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

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We study the effectiveness of pay-and-chase lawsuits and upfront regulations for combating health care fraud. Between 2003 and 2017, Medicare spent $7.7 billion on 37.5 million regularly scheduled ambulance rides for patients traveling to and from dialysis facilities even though many did not satisfy Medicare's criteria for receiving reimbursements. Using an identification strategy based on the staggered timing of regulations and lawsuits across the US, we find that adding a prior authorization requirement for ambulance reimbursements reduced spending much more than pursuing criminal and civil litigation did on their own. We find no evidence that prior authorization affected patients' health.

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1 Introduction

Fraud poses a serious problem for Medicare: it both distorts patient care and wastes limited public resources. In 2019, improper payments that did not meet Medicare's statutory, regulatory, administrative, or other legally applicable requirements totaled $31.2 billion, or 7.4% of overall spending (Centers for Medicare and Medicaid Services, 2023). To combat and deter this fraud, the federal government uses two main approaches: litigation through the courts, which attempts to recover funds that have already been paid out, and administrative regulations such as prior authorization that prevent improper payments from being made in the first place. Although in theory both approaches can be used effectively, the costly and expansive monitoring required to implement wide-reaching regulations has prompted a long literature in law and economics favoring the use of targeted litigation instead (Coase, 1960; Becker, 1968). The enforcement of most US health care policies reflects this view (Office of Inspector General, 2021), yet no large-scale empirical studies have compared the effectiveness of commonly used pay-and-chase litigation to the preemptive regulations used increasingly throughout health care.

In this paper, we study the unnecessary use of ambulances to transport patients between their homes and dialysis facilities to provide the first systematic empirical evidence that administrative regulations can reduce health care fraud more effectively than relying solely on ex post litigation. Although Medicare reimburses ambulance transportation costs for those with a demonstrated medical need for assistance, unscrupulous companies have exploited a historically lax enforcement of the rules to provide fraudulent rides to ineligible patients, essentially 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.

While the billions of dollars at stake make a study of fraudulent ambulance rides worthwhile on its own, this particular form of fraud represents a larger class of illicit activity in which providers violate Medicare's reimbursement policies by seeking payments for treatments and services without first establishing a medical need for them. A lack of medical necessity has been a key factor in cases as varied as inpatient hospitalizations, physician-administered drugs, nursing homes, and durable medical equipment, amounting to a sizable and preventable waste of Medicare's scarce resources.

The US government uses an array of policies and mechanisms to prevent health care fraud. One prominent approach, commonly referred to as pay-and-chase litigation, pursues criminal and civil enforcement through the court system, with criminal convictions resulting in jail time and civil judgments imposing heavy penalties on those found guilty of fraud. In contrast to the large expenses incurred by the FBI and DOJ to investigate and litigate fraud after the fact, the Centers for Medicare and Medicaid Services (CMS) can impose ex ante administrative regulations that restrict reimbursements from being paid out in the first place, such as prior authorization that requires a provider submit proof of a patient's legitimate medical needs before rendering a service and receiving payment for it.

For our empirical analysis, we use a novel data set of all criminal and civil lawsuits filed against providers accused of ambulance fraud in Medicare's dialysis program over the past two decades combined with Medicare claims data and the staggered rollout of prior authorization across the country to identify the effects of both litigation and regulation on the use of non-emergency ambulance rides, the firms that provide them, patients' access to care, and their resulting health outcomes. We find that adding prior authorization was much more effective at reducing wasteful spending than exclusively pursuing lawsuits against fraudulent providers. Adopting prior authorization caused an immediate and persistent drop in non-emergency ambulance rides of 68%, a substantially larger effect than either criminal or civil litigation had on their own. When weighed against the associated costs of prior authorization and litigation, our results suggest that this type of regulation is an efficient way to reduce unnecessary Medicare expenses.

In addition to causing a large drop in the number of non-emergency ambulance rides to dialysis facilities, prior authorization also led to substantial changes in the market for ambulance services. We find that the number of ambulance companies fell sharply in the markets subject to prior authorization and those that remained became more specialized in providing only non-emergency dialysis rides, underscoring important mechanisms through which preemptive regulations can reduce fraud. In line with its limited impact on ridership, however, we find that litigation had a limited effect on the firms not directly prosecuted in the market.

To determine whether the decline in ridership constitutes a reduction in wasteful spending rather than a cut to essential services, we also consider the extent to which prior authorization may have 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 developing serious complications and diminishing their quality of life. Despite this possibility, we find no evidence that the regulatory change disrupted patients' care or led to worse health outcomes, suggesting that prior authorization resulted in a better use of Medicare's resources. We estimate that the federal government would have saved $4.8 billion had it started requiring prior authorization in 2003, when our data begin, rather than waiting until 2014 to pilot the program, and would have done so without any negative health consequences for patients.

We conclude our paper by connecting our empirical results to prominent theories of enforcement and regulation to explain why prior authorization was effective at reducing ambulance fraud while litigation alone was not. A large empirical literature has found that various types of enforcement can effectively reduce criminal behavior, and can do so even without an amendment to the underlying law or deliberate change in the probability of enforcement (e.g., Boning et al., 2020). Because the assignment of ex post liability through litigation can deter fraud without incurring upfront monitoring or enforcement costs, the most basic model comparing regulation to litigation would show that regulation is inefficient (Becker, 1968; Stigler, 1970; Shavell, 1984). For ambulance fraud, however, we identify two primary factors undermining the effectiveness of litigation: the limited liability of those committing fraud and their low probability of being detected for it. By directly curtailing the potential gains from providing illegitimate rides, prior authorization does not face the same limitations as litigation and therefore effectively preempts the fraudulent behavior.

We incorporate these insights into a stylized model that connects our findings to past research on limited liability and the likelihood of enforcement, such as Shavell (1984) and Polinsky and Shavell (2000), and extend this literature to a setting where the crime is financial fraud against the government perpetrated by a host of unscrupulous providers. Our work also relates to Glaeser and Shleifer (2003) and Behrer et al. (2021), who 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). Connecting these distinct literatures, we believe ours is the first large-scale empirical analysis to study the relative effectiveness of combating fraud by devoting more resources to enforce existing laws or implementing new regulations.[1]

Our findings also add to the literature on fraud and overbilling in Medicare. The seminal works of Silverman and Skinner (2004) and Dafny (2005) lay out 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. Others have developed ways to detect suspicious behavior in claims data, such as Fang and Gong (2017), who estimate the time intensity of outpatient procedures to identify providers who bill for an unrealistically large number of hours.[2] Also related is the work of Sanghavi et al. (2021), who 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 rides nationwide. In addition, O'Malley et al. (2021) find that home health care fraud diffuses faster in cities where firms have more patients in common, while Leder-Luis (2023) studies the economics of civil anti-fraud health care litigation conducted against large institutional providers. These studies have largely focused on the incentives to commit fraud and the ways to detect it, which we extend by considering the mechanisms available to combat this type of illicit behavior and the consequences for patients' health.

Our finding that prior authorization reduced spending without harming patient care relates

[1] There are several case studies of regulation and litigation in other domains that provide suggestive evidence in favor of one over the other. See, for example, Harrington et al. (2014) or the studies in Kessler (2011). Our paper advances this literature by using modern econometric techniques to identify and quantify the causal impact of each approach in a single, large-scale empirical setting.

[2] Also of note is the related discussion in Matsumoto (2020) and Fang and Gong (2020).

to the recent debate surrounding administrative burdens in health care (Sahni et al., 2021; Brot-Goldberg et al., 2022). In contrast to past work showing that these frictions can limit enrollment (Shepard and Wagner, 2021), prompt physicians to stop accepting patients (Dunn et al., 2023), and impose large billing costs on providers without reducing expenditures (League, 2023), the modest cost of requiring an ambulance company to obtain prior approval from a physician before transporting a patient to dialysis seems well justified given its success at reducing unnecessary rides and the billions of dollars previously spent on them. In contemporaneous work, a federally funded evaluation study by Contreary et al. (2022) corroborates our finding that prior authorization reduces Medicare expenditures on non-emergency ambulance rides, though they do not study the corresponding role of litigation, do not consider whether the regulation effectively screens patients who should not be riding in ambulances, and do not investigate its effect on the market for ambulance companies.[3]

Our results suggest that prepayment regulations can be used to curb waste in other federal spending programs where pay-and-chase is the norm, such as the recent wave of fraud in Covid-19 relief aid and the unsuccessful attempts to recover stolen funds (Ackerman and Omeokwe, 2022). To this point, the inspector general of the Small Business Administration (SBA) reported that "SBA's lack of adequate front-end controls to determine eligibility contributed to the distribution" of fraudulent loans, making a case for regulations like the type of prior authorization that we study in this paper (SBA Inspector General, 2021). Similar examples of public expenditure fraud abound. The GAO, for instance, estimated that as much as $1.4 billion of Hurricane Katrina relief funds went to improper or fraudulent payments, citing inadequate claim verification as a primary reason (United States Government Accountability Office, 2006). Fraud, waste, and abuse in the Iraq reconstruction efforts were estimated at more than $8 billion, with litigation yielding less than $200 million in reclaimed funds (Bowen, 2013).

Finally, our paper contributes to a specific literature that has scrutinized the dialysis industry for a host of improper practices. 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 detrimental to patients' health. The approach in Fang and Gong (2017) that uses the number of hours worked by a physician to detect overbilling also shows that nephrology is one of the highest categories with claims 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 discusses the institutional details of dialysis and anti-fraud enforcement. Section 3 describes the data and highlights notable descriptive statistics. Section 4 outlines our empirical framework. Section 5 presents our empirical results, including

[3]Contreary et al. (2022) was submitted in February 2022 and cites our November 2021 NBER working paper version of this article.

the effects of prior authorization and litigation on non-emergency ambulance rides, the firms that provide them, patients' health outcomes, and the subsequent characteristics of riders. Section 6 develops a stylized model to orient our empirical findings within the theoretical literature studying the effectiveness of regulation and litigation. Section 7 concludes with our arguments for why regulatory actions are a cost-effective way to prevent health care fraud.

2 Background

Medicare's End-Stage Renal Disease (ESRD) program covers patients needing dialysis, a procedure that cleans the blood of those without well-functioning kidneys. Dialysis patients typically visit one of the nation's more than 7,000 dialysis facilities three times per week to receive treatments that last three to four hours per session. Many patients arrange transportation to dialysis on their own, either in a personal vehicle or on public transportation, but those with severe medical conditions require an ambulance. Medicare pays for transportation to and from dialysis only when an ambulance is medically necessary, meaning that the patient has no other safe way to travel due to their medical condition.[4]

Ambulance companies must satisfy several 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, and that the vehicles must be specifically designed as ambulances.[5] In addition, providers 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.[6]

Medicare pays for ambulance rides through Part B, making patients responsible for a 20% copayment on top of their annual deductible. The payment rates 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. For non-emergency ground transportation, the focus of our paper,

[4]The Medicare Benefit Policy Manual specifies that "in any case in which some means of transportation other than an ambulance could be used without endangering the individual's health, whether or not such other transportation is actually available, no payment may be made for ambulance services." Submitting claims for care that fails to meet the medical necessity standard constitutes health care fraud.

[5]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.

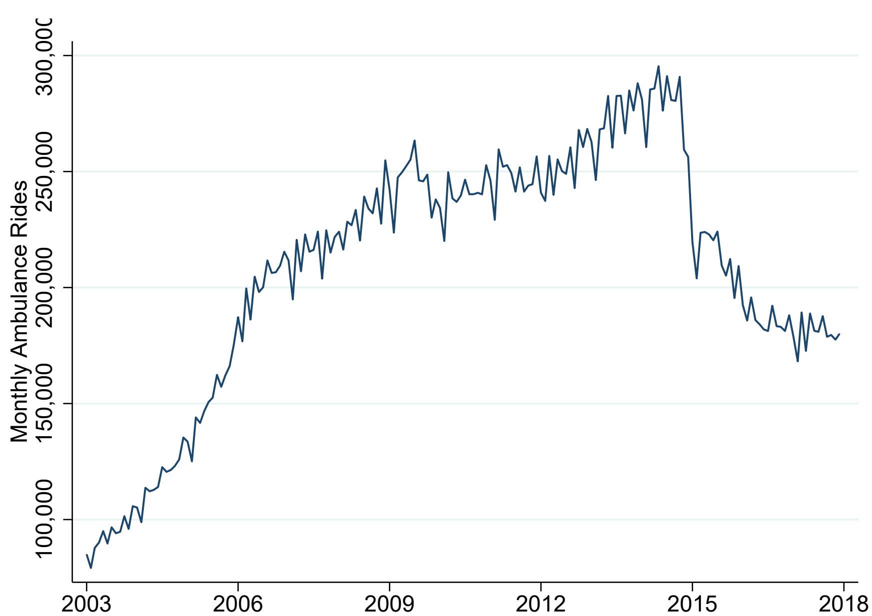
[6]The Code of Federal Regulations, Title 42, Chapter IV, Part 410.40, stipulates, "Nonemergency transportation by ambulance is appropriate if either: the beneficiary is bed-confined, and it is documented that the beneficiary's condition is such that other methods of transportation are contraindicated; or if his or her medical condition, regardless of bed confinement, is such that transportation by ambulance is medically required…For a beneficiary to be considered bed-confined, the following criteria must be met: (i) The beneficiary is unable to get up from bed without assistance. (ii) The beneficiary is unable to ambulate. (iii) The beneficiary is unable to sit in a chair or wheelchair."

the current base and mileage rates are \$272.44 and \$8.76, respectively, up from \$209.65 and \$6.74 in 2010, with rates adjusted by location.

Fraud has become a major concern for all of Medicare's ambulance reimbursements, not just among dialysis patients. The Department of Health and Human Services Office of Inspector General has published several reports about Medicare's ambulance benefit, including a 2006 study, "Medicare Payment for Ambulance Transport," which found that 20% of non-emergency transports were improper in that they did not meet Medicare's coverage requirements.

The issue is particularly acute in dialysis, however, where for many years ambulance companies transported patients who did not meet Medicare's criteria for receiving medically necessary rides. The large reimbursements paid by Medicare, coupled with patients' regularly scheduled and recurring visits to facilities, create a strong financial incentive for unscrupulous providers to engage in fraud, especially if they transport non-emergency patients who do not require costly medical attention during the ride. From 2007 to 2011, the volume of transports to and from dialysis facilities increased by more than twice the rate of all other ambulance transports. In 2011, ambulance rides 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 growth, Figure 1 shows the number of rides in our data more than tripling from 2003 to 2014, a period when the number of ESRD patients increased by only 54%.

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



Notes: The sample includes non-emergency 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-7b(b)), with the crimes investigated by the FBI and prosecuted by DOJ district offices nationwide. The US compounds its enforcement with laws against conspiracy, racketeering, organized crime, and lying to investigators. Employing this pay-and-chase approach, over the past twenty years the DOJ has pursued 43 criminal lawsuits against ambulance company operators for providing fraudulent rides to dialysis patients, alleging illegal behavior like paying kickbacks to patients to induce them to ride, giving 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 up to triple damages on firms that overbill federal health care programs. The False Claims Act contains a qui tam whistleblower provision where individuals with knowledge and evidence of fraud can file their own lawsuits against those who submit fraudulent claims on behalf of the US government in exchange for 15–30% of the recovered funds, while the DOJ can also initiate civil lawsuits on their own. We identify 26 civil lawsuits, from as early as 1996, alleging the unnecessary transport of dialysis patients by ambulance companies.

