposed CZs. On the other hand, Panels 4C and 4B show that merger exposure leads to an increase in new credit card debt over the previous 12 months, which translates to a higher total stock of credit card debt.

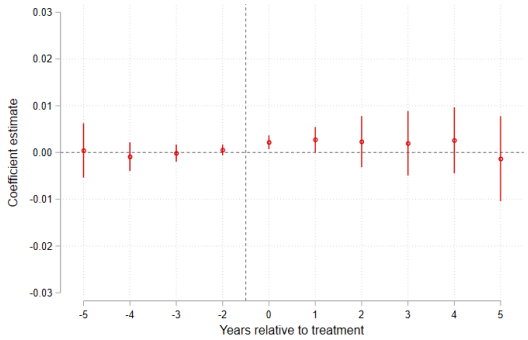
The intensive-margin pattern for credit cards—more debt conditional on having any, but no change in the probability of holding credit card debt—is consistent with a specific institutional feature of medical billing. Hospitals routinely condition access to non-emergency care on the resolution of outstanding balances, effectively requiring patients to pay prior debts before receiving further treatment (Kliff and Silver-Greenberg, 2023). Patients facing such demands are likely to turn to credit cards as a means of financing those payments, particularly when the alternative is being denied care. This channel would generate exactly the pattern we observe: merger-induced increases in medical costs raise the amount that existing credit card holders charge, without drawing new individuals into credit card borrowing. It also implies that the medical debt in collections captured by our baseline estimates understates the full financial burden that hospital mergers impose on patients, since some merger-induced medical expenses are absorbed into credit card balances rather than appearing as medical collections tradelines.

The effect of merger exposure on delinquent credit card debt would arguably provide a better measure of the kind of financial distress that could eventually lead to having medical debt in collections. Panel 4D shows mergers correspond to a *decline* in the share of credit card debt that is delinquent. It is important to emphasize again here that all medical debt on credit reports is in some sense delinquent—it represents unpaid bills—whereas only some credit card balances are

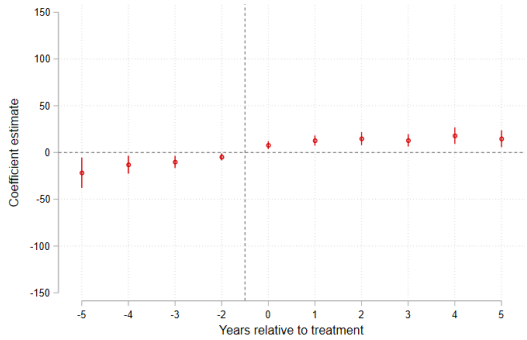
delinquent. The event study specifications suggest that severe pre-treatment trends are present for this outcome—individuals in CZs that experienced hospital mergers were already experiencing a decline in credit card delinquency relative to control CZs. This violation of a key condition for identification makes a causal interpretation of the resulting estimates impossible.

FIGURE 4: Event study plots for the effect of hospital merger exposure on credit card debt.

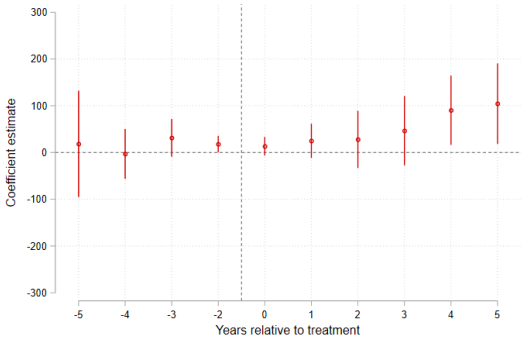
(A) Likelihood of having credit card debt



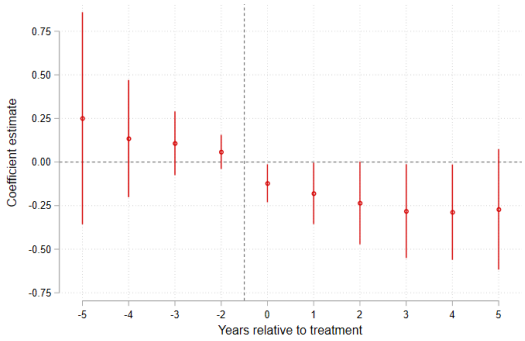
(B) New credit card debt (last 12 months)



(C) Total credit card debt



(D) Share of delinquent credit card balance



IV.2.2 Placebo Test: Non-medical debt in collections

A potential concern regarding the research design we use in this paper is that there is an underlying phenomenon that broadly overlaps, across space and time, with hospital mergers, which could be causing an increase in debt in collections across the board, rather than just medical debt. While it is never possible to fully disprove this alternative, conducting placebo tests in which we apply the baseline specification to outcomes that are plausibly unaffected by the treatment (i.e., by hospital merger exposure) should help allay concerns that the results we have found so far result from

spurious correlation between hospital mergers and overall deteriorating credit conditions.

In this sub-section, we construct outcomes based on other types of non-medical debt in collections. Specifically, we focus on categories of non-medical debt in collections that meet the following three criteria: first, they can reasonably be assumed not to be directly affected by the treatment in question. Second, they are prevalent enough in the UCCCP sample that they are geographically widespread and the resulting estimates will not be overly noisy and underpowered. We satisfy this by relying on categories that represent at least 5% of the total number of tradelines in collections (for comparison, medical debt represents 58% of the number of collections tradelines in the UCCCP data). Third, like medical debt, they must represent unpaid bills as opposed to credit that is extended ex-ante, so that the outcomes of interest can be constructed analogously from credit reporting data.

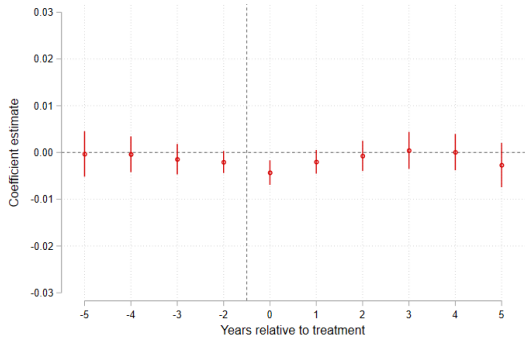
Applying these criteria, we focus on two categories of non-medical debt in collections: utilities and cable/cellular. Like medical debt, utility debt represents an unpaid bill for which the provider is to some degree restrained from discontinuing service for non-payment, and so those providers are granted access to consumers' credit reports as a second-best means of enforcing payment.[10] We pool these tradelines together and define the three resulting outcomes analogously to the baseline outcomes for medical debt. Ex-ante, we expect the treatment not to directly affect these categories of debt in collections; this should translate into point estimates that are not statistically significant and are centered around zero.

Figure 5 displays the resulting event study plots, which are broadly in line with this expectation. Panels 5A and 5C show largely non-significant estimates that oscillate around zero both before and after the treatment. Panel 5B (new debt in collections) shows a slight increase in point estimates after the treatment year, but these are largely non-significant and very small in magnitude, such that they never translate into a positive effect on total utility/cable/cellular debt in collections, as shown by Panel 5C.

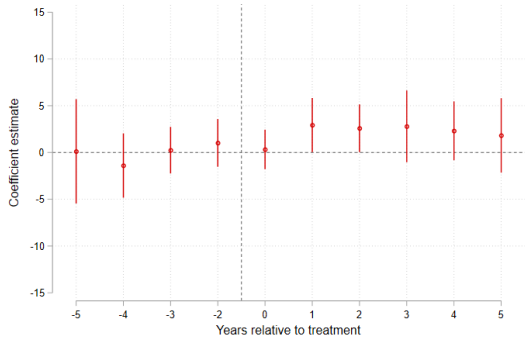
[10] The alternative for secured loans is repossession.

FIGURE 5: Event study plots for the effect of hospital merger exposure on utility, cable, and cellular debt in collections.

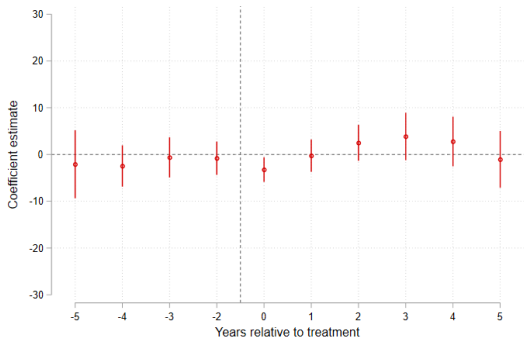
(A) Likelihood of having utility debt in collections



(B) New utility debt in collections (last 12 months)



(C) Total utility debt in collections



V Testing Mechanisms

The preceding sections establish that hospital mergers cause economically meaningful increases in individual medical debt: five years after treatment, merger exposure raises the likelihood of having medical debt in collections by 11 percent, the flow of new medical debt by 20 percent, and the total outstanding stock by 23 percent. A natural next question is **why**. Medical debt arises when patients face out-of-pocket costs they cannot (or choose not to) pay, so the causal chain from a hospital merger to higher medical debt must run through some combination of higher prices for care, reduced insurance coverage for a given quantity of care received, changes in the volume or intensity of services delivered, dissatisfaction and/or surprise on the patient's part, diminished

patient capacity to cover out-of-pocket costs due to lower income or unemployment, or more aggressive collection practices by the merged entity. In this section, we use a separate, comprehensive dataset on healthcare spending and utilization from the Institute for Health Metrics and Evaluation (IHME). Because the IHME data cover all major payers and types of care at the county level, they allow us to decompose the spending response to a merger into price and quantity components and to trace those responses across payers—something that is not possible with the consumer credit data used in the main analysis. We also use labor market flows data from Quarterly Workforce Indicators to test the channels that run through hospital mergers' effect on labor markets.

The structure of this decomposition maps directly onto the candidate mechanisms. If mergers raise the price of care, we should observe higher spending per encounter with stable or declining utilization; if they instead reduce access or shift patients toward less generous coverage, we should see changes in spending shares by payer or beneficiary counts. If mergers generate operational efficiencies that lower costs without restricting access, spending per encounter should fall. Distinguishing among these patterns matters both for interpreting the medical debt estimates and for informing the welfare analysis of merger enforcement, since the policy implications differ sharply depending on whether the additional debt reflects higher prices, reduced coverage, lower earnings, patient dissatisfaction, or some other channel.

V.1 Aggregated Claims Data: The IHME Disease Expenditure Project

We draw on the Disease Expenditure estimates produced by the Institute for Health Metrics and Evaluation (IHME) and described in Dieleman et al. (2025). The IHME project combines more than 40 billion insurance claims with 836 million facility records to produce county-level estimates of healthcare spending, utilization, and enrollment for 148 health conditions across four payer categories—private insurance, Medicare (including Medicare Advantage), Medicaid, and out-of-pocket (OOP)—and seven types of care: ambulatory, inpatient, emergency department, nursing facility, home health, dental, and pharmaceutical. All estimates are age- and sex-standardized to a common reference population. The data span the years 2010–2019, covering 3,110 counties that account for 97.3 percent of the U.S. population. The claims cover 76.6 percent of personal healthcare expenditures.

We aggregate the county-level IHME estimates to commuting zones (CZs) to match the geography of the main analysis. County-to-CZ crosswalks follow the 1990 CZ definitions used throughout the paper, and CZ population denominators come from the Census Bureau's Small Area Health Insurance Estimates (SAHIE). The resulting panel contains one observation per CZ–year–payer combination for each of the spending and utilization measures described below.

Variable construction. The variables reported in Table 2 fall into six groups. In each case, spending is aggregated across all seven types of care, because hospital mergers can plausibly shift expenditures across care settings and are always expressed in USD. Encounter and beneficiary counts, by contrast, are restricted to inpatient and emergency department care because the encounter unit

is not comparable across care types—a pharmacy fill and a hospital admission each count as one "encounter," so summing across all types of care would produce an uninterpretable total. This restriction means that the encounter-based variables capture acute care utilization specifically, which is the margin most directly affected by hospital consolidation.

- **Spending per capita** (Panel A) divides total spending, summed across all four types of payer and all types of care, by CZ population. We report this variable separately for each payer type as well as in total.
- **Spending shares** (Panel B) express each type of payer's spending as a fraction of total spending in the CZ–year cell. Together, Panels A and B characterize the overall level of healthcare expenditures and the payer type composition.
- **Beneficiaries per capita** (Panel C) divides the number of inpatient-plus-ED beneficiary-episodes by CZ population. A beneficiary-episode is a condition-specific count: a patient treated for two distinct conditions in the same year contributes two beneficiary-episodes. This variable therefore measures the population-adjusted volume of acute care demand by payer, rather than a count of unique patients. The IHME data do not report a beneficiary count for out-of-pocket spending, so this panel covers private insurance, Medicare, and Medicaid only.
- **Encounters per capita** (Panel D) divides the number of inpatient-plus-ED encounters by CZ population. Encounters differ from beneficiary-episodes in that they count visits rather than treated individuals; a patient with two ED visits for the same condition in the same year generates two encounters but one beneficiary-episode. This variable is available for all four payers.
- **Spending per beneficiary** (Panel E) divides total spending (across all types of care) by the number of inpatient-plus-ED beneficiary-episodes. Because the numerator includes non-hospital spending while the denominator counts only acute care episodes, this ratio should be interpreted as total payer outlay per acute care beneficiary-episode—a reduced-form measure of the costliness of a treated episode—rather than a pure inpatient price. However, this set of outcomes can also be affected by shifting care across settings. Panel E again excludes the OOP payer.
- **Spending per encounter** (Panel F) replaces beneficiary-episodes with encounters in the denominator but is otherwise identical to Panel E. It captures the average payer outlay per acute care visit and is available for all four payers.

Finally, we report the total CZ population and the count of CZs in each treatment cohort, plus the control group.

Table 2 presents summary statistics for these variables, pooling across CZ–year observations in the analysis sample.

TABLE 2 Summary statistics for IHME healthcare spending variables, by cohort, in 2013 (last year before the first treatment year).

