Administrative Fragmentation in Health Care*

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Abstract

The fragmentation of the US health care system is often thought to contribute to its high levels of spending and inefficiency. This paper investigates the impact of reducing "administrative fragmentation" – the fragmentation of billing and administrative processes – on provider behavior, health care spending, and patient outcomes. We leverage the decentralized nature of the Medicare program, which is administered by a patchwork of contractor firms who process claims and make coverage determinations across different geographic regions and service types. We study a reform that consolidated the administrative processes for inpatient and outpatient care, and consider the effects on followup care for patients recently discharged from a hospital stay. Leveraging the staggered implementation of the reform and differential exposure across hospitals based on their pre-reform contractors, we find that the reform substantially reduced administrative fragmentation and slightly reduced claim denial rates, but had no impact on health care spending, utilization, or patient health outcomes. Specifically, the reform did not alter post-discharge care or spending, or rehospitalizations. Additionally, there is no evidence of changes in administrative costs or hospital administrative technology adoption. Our results suggest that reducing administrative fragmentation alone is unlikely to meaningfully improve efficiency or patient outcomes in the US health care system.

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1 Introduction

One defining feature of the US health care system is its administrative complexity, due in part to the fragmentation of health insurance across multiple public and private payers. This generates "administrative fragmentation": in order to pay for care, health care providers and patients must navigate a patchwork system comprised of different insurers, each of which has separate billing systems and administrative processes. This fragmentation is prevalent not only across but also within individuals – 13 percent of Americans and 52 percent of Americans over 65 have two or more sources of health insurance (Mykyta et al., 2023). Since there is often little coordination across payers on these processes, it usually falls on patients and providers to ensure that their claims are being processed and paid for correctly.

Administrative fragmentation raises two key concerns. First, it could add to unnecessary administrative costs within the health care sector. The US has much higher health care administrative costs than comparable countries, and many have pointed to inefficiencies arising from fragmentation as one potentially addressable driver (Cutler and Ly, 2011; Gottlieb et al., 2018; Cutler, 2020). Indeed, one argument often made in favor of moving to a single-payer system is that there would be substantial cost savings from eliminating the administrative costs and duplication arising from fragmentation across multiple payers. 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 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 be deterred from providing care or even refuse to see certain patients altogether (Li, 2023; Dunn et al., 2024). In contrast to utilization management tools, which are administrative hassles designed specifically to target wasteful care, the hassles arising fragmentation are unlikely to be well-targeted based on value and instead will broadly apply to all care (Brot-Goldberg et al., 2023; Gandhi and Shi, 2024). Thus, any distortions to care arising from fragmentation are a particular cause for concern.

These concerns have motivated recent policy efforts to improve the efficiency of the health care system by streamlining 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,[1] and establishing a national clearinghouse for bill submission (Cutler, 2020).

However, it is not clear to what extent efforts to reduce fragmentation would reduce administrative 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 – most notably, in their payment rates and benefits structures. So, directly comparing patients with multiple vs. one payer or comparing providers who work with more or fewer payers would risk conflating the effects of fragmentation with these other across-payer differences.[2]

We address this challenge by leveraging a large Medicare policy reform as a natural experiment that reduced fragmentation in the billing process and consolidated administrative 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 experience with Medicare is shaped by the contractor they are interacting with. The contractors handle most of the 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 coverage denial appeals, and answer provider inquiries. The Medicare program is fairly hands-off in terms of how these firms run their administrative processes. For example, in the time period we study, 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 Medicare, Part A provides their inpatient hospital care benefit (as well as for in-facility post-acute care

[1] 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)

[2] 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, more cumbersome administrative processes, and higher denial rates.

in nursing homes or for home health) while Part B pays for physician services and other outpatient care. 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 different Part A contractors than their jurisdiction's Part B contractor went from having two separate firms processing their inpatient and outpatient claims to relying on a single firm. 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. Since coverage for outpatient care can depend on the details of the patient's inpatient stay,[3] outpatient providers had to coordinate between different contractor firms to ensure their claims had supporting records in order to be approved. If outpatient providers navigating this fragmented billing process face delays and unwarranted denials, this could distort the care they provide. After the reform, this process would be 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 billing processes, minimizing inconsistencies in coverage decisions, and reducing provider and patient hassle.

Our empirical strategy exploits the staggered adoption of the reform across Medicare's different contracting jurisdictions as well as within-jurisdiction, across-hospital differences in the effect of the reform on fragmentation. 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 of the reform on administrative fragmentation varied 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 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

[3]One example is for rehabilitative care after a cardiac-related inpatient stay, as discussed in further detail in Section 2.2 (Code of Federal Regulations, 2023).

by pre-reform fragmentation. We first quantify the direct effect of the reform on administrative fragmentation: a hospital-level indicator for whether the Part A and the Part B contracts are held by the same firm. We find a strong "first stage" effect of 0.7, indicating that 70 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 confirm that the reform also reduced fragmentation using a claims-based measure: whether the patient had claims processed by more than one contractor during and after their inpatient stay. We find that the reform dramatically reduced realized fragmentation, as after the reform this likelihood is halved.

We next consider the effects on measures of administrative efficiency. 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 in-office care. However, the magnitude of the effect is fairly modest – the reform only reduced denials by 2.1 percent relative to baseline. 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 measures of provider administrative costs. We find no evidence that hospital 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 slow down the take-up of 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 a wide range of post-discharge settings and find no evidence of changes in the likelihood or amount of outpatient office, nursing facility, home health, hospital outpatient, and durable medical equipment spending. There are also no meaningful changes in patient health, as measured by 30-day readmissions.

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 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 and the resulting administrative hassles. In contrast to work which has found that provider behavior is highly sensitive to increases in administrative hurdles (Eliason et al., 2021; League, 2023; Brot-Goldberg et al., 2023; Gandhi and Shi, 2024; Dunn et al., 2024; Shi, 2024), we find that reducing the *fragmentation* of these administrative hurdles has no meaningful effect on administrative costs or provider behavior. We also contribute to the literature on 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 billing to multiple payers, holding fixed the characteristics of the coverage itself. Finally, our work contributes to the literature on the effects of fragmentation of the health care system, 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. While the government bears all actuarial risk, sets prices for each procedure, and determines the vast majority of coverage policies, these firms are responsible for the day-to-day administrative operations of Medicare. 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 (CMS, 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 that while 59 percent of procedure codes were

subject to a contractor-specific billing rule by at least one contractor, only 24 percent were subject to a billing rule with every contractor, meaning there was substantial variation in how Medicare's statutory standards are interpreted and implemented by different contractors (Levinson, 2014). Furthermore, different contractors have historically operated 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 interpretation of billing 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 and Part B 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,[4] 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.[5] 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.

Due to concerns with this arrangement, the Medicare Modernization Act of 2003 reformed Medicare contracting such that each region would have a single contractor processing both Part A and Part B claims, called a "Medicare Administrative Contractor" (MAC).[6] These contractors 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. Policymakers had high hopes for the reduction in administrative hassles associated with the reform, predicting "less [administrative] staff time for providers as a result of interacting with fewer contractors" (Leavitt, 2005), "improved provider education and

[4]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).

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

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

training" on billing rules (Lathroum, Sue, 2006), and increased coordination from contractor assistance "with obtaining information on behalf of patients about items or services received from another provider" (Chicoine, Julie E., 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 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).

To further illustrate the potential reductions in administrative costs and increased coordination of care coming from the reform, consider the following example.[7] A Medicare beneficiary is hospitalized for congestive heart failure before being discharged and referred to a cardiologist for follow-up care and monitoring. Medicare will only cover rehabilitative services rendered by the cardiologist if they occur after a qualifying inpatient stay for a specific set of diagnoses (Code of Federal Regulations, 2023). Prior to the reform, the cardiologist could face an unwarranted claim denial or a payment delay if the hospital's Part A claim for the inpatient stay and the cardiologist's Part B claim for outpatient follow-up care were by processed different firms. This could arise, for example, if the Part B contractor could not verify the patient's stay due to mismatched records or miscommunication with the Part A contractor. In this case, the burden falls on the cardiologist to track down the documentation of the inpatient stay that justifies the outpatient bill. After the reform, the same firm processes both the Part A and Part claims and will have immediate access to accurate records for both. Furthermore, should an issue arise with the claim, the cardiologist would have a single point of contact for resolving them, rather than having to communicate with each of the Part A and Part B contractors separately.

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 are originally the same firm, the plurality of hospitals – though not all – would *not* have faced a reduction in 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.[8] Panel (c) shows the planned final contracting jurisdictions after the reform

[7] Leavitt (2005) provides several additional examples in a Report to Congress about the reform, including of how the reform could reduce patient-facing administrative hassles as well.

[8] 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

was implemented.[9]

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 1 panel (a) plots the share of hospitals in a state that were expected to see a reduction in 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. Panel (b) illustrates 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.

3 Data and Sample Construction

Our main source of data is the Medicare claims data covering inpatient stays and skilled nursing stays (MedPAR), Outpatient claims, Carrier (i.e, in-office physician care) claims, Home Health Agency claims, and Durable Medical Equipment claims from 2000 to 2017. 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.

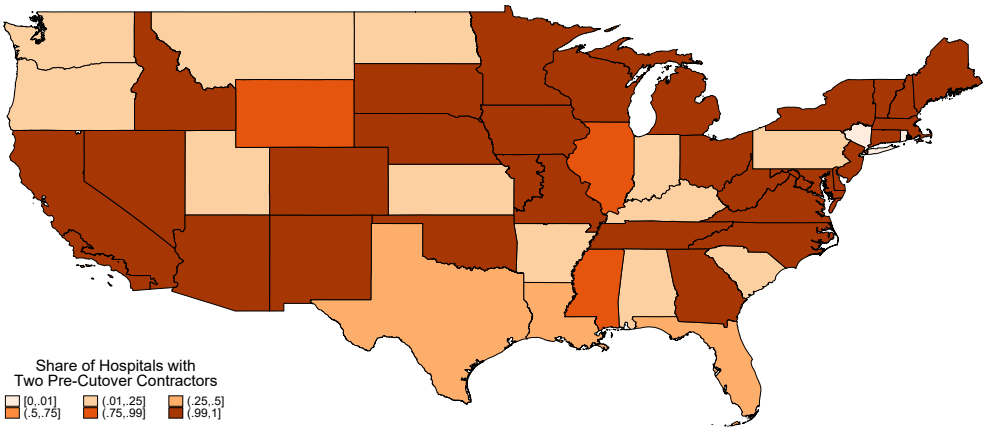
We restrict our analyses to traditional Medicare patients discharged from hospitals after index admissions, which are defined as inpatient stays for patients who have not had another admission in the last 30 days. We use index admissions instead of all admissions in order to use the 30-day rehospitalization 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, number of unique providers seen, and number of unique contractors. These measures are constructed both separately for different claim types as well as combined. For analyses that rely on the Carrier file (including admission-level totals), we restrict our analysis to the 20% sample of beneficiaries as this is our Carrier

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).