Medicare administrators also attempt to stop overbilling and fraud by enacting new regulations. In the case of medically unnecessary ambulance rides, Medicare began implementing prior authorization for ambulance claims in 2014, stipulating that providers will only receive payment for repetitive, non-emergency rides to dialysis facilities if they have already submitted documentation of a patient's medical necessity, rather than allowing providers to submit claims for payment first and then responding to any subsequent requests to verify a patient's eligibility. Medicare began rolling out the new requirement in 2014 in three states that had particularly high rates of non-emergency ambulance claims — New Jersey, South Carolina, and Pennsylvania — and then extended it in 2016 to the nearby states Delaware, DC, Maryland, North Carolina, Virginia, and West Virginia. Plans to expand prior authorization nationwide were postponed in 2020 due to Covid-19 but eventually completed in August 2022, with policymakers still debating the merits of the regulation (Lotven, 2022).

3 Data & Descriptive Statistics

We use the 100% sample of claims data compiled by the United States Renal Data System (USRDS) for the entire universe of patients diagnosed with ESRD and enrolled in Medicare between 2003 and 2017.[7] 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 the facility-level data have information on location and ownership. Our data also allow us to observe each ambulance ride to and from a dialysis facility billed to Medicare, which amounts to more than 37.5 million non-emergency rides and over $7.7 billion in spending. For firms that provide non-emergency ambulance rides, we have additional data on their other claims for Medicare ESRD beneficiaries, such as emergency hospital transports. In the last six years of our data alone, we observe 3,081 firms providing non-emergency rides to dialysis patients. Because the USRDS data only began recording firm identifiers in 2012, we supplement these data with a 20% sample of claims for all Medicare beneficiaries between 2007 to 2019.[8]

Table 1 provides summary statistics for patient characteristics, ridership, and health outcomes for those who receive any non-emergency ride to a dialysis facility, split across months with and without rides, as well as summary statistics for dialysis patients who never receive such a ride. 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 10 round-trip rides each month, on average, amounting to 20 claims total, with a lifetime average of 660 claims. Because dialysis patients receive approximately 12 treatments per month, these averages imply that patients who take an ambulance to and from their facility do so for nearly 9 out of 10 sessions.

We supplement these data with information on criminal and civil enforcement against fraud. Using publicly available press releases from the DOJ, corroborated for completeness by internet searches, we identify 69 lawsuits across 26 federal judicial districts against dozens of ambulance companies and individuals for unnecessary ambulance rides related to dialysis. For each of these lawsuits, we collect court records from the Public Access to Court Electronic Records (PACER) system, which include specific fraud allegations and data on the lawsuit's timing and location of enforcement.[9]

[7]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 (United States Renal Data System, 2020).

[8]Because the USRDS data are a 100% sample of claims, we use this as our primary data source, relying on the 20% sample only when assessing firm-level outcomes related to criminal and civil litigation, which often occurred before 2012. Unless otherwise noted, all calculations, tables, and figures rely on the USRDS data.

[9]We use the court filing or complaint date as the treatment date. Civil lawsuits are often filed under seal, meaning it is unlikely that the lawsuit's existence was known prior to the filing date. Criminal lawsuits may involve investigations before the lawsuit is filed and there is some chance that firms become aware of them prior to the filing date. However, in both cases this date likely represents the best possible case for this analysis. It is also the standard date used in the literature on health care fraud and beyond (Agan et al., 2021, 2023; Leder-Luis, 2023; Gruber et al., 2023). Furthermore, our event study design allows us to demonstrate the robustness of this decision.

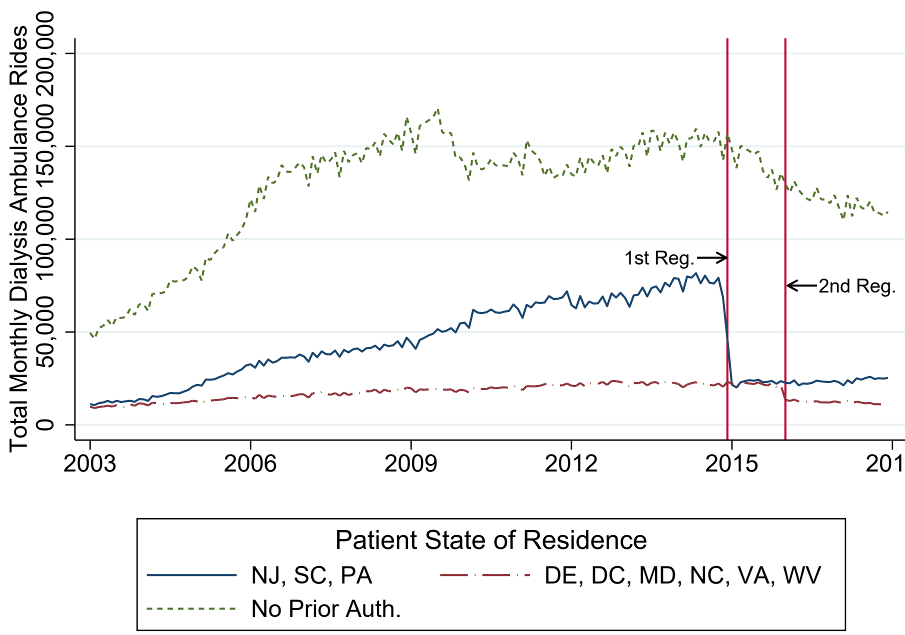
Table 1: Summary Statistics of Patient-Month Data

	Patient Rider Status			
	Never-Rider	Rider, Non-Riding Month	Rider, Riding Month	Overall
Patient Characteristics				
Age (Years)	61.97	67.20	69.27	62.99
Months with ESRD	55.99	60.38	54.05	56.49
Black	0.377	0.418	0.451	0.386
Male	0.561	0.496	0.457	0.548
Diabetic	0.524	0.620	0.661	0.543
Drug User	0.014	0.011	0.008	0.013
Smoker	0.065	0.055	0.045	0.063
Drinker	0.013	0.013	0.011	0.013
Uninsured at Incidence	0.129	0.089	0.061	0.120
Employed at Incidence	0.181	0.099	0.066	0.165
Ridership				
Non-Emergency Dialysis Rides	0.00	0.00	19.54	0.87
Emergency Rides	0.100	0.179	0.408	0.125
Total Lifetime Rides	0.0	132.8	660.3	47.1
Continuing to Ride Next Month	.	.	0.838	0.838
Facility Characteristics				
Facility Age (Years)	16.94	16.48	16.30	16.85
Freestanding Facility	0.956	0.966	0.972	0.958
Chain Affiliation				
DaVita	0.345	0.333	0.325	0.343
Fresenius	0.364	0.372	0.377	0.366
Other Chain	0.135	0.148	0.153	0.137
Independent	0.156	0.146	0.146	0.154
Health Outcomes				
Dialysis Sessions	12.18	12.05	11.29	12.13
All-Cause Hosp.	0.110	0.152	0.250	0.122
Fluid Hosp.	0.011	0.016	0.020	0.012
Mortality	0.009	0.007	0.034	0.010
Patient-Months	15,611,284	2,533,118	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 emergency rides are based on non-emergency 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 the focal month. Fluid hospitalizations are those for which the primary diagnosis indicates excess fluids, an indication of insufficient dialysis.

As discussed in Section 2, Medicare's regulation requiring prior authorization stipulates that ambulance companies must obtain approval for each patient receiving repetitive, non-emergency ambulance transports before they provide the service, with the approval renewed periodically.[10] This policy was piloted on December 15, 2014, in New Jersey, Pennsylvania, and South Carolina, and then expanded on January 1, 2016, to Delaware, DC, Maryland, North Carolina, Virginia, and West Virginia. Figure 2 shows preliminary evidence of the regulation's effectiveness: rides for patients in Pennsylvania, New Jersey, and South Carolina fell sharply after Medicare first imposed prior authorization, with states included in the second wave experiencing a similar decline immediately upon the policy's expansion.

Figure 2: Rides by Prior Authorization Regulation



Notes: For each of the three lines, the vertical axis measures total rides per month in the represented states. Sample includes non-emergency 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 that in DE, DC, MD, NC, VA, and WV.

[10]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 (Centers for Medicare and Medicaid Services, 2020b). Prior authorization is required for the fourth ride in a 30-day period.

4 Empirical Strategy

We use the staggered rollout 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 unnecessary rides and their impact on patients.[11] For our estimates, we present results using both traditional two-way fixed effects (TWFE) methods in the main text and several alternative estimators in Appendix B. For the traditional TWFE results, we estimate

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

for district d in month t, where Y_{dt} is the outcome of interest (e.g., payments or rides, measured in both levels and logs), $T_{dt}(e)$ is an indicator for an observation falling e months from the treatment date with base period $e = -1$, α_d and α_t are district and month fixed effects, and X_{dt} is a matrix of indicators for having already been subject to a different type of enforcement or prior authorization. Because districts are geographic subsets of states, district fixed effects account for state fixed effects.

To avoid the compositional issues that have been noted by, for example, Callaway and Sant'Anna (2021), we set $K = 24$ and $L = 23$, defining $T_{dt}(e)$ only for units 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 $T_{dt}(e) = 0$ for all e. We also use alternative estimators that directly address compositional issues in Appendix B.

To aggregate these results into a single parameter, we estimate

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

This is similar to the more traditional pre-post estimator, but rather than comparing the entire pre-period to the entire post-period, the entire post-period is compared only to the period immediately before treatment (i.e., $e = -1$). 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 potentially could be quite different. By setting $K = 24$ and $L = 23$, we capture the effect of treatment in the two years following treatment.

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

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

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

and

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

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.

For further justification of our research design, we provide a balance table comparing control states to prior authorization states by each wave of the regulation's rollout in Table A8 in Appendix C. Although some small differences exist, the health outcomes are similar in terms of hospitalization and mortality rates as well as the rate of emergency ambulance rides. The second-wave states are also similar to the control states in terms of non-emergency ridership, although the first-wave states did have higher ridership overall. Similarly, Table A9 in the same appendix shows that observable characteristics are balanced across districts subject to prior authorization and litigation, supporting our comparison of the effects of each intervention.

Finally, we perform several robustness checks using alternative difference-in-differences estimators suggested by recent developments in the literature on using TWFE estimators with staggered treatments or heterogeneous treatment effects (Callaway and Sant'Anna, 2021; Cengiz et al., 2019; Borusyak et al., 2017). Because the traditional TWFE approach relies solely on within-group variation in the treatment variable to eliminate possible unobserved confounders related to districts or time trends, staggered treatment timing may result in inappropriate comparisons, such as including already treated districts as controls, and potentially bias our estimates. In light of this concern, we show in Appendix B that none of the alternative difference-in-differences estimators affect our results, while in Appendix D we show our results are robust to using alternative control groups (e.g., using only not-yet-treated districts or only bordering districts as controls).

Our empirical strategy allows us to identify three important, policy-relevant parameters: the average treatment effects of (i) adding prior authorization, (ii) pursuing criminal litigation, and (iii) pursuing civil litigation. Because the possibility of litigating fraudulent ambulance companies always exists throughout our sample period, we cannot consider the impact of imposing a new litigation regime. Instead, our empirical design compares a policymaker's two primary options when current enforcement mechanisms do not deter fraud effectively: pursuing litigation to enforce existing laws or implementing new regulations that make fraud less lucrative.

5 Empirical Results

5.1 Payments and Rides

We first consider the effect of prior authorization on rides and spending. Table 2 provides estimates of the policy's effect on the number of non-emergency ambulance rides between a dialysis facility and a patient's home, as well as Medicare payments for such rides, in all treated districts in the two years following treatment, as represented by β in equation (2). Outcomes are measured both in levels and by adding one and taking the natural log.

We find that prior authorization reduces payments for non-emergency ambulance rides by 1.129 log points, or 67.7%.[12] Figure 3 shows the dynamic difference-in-differences results, or estimates of β_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.[13]

Table 2: Effect of Prior Authorization on Ambulance Rides and Spending

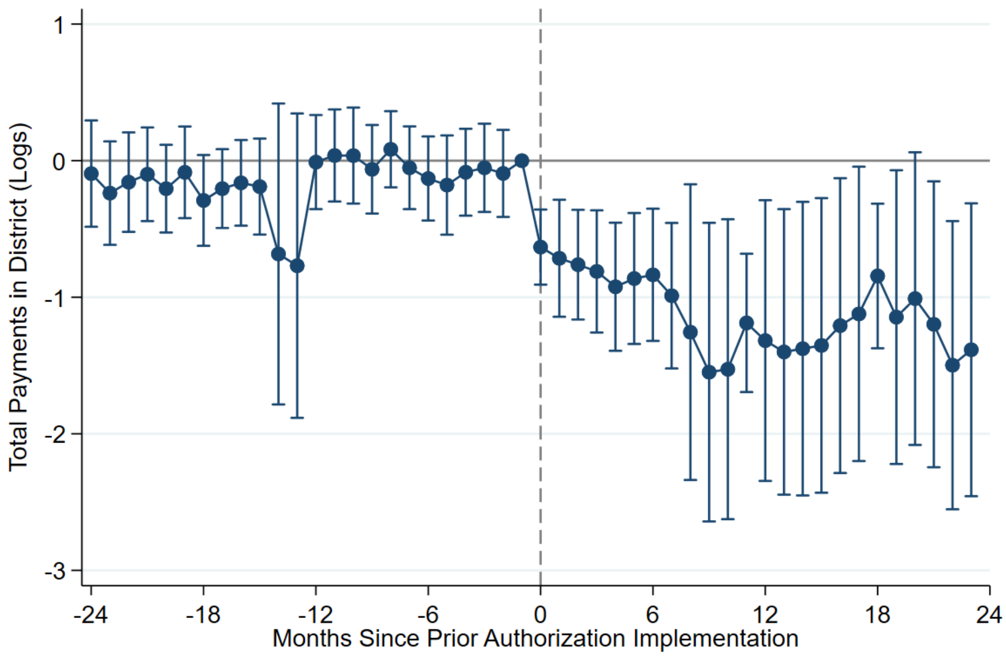
	(1) Total Ride Payments (Log)	(2) Total Ride Payments	(3) Total Rides (Log)	(4) Total Rides
Prior Authorization	-1.129**	-738674.2^{+}	-0.913***	-3714.6^{+}
	(0.350)	(405698.1)	(0.176)	(2039.7)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	9.934	415286.7	5.357	2005.3
Observations	7272	7272	7272	7272

Notes: Estimates of β from equation (2). All rides are non-emergency 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% levels, respectively.

[12]The second wave of the prior authorization rollout occurred two years before the end of our data, meaning that both treatment waves are included in this parameter. To address the possibility that this masks meaningful differences in the effect across the two waves, we also estimate separate treatment effects for each wave and show the results in Appendix E. We find a reduction in payments of 1.21 log points in the first-wave states and 1.07 log points in the second-wave states. The difference between these two estimates is not statistically significant.

[13]In Appendix F, we perform a similar analysis at the firm-month and patient-month levels, finding that the large effect of prior authorization is robust. We also show that our results are robust to using the inverse hyperbolic sine transformation or a Poisson specification. Finally, we also consider a falsification test that shows prior authorization had no impact on the number of emergency rides.

Figure 3: Effect of Prior Authorization on Ambulance Spending



Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is total payments for non-emergency 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 represent pointwise 95% confidence intervals.

In contrast to prior authorization, which requires approval ahead of time, both criminal and civil litigation seek to identify and prosecute illicit behavior after it has already taken place, a pay-and-chase approach aimed at both deterring fraud and punishing those who commit it. Under this system, the threat of litigation is always present, although successful litigation may act as an even stronger deterrent by changing the incentives for those who might commit similar fraudulent acts (Leder-Luis, 2023). For that reason, we estimate the impact of realized litigation in the local district rather than the most general deterrence that would arise from having a law already in place or enacting a new one.