	2014 cohort	2015 cohort	2016 cohort	2017 cohort	2018 cohort	Control CZs
Panel A: Spending per capita ($)						
Total spending per capita	7,436	9,876	7,314	8,322	7,045	7,458
Private ins. spending per capita	3,156	4,390	3,096	3,491	2,686	2,870
Medicare spending per capita	2,011	2,584	2,057	2,161	2,083	2,250
Medicaid spending per capita	1,267	1,547	1,111	1,438	1,365	1,358
OOP spending per capita	1,002	1,354	1,050	1,232	911	980
Panel B: Spending shares						
Private ins. spending share	42.5%	43.4%	41.7%	40.8%	38.6%	38.0%
Medicare spending share	27.0%	27.2%	28.8%	26.4%	29.2%	30.2%
Medicaid spending share	17.1%	15.8%	14.7%	18.2%	19.1%	18.6%
OOP spending share	13.4%	13.6%	14.8%	14.6%	13.1%	13.1%
Panel C: Beneficiaries per capita						
Private ins. beneficiaries per capita	1.3505	1.9226	1.2783	1.6584	1.4716	1.5017
Medicare beneficiaries per capita	0.3153	0.4334	0.3264	0.3656	0.3674	0.4106
Medicaid beneficiaries per capita	0.4076	0.4846	0.3446	0.4260	0.4365	0.4635
Panel D: Encounters per capita						
Private ins. encounters per capita	71.8283	85.7805	65.2255	85.0602	74.9659	75.1660
Medicare encounters per capita	546.8361	610.1099	506.9019	586.4443	575.6214	549.2017
Medicaid encounters per capita	213.4058	223.1356	217.2777	239.9698	252.5338	259.9587
OOP encounters per capita	228.8906	210.7870	215.9752	247.0985	250.4751	253.7303
Panel E: Spending per beneficiary ($)						
Private ins. spending per beneficiary	2,407	2,273	2,441	2,058	1,826	1,888
Medicare spending per beneficiary	6,538	6,125	6,446	5,890	5,655	5,509
Medicaid spending per beneficiary	3,212	3,267	3,100	3,384	3,085	2,948
Panel F: Spending per encounter ($)						
Private ins. spending per encounter	47	55	51	42	37	41
Medicare spending per encounter	4	4	4	4	4	4
Medicaid spending per encounter	7	8	5	7	6	6
OOP spending per encounter	5	7	6	5	4	4
Panel G: Coverage and population						
Average CZ population	6,142,826	2,224,671	2,233,767	2,422,341	420,717	649,978
Number of CZs	63	34	12	18	11	568
Total cohort population	99,074,641	37,547,268	14,630,555	18,322,409	3,581,959	79,833,350

Note: This table displays population-weighted CZ-level means of healthcare spending variables from the IHME Disease Expenditure Project (Dieleman et al., 2025), by treatment cohort and for control CZs, in 2013 (one year before the start of the treatment period). Spending is measured in 2019 US dollars and is age- and sex-standardized. Encounters and beneficiaries are restricted to inpatient and emergency department care and reflect condition-specific accounting units summed across the IHME's 148 health conditions; they are not directly comparable to unique patient or visit counts (see text for discussion). Medicare is inclusive of Medicare Advantage. Population data are from the Small Area Health Insurance Estimates (SAHIE) program.

Estimating equation. The IHME outcomes are measured at the commuting zone level, so the event study specifications in this section estimate a CZ-level analog of Equation 1:

$$y_{z,t} = \sum_{e=-3}^{E_z} \beta_e D_z \mathbb{1}\{t - c_z = e\} + \gamma_z + \lambda_t + \phi x_{z,t} + \varepsilon_{z,t} \tag{2}$$

where $y_{z,t}$ is the outcome of interest for commuting zone z in year t; D_z indicates that CZ z experienced an HHI-increasing hospital merger during the 2014–2018 treatment window; c_z is the year of first treatment; γ_z and λ_t are CZ and year fixed effects, respectively; and $x_{z,t}$ is the binary Medicaid expansion control defined as in Equation 1.[11] As in the individual-level analysis, we employ the Sun and Abraham (2021) estimator. Treatment cohorts, control group composition, and the event-time window are defined identically to Section III.1, and standard errors are clustered at the CZ level.

V.2 Hospital mergers lead to higher prices for commercially-insured patients

A large body of evidence documents that hospital mergers raise the prices negotiated between hospitals and commercial insurers (Cooper et al., 2019; Gowrisankaran, Nevo and Town, 2015). Higher negotiated prices translate mechanically into higher out-of-pocket costs for commercially insured patients through co-insurance, co-pays, and deductibles: even if the cost-sharing percentage remains unchanged, the dollar amount the patient owes rises proportionally with the price of care. This channel provides perhaps the most direct causal pathway from hospital mergers to medical debt.

We test for this pathway by estimating Equation 2 using four spending outcomes from the IHME data. The results are presented in Figure 6. Panel 6A shows the effect on log total healthcare spending across all types of payers. Mergers generate a steady increase in aggregate spending, reaching approximately 0.025 at $t = 5$, with no evidence of differential pre-trends. This establishes that hospital mergers raise the overall cost of healthcare in affected commuting zones.

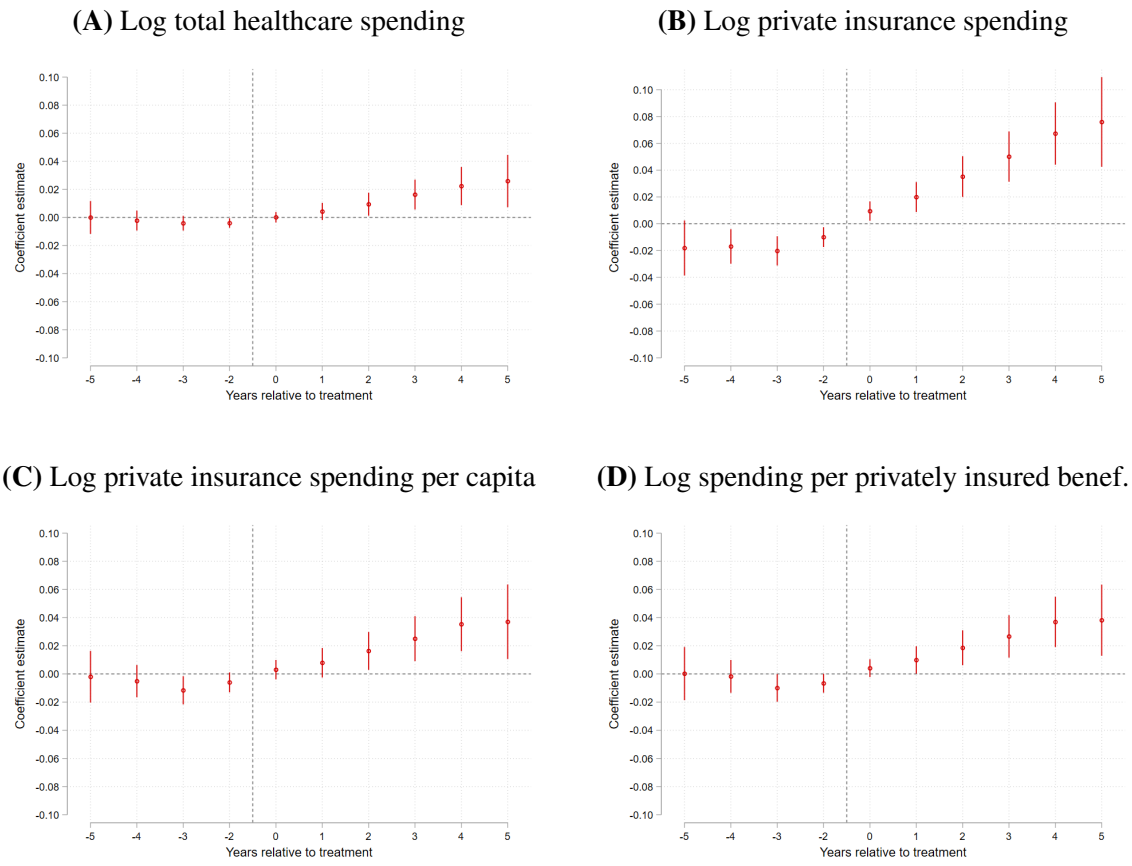
Panel 6B decomposes this aggregate effect by showing the impact on log private insurance spending specifically. The effect is substantially larger than the total spending effect: point estimates rise steadily to approximately 0.075 at $t = 5$. The fact that the private insurance effect is several times larger than the all-payer effect indicates that commercially insured care is the primary margin along which merger-induced spending increases occur, consistent with the well-documented role of commercial insurance as the payer class with the least administered price regulation. Equivalent event studies for Medicare and Medicaid spending show outright declines in the aftermath of local hospital mergers, a mechanism we return to in subsection V.4.

[11] For the purpose of assigning the Medicaid expansion binary variable to commuting zones, for commuting zones that cross state lines we assign the binary associated with the state where the majority of the commuting zone population resides.

To further decompose the private insurance spending increase, Panels 6C and 6D present the effects on log private insurance spending per capita and log spending per privately insured beneficiary-episode, respectively. Both outcomes show clean pre-treatment periods and clear post-treatment increases that grow over time. Spending per capita reaches approximately 3.5 percent at $t = 5$, while spending per beneficiary-episode—our reduced-form proxy for the price of a treated episode, as described in Section V.1—reaches approximately 4 percent at $t = 5$. The increase in spending per beneficiary is particularly informative: because the denominator counts condition-specific acute care episodes, this result indicates that the cost per episode of care has risen, not merely that more care is being delivered. The similarity of the per-capita and per-beneficiary estimates further suggests that the spending increase is driven primarily by higher unit prices rather than by an expanding volume of treated episodes.

Taken together, these results provide strong evidence for the price channel. Hospital mergers raise the amount that commercial insurers—and by extension, their enrollees—pay per episode of acute care. Through the standard cost-sharing provisions of commercial insurance plans, these higher prices translate directly into higher out-of-pocket obligations for patients. Given the well-documented sensitivity of medical debt to out-of-pocket costs, this channel provides a clear and direct explanation for the increases in medical debt documented in Section IV.

FIGURE 6: Event study plots for the effect of hospital merger exposure on healthcare spending.



Note: Each panel shows the point estimates and 95% confidence intervals for β_e, obtained from implementing Equation 2 at the CZ level using the IHME Disease Expenditure data. All outcomes are log-transformed. Panel 6A uses total spending across all types of payers; Panels 6B–6D use private insurance spending in total, per capita, and per inpatient-plus-ED beneficiary-episode, respectively. All regressions include CZ and year fixed effects and a binary control for state-level Medicaid expansion; standard errors are clustered at the CZ level. The horizontal axis denotes years relative to the merger event; event time $t = -1$ is omitted as the reference period, with the dashed vertical line indicating the period just before the merger took place.

V.3 Hospital mergers lead to broader patient participation in healthcare costs

The price channel documented above operates along the intensive margin: each episode of commercially insured care becomes more expensive. However, hospital mergers could also increase medical debt along the extensive margin of out-of-pocket care by expanding the set of care episodes

for which patients bear direct financial responsibility. If mergers lead to restructured cost-sharing arrangements—for instance, by introducing co-pays on services previously covered without them, or by shifting patients into plans with higher deductibles—then more healthcare encounters would generate out-of-pocket obligations, even if the charge per encounter remains unchanged. This extensive-margin channel would increase both the probability that any given patient faces an unexpected medical bill and the total burden of out-of-pocket spending across the population.

We investigate this pathway using four out-of-pocket (OOP) spending and utilization outcomes, estimated via Equation 2. The results are presented in Figure 7. Panel 7A shows the effect on log total OOP spending. There is a clear and immediate increase in OOP spending at the time of treatment, with point estimates rising to approximately 0.065 at $t = 5$—a roughly 7 percent increase. While the earlier pre-treatment estimates (at $t = -5$ and $t = -4$) are slightly negative, the estimates at $t = -2$ and $t = -3$ are close to zero, and the sharp upward break at $t = 0$ is evident.

To determine whether this increase reflects higher charges per encounter or a broader set of encounters involving patient payments, we turn to the remaining three panels. Panel 7B shows the effect on OOP encounters per capita—the number of inpatient and emergency department encounters per capita for which patients made direct out-of-pocket payments. The point estimates shift from slightly negative in the pre-treatment period to consistently positive after the merger, rising to approximately 21 additional encounters per capita at $t = 5$. Although the estimates are imprecise, particularly at longer horizons, the direction of the shift is clear: mergers increase the number of acute care episodes that involve out-of-pocket payments.

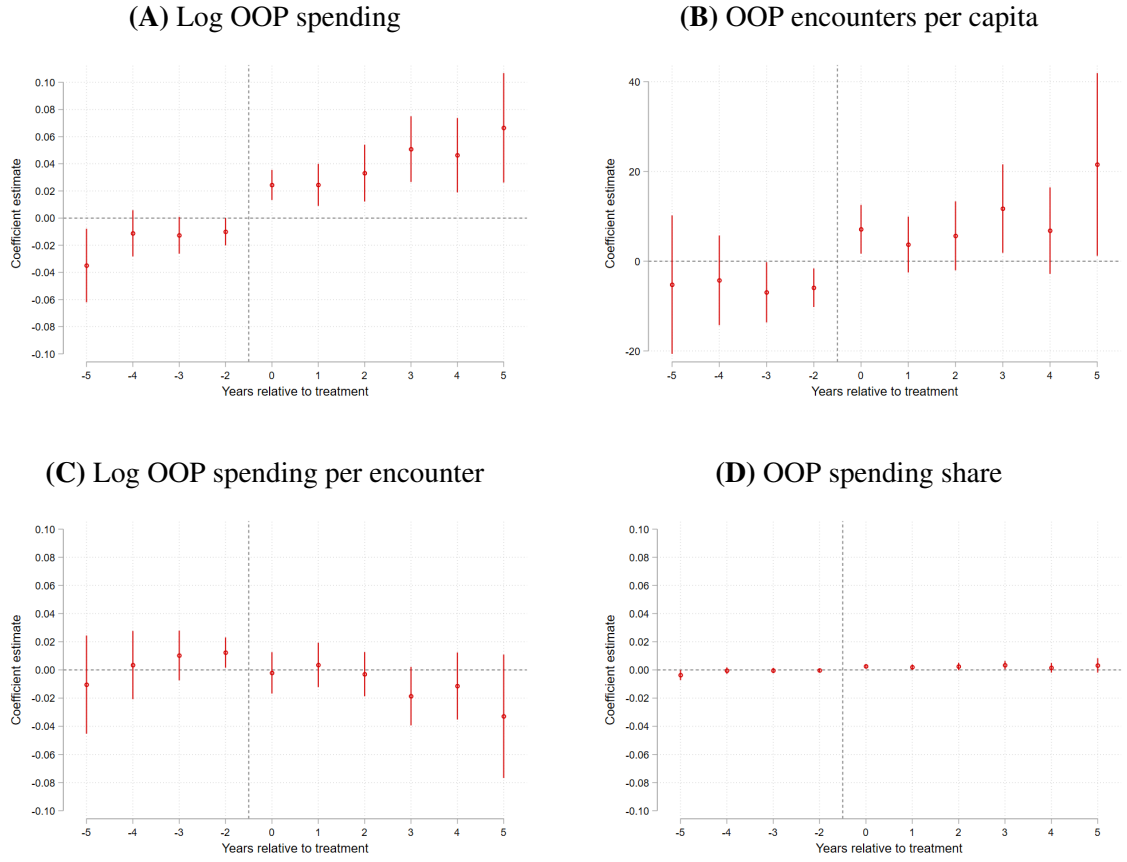
Panel 7C presents the complementary result. It shows the effect on log OOP spending per encounter, which captures the average patient payment conditional on an encounter occurring. In contrast to the total spending effect in Panel 7A, this outcome shows no post-treatment increase. If anything, per-encounter OOP spending drifts slightly downward at longer horizons. The combination of Panels 7B and 7C implies that the mechanism operates along the extensive margin rather than the intensive margin of out-of-pocket care: mergers increase OOP spending not because each encounter becomes more expensive for the patient, but because a larger share of healthcare episodes generate out-of-pocket obligations in the first place.

