# Ambulance Taxis: The Impact of Regulation and Litigation on Health Care Fraud *

Paul Eliason[†], Riley League[‡], Jetson Leder-Luis[§], Ryan C. McDevitt[¶], James W. Roberts[‖]

October 2021

We study the relative effectiveness of administrative regulations, criminal enforcement, and civil whistleblower lawsuits for combatting health care fraud. Between 2003 and 2017, Medicare spent $7.7 billion on 37.5 million regularly scheduled, non-emergency ambulance rides for patients traveling to and from dialysis facilities, with dozens of lawsuits alleging that Medicare reimbursed rides for patients who did not meet the requirements for receiving one. Using a novel data set and an identification strategy based on the staggered timing of regulations and lawsuits across the US, we find that a regulation requiring prior authorization for ambulance reimbursements reduced spending much more than criminal and civil lawsuits did. Despite the sharp drop in both ambulance transports and the companies that provide them following prior authorization, patients' health outcomes did not change, indicating that most rides were not medically necessary. Our results suggest that administrative actions have a much larger impact than targeted criminal enforcement, providing novel evidence that regulations may be more cost-effective than ex post ligation for preventing health care fraud.

*We thank Tal Gross, Atul Gupta, Andrei Shleifer, John Skinner, Becky Staiger, Jonathan Gruber, Steven Medema, and participants of the ASHEcon 2021 Conference for their helpful feedback. We also thank Paul Kaufman for very helpful discussions about this industry. Evan Henley, Yicheng Liu, Mingrui Ma, Baiden Wright, and Peck Yang provided excellent research assistance.

[†]Department of Economics, Brigham Young University

[‡]Department of Economics, Duke University

[§]Boston University

[¶]Duke University

[‖]Department of Economics, Duke University and NBER

# 1 Introduction

Fraud poses a serious problem for Medicare: it can both distort patient care and waste limited public resources. In 2019, improper payments made by Medicare — defined as "payments that did not meet statutory, regulatory, administrative, or other legally applicable requirements" — totaled \$28.9 billion, or 7.3% of overall spending (CMS, 2020). In this paper, we assess the different approaches used to combat a particularly widespread and egregious type of fraudulent behavior, the unnecessary use of ambulances to transport patients between their homes and dialysis facilities, to better understand when regulations will be more effective than litigation at reducing wasteful health care spending.

We study ambulance rides for dialysis patients because they provide an empirical setting well suited to an analysis of anti-fraud policy in health care. For those with kidney failure, dialysis replaces the life-sustaining function of kidneys by filtering wastes and toxins out of the blood, with approximately half a million patients in the US going to a dialysis facility three times per week for treatment. Although Medicare reimburses transportation costs for those who demonstrate a medical need for assistance, unscrupulous ambulance companies often exploited a lax enforcement of the rules to provide fraudulent rides to ineligible patients, effectively serving as a very expensive taxi service. From 2003 to 2017, Medicare spent \$7.7 billion on 37.5 million non-emergency ambulance rides for dialysis patients provided by over 3,000 firms across the US.

While the billions of dollars at stake make a study of fraudulent ambulance rides worthwhile on its own, the relevance of our findings extends beyond the narrow setting of dialysis. This particular form of fraud represents a larger class of illicit activity in which providers seek payments for health care services without first establishing a medical necessity, a violation of the regulatory standards for receiving a valid reimbursement. Of the nearly \$30 billion Medicare loses to improper payments each year, to say nothing of the losses in other federal and state programs, a lack of medical necessity has been a key factor in cases as varied as inpatient hospitalizations, physician-administered drugs, nursing homes, medical devices, and hospice care.

The US government uses an array of policies and mechanisms to prevent health care fraud. Both criminal and civil enforcement work through the court system, with the former potentially resulting in jail time and the latter imposing heavy penalties for those found guilty of fraud. In contrast to the substantial resources expended by the Federal Bureau of Investigation and Department of Justice to investigate and litigate each case of alleged fraud after it occurs, administrative regulations imposed by the Center for Medicare and Medicaid Services (CMS) require additional documentation, called prior authorization, from every provider seeking reimbursement before care is rendered.

For our empirical analysis, we combine Medicare dialysis claims data with a novel data set of all criminal and civil enforcements of fraudulent ambulance firms to study the effects of litigation and regulation on the use of non-emergent dialysis rides, patients' access to care, and their health outcomes. Using the staggered rollout of Medicare's requirements for prior authorization as an identification strategy, we find that regulation is much more effective than litigation at reducing wasteful spending. Prior authorization caused an immediate and persistent drop in non-emergency ambulance rides of nearly 53%, whereas civil enforcement had a minimal effect and criminal enforcement resulted in only a gradual reduction in the upward trend in spending. We also show that prior authorization substantially transformed the ambulance market: the number of companies providing non-emergent rides fell 27% immediately following prior authorization, while those that remained became more specialized.

To determine whether this drop in ridership represents a reduction in wasteful spending, we consider whether prior authorization impeded patients' access to care. In this case, the sharp drop in ambulance rides following prior authorization could have made some patients more likely to miss dialysis sessions, increasing their risk of serious complications. Despite this possibility, we find no evidence that the regulatory change disrupted patients' care or led to worse downstream health outcomes, suggesting that prior authorization resulted in a more efficient use of Medicare's resources. We estimate that the federal government would have saved $4.8 billion if it had required prior authorization in 2003, when it first criminally prosecuted ambulance fraud, rather than waiting until 2014.

We conclude our paper by connecting our empirical results to prominent theories of enforcement and regulation to explain why litigation failed to reduce ambulance fraud while prior authorization succeeded. Most directly related are the models of Glaeser and Shleifer (2003) and Behrer et al. (2021) that consider the tradeoffs between regulation and litigation, though the idea that regulation may be a necessary complement to court enforcement was first considered at least a century ago (Wilson, 1913).

Building on these theoretical insights, a large empirical literature has established that criminal behavior responds to various types of enforcement, like increased policing (e.g., Levitt, 1997), with more-recent results also showing the importance of regulatory reforms for securing property rights (Behrer et al., 2021). This prior work notwithstanding, the relative effectiveness of regulatory, criminal, and civil enforcement remains an open empirical question. In our setting, the most relevant factors that shift the balance in favor of regulation include the reluctance of prosecutors to hold impoverished and seriously ill patients liable for fraud, the difficulty of recovering payments from fly-by-night firms, the diffuse nature of the harm, the need for specialization among regulators, and the "bright line rules" of prior authorization that make it easy to enforce.

Our empirical results also add to the literature on fraud and overbilling in Medicare. The seminal work of Silverman and Skinner (2004) and Dafny (2005) describes the incentives for hospitals to upcode inpatient care to receive larger reimbursements, while Esson (2021) finds that Medicare's rules for establishing medical necessity also lead to upcoding in emergency ambulance services. Fang and Gong (2017) continue this thread by estimating the time intensity of outpatient procedures to identify providers who bill for unrealistic hours.[1] Similarly, Sanghavi et al. (2021) link emergency ambulance rides to hospital claims to identify "ghost rides" — rides that do not appear to be substantiated by a hospital visit — among all Medicare beneficiaries, estimating that they make up nearly 2% of ambulance transports nationwide, and O'Malley et al. (2021) find in their recent work that home health care fraud diffuses faster in cities where firms have more patients in common. These studies have largely focused on the detection and incentives for fraud in specific contexts, however, which we extend by considering the mechanisms available to combat this type of illicit behavior.

Some recent evidence suggests that civil litigation by whistleblowers deters overbilling. Most notably, Howard and McCarthy (2021) show that whistleblowing prevents the excessive use of implantable cardiac devices, while Leder-Luis (2019) finds that whistleblowing deters Medicare fraud in a series of case studies covering many different types of fraud and care. We complement this literature by considering the effect of criminal enforcement on Medicare fraud and the relative effectiveness of using criminal versus civil enforcement to prevent overbilling.

In addition to the unnecessary ambulance rides we study in this paper, the dialysis industry has been subject to scrutiny for a host of other improper practices as well. As one example, Eliason et al. (2020) show that independent dialysis facilities acquired by large chains engage in behavior consistent with wasteful drug dumping and increase patients' doses of highly reimbursed drugs, practices found to be severely detrimental to patients' health. The approach in Fang and Gong (2017) that uses hours worked to detect overbilling also shows that dialysis makes up a significant share of the physician services flagged as infeasible. This literature reflects the pervasive issue of overbilling in dialysis, although not all of it rises to the level of criminal fraud.

Our paper proceeds as follows. Section 2 describes the institutional details of dialysis and anti-fraud enforcement. Section 3 outlines the data used for our study. Section 4 presents our empirical analysis of the effects of prior authorization and litigation. Section 5 considers the effect of these enforcement actions on the industrial organization of ambulance companies. Section 6 shows the effects of prior authorization on patients' health outcomes. Section 7 documents the change in riders' characteristics following prior authorization. Section 8 places our empirical findings within the theoretical literature studying the effectiveness of regulation and litigation. Section 9 concludes with our arguments for why regulatory actions are a cost-effective way to prevent health care fraud.

[1] The validity of this measurement has been debated further in Matsumoto (2020) and Fang and Gong (2020).

## 2 Background

Medicare's End Stage Renal Disease (ESRD) program covers patients needing dialysis, a procedure that cleans the blood for those without well-functioning kidneys. Dialysis patients typically visit one of the nation's more than 7,000 dialysis facilities three times per week for three to four hours each session. Due to the frequent nature of these visits, patients spend a considerable amount of time traveling to and from facilities. Many patients arrange for transportation on their own, either in a personal vehicle or on public transportation, but some with severe medical conditions require an ambulance. Medicare pays for transportation to and from dialysis sessions only when an ambulance is medically necessary.

Ambulance companies must satisfy a number of requirements to receive Medicare reimbursements for providing rides to dialysis facilities. Federal regulations stipulate that ambulances must be staffed by at least two people, with at least one certified as an emergency medical technician (EMT), and the vehicles themselves must be specifically designed as ambulances.[2] To receive a reimbursement, participating providers first need a National Provider Identifier (NPI), and dialysis patients must be bedridden or need lifesaving procedures in transit for the ride to qualify as medically necessary.

Medicare pays for ambulance rides through Part B, making patients responsible for a 20% copayment on top of their annual deductible. The payment rates set by Medicare consist of a base fee, which depends on the level of life support (e.g., whether the ride was an emergency or, in rare cases, required air transportation) and a per-mile fee, for which ambulances receive a bonus if the pickup is in a rural location. Today, the base and mileage rates are $231.98 and $7.62, respectively, up from $209.65 and $6.74 in 2010. Medicare also adjusts rates by location, and at times they have been subject to bonuses if, for example, all of the mileage is in a rural area.

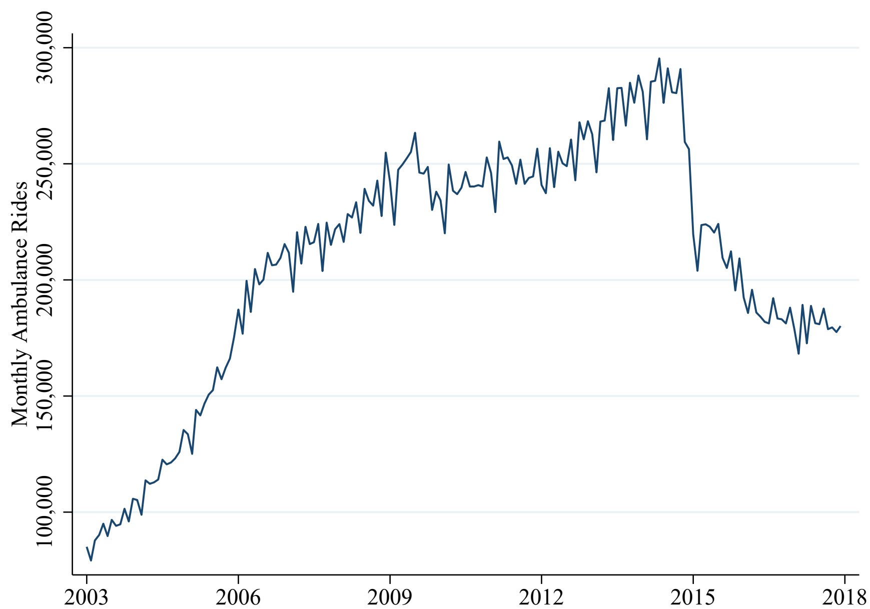
Fraud has become a major concern for Medicare's ambulance reimbursements as a whole, not just among dialysis patients. The Department of Health and Human Services Office of Inspector General (OIG) has published several reports about Medicare's ambulance benefit, concluding that it is often abused. For example, a 2006 OIG study, "Medicare Payment for Ambulance Transport," evaluated the appropriate use of the ambulance benefit and found that 20% of non-emergent transports were improper in that they did not meet Medicare's coverage requirements.

The issue is particularly acute in dialysis, where for many years ambulance companies transported patients who did not have a medical necessity under Medicare's criteria. The large reimbursements paid by Medicare provide a strong financial incentive for unscrupulous providers, especially if they transport non-emergent patients who do not require costly medical attention

[2]States may also impose their own regulations, such as the certificate of need laws currently in place in Arizona, Hawaii, Iowa, Kentucky, New Jersey, and New York. All states also license various levels of emergency medical service occupations and have different requirements for these licenses.

during the ride. And because dialysis patients must make regular visits to their facilities, they can be an especially lucrative target for those providing fraudulent rides. From 2007 to 2011, the volume of transports to and from dialysis facilities increased 20%, more than twice the rate of all other ambulance transports. In 2011, ambulance transports to and from dialysis facilities accounted for nearly $700 million in Medicare spending, or approximately 13% of Medicare's total expenditures on ambulance services (Centers for Medicare and Medicaid Services, 2020b). Reflecting this, Figure 1 shows the initial growth and eventual decline of dialysis ambulance transports from 2003 to 2017.

Figure 1: Non-Emergent Basic Life Support Dialysis Rides over Time




*Notes:* The sample includes non-emergent basic life support ambulance rides from a dialysis facility to a place of residence for ESRD patients from 2003–2017.

The US government has used several different approaches to prevent unnecessary ambulance rides for dialysis patients. Those who commit Medicare fraud can run afoul of criminal statutes, including the health care fraud statute (18 U.S.C. §1347) and the anti-kickback statute (42 U.S.C. §1320a-7a(a)(5)), with the crimes investigated by the Federal Bureau of Investigation and prosecuted by Department of Justice district offices nationwide. The US compounds its enforcement with laws against conspiracy, racketeering, organized crime, and lying to investigators. Beginning in 2003, the Department of Justice has pursued 38 criminal lawsuits against ambulance company operators who engaged in criminal fraud to provide dialysis ambulance transports. Along with knowingly billing the government for medically unnecessary care, allegations in these cases include paying kickbacks to patients to induce them to ride, paying referral bonuses to patients who recruited others to participate in the scheme, and concealing or manipulating documentation to justify the ongoing use of ambulances.

In addition to criminal statutes, federal health care fraud violates the False Claims Act, a civil statute that imposes monetary penalties of triple damages on firms that overbill federal health care programs. The False Claims Act contains a qui tam whistleblower provision, wherein individuals with knowledge and evidence of fraud can file their own lawsuits on behalf of the US government against those who bill fraudulently, in exchange for 15–30% of the recovered funds. The Department of Justice can also initiate civil lawsuits against those accused of fraud on their own. We identify 19 civil lawsuits, from as early as 1996, alleging the unnecessary transport of dialysis patients by ambulance companies.