To study the impact of litigation on ambulance fraud, we use the same approach as above for civil and criminal enforcement actions.[14] Table 3 provides estimates of β from equation (2),

[14]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 G, we show that the effects of enforcement are highly localized, with no negative impacts on rides or payments in neighboring districts.

where the treatment date is determined by the start of each type of enforcement in the district.[15]

Table 3: Effect of Litigation on Ambulance Spending and Rides

	Civil		Criminal	
	(1) Total Ride Payments (Log)	(2) Total Rides (Log)	(3) Total Ride Payments (Log)	(4) Total Rides (Log)
Enforcement	-0.0424	0.0257	-0.211^{+}	-0.280**
	(0.110)	(0.0663)	(0.106)	(0.0994)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	9.221	4.835	9.354	4.928
Observations	14160	14160	14436	14436

Notes: Estimates of β from equation (2). All rides are non-emergency 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% levels, respectively.

We find that civil enforcement does not have a statistically significant effect on rides or total payments, whereas criminal enforcement reduces monthly payments by 19% and rides by 24% in the subsequent two years.[16] Figure 4 shows the dynamic effects of the first indictment of each type. Although we find no decrease in payments following civil enforcement, our results suggest that criminal enforcement gradually reduces payments over time.

The relative magnitudes of these enforcement approaches are highlighted in Figure 5, which presents the estimates from Figures 3 and 4 in a single panel to illustrate the stark difference between the effect of prior authorization and the effects of both criminal and civil litigation. Even two years after the enforcement action, our results suggest that criminal enforcement has only 20–25% of the effect of prior authorization and civil enforcement continues to have no impact whatsoever.[17] In Appendix H, we present analogous figures for other outcomes to demonstrate the

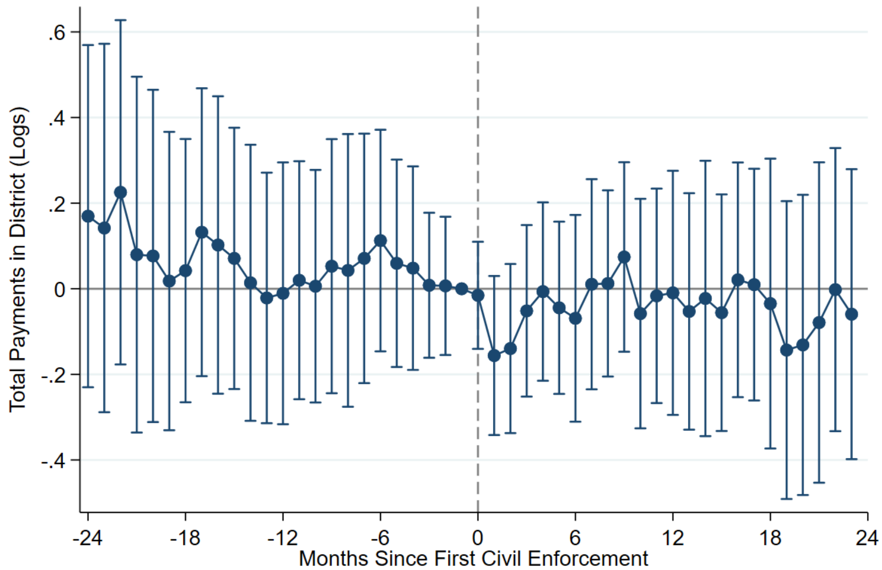
[15] Because Illinois North, Massachusetts, Arkansas East, North Carolina East, and California Central had civil actions before or within the first year of our sample period and the first civil actions in Georgia South and Virginia East occurred too late in our data, we exclude these districts from our analysis of civil enforcement. Similarly, Arkansas East, California Central, and North Carolina East are excluded from our analysis of criminal enforcement because the associated actions occurred too early in our data, while Kentucky East is excluded because its enforcement actions occurred too late.

[16] In Appendix D, we show that these results are robust to alternative functional form assumptions and control groups. In Appendix E, we investigate a number of potential dimensions of heterogeneity in this effect that may indicate endogenous enforcement, including heterogeneity by enforcement date, pre-litigation ridership, and the number of cases pursued in the DOJ district. We find little evidence of such heterogeneity. In Appendix D, we also estimate a single specification that includes both litigation and regulation, finding effects similar to those above.

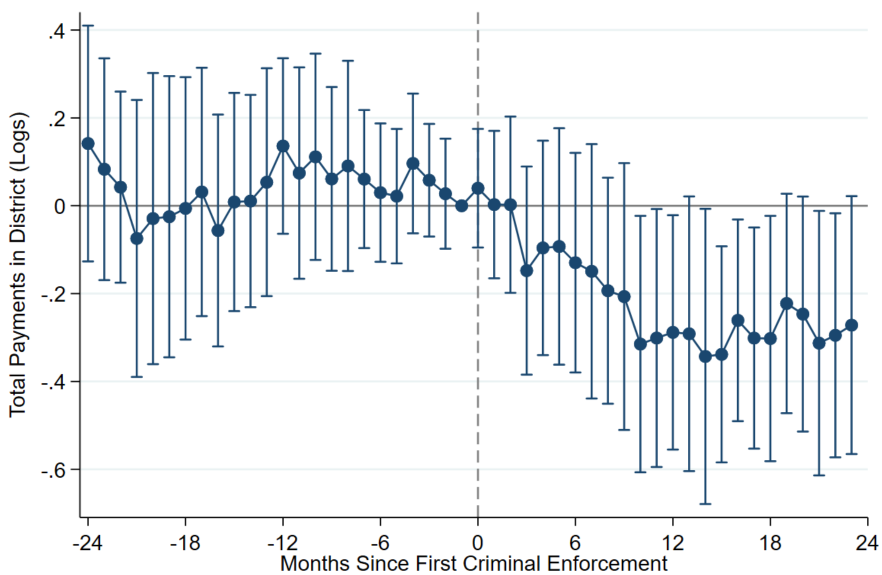
[17] A natural question is whether litigation would have a larger effect if multiple cases were brought in a district. While few districts have multiple cases, we present evidence in Table A20 of Appendix E that there are not large deterrence effects from cases subsequent to the first in a district.

Figure 4: The Impact of Litigation on Ambulance Payments

(a) Civil Enforcement



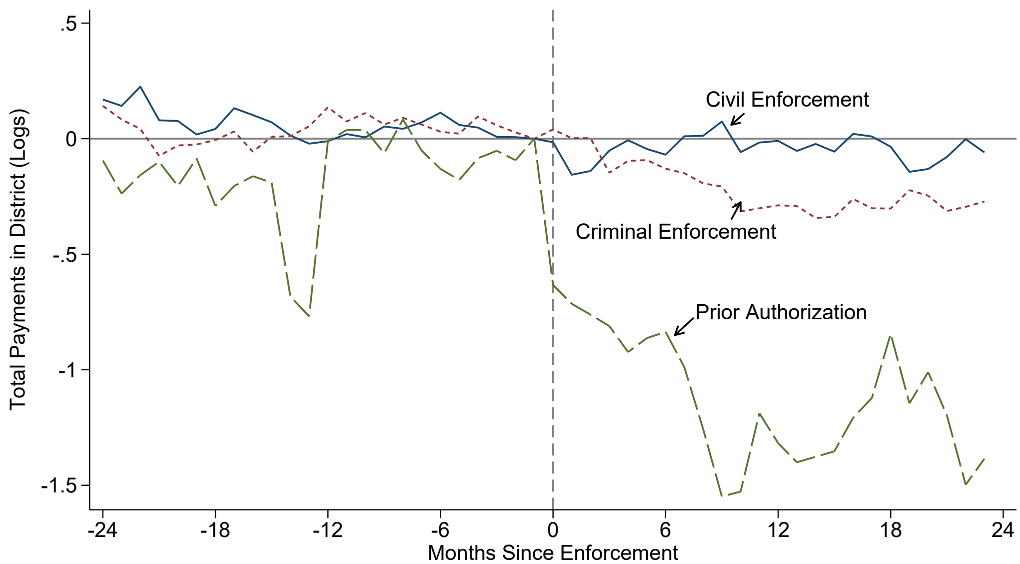
(b) Criminal Enforcement



Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is total payments for non-emergency 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 represent pointwise 95% confidence intervals.

notable differences across each type of intervention. In all cases, the impact of prior authorization is qualitatively much larger than litigation and the differences are statistically significant.

Figure 5: Effect of Prior Authorization and Criminal and Civil Litigation on Ambulance Payments



Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is total payments for non-emergency 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.

5.2 Patient Health

Although prior authorization reduced the number of ambulance rides taken by dialysis patients, the additional administrative burden may have resulted in some patients forgoing treatment if they could not find another safe way to reach their facilities. If these missed sessions resulted in adverse events such as hospitalization or death, Medicare's savings from fewer ambulance reimbursements could have been offset by higher costs in other parts of the ESRD program, to say nothing of the 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 the outcome variables. We control for a rich set of patient and facility characteristics and include facility fixed effects while clustering standard errors at the district level.

Table 4 presents the effects of prior authorization on patients' adherence to dialysis and on downstream health outcomes such as hospitalizations and mortality. We find no evidence that prior authorization led to either meaningful decreases in dialysis sessions or increases in adverse events, ruling out even a 0.6% decrease in monthly dialysis sessions at the 95% confidence level.

Although we do not find that prior authorization harmed patients' health on average, it could be that some patients were harmed in ways not captured by our point estimates. To rule

Table 4: Effect of Prior Authorization on Adherence and Adverse Events

	(1) Dialysis Sessions	(2) Mortality	(3) All-Cause Hosp.	(4) Fluid Hosp.
Prior Auth.	-0.0256 (0.0191)	0.000372 (0.000580)	-0.00132 (0.00136)	-0.000854 (0.000777)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Pat/Fac Controls	1	1	1	1
Facility FE	1	1	1	1
R-squared	0.0108	0.00493	0.0159	0.00610
Dep. Var. Mean	12.12	0.00988	0.122	0.0116
Observations	15077158	15077158	15077158	15077158

Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2011–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and 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 fluid overload, 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% levels, respectively.

out 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-emergency ambulance rides to dialysis facilities before prior authorization and compare the outcomes of these frequent riders throughout the staggered rollout of prior authorization across districts. Table 5 shows that, even for the most frequent riders, nothing suggests prior authorization resulted in worse health outcomes.

We also find no evidence of meaningful changes in patients' health following criminal and civil litigation, as shown in Tables A27–A28 in Appendix G. With litigation affecting ridership much less than prior authorization did, it is perhaps not surprising that we similarly see no impact on health outcomes for both of these measures as well.

5.3 Mechanisms of Prior Authorization

Not only did prior authorization cause a large drop in the number of non-emergency ambulance rides to dialysis facilities, it also led to substantial changes in the underlying market for ambulance services. As shown in Table 6 and the corresponding event study in Figure 6, prior authorization resulted in an abrupt reduction in the number of ambulance companies providing non-emergency dialysis rides by 0.286 log points, or 24.9%. We find that, beyond simply reducing the number of ambulance companies, prior authorization also led to greater firm specialization: firms with a higher share of non-emergency rides were more likely to exit following the first

Table 5: Effect of Prior Authorization on Frequent Riders

	(1) Dialysis Sessions	(2) Mortality	(3) All-Cause Hosp.	(4) Fluid Hosp.
Prior Auth.	-0.0226	-0.000433	-0.00828	-0.00137
	(0.0312)	(0.00167)	(0.00517)	(0.00176)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Pat/Fac Controls	1	1	1	1
Facility FE	1	1	1	1
R-squared	0.0742	0.0109	0.0281	0.0148
Dep. Var. Mean	11.88	0.0115	0.179	0.0155
Observations	905331	905331	905331	905331

Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2011–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and 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 fluid overload, often an indication of insufficient dialysis. The sample is limited to patients who took at least 100 non-emergency 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% levels, respectively.

wave of prior authorization's rollout, while the number of firms providing only non-emergency dialysis rides increased. The distribution of firms broken down by the share of non-emergency rides that they provide to dialysis patients in Figure 7 shows that many of the firms that provide non-emergency ambulance rides to dialysis patients provide very few emergency rides to the same population. After prior authorization, fewer firms provide non-emergency rides to dialysis patients overall, but the effect is most pronounced among firms that provide a moderate share of non-emergency rides. At the same time, the number of firms providing only non-emergency rides to dialysis patients increased by a third, from 93 to 120, indicating that regulation led to specialization among the firms that continued to provide this service.

A within-firm analysis provides further evidence of specialization following prior authorization. Firms that initially provided few non-emergency rides were much more likely to stop providing rides altogether after prior authorization: three-quarters of the firms for which non-emergency dialysis rides comprised less than 20% of their total rides no longer provide the service at all. At the other extreme, firms more concentrated in non-emergency rides before the regulation were much less likely to exit this market following prior authorization and in some cases began to specialize even more in providing them, as shown in Figure A18 in Appendix I.

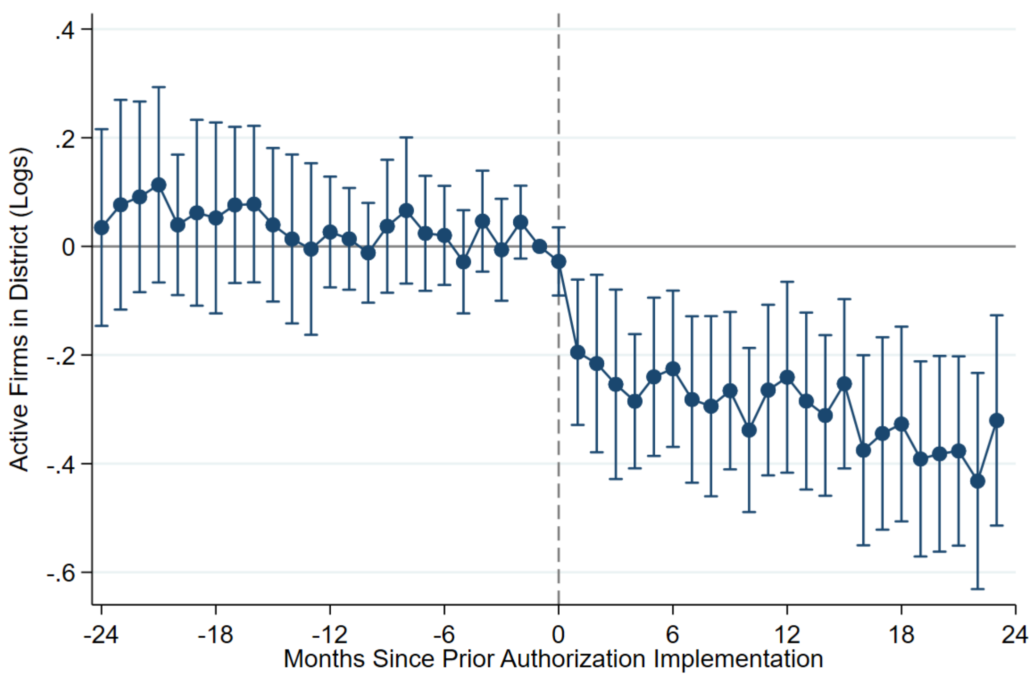
Prior authorization may also reduce fraudulent activity by ensuring that only patients who qualify for rides under Medicare's reimbursement policy end up receiving them. To qualify for a non-emergency ambulance ride, a dialysis patient must be unable to travel safely by any other means, as in the case of a permanently bedridden patient or one who needs a short stint of rides

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

	(1) Active Firms (Log)	(2) Active Firms
Prior Authorization	-0.286***	-13.96*
	(0.0657)	(5.906)
Year-Month FE	1	1
District FE	1	1
Dep. Var. Mean	2.152	17.23
Observations	6336	6336

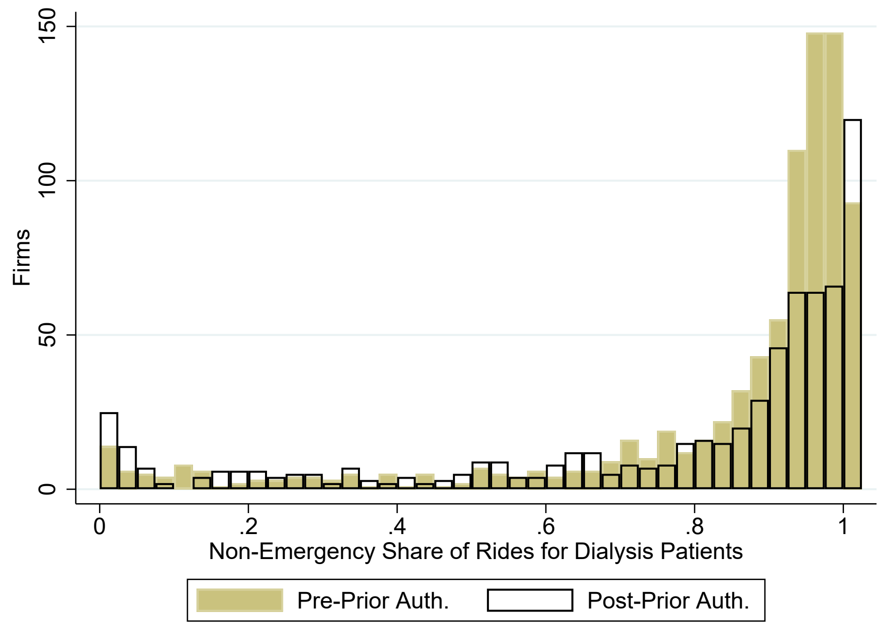
Notes: Estimates of β from equation (2). Dependent variables are the number of firms providing non-emergency basic life support rides between a dialysis facility and a patient's home in a district–month and the natural logarithm of one plus the same. These data include rides from 2012–2017. An observation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

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



Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is the number of firms providing non-emergency 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 represent pointwise 95% confidence intervals.