Finally, Panel 7D shows the effect on the OOP share of total healthcare spending. The estimates are very precisely estimated and show only a very small positive effect, on the order of 0.3 percentage points. This is consistent with the findings in the preceding subsection: because total spending is also rising (driven primarily by the private insurance price channel), the OOP share remains roughly stable even as absolute OOP spending increases substantially.

This pattern—more OOP encounters without higher per-encounter charges—is consistent with several possible mechanisms at the plan-design level. Merged hospital systems may negotiate contracts that shift more costs to patients, insurers responding to higher hospital prices may redesign plans with broader cost-sharing requirements, or the elimination of competing providers may reduce patients' ability to seek care at facilities where their coverage provides more generous terms.

Whatever the specific plan-level mechanism, the aggregate result is that mergers expand the set of care episodes on which patients have direct financial exposure—a novel finding in the hospital mergers literature, to our knowledge. This extensive-margin channel complements the price channel: the price increases documented in the preceding subsection raise the cost of each episode where patients have financial exposure, while the cost-sharing channel documented here expands how many episodes create such exposure. Further, our results are consistent with overall commercial health insurance plans becoming less generous to members relative to the total (rising) cost of their healthcare.

FIGURE 7: Event study plots for the effect of hospital merger exposure on out-of-pocket healthcare spending and utilization.



Note: Each panel shows the point estimates and 95% confidence intervals for β_e, obtained from implementing Equation 2 at the CZ level using the IHME Disease Expenditure data. Panels 7A and 7C use log-transformed spending outcomes; Panel 7B uses the level of inpatient-plus-ED out-of-pocket encounters per capita; Panel 7D uses the OOP share of total healthcare spending. All regressions include CZ and year fixed effects and a binary control for state-level Medicaid expansion; standard errors are clustered at the CZ level. The horizontal axis denotes years relative to the merger event; event time $t = -1$ is omitted as the reference period, with the dashed vertical line indicating the period just before the merger took place.

V.4 Hospital mergers shift the payer mix away from Medicare and toward commercially-insured and out-of-pocket care

The two channels documented above—higher commercial prices and broader patient cost-sharing—both increase the financial exposure of commercially insured patients. A third channel can amplify

these effects: if hospital mergers shift the local payer mix away from public insurance and toward commercial coverage, then a larger share of healthcare utilization is subject to the price and cost-sharing increases documented in the preceding subsections.

We investigate this channel using Medicare-related outcomes from the IHME data. An important feature of the IHME Disease Expenditure data is that Medicare spending includes Medicare Advantage (MA) plans alongside traditional fee-for-service (FFS) Medicare. Consequently, a decline in IHME Medicare beneficiary-episodes cannot be attributed to a shift from FFS to MA within the Medicare program. Rather, because the IHME beneficiary measure counts condition-specific inpatient and ED episodes covered by Medicare (not unique enrollees), a decline indicates that fewer acute care admissions in the local area are being covered by Medicare—even if the underlying number of Medicare-enrolled individuals is unchanged.

Figure 8 presents the results. Panel 8A shows the effect on Medicare beneficiaries per capita—the number of condition-specific inpatient-plus-ED beneficiary-episodes covered by Medicare, divided by CZ population. The pre-treatment estimates are centered at zero and the post-treatment trajectory shows a clear, monotonic decline that reaches approximately -0.015 at $t = 5$. Relative to the 2013 baseline for the 2014 cohort (0.315 beneficiary-episodes per capita, from Table 2), this corresponds to a decline of roughly 5 percent. This is the cleanest individual result in the coverage channel: merger-affected CZs experience a persistent reduction in the number of acute care episodes covered by Medicare, suggesting that merged systems may be throttling the volume of inpatient and ED admissions billed to Medicare.

Panel 8B shows the corresponding effect on log Medicare spending. The point estimates turn negative immediately after treatment and remain so throughout the post-treatment period, reaching approximately -0.008 at $t = 5$. The estimates are less precise than those for beneficiary-episodes, but the direction is consistent: as fewer acute care episodes are covered by Medicare, total Medicare spending in affected commuting zones declines as well.

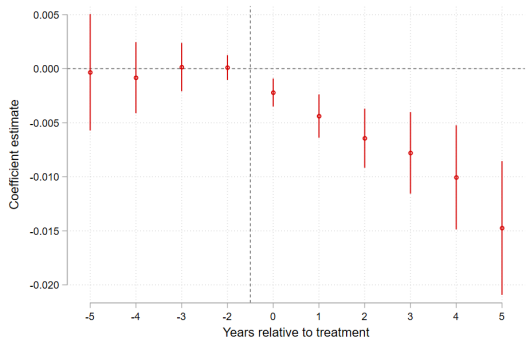
Panels 8C and 8D display the consequences for the payer composition of local healthcare spending. Panel 8C shows that Medicare's share of total spending declines by approximately 1.2 percentage points by $t = 5$, estimated with great precision. Panel 8D shows the mirror image: private insurance's share of total spending rises by approximately 2 percentage points over the same horizon. Together, these panels document a recomposition of the local payer mix away from Medicare and toward commercial insurance. The private insurance share increase exceeds the Medicare share decline, which is consistent with some of the shift also coming from Medicaid: the Medicaid spending share and Medicaid beneficiary-episodes per capita likewise decline after treatment, although the Medicaid results are less precisely estimated and show some evidence of pre-trends, so we do not emphasize them here.

The decline in Medicare-covered beneficiary-episodes could reflect several underlying mechanisms. Merged hospital systems, which derive higher margins from commercially insured patients, may steer capacity toward commercial admissions at the expense of Medicare-covered ones. Ser-

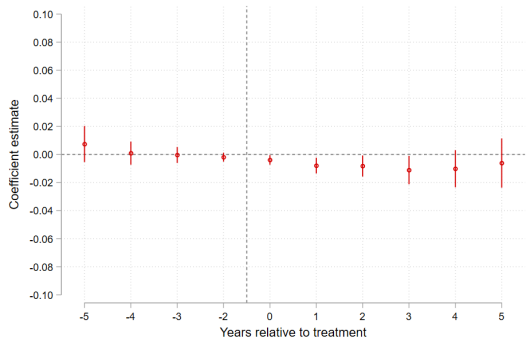
vice line consolidation or facility closures following mergers may disproportionately reduce access to the types of inpatient and ED care most heavily utilized by Medicare beneficiaries. Alternatively, merged systems may reclassify or restructure episodes in ways that shift billing away from Medicare and toward patients' supplemental or commercial coverage. We cannot distinguish among these explanations with the IHME data, but the aggregate pattern is clear: mergers reduce the share of acute care covered by Medicare and shift the payer mix toward commercial insurance. To the extent that care previously billed to Medicare is instead billed to commercial plans or directly to patients, the individuals affected face the higher prices and broader cost-sharing obligations documented in the preceding subsections. This coverage channel thus acts as a multiplier on the price and cost-sharing effects: not only are commercial prices higher and out-of-pocket episodes more frequent, but a larger share of local healthcare utilization is subject to commercial pricing.

FIGURE 8: Event study plots for the effect of hospital merger exposure on Medicare coverage and payer composition.

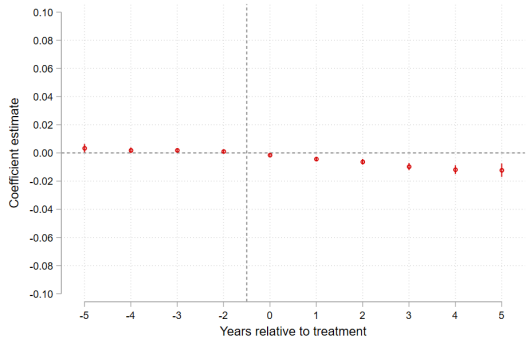
(A) Medicare beneficiaries per capita



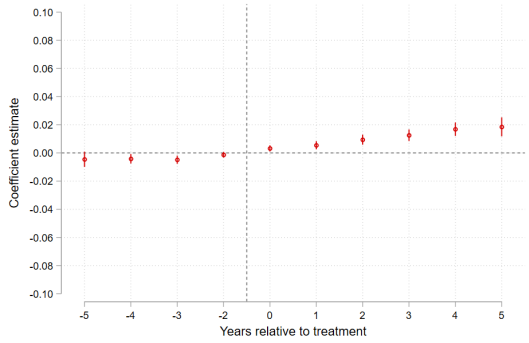
(B) Log Medicare spending



(C) Medicare spending share



(D) Private insurance spending share



Note: Each panel shows the point estimates and 95% confidence intervals for β_e, obtained from implementing Equation 2 at the CZ level using the IHME Disease Expenditure data. Panel 8A uses the level of inpatient-plus-ED Medicare beneficiary-episodes per capita; Panel 8B uses log Medicare spending; Panels 8C and 8D use the Medicare and private insurance shares of total healthcare spending, respectively. Medicare is inclusive of Medicare Advantage. All regressions include CZ and year fixed effects and a binary control for state-level Medicaid expansion. The horizontal axis denotes years relative to the merger event; event time $t = -1$ is omitted as the reference period, with the dashed vertical line indicating the period just before the merger took place.

V.5 Anti-competitive hospital–insurer contract provisions as a mechanism linking mergers to higher prices and and more cost-sharing

The three quantitative channels documented above in subsections V.2-V.4 describe *what* changes in the healthcare market following hospital mergers. An important remaining question is *how* merged

systems bring about these changes, given that commercial insurers have strong incentives to resist price increases and to steer their enrollees toward lower-cost providers. In this subsection, we describe a contractual mechanism, anticompetitive provisions in hospital–insurer contracts, that provides the institutional foundation for the price and cost-sharing effects we document. While we cannot directly test this channel with our data, the mechanism is well-documented in antitrust enforcement actions and is consistent with the patterns in our preceding IHME results.

V.5.1 Anti-steering contractual provisions and their economic logic

In a competitive hospital market, insurers discipline provider prices through two principal instruments: *steering*, in which the insurer uses financial incentives (lower copays, reduced co-insurance rates) to encourage enrollees to choose lower-cost or higher-quality providers within the same market; and *tiering*, in which the insurer places providers in differentiated network tiers, with enrollees paying lower cost-sharing for preferred-tier facilities. Together, steering and tiering create demand elasticity facing individual hospitals: a hospital that raises its prices risks losing patient volume as the insurer redirects demand to cheaper competitors. This, together with provider inclusion in insurance plans at all, is the central pro-competitive role commercial insurance plays in healthcare markets, if it plays any such role.

Dominant hospital systems, including, critically, systems that have grown through the type of horizontal mergers studied in this paper, have developed contractual instruments to neutralize both of these mechanisms. The most important are:

Anti-steering provisions contractually prohibit insurers from using financial incentives to direct enrollees away from the contracting hospital system and toward lower-cost competitors. These provisions eliminate the primary source of demand elasticity that would otherwise constrain post-merger price increases.

Anti-tiering provisions require that the hospital system be placed in the insurer's most preferred network tier regardless of its cost or quality, preventing the insurer from signaling to enrollees that cheaper alternatives exist. Anti-tiering and anti-steering provisions frequently appear together and are mutually reinforcing.

All-or-nothing provisions require that if an insurer includes any facility in a health system's network, it must include all of them. This forecloses the insurer's ability to selectively contract with, for example, the lower-cost suburban hospital in a system while excluding the higher-cost flagship—effectively extending the system's bargaining leverage across its entire portfolio of facilities (and as such an inducement for further horizontal and vertical consolidation among providers).

Gag clauses and price secrecy provisions prevent insurers from disclosing negotiated prices to enrollees, employers, or competing providers, thereby suppressing the price transparency that would be necessary for market-based steering to operate even absent explicit anti-steering language.

These provisions are the hospital-sector analog of the Most-Favored-Nations (MFN) clauses that

have been the subject of antitrust litigation in platform markets. In Ohio v. American Express Co. (2018), the Supreme Court considered whether a credit card network could contractually prohibit merchants from steering customers to lower-cost payment methods—a restriction that, like hospital anti-steering clauses, operates by preventing the intermediary from channeling demand toward cheaper alternatives. The economic logic is identical: in both cases, a firm with market power uses vertical contractual restraints to foreclose downstream price competition, insulating itself from the demand elasticity that would otherwise discipline its pricing.

V.5.2 How mergers amplify anticompetitive contracting

The relationship between hospital mergers and these contract provisions is mutually reinforcing. First, mergers increase a system's bargaining leverage over insurers by making the system more essential for network adequacy: an insurer that cannot include the dominant local system in its provider network cannot credibly sell coverage in that market. This leverage makes it easier to demand and enforce anticompetitive contract terms at each contract renewal. Second, large national hospital chains responsible for much of the acquisition activity in our sample period maintain standardized contracting templates that systematically include these provisions. When such a system acquires a community hospital, it may import these contracting practices to a market where the acquired hospital would not previously have had the leverage to demand them. Third, post-merger consolidation reduces the number of credible alternative providers available to insurers, making the threat to exclude the merged system from a network less credible and thereby reducing the insurer's ability to resist anticompetitive clauses.

V.5.3 Documentary evidence: Atrium Health and Sutter Health

Although contracts between hospitals and insurers are generally proprietary, antitrust enforcement actions have produced detailed public records documenting these practices.

Atrium Health (Charlotte, NC). In 2018, the U.S. Department of Justice filed a complaint against Carolinas Healthcare System (now Atrium Health), alleging that the system used anti-steering provisions in its contracts with every major commercial insurer in the Charlotte metropolitan area (United States v. Charlotte-Mecklenburg Hospital Authority, 2019). Atrium's market position had grown substantially through a series of acquisitions over the preceding decade, and the DOJ's complaint established that anti-steering provisions were imposed as that leverage increased, directly linking the provisions to acquired market power. The complaint documented that Atrium's own senior leadership identified insurer steering as the single greatest threat to the system's revenues, and that Atrium responded by contractually prohibiting insurers from steering and from disclosing price information that would facilitate market-based provider choice.

The consent decree made public the actual contractual language in Atrium's insurer agreements. For example, Atrium's contract with UnitedHealthcare stated: "Plan shall not directly or indirectly steer business away from Hospital." Its contract with Cigna provided that "CIGNA will

not steer business away from MHR Participating Providers" and further required that "All MHR entities...will be represented in the most preferred benefit level for any and all CIGNA products." The contract with Aetna permitted termination if Aetna introduced products that had "the effect of placing Participating PHO Providers in a non-preferred position." These provisions, spanning every major commercial insurer in the Charlotte area, contractually eliminated the demand elasticity that would otherwise constrain post-merger price increases. The case settled with a consent decree prohibiting Atrium from using anti-steering provisions for ten years.