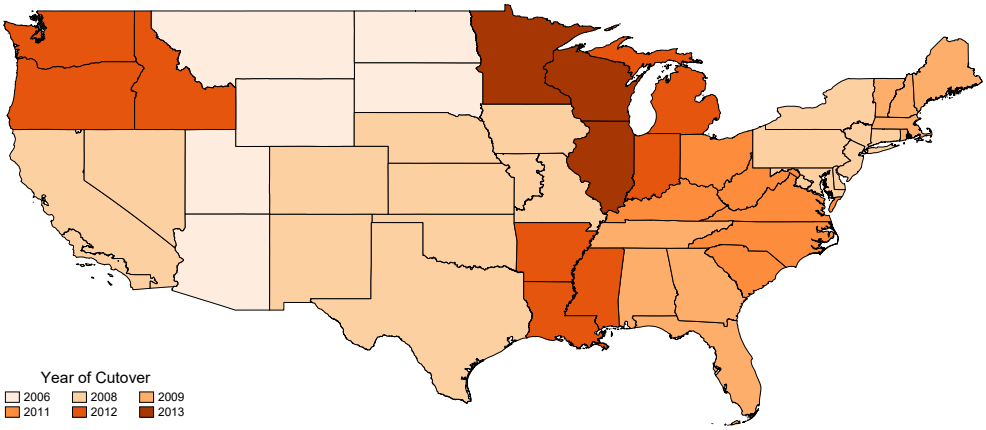
[9]Ultimately, the contracting jurisdictions were not consolidated as much as planned due to "the impact that further consolidations may have on competition among the MACs" (Centers for Medicare & Medicaid Services, 2014).

Figure 1. Map of Incidence of the Reform

(a) Share of Hospitals Facing Reduced 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 claims for the hospital is not the same firm as the Part B contractor assigned to that jurisdiction. Panel (b) plots year the MAC transition was implemented in each jurisdiction.

file sample, 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.[10] 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-

[10]Hospital characteristics are determined using Provider of Services (POS) data derived from Medicare certification applications. We are unable to determine the pre-reform Part A contractor for hospitals that exit before or enter after they are exposed to the reform, or have so few admissions in the two years before the reform that the contractor could not be reliably determined.

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 to identify each hospital's treatment status, we use all of a hospital's inpatient claims in MedPAR (not just of index admissions) and assign the modal contractor listed as processing inpatient claims as that hospital's Part A contractor for a given month. Specifically, treated hospitals are those that had a different Part A contractor from their jurisdiction's Part B contractor in the period 12-24 months prior to the date the jurisdiction was subject to the reform, while control hospitals are those that had the same Part A contractor as their jurisdiction's Part B contractor in that period.

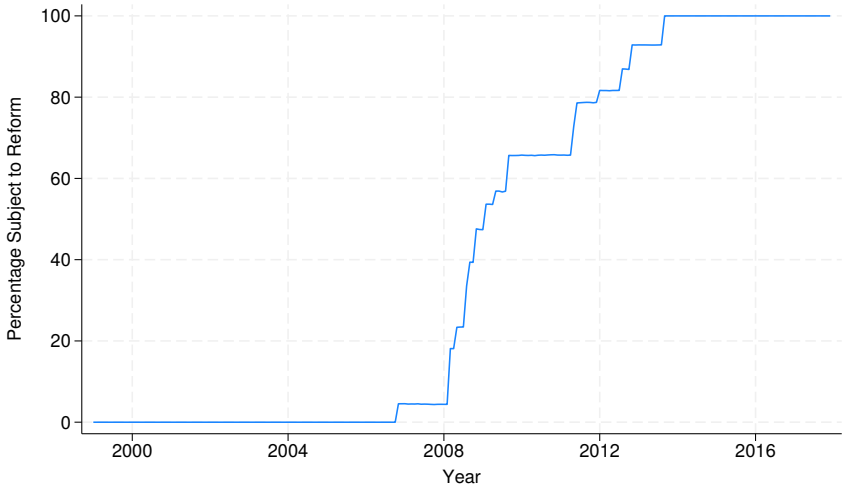
Figure 2 panel (a) plots the time series of reform implementation status at the jurisdiction-level. No jurisdictions were subject to the reform prior to 2006, after which it was implemented in a staggered manner until all jurisdictions were treated by 2014. Figure 2 panel (b) plots the share of hospitals 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 contractor does not match the region's Part B are ones that have individually opted to remain with their legacy Part A contractor or are part of a cross-jurisdiction chain and have chosen to have their claims processed by the Part A contractor covering their home office.

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 (HIMSS) database for 2002-2017. We supplement this hospital-level data with 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.

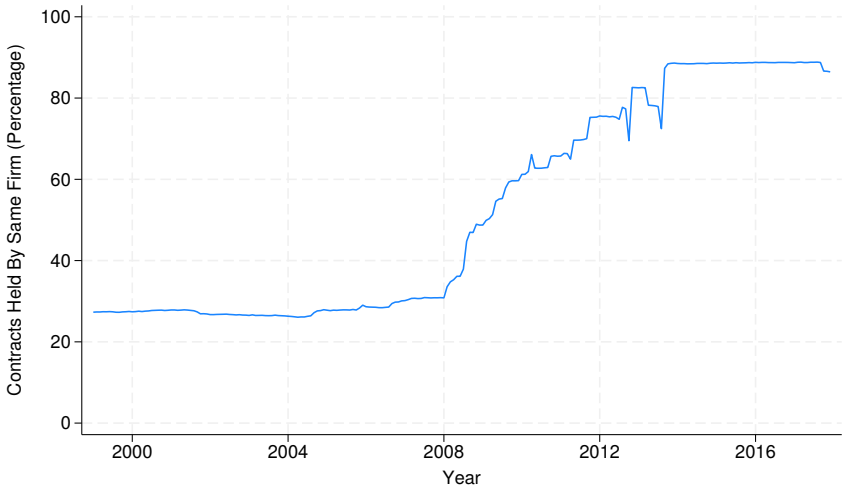
Table 1 presents baseline summary statistics for treated hospitals, who had different Part A and B contractors prior to when the reform began in 2006, and control hospitals, whose Part A contractor matched the jurisdiction's Part B contractor. While treatment hospitals are smaller and more likely to be for-profit, their patients are similar in terms of demographics, spending, 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, 91 percent of patients in treated hospitals interact with at least two contractors during and after their stay, while only 49 percent

Figure 2. Time Series of Reform Exposure, 1999-2017

(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 1999 to 2017. Panel (b) plots the share of hospitals where the hospital's Part A contractor matches the region's Part B contractor. The identity of the Part A contractor is the modal contractor processing inpatient stays in MedPAR in each hospital-month, while the identity of the Part B contractor is derived from the modal contractor processing Carrier claims in each jurisdiction-month.

of those in control hospitals do.[11] This confirms that our design is captures a meaningful

[11]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 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 and home health, 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

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

	(1) Treatment	(2) Control	(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	0.910	0.485	0.776
Denial Rate	0.090	0.088	0.089
Hospital Admin. Cost (Million $)	15.34	15.56	15.41
Hospital Admin. Salaries (Million $)	5.467	5.793	5.567
Hospital Share of Health IT Used	0.543	0.551	0.546
Downstream Outcomes			
Medicare Payments ($)	5392	5558	5444
Providers Encountered	5.69	5.70	5.69
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 ("Treatment") or if they were the same firm ("Control"). 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.

jurisdiction is exposed to the reform, some control hospitals may have changed contractors from 2000–2005

amount of administrative fragmentation in the pre-reform period.

4 Empirical Strategy

Our empirical strategy leverages the staggered timing of the transition across jurisdictions and the differential effects of the reform on fragmentation across hospitals 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. Our main estimating equation is:

$$Y_{ihjt} = \alpha Window_{jt} + \gamma Post_{jt} + \delta Window_{jt} \times Treat_h + \beta Post_{jt} \times Treat_h + \eta_h + \eta_t + \varepsilon_{ihjt}, \quad (1)$$

where Y_{ihjt} is the outcome for patient i treated at hospital h in jurisdiction j in period t. $Window_{jt}$ is an indicator for whether the observation falls within the treatment window around the implementation of the reform in a jurisdiction;[12] our main specification uses a window of 24 months prior to and after the reform and excludes the 6 months prior to the reform due to evidence of anticipation effects.[13] $Post_{jt}$ is an indicator for the observation being in the post-reform period of the treatment window, when jurisdiction j transitions from separate Part A and B contracts to a single combined contract. $Treat_h$ is an indicator for whether the hospital had different firms processing its Part A and Part B claims prior to the reform. In other words, $Treat_h$ indicates whether the hospital is expected to face a reduction in administrative fragmentation from the combination of the Parts A and B contracts. 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.

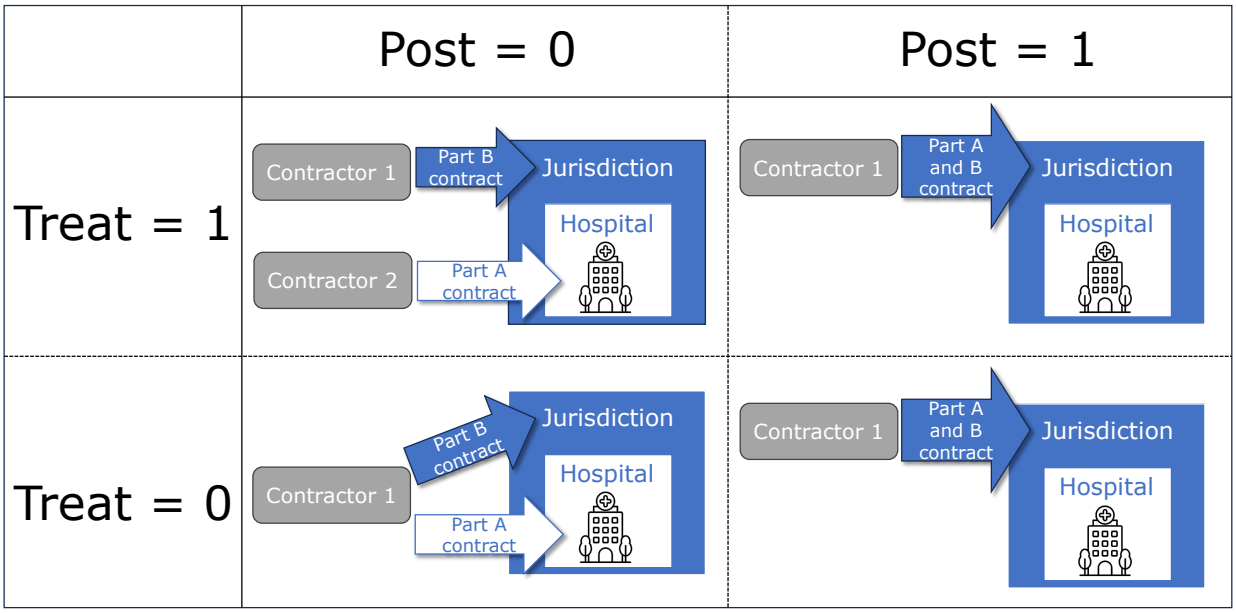
The key parameter of interest is β, which reflects the differential change for patients of hospitals slated to face a reduction in contractors because of the reform, relative to those that should not face such a reduction. Note that we have controlled for any effects of the reform unrelated to the reduction in fragmentation with the inclusion of $Post_{jt}$, captured by the γ coefficient. β thus isolates the effect of the reform's impact on the number of contractors a hospital faces from any other effects of the reform. Furthermore, the estimate represents an "intent-to-treat," as treatment status is assigned based on the number of contractors the hospital had pre-reform, rather than the number they eventually faced post-reform. Thus,

(the period of the table) to the hospital's treatment date.