Figure 7: Change in Distribution of Firms by Share of Non-emergency Rides



Notes: Figure gives the distribution of ambulance firms that served dialysis patients from in the three years before and after prior authorization in states subject to prior authorization in December 2014. A firm's pre-prior authorization non-emergency share is determined by the share of total rides given by the firm in the 36 months before the start of prior authorization in that state that were non-emergency rides between a dialysis treatment facility and a patient's residence. The post–prior authorization share is the same share for the 36 months following the implementation of prior authorization. Firms that gave no non-emergency dialysis rides in the relevant period are excluded.

following a hospitalization. In contrast to litigation that only targets fraudulent behavior, the success of prior authorization depends on its ability to deter fraudulent rides while at the same time not deterring legitimate ones. Despite this delicate tradeoff, several stylized facts suggest that the regulation achieved its primary aim of reducing unnecessary rides without discouraging those who need them.

First, we find that prior authorization led not only to fewer riders overall, but also to less persistent and shorter ridership spells among those who take ambulances, a result consistent with the benefit being used predominately by acutely ill patients who ride for only a limited time. As shown in column (1) of Table 7, which contains estimates of equation (4) with different outcome variables restricted to patients taking an ambulance in the current month, the probability that a current rider continues riding in the following month fell after prior authorization, indicating that ridership became less persistent. Also consistent with this interpretation, the median number of months in which a rider takes a non-emergency ride fell from six to three and the total number of rides taken by each rider decreased substantially in the two years after prior authorization compared to the two years immediately preceding it, as shown in Figure 8.

Table 7: Effect of Prior Authorization on Patient Selection

	(1) Rides Next Month	(2) Hospitalizations	(3) Mortality
Prior Auth.	-0.0633	0.0117^{+}	0.00711^{*}
	(0.0529)	(0.00630)	(0.00348)
Year-Month FE	1	1	1
District FE	1	1	1
Pat/Fac Controls	1	1	1
Facility FE	1	1	1
R-squared	0.113	0.0422	0.0239
Dep. Var. Mean	0.829	0.256	0.0352
Observations	603917	603917	603917

Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2011–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and facility characteristics including chain ownership status, demographic characteristics of the ZIP code, and whether the facility is freestanding or hospital based. The dependent variable in column (1) is an indicator for whether the patient rides in the following month. The dependent variable in column (2) is an indicator for whether the patient is hospitalized in the same month in which he or she is observed to be riding. The dependent variable in column (3) is an indicator for whether a patient dies in the same month that he or she is observed to be riding. Sample is limited to patient–months in which the patient receives at least one non-emergency 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% levels, respectively.

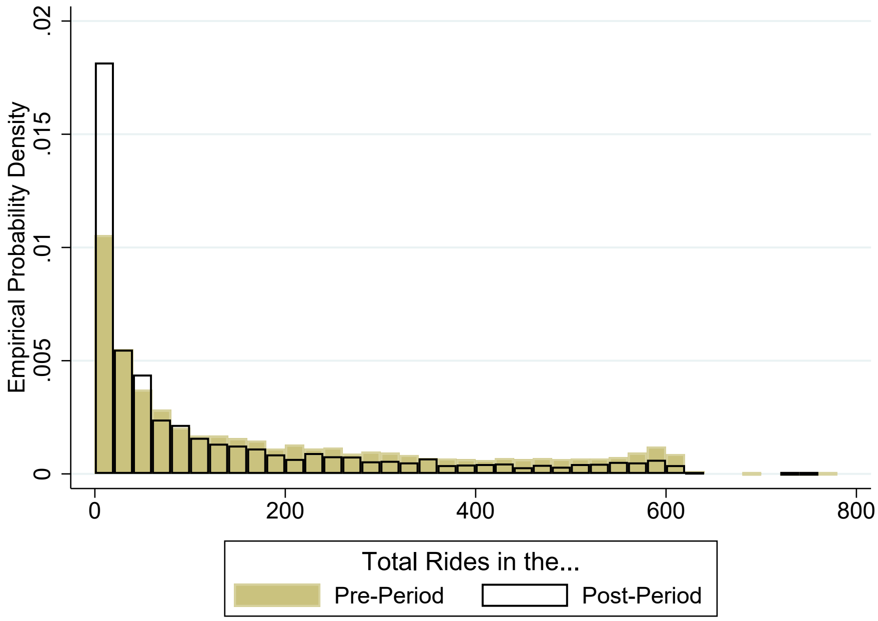
In addition to reducing the duration of ridership spells, prior authorization also resulted in rides being targeted to patients in poorer health. Columns (2) and (3) of Table 7 and panels (b) and (c) of Figure 9 show that the share of ambulance riders suffering an adverse event in the same month they take a ride increased after prior authorization, suggesting a larger proportion of riders with a legitimate need for an ambulance. Taken as a whole, these results indicate that the patients receiving non-emergency 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 ambulances. Although we do not observe the requests submitted by providers to obtain prior authorization, we do observe whether a claim was paid after it was submitted for reimbursement. Figure 10 shows that, immediately following prior authorization, the share of claims denied by Medicare jumped sharply and then declined gradually.[18] Furthermore, Figure A10 shows the denial rates of all firms in panel (a) contrasted with

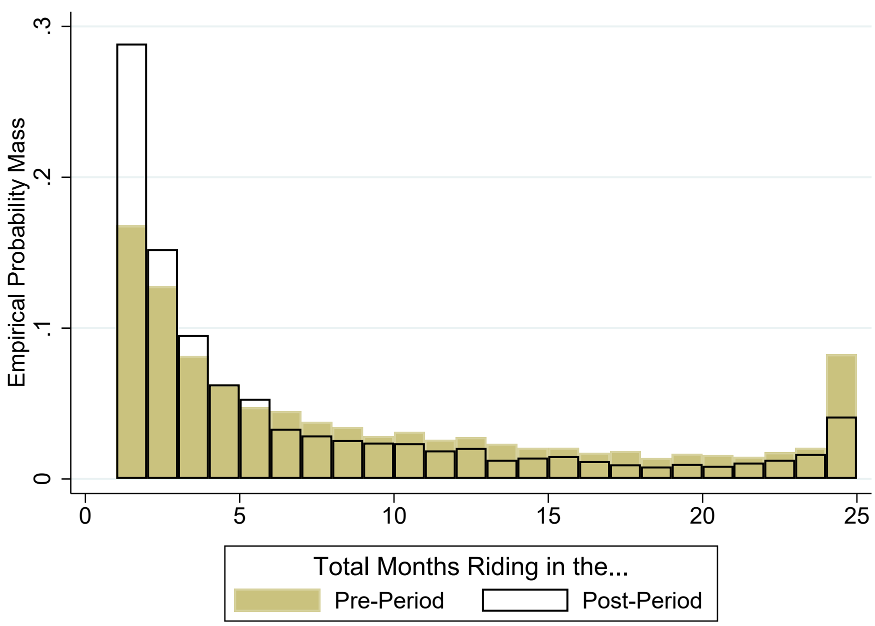
[18] Because these denial rates capture only claims that were submitted after providers could obtain prior authorization for the service, rather than including those that were denied prior authorization, the increase in denial rates after prior authorization is likely a lower bound for the true increase. Indeed, the 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% and 66%.

Figure 8: Histogram of Ridership Among Riders

(a) Total Rides Taken



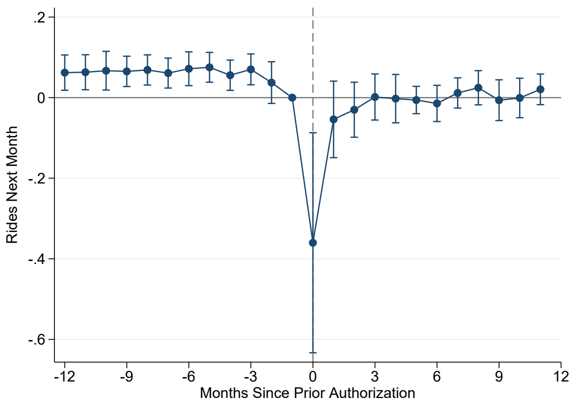
(b) Months with at Least One Ride Taken



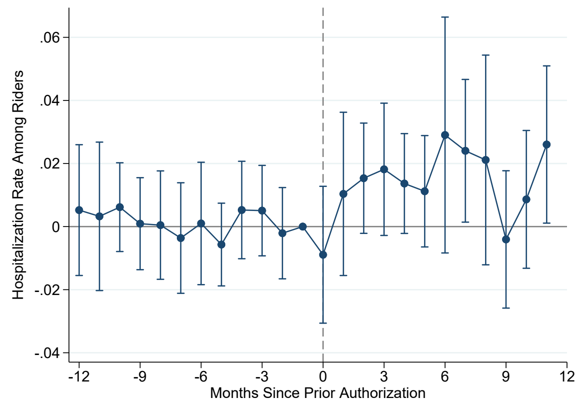
Notes: Panel (a) gives histograms of total rides taken by patients in districts subject to prior authorization in the 24 months before and after the implementation of prior authorization. Panel (b) gives analogous histograms for the total number of months in which the patient takes at least one ride.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

Figure 9: Effect of Prior Authorization on Patient Selection

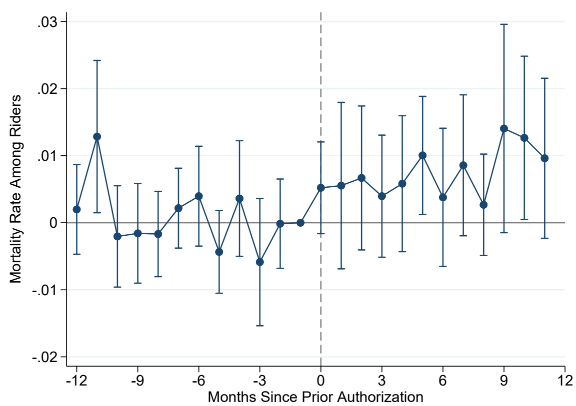
(a) Share of Riders Riding Next Month



(b) Hospitalization Rate Among Riders



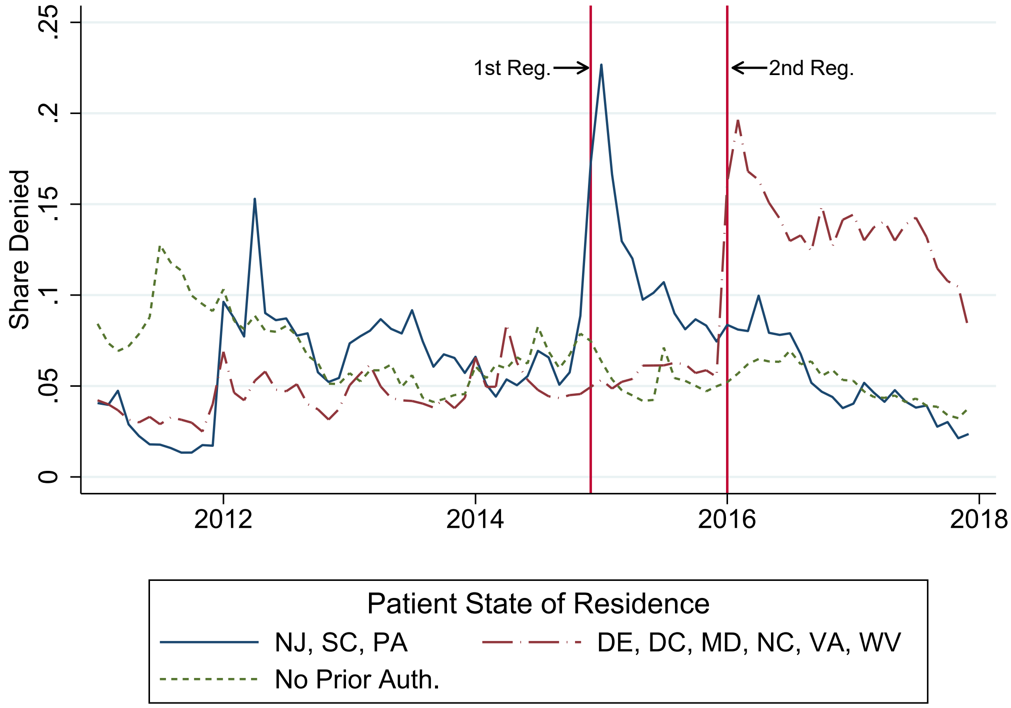
(c) Mortality Rate Among Riders



Notes: Estimates of β_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 fixed effects and 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-emergency dialysis ambulance ride. Standard errors are clustered at the district level. Error bars represent pointwise 95% confidence intervals.

the denial rates of only those firms that continued providing non-emergency rides in panel (b). Both panels have similar patterns for denial rates, although the spike is slightly less pronounced for those that continued to serve the market. In panel (c), we decompose the sample further into firms exiting in the first months of prior authorization, those that did not exit immediately but that did not continue providing rides for at least the next two years, and those that continued regularly providing rides; the spike in denials is most pronounced for firms that exited immediately upon the start of prior authorization. These results suggest that the pattern in denial rates comes from both firms whose claims were denied and then exited the market as well as those whose denial rates initially increased and then declined. That the overall denial rate decreased

Figure 10: Claim Denial Rates by Prior Authorization Status



Notes: The sample includes non-emergency 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.