Sutter Health (Northern California). The Sutter Health litigation provides an even more direct illustration of the link between hospital mergers and anticompetitive contracting. In December 1999, Sutter merged its Alta Bates hospital with Summit Medical Center in the East Bay market. The merger was challenged by the State of California (California v. Sutter Health System, 2000), but the district court approved it on the grounds that insurer steering would prevent Sutter from exercising market power. The ruling relied in part on Sutter's own expert testimony that "the primary mechanism by which MCOs and IPAs keep prices low is through the 'steering' of patients" and that insurers facing price increases "can attempt to steer patients to lower cost health care providers and away from the hospital imposing a price increase, thereby pressuring the hospital to eliminate the price increase."

Having secured merger approval on this basis, Sutter then systematically imposed contract provisions that prohibited the very steering the court had relied upon to conclude the merger would not be anti-competitive. The California Attorney General's subsequent complaint alleged that beginning no later than 2002, Sutter compelled commercial insurers across Northern California to accept all-or-nothing terms, anti-tiering provisions (which the complaint termed "Anti-Incentive Contract Terms"), and price secrecy clauses (UFCW & Employers Benefit Trust v. Sutter Health, 2021). The complaint documented that Sutter used punitive pricing to enforce these terms: if an insurer resisted including a high-priced newly-acquired facility, Sutter could substantially increase rates at its existing facilities, effectively coercing the insurer into accepting the system-wide terms. An FTC study found that post-merger price increases at Summit ranged from approximately 23 to 50% percent depending on the insurer, among the highest in California (Tenn, 2011). The case settled in 2021 for $575 million, with behavioral remedies requiring Sutter to cease all-or-nothing contracting, anti-tiering, and price secrecy practices.

The Sutter case is particularly informative because it demonstrates the full causal sequence: a hospital system merges, the merger is approved because steering is expected to constrain the exercise of market power, the merged system then contractually eliminates steering, and prices rise dramatically. This sequence is precisely the mechanism we would expect to generate the increases in private insurance spending per beneficiary-episode following mergers in our treatment sample shown in figure 6.

V.5.4 Consistency with the IHME results and connection to patient cost-sharing

The anti-steering mechanism helps explain two features of our IHME results that are otherwise puzzling. First, the private insurance spending increase documented in Subsection V.2 is sustained and amplifying over time, reaching approximately 4 percent per beneficiary-episode five years after treatment. In the absence of anti-steering provisions, we would expect insurer steering to partially offset post-merger price increases over time as insurers renegotiate contracts and redirect enrollees. The fact that the effect grows rather than attenuates is consistent with merged systems locking in contractual protections against steering that prevent competitive responses from eroding the price increase.

Second, the extensive-margin cost-sharing result documented in Subsection V.3—more healthcare encounters generating out-of-pocket obligations, without higher per-encounter charges—is naturally explained by the interaction between anti-steering provisions and insurer plan design. When anti-steering clauses prevent insurers from channeling enrollees to lower-cost providers through network design, one of the few remaining instruments available to insurers is to redesign their benefit plans to include broader patient cost-sharing. By imposing copays on a wider range of services or increasing deductibles, insurers can shift the burden of utilization management onto patients themselves: if the insurer cannot deter enrollees from using a high-cost hospital by designating it as non-preferred, it can at least ensure that enrollees internalize more of the cost of each encounter. This institutional logic produces the pattern we observe in the IHME data: a broader set of encounters with patient financial exposure (the extensive margin) without higher per-encounter out-of-pocket charges (the intensive margin), because the plan redesign operates by expanding cost-sharing obligations rather than increasing the rate charged per encounter.

We emphasize that we cannot directly test whether the hospitals in our treatment sample used anti-steering provisions. Hospital–insurer contracts are proprietary and not available at the scale necessary to construct a panel that would enable estimating equation 2 with anti-steering contractual provisions as the outcome of interest. However, the enforcement record establishes that these provisions are a documented, empirically consequential practice among consolidated hospital systems that achieve market power in local markets by means of mergers. The mechanism is consistent with our quantitative findings and provides a coherent institutional account of how merged systems translate acquired bargaining leverage into sustained price increases and expanded patient financial exposure.

V.6 Hospital mergers reduce local labor demand or concentrate the market for hospital workers

Here we consider whether the medical debt effects documented in Section IV are partly the result of lower local labor demand or employer power resulting from hospital mergers, which could reduce earnings and/or employment in local populations and thereby render them less able to pay medical bills.

Several recent papers have found hospital mergers negatively impacted healthcare workers. For example, Prager and Schmitt (2021) find that hospital mergers which result in large increases in local concentration negatively impact wage growth for workers with hospital industry-specific skills, while Allegretto and Graham-Squire (2023) find that nurses' wages are negatively impacted by hospital system consolidation in smaller markets. Andreyeva et al. (2024) find large cost savings result from systems acquiring independent hospitals, primarily through employee reductions for support and back-office staff. Such effects of hospital mergers could financially stress hospital industry workers and make them less able to service their debts.

Conversely, Brot-Goldberg et al. (2024) use hospital mergers to instrument for increases in local healthcare prices and find the consequent increases in local non-wage labor compensation (driven by more expensive employer-sponsored health insurance) have no effects within healthcare employers but significantly reduce payroll and employment at non-healthcare industry employers, driving significantly lower average earnings and higher unemployment at the county level. In effect, employers move up and to the left on the local aggregate labor demand function. The price channel documented in Subsection V.2—which shows an approximately 8 percent increase in private insurance spending five years after treatment—is precisely the type of commercial insurance cost increase that Brot-Goldberg et al. (2024) argue passes through to employer-sponsored health insurance premiums and depresses local labor demand. If operative, this channel could contribute to our medical debt findings by reducing workers' capacity to pay for healthcare, independently of the direct price and cost-sharing effects on patients.

We test for evidence of hospital merger impacts on local earnings and employment, as well as on local hiring and separations, for CZ-wide aggregate effects and within the Health Care and Social Assistance (NAICS 62) sector using the county-by-industry-by-quarter Quarterly Workforce Indicators data, which we aggregate at the CZ-by-industry-by-year level. Further disaggregation of the Health Care and Social Assistance Sector results in sharply higher numbers of suppressed cells, which recommends against doing so.

To dig deeper into the impacts of hospital mergers on hospital industry-specific employment, we utilize the hospital-level annual full-time equivalent (FTE) employment counts from the American Hospital Association data. Earnings are not observed in this data set.

The results of our analyses are presented in Appendix F.1. Figure F.1 shows the estimated impacts on the log of average earnings and employment by sector. There is no evidence of an effect on CZ-wide average earnings (Panel A), but there is some evidence of a small, negative effect on average earnings in the Health Care and Social Assistance sector (Panel B). There is also some evidence of a small, positive impact on CZ-wide employment (Panel C), though the pre-trend is not especially clean so we do not put much stock in these estimates. Panel D shows a clear increase in employment in the Health Care and Social Assistance sector, which taken with the sectoral average earnings effect in Panel B suggests that hospital mergers resulted in an increased number of lower-wage or part-time workers being hired within the sector. Interestingly, Panel E suggests no evidence that mergers increased FTE employment within hospitals (though also no evidence of

a decrease in hospital FTE employment).

To further understand the employment patterns described above, Figure F.2 shows our estimated impacts of hospital mergers on hires and separations, by sector. For all sectors within treated CZs there is noisy evidence of an increase in hires (Panel A) and slightly stronger evidence of an increase in separations (Panel C), though neither pre-trend is especially clean. These results further cast doubt on the marginally positive estimated effects on all-sector employment in Figure F.1. Focusing on the Health Care and Social Assistance sector, there is much stronger evidence of an increase in both hires and separations, which hint that notably higher churn (which is defined at the level of the employer (Lazear and McCue, 2018), so not directly measurable here) is driving the sectoral increase in employment and decrease in average earnings seen in Figure F.1.

In sum, despite the fact that the price channel findings in Subsection V.2 provide the upstream condition identified by Brot-Goldberg et al. (2024)—higher commercial insurance costs—we do not find evidence that this translates into the downstream labor market contraction they document. The absence of a CZ-wide earnings decline suggests that the labor demand channel is not a primary driver of our medical debt results. It is possible that increased churn within the Health Care and Social Assistance sector may have briefly caused some financial disruption for workers in that sector, but the earnings and employment results suggest any such impact would have been very modest and relatively short-lived. The medical debt effects we document are therefore more likely attributable to the direct price, cost-sharing, and coverage channels than to an indirect labor market pathway.

V.7 Other potential mechanisms

The three channels documented above—higher prices for commercially-insured patients, broader patient cost-sharing, and a shift in the payer mix away from Medicare—provide a coherent account of how hospital mergers increase medical debt. However, they do not exhaust the set of plausible mechanisms. Two additional channels merit discussion, though we lack the data to test them directly.

First, hospital mergers may be downstream from financial distress on the part of merger targets. If financially distressed hospitals resort to both price increases and more aggressive debt collection in the hope of shoring up their balance sheets or making themselves more financially attractive to potential acquirers, then the merger event itself may be an indicator rather than a cause of the conditions generating medical debt. Disentangling this reverse-causality story from the forward-looking causal channels would require detailed data on hospital financial health in the years preceding mergers, which we do not (yet) observe.

Second, hospitals are heterogeneous in the aggressiveness of their debt collection practices, and mergers may transmit the acquiring system's collection norms to newly acquired facilities. If acquiring systems are systematically more aggressive collectors—whether through more frequent use of lawsuits, wage garnishment, or credit reporting—then mergers could increase medical debt in

collections even if out-of-pocket costs were unchanged. Testing this channel would require facility-level data on collection practices before and after acquisition, which is not currently available at scale.

We leave empirical investigation of these channels to future research.

VI Discussion and conclusion

This paper demonstrates that hospital mergers cause economically meaningful increases in medical debt. Using a staggered difference-in-differences design that exploits variation in the timing and geography of mergers predicted to raise local hospital market concentration, we find that merger exposure increases the likelihood of having medical debt in collections by 11 percent, the flow of new medical debt by 20 percent, and the total outstanding stock by 23 percent, five years after treatment. These effects amplify steadily over time, are robust to alternative sample definitions and treatment thresholds, and are not driven by pre-existing trends. A placebo test using utility and cable/cellular debt in collections—categories plausibly unaffected by hospital mergers—yields null results, reinforcing the interpretation that the medical debt effects are specific to the healthcare sector rather than reflecting a broader deterioration in credit conditions in merger-affected areas.

Beyond the medical debt that appears on credit reports as collections tradelines, we find that hospital mergers increase credit card balances along the intensive margin: individuals in merger-exposed areas carry higher credit card debt without any change in the probability of holding a credit card. This pattern is consistent with patients financing medical expenses on credit cards, particularly when hospitals condition access to non-emergency care on the resolution of outstanding balances (Kliff and Silver-Greenberg, 2023). The implication is that the medical collections tradelines we study as our primary outcome represent a lower bound on the total financial burden that hospital mergers impose on patients.

A central contribution of this paper is to trace the mechanisms linking hospital mergers to individual medical debt. Using comprehensive county-level healthcare spending and utilization data from the IHME Disease Expenditure Project (Dieleman et al., 2025), we identify three complementary channels. First, mergers raise the price of commercially insured care. Private insurance spending per beneficiary-episode—a reduced-form proxy for the price of a treated episode—increases by approximately 4 percent five years after treatment, while utilization remains stable. This finding is consistent with the large existing literature documenting that hospital mergers confer pricing power over commercial insurers (Cooper et al., 2019; Gowrisankaran, Nevo and Town, 2015), and it confirms that the standard cost-sharing provisions of commercial insurance plans translate those higher negotiated prices into higher out-of-pocket obligations for patients.

Second, mergers expand the set of healthcare encounters for which patients bear direct out-of-pocket financial responsibility. Total out-of-pocket spending rises by roughly 7 percent, but this increase is driven entirely by a greater number of encounters involving patient payments rather than

by higher charges per encounter. This extensive-margin result is, to our knowledge, novel in the hospital mergers literature. It implies that mergers do not merely make each episode of care more expensive for patients; they also broaden the scope of care on which patients have direct financial exposure, whether through restructured cost-sharing arrangements, the elimination of competing providers where patients had more favorable coverage terms, or other changes to the insurance-provider contracting environment that follow from consolidation.

Third, mergers shift the local payer mix away from Medicare and toward commercial insurance. The number of acute care beneficiary-episodes covered by Medicare declines by approximately 5 percent, while the private insurance share of total spending rises by roughly 2 percentage points. Because commercially insured care is subject to the higher prices and broader cost-sharing documented in the first two channels, this recomposition of the payer mix acts as a multiplier: not only are commercial prices higher and out-of-pocket episodes more frequent, but a larger share of local healthcare utilization is subject to commercial pricing in the first place.

These three channels operate in concert. The price channel raises the cost of each episode where patients have financial exposure. The cost-sharing channel expands how many episodes create such exposure. And the payer mix channel ensures that a growing share of care is subject to both forces. Together, they imply that commercial health insurance plans effectively become less generous relative to the total cost of care in the wake of hospital mergers, even absent any explicit change in plan design. This interpretation is consistent with the credit card findings: as patients face both higher and more frequent out-of-pocket obligations, some absorb those costs through credit card borrowing rather than allowing them to proceed to collections, while others cannot pay at all and accumulate medical debt.

A fourth mechanism, anti-competitive provisions in hospital–insurer contracts, provides the institutional foundation for these effects. Anti-steering clauses, documented in federal and state enforcement actions against Atrium Health and Sutter Health (United States v. Charlotte-Mecklenburg Hospital Authority, 2019; UFCW & Employers Benefit Trust v. Sutter Health, 2021) contractually prevent insurers from directing enrollees to lower-cost competitors, thereby eliminating the demand elasticity that would otherwise discipline post-merger price increases. These provisions are the hospital-sector analog of platform MFN clauses at issue in cases such as Ohio v. American Express Co. (2018). The Sutter Health litigation is especially instructive: the system's 1999 merger was approved on the grounds that insurer steering would constrain the exercise of market power (California v. Sutter Health System, 2000), yet Sutter subsequently imposed contractual provisions eliminating steering across Northern California, and post-merger prices rose (Tenn, 2011). When merged systems bar insurer steering by contract, one of the few instruments remaining to insurers is to redesign their plans to include broader patient cost-sharing, shifting the burden of utilization management onto patients and generating the extensive-margin out-of-pocket spending increases documented in our IHME analysis. While we cannot directly test the anti-steering channel with our data, the enforcement record, combined with the consistency between the mechanism and our quantitative findings, strongly suggests that anticompetitive contracting is a central institutional

channel through which hospital mergers translate into higher medical debt.