[12] 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.

[13] 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.

Figure 3. Illustration of Empirical Strategy



Notes: This figure illustrates how $Post_{jt}$ and $Treat_h$ are assigned in Equation 1. When a jurisdiction is exposed to the reform it goes from $Post_{jt} = 0$ to $Post_{jt} = 1$, 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 $Treat_h = 0$ or $Treat_h = 1$ based on whether the reform is expected to reduce fragmentation for them—that is, whether their pre-reform Part A and Part B contracts were originally held by the same or different contractors.

this specification captures the exogenous effect 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 key identifying assumption is the parallel trends assumption: that after accounting for the timing of exposure to the reform and the hospital and time fixed effects, the only differential change between treated and control hospitals is the effect of the reform on administrative fragmentation across contractors. That is, we assume that the potential outcomes of these two types of hospitals would evolved along parallel trends in the absence of the reform. In order to assess any differential trends in outcomes prior to the reform as well as to understand any dynamic effects of reductions in administrative fragmentation, we also estimate:

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

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 the hospital having two contractor firms before the reform and being in a jurisdiction that is exposed to the reform e months (where e denotes event time).[14] 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 will allow 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 the Part B contractor is always assigned at the jurisdiction-level and the Part A contractor is assigned at the jurisdiction-level after treatment, this specifically also 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.

5 Results

Administrative Fragmentation Figure 4 and Table 2 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. 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 0.7 – that is, 70 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 30 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

[14] Note that $Window_{jt} \equiv \sum_{e=-K}^{L} T_{jt}(e)$.

and not the realized effect of the reform on 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
$Post_{jt} \times Treat_h$	0.733***	-0.341***	-0.628***	0.021***
	(0.013)	(0.007)	(0.012)	(0.005)
R^2	0.740	0.662	0.723	0.603
Dep. Var. Mean	0.530	0.680	0.531	0.400
Admissions	30.1m	30.1m	30.1m	30.1m

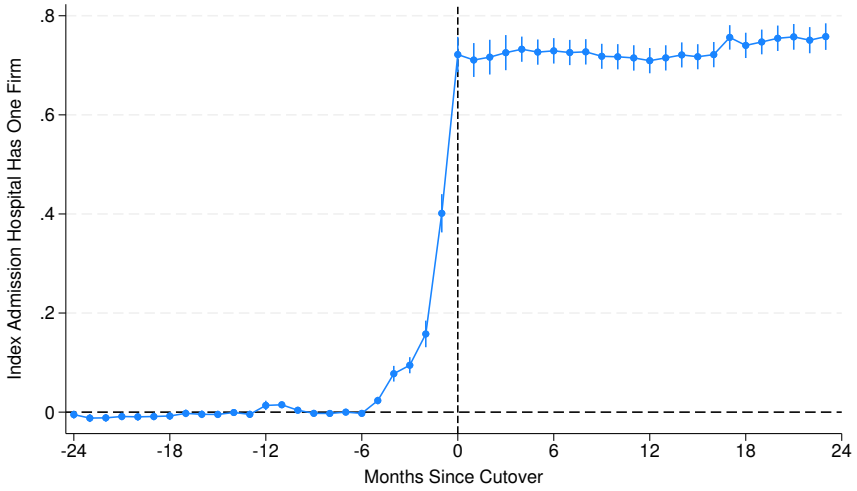
Notes: OLS estimates of equation (1). An observation is an admission from 2000–2017. Dependent variable in column (1) is an indicator for the index admission hospital's Part A contractor being the same firm as the jurisdiction's Part B contractor, in column (2) is an indicator for whether the patient 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). This captures whether the change in the number of contractors covering a patient's hospital and jurisdiction had a practical effect on the administrative fragmentation patients were exposed to, which 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 if the reform had a dramatic effect on whether the Part A and B contractor match at a hospital-level, it could have no practical effect on administrative fragmentation if patients rarely received Part B services after discharge, or if there was an offsetting increase in fragmentation across other Part A providers the patients encounter. 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 34 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 shows that this reduction was driven by a reduction in the likelihood of receiving Part B services from a different contractor, rather than a reduction in the likelihood of encountering multiple Part A contractors. While this latter form of fragmentation could

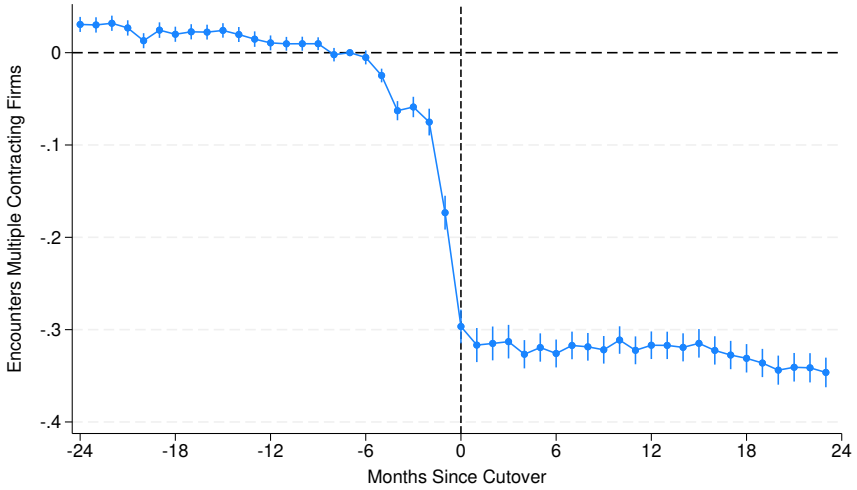
also have been affected by the reform through a reduction in heterogeneity in contractors across Part A providers, we find this was not the case.[15]

Figure 4. Effect on Administrative Fragmentation

(a) Share of Admissions with at Hospitals 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.

[15]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.

Claims-Based Administrative Outcomes Having established that the reform leads to a large reduction in administrative fragmentation for these patients, we next turn to assessing the effect of the reform on the administrative outcomes it most directly targeted. 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, marginally significant reduction in their denial rate – the share of their claims within 30 days of discharge that were denied – that appears to grow over time to become statistically different from zero after 18 months. This reduction is driven by a marginally significant reduction in denials for physician services (column 3) and slightly offset by a small increase in denials for other services (column 4). We find a statistically insignificant reduction in the likelihood of having any claim denied (column 2)

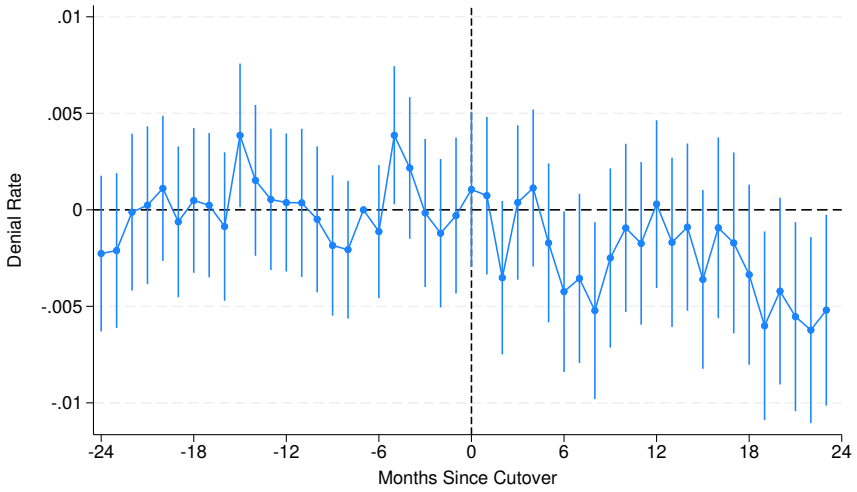
Insofar as there is any reduction in the denial rate, it is small in magnitude: our results imply that a 50 percent reduction in the likelihood of encountering multiple contractors results in only a 2.1 percent reduction in denial rates. By contrast, League (2023) studies the mechanical changes in denial 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. Our results suggest that much of the baseline 11 percent denial rate is not driven by administrative fragmentation across different contractors. Similarly, we do not find evidence that reducing fragmentation reduces bill processing time as measured as the number of days between when the service was rendered and when the bill was processed, which is about 9 days at baseline (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
$Post_{jt} \times Treat_h$	-0.002+	-0.004	-0.003+	0.002*	-0.024
	(0.001)	(0.003)	(0.001)	(0.001)	(0.026)
R^2	0.349	0.484	0.342	0.309	0.509
Dep. Var. Mean	0.111	0.506	0.109	0.128	8.634
Coeff. as % of Mean	-2.13	-0.73	-2.32	1.73	-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. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

Figure 5. Effect on Administrative Outcomes: Denials

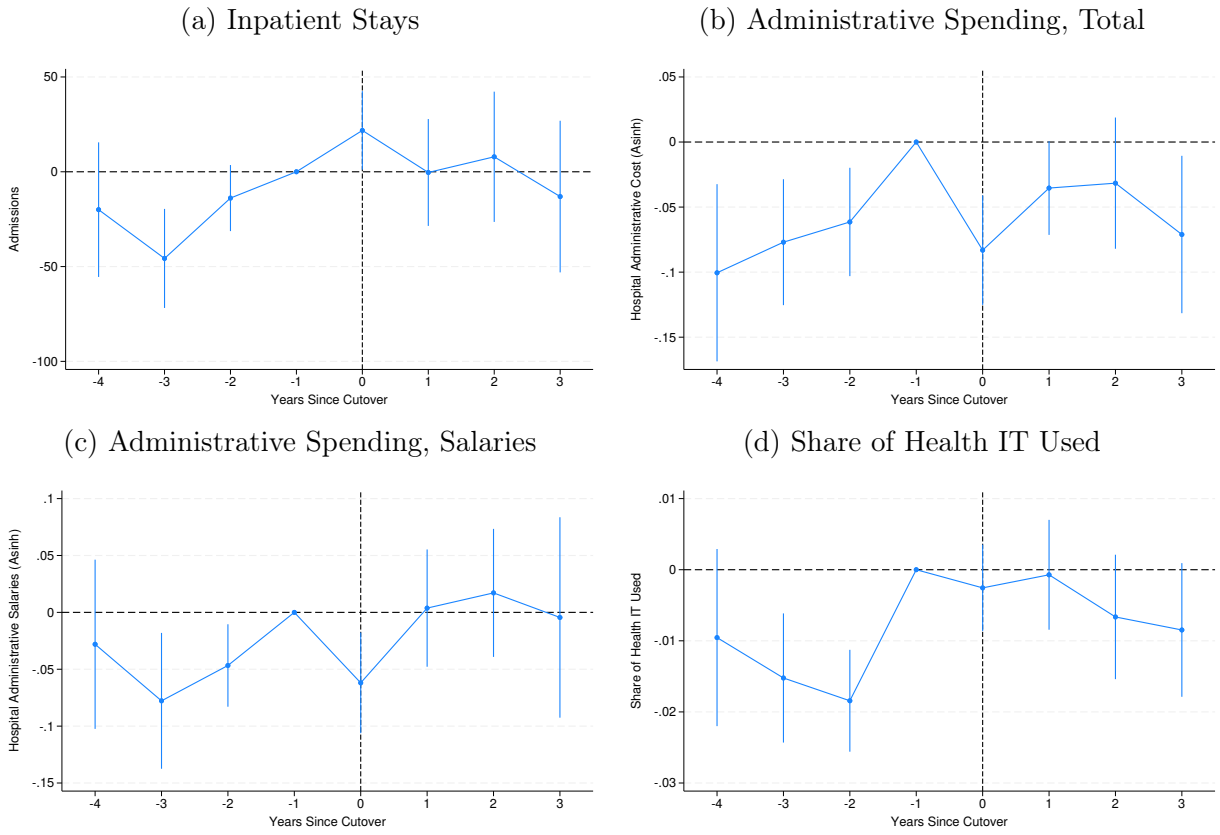


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -7$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. Error bars give the 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 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. Given that prior research suggests that 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 since 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 hospitals at baseline did not incur the bulk of the hassle associated with Part A and B fragmentation, since they primarily billed to the Part A contractor. This is reflected in policymakers' discussion of the reform, which was mostly focused on how it would relieve the administrative burden for individual providers as opposed to 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

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
$Post_{jt} \times Treat_h$	23.98	-0.098	0.006	0.034	0.028	0.006+
	(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 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 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. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

reporting having adopted at least basic 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)	(3)
	Percentage with Basic EHR		
$Post_{jt} \times Treat_h$	5.582	-0.467	6.713
	(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,

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 outpatient providers were reluctant to treat patients due to the difficulty with billing due to the fragmentation, then this 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 effect sizes larger than 1.2 percent in both the positive and negative directions. We also find no effect on the number of providers a patient sees after discharge and are able to rule out out effects over 0.5 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.

