Administrative Fragmentation in Health Care*

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September 23, 2025

Abstract

This paper examines the impact of reducing one commonly cited source of inefficiency in US health care: administrative fragmentation, or the lack of standardization in billing and payment processes. We study a Medicare reform that consolidated administrative processes across service types, using its staggered rollout and hospitals' prior levels of administrative fragmentation for identification. The reform dramatically reduced fragmentation and modestly lowered claim denial rates, but had no effect on spending, post-discharge care, or rehospitalizations. It also did not affect administrative costs or technology adoption. These findings suggest that addressing administrative fragmentation alone is unlikely to significantly improve health care efficiency.

*We thank Zarek Brot, Joshua Gottlieb, Gina Li, Julian Reif, and Mark Shepard for helpful comments and feedback. The authors gratefully acknowledge support from the National Institute on Aging of the National Institutes of Health (award no. T32-AG000186).

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1 Introduction

A defining feature of the US health care system is its fragmentation, stemming in large part from the patchwork of public and private payers that provide insurance coverage. In contrast to most other countries, where a single public payer typically dominates, the US health care market is unique in the extent of its fragmentation. In 2022, there were over 1,100 private health insurers operating in the US (NAIC, 2023). Even public payers have become more fragmented over time as they increasingly move toward privately-run managed care: the average Medicare beneficiary can choose among 8 firms offering Medicare Advantage coverage (MedPAC, 2024; Zhu et al., 2025). As a result of this payer fragmentation, providers must maintain relationships with multiple payers across several plan contracts, as their patient populations are usually spread across different insurers.[1] Patients likewise face similar challenges: 13 percent of Americans and 52 percent of those over 65 have two or more sources of health insurance (Mykyta et al., 2023).

One important consequence of payer fragmentation is that it produces "administrative fragmentation": the lack of standardization in the administrative processes used to handle billing and paperwork. Rather than using a common clearinghouse to process bills, a shared platform to handle patient and provider communication, or standardized forms to file paperwork, each payer typically uses its own systems to handle these administrative tasks.[2] Critics often cite this particular facet of fragmentation as a source of inefficiency in the US health care system. (Cutler, 2020; Sahni et al., 2021a,b).

The prevalence of administrative fragmentation raises two main concerns. First, the lack of coordination in administrative processes could impose unnecessary burdens on patients and providers. US physicians spend much more time on administrative tasks—up to a quarter of their working hours—than those in comparable countries (Cutler and Ly, 2011; Woolhandler and Himmelstein, 2014; Rao et al., 2017), while 26 percent of patients report dealing with a billing issue in the past year (Kyle and Frakt, 2021). Indeed, one argument often made in favor of moving to a single-payer system is the administrative cost savings from

[1] A 2025 industry survey found that surveyed provider practices reported having on average 20 plan contracts (Council for Affordable Quality Healthcare, 2025), while a study of a large hospital chain found that each of its hospitals contracted on average with 33 payers and negotiated 52 different contracts (Henderson and Mouslim, 2024).

[2] Payer fragmentation often entails much more fragmentation that just the administrative fragmentation we study. For example, in addition to varying administratively, payers generally vary in the providers included in their network, the prices they pay these providers, the cost sharing structures they offer, and the services and illnesses they cover. Fragmentation in these other payer characteristics can also potentially impose costs on providers and patients, but this product differentiation may also be very valuable (Dafny et al., 2013; Marone and Sabety, 2022). By contrast, the administrative fragmentation that we study is often thought to have no offsetting benefits.

eliminating fragmentation. As noted by the Congressional Budget Office, "the administrative costs of private insurance stem in part from its fragmented nature, with administrative details varying among employers and providers... Under a single-payer system, costs stemming from those factors would not occur" (U.S. Congressional Budget Office, 2020).

Second, the hassles generated by administrative fragmentation could directly distort provider behavior and affect patient care. Inconsistencies in administrative processes across payers may cause unwarranted delays or denials, as patients and providers are tasked with identifying missing documentation and sorting out inconsistencies in coverage. If these hassles are substantial, then providers may provide an insufficient amount of care or even refuse to see certain patients altogether (Li, 2023; Dunn et al., 2024), and patients may likewise delay or forgo care (Kyle and Frakt, 2021). Unlike utilization management tools—administrative hassles designed to target wasteful care—the hassles arising from administrative fragmentation are untargeted and affect both low- and high-value care alike (Brot-Goldberg et al., 2023; Gandhi and Shi, 2025). Thus, any distortions to care that arise from administrative fragmentation are a particular cause for concern.

These concerns have motivated recent policy efforts to streamline and standardize administrative processes. In addition to proposals to switch to a single payer system, there have been proposals for improvements within a multi-payer system like aligning administrative processes for the Medicare and Medicaid programs, harmonizing quality measure reporting across private and public insurers,[3] and establishing a national clearinghouse for bill submission (Cutler, 2020). These reforms aim to retain the benefits of consumer choice across differentiated plans that can arise in a multi-payer system (Dafny et al., 2013; Marone and Sabety, 2022), while still addressing the inefficiencies associated with administrative fragmentation.

However, it is unclear to what extent reducing administrative fragmentation would reduce costs or correct distortions in care provision. Isolating the causal effect of administrative fragmentation in a multi-payer system is empirically challenging, as individual payers differ along many dimensions besides their administrative processes—for example, in their payment rates and benefits structures. So comparing patients or providers who interact with different numbers of payers risks conflating the effects of administrative fragmentation with the effect of differences in these other, non-administrative payer and plan characteristics.[4]

[3] For example, CMS is testing "Financial Alignment Initiative" models for Medicare-Medicaid enrollees to coordinate benefits and services across the two programs (link, last accessed 12/9/24). CMS and America's Health Insurance Plans, an industry group for private insurers, have streamlined quality reporting through the Core Quality Measure Collaborative (link, last accessed 12/9/24)

[4] For example, in the case of patients who are dually eligible for Medicaid and Medicare, it would be difficult to distinguish administrative fragmentation from the effects of Medicaid's lower payment rates or differences in the services covered by each payer.

We address this challenge by leveraging a large Medicare policy reform as a natural experiment that reduced administrative fragmentation by consolidating billing processes, but held other key payer characteristics—payment rates, patient enrollment, and insurance generosity—fixed. We focus in particular on Medicare contractor firms, which are privately-owned companies that administer traditional Medicare on a regional or service category-level basis (League, 2023). While all contractors must honor the same nationally-set payment rates and broad coverage requirements, much of providers' day-to-day administrative experiences with Medicare are shaped by the contractor they are interacting with. The contractors handle all billing-related interactions between providers, patients, and the Medicare program: they process claims, make payments, request and review documentation, issue coverage determinations and denials, handle denial appeals, and answer provider inquiries. Medicare gives contractors considerable discretion in handling administrative processes. For example, during our study period, each firm operated independent data processing centers, used different accounting standards, maintained distinct claim submission portals, and managed separate provider support services to respond to inquiries (Centers for Medicare & Medicaid Services, 2019, 2006; Novitas Solutions, 2024, 2025; Noridian Healthcare Solutions, 2024; Palmetto GBA, 2024, 2025).

As part of the Medicare Modernization Act of 2003, Medicare transitioned from having separate contracts within a geographic jurisdiction for administering Medicare Parts A and B to having a single contract to administer both. For individuals insured by traditional Medicare, Part A covers inpatient and post-acute care, while Part B covers physician and other outpatient services. Prior to the reform, the contractor firm handling Part A administration was chosen separately by each hospital, while Part B contracts were assigned to firms by geographic jurisdiction. After the reform, a single firm was contracted to process *both* Part A and B claims across an entire jurisdiction. Thus, patients of hospitals that initially had a different Part A contractor than their jurisdiction's Part B contractor went from having two separate firms processing their inpatient and outpatient claims to having a single firm to process both. The goal of the reform was to address concerns that "these multiple interfaces with Medicare increase the frustration for beneficiaries and providers by making it difficult to get answers on coverage questions quickly. . . [and] providers also face increased expenses due to separate processing, and have less ability to freely understand and coordinate, where appropriate, services on behalf of their patients" (Leavitt, 2005).

To understand the effect of this reform, we focus on Medicare patients who were recently discharged from an inpatient hospital stay, as these patients and their providers must navigate the transition of care from an inpatient to an outpatient setting. Prior to the reform, since coverage for outpatient care can depend on the details of the patient's inpatient stay, outpatient providers had to coordinate with the Part A contractor to ensure their claims had supporting records in order to be approved by the Part B contractor. After the reform, this process became streamlined as both inpatient and outpatient services were billed and processed by the same contractor. The reform thus captures many of the goals of reducing administrative fragmentation: eliminating redundant administrative processes, minimizing inconsistencies in coverage decisions, and reducing provider and patient hassle.[5]

Our empirical strategy exploits the staggered adoption of the reform across contracting jurisdictions as well as within-jurisdiction, across-hospital differences in the implied effect of the reform on administrative fragmentation for these patients. The reform was implemented in different jurisdictions at different times from 2006 through 2013, giving rise to geographic variation in exposure to the reform over time. Furthermore, within a jurisdiction, the implied effect on administrative fragmentation could also vary across hospitals: because the reform consolidated the Part A and B contracts, it only reduced fragmentation for patients in hospitals where their Part A billing was originally handled by a different contractor than the jurisdiction's Part B contractor. This across-hospital comparison allows us to isolate the causal effect of administrative fragmentation in particular from any other effects of the reform.

We use a triple difference-in-differences study design to compare hospitals over time and by pre-reform administrative fragmentation. We first quantify the direct effect of the reform on administrative fragmentation: a hospital-level indicator for whether the Part A and Part B contracts are held by the same firm. We find a strong "first stage" effect of 0.73, indicating that 73 percent of hospitals slated to have their Part A and B contracts combined ended up doing so. This is less than 1 because some hospitals—most often those that were part of cross-jurisdiction chains—were able to delay transitioning to new combined contract. We also confirm that the reform reduced administrative fragmentation using a patient-level, claims-based measure: whether the patient's claims during and after their inpatient stay were processed by more than one contractor. We find that the reform dramatically reduced realized administrative fragmentation, as after the reform this likelihood is nearly halved.

We next consider the effects on claims-based measures of administrative efficiency: denials and delays. Looking at all Medicare claims within 30 days of discharge, we look at the effects on claim denials and processing delays. We find some evidence the reform reduces denials, particularly for outpatient physician services claims. However, the magnitude of the effect

[5] This setup allows us to study the effects of administrative fragmentation *within* a single individual enrollee. This is a particularly severe form of administrative fragmentation, though it is common in its own right (Mykyta et al., 2023). However, our findings also speak to the issue of *across*-patient administrative fragmentation, since both within- and across-patient fragmentation are driven by the same underlying issue: the fact that providers have to interact administratively with multiple payers.

is fairly modest: the reform only reduced denials by 2.3 percent relative to baseline. A back-of-the-envelope estimate indicates that in total, the administrative cost savings implied by this reduction in denials amounts to only 33 cents per admission. Furthermore, we find a precisely estimated null effect on bill processing time as measured by the days between the procedure being performed and when the bill is paid.

We then explore the effects on direct measures of provider administrative costs. We find no evidence that hospital-reported administrative costs changed as a result of the reform. We also consider the take-up of billing software: providers that expect to face a less complex billing process may less rapidly take up this software or let contracts expire. We find no evidence that reducing administrative fragmentation reduces software take-up among either hospitals or office-based physicians.

Turning to the effects on care utilization and patient health, across a wide range of outcomes we find precise null effects: reducing administrative fragmentation does not affect the quantity or type of care patients receive after discharge. We look across several post-discharge settings and find no evidence of changes in the likelihood or amount of physician services, nursing facility, home health, hospital outpatient, or durable medical equipment spending. There are also no meaningful changes in patient health, as measured by 30-day readmissions. Overall, the null effect on administrative costs and utilization is likely attributable to the relatively small improvements in administrative efficiency that we find — while the reform successfully streamlined billing processes, it had limited impact on realized measures of efficiency like claim denials and billing delays. Our results imply that efforts targeted at reducing administrative fragmentation in particular may have limited effect in improving efficiency. Furthermore, they suggest that inconsistent administrative processes across payers is not a key source of distortions within the US health care system.

To test the robustness of our results, we consider several alternative specifications: we add jurisdiction-by-month fixed effects to exploit variation only across hospitals within the same jurisdiction, expand the event study window, and use a local projections difference-in-difference approach to address concerns arising from the staggered timing approach (Dube et al., 2023). We also investigate heterogeneity in the effects by focusing on types of hospital stays that have relatively high post-discharge spending, cross-provider coordination, or denials, finding no evidence of changes in outcomes even among these patients.

Our findings contribute to the literature on how and why American health care has uniquely high spending (Garber and Skinner, 2008; Cutler, 2018). We investigate one factor that has been cited for US health care's productive inefficiency: its fragmentation across payers. We focus on one particular purported driver of waste resulting from payer fragmentation: *administrative* fragmentation. In contrast to work which has found that provider behavior is highly sensitive to increases in administrative hurdles (Brot-Goldberg et al., 2023; Eliason et al., 2021; League, 2023; Dunn et al., 2024; Gandhi and Shi, 2025; Shi, 2024), we find that reducing the *fragmentation* of these administrative processes has no meaningful effect on costs or provider behavior. We also contribute to the literature on payer fragmentation at the patient-level: patients covered simultaneously by multiple payers. This literature has looked at private supplemental coverage (Cutler, 2003; Buchmueller et al., 2004; Fang et al., 2008; Cabral and Mahoney, 2019) as well as patients who are dually eligible for Medicare and Medicaid (McInerney et al., 2017; Carey et al., 2020; Cabral et al., 2021; Li, 2023), but has been primarily focused on how changes in coverage or incentives affect patient and provider behavior. By contrast, we are interested in isolating the effect of the administrative complexity associated with being covered by multiple payers, holding fixed the characteristics of the coverage itself. Finally, our work contributes to the literature on the effects of fragmentation in the health care system more generally, which has mostly focused on fragmentation in care across different *providers* (Nyweide et al., 2013; Frandsen et al., 2015; Agha et al., 2019). By focusing on fragmentation on the *payer* side, we are studying a prevalent but under-studied dimension of fragmentation within the US health care system.

2 Background

2.1 Medicare Contractors

Though traditional Medicare is a centralized federally-run insurance program, in practice it is administered by a series of private contractor firms across different geographic regions and service types.[6] While the government sets prices, determines the vast majority of coverage policies, and bears all actuarial risk, contractors handle Medicare's day-to-day administrative operations. The administrative tasks performed by these contractors include processing medical claims and prior authorization requests, determining the conditions under which Medicare will reimburse providers for various health care services, and educating providers about these billing rules (Centers for Medicare & Medicaid Services, 2022).

These firms have wide discretion over how to implement the broad statutory standards they must follow (MedPAC, 2018; Levinson, 2014). Reflecting this discretion, an Office of the Inspector General (OIG) report found substantial variation in coverage rules: 59 percent of procedures had contractor-specific billing rules, but only 24 percent had billing rules in *all* jurisdictions (Levinson, 2014). Furthermore, different contractors have historically oper-

[6]Note that this discussion and our empirical analysis exclude Medicare Advantage, the more privatized alternative to traditional Medicare, because the contractors discussed here do not process Medicare Advantage claims.

ated independent data processing centers (Centers for Medicare & Medicaid Services, 2019) and used different accounting standards (Centers for Medicare & Medicaid Services, 2006), and currently use claim submission portals with varying functionalities and manage different communication channels for provider inquiries (Novitas Solutions, 2024, 2025; Noridian Healthcare Solutions, 2024; Palmetto GBA, 2024, 2025). This variation in the interpretation of coverage rules and administrative infrastructure can result in large differences in the experience providers have with the Medicare program; for example, League (2023) finds dramatic differences in the likelihood claims are denied across contractors.

2.2 Medicare Contractor Reform

When Medicare was first introduced in the 1960s, Medicare contracted out the Part A (i.e., inpatient) and Part B (i.e., outpatient) administrative services separately. Firms contracted to process Part A claims were called "fiscal intermediaries" while those processing Part B claims were called "carriers." Carriers were contracted to provide administrative services for distinct regional jurisdictions determined by CMS,[7] while fiscal intermediaries were nominated by provider groups and approved by the Secretary of Health and Human Services with each provider able to use any of the approved fiscal intermediaries.[8] These separate contracts for processing Part A and B claims meant that providers and patients could have two different firms processing their Part A and Part B claims.

The Medicare Modernization Act of 2003 addressed concerns with this arrangement by instead implementing a single contract per region to handle both Part A and B claims. These newly created contractors were called "Medicare Administrative Contractors" (MACs) and would serve as a single point of contact for providers, with the reform meant to streamline the coordination of claims across Medicare Parts A and B as well as across providers.[9] Policymakers expected the reform to greatly reduce administrative hassles, predicting "less [administrative] staff time for providers as a result of interacting with fewer contractors" (Leavitt, 2005), "improved provider education and training" on billing rules (Lathroum, 2006), and increased coordination from contractor assistance "with obtaining information on behalf of patients about items or services received from another provider" (Chicoine, 2006). This optimism is easy to understand given the pre-reform state of Medicare claims processing: an OIG report from the time says, "The current... environment features procedures that have

[7]These jurisdictions had not been changed since the inception of Medicare, when they were primarily determined by existing health insurers' ability to quickly implement the then-new program (Mennemeyer, 1984).

[8]See Section 1816 of the Social Security Act.

[9]For simplicity, we will refer to fiscal intermediaries, carriers and MACs as "contractors" or "contracting firms."

different claims for the same patient being processed by separate contractors and antiquated IT systems that are unable to easily show the complete care received" (Leavitt, 2005).