Medicare administrators also thwart overbilling and fraud by enacting new regulations. Beginning in 2014, Medicare imposed prior authorization requirements through Medicare Administrative Contractors (MACs), the companies that process Medicare claims, stipulating that they would not pay claims for non-emergency dialysis ambulance rides without first documenting medical necessity. Providers could receive authorization before the ride by submitting documentation or could file a claim for rides already completed and submit documentation afterwards, but they would not be paid without documentation. In 2014, Medicare first rolled out prior authorization in New Jersey, South Carolina, and Pennsylvania — states that had some of the heaviest use of dialysis ambulance rides — and extended the regulation in 2016 to include Delaware, DC, Maryland, North Carolina, Virginia, and West Virginia. Plans to expand prior authorization nationwide were postponed in 2020 due to the Covid-19 pandemic, with a resumption scheduled from December 2021 to August 2022.

## 3 Data & Descriptive Statistics

We use a 100% sample of claims data for the entire universe of patients diagnosed with ESRD and enrolled in Medicare between 2003 and 2017. These data consist of patient- and facility-level information compiled by the United States Renal Data System (USRDS).[3] The patient-level data allow us to observe demographics (e.g., sex, race, body mass index, cause of ESRD, payer, comorbidities, ZIP Code, and a facility identifier) and complete ESRD treatment histories, while facility-level data have information on location and ownership. Importantly, our data allow us to observe each ambulance trip to and from a dialysis facility billed to Medicare. For firms that provide non-emergency ambulance rides, we also have data on their other claims for Medicare

[3]USRDS combines data from a variety of sources, including Medicare claims, annual facility surveys, and dialysis treatment histories, to create the most comprehensive data set for studying the US dialysis industry. For a more thorough description of USRDS, please see the *Researcher's Guide to the USRDS System* at USRDS.org.

ESRD beneficiaries, such as emergency hospital transports.[4]

Table 1 provides summary statistics for patient characteristics, ridership, and health outcomes for those who receive any non-emergent ride to a dialysis facility as well as ESRD patients who never receive such a ride, split across months with and without rides. Riders are older, more likely to be women, more likely to be Black, and more likely to have diabetes. Patients who use ambulances for non-emergency transportation to dialysis facilities take on average 10 round-trip rides each month, amounting to 20 claims total, with a lifetime average of 561 claims. Given that dialysis patients receive roughly 12 treatments per month, these averages imply that patients who take an ambulance to and from their facility do so for nearly nine out of ten sessions.

Table 1: Summary Statistics of Patient-Month Level Data

| | Patient Rider Status | | | |
|---|---|---|---|---|
| | Never-Rider | Rider, Non-Riding Month | Rider, Riding Month | Overall |
| **Patient Characteristics** | | | | |
| Age (Years) | 62.01 | 67.44 | 69.27 | 62.99 |
| Months with ESRD | 56.51 | 57.29 | 54.05 | 56.49 |
| Black | 0.378 | 0.417 | 0.451 | 0.386 |
| Male | 0.560 | 0.496 | 0.457 | 0.548 |
| Diabetic | 0.524 | 0.626 | 0.661 | 0.543 |
| Drug User | 0.014 | 0.011 | 0.008 | 0.013 |
| Smoker | 0.065 | 0.056 | 0.045 | 0.063 |
| Drinker | 0.013 | 0.013 | 0.011 | 0.013 |
| Uninsured at Incidence | 0.129 | 0.086 | 0.061 | 0.120 |
| Employed at Incidence | 0.180 | 0.098 | 0.066 | 0.165 |
| **Ridership** | | | | |
| Non-Emergent Dialysis Rides | 0.00 | 0.00 | 19.54 | 0.87 |
| Emergent Rides | 0.101 | 0.183 | 0.408 | 0.125 |
| Total Lifetime Rides | 0.0 | 116.1 | 561.4 | 39.0 |
| Continuing to Ride Next Month | . | . | 0.838 | 0.838 |
| **Health Outcomes** | | | | |
| Dialysis Sessions | 12.18 | 12.03 | 11.29 | 12.13 |
| All-Cause Hosp. | 0.111 | 0.154 | 0.250 | 0.122 |
| Fluid Hosp. | 0.011 | 0.016 | 0.020 | 0.012 |
| Mortality | 0.009 | 0.006 | 0.034 | 0.010 |
| Patient-Months | 15,854,406 | 2,289,996 | 846,573 | 18,990,975 |

*Notes:* Data are from 2011–2017. Patient characteristics except age and dialysis tenure are at incidence of ESRD. All ridership variables other than emergent rides are based on non-emergent basic life support rides between a dialysis facility and a patient's home. The probability of continuing to ride is the conditional probability of riding in the next month given the patient rides in this month. Fluid hospitalizations are those for which the primary diagnosis indicates excess fluids, an indication of insufficient dialysis.

We supplement these data with information from the criminal and civil enforcement of fraud. Using publicly available press releases from the Department of Justice, corroborated for completeness by internet searches, we identify 56 lawsuits in 23 different judicial districts against dozens of ambulance companies and individuals for unnecessary ambulance transports related to dialysis. For each of these lawsuits, we collect court records from the Public Access to Court

[4] USRDS began recording identifiers for ambulance companies in 2012, so our firm-level analyses use data from 2012 to 2017.

Electronic Records (PACER) system, which include specific fraud allegations and data on the lawsuit's timing and location of enforcement. For context, Table 2 provides descriptive statistics for different types of ambulance rides, broken down by status (i.e., emergent or non-emergent) and type of firm (e.g., indicted or non-emergent only).

Table 2: Summary Statistics for Selected Ride Types

| | Total Rides | Total Payments | Firms Involved* |
|---|---|---|---|
| All Non-Emergent Dialysis Rides | 37,501,752 | $7,733,452,800 | 3,081 |
| Rides by Non-Emergent-Only Firms* | 921,419 | $190,515,568 | 262 |
| Rides by Indicted Firms* | 642,032 | $139,586,464 | 42 |
| All Emergent Rides | 5,986,533 | $2,082,876,032 | 10,532 |

*Notes:* Unless explicitly identified as an emergent ride, rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. These data include rides from 2003–2017. Non-emergent-only firms are those that are never observed giving an emergency ambulance ride in the USRDS data.

*Firm identifiers are available from 2012–2017, and figures reported in this row or column use only data from this period.

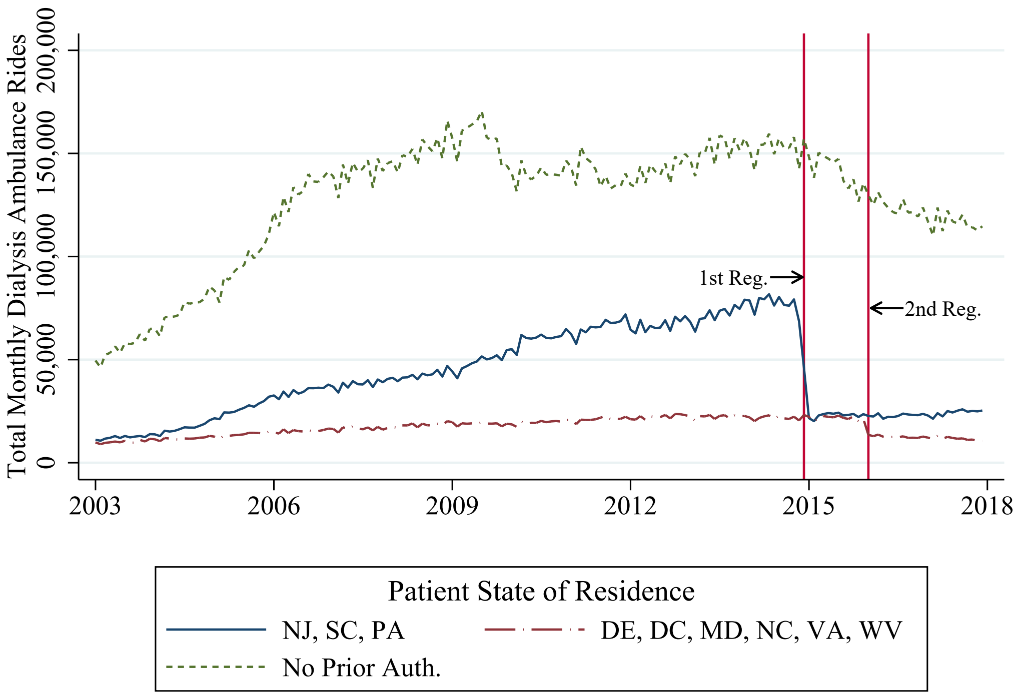
# 4 Empirical Analysis

The data described above allow us to study the effectiveness of regulations relative to litigation in combating ambulance fraud. As discussed in Section 2, Medicare regulations requiring prior authorization stipulate that ambulance companies obtain approval for each patient receiving repetitive, non-emergent ambulance transports, which must be renewed periodically.[5] The policy was piloted on December 15, 2014, in New Jersey, Pennsylvania, and South Carolina, the top three states in terms of per-beneficiary dialysis ambulance expenditures for Medicare at the time. On January 1, 2016, Medicare expanded prior authorization to Delaware, DC, Maryland, North Carolina, Virginia, and West Virginia. As shown in Figure 2, rides for patients residing in Pennsylvania, New Jersey, and South Carolina drop sharply after Medicare first imposed prior authorization, followed by another drop corresponding to the states included in the second wave of regulation.

Authorities also use legal actions like criminal enforcement and civil lawsuits to deter fraud, and a case study of the Pennsylvania East District helps motivate our research strategy for

[5] Medicare considers "three or more round trips during a 10-day period, or at least one round trip per week for at least three weeks" to be repetitive transports. Prior authorization is required for the fourth ride in a 30-day period.

Figure 2: Rides by Prior Authorization Regulation

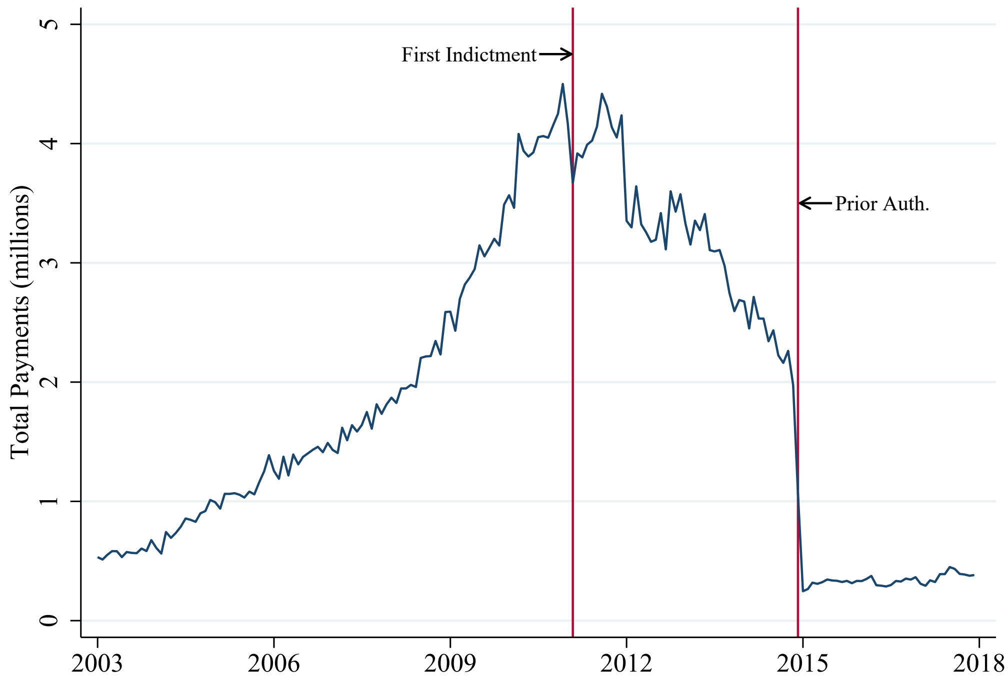



*Notes:* Sample includes non-emergent basic life support ambulance rides from a dialysis facility to a place of residence for dialysis patients from 2003–2017. State determined by the transported patient's residence. The first vertical line marks the start of prior authorization in NJ, SC, and PA, and the second marks DE, DC, MD, NC, VA, and WV.

identifying the effects of such litigation.[6] Figure 3 shows the growth in Medicare spending on non-emergent dialysis rides between 2003 and 2010 in this district. Following the pronounced spike in rides, authorities brought multiple cases against individuals and firms suspected of fraud. The left-most vertical red line marks the indictment date of the first criminal case, and the gradual decline in payments following the initial indictment suggests that criminal prosecution reduced the number and cost of ambulance rides in the district. The sharp and immediate drop in rides following prior authorization, however, implies that regulation may have an even stronger effect than litigation. Below, our empirical analysis will use variation in the timing of criminal indictments and civil cases brought in each district to test whether the trend in ambulance expenditures systematically changed following these proceedings and whether they were as effective as prior authorization.

[6]We highlight this district for a few reasons. First, it was subject to both criminal and regulatory enforcement, allowing us to highlight the potentially different treatment effects of these two enforcement methods. In fact, it was subject to more litigation than any other district, with 10 separate criminal cases brought in this district alone. Furthermore, this district had an unusually high level of ambulance activity, being among the top five districts in terms of number of ambulance rides every month from April 2009 through June 2015, despite being much smaller geographically and in terms of population than many other districts.

Figure 3: Total Payment for Ambulance Rides in Pennsylvania East District




*Notes:* The sample includes non-emergent basic life support ambulance rides from a dialysis facility to a place of residence for dialysis patients from 2003–2017 for patients whose county of residence is within the Pennsylvania East judicial district. The first vertical line marks the first criminal or civil indictment of an ambulance firm in this district and the second vertical line marks the implementation of prior authorization in Pennsylvania.

## 4.1 Methodology

We use the staggered roll out of prior authorization and the differential timing of criminal and civil enforcement across US federal judicial districts to identify the causal effects of these respective approaches for reducing rides as well as their impact on patients.[7] For our estimations, we present results using both traditional two-way fixed effects (TWFE) methods in the main text along with various alternative estimators, including those introduced by Callaway and Sant'Anna (2020), Cengiz et al. (2019), and Borusyak et al. (2021), in Appendix C. For the traditional TWFE results, we estimate

$$Y_{dt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \sum_{e=0}^{L} \beta_e T_{dt}(e) + \alpha_d + \alpha_t + \Gamma X_{dt} + \varepsilon_{dt}, \tag{1}$$

[7]There are 94 US federal judicial districts, each of which are wholly contained within states; these are the regions at which the Department of Justice and the US federal court operate, each with its own US attorney and Department of Justice office. We provide a map of these districts in Appendix A.

for district $d$ in month $t$, where $T_{dt}(e)$ is an indicator for being $e$ months from the treatment date, $\alpha_d$ and $\alpha_t$ are district and month fixed effects, and $X_{dt}$ is a matrix of indicators for having already been subject to a different enforcement type or prior authorization. To avoid the compositional issues that have been noted by, for example, Callaway and Sant'Anna (2020), we set $K = 24$ and $L = 23$ and only define $T_{dt}(e)$ for units that are in the sample for the entire 48 month period around the treatment date and only for observations in that window. For untreated units, we set $T_{it}(e) = 0$ for all $e$.

To aggregate these results into a single parameter, we also estimate

$$Y_{dt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \beta \max\{T_{dt}(0), \ldots, T_{dt}(L)\} + \alpha_d + \alpha_t + \Gamma X_{dt} + \varepsilon_{dt}. \tag{2}$$

This is similar to the more traditional pre-post estimator, but rather than comparing the entire pre-period to the entire post-period, the post-period is compared to only the period immediately before treatment, with only the $L$ periods after treatment entering the post-period. Unlike the more familiar pre-post indicator, this estimator ignores any trends in the outcome level before treatment by fixing the comparison period. Perhaps more importantly, this estimator explicitly captures the average treatment effect on the treated over the first $L$ months of treatment, rather than the varying lengths of time captured by a pre-post indicator, which could potentially be quite different.[8] By setting $K = 24$ and $L = 23$, we capture the effect of treatment in the two years following treatment.