Table 6. Patient Admission-Level Outcomes

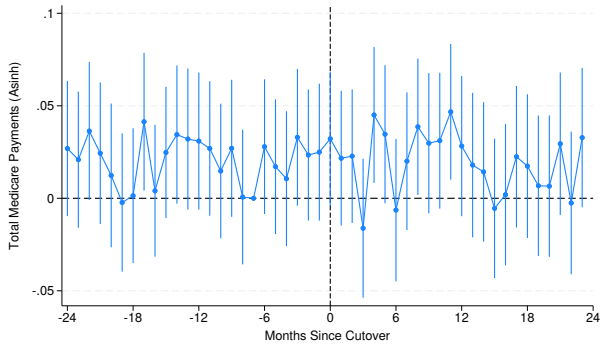
	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$Post_{jt} \times Treat_h$	-0.000	-0.004	0.001	-0.000
	(0.006)	(0.003)	(0.002)	(0.001)
R^2	0.533	0.535	0.565	0.512
Dep. Var. Mean	7.895	2.981	2.263	0.198
Coeff. as % of Mean	-	-	-	-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. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

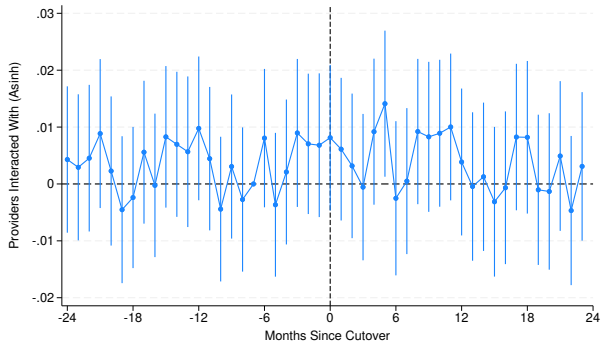
In Appendix Section B1, we disaggregate these results into categories of care: skilled nursing stays, physician office care, home health agency, durable medical equipment, and subsequent hospitalizations. Across all of these categories, we find null effects on reduced administrative fragmentation on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

Figure 7. Patient Admission-Level Outcomes

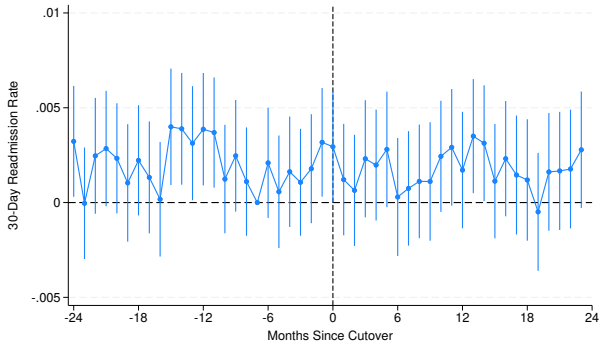
(a) Total Spending



(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. Finally, we confirm that our results are robust to the inclusion of index admission-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.

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 G. In summary, we find no evidence that reducing administrative fragmentation improves health care efficiency among, even the patients likely to gain the most.

6 Conclusion

This paper studies the causal effect of fragmentation in billing and administrative processes in US health care. We study a large reform which reduced administrative fragmentation within the Medicare program. Our difference-in-difference 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 on fragmentation. The reform leads to a large reduction in administrative fragmentation, as measured by the number of separate firms processing a patient's claims. This is associated with a small reduction in denial rates for post-discharge claims but no change in claims processing time. We find no evidence that reducing fragmentation affected administrative costs or the adoption of billing software. We also find precise nulls on downstream utilization, spending, and patient health effects.

Our results suggest that, on their own, efforts to streamline billing and reduce administrative fragmentation may not meaningfully reduce administrative costs in the US health care system. Furthermore, they imply that this fragmentation does not distort patient care or health in a meaningful way. The benefit of the particular reform we study is that many

across-payer characteristics are held fixed, which allows us to isolate the causal effect of *administrative* fragmentation distinctly from other aspects of a multi-payer system. However, this identification advantage also means that our results cannot speak to the effects of efforts to harmonize other aspects of payer fragmentation. Though the Medicare contractors differ in how they process data, interpret billing rules, and interact with providers, because they ultimately serve one payer, they do share many administrative characteristics that would differ across different payers. They enforce federally-set payment rules, share a uniform set of forms and documentation, and are required to follow similar protocols when making documentation requests and coverage determinations. By contrast, when comparing across *different* payers, these dimensions would differ as well. Thus, the effects of consolidating administrative processes for two completely different payers—which has been proposed for the Medicare and Medicaid programs—may differ if the consolidation harmonizes more than the reform we examine. That is, for across-payer administrative consolidation to successfully reduce administrative costs, our results suggest that efforts at harmonizing payment rates, coverage rules, and benefit eligibility will be more important than simply routing all the claims through the same claims processing apparatus. In short, we find that reducing administrative fragmentation at least within traditional Medicare had very limited effects on spending and patient outcomes. However, broader efforts to integrate payments, policies, and administration across payers could yield different effects that warrant further study.

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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.

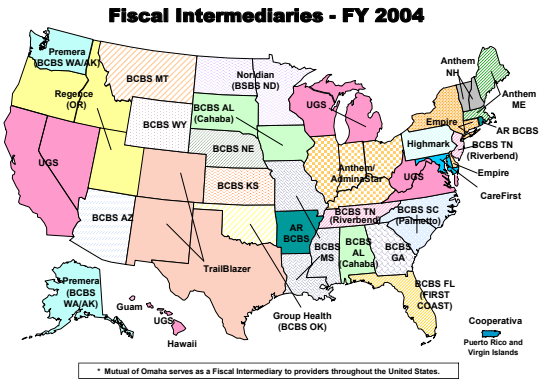
Appendix F provides results with the inclusion of jurisdiction-by-month fixed effects.

Appendix G investigates heterogeneity in the effects by admitting diagnosis.

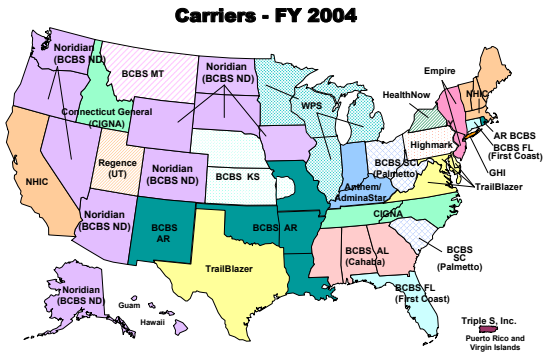
A Additional Figures Referenced in Main Text

Figure A1. Pre- and Post-Reform Contractors

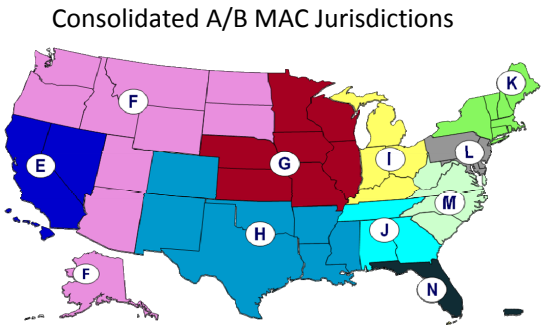
(a) Most Common Pre-Reform Part A Contractors by Jurisdiction



(b) Pre-Reform Part B Contractors



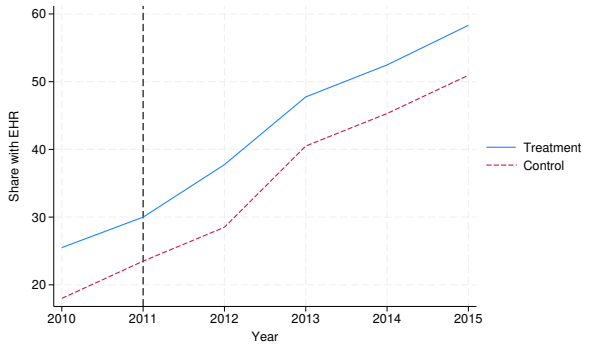
(c) Post-Reform Consolidated Contractors



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

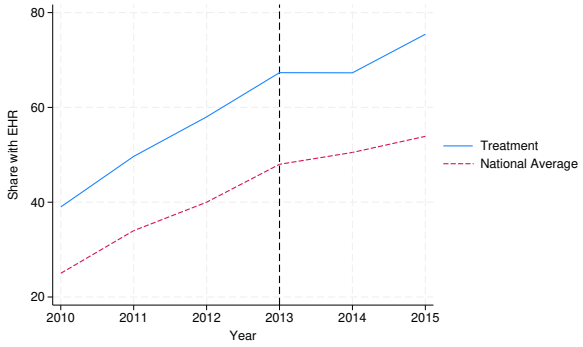
(a) Jurisdictions Subject to Reform in 2011



(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% 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
$Post_{jt} \times Treat_h$	-0.001	-0.109	-0.001	23.27+	-0.002*	21.14
	(0.001)	(3.425)	(0.001)	(11.89)	(0.001)	(14.79)
R^2	0.797	0.799	0.741	0.770	0.548	0.646
Dep. Var. Mean	0.209	531.11	0.218	2061.78	0.264	3135.67
Coeff. as % of Mean	-0.63	-0.02	-0.54	1.13	-0.78	0.67
Admissions	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. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

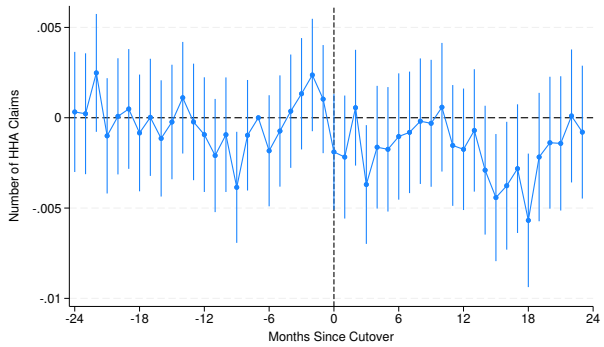
Table B2. Utilization Outcomes: Part B Services