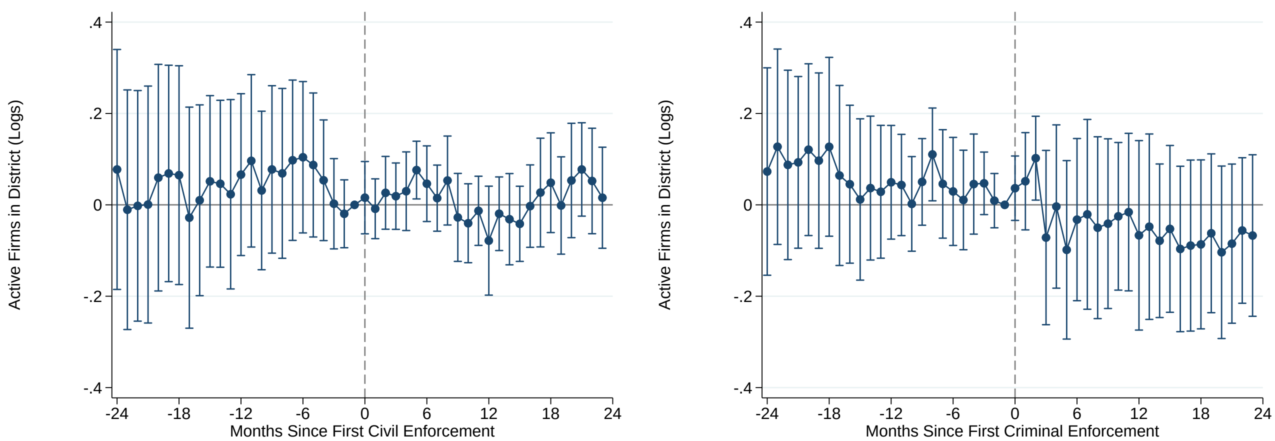
following the initial spike indicates that some firms stopped submitting claims that would be denied under the heightened scrutiny of prior authorization, which we interpret as evidence that prior authorization acts as a screening mechanism that effectively deters fraud.

5.4 Mechanisms of Litigation

In line with its limited impact on ridership and payments, we also find that realized litigation had a negligible effect on the overall structure of the market for ambulance companies. Both Figure 11 and Table 8 demonstrate that civil enforcement does not reduce the number of active firms, while criminal enforcement leads to an imprecisely estimated 4.4% drop. In Appendix G, we further show that, unlike prior authorization, litigation does not affect firm specialization.

Figure 11: Effect of Litigation on Number of Firms in District

(a) Civil Enforcement (b) Criminal Enforcement



Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is the number of firms providing non-emergency 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 come from a 20% sample of all Medicare beneficiaries and include rides from 2007–2019. An observation is a district–month. Standard errors are clustered at the district level. Error bars represent pointwise 95% confidence intervals.

In contrast to its impact on the wider market, we do find that criminal litigation effectively incapacitates the prosecuted firms themselves. Figure 12 uses our 20% sample of all Medicare beneficiaries' rides to show that payments fall nearly to zero for the firms subject to criminal indictments shortly following the indictment, whereas civil litigation has no apparent effect on the indicted firms.

We can further disentangle the respective mechanisms of litigation by contrasting an incapacitation effect — the direct effect of an enforcement action on the defendants themselves — against a deterrence effect of litigation on the other firms in the market not included in the lawsuit. The approximately $12,000 per month reduction in payments per firm following criminal indictments scales up to approximately $60,000 when accounting for our 20% sample of claims. Moreover, in districts subject to criminal litigation in the period for which we observe firm identifiers, the DOJ indicted 14 firms across all districts in the two years following the first indictment, which means that the estimated treatment effect of criminal litigation overall corresponds to the effect

Table 8: Effect of Litigation on Number of Active Firms

	Civil		Criminal	
	(1) Active Firms (Log)	(2) Active Firms	(3) Active Firms (Log)	(4) Active Firms
Enforcement	0.0122	0.779	-0.0442	-5.651
	(0.0290)	(0.652)	(0.0731)	(6.436)
Month-Year FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	1.298	7.192	1.314	6.906
Observations	12143	12143	12203	12203

Notes: Estimates of β from equation (2). Dependent variables are the number of firms providing non-emergency basic life support rides between a dialysis facility and a patient's home in a district–month and the natural logarithm of one plus the same. These data come from a 20% sample of all Medicare beneficiaries and include rides from 2007–2019. 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% levels, respectively.

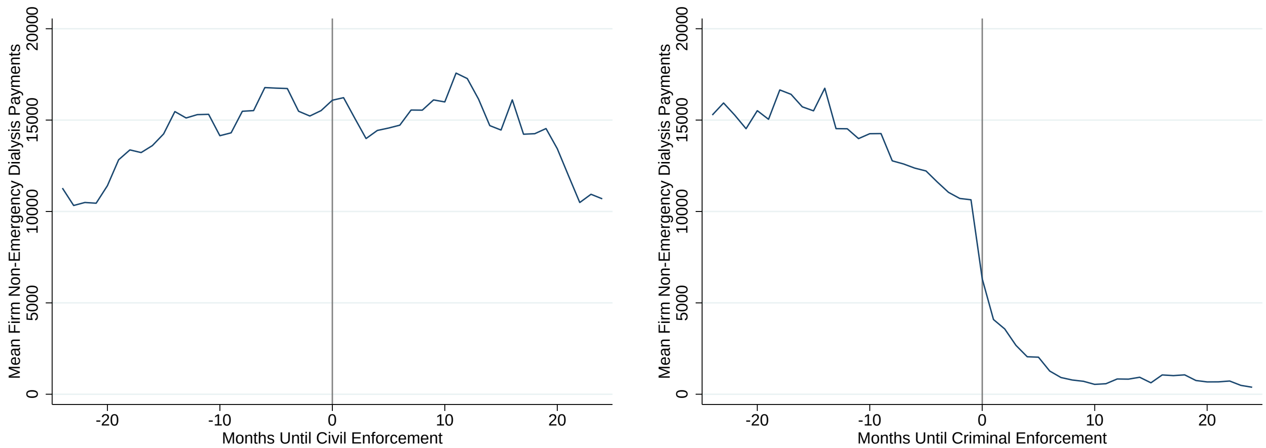
of 1.6 firms being indicted, on average, in each district. Based on these estimates, only $93,000 of the $615,000 per district-month reduction in spending in Table A13 comes from indicted firms, with the remainder coming from firms not directly tied to the enforcement action. Put another way, the criminal incapacitation effect accounts for a comparatively small 15.2% of the overall effect of realized criminal enforcement. Compared to prior authorization, where the regulation reduces payments and rides through claim denials and consequently drives many firms out of the market, litigation has both a direct incapacitation effect on the firms indicted in the lawsuit and a larger deterrence effect on the firms not being indicted as they learn about the enforcement action and decide whether to change their behavior in response.

To test whether lawsuits have a limited impact because firms respond to the threat of enforcement, called general deterrence, rather than realized enforcement, which signals an increase in enforcement capacity, we use hand-collected data from the U.S. Department of Justice on the personnel hours devoted to civil and criminal enforcement in each federal court district and measure enforcement capacity at a district-year level by the number of hours spent in federal criminal or civil court by attorneys in the US Attorney's Office in that district-year on all types of cases, not just health care fraud.[19] Because the budget for US Attorney's Offices is determined through a political process, where national priorities are set and budgets are requested, the rate of health care fraud, and particularly ambulance fraud, has no effect on the level at which different district offices are funded. Any changes in the annual hours of US Attorney's Office staff are therefore plausibly exogenous to the outcome variables measuring ambulance fraud, and we can use this variation to measure the effect of increased enforcement capacity.

[19] See `https://www.justice.gov/usao/resources/annual-statistical-reports`.

Figure 12: Estimates of Incapacitation Effect

(a) Civil Enforcement (b) Criminal Enforcement



Notes: Average monthly Medicare payments to firms subject to civil or criminal enforcement in the 24 months before and after complaint or indictment date. These data come from a 20% sample of all Medicare beneficiaries and include rides from 2007–2019. An observation is a firm–month.

Table 9 shows the effect of various types of enforcement capacity on rides and payments. We do not find a meaningful relationship between ridership and personnel hours for any measure, ruling out at the 95% confidence level, for example, an elasticity of payments with respect to civil enforcement capacity of -0.20 and an elasticity with respect to criminal capacity of -0.32. Although actively pursuing criminal litigation can reduce spending, we see no clear effect of marginal changes in latent enforcement capacity by itself.

Table 9: Effect of Enforcement Capacity on Ambulance Spending and Rides

	(1) Total Ride Payments (Log)	(2) Total Rides (Log)	(3) Total Ride Payments (Log)	(4) Total Rides (Log)	(5) Total Ride Payments (Log)	(6) Total Rides (Log)
Civil Court Hours (Log)	0.0117	-0.0124				
	(0.105)	(0.0574)				
Criminal Court Hours (Log)			0.0631	-0.0705		
			(0.191)	(0.144)		
Total Court Hours (Log)					0.102	-0.0478
					(0.262)	(0.196)
Month-Year FE	1	1	1	1	1	1
District FE	1	1	1	1	1	1
Dep. Var. Mean	12.74	7.701	12.74	7.701	12.74	7.701
Observations	1410	1410	1410	1410	1410	1410

Notes: Estimates from a regression of measures of ridership on log personnel hours: civil hours in columns (1) and (2), criminal hours in columns (3) and (4), and total hours in columns (5) and (6). All rides are non-emergency 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. All specifications include district and time fixed effects. Standard errors are clustered at the district level. $^{+}$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

6 Why Upfront Regulation Outperformed Pay-and-Chase

An extensive theoretical literature has considered whether ex ante regulation or ex post litigation is more effective at combating illegal behavior. Much of this prior work has addressed torts and property rights violations, where individuals or private parties are harmed. We provide an important and natural extension of these studies to circumstances where the injured party is the government, the type of crime is financial fraud, and the illegal behavior is perpetrated by a large number of fraudulent actors. To frame these empirical results, we develop a stylized model to revisit the question of when and how litigation may effectively deter fraud on its own or when regulation must be used in conjunction with it.

Consider a firm deciding whether to commit fraud. The firm will do so if

$$G(Reg) > P_{Crim}F_{Crim} + P_{Civ}F_{Civ}, \tag{5}$$

where $G(Reg)$ is the gain from fraud, which depends on whether prior authorization is in place, captured by $Reg \in \{0,1\}$; P_{Crim} and P_{Civ} are the probabilities of facing criminal or civil enforcement; and F_{Crim} and F_{Civ} are the criminal and civil penalties the firm faces if caught and successfully prosecuted. The gains from fraud are the difference in the fraudulent payments the firm receives from Medicare and the firm's operating costs,

$$G(Reg) = R(Reg) - C(Reg).$$

Costs $C(Reg)$ are higher under prior authorization due to the hassle costs of navigating the regulatory environment, while revenue $R(Reg)$ is lower because the regulation leads to more claim denials, reducing the ability of the firm to steal funds in the first place. The penalties for being caught are

$$F_{Civ} = \min(3R(Reg), Assets) \qquad \text{and} \qquad F_{Crim} = \min(3R(Reg), Assets) + J,$$

which reflects the stipulation of the False Claims Act that financial penalties from a civil judgment are three times the amount stolen but bounded by the firm's assets. That is, the firm faces only limited liability. The parameter J within criminal enforcement captures the firm operator's disutility from going to jail, and jail costs can be imposed in criminal cases even against firms unable to pay the financial penalty.

This stylized model highlights the main factors that determine the relative effectiveness of civil litigation, criminal litigation, and prior authorization and provides a framework for explaining both why firms committed fraud and why regulation was much more effective at stopping them. In short, the potential for lucrative Medicare reimbursements coupled with firms' limited liability and low probability of being detected resulted in widespread fraudulent activity. Prior authorization helps resolve these issues by limiting the initial gains from committing fraud while at the same time not requiring high probabilities of detection or high rates of recovery.

6.1 Limited Liability

In the case of ambulance fraud, the government faces several constraints that make litigation unlikely to have a widespread effect on illegal behavior. First among these is firms' limited liability, as litigation may fail to curtail illicit behavior if severe penalties cannot be enforced (Shavell, 1984; Polinsky and Shavell, 2000). A fly-by-night ambulance company can spend its ill-gotten gains G before being prosecuted and can shut down in response to the financial penalties imposed by the courts, making a pay-and-chase approach largely ineffective. In the model, this is captured in $min(3R, Assets)$, where $Assets$ are endogenously chosen by the firm and may be drawn down quickly to reduce the amount that might have to be repaid upon conviction. Even for a successfully prosecuted firm, the state's likelihood of securing full restitution is low, essentially limiting the firm's liability. Despite judgments regularly reaching millions of dollars, the DOJ warns that restitution for criminal penalties is often 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).

To test this hypothesis in our empirical setting, we filed Freedom of Information Act requests with each of the US Attorney's Offices for the actual financial recoveries from all of the ambulance fraud cases involving dialysis patients in which we observe a prosecution. We were able to determine recovery amounts for 27 cases, which averaged less than \$1.2 million in recovered funds, or 51% of the total amount owed. In only 10 of these cases has the full amount of penalties been paid, while in 11 cases the recovery funds amount to less than 20%, reflecting the limited liability of many defendants. Given that the median case for which we have data closed in 2016, it seems unlikely that full amount for the remaining cases will ultimately be recovered.

The challenge of enforcing financial penalties against this particular population may explain why criminal lawsuits are more effective than civil enforcement. Civil lawsuits impose only monetary penalties or exclusion from the Medicare program, penalties that may not have much impact on firms that can either shut down after allegations of fraud or even continue committing fraud after paying a fine. 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 and incapacitate the operator. This is reflected in our model by J, which is not subject to limited liability. In Section 5 and Appendix Section I, we show that, in practice, nearly all of the accused firms in our data shut down after criminal indictments, whereas civil complaints had almost no effect on the probability of the targeted firm remaining in the market.

Limited liability is not confined to firms alone, as the beneficiaries who participate in the fraud may escape liability as well. Although not included in our stylized model, patients were often a key part of the fraudulent schemes, with some criminal lawsuits alleging that they received kickbacks for riding and referring others. We identify 6,789 unique beneficiaries who rode with the firms that were prosecuted from 2012–2017 and more than 2,700 who immediately stopped riding in the first three states subject to prior authorization, perhaps reflecting a large faction of complicit beneficiaries. Despite compelling evidence of widespread involvement among dialysis patients, the government has criminally prosecuted only four of them for health care fraud, likely owing to their vulnerable conditions as well as the exorbitant costs of imprisoning them in one of the six overcrowded Bureau of Prisons Medical Centers, the institutions for prisoners with acute medical needs like dialysis (Office of the Inspector General, 2015; Federal Bureau of Prisons Clinical Guidance, 2019).

6.2 Low Probability of Detection

In addition to the challenge of levying and collecting large penalties against fraudulent firms, litigation may be hindered by the difficulty of detecting and successfully prosecuting illicit behavior at a sufficiently large scale. In our model, the probability of enforcement is captured by P_{civ} and P_{crim}, where lower values would mean that firms have a higher expected value of committing fraud. From 2007–2014, only 28 firms were subject to criminal litigation and 44 to civil litigation, while we estimate that approximately 1,150 firms may have provided fraudulent rides over this period, implying just a 2.4% chance of being pursued for criminal litigation and 3.8% for civil.[20]

One primary reason for such low detection rates is that health care fraud can be difficult to prove after the fact and criminal lawsuits require a "beyond a reasonable doubt" evidentiary standard, such as video recordings of purportedly bedridden patients walking on their own.[21] With thousands of firms providing non-emergency ambulance rides and the limited resources of the DOJ and FBI constraining their ability to widely prosecute such cases, the chance that any given fraudulent ambulance company will be detected is very low.