We do not find evidence that an indirect labor market channel is a primary driver of our results. Despite the fact that the price channel findings provide exactly the upstream condition—higher commercial insurance costs—that Brot-Goldberg et al. (2024) argue depresses local labor demand through employer-sponsored insurance premiums, we observe no decline in commuting-zone-wide average earnings following merger exposure. Within the Health Care and Social Assistance sector, there is some evidence of lower average earnings and higher worker churn, but these effects are modest and unlikely to account for the sustained, amplifying increases in medical debt that we document.

Several limitations of our analysis merit discussion. First, because changes in the reporting of medical debt by credit bureaus compromise the reliability of the data after 2019, our study period ends before the COVID-19 pandemic. The pandemic brought an unprecedented combination of hospital financial distress, emergency federal relief, and further consolidation activity, all of which may have altered the relationship between hospital market structure and medical debt in ways our data cannot capture. Second, we cannot link individual credit records to individual insurance claims, which prevents us from observing the specific insurance plans, cost-sharing terms, and medical services underlying each debt. The IHME data allow us to characterize the aggregate spending and utilization channels at the commuting zone level, but individual-level variation in these mechanisms remains unobserved. Third, as discussed in Section V, two additional mechanisms that we cannot test directly may contribute to the effects we document: pre-merger financial distress at target hospitals, which could generate both more aggressive billing and the merger event itself, creating a reverse-causality concern; and the transmission of acquiring systems' collection practices to newly acquired facilities, which could increase the rate at which unpaid bills are sent to collections even if underlying out-of-pocket costs are unchanged. Empirical investigation of these channels would require facility-level data on hospital financial health and collection practices before and after acquisition, which are not currently available at scale.

Our findings carry implications for antitrust enforcement and healthcare policy. The prevailing framework for evaluating hospital mergers focuses on the price effects of consolidation—whether mergers raise the prices that hospitals negotiate with commercial insurers. Our results indicate that the consequences of hospital mergers extend well beyond negotiated prices to encompass the broader financial relationship between patients and the healthcare system. The extensive-margin cost-sharing channel and the payer mix shift we document would not be captured by an enforcement framework that focuses narrowly on price, yet they contribute materially to the financial burden that patients bear. To the extent that merger review aims to protect consumer welfare, the downstream effects on medical debt and household financial distress warrant consideration alongside the direct price effects.

More broadly, our findings speak to the interaction between market structure and the design of the U.S. healthcare financing system. Medical debt arises in the gap between the cost of care and what insurance covers, and that gap is shaped by the bargaining power of hospitals relative to

insurers and patients. Hospital mergers widen this gap from both sides: they raise the cost of care through higher commercial prices, and they erode the effective generosity of insurance coverage by expanding the set of encounters on which patients have financial exposure and by shifting care toward payers with less administered price regulation. The result is a transfer of financial risk from hospitals and insurers to patients—precisely the population least equipped to bear it. Addressing medical debt as a policy matter therefore requires attention not only to the credit reporting rules and collection practices that determine how medical debt appears on consumers' records, but also to the upstream market structure conditions that generate the debt in the first place.

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Appendices

A. Data extraction and cleaning

A.1 Data extraction

A.1.i Attribute files

- Extracted Q4 for each year from 2011 to 2024. Even though the last period used is 2019Q4, the additional time periods are useful when making some demographic corrections (e.g., gender or race).
- The extraction is limited to ”Primary Records”, i.e., the observations where the variable ”source_cd_pr” is defined as “PR”.

A.1.ii Tradeline files

- Extracted Q4 from 2011 to 2019. We focus on the last quarter of every year because the hospital data from which we obtain merger information is defined at the yearly level.
- **Medical debt in collections:** these tradelines are categorized as being in ”Collection” or with a ”Debt buyer”, according to the ”loan_type” variable. In addition, the following conditions need to be met:
  - The variable for the original creditor classification code (”orig_creditor_cls_cd”) is coded as ”02” (”Medical/Health Care”).
  - The variable for the enhanced account type code (”enhanced_acct_type_cd”) is coded as ”90” (”Medical debt”)
  - The variable identifying the creditor’s business code (”kob_cd”) is set to ”MG” (”Medical group”), ”MH” (”Hospitals and Clinics”), or ”MZ” (”Medical and related health - nonspecific”).
- **Utility, cable, and cellular debt in collections:** these tradelines are categorized as being in ”Collection” or with a ”Debt buyer”, according to the ”loan_type” variable. In addition, the following conditions need to be met:
  - The variable for the original creditor classification code (”orig_creditor_cls_cd”) is coded as ”10” (”Utilities”) or 11 (”Cable/Cellular”).
  - The variable for the enhanced account type code (”enhanced_acct_type_cd”) is coded as ”92” (”Utility company”) or ”4D” (”Telecommunications/Cellular”).

  - The variable identifying the creditor's business code ("kob_cd") is set to "UA" ("Water utilities/Bottled water"), "UC" ("Cable TV provider"), "UD" ("Garbage and rubbish disposal companies"), "UE" ("Electric light and power companies"), "UF" ("Fuel oil distributors"), "UG" ("Gas companies, natural and bottled"), "UH" ("Coal and wood suppliers"), "UL" ("Long-distance telephone service"), "UO" ("Online/internet access"), "UP" ("Cellular and paging company"), "UR" ("Waste recycling/Waste handlers"), "US" ("Satellite TV/Direct broadcast"), "UT" ("Local telephone service provider"), "UV" ("Home security company"), "UW" ("Wireless telephone service"), or "UZ" ("Utilities and fuel - nonspecific").

- **Non-medical debt in collections:** these correspond to the set of tradelines that are categorized as being in "Collection" or with a "Debt buyer", according to the "loan_type" variable, but identified as not being medical debt, based on the criteria defined earlier in this section.
- **Credit card debt:** these tradelines are identified based on the variable "loan_type" created by UCCCP. Specifically, we consider all the tradelines identified as "Credit card" or "Retail credit card" by that variable.

A.2 Clean attribute files

We adopt the following procedures to improve the consistency and reliability of the key demographic variables included in the attribute files, as well as to address the issues caused by fragmented files, i.e., partial files that should not be in the data, such as cases where two people are present with different unique identifiers because the credit bureau is unable to link the two records (Gibbs et al., 2025):

- **Birth date:** initially missing for 21% of observations. We correct this in two main ways. First, for each individual, we assign them their modal birth date. Second, in cases where only the birth date is missing but the birth year is present, we assign January as the birth month. With this procedure, we reduce the share of observations with missing birth date to 9%.

- **Age:** this is a variable created by UCCCP, but it is not clear how it is being created, i.e., how age is being imputed when the birth date variable is missing. We correct this variable (regardless of whether it was previously missing or not) based on the updated birth date variable and the reporting date associated with the corresponding attribute file. This results in edits to 20% of the observations and reduces the share of missing observations from 10% to 9%.
- **Drop fragmented files:** this is done in 3 steps, following UCCCP's procedure (in total, these drop about 17% of the observations):
  - Drop observations whose credit score is below 300.

  - Drop observations with a missing birth date.
  - Drop observations where the individual is listed as deceased by the "deceased_in" variable.

- **Drop individuals inconsistently tagged as deceased:** this is defined as being identified as not deceased in year t after being identified as deceased in year $t-1$. This procedure results in dropping approximately 0.9% of the individuals in the sample.

- **Gender:** initially missing for about 23% of observations. To reduce this share as well as the cases of inconsistent gender coding within individuals, we assign each individual their modal gender during the period. In cases where there is more than one modal gender, we set it to missing. With this procedure, we reduce the share of observations with missing gender to 13%.

- **Race:** this variable is coded in UCCCP as a probability of being non-Hispanic white, non-Hispanic Black, Hispanic, non-Hispanic Asian, non-Hispanic American Indian or Alaska Native, or non-Hispanic multiracial or another category. It is initially missing for 87% of observations, as it is only assigned in 2023 and 2024. For each individual where this probability is non-missing, we take the average of the 2023 and 2024 values and assign the race based on the highest probability value. With this procedure, we reduce the share of observations with missing race to 23%.

- **Location:**
  - UCCCP includes several geographic location variables, including information on the state, county, and zip code where each individual is located. We take the zip code variable as the most reliable. Ultimately, we want to assign each individual to a commuting zone.
  - We use the zip-to-county crosswalks available at the US HUD's Office of Policy Development and Research for Q4 of each year to assign each zip to a county. For about 75% of all US zip codes, they are fully contained within a county, such that the match is exact. In the remaining cases, we assign each zip to the county that has the highest share of total addresses from that zip; in the cases where there is a tie, we drop those zip codes. We use HUD's zip-to-county crosswalk to assign individuals in the UCCCP data to a "fips_code." As this crosswalk covers the near universe of observations in UCCCP, this approach allows us to reduce the share of observations with a missing fips_code to less than 0.1%. To address those remaining observations without a county identifier, we take the following steps:
    * For each individual that (i) has a missing fips_code in year t but non-missing in $t-n$ or $t+n$, and that (ii) has not changed zip code between those years: assign

the fips_code from year $t-n$ or $t+n$ to year t.

* In the cases where a given zipcode is always assigned to the same county in the UCCCP data but the fips_code is missing for that observation (e.g., because it is a zip code that no longer exists in a given year t and therefore does not have a match in the yearly zip-to-county crosswalk, we assign the modal fips_code that corresponds to that zip.

* We generate an alternative county identifier variable by concatenating the variables "state_abb" and "fips_county_cd" that are present in the UCCCP raw data. We then take, for each zip, the corresponding modal county identifiers that result both from the "fips_code" variable obtained from the zip-to-county crosswalks and from this alternative county identifier. In the cases where the modal value from both approach is identical, we assign that county.

* We then assign the alternative county identifier.

* We again repeat the procedure described above where, for each individual that (i) has a missing "fips_code" in year t but non-missing in $t-n$ or $t+n$, and that (ii) has not changed zip code between those years, we assign the "fips_code" from year $t-n$ or $t+n$ to year t.

– **Run two geographic crosswalks.** The first, based on county change records from Dorn (2021) and **?**, accounts for county FIPS code changes over time and maps them into consistent 1990 county FIPS codes. The second uses David Dorn's crosswalk (Autor and Dorn, 2013) to map the county FIPS codes to 1990 commuting zones (CZs), assigning each individual to the 1990 CZ classification.

  * The majority of the counties that do not match with Dorn's county-to-CZ crosswalk are in AK or in US territories. The remaining cases seem to result from fips_codes that do not exist (e.g., because of credit bureau data entry errors in either the state or the county FIPS). In those cases, the fips_code is replaced with fips_code_hud.

– Drop remaining observations with missing fips_code ($<0.05\%$).

A.3 Clean tradeline files for medical debt in collections

- **Drop individuals when a tradeline is assigned to more than one person.** This drops approximately 0.1% of individuals in the sample (i.e., the set of individuals who show up in UCCCP as having medical debt in collections sometime between 2011 and 2019).
- **Assign the modal opening date:** for cases where acct_open_dt is not constant within the tradeline, assign the modal acct_open_dt; if there is more than one mode, the oldest date is

assigned. This affects less than 0.3% of the observations.

- **Assign the modal original amount:** first, set to missing in cases where it is recorded as zero (<0.01% of observations). Then, for the cases where the original amount in collections is not constant within the tradeline, assign the modal amount; if there is more than one mode, the smaller mode is assigned. This affects approximately 0.3% of the observations.
- **Linearly interpolate the outstanding balance:** if the balance for a given tradeline is zero in year t and non-zero in years $t-n$ and $t+n$, replace the zero(s) with the linear interpolation between the two non-zero values. This affects less than 0.01% of the observations.
- **Set the outstanding balance to zero in tradelines deemed to have been paid or discharged:** we define this type of tradeline based on the "acct_cond_cd" and "enhanced_spcl_cmmt" variables. Specifically, when the former takes the values "A2" ("Paid account/zero balance"), "A3" ("Closed account"), "21" ("Consumer reported as deceased"), "67" ("Debt included or discharged through Chapter 7, 11, or 12 bankruptcy"), "68" ("Account legally paid in full for less than the full balance"), and "69" ("Debt included or discharged through Chapter 13 bankruptcy"), or when the latter takes the values "18" ("Account closed at credit grantor's request"), "19" ("Account closed at consumer's request"), "25" ("Paid by cosigner"), "28" ("Account closed due to transfer or refinance"), "50" ("Account closed due to refinance"), "51" ("Account closed due to transfer"), "52" ("Account legally paid in full for less than the full balance"), "73" ("Paid through insurance"). This affects only about 0.05% of tradelines.

- **For accounts deemed to have been paid or discharged in year t, set any subsequent balance amount to zero:** this affects about 0.01% of tradelines.
- **Set the outstanding balance to zero if the debt is disputed by the consumer:** this follows the approach from Kluender et al. (2021) and is based on the "enhanced_spcl_cmnt_cd" variable taking the values "78" ("Account information disputed by consumer") or "83" ("Account closed at consumer's request and in dispute under FCRA"). This affects approximately 2% of tradelines. In this case, the tradeline can take non-zero values in subsequent periods, if the code associated with it changes.
- **Drop tradelines where the ouststanding amount is always zero**.
- **Cases where a tradeline is absent in year t but present in both $t-n$ and $t+n$:** add those observations to the data, linearly interpolate the outstanding balance amounts, and carry forward the location variables. This affects approximately 0.8% of tradelines.
- **Cases where a tradeline does not appear in the credit bureau data in the year when it is first created:** approximately 18% of tradelines take more than 12 months to show up in the credit reporting data, relative to the date when they were created (given by the variable "acct_open_dt"). For these cases, the corresponding observations are created and added to

the dataset. The values for the key variables are then imputed, for that set of observations, based on the following rules:

  - Location/demographic variables: first, they are assigned the individual's modal value for that year. If the modal value is not defined, the values are assigned based on the first/earliest observation available for the individual.
  - Original tradeline amount: imputed based on the modal value for the tradeline.
  - Account balance amount: linearly interpolated, taking the starting balance as the original tradeline amount and the final balance as the balance amount that is observed when that tradeline is first recorded in the UCCCP data.

- **Define the key outcome variables of interest.** For the three outcomes of interest, two alternative definitions were constructed:

  1. An "adjusted" measure that took into account (i) the changes that set the balance amounts outstanding to zero (because they were deemed to have been paid or because they were disputed by the debtors) and (ii) the various linear interpolations implemented to address the issue of missing tradelines, as described above.
  2. A "raw" measure that did not do either of those adjustments.

  The three outcomes of interest were defined as follows:

  - **Total medical debt in collections:** at the individual level and for each year, this corresponds to the sum of the outstanding balance amounts for each tradeline that is categorized as medical debt in collections. The individuals who are present in the attributes files but not in the tradeline files for medical debt in collections for a given year (or for all years) are considered to not have medical debt in collections and are assigned a value of zero.
  - **New medical debt generated over the previous 12 months:** this variable seeks to measure the flow of medical debt that occurs each year and complements the "stock" measure that considers the total amount of debt outstanding.
    For the "adjusted" measure, this was defined based on the "acct_open_dt" variable. For the observations where the opening year is the same as the reporting year, then the respective original loan amount for this tradeline is defined as new medical debt in collections generated over the previous 12 months.
    For the "raw" measure, it corresponds to the original loan amount in the reporting year that a tradeline is first observed.
    For both types of measures, the tradeline amount is then aggregated at the individual-year level.
  - **Has medical debt:** binary variable that takes the value of 1 if the "total medical debt in collections" has a positive value.