	(1) Durable Medical Equipment Claims	(2) Durable Medical Equipment Spending	(3) Physician Services Claims	(4) Physician Services Spending
$Post_{jt} \times Treat_h$	0.003	-0.507	-0.022	-0.702
	(0.002)	(0.833)	(0.025)	(0.469)
R^2	0.547	0.405	0.524	0.469
Dep. Var. Mean	0.546	97.632	8.114	831.37
Coeff. as % of Mean	0.54	-0.52	-0.27	-0.08
Admissions	152m	152m	30.1m	30.1m

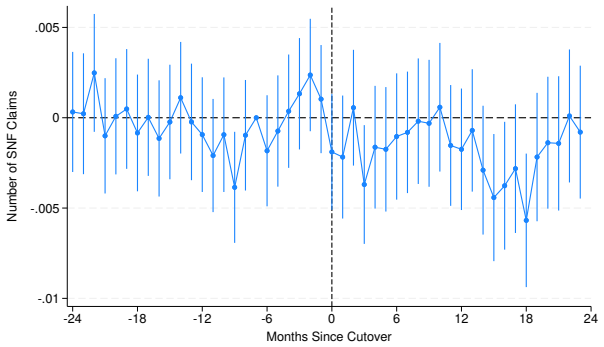
Notes: OLS estimates of equation (1). An observation is an index admission from 2000–2017. The dependent variable in column (1) is the number of durable medical equipment claims, in column (2) is total durable medical equipment spending, in column (3) is the number of physician services claims, and in 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

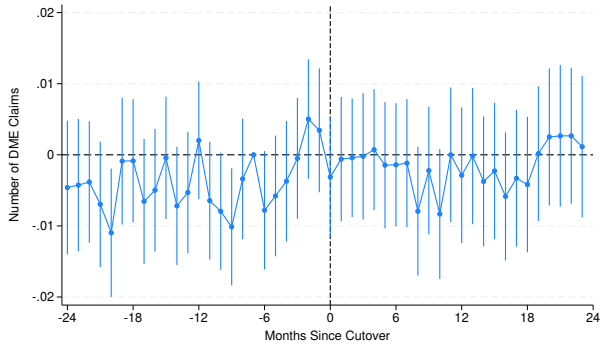
(a) Home Health Agency Claims



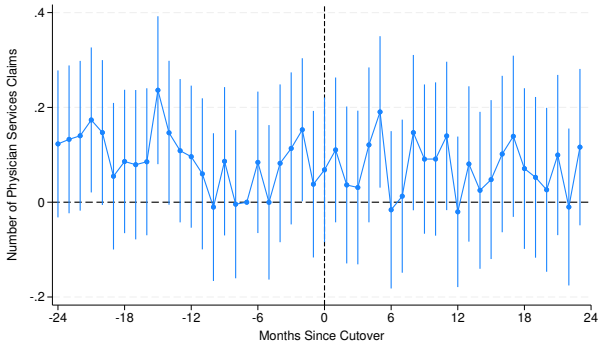
(b) Skilled Nursing Facility Claims



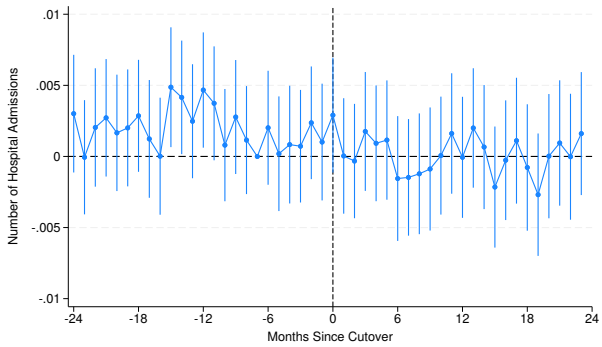
(c) Durable Medical Equipment Claims



(d) In-Office Physician Claims



(e) Hospitalization Claims

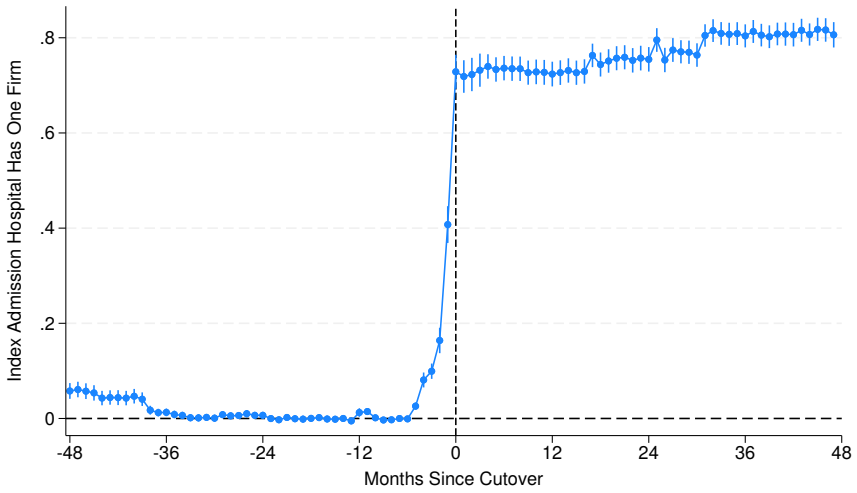


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-48, 47]$. Period $e = -7$ is the reference period. The dependent variable for panel (a) is the number of in-office physician claims (Carrier) within 30 days of discharge, panel (b) is the number of skilled nursing facility claims, panel (c) is the number of home health agency claims, panel (d) is the number of durable medical equipment claims, and panel (e) is the number of hospitalization claims. 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.

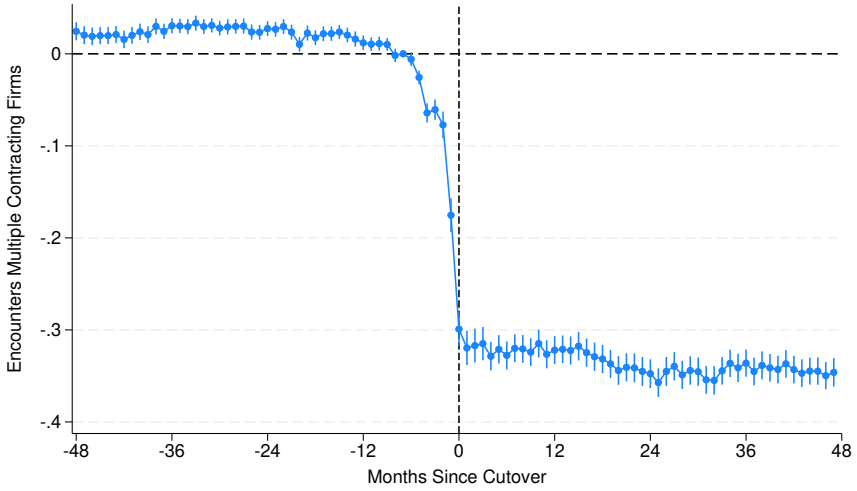
C Longer Treatment Window

Figure C1. Effect on Administrative Fragmentation, Longer Treatment Window

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



(b) Share of Admissions with Encounters Multiple Contractors



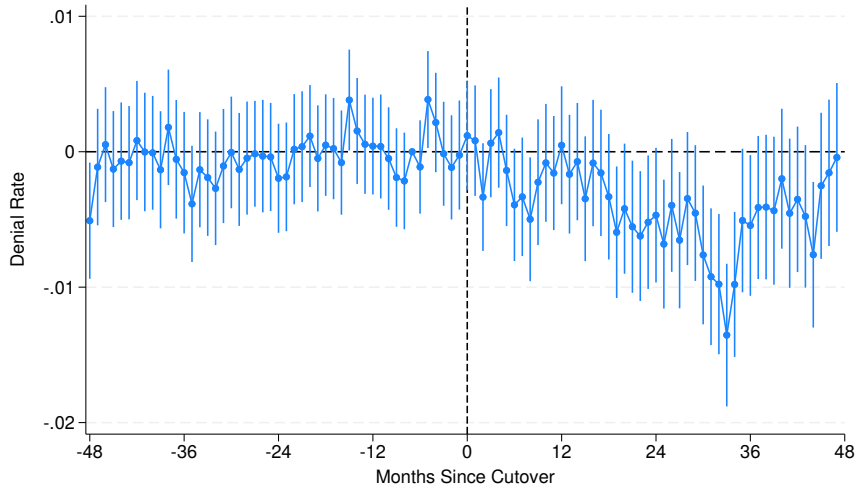
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-48, 47]$. Period $e = -7$ is the reference period. The dependent variable for panel (a) is an indicator for whether the index admission occurs at a hospital whose Part A contractor is the same as the jurisdiction's Part B contractor 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
$Post_{jt} \times Treat_h$	0.751***	-0.356***	-0.657***	0.023***
	(0.012)	(0.007)	(0.011)	(0.005)
R^2	0.777	0.691	0.760	0.621
Dep. Var. Mean	0.530	0.680	0.531	0.400
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
$Post_{jt} \times Treat_h$	-0.003*	-0.006*	-0.004*	0.002*	-0.032
	(0.001)	(0.003)	(0.002)	(0.001)	(0.028)
R^2	0.350	0.485	0.343	0.309	0.509
Dep. Var. Mean	0.111	0.506	0.109	0.128	8.634
Coeff. as % of Mean	-2.93	-1.12	-3.54	1.95	-0.37
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. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

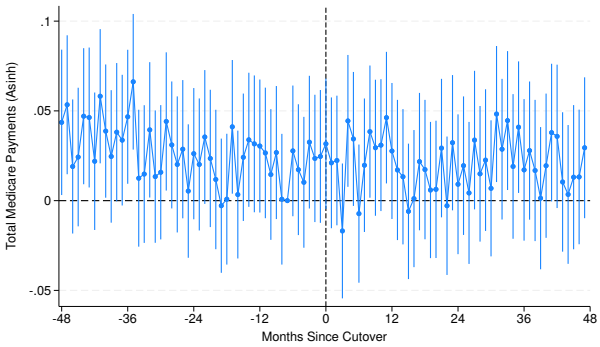
Table C3. Patient Admission-Level Outcomes, Longer Treatment Window

	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$Post_{jt} \times Treat_h$	-0.007	-0.005	0.001	-0.001
	(0.006)	(0.003)	(0.002)	(0.001)
R^2	0.533	0.535	0.566	0.512
Dep. Var. Mean	7.895	2.981	2.263	0.198
Coeff. as % of Mean	-	-	-	-0.53
N	30.1m	30.1m	30.1m	30.1m

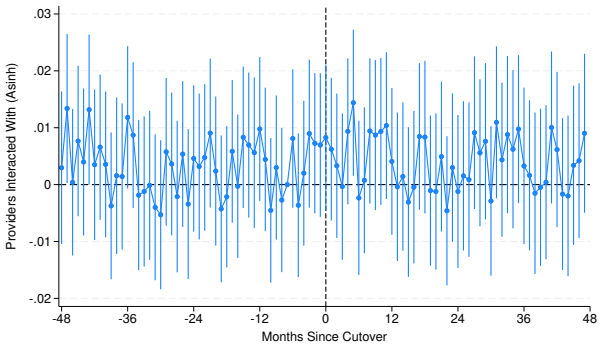
Notes: OLS estimates of equation (1) with $K = 48$ and $L = 47$. An observation is an admission from 2000–2017. Dependent variable in column (1) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in column (2) is the inverse hyperbolic sine of the total number of 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