A lack of specialization among prosecutors and judges may also partly explain the low detection rates (Landis, 1938). Almost two dozen different judicial districts were involved in the lawsuits that we study, which means that dozens of different investigators, attorneys, and judges were responsible for understanding the complex nature of this fraud in order to successfully prosecute it. Moreover, DOJ 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. We validate this empirically with data from the DOJ National Caseload Data from 2001

[20]Details on these calculations are available in Appendix J.

[21]For example, such evidence was used in the prosecution of Saltville Rescue Squad; case 1:12-00002, Western District of Virginia.

to 2021. Among the US Attorney's Office staff ever assigned as lead attorney in a health care fraud case, the median attorney has five criminal health care fraud cases throughout their career, constituting only 1.8% of their case load, with an interquartile range of 0.5% to 7.9%.

The low rates of detection for ambulance fraud relate to the work of Behrer et al. (2021) and Mookherjee and Png (1992), who argue that, in the case of private harms, litigation alone is ineffective when the harm in question affects a large number of individuals and the private reporting of harm is insufficient. In the case of health care fraud, the injured party is every US taxpayer, and individuals are not empowered to protect the public interest. The government also faces agency costs, because the stolen money does not directly impact the federal employee charged with carrying out enforcement. That is, failing to detect health care fraud has limited consequences for those directly responsible for combating it.

6.3 Gains from Fraud

While the risks to fraudulent firms from litigation were low, the potential gains were large before the onset of prior authorization, as reflected by $G(Reg = 0)$ in our model. Among the 65 litigated firms that we observe in our claims data from 2007–2019, each received an estimated \$5.4 million in payments from Medicare.[22] Although we do not know the costs of perpetrating this fraud, anecdotal evidence indicates they are likely to be very low.

Given the low probability of detection — and, conditional on being prosecuted, the limited recovery rate of fraudulent payments — the expected financial cost of fraud is approximately \$72,000.[23] Ignoring jail time, this figure implies that committing fraud is profitable as long as the firm has a profit margin greater than 1.4%, an exceedingly low hurdle that can explain the widespread proliferation of ambulance taxis before prior authorization.

6.4 Why Regulation Succeeded

Regulation succeeded where litigation failed because it solves the problems of limited liability and a low probability of detection by directly reducing the potential gains from fraud. In the context of our model, this would be captured by a low value for $G(Reg = 1)$, so even with low probabilities of detection and relatively small fines, fraud is no longer profitable. In short, regulation succeeds primarily by preventing fraudulent funds from being paid out in the first place. Under prior authorization, firms are not paid for their claims until they establish that their patients meet Medicare's criteria for a medically necessary ride, which means they cannot spend their ill-gotten gains during the intervening period when the fraud goes undetected. In the model, this comes from a sharp reduction in $R(Reg)$. As shown by Figure 10, the claim denial

[22] This number is approximate because we observe only a 20% sample of claims for these firms.

[23] This figure is the estimated probability of facing civil or criminal litigation multiplied by the average amount recovered by the government in the cases we observe. See Appendix J for details.

rate spiked in states subject to prior authorization, going from 5.7% in the year before prior authorization to 22.7% in January 2015. Limiting the sample to firms that exited at the start of prior authorization, and who are therefore more likely to be fraudulent, we see that the denial rate jumped from 8.1% to 52.5%. Furthermore, the Centers for Medicare and Medicaid Services (2020a) reports that 65% of prior authorization requests submitted in 2015 were denied, with this rejection rate falling to 44% over the next three years. Based on these figures, prior authorization reduced fraudulent firm revenues by roughly 70%, or $3.8 million per firm on average.

Beyond significantly curtailing the ability of fraudulent firms to extract revenue from Medicare, prior authorization also increases firms' costs through administrative burdens and paperwork, captured by $C(Reg = 1)$. Although others have estimated these hassle costs to be large in some settings (e.g., Herd and Moynihan, 2018; Dunn et al., 2023; League, 2023), we consider it unlikely that prior authorization imposes a large burden on patients or physicians in this case. For example, CMS focus groups of physicians indicated that nursing staff generally fill out the forms before a physician signs them, likely imposing a low cost of complying with the regulation. Although ambulance companies expressed more frustration with the process, denials of prior authorization "typically resulted from beneficiaries not meeting CMS's existing (premodel) medical necessity requirements" rather than from clerical errors in filling out the proper paperwork (Weinstock et al., 2018). Furthermore, calibrating the paperwork cost of prior authorization for ambulance rides to those found elsewhere in the literature, we estimate that, even under extreme assumptions, they amount to no more than $3,500 over the entire life of the average ambulance company.[24] Increasing the costs of fraud appears to have contributed much less to the effectiveness of prior authorization than reducing the initial outlay of revenue did.

The large drop in revenue paired with the very modest increase in costs stemming from prior authorization rationalizes the large reduction in fraud that we observe in the data. Using our estimate on the revenue-reducing effect of prior authorization, our model indicates that even if we ignore the disutility of jail time, fraud would be unprofitable under prior authorization as long as fraudulent firms' profit margins without prior authorization are less than 250%.[25] The primary advantage that prior authorization has over litigation, then, is its ability to prevent improper payments from ever being paid out.

Beyond the framework of our stylized model, regulation may complement litigation in other important ways as well. For example, regulations may improve detection rates by making noncompliance more obvious and easier to prosecute in court. Although courts may find it difficult to assess medical necessity, regulations can create "bright-line rules" that are easy to monitor (Kaplow, 1992; Glaeser and Shleifer, 2002). With prior authorization, it is much simpler to provide enough evidence that a firm failed to submit paperwork than it is to prove that a patient

[24]See Appendix J for details.
[25]See Appendix J for details.

did not have a legitimate medical reason for using an ambulance. As discussed in Glaeser and Shleifer (2001), simple, easy-to-enforce regulations strengthen the ability of the government to stop illegal behavior.

Also related is the prior theoretical work of Glaeser and Shleifer (2003) comparing pure litigation-based enforcement to a regime that uses administrative rules as well. Most relevant for our setting, they find that adding administrative rules is optimal in cases where litigation can be subverted. Although not addressed in prior work, the unwillingness of prosecutors to pursue complicit beneficiaries and the challenge of recovering funds from fly-by-night firms are both forms of subversion that make litigation ineffective at assigning liability on its own. We provide suggestive evidence in Appendix I that prior authorization was especially effective at shutting down what appear to be fly-by-night firms, as the increased likelihood that a firm exits after prior authorization was most pronounced among small firms that specialized almost entirely in non-emergency ambulance services.

Administrative enforcers can also be more specialized than judges or prosecutors, which facilitates enforcement (Landis, 1938). As we discussed above, DOJ attorneys are not medical experts or even specialists in health care fraud; these attorneys must convince unspecialized judges and juries that care was not medically necessary, a challenge perhaps best reflected by the ongoing circuit split in which different appellate courts have different standards for whether medical decisions without a consensus opinion can be prosecuted for fraud (Jones Day, 2021). By contrast, the administrators responsible for checking prior authorization requirements for ambulance reimbursements focus solely on Medicare regulations and are well equipped to evaluate medical necessity.[26]

6.5 Relative Cost Effectiveness

Because monitoring paperwork for prior authorization is much simpler than conducting ex post enforcement against fraudulent claims, regulation can accomplish a higher level of deterrence at a much lower cost. As it relates to our setting, 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" (Spitalnic, 2018). Given that we estimate a reduction in Medicare spending of over $300 million in the eight states subject to the pilot program in its first two years, prior authorization is much more cost-effective than widespread

[26] As one administrator noted, "The staff reviewing these claims will be experienced with Medicare's coverage, coding and payment requirements for existing policies and procedures" (Mauch, 2022), while another emphasized that "clinical reviewers receive specialized training for the types of services they are reviewing and have detailed procedures to reference for consistent, calibrated review approaches" (Portzline, 2022).

litigation, which costs \$250,000–\$300,000 per case.[27] Our results in Table A3 suggest that the \$6,500,000 spent on litigating civil cases (26 cases $\times$ \$250k/case) had no effect beyond the cases themselves, and Appendix Table A20 suggests that prosecuting additional firms does not amplify the effects of criminal litigation. In the context of the model, realized criminal and civil litigation can potentially reduce fraud among non-indicted firms by raising the perceived probability of detection, either because the actual detection probabilities are higher or because their salience increases subjective beliefs about them. Even when a fraudulent ambulance company faces civil prosecution with absolute certainty, however, our model estimates still imply that prior authorization has greater deterrence effects in light of firms' limited liability. Expending additional resources pursuing widespread litigation will never be able to achieve the deterrence of prior authorization and would cost substantially more.

In addition to deterrence, regulation and litigation can have other effects that are difficult to measure empirically. In response to the increased scrutiny of ambulance taxis, some firms may simply choose to forgo this type of fraudulent activity in the first place, a general deterrence effect of unknown magnitude (Shavell, 1991; Leder-Luis, 2023). 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. On the other hand, regulation may create additional non-monetary costs. For example, regulation may be costly if it results in care being rationed inefficiently (American Medical Association, 2021), which then leads to a lower quality of care. As noted above, however, we find no evidence that prior authorization led to worse outcomes for patients in our setting.

Finally, the administrative burden associated with regulation may impose hassle costs on non-fraudulent firms or may beneficially serve as a screening mechanism. As discussed above, the paperwork costs of this particular regulation are low, particularly when compared to the reduction in Medicare spending. As shown in Table A24 in Appendix F, even under extreme assumptions well outside the range of cost estimates in the literature, the total paperwork costs of prior authorization are less than \$60,000 per district per month, or only 8.1% of the estimated reduction in Medicare spending. Beyond its relatively low direct costs, regulation may be well targeted such that only medically necessary services are rendered as providers and patients

[27] We arrive at this estimate using two different approaches. First, Leder-Luis (2023) measures public spending on False Claims Act cases, finding \$108.5 million spent on 446 civil cases, or \$243,000 per case. Second, the Federal Health Care Fraud and Abuse Control Program Annual Report provides details on the number of civil and criminal health care fraud investigations, estimating that \$1,059,315,473 was spent on 3,603 investigations in 2019, or \$294,000 per case. Specifically, the DOJ opened 1,060 new criminal health care fraud investigations, and it opened 1,112 new civil health care fraud investigations. In addition, investigations conducted by HHS's Office of Inspector General (HHS-OIG) resulted in 747 criminal actions and 684 civil actions against individuals or entities that engaged in crimes related to Medicare and Medicaid. We arrive at the 3,603 figure by summing these investigations and actions. Note that our estimate is likely somewhat biased downward as an estimate of the cost of litigation since there are investigations that do not result in actions and these figures do not include other relevant budgetary figures (e.g., from the FBI budget).

anticipate that only valid claims will be approved (Zeckhauser, 2021), resulting in an equilibrium in which the regulation is not costly to enforce because fewer claims are filed in the first place. In our setting, this is consistent with the changes that we observe for both denial rates and the mix of patients riding in ambulance following prior authorization.

7 Conclusion

We find that imposing prior authorization on ambulance rides for dialysis patients was much more effective at reducing wasteful spending than pursuing criminal or civil litigation on their own. Prior authorization caused an immediate and persistent drop in non-emergency ambulance rides of nearly 68%, whereas lawsuits against fraudulent providers had a much smaller effect. Had the federal government required prior authorization throughout our sample period, it would have saved $4.8 billion and prevented 21.2 million unnecessary rides at an administrative cost of only $28 million per year (Spitalnic, 2018).[28] When compared to pay-and-chase enforcement and the relatively large costs associated with it, prior authorization is much more efficient.

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

Our results relate to the economic theory of why regulation is necessary — and litigation alone insufficient — for successfully combating Medicare fraud. Criminal and civil penalties are often too low given prosecutors' inability to levy large penalties against 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. This points to health care fraud as being an area in need of regulatory innovations to complement the use of legal enforcement through prosecution. Medicare has recently moved in this direction, expanding prior authorization to other medical expenditures that may be especially susceptible to fraud, such as power mobility devices, home health services, and hyperbaric oxygen. Our results suggest that such reforms are likely to be successful.

Our results also highlight a way to reduce fraud in other areas of government expenditure. Whenever dealing with a multitude of small firms, the government faces the same challenges of limited liability and a low probability of detection that hindered its response to ambulance taxis. In cases such as pandemic aid (Griffin et al., 2021; Autor et al., 2022) and defense contracting (Karpoff et al., 1999), regulations like prior authorization that verify upfront whether a payment is appropriate can be used to deter fraud effectively.

[28] See Appendix K for details of the calculation of savings from prior authorization.

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Appendix: For Online Publication

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

Appendix A provides more detail on the litigation activity that we observe.

Appendix B estimates our main results using alternative estimation methods.

Appendix C presents evidence of balance in characteristics across areas subject to different forms of enforcement at different times.

Appendix D contains robustness checks of our estimates.

Appendix E investigates potential heterogeneity in the effects of each intervention.

Appendix F presents more results on the impact of prior authorization that are referenced in the text.

Appendix G presents more results on the impact of criminal and civil litigation that are referenced in the text.

Appendix H directly compares the effects of civil and criminal litigation and prior authorization.

Appendix I presents evidence of the effect of enforcement on the firms directly subject to it.

Appendix J provides more detail on the simple model presented in Section 6.

Appendix K provides details on how our counterfactual spending figures are calculated.

A Detailed Information on Litigation Activity

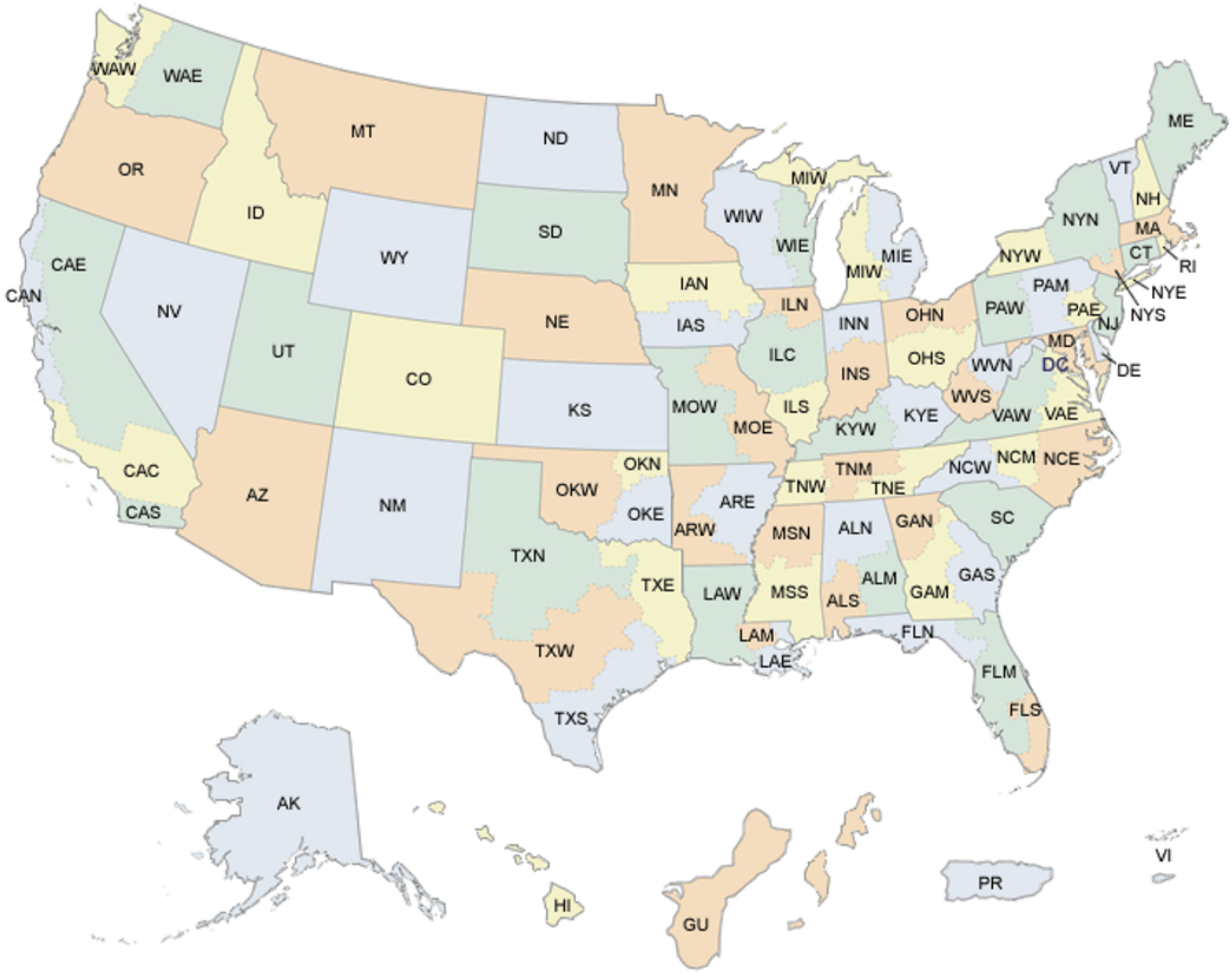
In this appendix, we report the data that we collected on litigation regarding fraudulent dialysis ambulance transports. These data were collected through extensive searches of PACER, Department of Justice press releases, and news articles. To be included, cases must have explicitly mentioned dialysis facilities as a destination of fraudulent rides. District-level information on enforcement is given in Table A1. Figure A1 shows the geographic boundaries of US court districts.