The reform was implemented in a staggered fashion across contracting jurisdictions. Appendix Figure A1 panel (a) shows the primary contractor firm processing Part A claims (i.e., the firm holding the contracts with the most hospitals) in each jurisdiction in 2004 before the reform was rolled out, and panel (b) shows the contractor holding each jurisdiction's Part B contract at the same time. In jurisdictions where these were originally the same firm, the plurality of hospitals—though not all—would *not* have faced a reduction in administrative fragmentation as a result of the reform. But even within these regions, some hospitals would face a reduction if their Part A contractor did not originally match the jurisdiction's Part B contractor. This is because in the pre-reform period, hospitals could select any contractor to process their Part A claims.[10] Panel (c) shows the planned final contracting jurisdictions after the reform was implemented.[11]

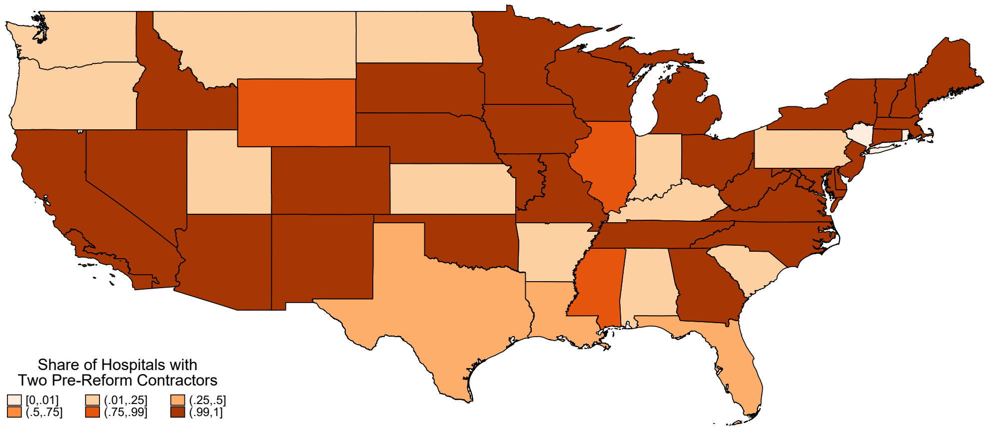
While all hospitals were eventually subject to the reform, they varied along two dimensions: the timing of their exposure to the reform and the implied effect on administrative fragmentation based on their pre-reform contractor. Figure 1a plots the share of hospitals in a state that expected to see a reduction in administrative fragmentation due to the transition—in other words, the share of hospitals with a different Part A contractor than their jurisdictions' Part B contractor in the pre-reform period. The effect of the reform was fairly evenly geographically distributed. Figure 1b illustrates the geographic distribution of the timing of the reform. Given that it was implemented at the jurisdiction-level, clusters of states were treated together in a given year; however, there does not seem to be an obvious geographic pattern in the timing of the reform across jurisdictions.

[10] After the reform, providers cannot generally make such a choice, with the Part A contractor being determined geographically just as the Part B contractor is. The main exception is for providers that are part of a chain that spans the jurisdictions of multiple contractors. These chain providers may request all of their claims be processed by the contractor covering their home office (Centers for Medicare & Medicaid Services, 2024b). Furthermore, after the reform, there remained so-called "out-of-jurisdiction" providers (OJPs) that would continue to have their claims processed by their incumbent contractor "until CMS undertakes the final reassignment of all OJPs to their destination MACs based on the geographic assignment rule and its exceptions" (Centers for Medicare & Medicaid Services, 2024b). As of 2024, CMS still "has not set a timetable for moving OJP's" (Centers for Medicare & Medicaid Services, 2024b).

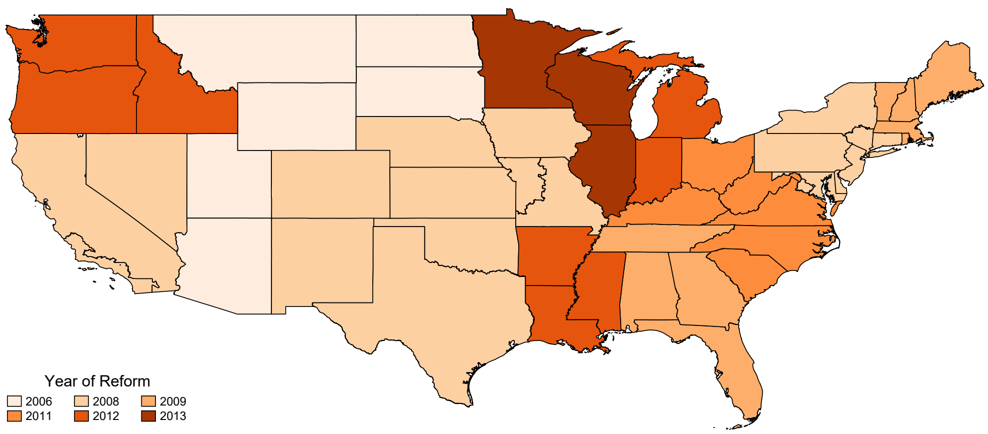
[11] Ultimately, the contracting jurisdictions were not consolidated as much as planned due to "the impact that further consolidations may have on competition among the MACs" (Centers for Medicare & Medicaid Services, 2014).

Figure 1. Map of Incidence of the Reform

(a) Share of Hospitals Facing Reduced Administrative Fragmentation



(b) Year of Reform



Notes: Panel (a) plots the share of hospitals in each state with two contractors in each jurisdiction's pre-treatment period, meaning the Part A contractor processing most of the claims for the hospital is not the same firm as the Part B contractor assigned to that jurisdiction. Panel (b) plots year the transition to MACs (i.e., a combined Part A and B contractor) was implemented in each jurisdiction.

3 Data and Sample Construction

Our main source of data is the Medicare claims data covering inpatient stays and skilled nursing stays (MedPAR), outpatient care, carrier (i.e, in-office physician care) claims, home health agency and hospice services, and durable medical equipment supplies from 2000 to 2017 (Centers for Medicare & Medicaid Services, 2000–2017b). These data include the dates of service, bill processing date, diagnosis codes, procedure codes, prices, a provider identifier, a contractor (fiscal intermediary, carrier, or MAC) identifier, as well as an indicator for whether a claim was denied.

We restrict our analyses to traditional Medicare patients discharged after an index hospital admission, which we define as an inpatient stay without an admission in the prior 30 days. We use index admissions instead of all admissions in order to use the 30-day rehospitalization rate as an outcome variable. After making this restriction, we construct several outcomes within 30 days of discharge from the index admission: denials, bill processing time, utilization, spending, the number of unique providers seen, and the number of unique contractors. We construct these measures both by claim type and in aggregate. For analyses that rely on the Carrier file (including admission-level totals), we restrict our analysis to the 20% sample of beneficiaries, while analyses that do not require the Carrier file (such as spending for other categories) use the 100% sample. We also limit our sample to admissions to active, non-critical access, short-stay hospitals for which we are able to determine the pre-reform Part A contractor.[12] We analyze admissions from 2000 through 2017.

A hospital's reform timing is assigned at the jurisdiction-month level and their treatment status—whether they were slated to face a reduction from two contractors to one as a result of the reform—is assigned at the hospital-level. To assign reform timing, we use the jurisdiction-month crosswalk derived from League (2023), which uses the contractor reported in the Carrier claims along with Medicare contract information to construct a crosswalk between geographic areas and Part B contractors. Then we assign treatment status based on the modal Part A contractor processing inpatient claims in each hospital month:[13] treated hospitals are defined as those with a different Part A contractor than their jurisdiction's Part B contractor 12-24 months prior to the reform, while control hospitals are those who had matching contractors in that period.

Figure 2a plots jurisdiction-level reform implementation status over time. No jurisdictions were subject to the reform prior to 2006, and the staggered roll-out not completed until 2014. Figure 2b plots the share of hospitals where the hospital's Part A contractor matches the region's Part B contractor. Note that at baseline this is not zero, which is consistent with the variation depicted in Figure 1 panel (a): prior to the reform, the contractors matched for about 27 percent of hospitals. This rises steadily as the reform is being rolled out and plateaus at about 86 percent. The remaining hospitals whose Part A contractors do not match the region's Part B are ones that have individually opted to remain with their legacy Part A contractor, or that are part of a cross-jurisdiction chain and have chosen to have their claims processed by the Part A contractor covering their home office.

[12]Hospital characteristics are determined using Provider of Services (POS) data derived from Medicare certification applications (Centers for Medicare & Medicaid Services, 2000–2017c). We are unable to determine the pre-reform Part A contractor for hospitals that exit before or enter after the reform, or had too few admissions to determine their contractor.

[13]We use all inpatient claims in MedPAR to assign contractors, rather than limiting our analysis to index admissions.

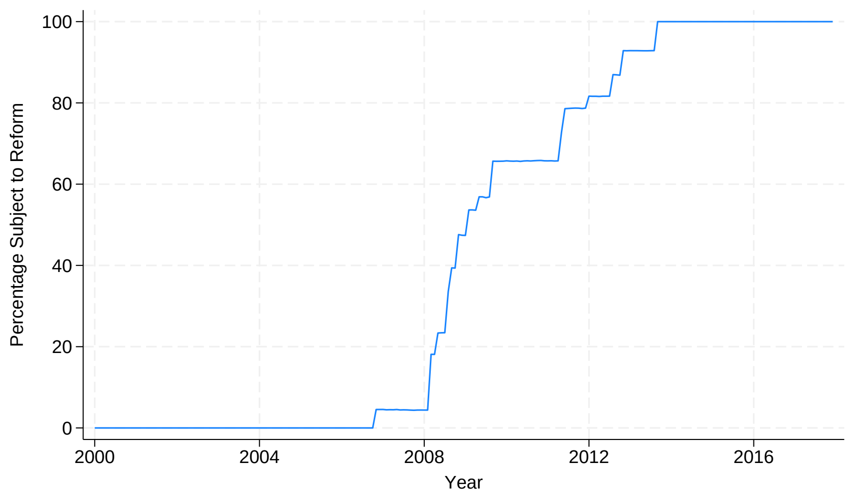
We also use hospital-year level data on administrative spending from health care Provider Cost Reporting Information System (HCRIS) from fiscal years 2000-2017 as well data on IT take-up from the Health Care Information and Management Systems Society (HIMSS) database for 2002–2017 (Centers for Medicare & Medicaid Services, 2000–2017a; Health Care Information and Management Systems Society, 2002–2017). We supplement this hospital-level data with 2010–2015 state-level data on the share of office-based physicians using electronic health records (EHR) from the Office of the National Coordinator for Health Information Technology (Office of the National Coordinator for Health IT, 2008–2019).

Table 1 presents baseline summary statistics for hospitals who had different Part A and B contractors prior to their jurisdiction going through the reform ("Reduced Fragmentation") and hospitals whose Part A contractor already matched the jurisdiction's Part B contractor ("No Effect on Fragmentation"). While treatment hospitals are smaller and more likely to be for-profit, their patients are similar in terms of demographics, spending, number of providers encountered after a hospitalization, and readmission rate. However, one notable difference across these two patient groups is in the likelihood that they encounter multiple contractors within 30 days of their admission – that is, whether they see a contractor that differs from the one processing their inpatient stay. At baseline, 92 percent of patients in treated hospitals interact with at least two contractors during and after their stay, while only 52 percent of those in control hospitals do.[14] This confirms that patients admitted to treatment and control hospitals faced very different levels of administrative fragmentation in the pre-reform period.

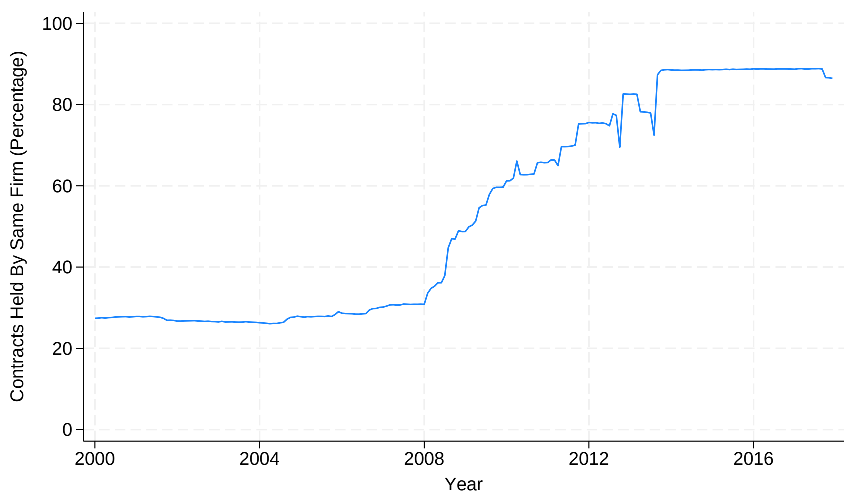
[14]Patients at control hospitals may encounter multiple contractors for a number of reasons. First, other Part A providers, such as skilled nursing facilities and hospitals that provide follow up care, were able to have their own Part A contractors (which may differ from the index admission hospital's contractor) prior to the reform. Furthermore, even after the reform, some services, such as durable medical equipment, home health, and hospice, have their claims processed by contractors assigned through a different process. Finally, because hospital treatment status is assigned using the hospital's Part A contractor immediately before the hospital's jurisdiction is exposed to the reform, some control hospitals may have changed contractors from 2000–2005 (the period of the table) to the hospital's treatment date.

Figure 2. Time Series of Reform Exposure, 2000-2017

(a) Percent of Jurisdictions Exposed to Reform



(b) Percent of Hospitals with Same Part A and B Contractors



Notes: Panel (a) plots the share of jurisdictions subject to the reform from 2000 to 2017. Panel (b) plots the share of hospitals where the hospital's Part A contractor matches the region's Part B contractor. We define each hospital's Part A contractor as the modal processor of inpatient claims in a month; Part B contractors are identified from modal Carrier claim processors in each jurisdiction-month.

Table 1. Summary Statistics by Hospital Reform Status, 2000-2005

	(1) Reduced Fragmentation	(2) No Change in Fragmentation	(3) Overall
Hospital Characteristics			
Urban	0.830	0.836	0.831
For-Profit	0.145	0.053	0.116
Beds	371.2	429.3	389.4
Resident Program	0.314	0.316	0.315
Monthly Index Admissions	389.8	445.6	407.4
Has One Contracting Firm	0.010	0.902	0.290
Patient Characteristics			
Age (years)	73.86	73.75	73.83
Male	0.430	0.432	0.431
Dual-Eligible	0.383	0.379	0.381
Administrative Outcomes			
Encounters Multiple Contracting Firms	0.915	0.519	0.790
Denial Rate	0.086	0.084	0.086
Hospital Admin. Cost (Million $)	15.34	15.56	15.41
Hospital Admin. Salaries (Million $)	5.467	5.793	5.567
Hospital Share of Health IT Used	0.543	0.551	0.546
Downstream Outcomes			
Medicare Payments ($)	5722	5886	5774
Providers Encountered	6.28	6.28	6.28
30-Day Readmission Rate	0.206	0.207	0.207
Number of Admissions	166,469	73,067	239,356

Notes: Table presents variable means for index admissions from 2000 to 2005, split by whether the admission was to a hospital that had a separate Part A contractor than its jurisdiction's Part B contractor immediately before the reform and thus were slated to see a reduction in fragmentation ("Reduced Fragmentation"), or if they were originally the same firm ("No Effect on Fragmentation"). An observation is an index admission. Hospital characteristics other than admissions and having one contracting firm are from the POS data. Admission count is average monthly number of index admissions. "Has One Contracting Firm" is an indicator for whether the hospital's Part A contractor is the same as the jurisdiction's Part B contractor in the month of the index admission. Patient characteristics are from the MSBF. "Encounters Multiple Contracting Firms" is an indicator for whether the patient has a claim processed by a contractor other than the index admission hospital's Part A contractor in the 30 days after discharge. "Share of Health IT Used" is the share of IT solutions tracked in the HIMSS data that the hospital has installed. The denial rate gives the share of claims for care rendered in the 30 days after discharge that is denied. Other administrative outcomes are given at the hospital-year level, with the share of health IT used available starting in 2002. For these outcomes, an observation is a hospital-year. "Providers Encountered" gives the number of unique providers from which the patient receives care in the 30 days after discharge. Readmissions are determined by whether the patient has a short-term hospital admission in the 30 days after discharge.

4 Empirical Strategy

Our empirical strategy uses the staggered roll-out of the reform and variation in the hospital-level effects on fragmentation to identify the causal effect of reductions in administrative fragmentation. Figure 3 illustrates the variation we exploit to form our triple difference-in-differences identification strategy, which compares hospitals slated to face a reduction in administrative fragmentation from the reform to those who were not. Our main estimating equation is:

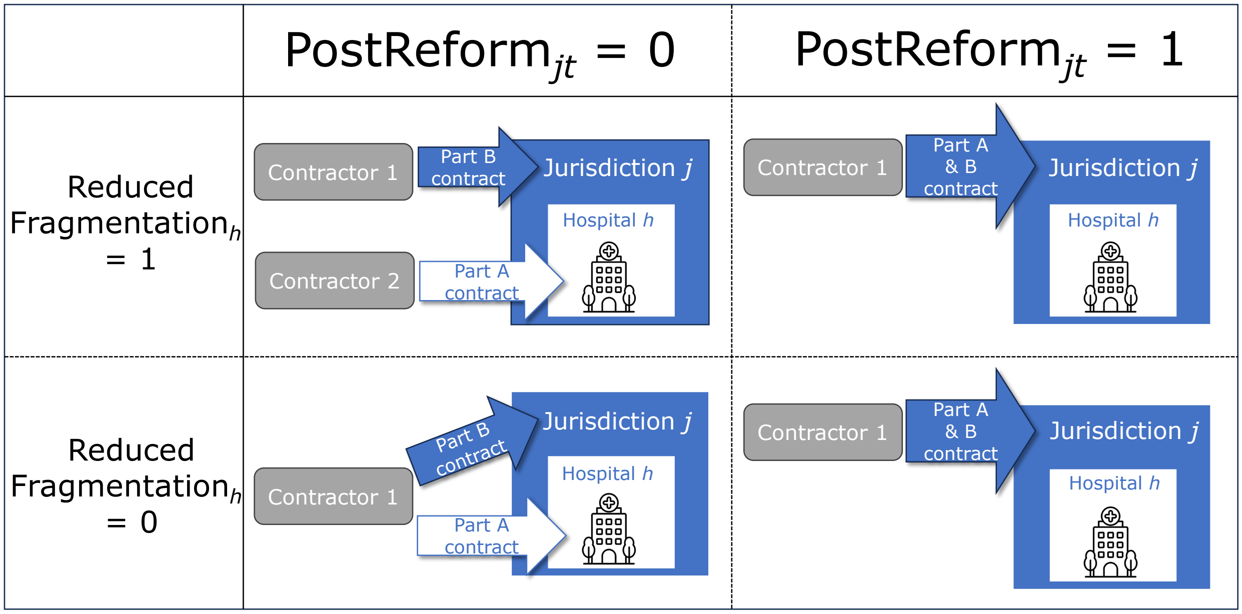
$$
\begin{aligned}
Y_{ihjt} = {} & \beta \text{PostReform}_{jt} \times \text{ReducedFragmentation}_h + \gamma \text{PostReform}_{jt} \\
& + \alpha \text{ReformWindow}_{jt} + \delta \text{ReformWindow}_{jt} \times \text{ReducedFragmentation}_h \\
& + \eta_h + \eta_t + \varepsilon_{ihjt}, \quad (1)
\end{aligned}
$$

where Y_{ihjt} is the outcome for patient i treated at hospital h in jurisdiction j in period t. PostReform_{jt} is an indicator for the observation being in the jurisdiction's post-reform period of the reform window, which is defined as the 24 months before or after the jurisdiction transitions from separate Part A and B contracts to a single combined contract. $\text{ReducedFragmentation}_h$ is an indicator for whether hospital h had different firms processing its Part A and Part B claims in the 12–24 months prior to being subject to the reform. In other words, $\text{ReducedFragmentation}_h$ indicates whether the hospital was expected to face a reduction in administrative fragmentation once the reform combined their jurisdiction's Part A and Part B contracts. ReformWindow_{jt} is an indicator for whether the observation falls within a 24 month window around that jurisdiction's reform date. We exclude the 6 months prior to the reform to account for anticipation effects.[15] η_h and η_t are hospital and time fixed effects, and ε_{ihjt} is the error term, which we allow to be correlated within hospital. For total inpatient count, HCRIS outcomes, and HIMSS outcomes, which are measured at a hospital-year level, we use the hospital-year analogue to this equation. For the provider EHR analysis, we use a jurisdiction-year analogue to this equation, as discussed further in Section 5.