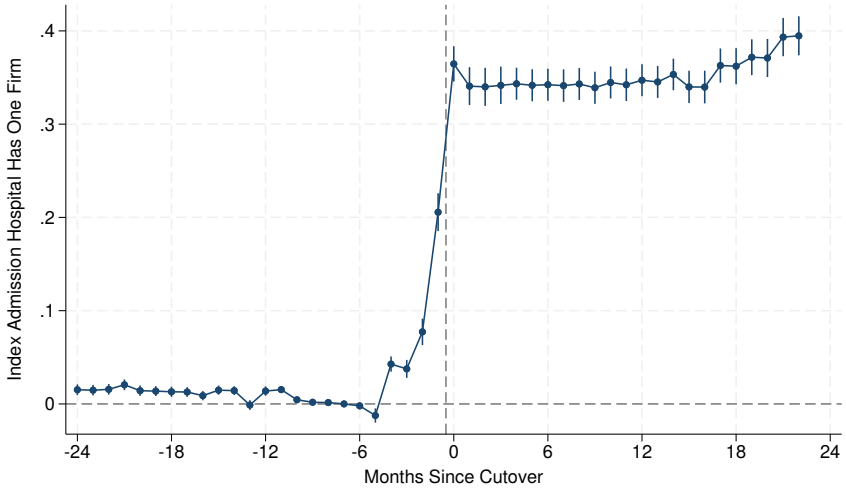


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-48, 47]$. Period $e = -7$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, and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

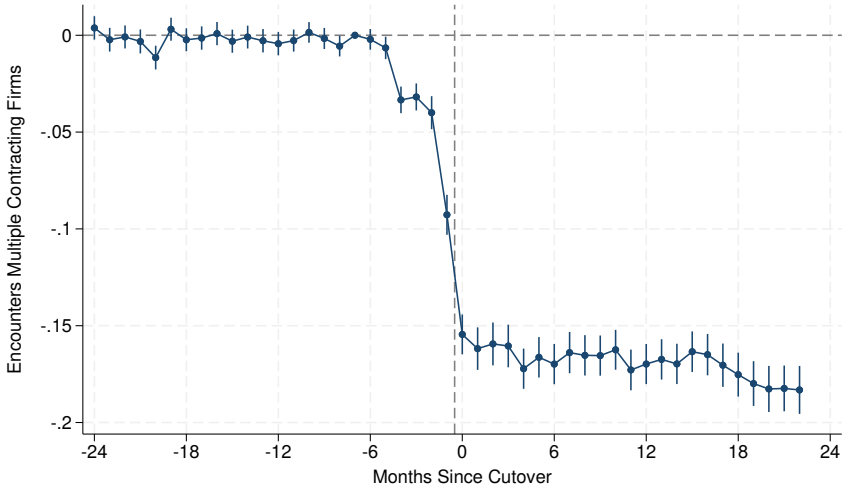
D Alternative Difference-in-Differences Estimator

Figure D1. Effect on Administrative Fragmentation, LP-DiD

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

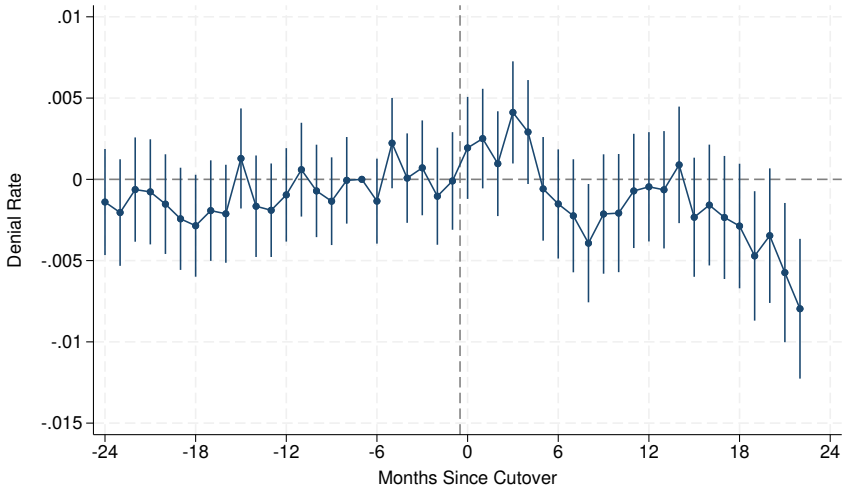


(b) Share of Admissions with Encounters Multiple Contractors



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

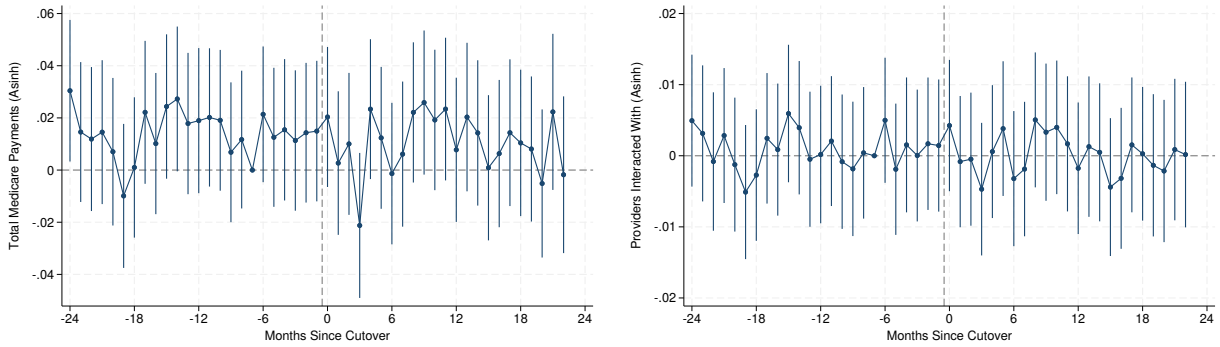


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ using the linear projection difference-in-differences estimator. Period $e = -7$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation 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

(a) Total Spending

(b) Number of Providers Encountered

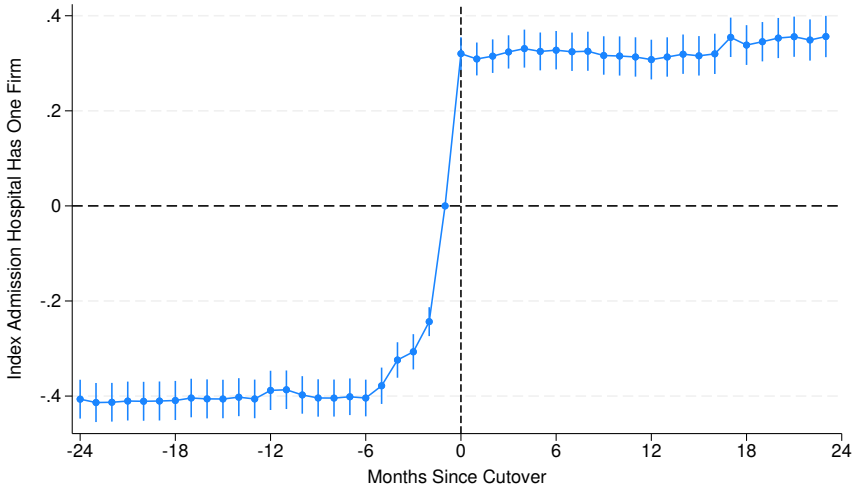


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ using the linear projection difference-in-differences estimator. Period $e = -7$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine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

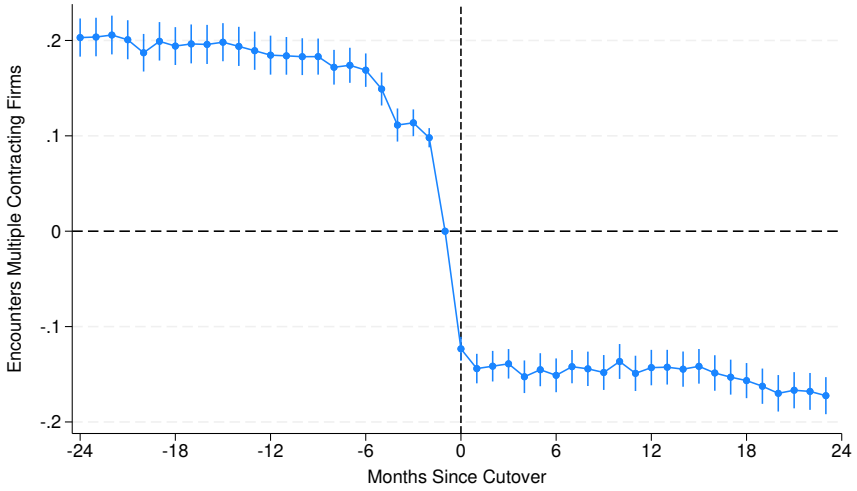
E Admission DRG-by-Month Fixed Effects

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

(a) Share of Admissions with at Hospitals 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 $e = -1$ 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
$Post_{jt} \times Treat_h$	0.700***	-0.320***	-0.590***	0.018***
	(0.012)	(0.007)	(0.011)	(0.005)
R^2	0.741	0.277	0.611	0.232
Dep. Var. Mean	0.530	0.680	0.531	0.400
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 $e = -1$ 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
$Post_{jt} \times Treat_h$	-0.002*	-0.005*	-0.003*	0.001	-0.008
	(0.001)	(0.002)	(0.001)	(0.001)	(0.023)
R^2	0.056	0.063	0.056	0.046	0.112
Dep. Var. Mean	0.103	0.506	0.100	0.091	8.633
Coeff. as % of Mean	-2.13	-0.95	-2.56	0.77	-0.09
Admissions	28.4m	28.4m	25.5m	30.1m	28.4m

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. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

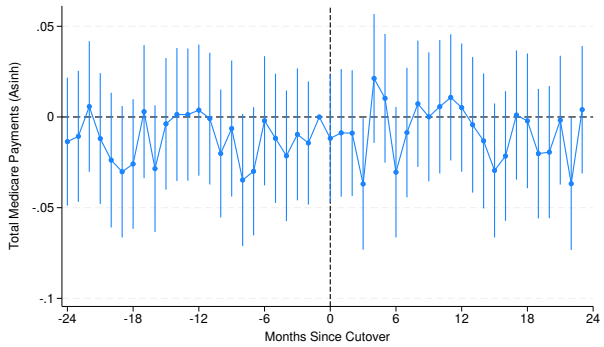
Table E3. Patient Admission-Level Outcomes, DRG-by-Month FEs

	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$Post_{jt} \times Treat_h$	0.004	-0.003	0.002	-0.000
	(0.005)	(0.003)	(0.002)	(0.001)
R^2	0.189	0.129	0.134	0.074
Dep. Var. Mean	7.895	2.981	2.263	0.198
Coeff. as % of Mean	-	-	-	-0.20
N	28.4m	28.4m	28.4m	30.1m