- **Balances are winsorized at the 99.99th percentile.** This is done at the individual - rather than tradeline - level and based on the distribution for each year, so the winsorization threshold varies accordingly.

A.4 Clean tradeline files for non-medical debt in collections and for utility, cable, and cellular debt in collections

The cleaning of these types of tradelines followed the same rules as the cleaning of tradelines related to medical debt in collections, outlined in section A.3.

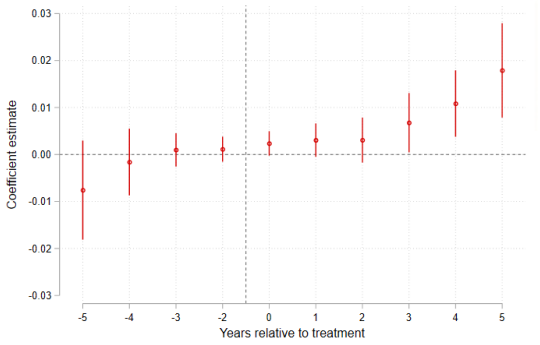
A.5 Clean tradeline files for credit card debt

The cleaning of these types of tradelines followed the same rules as the cleaning of tradelines related to medical debt in collections, outlined in section A.3, except that no adjusted amounts are computed and only the raw amounts are used.

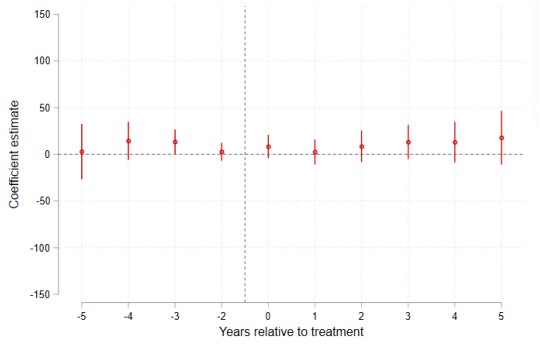
B. Event study plots for "raw/unadjusted" medical debt outcomes

FIGURE B.1: Event study plots for the effect of hospital merger exposure on medical debt ("raw/unadjusted" outcomes).

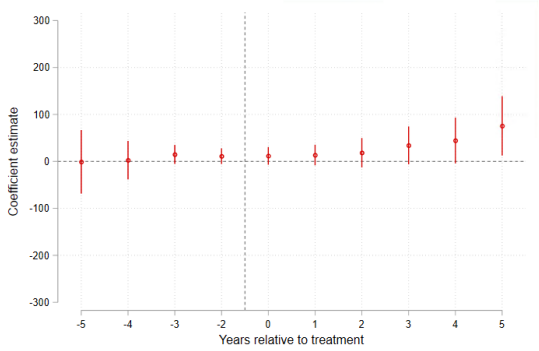
(A) Likelihood of having medical debt



(B) New medical debt (last 12 months)



(C) Total medical debt



Note: This figure replicates Figure 3 in the main draft, but using the "raw/unadjusted" outcomes instead of the adjusted ones. Each panel shows the point estimates and 95% confidence intervals for β_e, obtained from implementing equation 1, and illustrates the effect of exposure to hospital mergers on "raw/unadjusted" medical debt outcomes. The panels, outcomes, specification fixed effects and controls, and standard errors are otherwise as defined in Figure 3. The horizontal axis denotes years relative to the merger event; event time $t = -1$ is omitted as the reference period, with the dashed vertical line indicating the period just before the merger took place.

C. Event study regression tables for medical debt, baseline specifications

TABLE C.1 Effect of merger exposure on likelihood of having medical debt (adjusted) in collections (2011-2019).

	(1) Has medical debt (adj) Full sample	(2) Has medical debt (adj) Full sample	(3) Has medical debt (adj) Full sample	(4) Has medical debt (adj) Never movers	(5) Has medical debt (adj) Never movers Under 65	(6) Has medical debt (adj) HHI change >150	(7) Has medical debt (adj) HHI change >150 level>1800	(8) Has medical debt (adj) HHI change >150 level>2500
-5	-0.0054	-0.0058	-0.0058	-0.0060	-0.0081	-0.0083*	-0.0069	-0.0017
	(0.0055)	(0.0051)	(0.0052)	(0.0051)	(0.0059)	(0.0046)	(0.0053)	(0.0065)
-4	0.0002	-0.0003	-0.0003	0.0000	-0.0004	-0.0000	0.0012	0.0047
	(0.0034)	(0.0033)	(0.0033)	(0.0033)	(0.0038)	(0.0043)	(0.0050)	(0.0065)
-3	0.0025	0.0013	0.0013	0.0012	0.0013	-0.0014	-0.0005	0.0018
	(0.0018)	(0.0017)	(0.0017)	(0.0018)	(0.0020)	(0.0022)	(0.0028)	(0.0043)
-2	0.0012	0.0007	0.0007	0.0008	0.0009	-0.0010	-0.0005	-0.0021
	(0.0012)	(0.0011)	(0.0011)	(0.0011)	(0.0013)	(0.0016)	(0.0018)	(0.0021)
0	0.0022*	0.0023**	0.0024**	0.0022*	0.0034**	-0.0003	-0.0028	-0.0089**
	(0.0011)	(0.0011)	(0.0012)	(0.0012)	(0.0014)	(0.0023)	(0.0027)	(0.0035)
1	0.0021	0.0031*	0.0032*	0.0030*	0.0045**	0.0002	-0.0024	-0.0048**
	(0.0017)	(0.0016)	(0.0017)	(0.0017)	(0.0020)	(0.0026)	(0.0027)	(0.0024)
2	0.0025	0.0045*	0.0046**	0.0037	0.0059**	0.0048	0.0025	-0.0035
	(0.0025)	(0.0024)	(0.0024)	(0.0023)	(0.0028)	(0.0033)	(0.0038)	(0.0040)
3	0.0054	0.0079**	0.0079**	0.0065**	0.0092**	0.0037	-0.0007	0.0035
	(0.0034)	(0.0032)	(0.0032)	(0.0032)	(0.0038)	(0.0044)	(0.0053)	(0.0061)
4	0.0090**	0.0117***	0.0118***	0.0101***	0.0134***	0.0057	0.0051	0.0084
	(0.0040)	(0.0037)	(0.0038)	(0.0038)	(0.0045)	(0.0047)	(0.0066)	(0.0079)
5	0.0170***	0.0195***	0.0196***	0.0172***	0.0224***	0.0079	0.0112	0.0061
	(0.0065)	(0.0058)	(0.0058)	(0.0057)	(0.0068)	(0.0068)	(0.0091)	(0.0106)
Observations	40,825,098	40,519,769	40,519,769	31,648,263	24,646,169	17,719,228	16,320,305	14,303,468
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
CZ FEs	Y	Y	Y	Y	Y	Y	Y	Y
Individual FEs	N	Y	Y	Y	Y	Y	Y	Y
Medicaid exp.	N	N	Y	Y	Y	Y	Y	Y

*** p<0.01; ** p<0.05; * p<0.10

Notes: Robust standard errors clustered at the CZ level in parenthesis. The dependent variable is binary and takes the value of 1 if the individual has outstanding medical debts in collection. Columns (1) to (3) use the full sample and sequentially add fixed effects and a binary control for the state-level Medicaid expansion from 2014 onward. Column (4) restricts the sample to never movers and column (5) further restricts it to never mover under the age of 65. Column (6) goes back to the full sample as the starting point and then restricts the treatment group to CZs where the merger-induced HHI increase exceeded 150; column (7) further restricts the treatment group to CZs where the post-merger HHI exceeded 1800 and column (8) increases the post-merger HHI threshold to 2500.

TABLE C.2 Effect of merger exposure on new medical debt in collections (adjusted) created over the previous 12 months (2011-2019).

	(1) New med debt last 12m adj) Full sample	(2) New med debt last 12m adj Full sample	(3) New med debt last 12m adj Full sample	(4) New med debt last 12m adj Never movers	(5) New med debt last 12m adj Never movers Under 65	(6) New med debt last 12m adj HHI change >150	(7) New med debt last 12m adj HHI change >150 level>1800	(8) New med debt last 12m adj HHI change >150 level>2500
-5	-12.3	-12.7	-13.5	-12.3	-15.6	-22.6	-6.2	17.4
	(15.6)	(16.1)	(15.8)	(14.0)	(16.7)	(22.6)	(19.2)	(22.5)
-4	-0.3	-0.8	1.3	2.8	1.8	-13.3	-3.9	16.7
	(9.1)	(9.5)	(9.4)	(8.4)	(9.8)	(17.3)	(17.7)	(14.3)
-3	0.9	-0.6	-0.2	-1.6	-1.6	-1.4	4.1	4.5
	(5.9)	(6.0)	(5.9)	(5.4)	(6.6)	(7.4)	(7.9)	(8.4)
-2	-4.0	-4.7	-3.6	-3.8	-4.2	-4.6	-0.3	-1.6
	(5.0)	(5.2)	(5.3)	(4.9)	(5.9)	(7.2)	(8.5)	(9.7)
0	3.7	3.5	9.0*	4.4	6.9	-4.1	-5.1	2.0
	(4.7)	(5.0)	(5.1)	(4.6)	(5.7)	(6.0)	(7.2)	(12.6)
1	-1.0	-0.1	5.3	-1.7	-0.6	-3.1	-3.1	18.2
	(7.7)	(8.4)	(7.7)	(7.1)	(8.7)	(9.5)	(11.0)	(16.0)
2	6.9	9.2	15.0*	6.8	11.2	7.3	12.0	28.2
	(8.6)	(9.3)	(8.9)	(8.2)	(10.1)	(10.0)	(12.8)	(20.1)
3	7.7	10.8	16.8	6.9	11.0	-1.2	6.9	40.2*
	(10.6)	(11.4)	(10.9)	(10.1)	(12.5)	(12.2)	(16.2)	(21.5)
4	7.8	12.2	18.8	7.3	10.9	12.0	19.6	58.4**
	(11.9)	(12.7)	(12.1)	(11.3)	(13.9)	(15.3)	(19.2)	(25.0)
5	19.8	24.7	31.5*	19.2	27.8	39.5*	54.6**	93.5**
	(16.1)	(16.9)	(16.6)	(15.1)	(18.3)	(20.7)	(23.0)	(41.4)
Observations	40,825,098	40,519,769	40,519,769	31,648,263	24,646,169	17,719,228	16,320,305	14,303,468
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
CZ FEs	Y	Y	Y	Y	Y	Y	Y	Y
Individual FEs	N	Y	Y	Y	Y	Y	Y	Y
Medicaid expansion	N	N	Y	Y	Y	Y	Y	Y

*** p<0.01; ** p<0.05; * p<0.10

Notes: Robust standard errors clustered at the CZ level in parenthesis. The dependent variable is the total amount of outstanding medical debts in collection, measured in 2017 USD. Columns (1) to (3) use the full sample and sequentially add fixed effects and a binary control for the state-level Medicaid expansion from 2014 onward. Column (4) restricts the sample to never movers and column (5) further restricts it to never mover under the age of 65. Column (6) goes back to the full sample as the starting point and then restricts the treatment group to CZs where the merger-induced HHI increase exceeded 150; column (7) further restricts the treatment group to CZs where the post-merger HHI exceeded 1800 and column (8) increases the post-merger HHI threshold to 2500.

TABLE C.3 Effect of merger exposure on the total amount (adjusted) of medical debt in collections (2011-2019).

	(1) Total (adj) balance	(2) Total (adj) balance	(3) Total (adj) balance	(4) Total (adj) balance	(5) Total (adj) balance	(6) Total (adj) balance	(7) Total (adj) balance	(8) Total (adj) balance
	Full sample	Full sample	Full sample	Never movers	Never movers Under 65	HHI change >150	HHI change >150 level>1800	HHI change >150 level>2500
-5	1.1	1.5	0.7	0.0	2.5	-42.0	-15.9	26.5
	(35.1)	(33.6)	(34.2)	(31.2)	(38.1)	(38.5)	(40.9)	(66.4)
-4	6.8	6.5	8.8	9.7	12.8	0.7	15.3	49.6
	(21.7)	(21.0)	(21.2)	(19.6)	(24.1)	(27.7)	(30.2)	(44.3)
-3	23.3*	20.9*	21.4*	16.4	21.9	2.6	9.3	27.9
	(12.2)	(12.2)	(12.1)	(11.1)	(13.5)	(15.7)	(17.9)	(29.5)
-2	11.0	10.4	11.6	10.0	12.9	-2.9	3.7	9.0
	(8.7)	(9.0)	(9.2)	(8.4)	(10.2)	(13.5)	(14.4)	(21.7)
0	8.3	6.6	12.6	6.5	10.2	-2.1	-13.1	-34.0*
	(8.7)	(8.9)	(9.5)	(8.6)	(10.8)	(13.8)	(17.9)	(19.8)
1	11.2	11.6	17.5	7.8	11.3	-2.0	-13.7	-8.6
	(12.0)	(12.1)	(11.6)	(10.8)	(13.5)	(15.8)	(18.7)	(24.0)
2	23.8	25.8	32.2*	15.7	23.7	22.7	15.7	-4.3
	(18.5)	(18.0)	(17.3)	(16.1)	(19.9)	(19.1)	(23.5)	(31.9)
3	41.2	44.3*	50.9**	29.4	41.3	26.5	18.6	64.3
	(25.4)	(24.1)	(23.7)	(22.0)	(27.0)	(25.3)	(33.1)	(46.0)
4	57.9*	59.8**	67.1**	41.9	56.7*	54.5*	55.7	88.8*
	(30.8)	(28.7)	(28.5)	(26.5)	(32.3)	(30.1)	(36.3)	(48.1)
5	102.6**	102.2***	109.7***	76.3**	102.6**	98.9**	121.8**	178.6***
	(42.6)	(38.3)	(37.9)	(35.1)	(42.1)	(43.2)	(49.1)	(59.9)
Observations	40,825,098	40,519,769	40,519,769	31,648,263	24,646,169	17,719,228	16,320,305	14,303,468
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
CZ FEs	Y	Y	Y	Y	Y	Y	Y	Y
Individual FEs	N	Y	Y	Y	Y	Y	Y	Y
Medicaid expansion	N	N	Y	Y	Y	Y	Y	Y

*** p<0.01; ** p<0.05; * p<0.10

Notes: Robust standard errors clustered at the CZ level in parenthesis. The dependent variable is the total amount of outstanding medical debts in collection, measured in 2017 USD. Columns (1) to (3) use the full sample and sequentially add fixed effects and a binary control for the state-level Medicaid expansion from 2014 onward. Column (4) restricts the sample to never movers and column (5) further restricts it to never mover under the age of 65. Column (6) goes back to the full sample as the starting point and then restricts the treatment group to CZs where the merger-induced HHI increase exceeded 150; column (7) further restricts the treatment group to CZs where the post-merger HHI exceeded 1800 and column (8) increases the post-merger HHI threshold to 2500.