For results estimated at the patient level, our estimating equations are

$$Y_{idt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \sum_{e=0}^{L} \beta_e T_{dt}(e) + \alpha_d + \alpha_t + \Gamma X_{idt} + \varepsilon_{idt} \tag{3}$$

and

$$Y_{idt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \beta \max\{T_{dt}(0), \ldots, T_{dt}(L)\} + \alpha_d + \alpha_t + \Gamma X_{idt} + \varepsilon_{idt}, \tag{4}$$

for individual $i$ with observable patient and dialysis facility characteristics $X_{idt}$. Here we set $K = 12$ and $L = 11$ to capture the effect over the first year.

To further justify the validity of this research design, Table B1 in Appendix B contains a balance table comparing, by wave, control states with prior authorization states. Although some differences exist, the health outcomes are similar in terms of hospitalization and mortality rates,

[8] In the presence of heterogenous and dynamic treatment effects, the model using a pre-post treatment indicator would estimate a weighted average treatment effect over the entire post-period; given the different lengths of post-periods and potentially heterogenous effects, this parameter may not have any practical meaning Callaway and Sant'Anna (2020).

as well as the rate of emergency ambulance rides. Furthermore, the second-wave states are similar to the control states in terms of non-emergent ridership, although the first-wave states did have much higher ridership. This reflects the deliberate decision to first impose prior authorization on the states with the largest share of fraudulent ambulance rides, followed by a second group of states in close proximity to the first-wave states.

## 4.2 The Effect of Prior Authorization on Rides

We first consider the effect of prior authorization. Table 3 provides estimates of $\beta$ from Equation (2), the effect of prior authorization on all treated districts in the two years following treatment, where the outcomes are the number of non-emergent ambulance rides between a dialysis facility and a patient's home as well as their payments, measured both in levels and transformed by adding 1 and taking the natural log.[9] We find that prior authorization reduces payments for non-emergent ambulance rides by 1.127 log points, or 67.8%.[10]

Table 3: Effect of Prior Auth. on Ambulance Rides and Spending

| | (1) Total Ride Payments (Log) | (2) Total Ride Payments | (3) Total Rides (Log) | (4) Total Rides |
|---|---|---|---|---|
| Prior Authorization | -1.128$^{**}$ | -727738.1$^{+}$ | -0.912$^{***}$ | -3662.6$^{+}$ |
| | (0.350) | (400966.3) | (0.176) | (2016.9) |
| Month-Year FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Dep. Var. Mean | 9.970 | 416294.5 | 5.384 | 2009.6 |
| Observations | 7356 | 7356 | 7356 | 7356 |

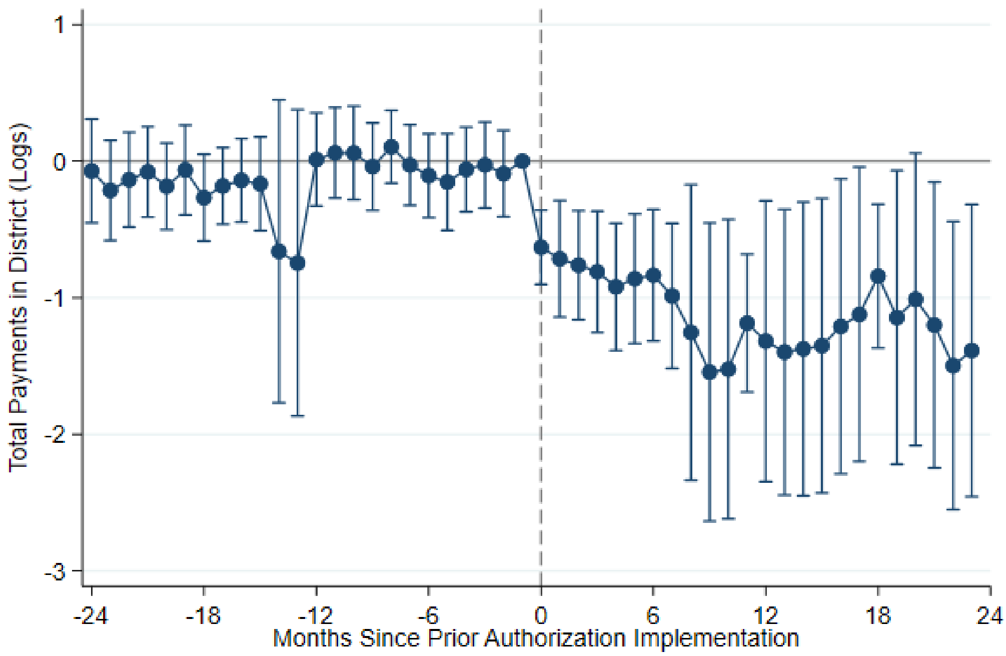
*Notes:* Estimates of $\beta$ from equation (2). All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variable in columns (1) and (3) are transformed by adding 1 and taking the natural log. These data include rides from 2011–2017. An observation is a district-month. Standard errors are clustered at the district level. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Figure 4 shows the dynamic difference-in-differences results, or estimates of $\beta_e$ for $e \in [-24, 23]/\{-1\}$ in Equation (1), with log transformed total payments as the dependent variable. We find that the effect of prior authorization was large, immediate, and persistent.

[9] The second wave of prior authorization occurs two years before the end of our data, meaning that both waves of treatment are included in this parameter.

[10] In Appendix D, we perform a similar analysis at the firm-month and patient-month level, finding that the large effect of prior authorization is robust. We also consider a falsification test that shows prior authorization had no impact on the number of emergent rides.

Figure 4: Effect of Prior Auth. on Ambulance Spending




*Notes:* Estimates of $\beta_e$ for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is total payments for non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data transformed by adding 1 and taking the natural log. These data include rides from 2011–2017. An observation is a district-month. Standard errors are clustered at the district level. Error bars represents the pointwise 95% confidence interval.

## 4.3 The Effect of Litigation on Rides

To study whether litigation reduces non-emergent ambulance rides, we use the same methodology to estimate separately the impact of civil and criminal enforcement actions.[11] Table 4 provides estimates of $\beta$ from Equation (2), where the treatment date is determined by the first enforcement action of each type in the district.[12]

We find that civil enforcement does not have a statistically significant effect on rides or total payments, whereas criminal enforcement reduces monthly payments by 14% and rides by 16% in the two years following enforcement. Figure 5 shows the dynamic effects of the first indictment of each type. Although we see no decrease in payments following civil enforcement,

[11]This methodology relies on districts that are not subject to enforcement serving as a reliable comparison group for those that are. In particular, if there are national or regional spillovers in the effect of indictments beyond the districts in which they occur, our estimates would be biased. In Appendix E, we show that the effects of enforcement are highly localized, with no negative impacts on ridership in neighboring districts.

[12]Because Illinois North, Massachusetts, and California Central had civil actions before our sample period and the first civil action in Virginia East was too late in our data, we exclude these districts from our analysis of the effect of civil enforcement. Kentucky East is excluded from our analysis of criminal enforcement for being subject to enforcement too late in our data.

our results suggest that criminal enforcement reduces payments gradually, inducing a downward trend without an immediate drop. This could mean that the effect of enforcement grows over time as information about the penalties for fraudulent behavior disseminates, or it could indicate a more cautious strategy by firms engaged in fraud that results in a gradual slowdown in spending. In Appendix E, we present results that suggest a degree of heterogeneity in the effect of these enforcement actions, with some having large effects and others having none at all.

Table 4: Effect of Litigation on Ambulance Spending and Rides

| | Civil | | Criminal | |
|---|---|---|---|---|
| | (1)<br>Total Ride<br>Payments (Log) | (2)<br>Total<br>Rides (Log) | (3)<br>Total Ride<br>Payments (Log) | (4)<br>Total<br>Rides (Log) |
| Enforcement | 0.0387 | $0.0783^{+}$ | -0.155 | $-0.180^{*}$ |
| | (0.0925) | (0.0442) | (0.108) | (0.0904) |
| Month-Year FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Dep. Var. Mean | 9.475 | 5.022 | 9.595 | 5.112 |
| Observations | 15324 | 15324 | 15420 | 15420 |

*Notes:* Estimates of $\beta$ from equation (2). All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district-month. The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are clustered at the district level. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Taken together, our results show that prior authorization was much more effective than litigation at deterring potentially fraudulent ambulance rides. Prior authorization caused a large and immediate drop in non-emergent ambulance rides that persisted over time, whereas criminal enforcement had only about one-fifth the effect and civil action had no impact whatsoever. In Section 6, we explore whether these rides were potentially wasteful by estimating the effect of prior authorization on patients' access to dialysis and their health outcomes.

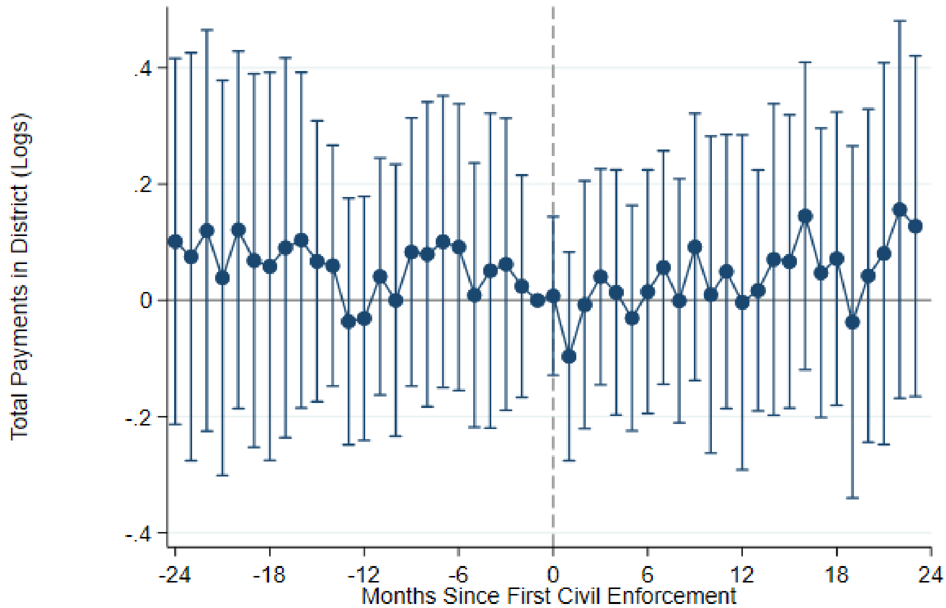
# 5 The Effect of Enforcement on Market Structure

Not only did prior authorization cause a large drop in the number of non-emergent ambulance rides to dialysis facilities, it also led to a large reduction in the number of firms that provide them. As shown in Table 5, prior authorization reduced the number of ambulance companies providing non-emergent dialysis rides by 0.312 log points, or 26.8%.

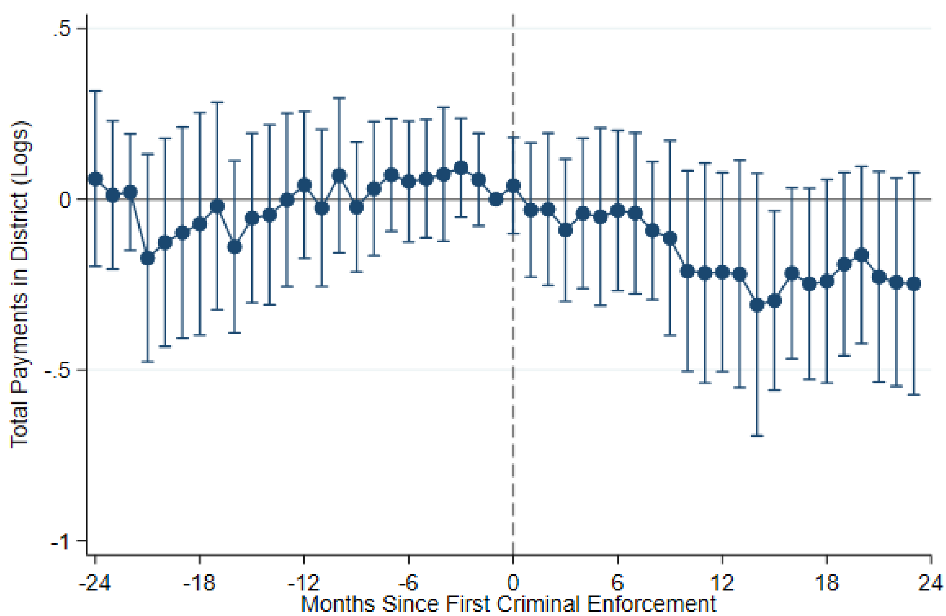
Beyond simply reducing the number of ambulance companies, we find that prior authorization also leads to greater firm specialization: firms with a high share of non-emergent rides are more likely to exit at the first wave of prior authorization, while the number of firms providing only

Figure 5: The Impact of Litigation on Ambulance Payments

(a) Civil Cases




(b) Criminal Cases



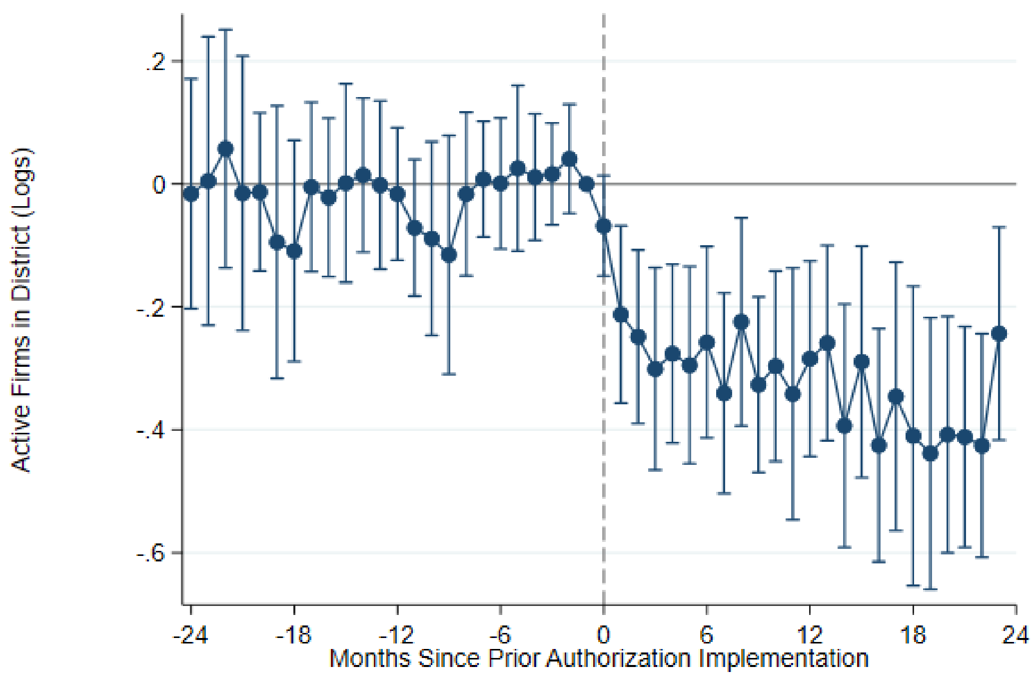

*Notes:* Estimates of $\beta_e$ for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is total payments for non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district-month. The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are clustered at the district level. Error bars represents the pointwise 95% confidence interval.