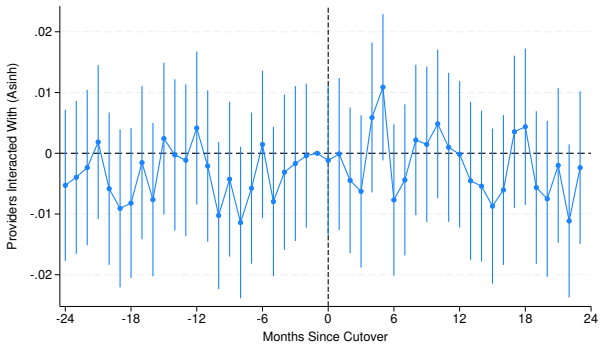
Notes: OLS estimates of equation (1) with DRG-by-month fixed effects. An observation is an admission from 2000–2017. Dependent variable in column (1) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in column (2) is the inverse hyperbolic sine of the total number 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

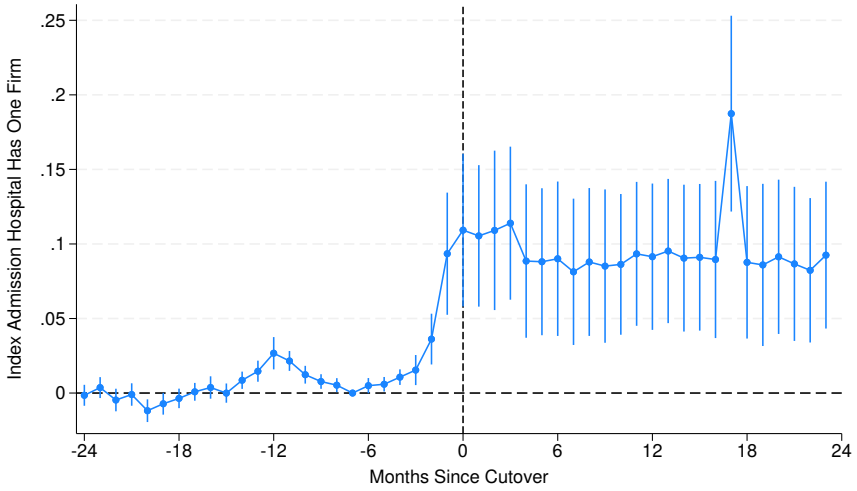


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with DRG-by-month fixed effects. Period $e = -1$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, and in panel (b) 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. Error bars give the point-wise 95% confidence interval. Standard errors are clustered at the hospital level.

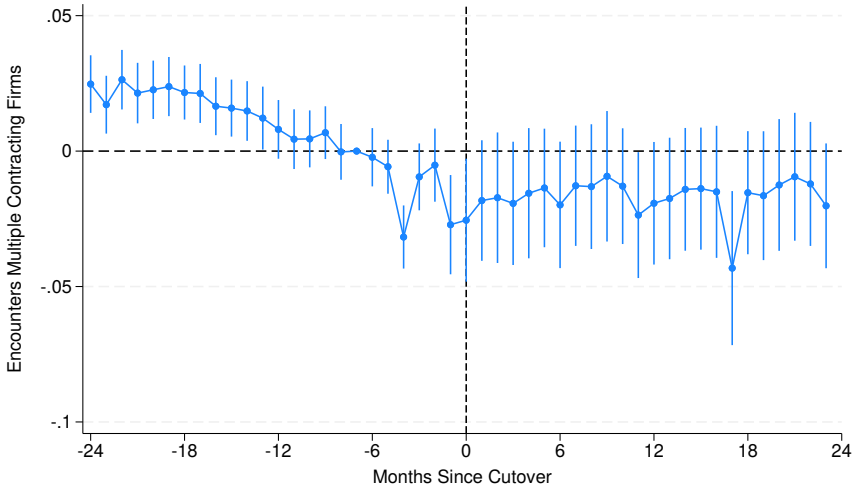
F Jurisdiction-by-Month Fixed Effects

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

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



(b) Share of Admissions with Encounters Multiple Contractors



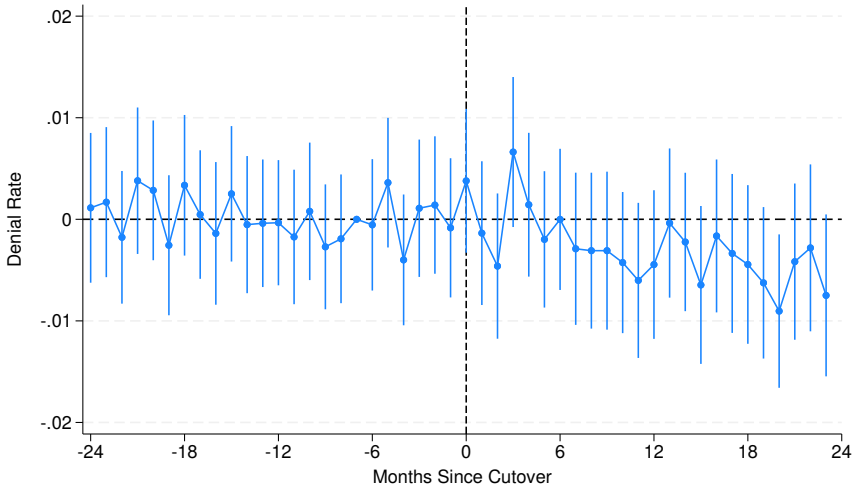
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with jurisdiction-month fixed effects. Period $e = -7$ is the reference period. The dependent variable for panel (a) is an indicator for whether the index admission occurs at a hospital whose Part A contractor is the 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
$Post_{jt} \times Treat_h$	0.092***	-0.032**	-0.053*	0.002
	(0.026)	(0.011)	(0.023)	(0.003)
R^2	0.917	0.863	0.907	0.859
Dep. Var. Mean	0.530	0.680	0.531	0.400
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
$Post_{jt} \times Treat_h$	-0.003	-0.001	-0.003	-0.002	-0.014
	(0.002)	(0.004)	(0.002)	(0.002)	(0.047)
R^2	0.410	0.532	0.407	0.352	0.528
Dep. Var. Average	0.111	0.506	0.109	0.128	8.634
Coeff. as % of Mean	-2.71	-0.28	-2.60	-1.22	-0.16
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. + $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$.

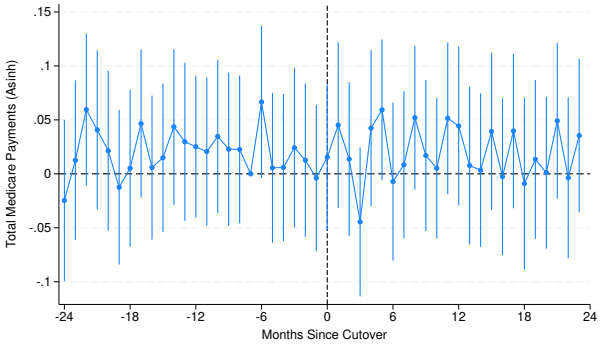
Table F3. Patient Admission-Level outcomes, with Jurisdiction-Month FEs

	(1) Total Spending (Asinh)	(2) Total Claims (Asinh)	(3) Number of Providers Encountered (Asinh)	(4) 30-Day Readmission Rate
$Post_{jt} \times Treat_h$	-0.000	-0.010*	-0.005	-0.000
	(0.010)	(0.005)	(0.003)	(0.001)
R^2	0.547	0.554	0.580	0.529
Dep. Var. Mean	7.895	2.981	2.263	0.198
Coeff. as % of Mean	-	-	-	-0.12
N	30.1m	30.1m	30.1m	30.1m

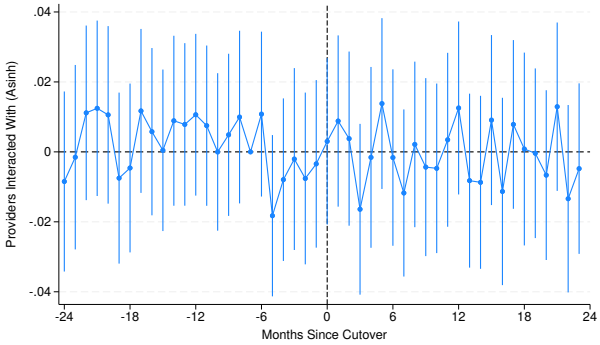
Notes: OLS estimates of equation (1) with jurisdiction-month fixed effects. An observation is an admission from 2000–2017. Dependent variable in column (1) is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge, in column (2) is the inverse hyperbolic sine of the total 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

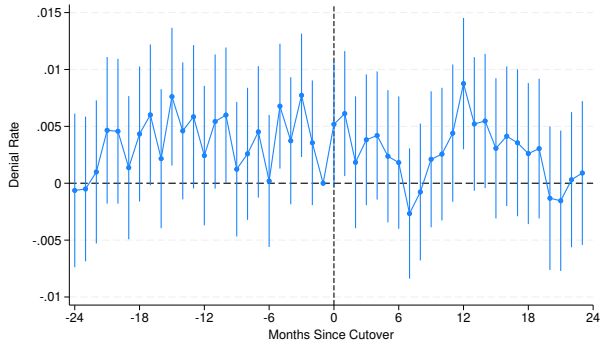


Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$ with jurisdiction-month fixed effects. Period $e = -7$ is the reference period. An observation is an admission from 2000–2017. Dependent variable in panel (a) is the inverse hyperbolic sine of total Medicare spending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

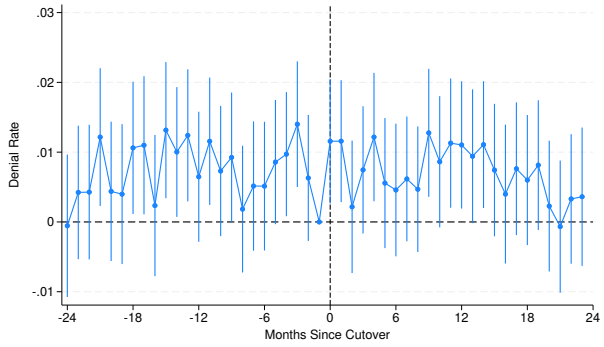
G Heterogeneity by Admission Diagnosis

Figure G1. Effect on Administrative Outcomes: Denials, for Certain Groups of DRGs