The key parameter of interest is β, which captures the differential change for hospitals slated to face a reform-induced reduction in contractors relative to hospitals that were not. Note that β is a triple difference-in-difference coefficient which specifically captures the effect of the reduction in administrative fragmentation. This is isolated from any other effects of the reform, as we have controlled for any effects of the reform unrelated to the fragmentation

[15] To be precise, this variable takes a value of 1 when the observation is not in the treatment window, so that the excluded category is the two years pre-treatment.

Figure 3. Illustration of Empirical Strategy



Notes: This figure illustrates how PostReform_{jt} and $\text{ReducedFragmentation}_h$ are assigned in Equation 1. When a jurisdiction is exposed to the reform it goes from $\text{PostReform}_{jt} = 0$ to $\text{PostReform}_{jt} = 1$, meaning its Part A and Part B contracts become consolidated into one contract. This applies to all hospitals in a jurisdiction unless a hospital requests an exemption to remain with a separate Part A contractor (usually because they are part of a cross-regional chain). Hospitals are assigned as $\text{ReducedFragmentation}_h = 0$ or $\text{ReducedFragmentation}_h = 1$ based on whether the reform is expected to reduce administrative fragmentation for them—that is, whether their pre-reform Part A and Part B contracts were originally held by the same or different contractors.

reduction with the inclusion of PostReform_{jt}, captured by the γ coefficient.[16] Furthermore, β is an "intent-to-treat" parameter, as a hospital's $\text{ReducedFragmentation}_h$ status is assigned based on the number of contractors the hospital had pre-reform, rather than the number they eventually faced post-reform.[17] Thus, this specification captures only the exogenous effect

[16]Thus, γ is the difference-in-differences estimate of the reform for hospitals that did not face a reduction in fragmentation. It is identified by the differential change of the outcome for these hospitals after they were subject to the reform, compared to hospitals not exposed to the reform at the same time (i.e., relative to the time and hospital fixed effects).

[17]More specifically, it is an intent-to-treat parameter that identifies the average treatment effect among the treated (ATT) hospitals that were slated to face a reduction in administrative fragmentation. One potential concern is that this ATT is less than the average treatment effect (ATE) among all hospitals because the hospitals that chose to have multiple contractors before the reform may be those that gain the least from having a single contractor. Thus, we may underestimate the effect of fragmentation reductions overall because we are focusing only on those hospitals for which the effect is smallest. To assess the plausibility of this concern, we conduct a robustness test that exploits the fact that some of the pre-reform Part B contractors did not also act as Part A contractors. That is, hospitals in jurisdictions assigned to some Part B contractors had no choice as to whether they would have multiple contractors—since their Part B contractor did not process Part A claims, they had to have multiple contractors. In Appendix G, we show that our results are

of the reform on whether a hospital's Part A contractor matched the jurisdiction's Part B contractor, as opposed to any endogenous factors that would influence a hospital to opt-in or out of actually adhering to the reform. The α coefficient captures any baseline pre-reform differences for jurisdictions undergoing the reform at time t compared to jurisdictions not subject to the reform at that time, and the δ coefficient captures any baseline pre-reform differences for hospitals expected to face a reduction in fragmentation, compared to hospitals that did not.

Identification relies on the parallel trends assumption: the only differential change after the reform between hospitals expecting a reduction in fragmentation vs. not is due to the change in administrative fragmentation. That is, we assume that these hospitals' potential outcomes would have evolved along parallel trends absent the reform. To assess pre-trends and dynamics of the causal effect, we also estimate:

$$Y_{ihjt} = \sum_{e=-K}^{L} \beta_e T_{jt}(e) \times \text{ReducedFragmentation}_h + \sum_{e=-K}^{L} \gamma_e T_{jt}(e) + \alpha \text{ReformWindow}_{jt} + \delta \text{ReformWindow}_{jt} \times \text{ReducedFragmentation}_h + \eta_h + \eta_t + \varepsilon_{ihjt}, \quad (2)$$

where K and L define the size of the treatment window. We exclude period -7 as the reference period. $T_{jt}(e)$ is an indicator for being in a jurisdiction that is exposed to the reform e months, where e denotes event time.[18] This is a dynamic version of Equation (1) that allows for the effect of transitioning to a single contract or single firm to differ by event time e within the treatment window. This specification allows us to assess whether outcomes between the treatment and control groups were evolving similarly prior to the reform.

In robustness checks, we also estimate a conservative specification that controls for jurisdiction-by-time period fixed effects. This restricts our comparison even further and only compares treated vs. untreated hospitals in the *same* jurisdiction within the same month. Because of the jurisdiction-level assignment of contractors after the reform (and of Part B contractors pre-reform), this approach implicitly includes Part B contractor-by-jurisdiction-by-month fixed effects in all periods and assigned Part A contractor-by-jurisdiction-by-month in the post-treatment period.

robust to limiting the treatment group to these hospitals.

[18]Note that $\text{ReformWindow}_{jt} \equiv \sum_{e=-K}^{L} T_{jt}(e)$.

5 Results

Administrative Fragmentation Table 2 and Figure 4 show the estimates from Equations 1 and 2 of the impact of the reform on two measures of administrative fragmentation. The first is an indicator for whether a hospital's Part A contractor matches their jurisdiction's Part B contractor ("Hospital has Single Contractor"). The likelihood begins to increase 6 months prior to the treatment date and the transition is completed once the reform is set to be fully in place at time 0. Comparing the pre- and post-treatment periods, the estimate of the overall effect is 0.73—that is, 73 percent of hospitals slated to move from having different contractors for Parts A and B to having only one as a result of the reform end up doing so. The remaining 27 percent of hospitals are able to delay or opt out. The fact that this value is less than one supports the need for an "intent-to-treat" specification, where treatment is assigned based on the expected and not the realized effect of the reform on administrative fragmentation.

Table 2. Effect on Administrative Fragmentation

	(1) Hospital Has Single Contractor	(2) Encounters Multiple Contractors	(3) Has Physician Service Claim from Different Contractor	(4) Has Non-Physician Service Claim from Different Contractor
$\text{PostReform}_{jt} \times$ $\text{ReducedFragmentation}_h$	0.733*** (0.013)	-0.315*** (0.007)	-0.628*** (0.012)	0.032*** (0.005)
R^2	0.740	0.652	0.723	0.669
Dep. Var. Mean	0.530	0.706	0.531	0.460
Admissions	30.1m	30.1m	30.1m	30.1m

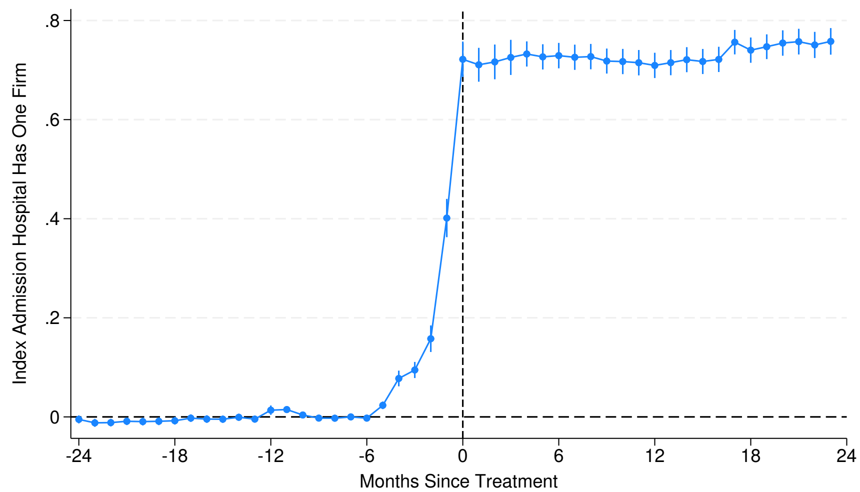
Notes: OLS estimates of equation (1). An observation is an admission from 2000–2017. Dependent variable in column (1) is an indicator for the index admission hospital's Part A contractor being the same firm as the jurisdiction's Part B contractor, in column (2) is an indicator for whether the patient encounters multiple contractors within 30 days of discharge, including the contractor processing their inpatient stay, in column (3) is an indicator for whether the patient has a Part B physician services claim processed by a different contractor in the 30 days after discharge, and column (4) is an indicator for whether the patient has any other claim (Part A or DME) processed by a different contractor in the 30 days after discharge. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

The second measure looks at the effects on patients with index admissions at these hospitals, and is an indicator for whether the patient has claims submitted to multiple contractors within 30 days of their discharge, including the contractor processing their index inpatient stay ("Encounters Multiple Contractors"). This tests whether hospital-level contractor consolidation translated into a meaningful reduction in patient exposure to administrative fragmentation. This depends on the extent to which patients actually billed for Part B services after their inpatient discharge and the extent to which the patient's other Part A providers (e.g., skilled nursing or home health) also changed contractors. Even with fewer hospital-level contractors, patient exposure to administrative fragmentation may persist if patients rarely received Part B services after discharge, or if there was an offsetting increase in fragmentation across other Part A providers. We find that this is not the case: mirroring the effect on the hospital-level measure, there is a substantial reduction in the share of patients who encounter multiple contractors during and after their inpatient stay. Once the reform is in place, there is a 32 percentage point reduction in the likelihood that a patient faces multiple contractors during and after their index admission. The third and fourth columns of Table 2 show that this reduction was driven by a reduction in the likelihood of receiving Part B services from a different contractor ("Has Physician Service Claim from Different Contractor"), rather than a reduction in the likelihood of encountering multiple Part A contractors ("Has Non-Physician Service Claim from Different Contractor"). While this latter form of fragmentation could also have been affected by the reform through a reduction in heterogeneity in contractors across Part A providers, we find this was not the case.[19]

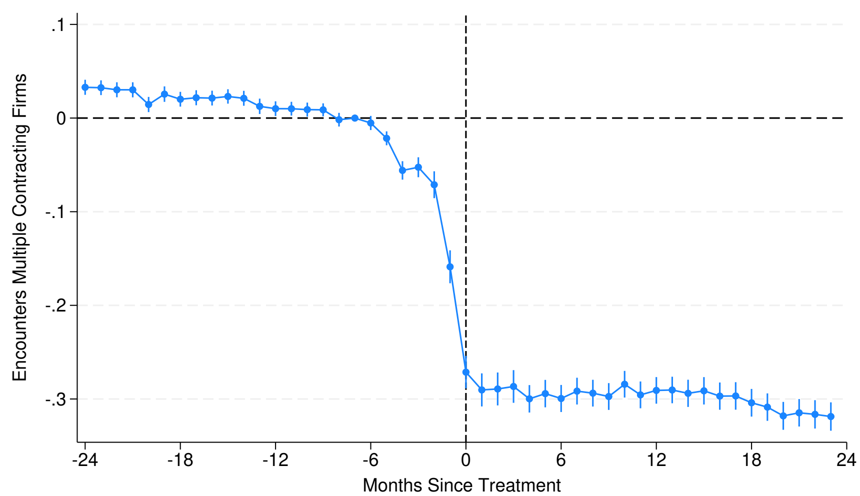
[19] The Medicare Modernization Act also created separate durable medical equipment contracts and home health and hospice contracts, introducing a small, offsetting increase in administrative fragmentation for patients that use these services.

Figure 4. Effect on Administrative Fragmentation

(a) Share of Admissions at Hospitals with Single Contractor for Part A and B



(b) Share of Admissions with Encounters Multiple Contractors



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The dependent variable for panel (a) is an indicator for whether the index admission occurs at a hospital whose Part A contractor is the same as the jurisdiction's Part B contractor and for panel (b) is an indicator for whether the patient encounters multiple contractors within 30 days of discharge, including the contractor processing their inpatient stay. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Claims-Based Administrative Outcomes Having shown the reform substantially reduced administrative fragmentation, we next assess its effects on key administrative outcomes.. Table 3 shows the results of estimating Equation (1) on various measures of claim denials and processing delays, and Figure 5 plots the coefficients from estimating Equation

(1) on the claim denial rate. Column (1) shows that patients who faced a reduction in administrative fragmentation see a small but statistically significant reduction in their denial rate, the share of their claims within 30 days of discharge that were denied. This corresponds to a very modest decrease in the likelihood of having any claim denied (column 2), with the reduction in denials being driven by a claims for physician services (column 3) with no detectable change in denials for other services (column 4).

The reduction in denials is small in magnitude: a nearly 50 percent reduction in the likelihood of encountering multiple contractors results in only a 2.3 percent reduction in denial rates and a 1.1 percent reduction in the likelihood of receiving any denial post-hospitalization. By contrast, League (2023) studies the mechanical changes in denial rate arising from changes in contractor identity over time, and finds that on average, these changes result in a nearly 20 percent change in the denial rate in absolute value. Applying the estimates from Dunn et al. (2024) on the cost of denials, we estimate that this reform reduced the expected administrative cost to providers of resubmitting denied claims by only 33 cents per admission.[20] In sum, our results suggest that much of the baseline 11 percent denial rate is not driven by administrative fragmentation across different contractors. Similarly, we find no evidence that reducing administrative fragmentation reduces bill processing time (column 5). We are able to rule out effects larger than a one percent decrease in processing time at the 95% confidence level.

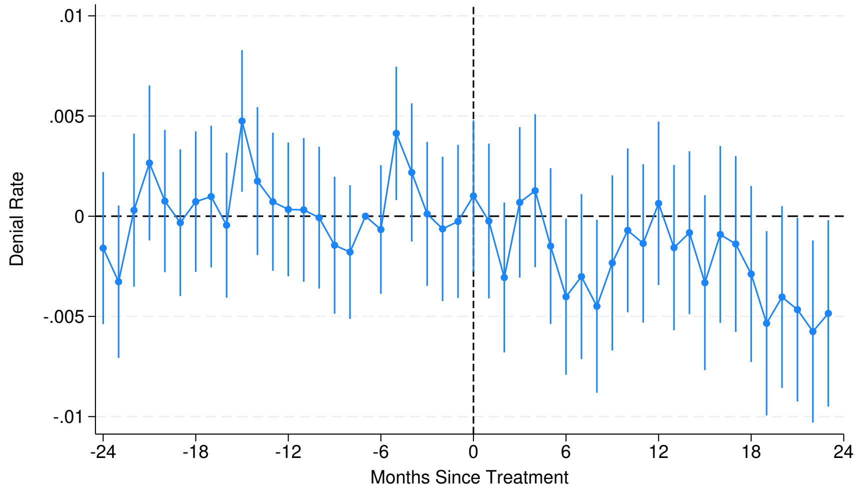
Table 3. Administrative Outcomes: Denials and Billing Delays

	(1) Denial Rate	(2) Encounters Any Denials	(3) Physician Services Denial Rate	(4) Non-Physician-Services Denial Rate	(5) Days From Procedure to Bill Paid
$\text{PostReform}_{jt} \times$	-0.002*	-0.005+	-0.003+	0.001	-0.025
$\text{ReducedFragmentation}_h$	(0.001)	(0.003)	(0.001)	(0.001)	(0.026)
R^2	0.353	0.543	0.342	0.381	0.539
Dep. Var. Mean	0.106	0.432	0.109	0.094	8.917
Coeff. as % of Mean	-2.31	-1.12	-2.32	0.98	-0.28
Admissions	30.1m	30.1m	30.1m	30.1m	30.1m

Notes: OLS estimates of equation (1). An observation is an admission from 2000–2017. Dependent variable in column (1) is the share of claims for care rendered in the 30 days after discharge that is denied, in column (2) is an indicator for whether the patient has any claim denied in the 30 days after discharge, in column (3) is the share of physician services claims for care rendered in the 30 days after discharge that is denied, column (4) the share of all other claims for care rendered in the 30 days after discharge that is denied, and in column (5) is the claim-level average number of days from the care being rendered to the claim being processed. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

[20] To arrive at this number, we multiply the average number of claims per admission in our data (15.9) by the estimated reduction in denials given in Table 3, the average number of resubmissions per claim initially denied by Medicare implied by Table II of Dunn et al. (2024) (0.831), and the average administrative cost of resubmitting a claim to Medicare reported in Figure IV of Dunn et al. (2024) ($10.25). Note that this back-of-the-envelope estimate captures only the administrative cost of additional resubmissions after a denial, rather than any changes in the cost of initial preparing a claim for submission.