Table 5: Effect of Prior Authorization on Number of Active Firms

| | (1) Active Firms | (2) Active Firms (Log) |
|---|---|---|
| Prior Authorization | -0.312*** | -12.07* |
| | (0.0712) | (5.223) |
| Month-Year FE | 1 | 1 |
| District FE | 1 | 1 |
| Dep. Var. Mean | 1.794 | 12.12 |
| Observations | 6408 | 6408 |

*Notes:* Estimates of $\beta$ from equation (2). Dependent variables are the number of firms providing non-emergent basic life support rides between a dialysis facility and a patient's home in a district-month and natural logarithm of one plus the same. These data include rides from 2012–2017. An observation is a district-month. Standard errors are clustered at the district level. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Figure 6: Effect of Prior Authorization on Number of Active Firms




*Notes:* Estimates of $\beta_e$ for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is the number of firms providing non-emergent basic life support rides between a dialysis facility and a patient's home in a district-month transformed by adding 1 and taking the natural log. These data include rides from 2012–2017. An observation is a district-month. Standard errors are clustered at the district level. Error bars represents the pointwise 95% confidence interval.

non-emergent dialysis rides increases. We can see this in panel (a) of Figure 7, which gives the distribution of firms by the share of non-emergent rides they provide to dialysis patients. Many of the firms that provide non-emergent ambulance rides to dialysis patients provide very few emergent rides to the same population, especially before prior authorization. After prior authorization, fewer firms provide rides to dialysis patients, particularly firms that provide very few non-emergent rides, but the number of firms that provide *only* non-emergent rides to dialysis patients more than tripled, increasing from 29 to 102.

Panel (b) of Figure 7 presents another view of how the market split following prior authorization. The vertical axis places firms in bins for each 20 percentage point increment based on their share of non-emergent rides before prior authorization, while the horizontal axis uses the same bins following prior authorization to highlight how firms transition. We see that firms that initially provided few non-emergent rides were very likely to stop providing them after prior authorization, with three-quarters of firms for which non-emergent dialysis rides comprised less than 20% of their rides exiting the non-emergent dialysis ambulance market completely. By contrast, none of the firms exclusively providing non-emergent rides, and only 13% of those providing over 80% non-emergent rides, exited the market. Although firms that previously provided few non-emergent rides tended to shrink or exit, firms that already provided a large share of non-emergent rides tended to stay the same size or grow. In other words, prior authorization seems to have split the market in two: some firms provide mainly emergent rides that do not require prior authorization while others successfully navigate the bureaucracy of prior authorization to provide mainly non-emergent ones.[13]

In contrast to the large impact of prior authorization on the market for non-emergent ambulance rides, we find little evidence that criminal and civil enforcement had any noticeable impact. Table 6 provides one possible explanation as to why: when a firm exits, roughly half of its patients continue to ride in the next month, with this number closer to 60% when the exiting firm was indicted. Among those who stop riding, almost all of them continue to receive dialysis, with only 2% of patients who ride with an exiting firm failing to make it to their sessions in the month after the exit, notwithstanding patients who were hospitalized or died. The fact that patients who received rides from indicted firms are more likely than not to continue receiving ambulance rides indicates that litigation did not do much to change their behavior.

[13] Our findings on firm exit are related to those in Bekelis et al. (2017), who study the heterogeneity among physicians who provide fewer carotid revascularizations. They find that more-experienced surgeons, and those for whom carotid revascularizations made up the lowest share of their services, were the ones who cut back on this procedure the most.

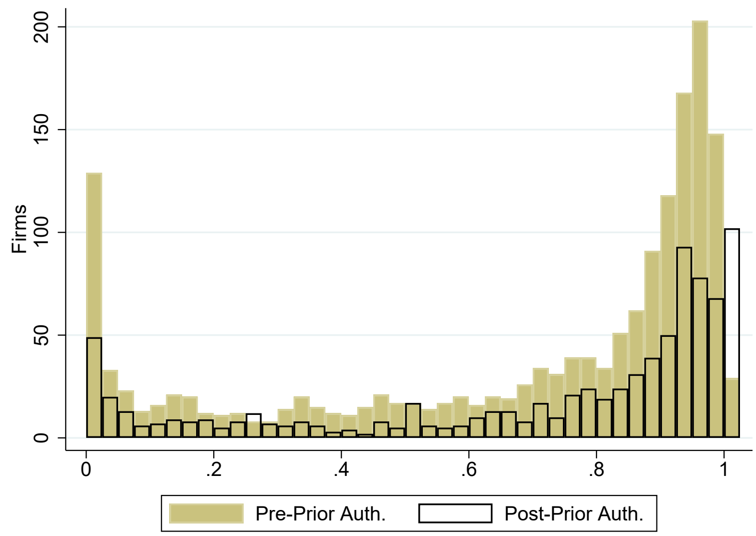
Table 6: Summary Statistics for Riders of Exiting Firms

| | Status of Exiting Firm | |
|---|---|---|
| | Not Indicted | Indicted |
| Continues Riding | 0.510 | 0.572 |
| Is Treated without Riding | 0.369 | 0.388 |
| Dies This Month | 0.060 | 0.013 |
| Is Hospitalized This Month | 0.038 | 0.007 |
| Is Not Treated Next Month | 0.022 | 0.020 |
| Observations | 4480 | 152 |

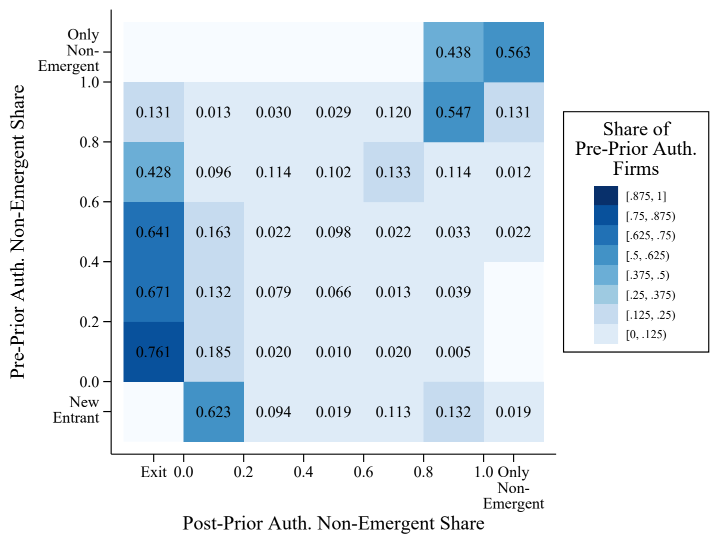
*Notes:* The sample is limited to patients that that rode with a firm in the two months prior to that firm's exit. Rows represent shares of patients in mutually exclusive categories of the patient's activity in the following month.

Figure 7: Change in Distribution of Firms by Share of Rides that are Non-Emergent

(a) Distribution of Firms




(b) Change by Initial Share




*Notes:* Panel (a) gives the distribution of ambulance firms that served dialysis patients from 2012-2017 in states subject to prior authorization. A firm's pre-prior authorization non-emergent share is determined by the the share of total rides given by the firm from 2012 until the start of prior authorization in that state that were non-emergent rides between a dialysis treatment facility and a patient's residence. The post-prior authorization share is the same share from the implementation of prior authorization through 2017. In panel (a), firms that gave no non-emergent dialysis rides in the relevant period are excluded.
Panel (b) gives the share of firms with pre-prior authorization non-emergent shares in each 20 percentage point bin that transition to each bin in the post-prior authorization period. Note that firm entry and exit are determined by a firm doing no non-emergent dialysis rides in the relevant period, while non-emergent only firms performed no emergent or non-dialysis non-emergent rides for dialysis beneficiaries.

# 6 Prior Authorization's Effect on Patient Health

Prior authorization reduced the number of non-emergent ambulance rides taken by dialysis patients. Although this regulation caused a sharp decline in potentially fraudulent payments, the additional administrative burden may have resulted in some patients forgoing treatment if they could not find alternative transportation. If these missed sessions resulted in adverse events like hospitalization or death, Medicare's savings from fewer ambulance reimbursements could have been offset by higher costs in other parts of the ESRD program, as well as a lower quality of life for the affected patients.

To assess the impact of prior authorization on health outcomes, we estimate Equation (4) at the patient-month level, with measures of patients' health as our outcome variables. We control for a rich set of patient and facility characteristics, including facility fixed effects, while clustering our standard errors at the district level.

Table 7 presents the effects of prior authorization on patient adherence to dialysis, as well as downstream health outcomes like hospitalizations and mortality. We find no evidence that prior authorization led to either decreases in dialysis sessions or increases in adverse events. In fact at the 95% confidence level, we can rule out even a 0.3% decrease in monthly dialysis sessions.

Table 7: Effect of Prior Auth. on Adherence and Adverse Events

| | (1) Dialysis Sessions | (2) Mortality | (3) All-Cause Hosp. | (4) Fluid Hosp. |
|---|---|---|---|---|
| Prior Auth. | -0.0269* | 0.000296 | -0.00138 | -0.000817 |
| | (0.0123) | (0.000516) | (0.00168) | (0.000610) |
| Year-Month FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Pat/Fac Controls | 1 | 1 | 1 | 1 |
| R-squared | 0.00843 | 0.00387 | 0.0108 | 0.00412 |
| Dep. Var. Mean | 12.12 | 0.00988 | 0.122 | 0.0116 |
| Observations | 15077249 | 15077249 | 15077249 | 15077249 |

*Notes:* Table gives estimates of $\beta$ from equation (4) at the patient-month level. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility characteristics including chain ownership status, demographic characteristics of the ZIP code, and whether the facility is freestanding or hospital-based. Fluid hospitalizations are those for which the primary diagnosis indicates excess fluids, often an indication of insufficient dialysis. Standard errors clustered at the district level are given in parentheses. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Although we find that prior authorization did not harm patients' health on average, it could be that some patients were harmed in ways not captured by our point estimates. To consider this possibility, we restrict our sample to the group of patients most likely to be affected by the policy change: those who relied most heavily on ambulance rides prior to the reform. Specifically, we restrict our sample to patients who took at least 100 non-emergent ambulance rides to dialysis facilities before prior authorization and compare the outcomes of these frequent riders across districts throughout the staggered rollout of prior authorization. Table 8 shows that even for the most-frequent riders, we find no evidence that prior authorization resulted in worse health outcomes.

Table 8: Effect of Prior Auth. on Frequent Riders

| | (1) Dialysis Sessions | (2) Mortality | (3) All-Cause Hosp. | (4) Fluid Hosp. |
|---|---|---|---|---|
| Prior Auth. | 0.00454 | -0.00109 | -0.0109$^{+}$ | -0.00102 |
| | (0.0376) | (0.00223) | (0.00634) | (0.00232) |
| Year-Month FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Pat/Fac Controls | 1 | 1 | 1 | 1 |
| R-squared | 0.0395 | 0.00424 | 0.00856 | 0.00390 |
| Dep. Var. Mean | 11.87 | 0.0115 | 0.179 | 0.0155 |
| Observations | 905472 | 905472 | 905472 | 905472 |

*Notes:* Table gives estimates of $\beta$ from equation (4) at the patient-month level. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility characteristics including chain ownership status, demographic characteristics of the ZIP code, and whether the facility is freestanding or hospital-based. Fluid hospitalizations are those for which the primary diagnosis indicates excess fluids, often an indication of insufficient dialysis. The sample is limited to patients that took at least 100 non-emergent ambulance rides to dialysis under the non-prior authorization regime. Standard errors clustered at the district level are given in parentheses. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Another potentially unintended consequence of prior authorization is that some patients who satisfy Medicare's criteria for a reimbursable ride might not receive one if their ambulance company goes out of business. To assess this possibility, Table 9 shows what happens to riders in the month after their ambulance company exits the market. Compared to patients whose ambulance company exited before prior authorization, an exit not induced by anti-fraud regulation, those who rode with companies that exited during the first month of prior authorization were not less likely to receive treatment even though they were much less likely to continue riding. That is, patients riding with ambulance companies that exited immediately following prior authorization did not miss more sessions than a typical patient whose ambulance company exited before the start of prior authorization. Taken together, these results suggest that prior authorization for non-emergent ambulance rides did not adversely affect patients' health: patients continue to receive treatment at the same rate as before and have no uptick in hospitalizations or mortality.

Table 9: Summary Statistics for Riders of Exiting Firms by Prior Authorization Status

| | Pre-Prior Auth. | At Prior Auth. | Post-Prior Auth. |
|---|---|---|---|
| Continues Riding | 0.651 | 0.097 | 0.407 |
| Is Treated without Riding | 0.278 | 0.849 | 0.428 |
| Dies This Month | 0.029 | 0.029 | 0.093 |
| Is Hospitalized This Month | 0.023 | 0.010 | 0.058 |
| Is Not Treated Next Month | 0.019 | 0.015 | 0.014 |
| Observations | 835 | 517 | 432 |

*Notes:* The sample is limited to patients that that rode with a firm in the two months prior to that firm's exit. The sample is further limited to patients residing in states subject to prior authorization, with the "at prior authorization" period corresponding to the first month of prior authorization and the month prior. Rows represent shares of patients in mutually exclusive categories of the patient's activity in the following month.

# 7 Non-emergent Rides and Prior Authorization

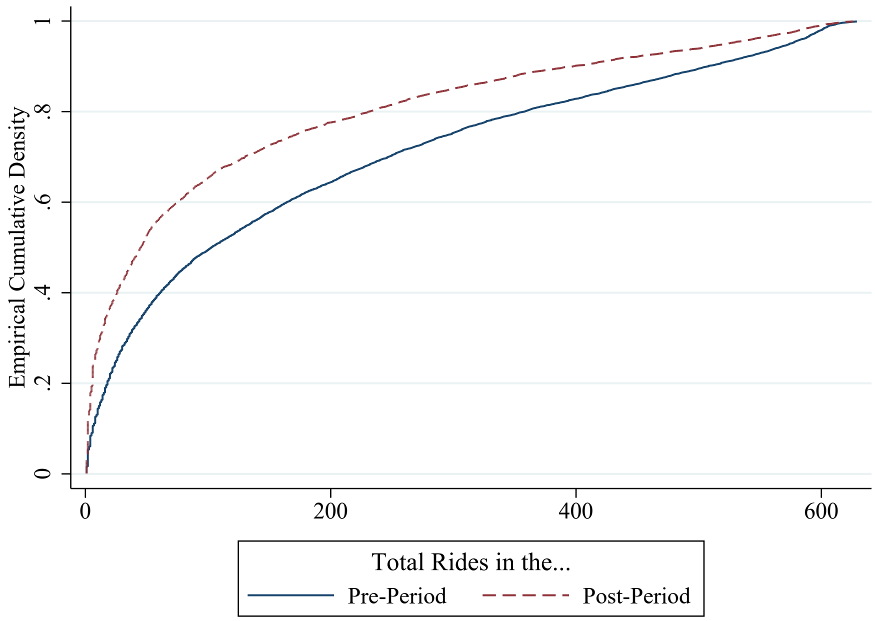
Although prior authorization greatly reduced the number of non-emergent dialysis rides, many patients continue to receive them despite the more-stringent regulations. Several stylized facts about these riders suggest that the regulation had its intended effect of ensuring that the patients who ride in ambulances are the ones who truly need to do so. First, column (1) of Table 10 shows that the probability that a current rider continues riding the following month falls after prior authorization, indicating that ridership is less persistent. Next, comparing the two years before prior authorization to the two years after, Figure 8 shows that the total number of rides taken by each rider decreased substantially. Finally, we find that the median number of months in which a rider takes a non-emergent ride falls from six months to three.

As further evidence that prior authorization resulted in a more-appropriate mix of patients taking ambulance rides, we note that patients who took many rides before prior authorization were more likely to continue riding after the policy change. Specifically, we show in Table 10 that, conditional on riding, the total number of rides taken over the life of the patient increased following prior authorization. This change occurred suddenly, as shown in Figure 9. Similarly, we find that the probability of suffering an adverse event during the same month a ride is taken — likely reflecting a legitimate need for an ambulance — increased after prior authorization. Taken as a whole, these results indicate that the patients who receive non-emergent ambulance rides after the start of prior authorization are less healthy, which is consistent with Medicare's aim for the program — to provide rides only when medically necessary.