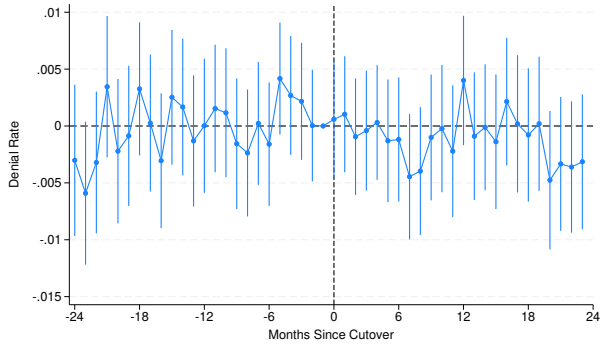
(a) Top 25% of Spending



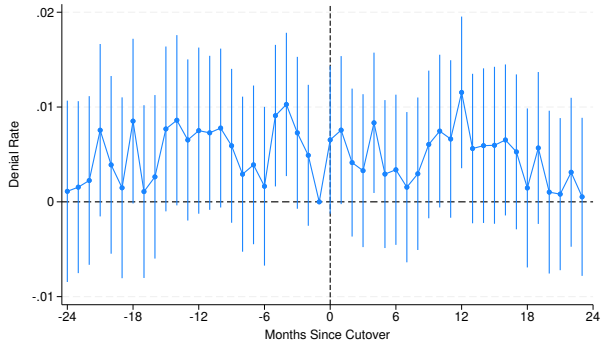
(b) Top 10% of Spending



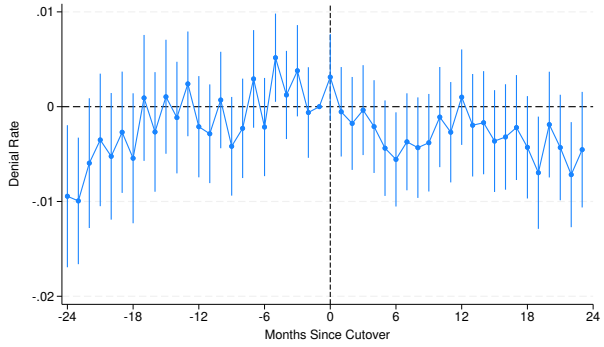
(c) Top 25% of Providers Encountered



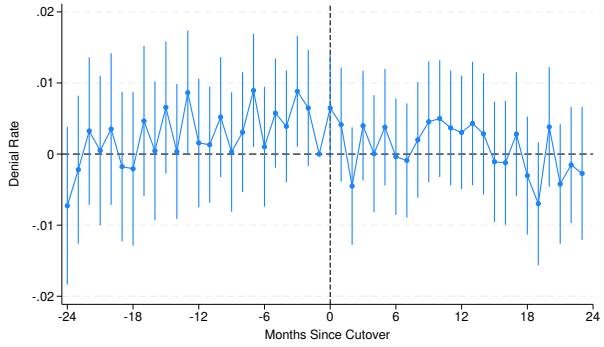
(d) Top 10% of Providers Encountered



(e) Top 25% of Denials



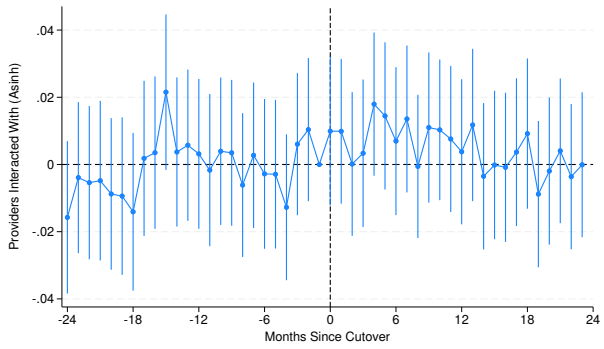
(f) Top 10% of Denials



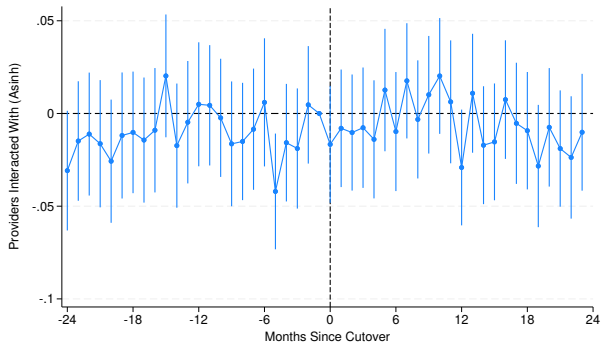
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -1$ is the reference period. The dependent variable is the share of claims for care within 30 days of discharge that were denied. An observation is an index admission from 2000–2017. In panel (a) the sample 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 G2. Effect on Patient Admission-Level Outcomes: Providers Encountered, for Certain Groups of DRGs

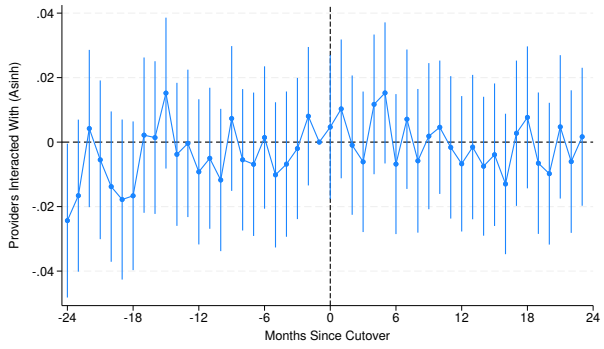
(a) Top 25% of Spending



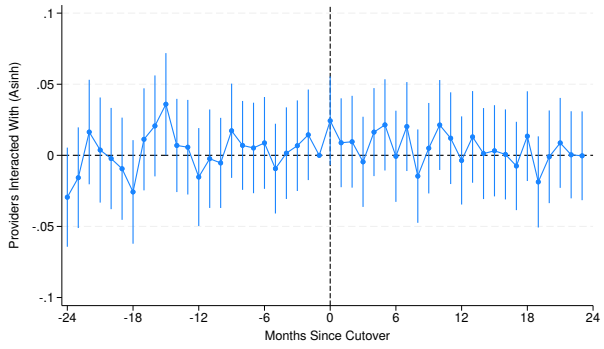
(b) Top 10% of Spending



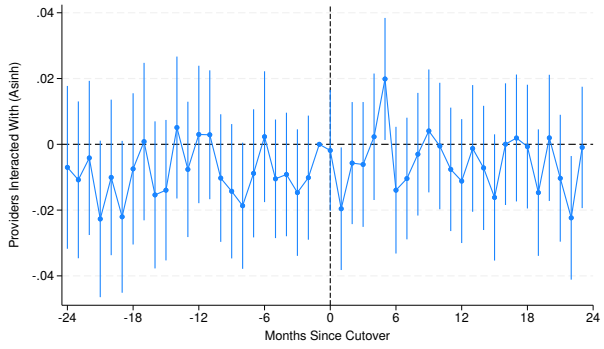
(c) Top 25% of Providers Encountered



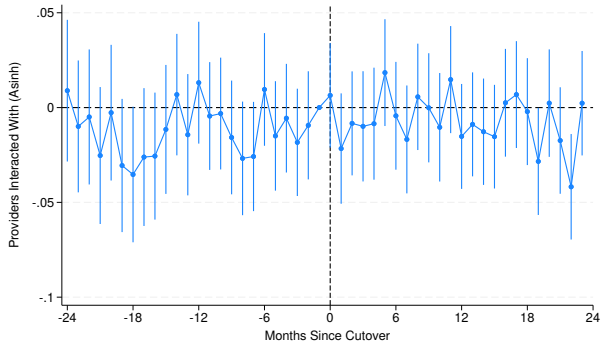
(d) Top 10% of Providers Encountered



(e) Top 25% of Denials



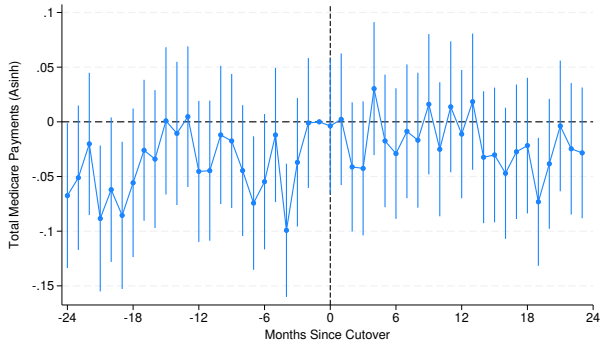
(f) Top 10% of Denials



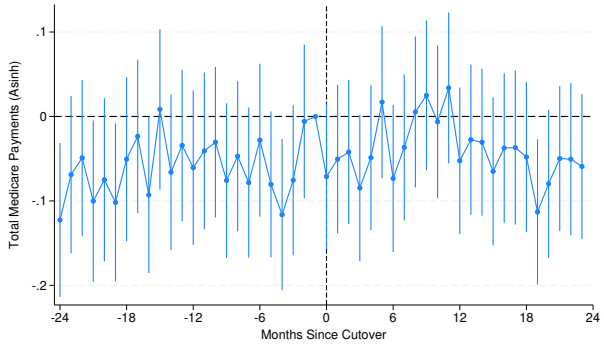
Notes: This figure plots the estimates of β_e from equation (2) for $e \in [-24, 23]$. Period $e = -1$ is the reference period. The dependent variable is the inverse hyperbolic sine of total Medicare spending in the 30 days after discharge. An observation is an index admission from 2000–2017. In panel (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 G3. Effect on Patient Admission-Level Outcomes: Spending, for Subsets of DRGs

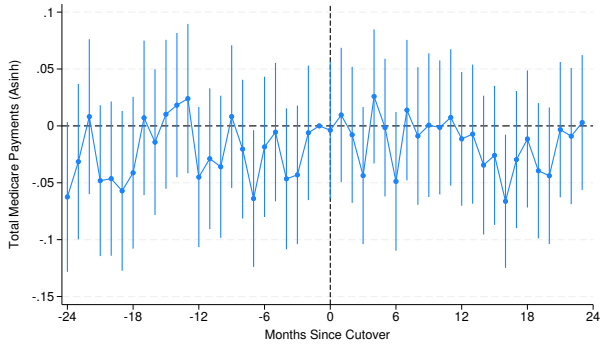
(a) Top 25% of Spending



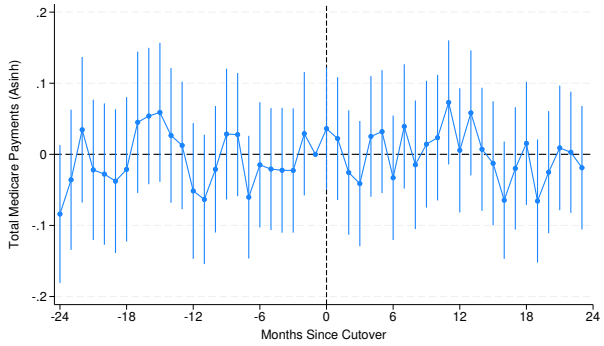
(b) Top 10% of Spending



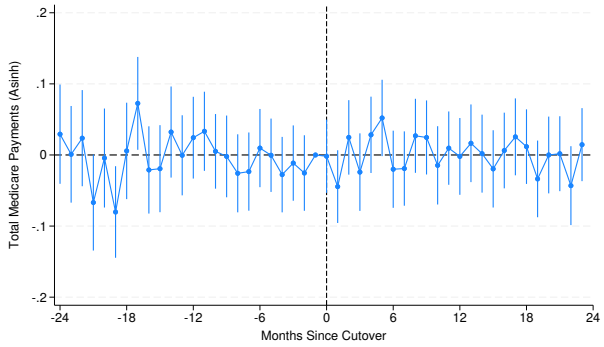
(c) Top 25% of Providers Encountered



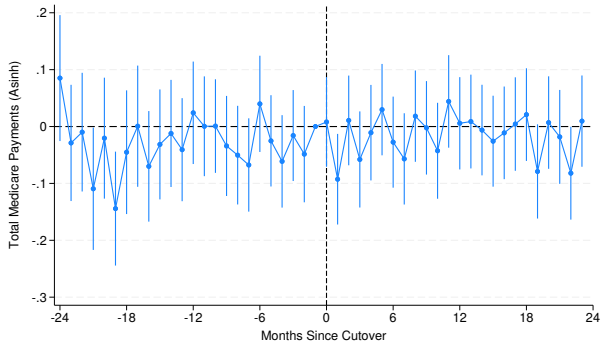
(d) Top 10% of Providers Encountered



(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 $e = -1$ 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.