Figure 5. Effect on Administrative Outcomes: Denials

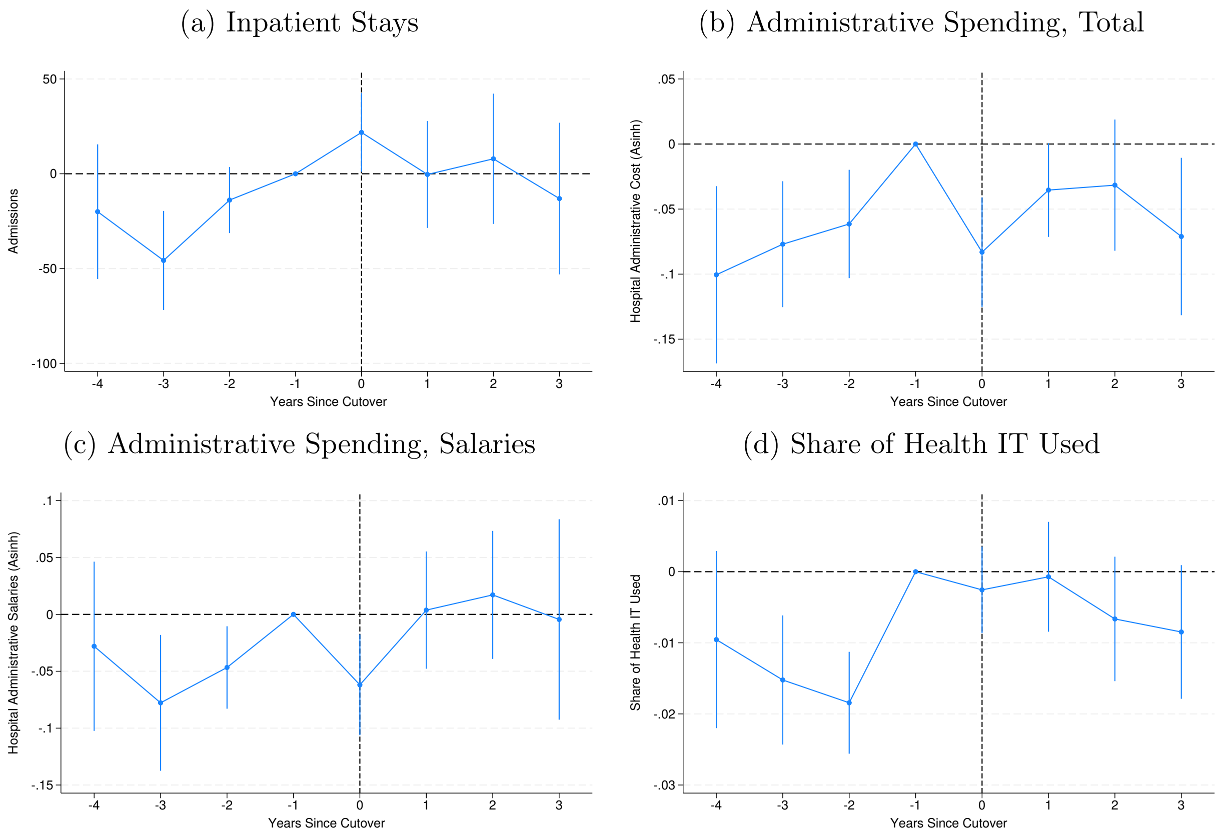


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Provider-Level Outcomes We next consider the effect of the reform on administrative spending and investments by health care providers. We present hospital-level outcomes in Figure 6 and Table 4. We first confirm that there is no change in hospital admission behavior in column (1). If hospitals became more or less likely to admit Medicare patients because of the reform, then we would erroneously interpret any differences driven by changing patient composition as the effect of reducing administrative fragmentation. Then looking to the hospital administrative spending measures in HCRIS, we do not find evidence that the reform reduced overall administrative costs or salaries for hospitals. We are able to rule out a reduction of administrative costs over 3.5 percent at the 95% confidence level.

We then look at the share of all IT solutions tracked by the HIMSS survey that the hospital has installed. This includes a wide array of IT software that could support billing and record-keeping, including document management, electronic medical records, and revenue cycle management. In the years before the reform was rolled out, hospitals had installed on average 54.6 percent of all possible solutions (Table 1). We find the reform had no meaningful effect on IT adoption in treated hospitals relative to control, estimating a marginally significant 0.6 percentage point (0.87 percent) increase in the share of health IT solutions used. As prior research suggests reductions in administrative hassles should *reduce* adoption of health IT (League, 2023; Shi, 2024), we conclude that the reform led to no meaningful change in hospital technology adoption.

Figure 6. Hospital-Level Outcomes



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-4, 3]$. Period $e = -1$ is the reference period. The dependent variable is (a) the number of total index admissions, (b) the inverse hyperbolic sine of total administrative costs (HCRIS), (c) the inverse hyperbolic sine of administrative spending on salaries (HCRIS), and (d) the share of total health IT solutions tracked by the HIMSS survey installed. An observation is a hospital-year from 2000 through 2017 for which the dependent variable is available. HIMSS data is only available starting in 2002. Standard errors are clustered at the hospital level.

One reason for the lack of effect on hospital-level measures of administrative burden could be that outpatient providers—not hospitals—incurred the bulk of the hassle associated with Part A and B administrative fragmentation, since hospitals primarily billed to the Part A contractor. Policymakers primarily framed the reform as reducing burdens for individual providers rather than for facilities Leavitt (2005). To investigate this, we make use of state-level survey data on the adoption of electronic health record (EHR) technology by *office-based* physicians. This data on administrative investment captures a much broader set of providers that may have been affected by the reform. Because of the limited years and granularity of the data (state-year-level observations from 2010–2015), we estimate a more parsimonious regression that assigns treatment status at the jurisdiction level as the share of treated hospitals. We regress the percentage of physicians reporting having adopted at least basic

Table 4. Hospital-Level Outcomes

	(1) Index Admissions	(2) Admin. Costs	(3) Admin. Costs (Asinh)	(4) Admin. Salaries	(5) Admin. Salaries (Asinh)	(6) Share of Health IT Used
$\text{PostReform}_{jt}\times$	23.98	-0.098	0.006	0.034	0.028	0.006+
$\text{ReducedFragmentation}_h$	(16.69)	(0.468)	(0.020)	(0.224)	(0.025)	(0.003)
R^2	0.959	0.810	0.599	0.691	0.579	0.801
Dep. Var. Mean	2501.60	24.295	16.837	7.628	15.683	0.683
Coeff. as % of Mean	0.96	-0.4	-	0.45	-	0.88
Hospital-Years	60927	60625	60625	60625	60625	39744

Notes: OLS estimates of equation (1). An observation is a hospital-year from 2000–2017 for which the dependent variable is available. Dependent variable in column (1) is the number of index admissions, in column (2) is the hospital's annual reported administrative costs in millions of dollars, in column (3) is the inverse hyperbolic sine of the hospital's annual reported administrative costs, in column (4) is the hospital's annual reported administrative salary costs in millions of dollars, in column (5) is the inverse hyperbolic sine of the hospital's annual reported administrative salary costs, and in column (6) is the share of health IT applications used, which is only available starting in 2002. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

EHR technology on this treatment variable, an indicator for whether the state has a single combined Part A and B contractor, and the interaction of the two, along with year and jurisdiction fixed effects.

Table 5. Office-Based Physician EHR Adoption

	(1)	(2) Percentage with Basic EHR	(3)
$\text{PostReform}_{jt}\times$	5.582	-0.467	6.713
$\text{ReducedFragmentation}_h$	(5.339)	(6.854)	(7.040)
R^2	0.865	0.907	0.827
Dep. Var. Mean	42.815	39.463	42.722
Coeff. as % of Mean	13.04	-1.18	15.71
Year of Reform	Any	2011	2012
Hospital-Years	335	36	54

Notes: OLS estimates jurisdiction-year-level regression of share of hospitals treated on treatment status along with jurisdiction and time fixed effects. An observation is a jurisdiction-year from 2010–2015. Dependent variable is the share of office-based physicians that have adopted at least basic electronic health record technology. Column (1) includes all jurisdictions, column (2) subsets to jurisdictions subject to the reform in 2011, and column (3) subsets to jurisdictions subject to the reform in 2012. Standard errors are clustered at the jurisdiction level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

As shown in Table 5, we find no evidence of an effect on office-based EHR adoption,

Table 6. Patient Admission-Level Outcomes

	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$\text{PostReform}_{jt} \times$	-0.007	-0.005	-0.001	-0.000
$\text{ReducedFragmentation}_h$	(0.007)	(0.003)	(0.002)	(0.001)
R^2	0.573	0.552	0.592	0.512
Dep. Var. Mean	7.667	2.862	2.209	0.198
Coeff. as % of Mean	-	-	-	-0.20
N	30.1m	30.1m	30.1m	30.1m

Notes: OLS estimates of equation (1). An observation is an admission from 2000–2017. Dependent variable in column (1) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in column (2) is the inverse hyperbolic sine of the total number of claims in the 30 days after discharge, in column (3) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in column (4) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

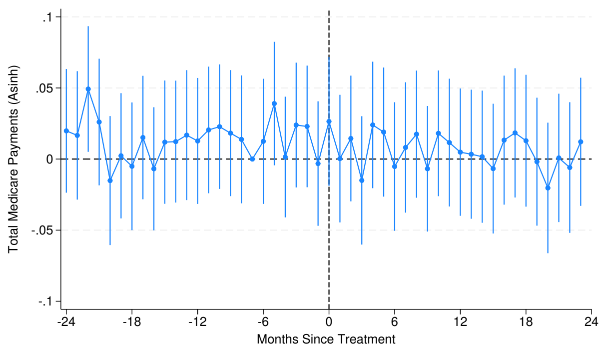
although our estimates are imprecise. For columns (2) and (3), we restrict the sample to jurisdictions that were subject to the reform in 2011 and 2012, the only two years with non-trivial variation in treatment status, and compare EHR adoption in regions that were differentially exposed to the reform at the same time. We find no evidence that reduced administrative fragmentation affected the speed of adoption of EHR among office-based physicians in any of these specifications. Appendix Figure A2 shows the overall trends in adoption by treatment status and corroborates the lack of effect.

Patient Admission-Level Outcomes We next consider the effect of the reform on the care patients received after their index admission. If administrative fragmentation discouraged outpatient providers from treating patients, then it could translate into lower post-discharge spending or reduced access to providers. If patients were not receiving necessary care post-discharge as a result, then this could result in negative health effects, as measured by rehospitalizations. We consider this in Table 6 and Figure E3. We find null effects on total spending and utilization within 30 days of discharge, and are able to rule out increases in spending of larger than 0.7 percent. We also find no effect on the number of providers a patient sees after discharge and are able to rule out an increase of over 0.4 percent. Finally, we also estimate a precise null effect on 30-day readmission rates, a proxy for patient health. Overall, we estimate precise nulls on patient spending and health outcomes, despite the large reduction in administrative fragmentation and modest reduction in denial rates. This indicates that administrative fragmentation in the billing process is not so cumbersome that it distorts care provision or affects patient health.

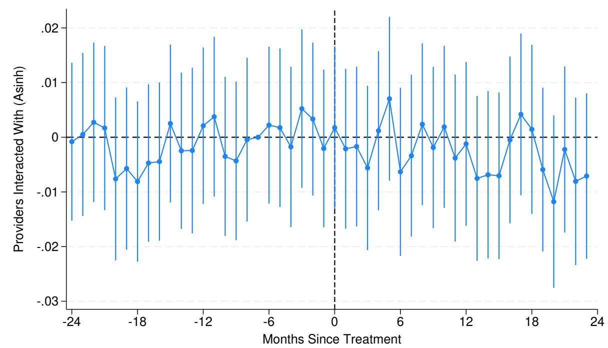
In Appendix Section B1, we disaggregate these results into categories of care: skilled nursing stays, physician services, outpatient facility care, home health agency, hospice, durable medical equipment, and subsequent hospitalizations. Across all of these categories, we find null effects of reduced administrative fragmentation on spending or utilization.

Figure 7. Patient Admission-Level Outcomes

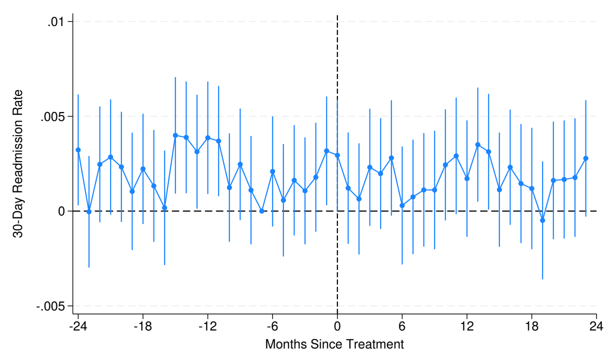
(a) Total Spending



(b) Number of Providers Encountered



(c) 30-day Readmissions



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in panel (b) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in panel (c) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Robustness Checks and Heterogeneity In the Appendix, we present robustness checks of our results. First in Appendix C, we consider a treatment window of up to 4 years. We find that the reductions in administrative fragmentation are persistent, but that the effect on denials grows until 10 quarters after the reform before shrinking back toward zero. We continue to find null results on downstream utilization and outcomes. Next in Appendix D, we use the linear projection difference-in-differences estimator of Dube et al. (2023) to account for possible heterogeneity in the dynamic effects of treatment, and find a somewhat smaller reduction in administrative fragmentation but similar other results. Then, we confirm that our results are robust to the inclusion of index admission diagnostic related group (DRG)-by-time fixed effects in Appendix E and jurisdiction-by-month fixed effects in Appendix F. The latter set of fixed effects restricts our comparison to patients admitted to different hospitals in the *same* jurisdiction in the same month, meaning the Part B and (after the reform) assigned Part A contractor are the same. Here, the first stage is weaker, but the downstream results are consistent with our main results. Finally, we limit the sample of hospitals that faced reduced fragmentation to only hospitals whose pre-reform Part B contractor did not also process Part A claims, meaning these hospitals could not have chosen to have the same Part A and B contractor in the pre-reform period. In Appendix G, we show that for this group, while the reduction in fragmentation was larger, there were still no meaningful effects on administrative or downstream outcomes.

In order to understand whether the lack of effects on average outcomes masks heterogeneity in the gains from decreased administrative fragmentation, we also examine subsets of index admissions for conditions that may have seen especially large gains from reduced administrative fragmentation. These include admissions for DRGs in the top quartile or decile of total Medicare spending, providers encountered, or claim denial rate in the 30 days after discharge. For these admissions, we find less consistent evidence of any decrease in denials but still precise null effects on spending and other downstream outcomes. These results are presented in Appendix H. In summary, we find no evidence that reducing administrative fragmentation improves health care efficiency, even among the patients likely to gain the most.

6 Conclusion

This paper considers the causal effect of administrative fragmentation—the lack standardization in billing and administrative processes—in US health care by studying a large reform that reduced administrative fragmentation within the Medicare program. Our difference-in-differences strategy focuses on patients transitioning from the inpatient to the outpatient setting and leverages the staggered timing of the reform as well as baseline differences across hospitals in the expected effect of the reform. We document that the reform indeed leads to a large reduction in administrative fragmentation, as measured by the number of separate firms processing a patient's claims. However, this is associated with only a modest improvement in administrative efficiency—there is a small reduction in denial rates for post-discharge claims, but no change in claims processing time. Furthermore, we find no evidence that reducing administrative fragmentation affected hospital administrative costs or the adoption of billing software. Finally, we find precise null effects on downstream utilization, spending, and patient health effects.

Our results indicate that, on their own, efforts to streamline billing processes and reduce administrative fragmentation may not reduce administrative costs in the US health care system. Furthermore, they imply that administrative fragmentation alone does not appear to distort patient care or health in a meaningful way. Our findings imply that simply standardizing administrative processes not have a meaningful effect on administrative costs or care provision.

However, it is important to note that our findings are less informative about other, more complicated aspects of payer fragmentation. In our setting, we are ultimately studying the consolidation of fragmented administrative processes within a *single*—albeit the largest—payer in the US. The contractors we study all serve the Medicare program, meaning they enforce federally-set payment rules and are required to follow similar protocols when making documentation requests and coverage determinations. Thus our results cannot speak to efforts to harmonize other aspects of payer fragmentation, like inconsistencies in payment rates, coverage guidelines, and benefit eligibility rules. Examining how these broader dimensions of payer fragmentation affect administrative costs, provider behavior, and patient outcomes remains an important direction for future research.

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Appendix

The following appendices provide additional robustness checks, analyses, and details on our data and setting

Appendix A contains additional figures referenced in the main text.

Appendix B disaggregates our utilization results by categories of care.

Appendix C presents estimates using a treatment window of 4 years.

Appendix D reports estimates using the linear projection difference-in-differences estimator.

Appendix E shows that our results are robust to the inclusion of DRG-by-month fixed effects.

Appendix F provides results with the inclusion of jurisdiction-by-month fixed effects.

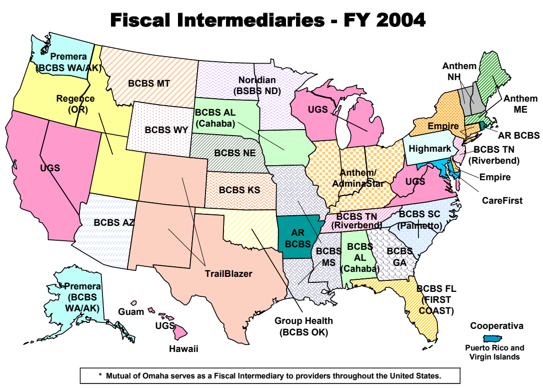
Appendix G presents results limiting the sample of hospitals with reduced fragmentation to those with no choice over their pre-reform level of fragmentation.

Appendix H investigates heterogeneity in the effects by admitting diagnosis.