D. Event study regression tables for medical debt, specifications with lagged dependent variable

TABLE D.1 Effect of merger exposure on likelihood of having medical debt (adjusted) in collections (2011-2019), specifications with a lagged dependent variable.

	(1) Has medical debt (adj)	(2) Has medical debt (adj)	(3) Has medical debt (adj)	(4) Has medical debt (adj)	(5) Has medical debt (adj)	(6) Has medical debt (adj)	(7) Has medical debt (adj)	(8) Has medical debt (adj)
	Full sample	Full sample	Full sample	Never movers	Never movers Under 65	HHI change >150	HHI change >150 level>1800	HHI change >150 level>2500
-5	0.0237***	0.0135***	0.0136***	0.0000	0.0000	0.0099*	0.0106*	0.0106**
	(0.0060)	(0.0049)	(0.0048)	(0.0000)	(0.0000)	(0.0052)	(0.0058)	(0.0054)
-4	0.0019	-0.0003	-0.0003	0.0002	-0.0010	-0.0040	-0.0036	-0.0014
	(0.0034)	(0.0041)	(0.0040)	(0.0038)	(0.0044)	(0.0036)	(0.0037)	(0.0032)
-3	0.0023	0.0020	0.0019	0.0024	0.0022	-0.0026	-0.0012	0.0011
	(0.0019)	(0.0021)	(0.0021)	(0.0022)	(0.0025)	(0.0028)	(0.0031)	(0.0039)
-2	0.0005	0.0005	0.0005	0.0006	0.0006	-0.0007	-0.0006	-0.0039
	(0.0013)	(0.0012)	(0.0012)	(0.0012)	(0.0013)	(0.0020)	(0.0024)	(0.0033)
0	0.0029***	0.0027**	0.0025**	0.0022**	0.0032**	-0.0008	-0.0030	-0.0097***
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0013)	(0.0022)	(0.0026)	(0.0036)
1	0.0018	0.0025*	0.0023*	0.0020	0.0032**	-0.0001	-0.0013	-0.0020
	(0.0012)	(0.0013)	(0.0014)	(0.0013)	(0.0016)	(0.0019)	(0.0021)	(0.0022)
2	0.0021	0.0035*	0.0033*	0.0022	0.0039*	0.0039*	0.0032	-0.0029
	(0.0016)	(0.0019)	(0.0019)	(0.0019)	(0.0022)	(0.0024)	(0.0030)	(0.0040)
3	0.0051***	0.0068***	0.0066***	0.0052**	0.0074***	0.0023	0.0003	0.0034
	(0.0018)	(0.0025)	(0.0024)	(0.0024)	(0.0028)	(0.0030)	(0.0039)	(0.0048)
4	0.0051***	0.0087***	0.0085***	0.0069***	0.0095***	0.0030	0.0034	0.0038
	(0.0019)	(0.0027)	(0.0027)	(0.0026)	(0.0031)	(0.0032)	(0.0044)	(0.0059)
5	0.0086***	0.0144***	0.0142***	0.0122***	0.0167***	0.0043	0.0072	0.0006
	(0.0031)	(0.0044)	(0.0044)	(0.0043)	(0.0052)	(0.0050)	(0.0064)	(0.0063)
Observations	35,015,334	34,764,225	34,764,225	27,139,007	20,876,665	15,227,211	14,024,296	12,291,171
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
CZ FEs	Y	Y	Y	Y	Y	Y	Y	Y
Individual FEs	N	Y	Y	Y	Y	Y	Y	Y
Medicaid expansion	N	N	Y	Y	Y	Y	Y	Y

*** p<0.01; ** p<0.05; * p<0.10

Notes: Robust standard errors clustered at the CZ level in parenthesis. The dependent variable is the total amount of outstanding medical debts in collection, measured in 2017 USD. All specifications include a lagged dependent variable as a control. Columns (1) to (3) use the full sample and sequentially add fixed effects and a binary control for the state-level Medicaid expansion from 2014 onward. Column (4) restricts the sample to never movers and column (5) further restricts it to never mover under the age of 65. Column (6) goes back to the full sample as the starting point and then restricts the treatment group to CZs where the merger-induced HHI increase exceeded 150; column (7) further restricts the treatment group to CZs where the post-merger HHI exceeded 1800 and column (8) increases the post-merger HHI threshold to 2500.

TABLE D.2 Effect of merger exposure on the total amount (adjusted) of medical debt in collections (2011-2019), specifications with a lagged dependent variable.

	(1) Total (adj) balance	(2) Total (adj) balance	(3) Total (adj) balance	(4) Total (adj) balance	(5) Total (adj) balance	(6) Total (adj) balance	(7) Total (adj) balance	(8) Total (adj) balance
	Full sample	Full sample	Full sample	Never movers	Never movers Under 65	HHI change >150	HHI change >150 level>1800	HHI change >150 level>2500
-5	120.9**	104.2**	103.7**	0.0	0.0	108.8	113.0	186.3**
	(47.9)	(46.9)	(47.9)	(0.0)	(0.0)	(73.4)	(80.7)	(85.6)
-4	5.4	4.1	3.3	9.9	8.1	-24.5	-22.7	4.7
	(29.6)	(31.1)	(30.9)	(26.7)	(32.3)	(35.4)	(37.1)	(46.5)
-3	-1.0	0.3	0.9	4.0	2.9	-11.7	-2.3	16.1
	(14.2)	(15.3)	(15.1)	(13.5)	(16.4)	(22.8)	(23.3)	(33.6)
-2	2.9	4.9	5.6	6.0	7.0	-7.4	-2.2	-2.0
	(10.1)	(9.8)	(10.0)	(9.0)	(10.8)	(14.7)	(16.4)	(26.2)
0	15.2*	12.2	15.5	9.2	12.9	-4.6	-13.4	-33.0
	(9.2)	(9.1)	(9.6)	(8.7)	(10.6)	(13.6)	(17.8)	(23.5)
1	16.2*	15.6*	18.8**	10.5	14.7	-3.5	-8.8	10.8
	(8.7)	(9.2)	(9.0)	(8.3)	(10.3)	(11.7)	(14.2)	(17.3)
2	25.1*	26.9*	30.4**	17.1	26.2	19.4	19.5	-6.0
	(13.7)	(15.2)	(14.9)	(13.7)	(16.8)	(13.6)	(16.4)	(27.7)
3	39.2***	44.3**	47.8***	32.1*	44.7**	21.8	22.7	58.8*
	(14.5)	(18.1)	(18.0)	(16.5)	(20.3)	(18.3)	(24.5)	(31.5)
4	32.2**	46.9**	50.8**	33.7*	47.1**	28.6	30.4	45.1
	(15.4)	(20.4)	(20.3)	(18.6)	(22.7)	(19.5)	(25.4)	(32.7)
5	63.3***	82.7***	86.8***	64.2***	89.1***	63.4**	68.7**	84.2***
	(20.0)	(27.0)	(26.8)	(24.4)	(29.6)	(25.1)	(33.6)	(31.5)
Observations	35,015,334	34,764,225	34,764,225	27,139,007	20,876,665	15,227,211	14,024,296	12,291,171
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
CZ FEs	Y	Y	Y	Y	Y	Y	Y	Y
Individual FEs	N	Y	Y	Y	Y	Y	Y	Y
Medicaid expansion	N	N	Y	Y	Y	Y	Y	Y

*** p<0.01; ** p<0.05; * p<0.10

Notes: Robust standard errors clustered at the CZ level in parenthesis. The dependent variable is the total amount of outstanding medical debts in collection, measured in 2017 USD. All specifications include a lagged dependent variable as a control. Columns (1) to (3) use the full sample and sequentially add fixed effects and a binary control for the state-level Medicaid expansion from 2014 onward. Column (4) restricts the sample to never movers and column (5) further restricts it to never mover under the age of 65. Column (6) goes back to the full sample as the starting point and then restricts the treatment group to CZs where the merger-induced HHI increase exceeded 150; column (7) further restricts the treatment group to CZs where the post-merger HHI exceeded 1800 and column (8) increases the post-merger HHI threshold to 2500.

TABLE D.3 Effect of merger exposure on new medical debt in collections (adjusted) created over the previous 12 months (2011-2019), specifications with a lagged dependent variable.

	(1) New med debt last 12m adj) Full sample	(2) New med debt last 12m adj Full sample	(3) New med debt last 12m adj Full sample	(4) New med debt last 12m adj Never movers	(5) New med debt last 12m adj Never movers Under 65	(6) New med debt last 12m adj HHI change >150	(7) New med debt last 12m adj HHI change >150 level>1800	(8) New med debt last 12m adj HHI change >150 level>2500
-5	103.7***	40.6	40.0	0.0	0.0	74.6	69.9	120.0***
	(28.1)	(25.5)	(26.3)	(0.0)	(0.0)	(46.7)	(51.2)	(32.5)
-4	-3.7	-8.9	-10.0	-7.6	-12.3	-19.8	-9.8	3.4
	(13.4)	(15.7)	(15.4)	(13.1)	(14.9)	(22.7)	(19.6)	(16.3)
-3	-3.4	-4.5	-3.6	-2.6	-3.7	-7.7	-2.3	1.1
	(6.2)	(7.4)	(7.1)	(6.2)	(7.6)	(12.1)	(11.4)	(9.3)
-2	-5.3	-5.2	-4.2	-4.5	-4.9	-5.3	-1.0	-2.2
	(5.1)	(5.2)	(5.4)	(4.8)	(5.9)	(7.2)	(8.3)	(9.4)
0	2.2	3.3	8.0	3.5	5.7	-5.3	-6.5	1.4
	(5.3)	(5.0)	(5.0)	(4.5)	(5.6)	(6.2)	(7.4)	(13.1)
1	-2.8	-0.3	4.3	-2.6	-1.9	-5.2	-4.9	17.9
	(7.3)	(8.3)	(7.7)	(7.1)	(8.7)	(9.6)	(11.2)	(16.4)
2	6.2	8.9	13.8	5.8	9.9	4.8	9.4	26.2
	(7.2)	(9.2)	(8.9)	(8.1)	(10.0)	(10.1)	(12.9)	(20.1)
3	4.9	10.8	15.9	6.3	10.5	-4.0	4.7	39.1*
	(8.6)	(11.3)	(10.9)	(10.0)	(12.4)	(12.4)	(16.5)	(22.4)
4	5.0	12.6	18.3	6.9	10.6	9.7	16.9	55.9**
	(9.5)	(12.5)	(12.1)	(11.0)	(13.7)	(15.5)	(19.5)	(26.0)
5	15.7	24.4	30.2*	18.3	26.7	36.1*	50.8**	90.1**
	(13.1)	(16.8)	(16.6)	(14.9)	(18.2)	(20.7)	(23.3)	(42.8)
Observations	35,015,334	34,764,225	34,764,225	27,139,007	20,876,665	15,227,211	14,024,296	12,291,171
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
CZ FEs	Y	Y	Y	Y	Y	Y	Y	Y
Individual FEs	N	Y	Y	Y	Y	Y	Y	Y
Medicaid expansion	N	N	Y	Y	Y	Y	Y	Y

*** p<0.01; ** p<0.05; * p<0.10

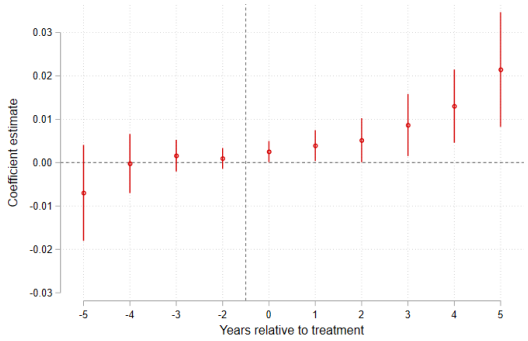
Notes: Robust standard errors clustered at the CZ level in parenthesis. The dependent variable is the total amount of outstanding medical debts in collection, measured in 2017 USD. All specifications include a lagged dependent variable as a control. Columns (1) to (3) use the full sample and sequentially add fixed effects and a binary control for the state-level Medicaid expansion from 2014 onward. Column (4) restricts the sample to never movers and column (5) further restricts it to never mover under the age of 65. Column (6) goes back to the full sample as the starting point and then restricts the treatment group to CZs where the merger-induced HHI increase exceeded 150; column (7) further restricts the treatment group to CZs where the post-merger HHI exceeded 1800 and column (8) increases the post-merger HHI threshold to 2500.

E. Heterogeneity dimensions

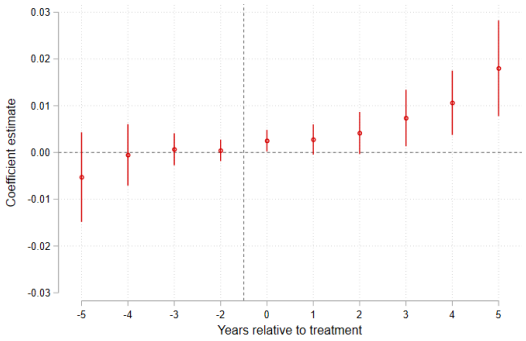
E.1 Gender

FIGURE E.1: Event study plots for the effect of hospital merger exposure on medical debt, gender subsamples.