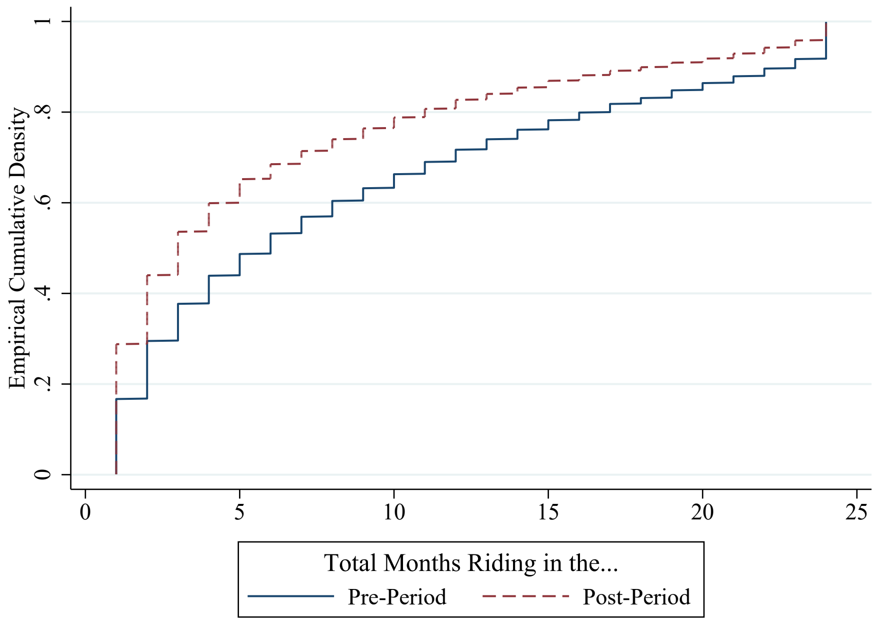
The denial rate for submitted claims provides additional evidence that prior authorization resulted in a more-appropriate use of ambulance rides. Although we do not observe requests submitted for prior authorization, we do observe whether a claim was paid if it was submitted

Figure 8: Empirical CDF of Ridership Among Riders

(a) Total Rides Taken




(b) Months Taking At Least One Ride




*Notes:* Panel (a) gives the empirical cumulative density functions of total rides taken by patients in districts subject to prior authorization in the 24 months before and after the implementation of prior authorization. Panel (b) gives analogous empirical cumulative density functions for the total number of months in which the patient takes at least one ride. All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data.

Table 10: Effect of Prior Auth. on Patient Selection

| | (1) Rides Next Month | (2) Lifetime Rides | (3) Hospitalizations | (4) Mortality |
|---|---|---|---|---|
| Prior Auth. | -0.0582*** | 71.01*** | 0.0142+ | 0.00719* |
| | (0.0113) | (7.458) | (0.00853) | (0.00318) |
| Year-Month FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Pat/Fac Controls | 1 | 1 | 1 | 1 |
| R-squared | 0.0758 | 0.174 | 0.0161 | 0.00857 |
| Dep. Var. Mean | 0.829 | 549.7 | 0.256 | 0.0353 |
| Observations | 604348 | 604348 | 604348 | 604348 |

*Notes:* Table gives estimates of $\beta$ from equation (4) at the patient-month level. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility characteristics including chain ownership status, demographic characteristics of the ZIP code, and whether the facility is freestanding or hospital-based. Sample is limited to patient-months in which the patient receives at least one non-emergent dialysis ambulance ride. Standard errors clustered at the district level are given in parentheses. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

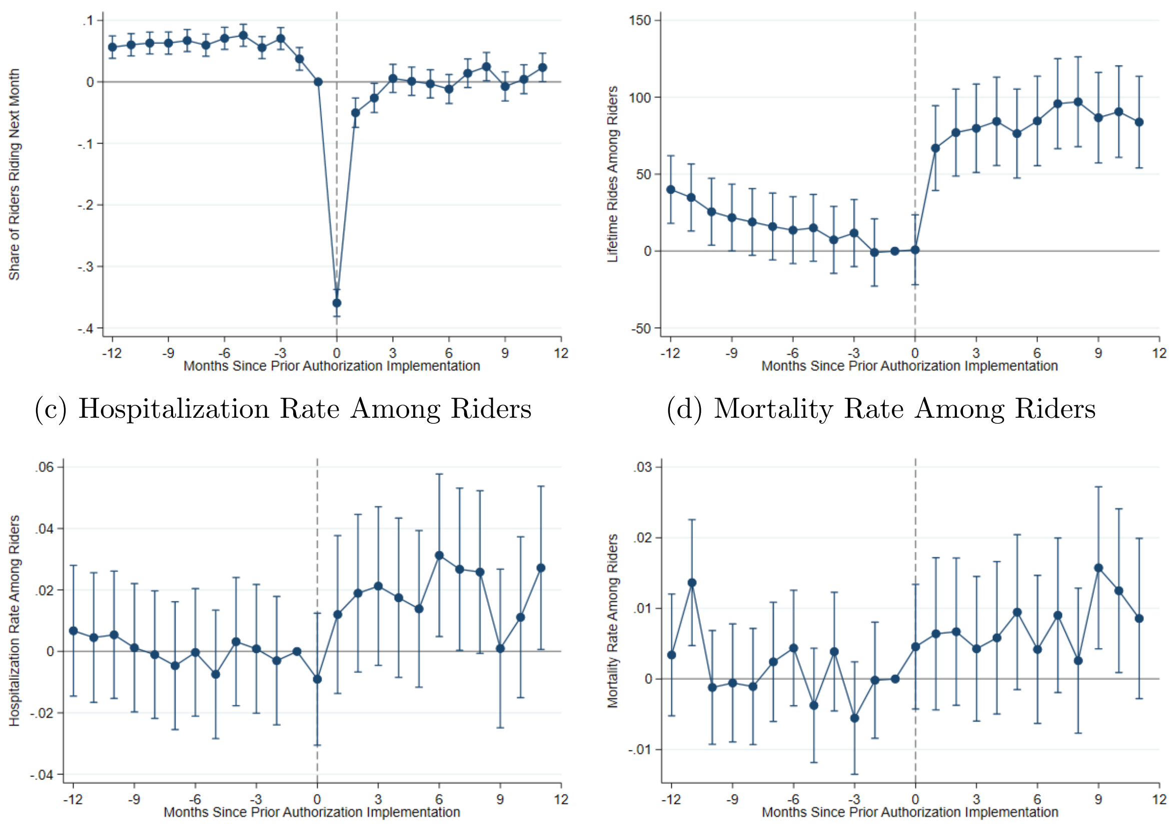
after the service was rendered. Figure 10 shows that immediately following prior authorization, the share of denied claims jumped sharply and then declined gradually.[14] That the denial rate decreased following the initial spike indicates that some firms stopped submitting claims that would be denied under the heightened scrutiny of prior authorization, which we interpret as evidence that prior authorization acts as a screening mechanism that effectively deters fraud.

# 8 Regulation versus Litigation

An extensive literature has considered whether it is better to use regulation or litigation to combat illegal behavior. Much of this prior work has addressed torts and property rights violations, where individuals or private parties are harmed. Our work provides a natural extension of these studies to circumstances where the injured party is the government and the type of crime is financial fraud. Moreover, our study makes a novel contribution given that many of the canonical results on deterrence do not apply to this form of illicit activity. In particular, the large literature examining torts and the assignment of property rights, such as Coase (1960), provides little guidance on how to efficiently deter financial fraud against the government. We therefore revisit the question of when and how litigation may efficiently deter fraud on its own, or when regulation must be used in conjunction with it.

[14]Because these denial rates capture only claims that were submitted after providers could obtain prior authorization for the service, rather than including those that were denied prior authorization, the increase in denial rates after prior authorization is likely a lower bound for the true increase. Indeed, Centers for Medicare and Medicaid Services (2020a) reports that in the first year of prior authorization, only 35% of prior authorization requests were affirmed, while in subsequent years this number was between 57–66%.

Figure 9: Effect of Prior Auth. on Patient Selection




(a) Share of Riders Riding Next Month

(b) Total Rides Taken

(c) Hospitalization Rate Among Riders

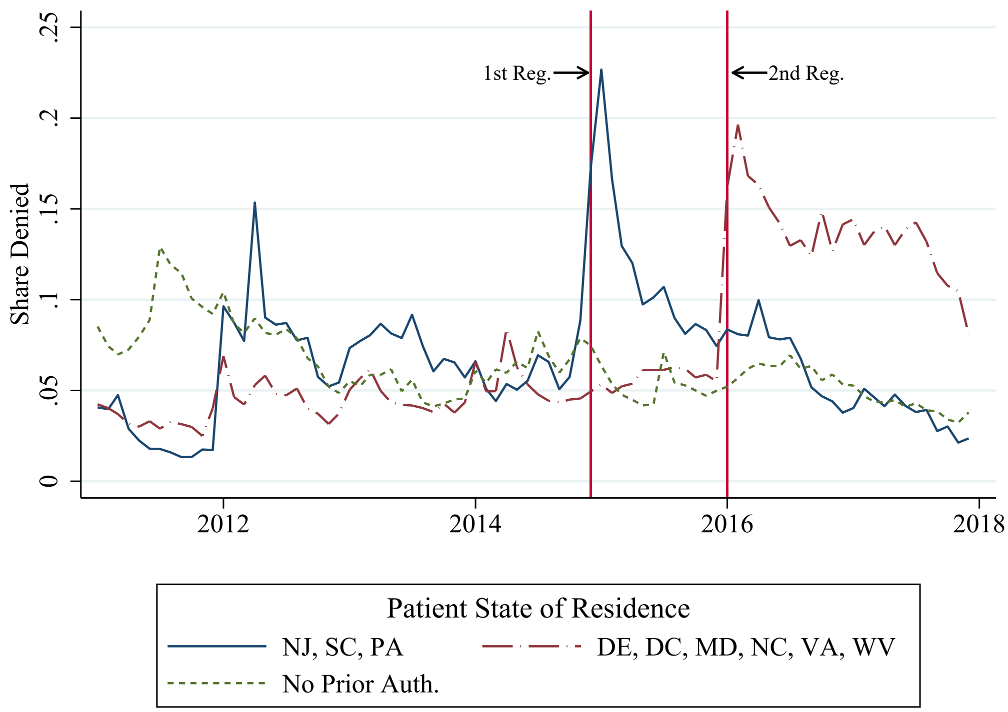
(d) Mortality Rate Among Riders

*Notes:* Estimates of $\beta_e$ for $e \in [-12, 11]/\{-1\}$ from equation (3). These data include rides from 2011–2017. An observation is a patient-month. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility characteristics including chain ownership status, demographic characteristics of the ZIP code, and whether the facility is freestanding or hospital-based. Sample is limited to patient-months in which the patient receives at least one non-emergent dialysis ambulance ride. Standard errors are clustered at the district level. Error bars represents the pointwise 95% confidence interval.

## 8.1 Why Litigation Failed

In the case of ambulance fraud, the government faces a number of constraints that make litigation unlikely to have a widespread effect on illegal behavior. First among these is the government's limited ability to levy large penalties on the fraudulent firms and operators we study, a necessary component of effective deterrence (Becker, 1968). Litigation may fail to curtail illicit behavior if severe penalties cannot be enforced (Shavell, 1984), like in the case of fly-by-night ambulance companies that may shut down or spend their ill-gotten gains before authorities can recover the financial penalties imposed by the courts. Even among successfully prosecuted firms, the likelihood of receiving full restitution is low. Despite regularly reaching millions of dollars, the Department of Justice itself warns that restitution for criminal penalties is often

Figure 10: Claim Denial Rates by Prior Authorization Status




*Notes:* The sample includes non-emergent basic life support ambulance rides from a dialysis facility to a place of residence for ESRD patients from 2011–2017. State is determined by the transported patient's state of residence. Vertical lines mark the implementation of prior authorization in NJ, SC, and PA, and in DE, DC, MD, NC, VA, and WV. The share of claims denied is the share of rides for which the submitted claim was not paid any positive amount.

difficult to enforce, writing, "Realistically, however, the chance of full recovery is very low...it is rare that defendants are able to fully pay the entire restitution amount owed" (Department of Justice, 2021). The difficulty of enforcing financial penalties may explain why we find that civil lawsuits are less effective than criminal enforcement. Civil lawsuits only impose monetary penalties or exclusion from the Medicare program, penalties that will not have much effect on firms that can simply shut down rather than change their behavior. Conversely, criminal lawsuits can impose jail time on the owners or operators of fraudulent firms, a non-monetary penalty that can be enforced even in the absence of recoverable funds.

Litigation may also have been ineffective because federal attorneys were unwilling to prosecute beneficiaries for being complicit in ambulance fraud. Patients were often a key part of the schemes, with criminal lawsuits alleging they received kickbacks for riding and for referring others — in one particularly striking case, a patient threatened to report ambulance company employees to the police if they did not increase her kickback. Moreover, about 2,000 patients immediately stopped riding in the first three states subject to prior authorization, perhaps reflecting a large faction of complicit beneficiaries. Despite such compelling evidence of their widespread involvement, the government has only criminally prosecuted a handful of dialysis patients for ambulance fraud, likely owing to the generally sympathetic nature of dialysis patients as well as the exorbitant costs of imprisoning them in one of the six overcrowded Bureau of Prisons Medical Centers, the highest-severity institutions (Office of the Inspector General, 2015; Federal Bureau of Prisons Clinical Guidance, 2019).

In addition to the challenge of levying high penalties against proven lawbreakers, litigation may be hampered by the difficulty of detecting and successfully prosecuting illicit behavior at a sufficiently large scale: over 3,000 firms participated in non-emergent ambulance transportation of dialysis patients over our sample period, yet only 98 companies or individuals were ever prosecuted. That prior authorization was so effective at deterring medically unnecessary rides even after litigators and prosecutors had already made concerted efforts to stop them further reflects a low detection rate. For example, the ten lawsuits filed by the Pennsylvania East judicial district were more than any other district, but despite such active litigation, the number of active firms still fell from 83 to 47 in the three months immediately following prior authorization, and the number of rides fell even further: an astounding 87.5% drop from 10,653 to 1,327. The large number of firms exiting in the face of regulation, even after extensive prosecution, suggests that criminal enforcement did not do much to deter fraudulent behavior.

A lack of specialization may partly explain these low detection rates (Landis, 1938). Twenty-three different judicial districts were involved in the lawsuits we study, which means 23 different sets of investigators, attorneys, and judges were responsible for understanding the complex nature of this fraud in order to successfully prosecute it. Moreover, the Department of Justice attorneys who work on health care fraud are responsible for enforcing many other parts of the federal criminal and civil code, as are the judges who try the cases.

Health care fraud may also be difficult to prove after the fact. Criminal lawsuits require a "beyond a reasonable doubt" evidentiary standard, and establishing a lack of medical necessity to this standard is challenging: the Department of Justice must amass incontrovertible evidence, such as video recordings of purportedly bedridden patients walking. With over 3,000 firms participating in nonemergency transportation, such cases cannot be widely prosecuted given the limited resources of the Department of Justice and the Federal Bureau of Investigation.

That the injured party is the government in our setting, rather than a private party, is another reason why litigation may not deter much illicit behavior on its own. Behrer et al. (2021) and Mookherjee and Png (1992) argue that litigation alone will be ineffective when the harm in question affects a large number of individuals and the private reporting of harm is insufficient. The injured party in the case of health care fraud is every American taxpayer, and individuals are not empowered to protect the public interest. The government also faces agency problems, because the stolen money does not directly impact the federal employee. That is, the failure to detect health care fraud has limited consequences for those responsible for combating it.

## 8.2 Why Regulation Succeeded

In contrast to criminal and civil enforcement, regulation effectively deterred the type of health care fraud we study. To better understand why, we place our results within the framework of Glaeser and Shleifer (2003) that compares pure litigation-based enforcement to a regime that also uses administrative rules. Most relevant for our setting, they find that adding administrative rules is optimal in cases where litigation can be subverted. As noted above, litigation can be most effective when the enforcer is able to assess large penalties (Becker, 1968), yet larger penalties provide a stronger incentive for subversion (Glaeser and Shleifer, 2003). Although not addressed in prior work, the unwillingness of prosecutors to pursue complicit beneficiaries and the challenge of recovering stolen funds from fly-by-night firms are both forms of subversion that also make litigation ineffective at assigning liability. Conversely, prior authorization prevented fraudulent funds from ever being paid out in the first place, making it unnecessary to assign ex post liability.