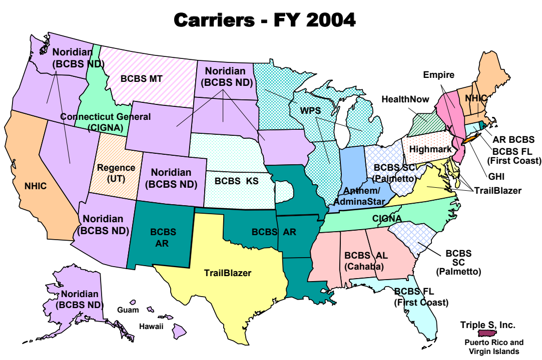
A Additional Figures Referenced in Main Text

Figure A1. Pre- and Post-Reform Contractors

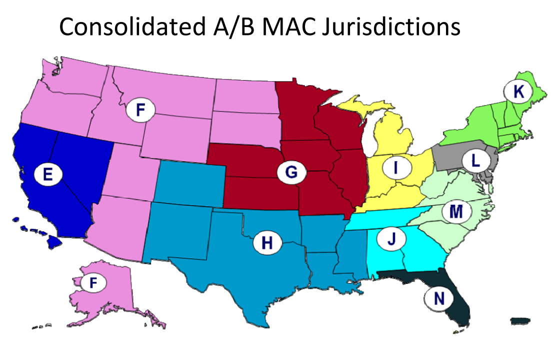
(a) Most Common Pre-Reform Part A Contractors by Jurisdiction



(b) Pre-Reform Part B Contractors



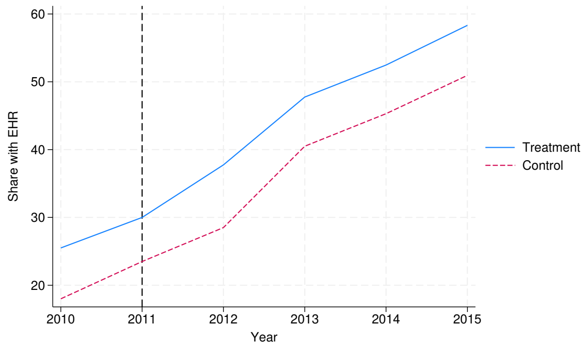
(c) Post-Reform Consolidated Contractors



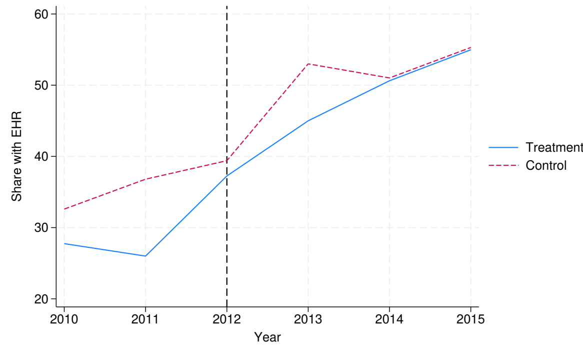
Notes: Each panel reports the firm holding the contract for processing Medicare claims in each jurisdiction. Pre-reform, Part A contractors were chosen at the hospital level, and the map in panel (a) shows the firm processing claims for a plurality of the hospitals for each jurisdiction. Panel (b) shows the assigned pre-reform Part B contractors for each jurisdiction. The map in panel (c) shows the final regions after the reform was implemented across all regions; all pre-reform regions that were consolidated into one MAC jurisdiction are shown in one color. Figures are from Centers for Medicare & Medicaid Services (2024a).

Figure A2. Trends in EHR Adoption

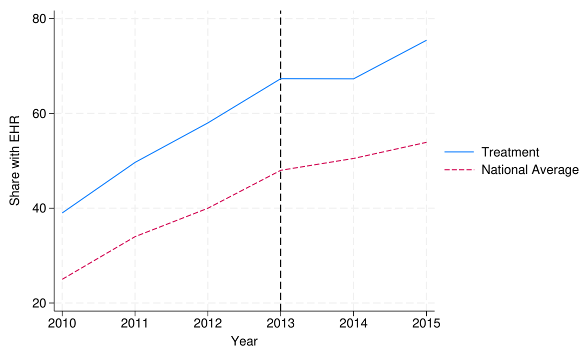
(a) Jurisdictions Subject to Reform in 2011



(b) Jurisdictions Subject to Reform in 2012



(c) Jurisdictions Subject to Reform in 2013



Notes: Figures plot the mean share of office-based physicians using at least basic electronic health record technology for jurisdictions subject to the reform in (a) 2011, (b) 2012, or (c) 2013 by whether more than half of the hospitals in the jurisdiction were exposed to the reform. For panel (c), there are no jurisdictions with fewer than 98 percent of hospitals exposed to the reform, and we instead plot the national mean. Vertical lines give the year of reform in the relevant jurisdictions.

B Disaggregated Outcomes

Table B1. Utilization Outcomes: Part A Services

	(1) Home Health Claims	(2) Home Health Spending	(3) Skilled Nursing Facility Claims	(4) Skilled Nursing Facility Spending	(5) Other Hospital Claims	(6) Other Hospital Spending	(7) Hospice Claims	(8) Hospice Spending	(9) Outpatient Facility Claims	(10) Outpatient Facility Spending
$\text{PostReform}_{jt}\times$	-0.001	-0.109	-0.001	23.27+	-0.002*	21.14	0.000	-1.642	0.003	2.065
$\text{ReducedFragmentation}_h$	(0.001)	(3.425)	(0.001)	(11.89)	(0.001)	(14.79)	(0.000)	(1.363)	(0.005)	(2.004)
R^2	0.797	0.799	0.741	0.770	0.548	0.646	0.504	0.546	0.824	0.735
Dep. Var. Mean	0.209	531.11	0.218	2061.78	0.264	3135.67	0.051	120.24	0.981	362.96
Coeff. as % of Mean	-0.63	-0.02	-0.54	1.13	-0.78	0.67	0.09	-1.37	0.33	0.57
Admissions	152m	152m	152m	152m	152m	152m	152m	152m	152m	152m

Notes: OLS estimates of equation (1). An observation is an index admission from 2000–2017. The dependent variable in column (1) is the number of home health claims, in column (2) is total home health spending, in column (3) is the number of skilled nursing facility claims, in column (4) is total skilled nursing facility spending, in column (5) is the number of hospitals claims outside of the index admission, and in column (6) is total spending on hospital claims outside of the index admission. All outcomes are measured within 30 days after discharge from an index admission. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

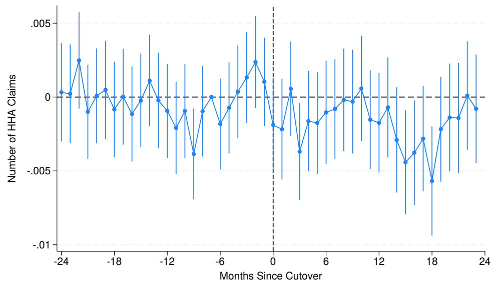
Table B2. Utilization Outcomes: Part B Services

	(1) Durable Medical Equipment Claims	(2) Durable Medical Equipment Spending	(3) Physician Services Claims	(4) Physician Services Spending
$\text{PostReform}_{jt}\times$	0.003	-0.507	-0.024	-0.967
$\text{ReducedFragmentation}_h$	(0.002)	(0.833)	(0.025)	(3.708)
R^2	0.547	0.405	0.568	0.509
Dep. Var. Mean	0.546	97.632	7.405	758.95
Coeff. as % of Mean	0.54	-0.52	-0.32	-0.13
Admissions	152m	152m	30.1m	30.1m

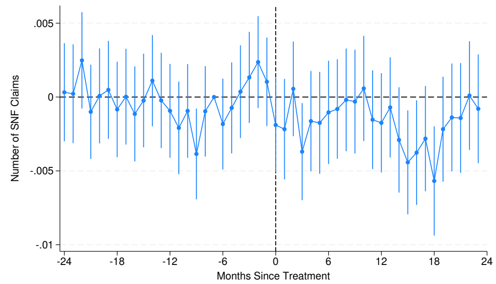
Notes: OLS estimates of equation (1). An observation is an index admission from 2000–2017. The dependent variable in column (1) is the number of durable medical equipment claims, in column (2) is total durable medical equipment spending, in column (3) is the number of physician services claims, and in column (4) is total physician services spending. Columns 1-2 present estimates for durable medical equipment claims and spending and columns 3-4 present estimates for physician services claims and spending. All outcomes are measured within 30 days after discharge from an index admission. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure B1. Effect on Disaggregated Utilization Outcomes

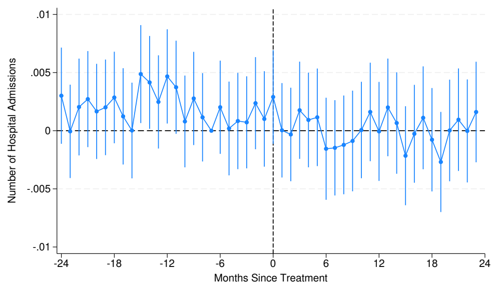
(a) Home Health Agency Claims



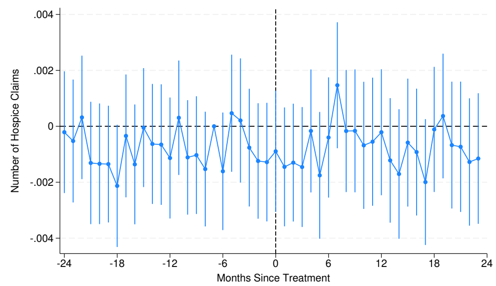
(b) Skilled Nursing Facility Claims



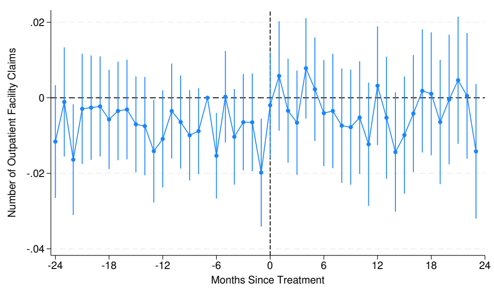
(c) Hospitalization Claims



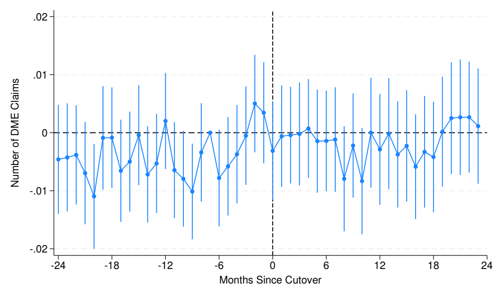
(d) Hospice Claims



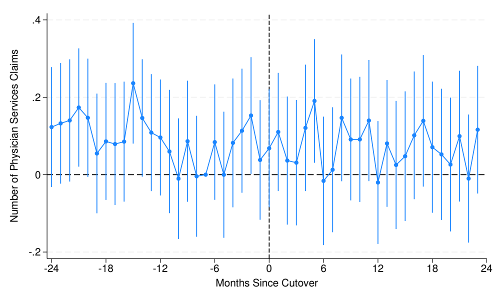
(e) Outpatient Facility Claims



(f) Durable Medical Equipment Claims



(g) Physician Services Claims

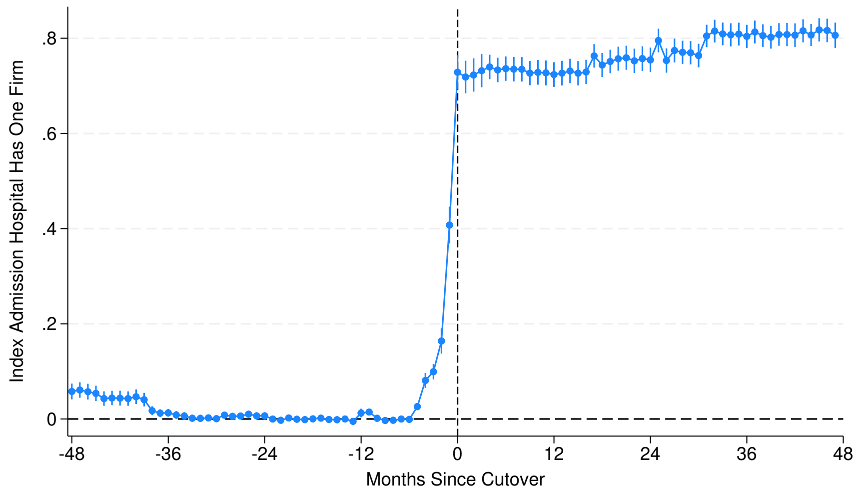


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-48, 47]$. Period $e = -7$ is the reference period. The dependent variable for panel (a) is the number of home health agency within 30 days of discharge, panel (b) is the number of skilled nursing facility claims, panel (c) is the number of hospitalization claims, panel (d) is the number of hospice claims, panel (e) is the number of outpatient facility claims, panel (f) is the number of durable medical equipment claims, and panel (g) is the number of in-office physician claims (Carrier). All outcomes are measured within 30 days after discharge from an index admission. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

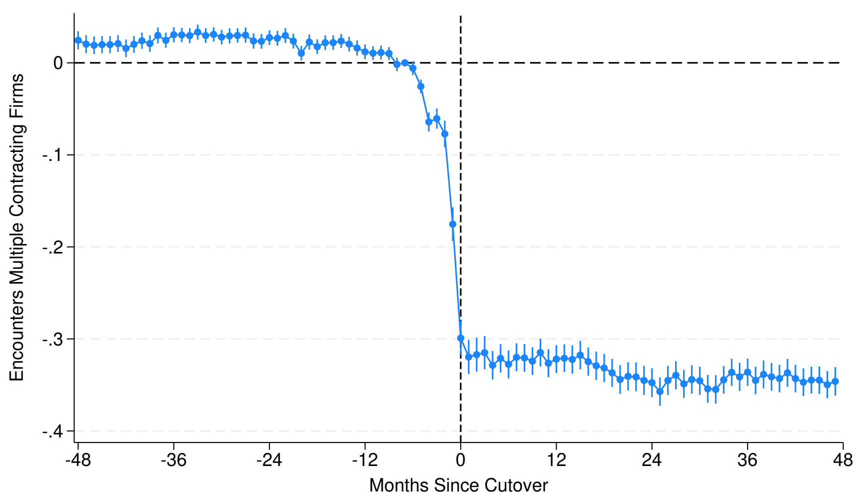
C Longer Treatment Window

Figure C1. Effect on Administrative Fragmentation, Longer Treatment Window

(a) Share of Admissions at Hospitals with Single Contractor for Part A and B



(b) Share of Admissions with Encounters Multiple Contractors



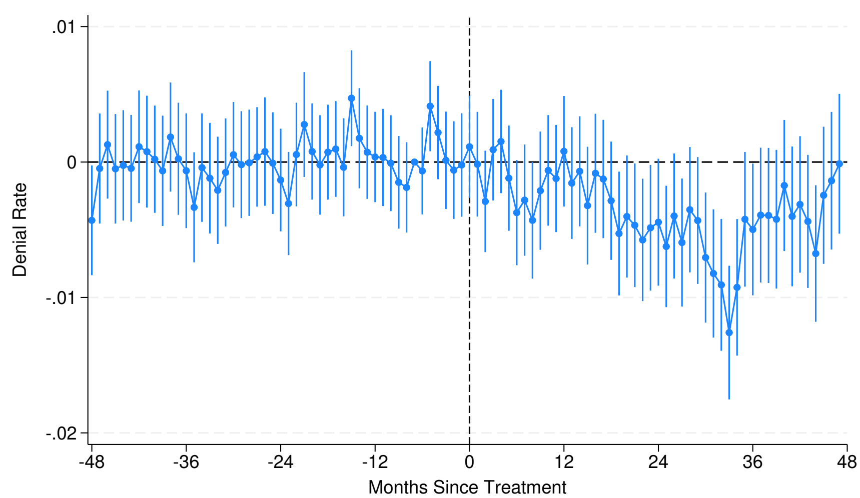
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-48, 47]$. Period $e = -7$ is the reference period. The dependent variable for panel (a) is an indicator for whether the index admission occurs at a hospital whose Part A contractor is the same as the jurisdiction's Part B contractor and for panel (b) is an indicator for whether the patient encounters multiple contractors within 30 days of discharge, including the contractor processing their inpatient stay. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Table C1. Effect on Administrative Fragmentation, Longer Treatment Window

	(1) Hospital Has Single Contractor	(2) Encounters Multiple Contractors	(3) Has Physician Service Claim from Different Contractor	(4) Has Non-Physician-Service Claim from Different Contractor
$\text{PostReform}_{jt}\times$	0.751***	-0.330***	-0.598***	0.032***
$\text{ReducedFragmentation}_h$	(0.012)	(0.006)	(0.010)	(0.005)
R^2	0.777	0.681	0.754	0.683
Dep. Var. Mean	0.530	0.706	0.481	0.460
Admissions	30.1m	30.1m	30.1m	30.1m

Notes: OLS estimates of equation (1) with $K = 48$ and $L = 47$. An observation is an admission from 2000–2017. Dependent variable in column (1) is an indicator for the index admission hospital's Part A contractor being the same firm as the jurisdiction's Part B contractor, in column (2) is an indicator for whether the patient has a claim processed by a contractor other than the index hospital's Part A contractor in the 30 days after discharge, in column (3) is an indicator for whether the patient has a physician services claim processed by a different contractor in the 30 days after discharge, and column (4) is an indicator for whether the patient has any other claim processed by a different contractor in the 30 days after discharge. Standard errors are clustered at the hospital level.
$+\ p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure C2. Effect on Administrative Outcomes: Denials, Longer Treatment Window



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-48, 47]$. Period $e = -7$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Table C2. Administrative Outcomes: Denials and Billing Delays, Longer Treatment Window

	(1) Denial Rate	(2) Encounters Any Denials	(3) Physician Services Denial Rate	(4) Non-Physician-Services Denial Rate	(5) Days From Procedure to Bill Paid
$\text{PostReform}_{jt} \times$	-0.003*	-0.008**	-0.004*	0.001	-0.028
$\text{ReducedFragmentation}_h$	(0.001)	(0.003)	(0.002)	(0.001)	(0.029)
R^2	0.354	0.544	0.343	0.381	0.539
Dep. Var. Mean	0.106	0.432	0.109	0.094	8.917
Coeff. as % of Mean	-3.23	-1.85	-3.54	1.19	-0.32
Admissions	30.1m	30.1m	30.1m	30.1m	30.1m

Notes: OLS estimates of equation (1) with $K = 48$ and $L = 47$. An observation is an admission from 2000–2017. Dependent variable in column (1) is the share of claims for care rendered in the 30 days after discharge that is denied, in column (2) is an indicator for whether the patient has any claim denied in the 30 days after discharge, in column (3) is the share of physician services claims for care rendered in the 30 days after discharge that is denied, column (4) the share of all other claims for care rendered in the 30 days after discharge that is denied, and in column (5) is the claim-level average number of days from the care being rendered to the claim being processed. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