Table A1: District-Level Data on Enforcement

	Criminal		Civil	
District	Start of Enforcement	Total Cases	Start of Enforcement	Total Cases
Alabama North		0	09/10/2009	1
Arkansas East	04/12/2000	2	04/23/1999	2
California Central	05/09/2002	4	03/12/2003	2
California South		0	11/20/2009	1
Connecticut		0		1
Florida Middle		0	03/10/2015	1
Georgia Middle		0	05/01/2015	1
Georgia South		0	10/26/2017	1
Guam	01/20/2016	1		0
Illinois North		0	10/04/1996	1
Indiana North	11/08/2012	4		0
Kentucky East	06/01/2017	1	01/31/2013	3
Massachusetts		0	10/13/1998	2
New Jersey	10/6/2014	1		0
North Carolina East	04/15/2004	3	02/28/2003	3
Ohio South	12/23/2015	2		0
Pennsylvania East	02/08/2011	10		0
Pennsylvania Middle	01/11/2012	1	06/01/2011	1
Rhode Island	05/12/2011	1	11/08/2011	2
South Carolina		0	01/23/2011	1
Tennessee Middle	01/06/2010	1	08/08/2012	1
Texas East	9/14/2006	3		0
Texas North	06/02/2009	1		0
Texas South	12/06/2006	6	10/07/2011	1
Virginia East		0	11/20/2017	1
Virginia West	01/15/2008	2		0

Notes: Date of treatment by litigation for each district subject to treatment. Start of enforcement is given by the first complaint date for civil cases filed in the district and the first indictment date for criminal cases filed in the district.

Figure A1: Map of US Court Districts



Notes: Figure from The United States Department of Justice (2018).

B 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).[29] To overcome this issue, we use several recently introduced methods to estimate the results that we present elsewhere in the paper.

B.1 Callaway and Sant'anna

The first of these methods is the group-time average treatment effect estimator introduced by Callaway and Sant'Anna (2021). This method estimates the effect of treatment separately for each group of districts treated at the same time, using only never-treated districts 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.[30] 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 β recovered using traditional TWFE methods. The parameter

$$\theta_{sel}(\tilde{g}) = \frac{1}{\mathcal{T} - \tilde{g} + 1} \sum_{t=\tilde{g}}^{\mathcal{T}} ATT(\tilde{g}, t) \tag{6}$$

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. Moreover, because districts are treated at different times, some treatment groups are treated later in our sample period

[29] See, for example, Borusyak et al. (2017); de Chaisemartin and D'Haultfœuille (2020); Goodman-Bacon (2021); Sun and Abraham (2020); Athey and Imbens (2022).

[30] Because this method does not allow for time-varying controls, ΓX_{dt} is not included in our estimating equation using this estimator.

than others, which means that 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 aggregate $ATT(g,t)$ only for groups treated for at least L months and recover the average treatment effect for treatment of length e on districts treated for at least e' periods:

$$\theta_{es}^{bal}(e;L) = \sum_{g\in\mathcal{G}} \mathbf{1}\{g+L \leq \mathcal{T}\} ATT(g,g+e) P(G=g|G+L\leq\mathcal{T}), \tag{7}$$

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

$$\theta_{es}^{O,bal}(L) = \frac{1}{L+1}\sum_{e=0}^{L}\theta_{es}^{bal}(e,L), \tag{8}$$

which is simply the unweighted average of the parameters given by equation (7) 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 outcome in the time period immediately before treatment. This parameter, along with $\theta_{es}^{bal}(e;L)$, can be estimated with the `csdid` command in Stata (Callaway and Sant'Anna, 2021; Sant'Anna and Zhao, 2020).

Table A2: Effect of Prior Authorization 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.	-681581.1^{+}	-1.111**	-3432.3^{+}	-0.895***	-13.16*	-0.282***
	(377211.5)	(0.345)	(1899.5)	(0.172)	(5.678)	(0.0641)

Notes: Estimates of $\theta_{es}^{O,bal}(23)$ using methods from Callaway and Sant'Anna (2021). All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables 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 and Sant'anna's bootstrap-based procedure. $^{+}$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

Figure A2 presents estimates of $\theta_{es}^{bal}(e;23)$ for $e \in [-24,23]$. We find that this estimation method yields estimates similar to those given in Figures 3, 4, and 6.

Table A3: 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	-0.0313	0.0306	-0.220	-0.268*
	(0.106)	(0.0656)	(0.138)	(0.131)

Notes: Estimates of $\theta_{es}^{O,bal}(24)$ using methods from Callaway and Sant'Anna (2021). All rides are non-emergency 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 and Sant'anna's bootstrap-based procedure. $^{+}$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

B.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 (albeit with different time values). We then estimate

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

where α_{dg} and α_{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

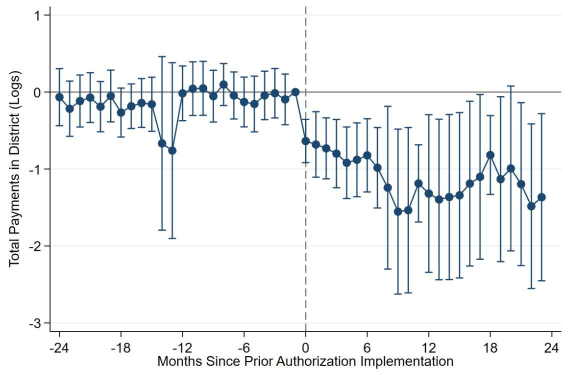
$$Y_{dt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \beta \sum_{e=0}^{L} T_{dt}(e) + \alpha_{dg} + \alpha_{tg} + \Gamma X_{dt} + \varepsilon_{dt} \tag{10}$$

on the stacked data.

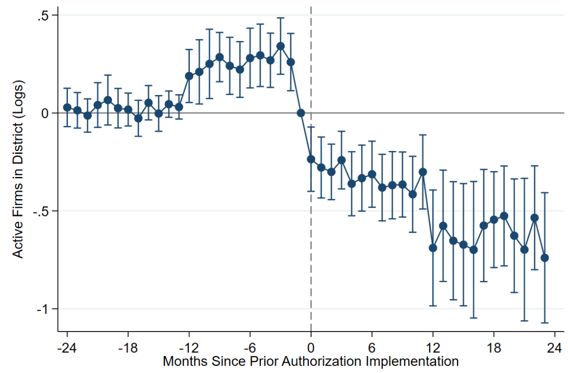
Figure A3 presents estimates of equation (9). We again find that this estimation method

Figure A2: Dynamic Treatment Effects, Callaway and Sant'anna

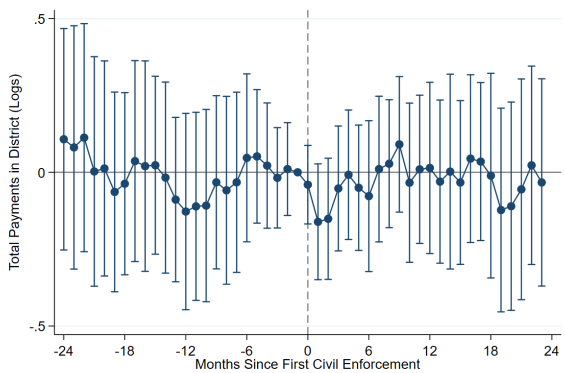
(a) Prior Auth: Total Payments (log)



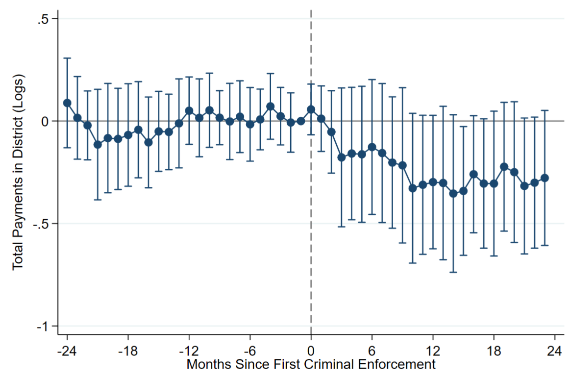
(b) Prior Auth: Active Firms (log)



(c) Civil: Total Payments (log)



(d) Criminal: Total Payments (log)



Notes: All rides are non-emergency 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 $\theta_{es}^{bal}(e;23)$ for $e \in [-24, 23]$ using methods from Callaway and Sant'Anna (2021). The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are obtained using Callaway and Sant'anna's bootstrap-based procedure. Error bars represent 95% confidence intervals.

Table A4: Effect of Prior Authorization on Ambulance Rides and Spending, Stacked Regression

	(1) Total Ride Payments (Log)	(2) Total Ride Payments	(3) Total Rides (Log)	(4) Total Rides	(5) Active Firms (Log)	(6) Active Firms
Prior Auth.	-1.116**	-718363.6^{+}	-0.900***	-3613.4^{+}	-0.286***	-13.70*
	(0.344)	(397999.5)	(0.172)	(1999.2)	(0.0642)	(5.773)
Dep. Var. Mean	9.838	399718.0	5.298	1977.1	2.114	16.60
Observations	8208	8208	8208	8208	8208	8208

Notes: Estimates of β from equation (10). All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables 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% levels, respectively.

Table A5: 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	-0.0314	0.0319	-0.179	-0.242*
	(0.105)	(0.0601)	(0.110)	(0.0980)
Dep. Var. Mean	9.582	5.049	9.484	5.004
Observations	36960	36960	44928	44928

Notes: Estimates of β from equation (10). All rides are non-emergency 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% levels, respectively.

results in estimates very similar to those given in Figures 3, 4, and 6.

B.3 Imputation Estimator

The final estimator that we consider is the imputation estimator introduced by Borusyak et al. (2017). To implement this estimator, we first estimate

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

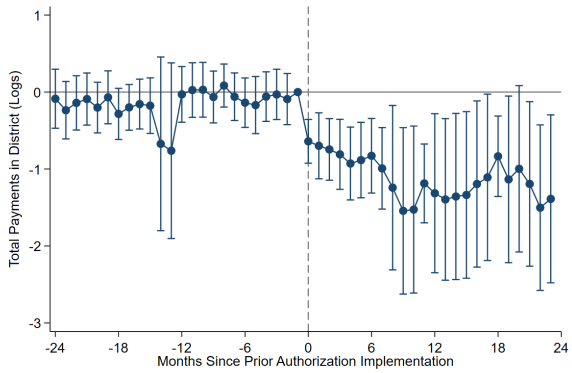
using the untreated observations, including all observations for never-treated districts and pre-treatment observations for treated districts. Then, we predict counterfactual outcomes for the treated observations using the estimates from the previous equation,

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

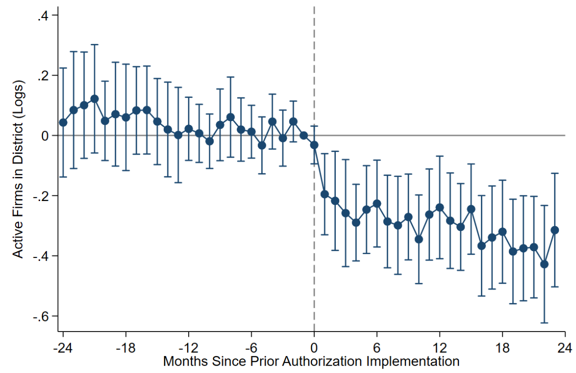
The difference between this and the realized outcome represents the observation-specific treatment effect (plus error), such that we can take a weighted average of these differences ($\hat{\tau}_{dt} = Y_{dt} - \hat{Y}_{dt}$) to obtain the ATT. Conveniently, this model can be estimated with the `did_imputation`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

Figure A3: Dynamic Treatment Effects, Stacked Regression

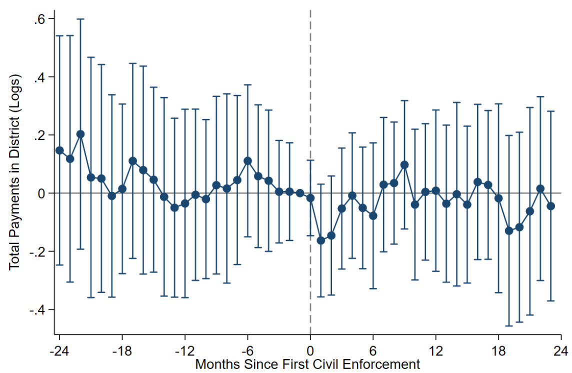
(a) Prior Auth: Total Payments (log)



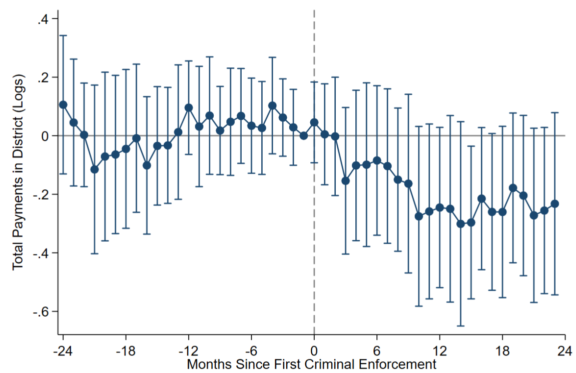
(b) Prior Auth: Active Firms (log)



(c) Civil: Total Payments (log)



(d) Criminal: Total Payments (log)



Notes: All rides are non-emergency 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 β_e for $e \in [-24, 23]/\{-1\}$ from equation (9). 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 represent 95% confidence intervals.

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.

Figure A4 presents estimates of $\Delta\tau(e)$ for $e \in [-24, 23]$. We again find that this estimation method results in estimates very similar to those given in Figures 3, 4, and 6.

Table A6: Effect of Prior Authorization on Ambulance Rides and Spending, Imputation Estimator

	(1) Total Ride Payments (Log)	(2) Total Ride Payments	(3) Total Rides (Log)	(4) Total Rides	(5) Active Firms (Log)	(6) Active Firms
Prior Auth.	-1.412**	-718657.8*	-1.038***	-3720.1*	-0.333***	-15.45**
	(0.545)	(356856.3)	(0.223)	(1824.7)	(0.0776)	(5.647)
Observations	7747	7747	7747	7747	6631	6631

Notes: Estimates of $\Delta\tau(23)$. All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables 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% levels, respectively.