Regulation may also be superior to litigation because it is easier to enforce at a large scale. Compared to the low rates of detection and punishment through the courts, claim denial rates rose above 20% after the start of prior authorization. In this case, MAC administrators successfully detected noncompliant rides en masse, underscoring the benefits of using regulations when litigation faces capacity constraints (Glaeser and Shleifer, 2003). Moreover, regulations may improve detection rates by making noncompliance more obvious. Although courts may find it difficult to assess medical necessity, regulations can include "bright-line rules" that are easy to monitor (Kaplow, 1992; Glaeser and Shleifer, 2002). For prior authorization, it is much simpler to show that a firm failed to submit paperwork than it is to prove a patient did not have a legitimate medical reason for taking an ambulance ride.

Relatedly, administrative enforcers can be more specialized than judges or prosecutors, which facilitates monitoring (Landis, 1938). Moreover, regulators are able to conduct investigations and design effective policies, while the judicial system produces case law based on the facts presented to the judge. In the case of ambulance fraud, assessing medical necessity requires specialized knowledge by the enforcer. Compared to Department of Justice attorneys, the MAC administrators responsible for checking prior authorization requirements focus solely on Medicare regulations. Thus, while the Department of Justice must convince unspecialized judges and juries that care was not medically necessary, MAC administrators can more competently and efficiently examine supporting documentation and decide whether a reimbursement is justified.

## 8.3 Broader Effects

In addition to deterrence, regulation and litigation can produce other effects that are difficult to measure empirically. In response to increased scrutiny, some may choose to forgo fraud in the first place, a general deterrence effect of unknown magnitude (Shavell, 1991; Leder-Luis, 2019). Conversely, individuals intent on committing health care fraud may substitute away from one particular scheme and pursue others that are more difficult for authorities to detect.

Regulation and litigation both have costs that affect their relative efficiency. Because monitoring paperwork for prior authorization is much simpler than ex post enforcement against fraudulent claims, regulation can likely accomplish the same level of deterrence at a much lower cost. The chief actuary for CMS estimated the cost of implementing prior authorization nationwide at only "$38.1 million in the first expansion year and $28.6 million per year in subsequent years," substantially less than the potential cost of prosecuting all fraudulent ambulance firms (Spitalnic, 2018). Furthermore, litigation expends scarce Department of Justice and Federal Bureau of Investigation resources, which may come with the opportunity cost of less effort in other areas.

On the other hand, regulation may be costly if it results in care being rationed inefficiently (American Medical Association, 2021) or imposes large hassle costs on patients (Herd and Moynihan, 2018) and providers (Dunn et al., 2021). In the context of non-emergent ambulance rides for dialysis patients, we find no evidence supporting the former concern, as discussed in Section 6, although this result may not hold for regulations in other settings. In terms of hassle costs, it is unlikely that patients or physicians bear large costs from prior authorization, as ambulance companies are largely responsible for supplying a proof of medical necessity to MAC administrators. On the other hand, our finding that some ambulance companies became more specialized in non-emergent rides after the reform, as shown in Figure 7, could reflect a barrier to entry for suppliers of non-emergent rides. This finding is also consistent with theoretical evidence that regulations can be efficient even when some firms profit from regulatory capture (Glaeser and Shleifer, 2003).

One potential benefit of imposing hassle costs through regulation, however, is the potential for regulation to act as a screening mechanism. If the regulation is well targeted, only medically necessary services will be rendered, as providers and patients anticipate that only proper claims will be approved (Zeckhauser, 2021). The pattern of denial rates shown in Figure 10 provides evidence of this phenomenon: following an initial spike in denied claims, the denial rate starts to fall once firms begin submitting claims only for medically necessary rides.

# 9 Conclusion

In this paper, we show that prior authorization is much more effective than criminal or civil enforcement at reducing wasteful ambulance rides. Prior authorization caused an immediate and persistent drop in non-emergency ambulance rides of nearly 53%, whereas criminal and civil lawsuits had a much smaller effect. Had the federal government required prior authorization when it first criminally prosecuted fraudulent activity in 2003, it would have saved $4.8 billion and prevented 21.2 million unnecessary rides.[15] Given the relative costs of litigation and regulation, we find that implementing prior authorization is much more efficient.

Importantly, we show that the decrease in non-emergent rides did not come at the expense of patient health even though it drove many ambulance companies out of the market. Following prior authorization, the dialysis patients who continued taking non-emergent ambulance rides were in poorer health, suggesting that the Medicare benefit was being used more efficiently.

We connect our results to the economic theory that explains why regulation is necessary — and litigation alone insufficient — for successfully combatting ambulance fraud, which also applies more broadly throughout the health care system. Criminal and civil penalties are often too low given prosecutors' unwillingness or inability to levy high penalties against patients or fly-by-night firms, and prosecution rates are held back by the challenges of detecting fraud, the diffuse nature of the harm, and the limited resources of unspecialized enforcers. In addition, litigation is unlikely to deter fraud more generally because health care fraud requires more specialization than other criminal activities. This points to health care fraud as being an area in need of regulatory innovations to complement any legal enforcement that comes through prosecution.

Medicare has recently moved in this direction by expanding prior authorization to other areas of health care that may be susceptible to fraud, including power mobility devices, home health services, and hyperbaric oxygen.[16] Our results indicate that these efforts are likely to be successful, motivating our future work on this topic.

[15] This is the sum of the amount paid from 2003–2017 for non-emergent dialysis ambulance rides above the mean levels within prior authorization waves from 2003 and 2004 ($4.1 billion dollars on 17.5 million rides) and the difference between realized spending and a linear projection of the spending within prior authorization waves from the time of the first wave of prior authorization to the end of 2017, fitting the trend to the five previous years ($703 million on 3.7 million rides).

[16] For more information on CMS's prior authorization programs, see `https://www.cms.gov/research-statistics-data-systems/medicare-fee-service-compliance-programs/prior-authorization-and-pre-claim-review-initiatives`.

## References


American Medical Association (2021). 2020 ama prior authorization (pa) physician survey. Technical report, American Medical Association.

Athey, S. and G. W. Imbens (2021). Design-based analysis in difference-in-differences settings with staggered adoption. *Journal of Econometrics*.

Becker, G. S. (1968). Crime and punishment: An economic approach. *Journal of Political Economy 76*(2), 169–217.

Behrer, A. P., E. L. Glaeser, G. A. M. Ponzetto, and A. Shleifer (2021). Securing property rights. *Journal of Political Economy*.

Bekelis, K., J. Skinner, D. Gottlieb, and P. Goodney (2017). De-adoption and exnovation in the use of carotid revascularization: retrospective cohort study. *BMJ 359*.

Borusyak, K., X. Jaravel, and J. Spiess (2021). Revisiting event study designs. *Available at SSRN 2826228*.

Callaway, B. and P. H. Sant'Anna (2020). Difference-in-differences with multiple time periods. *Journal of Econometrics*.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019, 05). The Effect of Minimum Wages on Low-Wage Jobs*. *The Quarterly Journal of Economics 134*(3), 1405–1454.

Centers for Medicare and Medicaid Services (2020a, Nov). Medicare Prior Authorization Model for Repetitive, Scheduled Non-Emergent Ambulance Transports Status Update. `https://www.cms.gov/files/document/status-update-11-18-2020.pdf`.

Centers for Medicare and Medicaid Services (2020b, Dec). Repetitive, scheduled non-emergent ambulance transport (RSNAT) prior authorization model frequently asked questions. Woodlawn, MD. `https://www.cms.gov/Research-Statistics-Data-and-Systems/Monitoring-Programs/Medicare-FFS-Compliance-Programs/Prior-Authorization-Initiatives/Downloads/AmbulancePriorAuthorization_ExternalFAQ_121517.pdf`.

CMS (2020). Fact sheet: 2020 estimated improper payment rates for centers for medicare and medicaid services (cms) programs.

Coase, R. H. (1960). The problem of social cost. *Journal of Law and Economics 3*(2), 1–44.

Dafny, L. S. (2005). How do hospitals respond to price changes? *American Economic Review 95*(5), 1525–1547.

de Chaisemartin, C. and X. D'Haultfœuille (2020, September). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review 110*(9), 2964–96.

Department of Justice (2021). Restitution process.

Dunn, A., J. D. Gottlieb, A. Shapiro, D. J. Sonnenstuhl, and P. Tebaldi (2021, July). A denial a day keeps the doctor away. Working Paper 29010, National Bureau of Economic Research.

Eliason, P. J., B. Heebsh, R. C. McDevitt, and J. W. Roberts (2020). How acquisitions affect firm behavior and performance: Evidence from the dialysis industry. *Quarterly Journal of Economics 1*(135), 221–267.

Esson, M. I. (2021). It's an emergency: Do medicare reimbursement rules increase unnecessary ambulance transports? *Working Paper*.

Fang, H. and Q. Gong (2017). Detecting potential overbilling in medicare reimbursement via hours worked. *American Economic Review 107*(2), 562–591.

Fang, H. and Q. Gong (2020). Detecting potential overbilling in medicare reimbursement via hours worked: Reply. *American Economic Review 110*(12), 4004–4010.

Federal Bureau of Prisons Clinical Guidance (2019). Care level classification for medical and mental health conditions or disabilities.

Glaeser, E. L. and A. Shleifer (2002, 11). Legal Origins. *The Quarterly Journal of Economics 117*(4), 1193–1229.

Glaeser, E. L. and A. Shleifer (2003, June). The rise of the regulatory state. *Journal of Economic Literature 41*(2), 401–425.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*.

Herd, P. and D. P. Moynihan (2018). *Administrative Burden: Policymaking by Other Means.* Russell Sage Foundation.

Howard, D. H. and I. McCarthy (2021). Deterrence effects of antifraud and abuse enforcement in health care. *Journal of Health Economics 75*, 102405.

Kaplow, L. (1992). Rules versus standards: An economic analysis. *Duke Law Journal 42*(3), 557–629.

Landis, J. M. (1938). *The administrative process.* Yale University Press.

Leder-Luis, J. (2019). Whistleblowers, the false claims act, and the behavior of healthcare providers. *Working Paper RePEc*. https://ideas.repec.org/jmp/2019/ple1069.pdf.

Levitt, S. D. (1997). Using electoral cycles in police hiring to estimate the effect of police on crime. *The American Economic Review 87*(3), 270–290.

Matsumoto, B. (2020). Detecting potential overbilling in medicare reimbursement via hours worked: Comment. *American Economic Review 110*(12), 3991–4003.

Mookherjee, D. and I. P. L. Png (1992). Monitoring vis-á-vis investigation in enforcement of law. *The American Economic Review 82*(3), 556–565.

Office of the Inspector General, U. D. o. J. (2015). The impact of an aging inmate population on the federal bureau of prisons.

O'Malley, A. J., T. A. Bubolz, and J. S. Skinner (2021). The diffusion of health care fraud: A network analysis. Technical report, National Bureau of Economic Research.

Sanghavi, P., A. B. Jena, J. P. Newhouse, and A. M. Zaslavsky (2021). Identifying outlier patterns of inconsistent ambulance billing in medicare. *Health Services Research 56*(2), 188–192.

Shavell, S. (1984). Liability for harm versus regulation of safety. *The Journal of Legal Studies 13*(2), 357–374.

Shavell, S. (1991, 10). Specific versus General Enforcement of Law. *Journal of Political Economy*.

Silverman, E. and J. Skinner (2004). Medicare upcoding and hospital ownership. *Journal of Health Economics 23*(2), 369–389.

Spitalnic, P. (2018). Certification of medicare prior authorization model for repetitive scheduled non-emergent ambulance transport (rsnat). Technical report, Centers for Medicare and Medicaid Services.

Sun, L. and S. Abraham (2020). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*.

The United States Department of Justice (2018, Mar). Find your united states attorney.

Wilson, W. (1913). *The new freedom: A call for the emancipation of the generous energies of a people*. Doubleday.

Zeckhauser, R. (2021). Strategic sorting: the role of ordeals in health care. *Economics & Philosophy 37*(1), 64–81.

# APPENDIX

## A US Court Districts

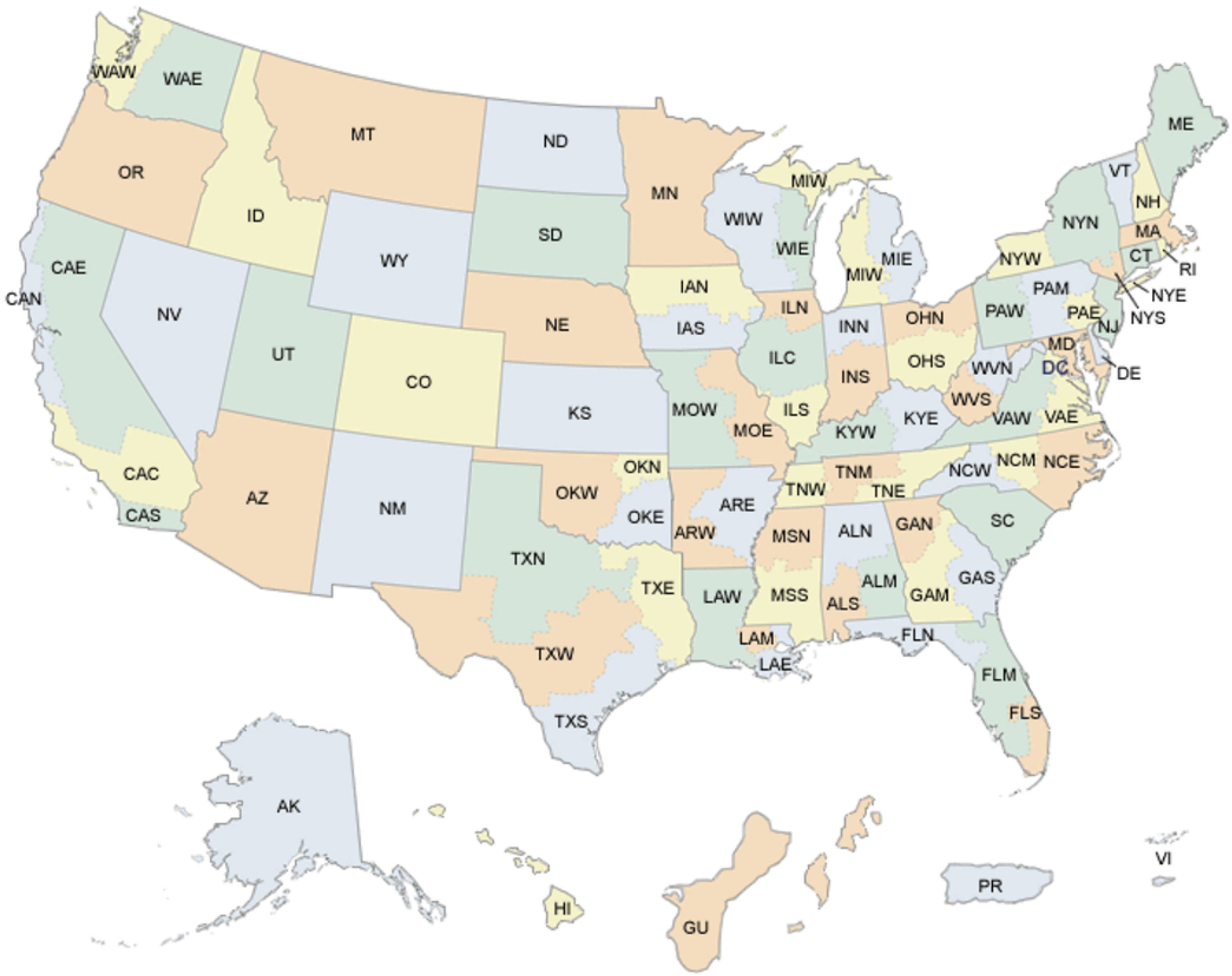



Figure A1: Map of US Court Districts from The United States Department of Justice (2018)