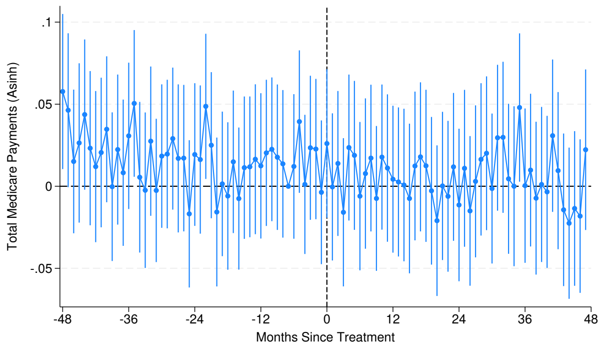
Table C3. Patient Admission-Level Outcomes, Longer Treatment Window

	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$\text{PostReform}_{jt} \times$	-0.012+	-0.006+	-0.001	-0.001
$\text{ReducedFragmentation}_h$	(0.007)	(0.004)	(0.003)	(0.001)
R^2	0.573	0.552	0.592	0.512
Dep. Var. Mean	7.667	2.862	2.209	0.198
Coeff. as % of Mean	-	-	-	-0.53
N	30.1m	30.1m	30.1m	30.1m

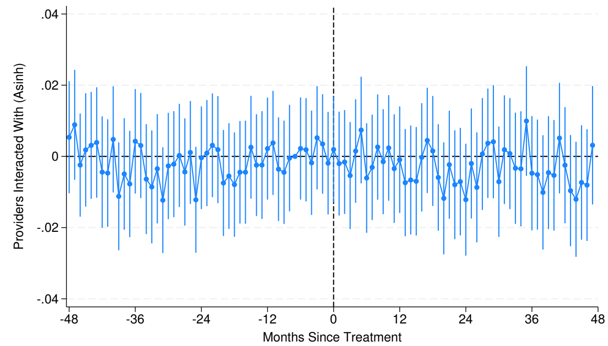
Notes: OLS estimates of equation (1) with $K = 48$ and $L = 47$. An observation is an admission from 2000–2017. Dependent variable in column (1) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in column (2) is the inverse hyperbolic sine of the total number of claims in the 30 days after discharge, in column (3) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in column (4) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure C3. Patient Admission-Level Outcomes, Longer Treatment Window

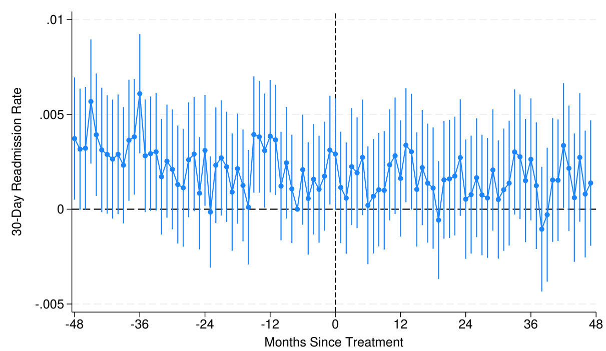
(a) Total Spending



(b) Number of Providers Encountered



(c) 30-day Readmissions

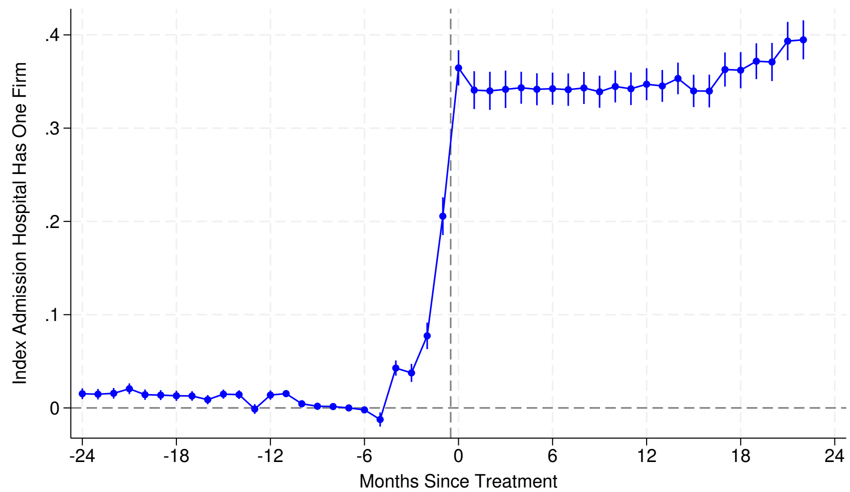


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-48, 47]$. Period $e = -7$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in panel (b) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in panel (c) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

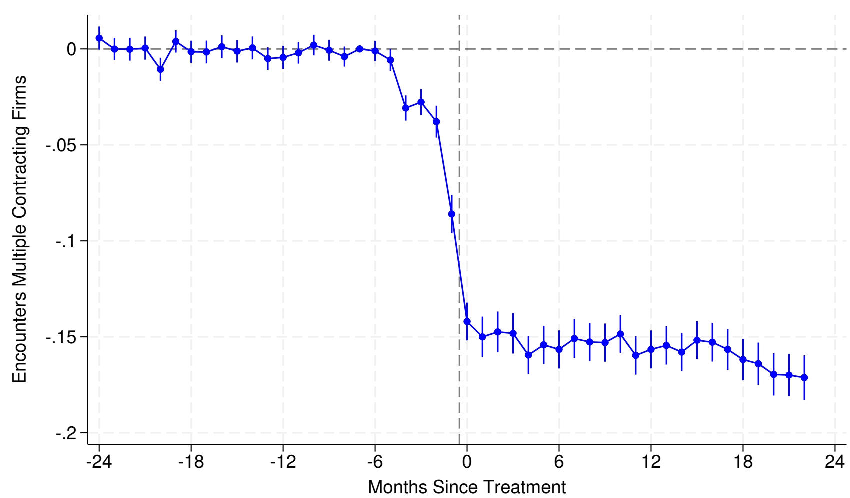
D Alternative Difference-in-Differences Estimator

Figure D1. Effect on Administrative Fragmentation, LP-DiD

(a) Share of Admissions at Hospitals with Single Contractor for Part A and B

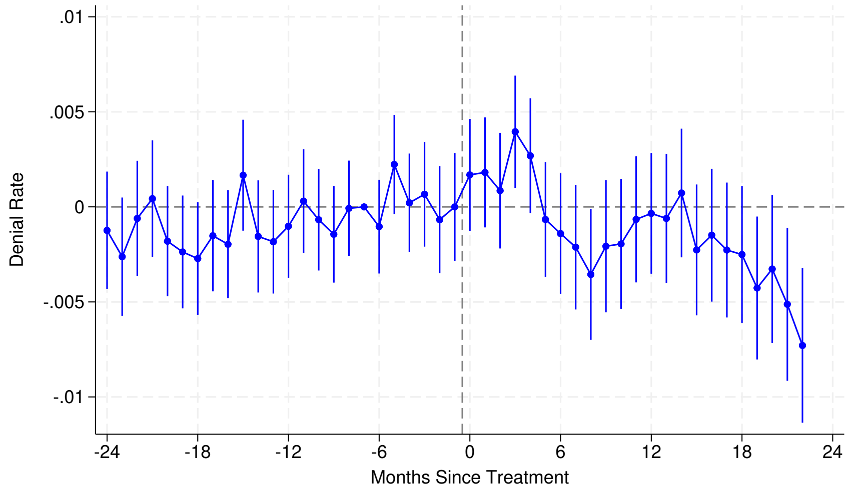


(b) Share of Admissions with Encounters Multiple Contractors



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ using the linear projection difference-in-differences estimator. Period $e = -7$ is the reference period. The dependent variable for panel (a) is an indicator for whether the index admission occurs at a hospital whose Part A contractor is the same as the jurisdiction's Part B contractor and for panel (b) is an indicator for whether the patient has a claim processed by a contractor firm other than that which processed the claim for the index admission in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Figure D2. Effect on Administrative Outcomes: Denials, LP-DiD



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ using the linear projection difference-in-differences estimator. Period $e = -7$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

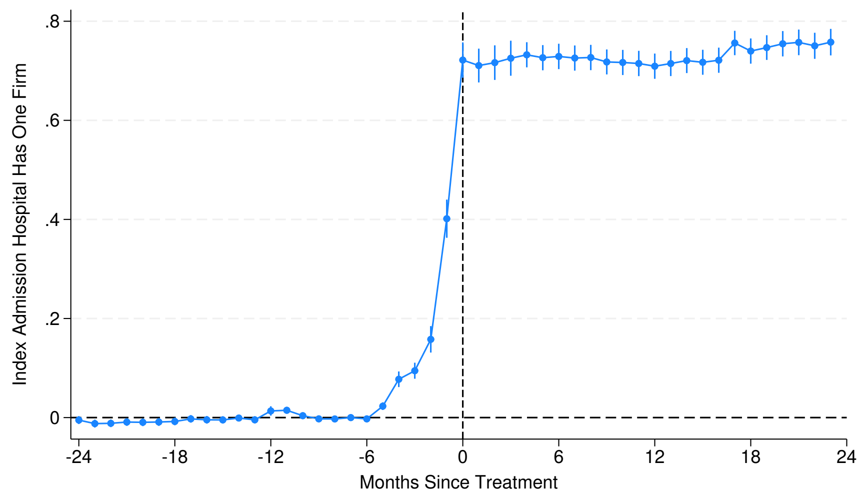
Figure D3. Patient Admission-Level Outcomes, LP-DiD

Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ using the linear projection difference-in-differences estimator. Period $e = -7$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in panel (b) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in panel (c) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

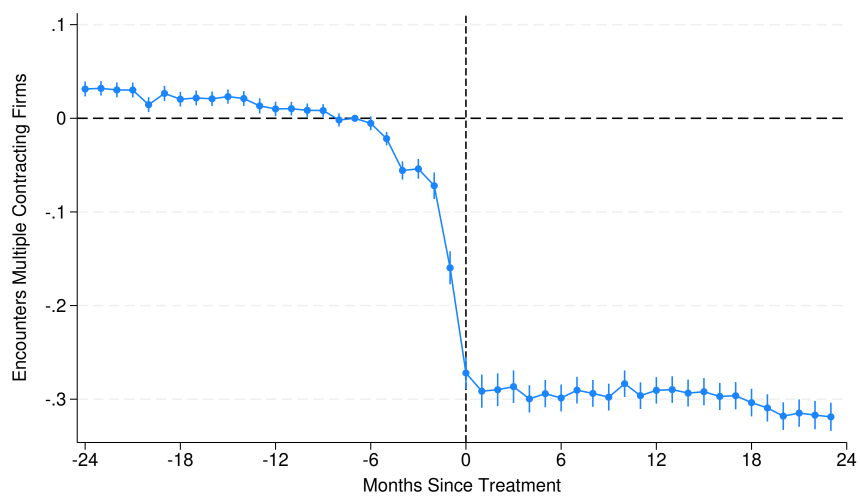
E Admission DRG-by-Month Fixed Effects

Figure E1. Effect on Administrative Fragmentation, DRG-by-Month FEs

(a) Share of Admissions at Hospitals with Single Contractor for Part A and B



(b) Share of Admissions with Encounters Multiple Contractors



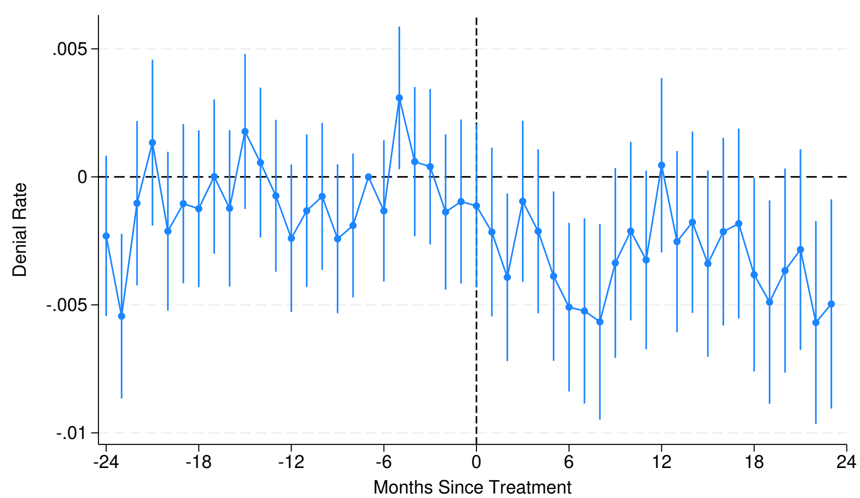
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with DRG-by-month fixed effects. Period $e = -7$ is the reference period. The dependent variable for panel (a) is an indicator for whether the index admission occurs at a hospital whose Part A contractor is the same as the jurisdiction's Part B contractor and for panel (b) is an indicator for whether the patient has a claim processed by a contractor firm other than that which processed the claim for the index admission in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Table E1. Effect on Administrative Fragmentation, DRG-by-Month FEs

	(1) Hospital Has Single Contractor	(2) Encounters Multiple Contractors	(3) Has Physician Service Claim from Different Contractor	(4) Has Non-Physician-Service Claim from Different Contractor
PostReform$_{jt}\times$	0.733***	-0.315***	-0.574***	0.031***
ReducedFragmentation$_h$	(0.013)	(0.007)	(0.011)	(0.005)
R^2	0.742	0.259	0.528	0.248
Dep. Var. Mean	0.530	0.706	0.481	0.460
Admissions	30.1m	30.1m	30.1m	30.1m

Notes: OLS estimates of equation (1) with DRG-by-month fixed effects. An observation is an admission from 2000–2017. Dependent variable in column (1) is an indicator for the index admission hospital's Part A contractor being the same firm as the jurisdiction's Part B contractor, in column (2) is an indicator for whether the patient has a claim processed by a contractor other than the index hospital's Part A contractor in the 30 days after discharge, in column (3) is an indicator for whether the patient has a physician services claim processed by a different contractor in the 30 days after discharge, and column (4) is an indicator for whether the patient has any other claim processed by a different contractor in the 30 days after discharge. Standard errors are clustered at the hospital level.
+ $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure E2. Effect on Administrative Outcomes: Denials, DRG-by-Month FEs



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with DRG-by-month fixed effects. Period $e = -7$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Table E2. Administrative Outcomes: Denials and Billing Delays, DRG-by-Month FEs

	(1) Denial Rate	(2) Encounters Any Denials	(3) Physician Services Denial Rate	(4) Non-Physician-Services Denial Rate	(5) Days From Procedure to Bill Paid
$\text{PostReform}_{jt}\times$	-0.002+	-0.005+	-0.002+	0.000	-0.019
$\text{ReducedFragmentation}_h$	(0.001)	(0.003)	(0.001)	(0.001)	(0.026)
R^2	0.052	0.083	0.056	0.058	0.120
Dep. Var. Mean	0.099	0.432	0.100	0.072	8.917
Coeff. as % of Mean	-2.05	-1.12	-2.23	0.58	-0.24
Admissions	29.0m	30.1m	28.4m	28.0m	30.1m

Notes: OLS estimates of equation (1) with DRG-by-month fixed effects. An observation is an admission from 2000–2017. Dependent variable in column (1) is the share of claims for care rendered in the 30 days after discharge that is denied, in column (2) is an indicator for whether the patient has any claim denied in the 30 days after discharge, in column (3) is the share of physician services claims for care rendered in the 30 days after discharge that is denied, column (4) the share of all other claims for care rendered in the 30 days after discharge that is denied, and in column (5) is the claim-level average number of days from the care being rendered to the claim being processed. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

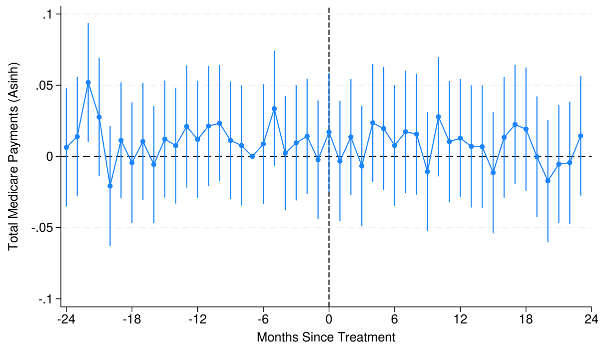
Table E3. Patient Admission-Level Outcomes, DRG-by-Month FEs

	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$\text{PostReform}_{jt}\times$	-0.004	-0.004	-0.000	-0.000
$\text{ReducedFragmentation}_h$	(0.006)	(0.003)	(0.002)	(0.001)
R^2	0.188	0.147	0.154	0.074
Dep. Var. Mean	7.667	2.862	2.209	0.198
Coeff. as % of Mean	-	-	-	-0.20
N	30.1m	30.1m	30.1m	30.1m

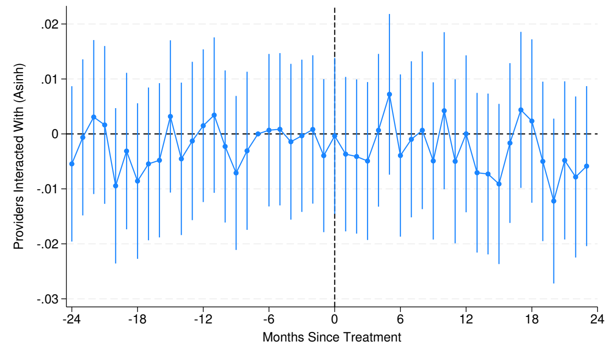
Notes: OLS estimates of equation (1) with DRG-by-month fixed effects. An observation is an admission from 2000–2017. Dependent variable in column (1) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in column (2) is the inverse hyperbolic sine of the total number of claims in the 30 days after discharge, in column (3) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in column (4) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure E3. Patient Admission-Level Outcomes, DRG-by-Month FEs