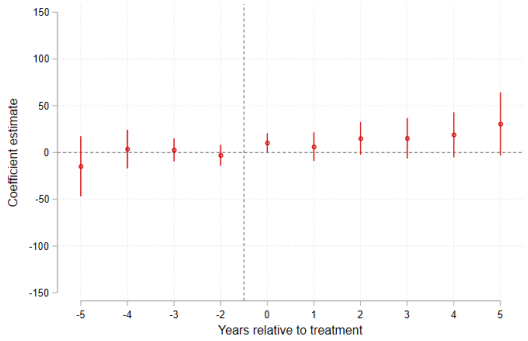
(A) Likelihood of having medical debt: female



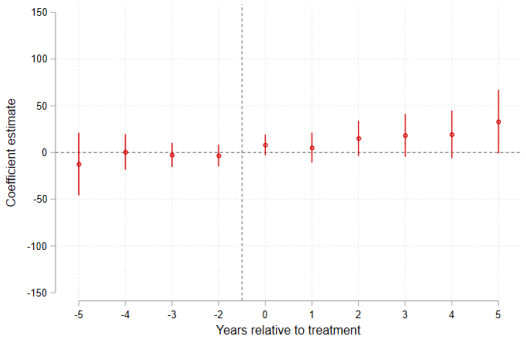
(B) Likelihood of having medical debt: male



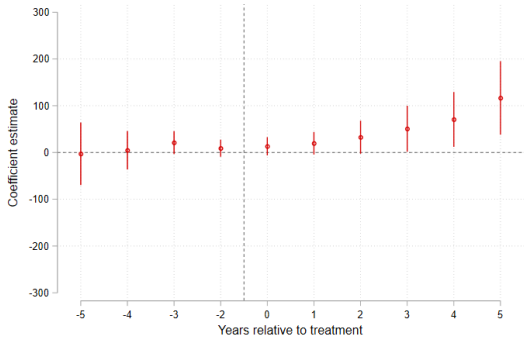
(C) New medical debt (last 12 months): female



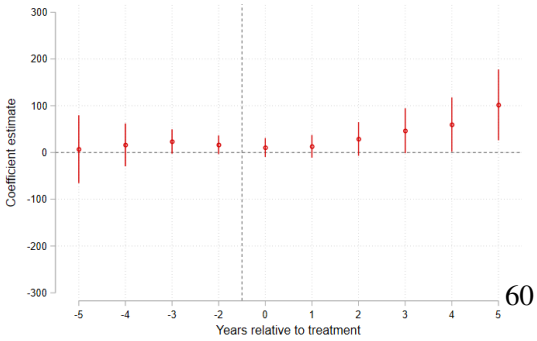
(D) New medical debt (last 12 months): male



(E) Total medical debt in collections: female



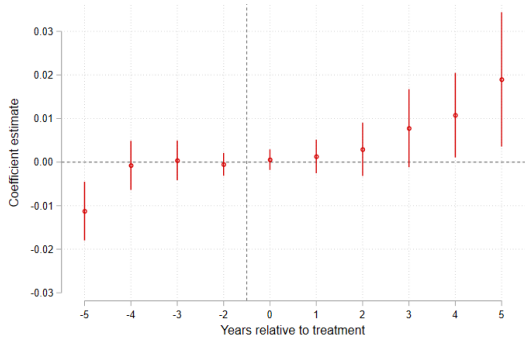
(F) Total medical debt in collections: male



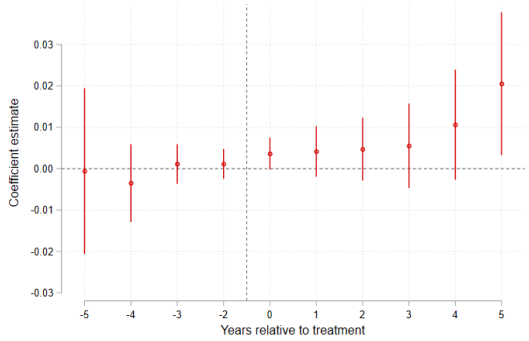
E.2 Medicaid expansion states

FIGURE E.2: Event study plots for the effect of hospital merger exposure on medical debt, subsample defined based on whether states expanded Medicaid coverage between 2014-2019.

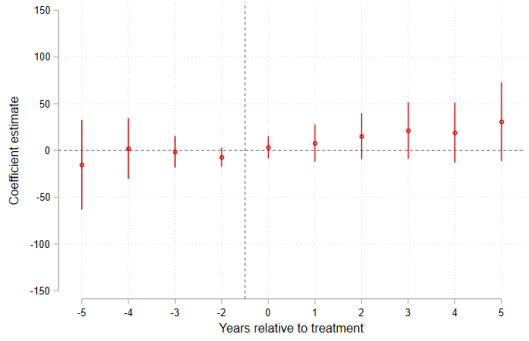
(A) Likelihood of having medical debt: expansion states



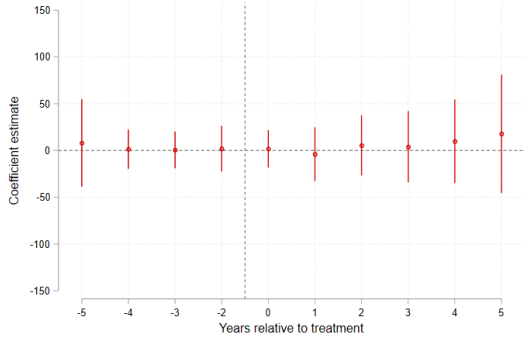
(B) Likelihood of having medical debt: non-expansion states



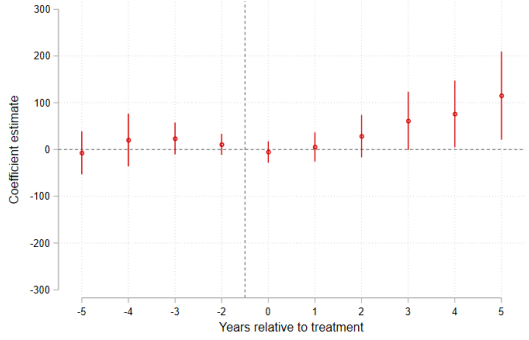
(C) New medical debt (last 12 months): expansion states



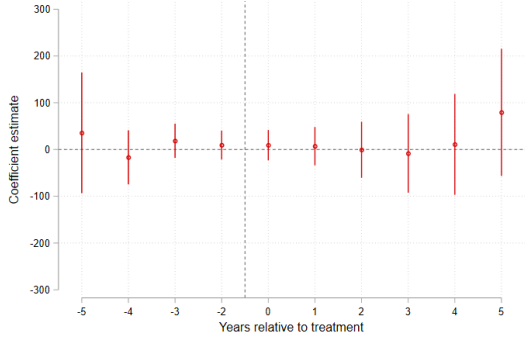
(D) New medical debt (last 12 months): non-expansion states



(E) Total medical debt in collections: expansion states



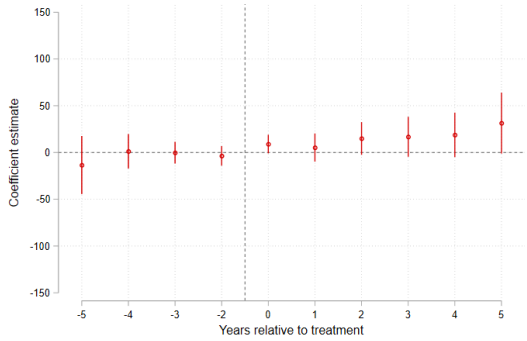
(F) Total medical debt in collections: non-expansion states



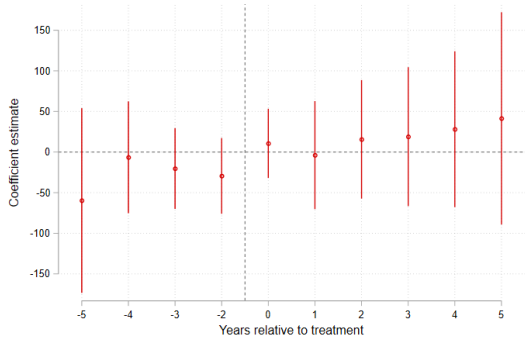
E.3 Subset with medical debt in collections in 2013

FIGURE E.3: Event study plots for the effect of hospital merger exposure on medical debt, subsample with medical debt in collections in 2013.

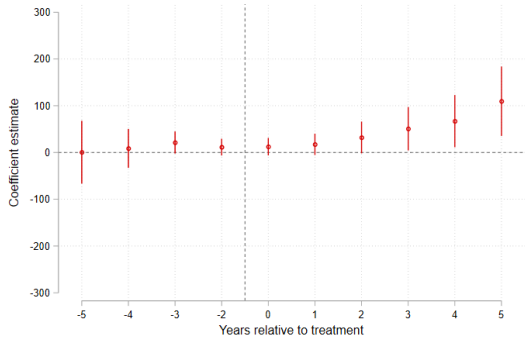
(A) New medical debt (last 12 months): baseline



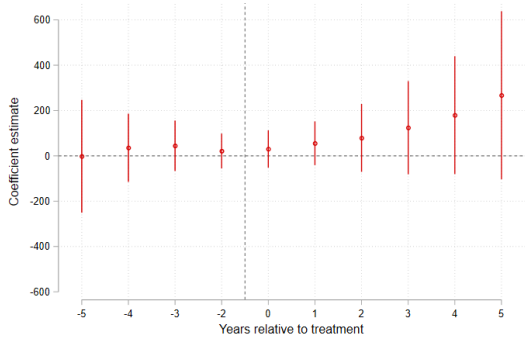
(B) New medical debt (last 12 months): subsample with debt in 2013



(C) Total medical debt in collections: baseline



(D) Total medical debt in collections: subsample with debt in 2013

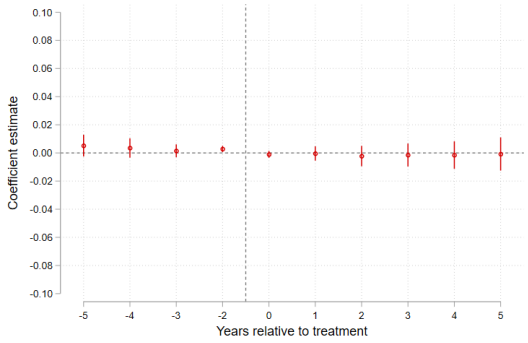


F. Potential mechanisms

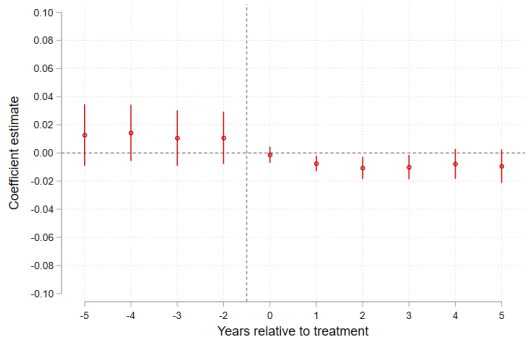
F.1 Labor market channels

FIGURE F.1: Event study plots for the effect of hospital merger exposure on earnings and employment, by sector

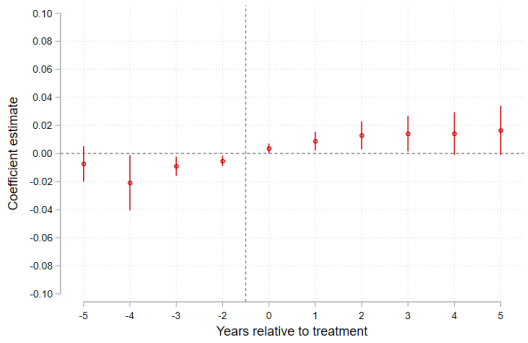
(A) Average earnings: All Sectors



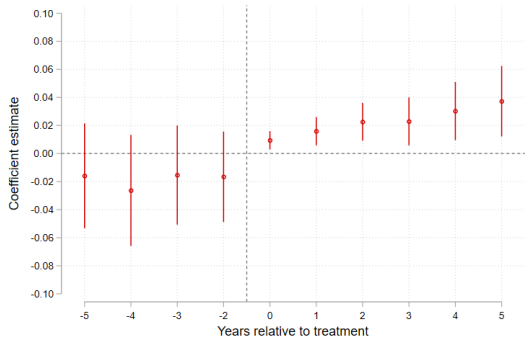
(B) Average earnings: Health Care and Social Assistance



(C) Employment: All Sectors



(D) Employment: Health Care and Social Assistance



(E) Employment (FTE): Hospitals

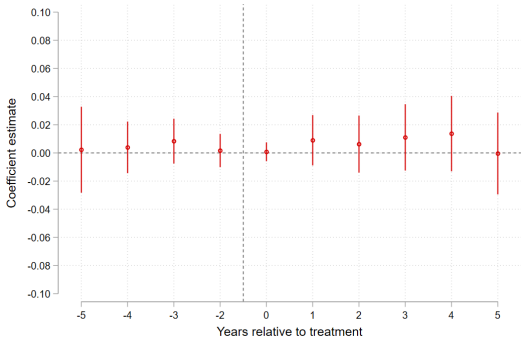
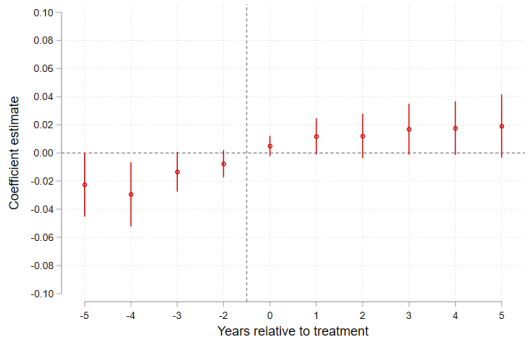
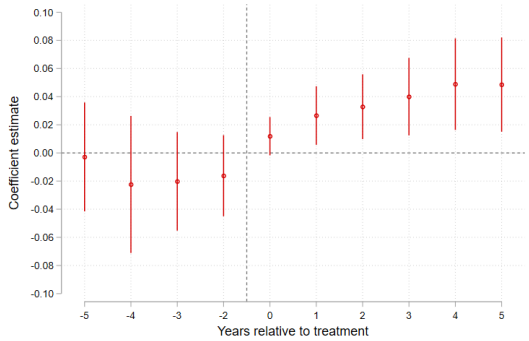


FIGURE F.2: Event study plots for the effect of hospital merger exposure on new hires, separations, and labor market tightness, by sector

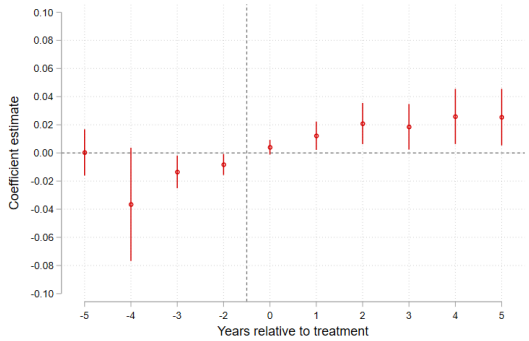
(A) Hires:
All Sectors



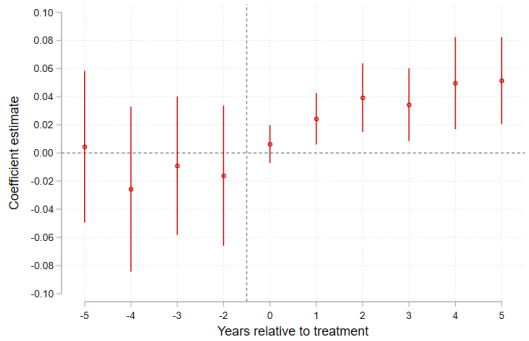
(B) Hires:
Health Care and Social Assistance



(C) Separations:
All Sectors



(D) Separations:
Health Care and Social Assistance



Note: Estimated using data from the QWI. The panels show the event study plots for the effect of hospital mergers on the log of (A) hires in all sectors across the CZ; (B) hires in the Health Care and Social Assistance sector; (C) separations in all sectors across the CZ; and (D) separations in the Health Care and Social Assistance sector.