# B Balance Table

Table B1: Summary Statistics of Patient-Month Level Data by Prior Authorization Wave

| | Prior Authorization Wave | | | |
|---|---|---|---|---|
| | Wave 1 | Wave 2 | Not Yet Treated | Overall |
| **Patient Characteristics** | | | | |
| Age (Years) | 64.23 | 62.69 | 62.77 | 62.90 |
| Months with ESRD | 53.34 | 55.81 | 53.03 | 53.34 |
| Black | 0.462 | 0.635 | 0.350 | 0.389 |
| Male | 0.556 | 0.530 | 0.543 | 0.543 |
| Diabetic | 0.504 | 0.514 | 0.541 | 0.535 |
| Drug User | 0.015 | 0.019 | 0.013 | 0.014 |
| Smoker | 0.065 | 0.074 | 0.062 | 0.063 |
| Drinker | 0.016 | 0.015 | 0.013 | 0.014 |
| Uninsured at Incidence | 0.103 | 0.120 | 0.128 | 0.125 |
| Employed at Incidence | 0.160 | 0.171 | 0.158 | 0.160 |
| **Ridership** | | | | |
| Non-Emergent Dialysis Rides | 3.12 | 0.91 | 0.77 | 1.01 |
| Emergent Rides | 0.127 | 0.124 | 0.124 | 0.124 |
| Total Lifetime Rides | 122.3 | 40.9 | 36.2 | 44.7 |
| Continuing to Ride Next Month | 0.890 | 0.851 | 0.835 | 0.852 |
| **Health Outcomes** | | | | |
| Dialysis Sessions | 12.12 | 12.12 | 12.13 | 12.12 |
| All-Cause Hosp. | 0.134 | 0.126 | 0.125 | 0.126 |
| Fluid Hosp. | 0.017 | 0.016 | 0.014 | 0.015 |
| Mortality | 0.011 | 0.010 | 0.010 | 0.010 |
| Patient-Months | 1,002,102 | 1,081,465 | 8,564,126 | 10,647,693 |

*Notes:* Data are from 2011–2014. Patient characteristics except age and dialysis tenure are at incidence of ESRD. All ridership variables other than emergent rides are based on non-emergent basic life support rides between a dialysis facility and a patient's home. Fluid hospitalizations are those for which the primary diagnosis indicates excess fluids, an indication of insufficient dialysis. State is determined by the transported patient's state of residence. Wave 1 states are NJ, SC, and PA, and wave 2 states are DE, DC, MD, NC, VA, and WV.

# C Alternative Estimation Methods

In settings that have heterogeneous treatment effects along different dimensions, traditional TWFE models may not recover the average effect of treatment on the treated ($ATT$).[17] To overcome this issue, we use several recently introduced methods to estimate the results we present in Sections 4 and 5.

## C.1 Callaway and Sant'anna

The first of these methods is the group-time average treatment effect estimator introduced by Callaway and Sant'Anna (2020). This method estimates the effect of treatment separately for each group of districts treated at the same time, using only districts that are never treated as the control group. That is, we estimate Equation (1) for each group of districts treated at the same time and those districts that never receive treatment separately for each group.[18] Under weak assumptions, this method recovers the average treatment effect at time $t$ for the group of districts treated at time $g$, which we refer to as $ATT(g,t)$. To simplify the interpretation of our results, we aggregate the $ATT(g,t)$ of each treatment group across time to obtain a treatment-group specific parameter analogous to the $\beta$ recovered using traditional TWFE methods. The parameter

$$\theta_{sel}(\tilde{g}) = \frac{1}{\mathcal{T} - \tilde{g} + 1} \sum_{t=\tilde{g}}^{\mathcal{T}} ATT(\tilde{g}, t) \tag{5}$$

gives the average treatment effect on districts treated at time $\tilde{g}$ from the first month in which they are treated until the last month in our data, $\mathcal{T}$.

Because we want to analyze the dynamic effects of treatment parsimoniously even though few districts are treated at any given time, we also aggregate our results across groups to recover the effect of treatment after $e = t - g$ months of exposure to treatment. And because districts are treated at different times, some treatment groups are treated later in our sample period than others, which means we must aggregate the results across groups to account for any compositional changes in treated units at different lengths of exposure. To do this, we only aggregate $ATT(g,t)$ for groups that are treated for at least $L$ months and recover the average treatment effect for treatment of length $e$ on districts that are treated for at least $e'$ periods,

$$\theta_{es}^{bal}(e;L) = \sum_{g\in\mathcal{G}} \mathbf{1}\{g + L \leq \mathcal{T}\} ATT(g, g+e) P(G = g | G + L \leq \mathcal{T}), \tag{6}$$

[17] See, for example, Borusyak et al. (2021); de Chaisemartin and D'Haultfœuille (2020); Goodman-Bacon (2021); Sun and Abraham (2020); Athey and Imbens (2021).

[18] Because this method does not allow for time-varying controls, $\Gamma X_{dt}$ is not included in our estimating equation using this estimator.

where $\mathcal{G}$ gives the set of treatment times and $\mathcal{T}$ is the last month in our data.

Finally, we further aggregate $ATT(g,t)$ into a single parameter that gives the average treatment effect for the first $L$ months of treatment in districts treated for at least $L$ months. This parameter is given by

$$\theta_{es}^{O,bal}(L) = \frac{1}{L+1}\sum_{e=0}^{L}\theta_{es}^{bal}(e,L), \tag{7}$$

which is simply the unweighted average of the parameters given by Equation (6) across the first $L$ months of treatment. Like the estimates of Equation (2) given in Section 4, this parameter estimates the effect of treatment relative to the time period immediately before treatment. This parameter, along with $\theta_{es}^{bal}(e;L)$ can be estimated using the `did` package in R.

Table C2: Effect of Prior Auth. on Ambulance Rides and Spending, Callaway and Sant'anna

| | (1) Total Ride Payments | (2) Total Ride Payments (Log) | (3) Total Rides | (4) Total Rides (Log) | (5) Active Firms | (6) Active Firms (Log) |
|---|---|---|---|---|---|---|
| Prior Auth. | -681107.6 | -1.110$^{**}$ | -3430.1 | -0.894$^{***}$ | -11.42$^{+}$ | -0.304$^{***}$ |
| | (525970.4) | (0.428) | (2635.9) | (0.173) | (5.955) | (0.0758) |

*Notes:* Estimates of $\theta_{es}^{O,bal}(23)$ using methods from Callaway and Sant'Anna (2020). All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variable in columns (2), (4), and (6) are transformed by adding 1 and taking the natural log. These data include rides from 2011–2017. An observation is a district-month. Standard errors are obtained using Callaway Sant'anna's bootstrap-based procedure. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

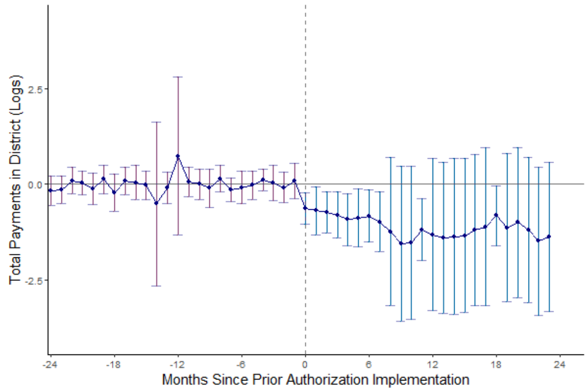
Table C3: Effect of Litigation on Ambulance Spending and Rides, Callaway and Sant'anna

| | Civil | | Criminal | |
|---|---|---|---|---|
| | (1) Total Ride Payments (Log) | (2) Total Rides (Log) | (3) Total Ride Payments (Log) | (4) Total Rides (Log) |
| Enforcement | 0.0697 | 0.1027$^{*}$ | -0.0103 | -0.0677 |
| | (0.0950) | (0.0452) | (0.0877) | (0.0950) |

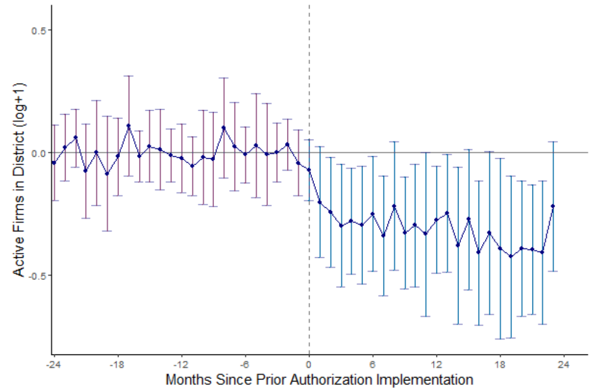
*Notes:* Estimates of $\theta_{es}^{O,bal}(23)$ using methods from Callaway and Sant'Anna (2020). All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district-month. The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are obtained using Callaway Sant'anna's bootstrap-based procedure. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Figure C2 presents estimates of $\theta_{es}^{bal}(e;23)$ for $e \in [-24, 23]$. We find that this estimation method results in similar estimates as those given in Figures 4, 5, and 6.

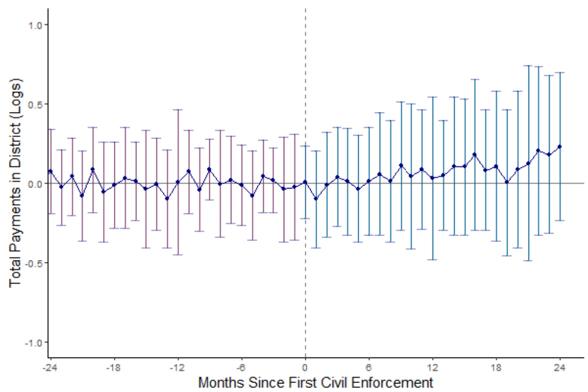
Figure C2: Dynamic Effects of Enforcement, Callaway and Sant'anna



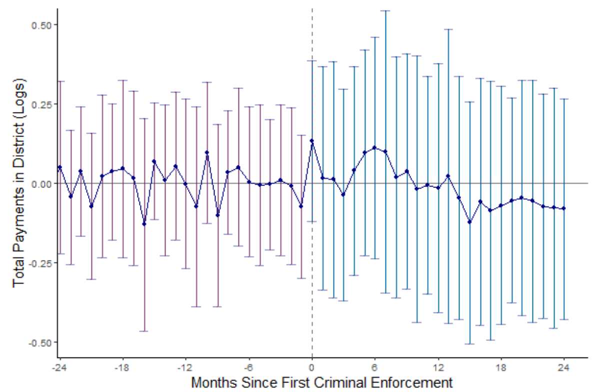

(a) Prior Auth: Total Payments (log)




(b) Prior Auth: Active Firms (log)




(c) Civil: Total Payments (log)




(d) Criminal: Total Payments (log)

*Notes:* All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log.Panel (a) includes rides from 2011–2017, panel (b) includes 2012–2017, and panels (c) and (d) include rides from 2003–2017. An observation is a district-month. Estimates of $\theta^{bal}_{es}(e; 23)$ for $e \in [-24, 23]$ using methods from Callaway and Sant'Anna (2020). The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are obtained using Callaway Sant'anna's bootstrap-based procedure. Error bars represents the 95% confidence interval.

## C.2 Stacked Regression

The next method for estimating Equation (1) is to explicitly pair treatment and control observations and create a stacked dataset, as outlined by Cengiz et al. (2019). To implement this method, we first create separate datasets for each wave of treatment $g$ consisting of units first treated at time $g$ and all never-treated units. Each of these datasets is appended (or "stacked") such that each treated unit appears once and each never-treated unit appears multiple times (although with different time values). We then estimate

$$Y_{dt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \sum_{e=0}^{L} \beta_e T_{dt}(e) + \alpha_{dg} + \alpha_{dg} + \Gamma X_{dt} + \varepsilon_{dt}, \tag{8}$$

where $\alpha_{dg}$ and $\alpha_{tg}$ are district-by-group and time-by-group fixed effects. These fixed effects account for the fact that control observations may appear more than once in this stacked dataset.

Again, we aggregate the post-period estimates into a single parameter by estimating

$$Y_{dt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \beta \max\{T_{dt}(0), \ldots, T_{dt}(L)\} + \alpha_{dg} + \alpha_{tg} + \Gamma X_{dt} + \varepsilon_{dt} \tag{9}$$

on the stacked data.

Table C4: Effect of Prior Auth. on Ambulance Rides and Spending, Stacked Regression

| | (1) Total Ride Payments | (2) Total Ride Payments (Log) | (3) Total Rides | (4) Total Rides (Log) | (5) Active Firms | (6) Active Firms (Log) |
|---|---|---|---|---|---|---|
| Prior Auth. | -701705.1$^{+}$ | -1.114$^{**}$ | -3534.5$^{+}$ | -0.900$^{***}$ | -11.81$^{*}$ | -0.308$^{***}$ |
| | (387047.3) | (0.344) | (1947.5) | (0.172) | (5.140) | (0.0699) |
| Month-Year FE | 1 | 1 | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 | 1 | 1 |
| Dep. Var. Mean | 400622.8 | 9.875 | 1981.0 | 5.326 | 11.63 | 1.760 |
| Observations | 8304 | 8304 | 8304 | 8304 | 8304 | 8304 |

*Notes:* Estimates of $\beta$ from equation (9). All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variable in columns (2), (4), and (6) are transformed by adding 1 and taking the natural log. These data include rides from 2011–2017. An observation is a district-month. Standard errors are clustered at the district level. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

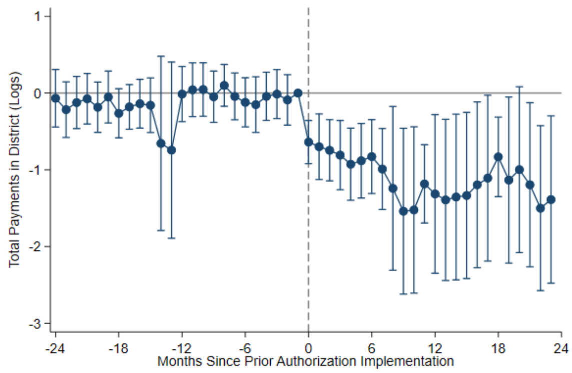
Table C5: Effect of Litigation on Ambulance Spending and Rides, Stacked Regression

| | Civil | | Criminal | |
|---|---|---|---|---|
| | (1) Total Ride Payments (Log) | (2) Total Rides (Log) | (3) Total Ride Payments (Log) | (4) Total Rides (Log) |
| Enforcement | 0.0398 | 0.0784$^{+}$ | -0.157 | -0.185$^{*}$ |
| | (0.0931) | (0.0444) | (0.109) | (0.0907) |
| Month-Year FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Dep. Var. Mean | 9.742 | 5.194 | 9.747 | 5.211 |
| Observations | 32256 | 32256 | 40320 | 40320 |

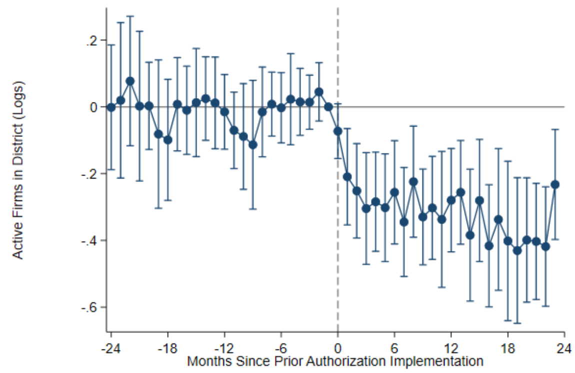
*Notes:* Estimates of $\beta$ from equation (9). All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district-month. The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are clustered at the district level. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Figure C3 presents estimates of Equation (8). We again find that this estimation method results in very similar estimates as those given in Figures 4, 5, and 6.