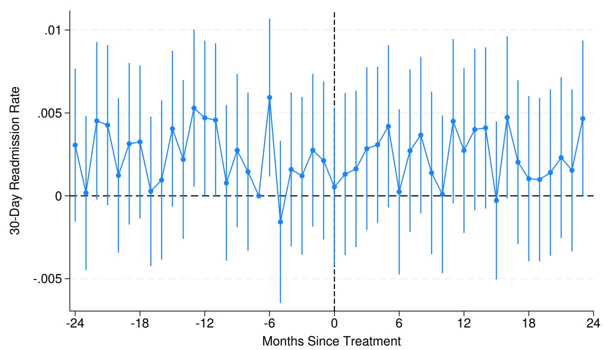
(a) Total Spending



(b) Number of Providers Encountered



(c) 30-day Readmissions

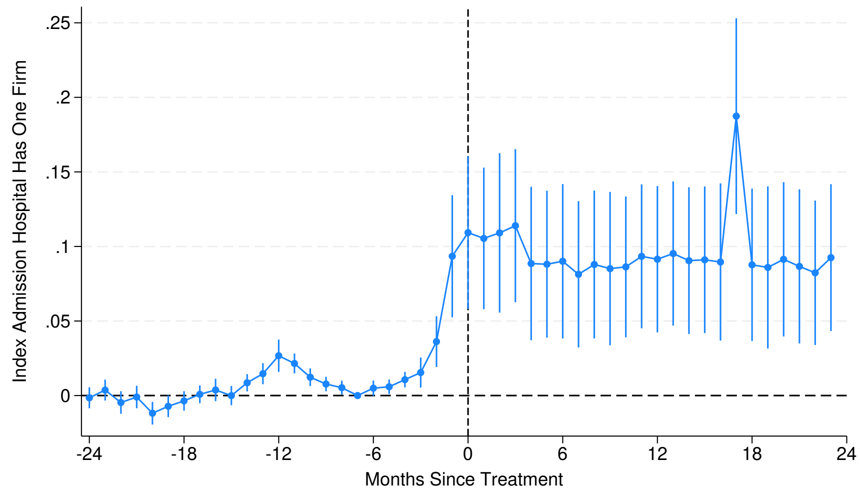


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with DRG-by-month fixed effects. Period $e = -7$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in panel (b) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in panel (c) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

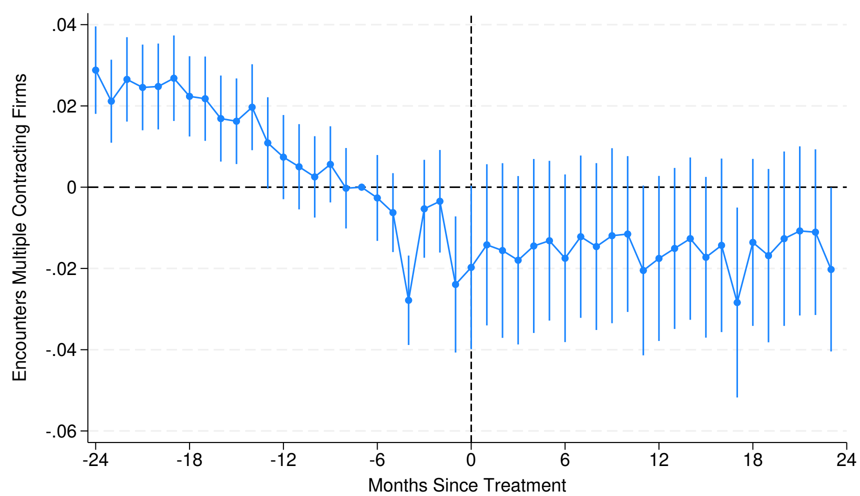
F Jurisdiction-by-Month Fixed Effects

Figure F1. Effect on Administrative Fragmentation, with Jurisdiction-Month FEs

(a) Share of Admissions at Hospitals with Single Contractor for Part A and B



(b) Share of Admissions with Encounters Multiple Contractors



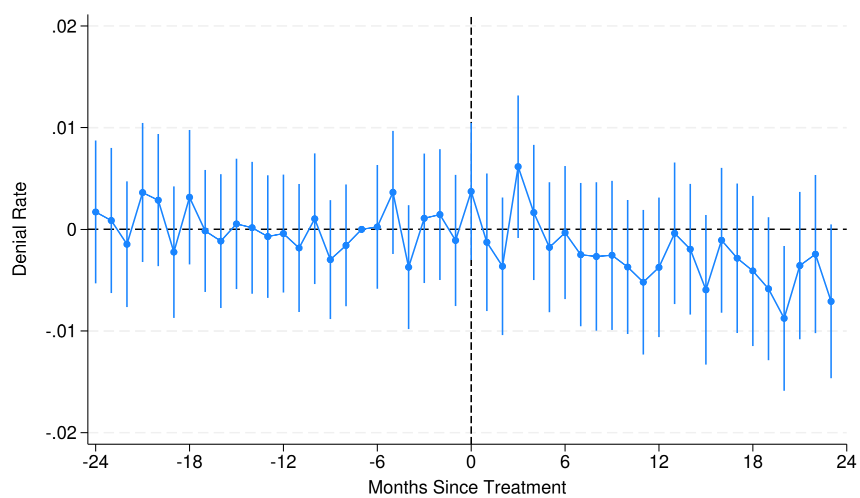
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with jurisdiction-month fixed effects. Period $e = -7$ is the reference period. The dependent variable for panel (a) is an indicator for whether the index admission occurs at a hospital whose Part A contractor is the same as the jurisdiction's Part B contractor and for panel (b) is an indicator for whether the patient has a claim processed by a contractor firm other than that which processed the claim for the index admission in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Table F1. Effect on Administrative Fragmentation, with Jurisdiction-Month FEs

	(1) Hospital Has Single Contractor	(2) Encounters Multiple Contractors	(3) Has Physician Service Claim from Different Contractor	(4) Has Non-Physician-Service Claim from Different Contractor
$\text{PostReform}_{jt} \times$	0.092***	-0.031**	-0.046*	0.008*
$\text{ReducedFragmentation}_h$	(0.026)	(0.009)	(0.021)	(0.003)
R^2	0.917	0.857	0.899	0.877
Dep. Var. Mean	0.530	0.706	0.481	0.460
Jurisdiction-Month FE	Yes	Yes	Yes	Yes
Admissions	30.1m	30.1m	30.1m	30.1m

Notes: OLS estimates of equation (1) with jurisdiction-month fixed effects. An observation is an admission from 2000–2017. Dependent variable in column (1) is an indicator for the index admission hospital's Part A contractor being the same firm as the jurisdiction's Part B contractor, in column (2) is an indicator for whether the patient has a claim processed by a contractor other than the index hospital's Part A contractor in the 30 days after discharge, in column (3) is an indicator for whether the patient has a physician services claim processed by a different contractor in the 30 days after discharge, and column (4) is an indicator for whether the patient has any other claim processed by a different contractor in the 30 days after discharge. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure F2. Effect on Administrative Outcomes: Denials, with Jurisdiction-Month FEs



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with jurisdiction-month fixed effects. Period $e = -7$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Table F2. Administrative Outcomes: Denials and Billing Delays, with Jurisdiction-Month FEs

	(1) Denial Rate	(2) Encounters Any Denials	(3) Physician Services Denial Rate	(4) Non-Physician-Services Denial Rate	(5) Days From Procedure to Bill Paid
$\text{PostReform}_{jt} \times$	-0.003	-0.001	-0.003	-0.001	-0.035
$\text{ReducedFragmentation}_h$	(0.002)	(0.004)	(0.002)	(0.002)	(0.049)
R^2	0.413	0.588	0.407	0.425	0.557
Dep. Var. Average	0.106	0.588	0.109	0.094	8.917
Coeff. as % of Mean	-2.37	-0.28	-2.60	-1.35	-0.39
Jurisdiction-Month FE	Yes	Yes	Yes	Yes	Yes
N	30.1m	30.1m	30.1m	30.1m	30.1m

Notes: OLS estimates of equation (1) with jurisdiction-month fixed effects. An observation is an admission from 2000–2017. Dependent variable in column (1) is the share of claims for care rendered in the 30 days after discharge that is denied, in column (2) is an indicator for whether the patient has any claim denied in the 30 days after discharge, in column (3) is the share of physician services claims for care rendered in the 30 days after discharge that is denied, column (4) the share of all other claims for care rendered in the 30 days after discharge that is denied, and in column (5) is the claim-level average number of days from the care being rendered to the claim being processed. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

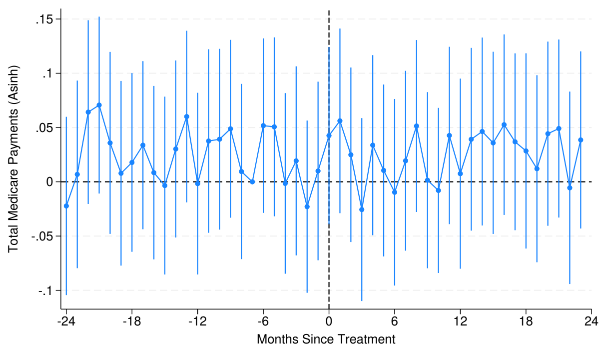
Table F3. Patient Admission-Level outcomes, with Jurisdiction-Month FEs

	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$\text{PostReform}_{jt} \times$	0.002	-0.009	-0.004	-0.000
$\text{ReducedFragmentation}_h$	(0.012)	(0.006)	(0.004)	(0.001)
R^2	0.591	0.577	0.613	0.529
Dep. Var. Mean	7.667	2.862	2.209	0.198
Coeff. as % of Mean	-	-	-	-0.12
N	30.1m	30.1m	30.1m	30.1m

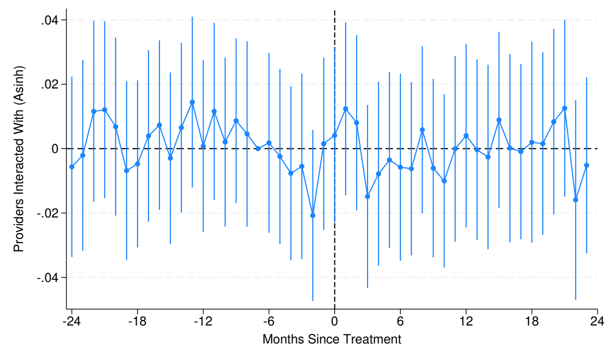
Notes: OLS estimates of equation (1) with jurisdiction-month fixed effects. An observation is an admission from 2000–2017. Dependent variable in column (1) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in column (2) is the inverse hyperbolic sine of the total number of claims in the 30 days after discharge, in column (3) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in column (4) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure F3. Patient Admission-Level Outcomes, with Jurisdiction-Month FEs

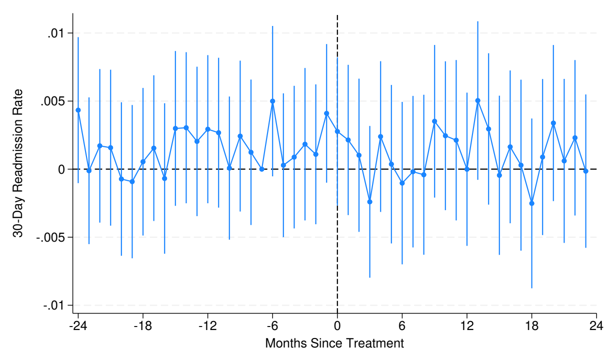
(a) Total Spending



(b) Number of Providers Encountered



(c) 30-day Readmissions

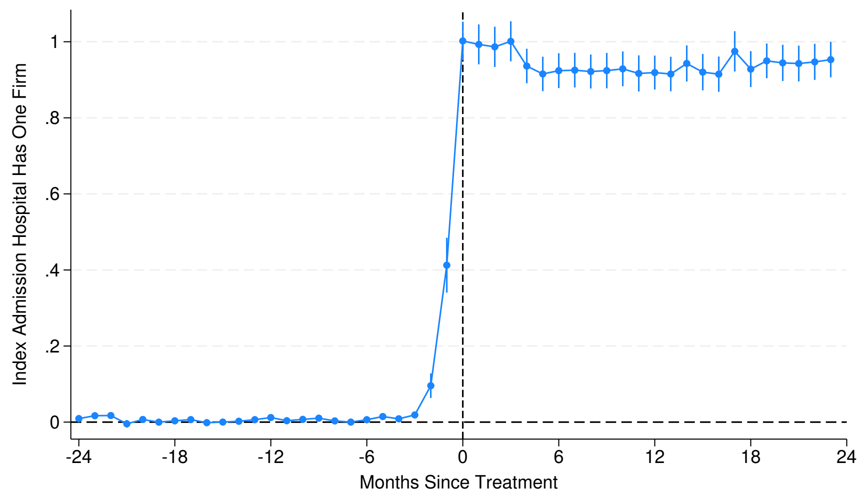


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with jurisdiction-month fixed effects. Period $e = -7$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in panel (b) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in panel (c) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

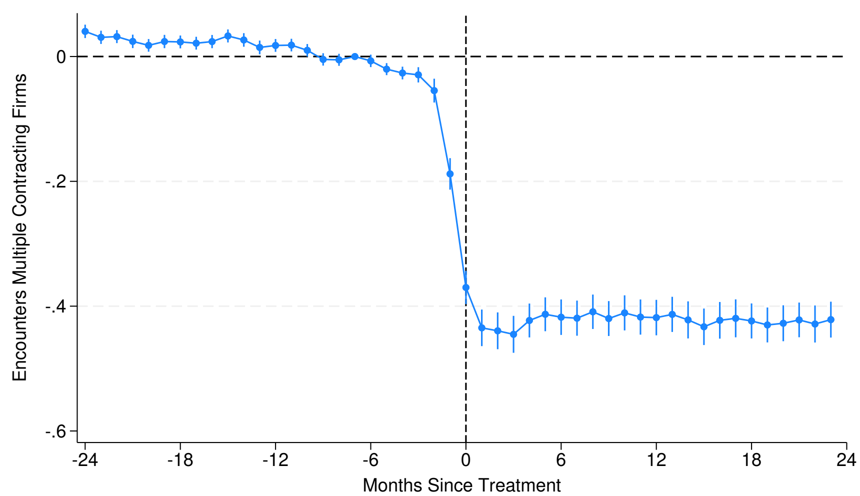
G Hospitals without Choice of Fragmentation

Figure G1. Effect on Administrative Fragmentation, Hospitals without Choice of Fragmentation

(a) Share of Admissions at Hospitals with Single Contractor for Part A and B



(b) Share of Admissions with Encounters Multiple Contractors



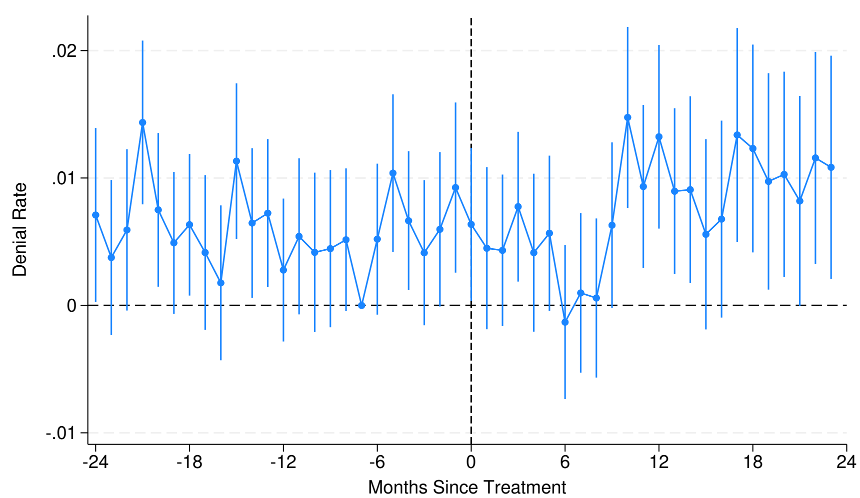
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The sample of hospitals with reduced fragmentation is limited to those in jurisdiction with a Part B contractor that did not also serve as a Part A contractor. The dependent variable for panel (a) is an indicator for whether the index admission occurs at a hospital whose Part A contractor is the same as the jurisdiction's Part B contractor and for panel (b) is an indicator for whether the patient has a claim processed by a contractor firm other than that which processed the claim for the index admission in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Table G1. Effect on Administrative Fragmentation, Hospitals without Choice of Fragmentation

	(1) Hospital Has Single Contractor	(2) Encounters Multiple Contractors	(3) Has Physician Service Claim from Different Contractor	(4) Has Non-Physician-Service Claim from Different Contractor
$\text{PostReform}_{jt} \times$	0.938***	-0.440**	-0.723***	-0.025***
$\text{ReducedFragmentation}_h$	(0.023)	(0.013)	(0.019)	(0.004)
R^2	0.703	0.549	0.660	0.713
Dep. Var. Mean	0.814	0.580	0.256	0.446
Admissions	12.3m	12.3m	12.3m	12.3m

Notes: OLS estimates of equation (1). The sample of hospitals with reduced fragmentation is limited to those in jurisdiction with a Part B contractor that did not also serve as a Part A contractor. An observation is an admission from 2000–2017. Dependent variable in column (1) is an indicator for the index admission hospital's Part A contractor being the same firm as the jurisdiction's Part B contractor, in column (2) is an indicator for whether the patient has a claim processed by a contractor other than the index hospital's Part A contractor in the 30 days after discharge, in column (3) is an indicator for whether the patient has a physician services claim processed by a different contractor in the 30 days after discharge, and column (4) is an indicator for whether the patient has any other claim processed by a different contractor in the 30 days after discharge. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure G2. Effect on Administrative Outcomes: Denials, Hospitals without Choice of Fragmentation



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The sample of hospitals with reduced fragmentation is limited to those in jurisdiction with a Part B contractor that did not also serve as a Part A contractor. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Table G2. Administrative Outcomes: Denials and Billing Delays, Hospitals without Choice of Fragmentation

	(1) Denial Rate	(2) Encounters Any Denials	(3) Physician Services Denial Rate	(4) Non-Physician-Services Denial Rate	(5) Days From Procedure to Bill Paid
$\text{PostReform}_{jt}\times$ $\text{ReducedFragmentation}_h$	0.002	-0.011**	0.003	0.000	-0.125**
	(0.002)	(0.004)	(0.002)	(0.001)	(0.041)
R^2	0.365	0.546	0.358	0.363	0.536
Dep. Var. Mean	0.107	0.430	0.109	0.096	8.766
Coeff. as % of Mean	0.84	-10.67	2.40	0.47	-1.42
Admissions	12.3m	12.3m	12.3m	12.3m	12.3m