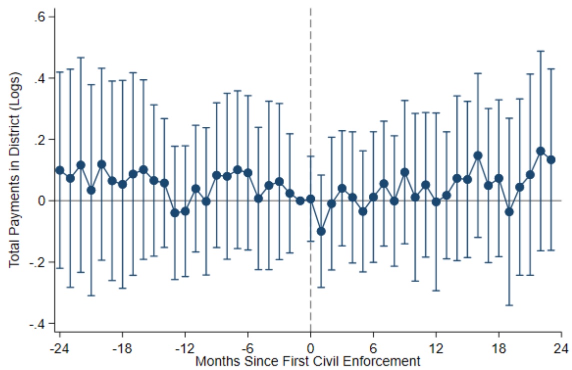
Figure C3: Dynamic Effects of Enforcement, Stacked Regression



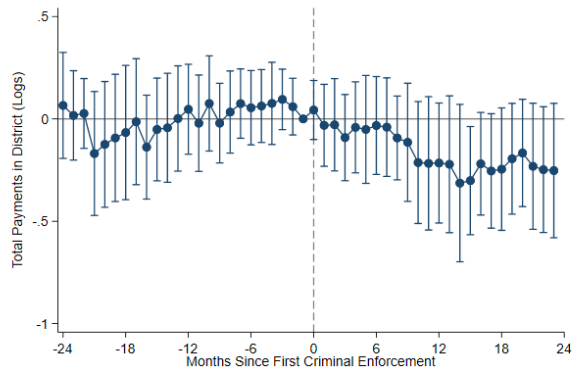

(a) Prior Auth: Total Payments (log)




(b) Prior Auth: Active Firms (log)




(c) Civil: Total Payments (log)




(d) Criminal: Total Payments (log)

*Notes:* All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district-month. Estimates of $\beta_e$ for $e \in [-24, 23]/\{-1\}$ from Equation (8). The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are clustered at the district-group level. Error bars represents the 95% confidence interval.

## C.3 Imputation Estimator

The final estimator we consider is the imputation estimator introduced by Borusyak et al. (2021). To implement this estimator, we first estimate

$$Y_{dt} = \alpha_d + \alpha_t + \Gamma X_{dt} + \varepsilon_{dt}$$

using the untreated observations, including all observations for never-treated districts and pre-treatment observations for treated districts. Then, we predict counterfactual outcomes for the treated observations using the estimates from the previous equation,

$$\hat{Y}_{dt} = \hat{\alpha}_d + \hat{\alpha}_t + \hat{\Gamma} X_{dt}.$$

The difference between this and the realized outcome represents the observation-specific treatment effect (plus error), such that we can take a weighted average of these differences ($\hat{\tau}_{dt} = Y_{dt} - \hat{Y}_{dt}$) to obtain the ATT. Conveniently, this model can be estimated using the `did_imputation` command in STATA.

As with the other estimators, we aggregate these treatment effects dynamically such that $\tau(e) = \frac{1}{D}\sum_{d=1}^{D}\hat{\tau}_{dt}$ for all $D$ treated districts where $t = g + e$ ($t$ is $e$ months from treatment date $g$). We estimate these parameters for $e \in [-24, 23]$. To make these estimates more analogous to those reported by other estimators, we report values for $\Delta\tau(e) = \tau(e) - \tau(-1)$, so that the estimated treatment effect is relative to the month before treatment.

Table C6: Effect of Prior Auth. on Ambulance Rides and Spending, Imputation Estimator

| | (1) Total Ride Payments | (2) Total Ride Payments (Log) | (3) Total Rides | (4) Total Rides (Log) | (5) Active Firms | (6) Active Firms (Log) |
|---|---|---|---|---|---|---|
| Prior Auth. | -719884.6$^{*}$ | -1.412$^{**}$ | -3728.4$^{*}$ | -1.039$^{***}$ | -13.16$^{*}$ | -0.245$^{***}$ |
| | (357118.2) | (0.543) | (1825.9) | (0.222) | (5.243) | (0.0664) |
| Month-Year FE | 1 | 1 | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 | 1 | 1 |
| Observations | 7831 | 7831 | 7831 | 7831 | 6703 | 6703 |

*Notes:* Estimates of $\Delta\tau(23)$. All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variable in columns (2), (4), and (6) are transformed by adding 1 and taking the natural log. These data include rides from 2011–2017 for columns (1)–(4) and 2012–2017 for columns (5) and (6). An observation is a district-month. Standard errors are clustered at the district level. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

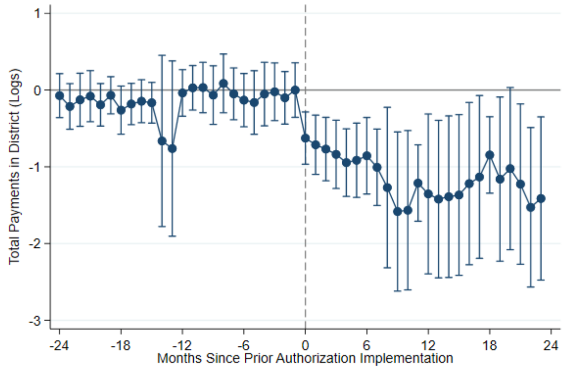
Table C7: Effect of Litigation on Ambulance Spending and Rides, Imputation Estimator

| | Civil | | Criminal | |
|---|---|---|---|---|
| | (1) Total Ride Payments (Log) | (2) Total Rides (Log) | (3) Total Ride Payments (Log) | (4) Total Rides (Log) |
| Enforcement | -0.0121 | 0.0281 | -0.462 | -0.436 |
| | (0.304) | (0.280) | (0.739) | (0.378) |
| Month-Year FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Observations | 16157 | 16157 | 16160 | 16160 |

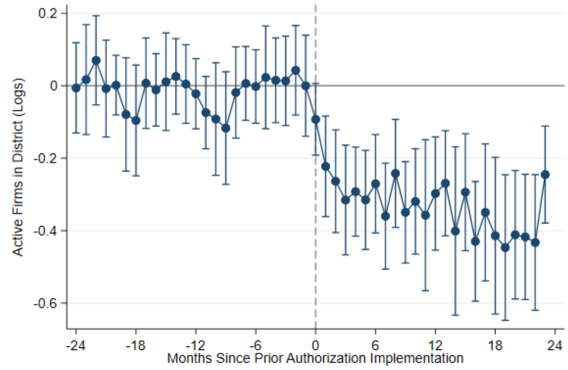
*Notes:* Estimates of $\Delta\tau(23)$. All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district-month. The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are clustered at the district level. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Figure C4 presents estimates of $\Delta\tau(e)$ for $e \in [-24, 23]$. We again find that this estimation method results in very similar estimates as those given in Figures 4, 5, and 6.

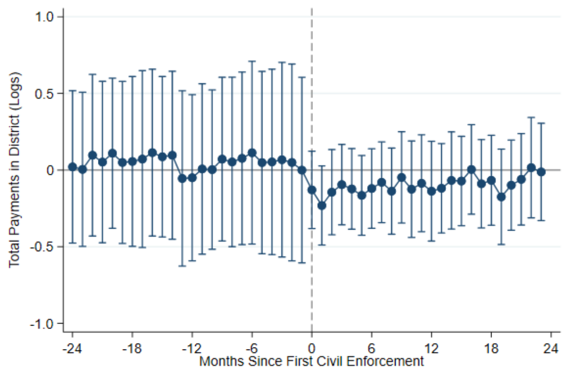
Figure C4: Dynamic Effects of Enforcement, Imputation Estimator



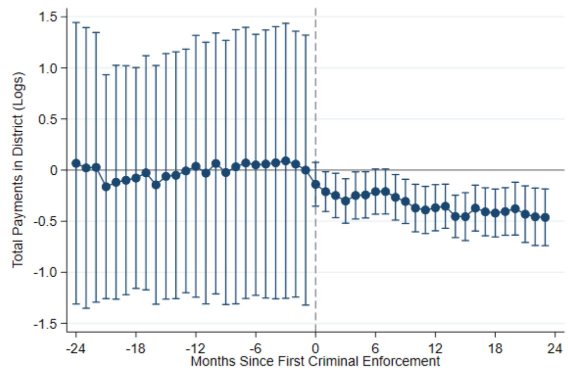

(a) Prior Auth: Total Payments (log)




(b) Prior Auth: Active Firms (log)




(c) Civil: Total Payments (log)




(d) Criminal: Total Payments (log)

*Notes:* All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. Panel (a) includes rides from 2011–2017, panel (b) includes 2012–2017, and panels (c) and (d) include rides from 2003–2017. An observation is a district-month. Estimates of $\tau(e)$ for $e \in [-24, 23]$ using the imputation estimator with $\tau(-1)$ normalized to zero. The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are clustered at the district level. Error bars represents the 95% confidence interval.

# D More Results on the Effects of Prior Authorization

In this appendix, we present additional results on the effects of prior authorization that we refer to throughout the paper. First, we show in Figure D5 that our estimate of the large effect of prior authorization on rides is robust at the firm-month and patient-month level using traditional TWFE methods. As a placebo test, we also show in Table D8 that prior authorization had no impact on the number of emergent rides.

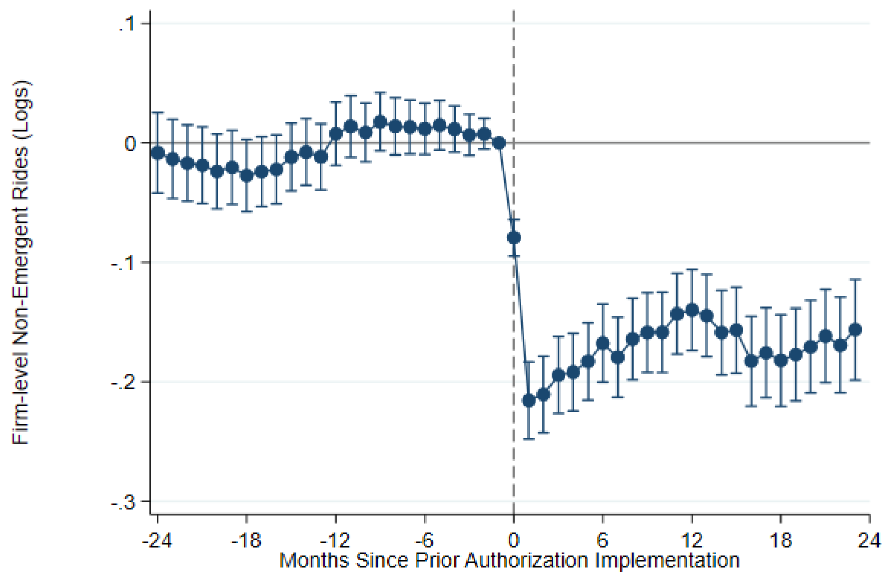
Table D8: Effect of Prior Auth. on Emergency Ambulance Spending

| | (1) Payments for Emergent Rides | (2) Payments for Emergent Rides (Log) | (3) Emergent Rides | (4) Emergent Rides (Log) |
|---|---|---|---|---|
| Prior Auth. | 5212.3 | -0.0129 | 13.54 | 0.000136 |
| | (3700.8) | (0.0445) | (9.451) | (0.0238) |
| Year-Month FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Dep. Var. Mean | 122082.4 | 11.15 | 331.4 | 5.310 |
| Observations | 7356 | 7356 | 7356 | 7356 |

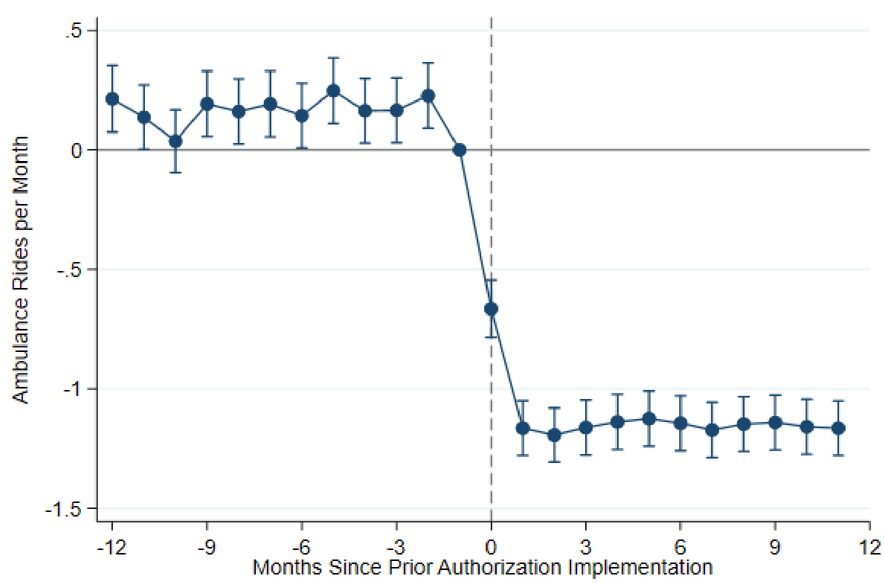
*Notes:* Dependent variable is the natural logarithm of the total payments for emergency ambulance transports in the district-month. These data include rides from 2011–2017. An observation is a district-month. Columns (1) and (2) give the estimate of $\theta_{es}^{O,bal}(23)$ using CS methods for civil enforcement, while columns (3) and (4) do the same for criminal enforcement. The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are obtained using CS's bootstrap-based procedure. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.

Figure D5: Effect of Prior Auth. on Ridership

(a) Firm-Level Effect on Non-Emergent Dialysis Rides (Log)




(b) Patient-Level Effect on Non-Emergent Dialysis Rides




*Notes:* All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Error bars represents the 95% confidence interval. Panel (a) gives estimates of $\beta_e$ for $e \in [-24, 23]/\{-1\}$ from equation (1), includes rides from 2012–2017, and an observation is a firm-state-month. The dependent variable is the number of rides given by the firm in that month transformed by adding 1 and taking the natural log. Standard errors are clustered at the firm-state level. Panel (b) gives estimates of $\beta_e$ for $e \in [-12, 11]/\{-1\}$ from equation (3), includes data from 2011–2017, and an observation is a patient-month. The dependent variable is the number of rides taken by the patient in the month. Standard errors are clustered at the dialysis facility level.

# E More Results on the Effects of Litigation

We present evidence that the negative treatment effect of criminal and civil enforcement is highly localized. To do this, we assign a district's treatment date to all bordering districts and remove the actually treated district from the sample. In this way, we compare district's bordering those subject to enforcement with those neither bordering districts subject to enforcement nor subject to enforcement themselves. Table E9 indicates that there is no detectible impact of civil or criminal enforcement on the total number of rides or payments in neighboring districts.

Table E9: Spillovers of Litigation on Ambulance Spending and Ridership

| | Civil | | Criminal | |
|---|---|---|---|---|
| | (1) Total Ride Payments (Log) | (2) Total Rides (Log) | (3) Total Ride Payments (Log) | (4) Total Rides (Log) |
| Neighboring Enforcement | 0.0321 | 0.00728 | -0.193 | -0.0722 |
| | (0.0791) | (0.0488) | (0.253) | (0.115) |
| Month-Year FE | 1 | 1 | 1 | 1 |
| District FE | 1 | 1 | 1 | 1 |
| Dep. Var. Mean | 12.02 | 6.761 | 11.86 | 6.727 |
| Observations | 1248 | 1248 | 1584 | 1584 |

*Notes:* Estimates of $\beta$ from equation (2). All rides are non-emergent basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district-month. The sample is limited to districts that are not subject to the relevant enforcement type. The treatment date is the earliest enforcement action of the relevant type in any district that geographically borders the district in question. Standard errors are clustered at the district level. $^{+}$, $^{*}$, $^{**}$ and $^{***}$ indicate significance at the 10%, 5%, 1% and 0.1% level, respectively.