Notes: OLS estimates of equation (1). The sample of hospitals with reduced fragmentation is limited to those in jurisdiction with a Part B contractor that did not also serve as a Part A contractor. An observation is an admission from 2000–2017. Dependent variable in column (1) is the share of claims for care rendered in the 30 days after discharge that is denied, in column (2) is an indicator for whether the patient has any claim denied in the 30 days after discharge, in column (3) is the share of physician services claims for care rendered in the 30 days after discharge that is denied, column (4) the share of all other claims for care rendered in the 30 days after discharge that is denied, and in column (5) is the claim-level average number of days from the care being rendered to the claim being processed. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

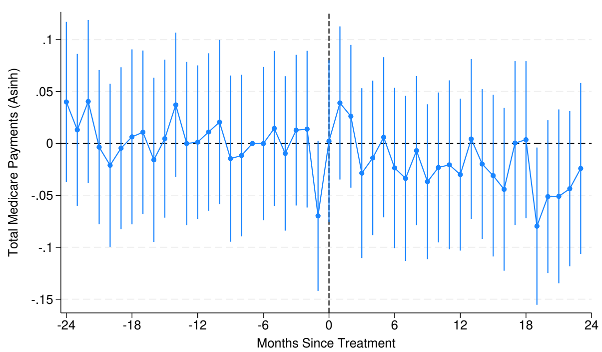
Table G3. Patient Admission-Level outcomes, Hospitals without Choice of Fragmentation

	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$Post_{jt} \times Treat_h$	-0.026*	-0.024***	-0.011**	-0.001
	(0.012)	(0.006)	(0.004)	(0.001)
R^2	0.578	0.548	0.584	0.526
Dep. Var. Mean	7.609	2.848	2.190	0.198
Coeff. as % of Mean	-	-	-	-0.79
N	12.3m	12.3m	12.3m	12.3m

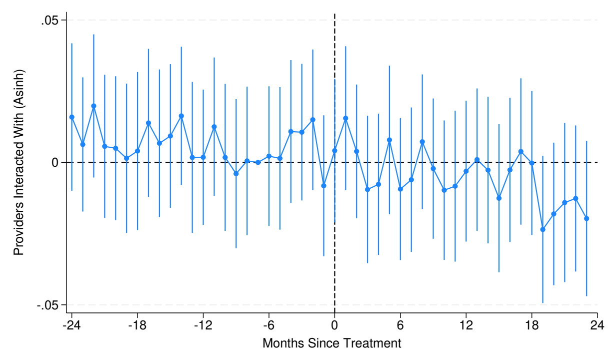
Notes: OLS estimates of equation (1). The sample of hospitals with reduced fragmentation is limited to those in jurisdiction with a Part B contractor that did not also serve as a Part A contractor. An observation is an admission from 2000–2017. Dependent variable in column (1) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in column (2) is the inverse hyperbolic sine of the total number of claims in the 30 days after discharge, in column (3) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in column (4) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. Standard errors are clustered at the hospital level. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure G3. Patient Admission-Level Outcomes, Hospitals without Choice of Fragmentation

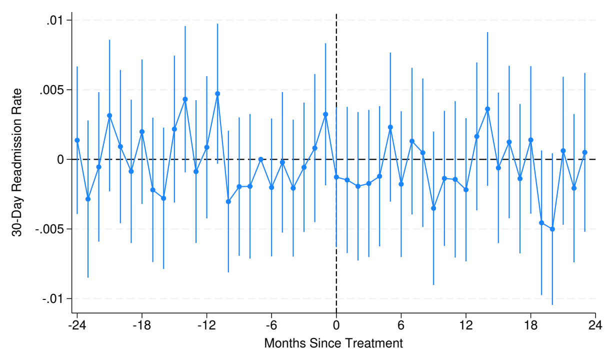
(a) Total Spending



(b) Number of Providers Encountered



(c) 30-day Readmissions

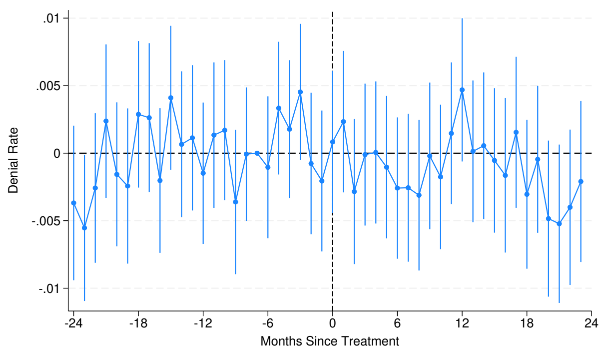


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The sample of hospitals with reduced fragmentation is limited to those in jurisdiction with a Part B contractor that did not also serve as a Part A contractor. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in panel (b) is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge, and in panel (c) is an indicator for having a short-stay hospitalization claim in the 30 days after discharge. An observation is an index admission from 2000–2017. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

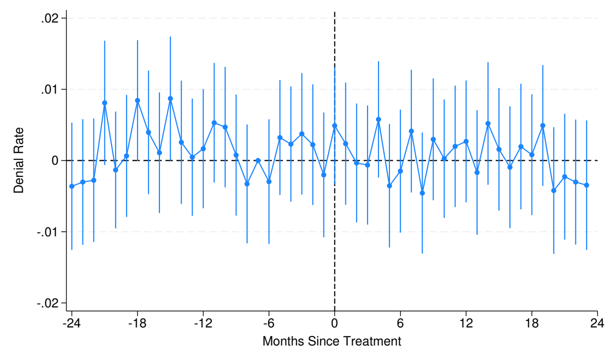
H Heterogeneity by Admission Diagnosis

Figure H1. Effect on Administrative Outcomes: Denials, for Certain Groups of DRGs

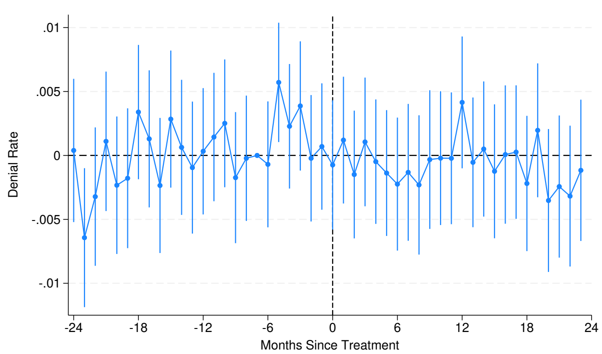
(a) Top 25% of Spending



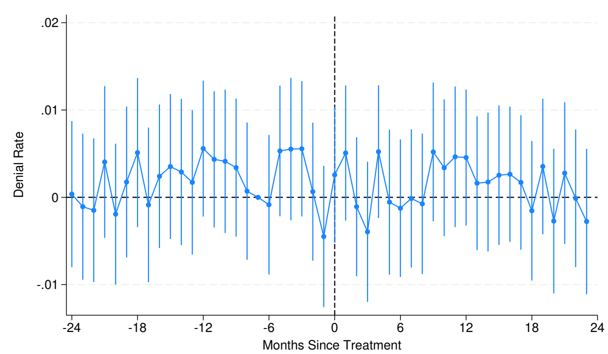
(b) Top 10% of Spending



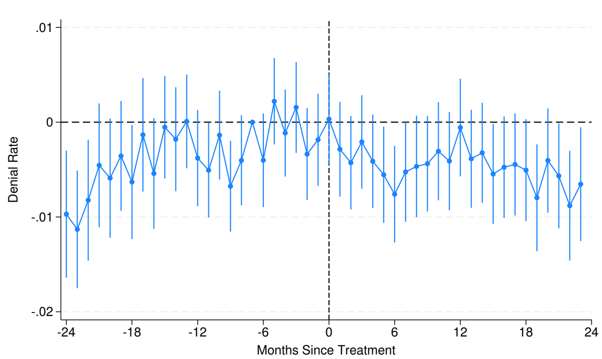
(c) Top 25% of Providers Encountered



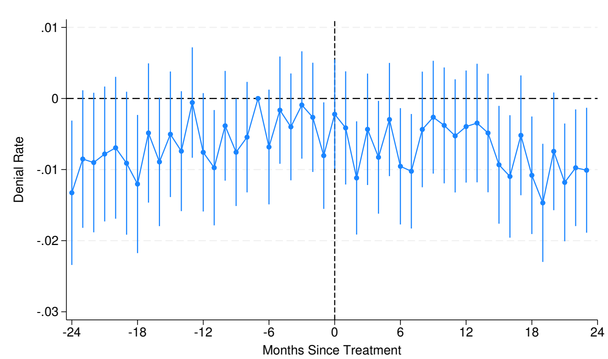
(d) Top 10% of Providers Encountered



(e) Top 25% of Denials

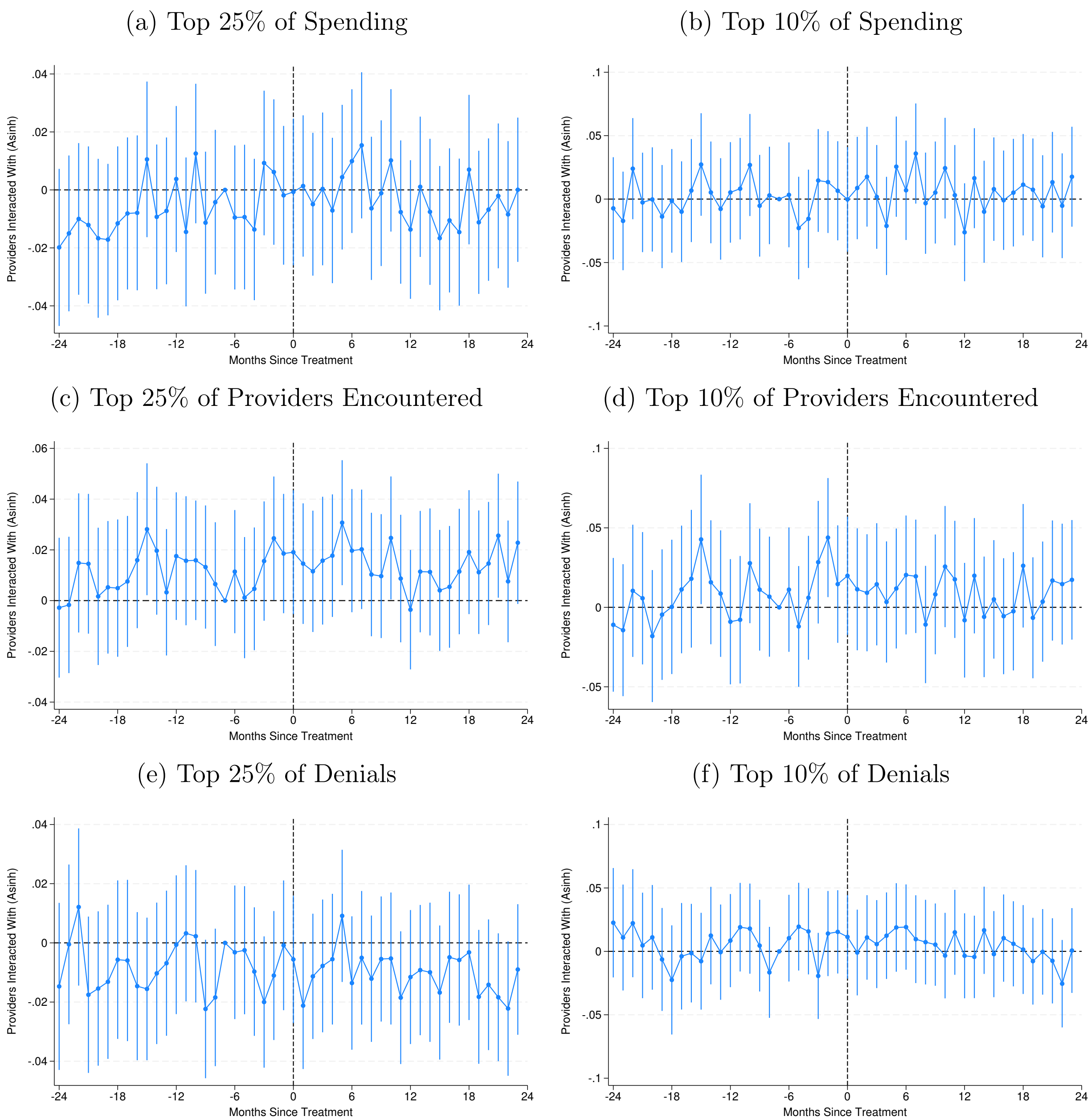


(f) Top 10% of Denials



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. In panel (a) the sample is limited to admissions with a DRG in the top 25% of spending over the 30 days after discharge and in panel (b), it is limited to the top 10% of DRGs by spending. In panels (c) and (d), the sample is limited to the top 25% and 10% of DRGs by the number of unique providers encountered in the 30 days after discharge. In panels (e) and (f), the sample is limited to the top 25% and 10% of DRGs by denial rate over the 30 days after discharge. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

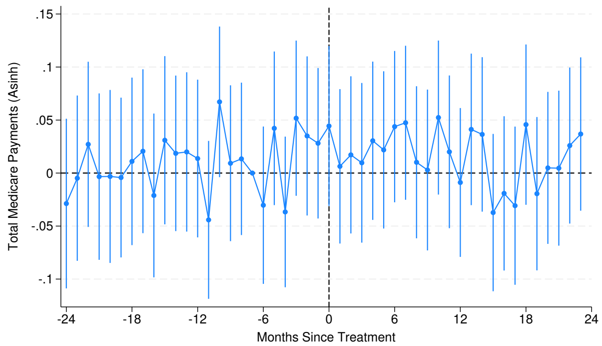
Figure H2. Effect on Patient Admission-Level Outcomes: Providers Encountered, for Certain Groups of DRGs



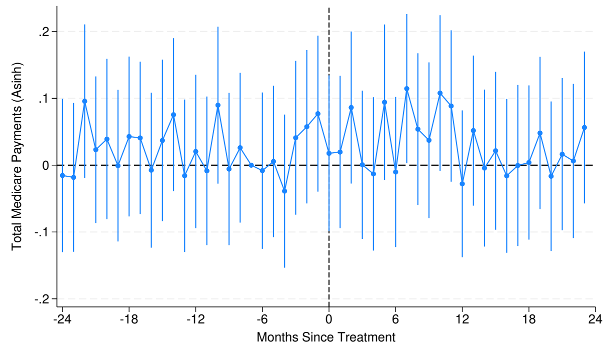
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The dependent variable is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge. An observation is an index admission from 2000–2017. In panel (a) the sample is limited to admissions with a DRG in the top 25% of spending over the 30 days after discharge and in panel (b), it is limited to the top 10% of DRGs by spending. In panels (c) and (d), the sample is limited to the top 25% and 10% of DRGs by the number of unique providers encountered in the 30 days after discharge. In panels (e) and (f), the sample is limited to the top 25% and 10% of DRGs by denial rate over the 30 days after discharge. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

Figure H3. Effect on Patient Admission-Level Outcomes: Spending, for Subsets of DRGs

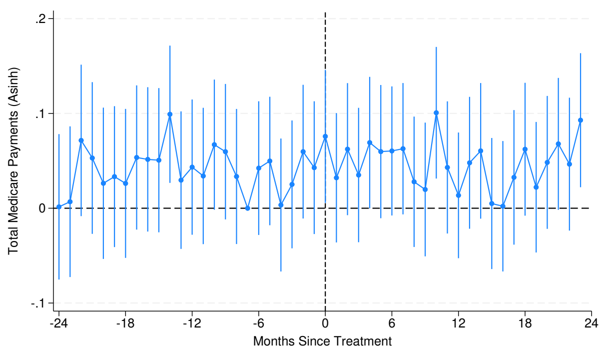
(a) Top 25% of Spending



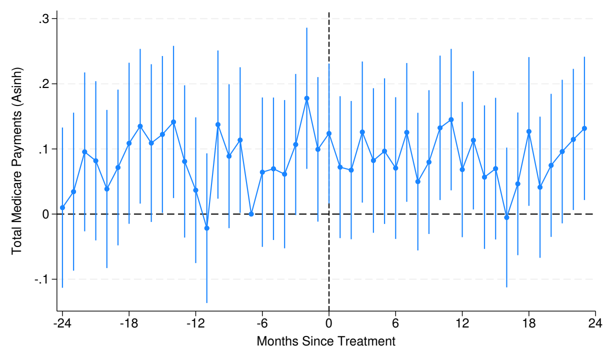
(b) Top 10% of Spending



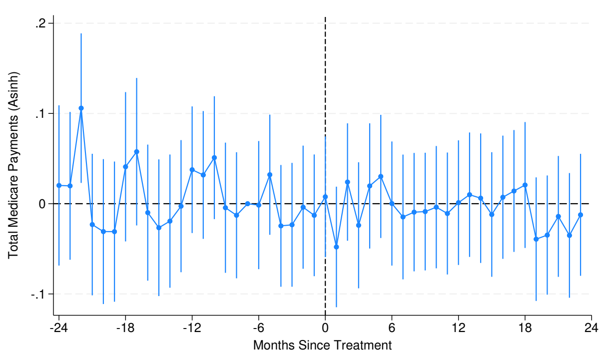
(c) Top 25% of Providers Encountered



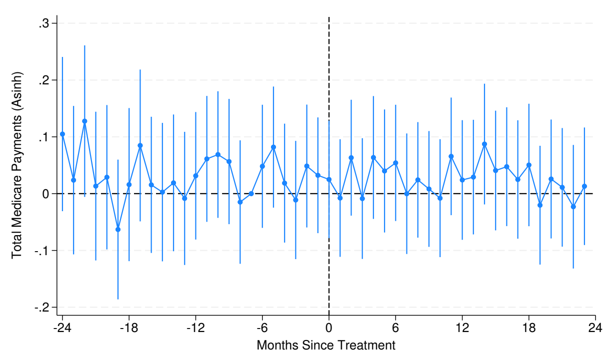
(d) Top 10% of Providers Encountered



(e) Top 25% of Denials



(f) Top 10% of Denials



Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The dependent variable is the inverse hyperbolic sine of the number of unique providers treated by in the 30 days after discharge. An observation is an index admission from 2000–2017. In panel (a) the sample is limited to admissions with a DRG in the top 25% of spending over the 30 days after discharge and in panel (b), it is limited to the top 10% of DRGs by spending. In panels (c) and (d), the sample is limited to the top 25% and 10% of DRGs by the number of unique providers encountered in the 30 days after discharge. In panels (e) and (f), the sample is limited to the top 25% and 10% of DRGs by denial rate over the 30 days after discharge. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.