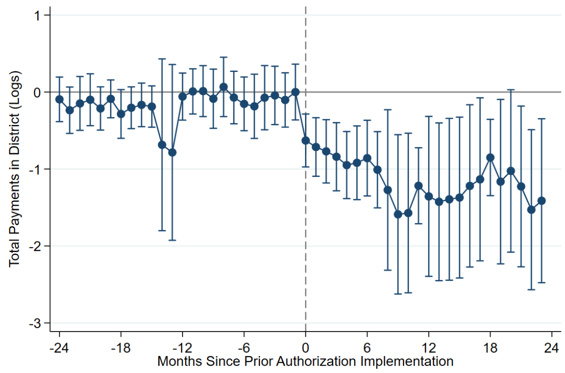
Table A7: Effect of Litigation on Ambulance Spending and Rides, Imputation Estimator

	Civil		Criminal	
	(1) Total Ride Payments (Log)	(2) Total Rides (Log)	(3) Total Ride Payments (Log)	(4) Total Rides (Log)
Enforcement	-0.198	-0.0922	-0.434	-0.451
	(0.333)	(0.295)	(0.573)	(0.307)
Observations	15425	15425	15547	15547

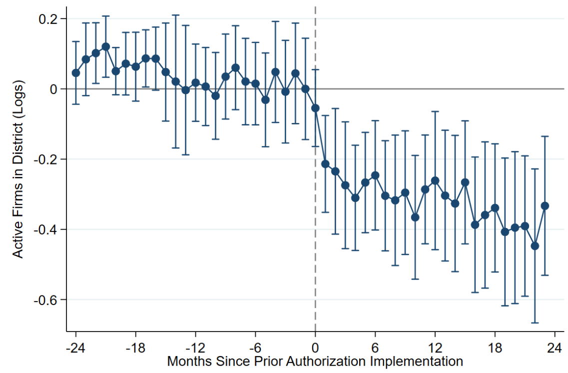
Notes: Estimates of $\Delta\tau(23)$. All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is 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% levels, respectively.

Figure A4: Dynamic Treatment Effects, Imputation Estimator

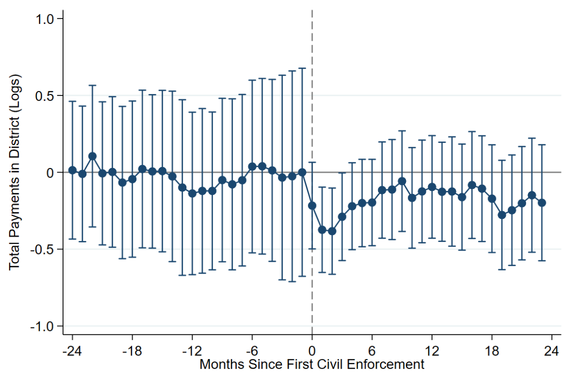
(a) Prior Auth: Total Payments (log)



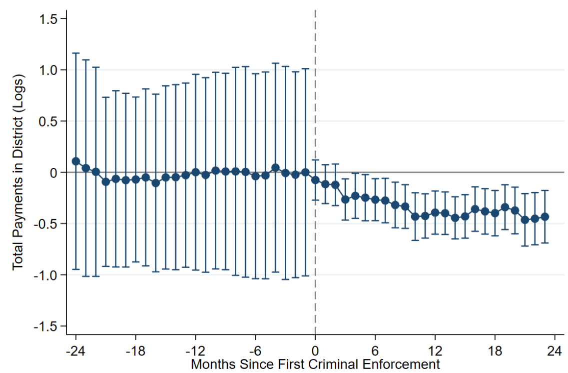
(b) Prior Auth: Active Firms (log)



(c) Civil: Total Payments (log)



(d) Criminal: Total Payments (log)



Notes: All rides are non-emergency 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 represent 95% confidence intervals.

C Balance Table

Table A8: Summary Statistics of Patient–Month-Level Data by Prior Authorization Wave

	Prior Authorization Wave			
	Wave 1	Wave 2	Untreated	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-Emergency Dialysis Rides	3.12	0.91	0.77	1.01
Emergency Rides	0.127	0.124	0.124	0.124
Total Lifetime Rides	152.7	50.0	46.2	56.6
Continuing to Ride Next Month	0.890	0.851	0.835	0.852
Facility Characteristics				
Facility Age (Years)	15.97	17.47	16.01	16.16
Freestanding Facility	0.948	0.964	0.952	0.953
Chain Affiliation				
DaVita	0.274	0.372	0.339	0.336
Fresenius	0.417	0.443	0.348	0.364
Other Chain	0.167	0.067	0.137	0.133
Independent	0.142	0.118	0.176	0.167
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 emergency rides are based on non-emergency 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.

Table A9: Patient and Facility Characteristics by Enforcement Type

	Treatment Exposed To				
	Prior Auth.	Civil	Criminal	None	Overall
Patient Characteristics					
Age (Years)	63.84	63.42	62.91	63.87	63.72
Months with ESRD	36.16	36.15	35.84	35.55	35.79
Black	0.527	0.410	0.342	0.333	0.375
Male	0.521	0.523	0.524	0.529	0.528
Diabetic	0.460	0.487	0.504	0.486	0.483
Drug User	0.014	0.010	0.010	0.010	0.011
Smoker	0.058	0.048	0.048	0.056	0.053
Drinker	0.016	0.014	0.012	0.013	0.013
Uninsured at Incidence	0.092	0.126	0.138	0.092	0.105
Employed at Incidence	0.121	0.111	0.112	0.121	0.119
Facility Characteristics					
Facility Age (Years)	12.82	12.89	12.38	11.94	12.22
Freestanding Facility	0.906	0.957	0.969	0.893	0.915
Chain Affiliation					
DaVita	0.190	0.227	0.225	0.186	0.194
Fresenius	0.351	0.311	0.308	0.229	0.272
Other Chain	0.243	0.288	0.241	0.258	0.262
Independent	0.215	0.175	0.226	0.327	0.272
Health Outcomes					
Dialysis Sessions	12.39	12.16	12.30	12.38	12.31
All-Cause Hosp.	0.157	0.152	0.151	0.147	0.151
Fluid Hosp.	0.018	0.018	0.018	0.016	0.017
Mortality	0.013	0.013	0.013	0.014	0.013
Patient-Months	1,252,059	1,794,478	1,388,267	3,089,369	6,066,724

Notes: Data are from 2003–2005. Patient characteristics except age and dialysis tenure are at incidence of ESRD. 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. Observations may appear in multiple columns if the patient's jurisdiction is subject to multiple interventions.

D Robustness Checks of Results

In this appendix, we present robustness checks of our estimates of the effects of litigation and prior authorization. First, we present evidence that separately estimating the effects of prior authorization and of civil and criminal litigation does not invalidate our comparisons of these effects. In order to estimate the effect of each of these forms of enforcement jointly, we estimate

$$Y_{dt} = \beta_1 PriorAuth_{dt} + \beta_2 Crim_{dt} + \beta_3 Civ_{dt} + \alpha_d + \alpha_t + \varepsilon_{dt}, \tag{11}$$

where $PriorAuth_{dt}$ is an indicator for prior authorization being in place, $Crim_{dt}$ is an indicator for a criminal indictment having occurred in the district, and Civ_{dt} is an indicator for a civil complaint having been filed in the district. Note that this is similar to our main specification of equation (2) although here we do not window the sample to be within a certain time period of treatment. Table A10 shows that our results are very robust to this alternative estimation strategy.

Table A10: Effect of All Three Treatments

	(1) Total Ride Payments (Log)	(2) Total Ride Payments	(3) Total Rides (Log)	(4) Total Rides
Prior Authorization	-1.046**	-437024.2^{+}	-0.804***	-2077.9^{+}
	(0.356)	(227497.8)	(0.226)	(1100.3)
Civil Enforcement	0.114	140577.9	0.237	666.7
	(0.281)	(321269.5)	(0.233)	(1481.1)
Criminal Enforcement	-0.317	-167849.2	-0.332*	-849.6
	(0.244)	(184610.3)	(0.150)	(835.2)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	9.802	408528.8	5.276	1981.0
Observations	16740	16740	16740	16740

Notes: Estimates of β_i for $i \in \{1, 2, 3\}$ from equation (11). All rides are non-emergency 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. 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% levels, respectively.

Next, we present evidence that our results using a natural logarithm transformation are robust to other transformations used to approximate percentage change. Table A11 presents results using the inverse hyperbolic sine transformation, while Table A12 presents results using Poisson regression.

Table A11: Effect of Treatment, Inverse Hyperbolic Sine Transformation

	Prior Auth.			Civil		Criminal	
	(1) Total Ride Payments (IHS)	(2) Total Rides (IHS)	(3) Active Firms (IHS)	(4) Total Ride Payments (IHS)	(5) Total Rides (IHS)	(6) Total Ride Payments (IHS)	(7) Total Rides (IHS)
Treatment	-1.156**	-0.946***	-0.299***	-0.0512	0.0193	-0.203+	-0.272**
	(0.377)	(0.197)	(0.0699)	(0.117)	(0.0698)	(0.109)	(0.0993)
Year-Month FE	1	1	1	1	1	1	1
District FE	1	1	1	1	1	1	1
Dep. Var. Mean	10.53	5.936	2.624	9.796	5.388	9.935	5.488
Observations	7272	7272	6336	14160	14160	14436	14436

Notes: Estimates of β from equation (2). All rides are non-emergency 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, except for the data in column (7), which include firms active from 2012–2017. Dependent variables are transformed by applying the inverse hyperbolic sine function. 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% levels, respectively.

Table A12: Effect of Treatment, Poisson Regression

	Prior Auth.			Civil		Criminal	
	(1) Total Ride Payments	(2) Total Rides	(3) Active Firms	(4) Total Ride Payments	(5) Total Rides	(6) Total Ride Payments	(7) Total Rides
Treatment	-0.829**	-0.840**	-0.355***	0.0946	0.0971	-0.278*	-0.279*
	(0.304)	(0.309)	(0.100)	(0.0677)	(0.0722)	(0.109)	(0.113)
Year-Month FE	1	1	1	1	1	1	1
District FE	1	1	1	1	1	1	1
Dep. Var. Mean	420139.8	2028.8	17.43	296463.9	1447.8	314348.4	1545.2
Observations	7188	7188	6264	14160	14160	14436	14436

Notes: Estimates of β from equation (2) using Poisson regression. All rides are non-emergency 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, except for the data in column (7), which include firms active from 2012–2017. 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% levels, respectively.

In Table A13, we show that the results presented in Table 3 are robust to measuring the outcomes in levels.

One potential concern regarding our comparison of districts subject to litigation and those that are not is that even though there are no differential pre-trends between treatment and control districts, they may still have been subject to differential shocks that occurred only after litigation. To address these questions, we conduct a number of tests using different control groups and specifications. In brief, every specification produces the same pattern: criminal enforcement caused a measurable but modest decrease in ambulance taxis spending, while civil enforcement produced no effect.

The first way we address these concerns is to redo our analysis to use only differences in the

Table A13: Effect of Litigation on Spending and Ridership, Levels

	Civil		Criminal	
	(1) Total Ride Payments	(2) Total Rides	(3) Total Ride Payments	(4) Total Rides
Enforcement	81487.9^{+}	390.9	-615088.4	-3154.1
	(48571.3)	(239.1)	(467497.0)	(2358.2)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	296463.9	1447.8	314348.4	1545.2
Observations	14160	14160	14436	14436

Notes: Estimates of β from equation (2). All rides are non-emergency 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. 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% levels, respectively.

timing of litigation among districts that are all subject to litigation at some point for identification instead of comparing treated versus untreated districts. This way, we only compare districts where litigation occurs to themselves rather than compare districts where litigation occurs to those where it does not.

Table A14 and Figure A5 show the results from this specification using the Callaway and Sant'anna estimator discussed in Appendix B.1, limiting the comparison to not-yet-treated jurisdictions, rather than including never-treated jurisdictions as controls as well. The Callaway and Sant'anna estimator was designed to flexibly capture average treatment effects using different potential control groups, making it an ideal estimator for this exercise. Our results are robust to limiting the control group to only districts not yet subject to litigation.

Table A14 and Figure A5 show strikingly similar effects to those in the paper. In particular, we see a modest effect of criminal enforcement, an effect of about -0.25 log points on rides and payments, and no effect of civil enforcement. This supports the argument that the control groups in our original, preferred specification are not a source of bias in our estimates.

Similarly, rather than comparing districts exposed to litigation to other districts, we can simply show what happens in the districts themselves, without having any control jurisdictions, by comparing the ridership in districts exposed to litigation before and after they are exposed.

Table A14: Effect of Litigation on Ridership, Not-Yet-Treated as Controls

	Civil		Criminal	
	(1) Total Ride Payments (Log)	(2) Total Rides (Log)	(3) Total Ride Payments (Log)	(4) Total Rides (Log)
Enforcement	-0.0296	0.0286	-0.245^{+}	-0.281^{*}
	(0.101)	(0.0646)	(0.145)	(0.135)

Notes: Estimates of $\theta_{es}^{O,bal}$(24) using methods from Callaway and Sant'Anna (2021) with the control group limited to not-yet-treated districts. All rides are non-emergency 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 and Sant'anna's bootstrap-based procedure. $^{+}$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

To do this, we estimate

$$Y_{dt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \sum_{e=0}^{L} \beta_e T_{dt}(e) + \alpha_d + \Gamma X_{dt} + \varepsilon_{dt}, \tag{12}$$

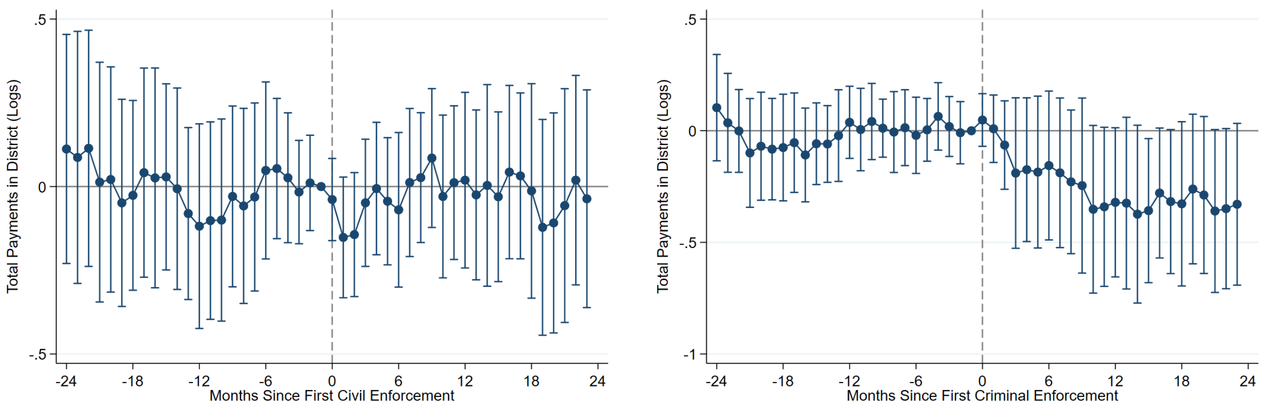
where the sample is limited to districts subject to the relevant form of litigation. Figure A6 below present estimates of this event study, where we see no change in ridership in districts subject to civil litigation and clear trend breaks for districts subject to criminal litigation.

As a final alternative comparison group, we compare districts subject to litigation to only those districts that border them. First, in Appendix G, we show that ridership in these nearby districts did not respond to litigation, i.e. that there are no spillovers onto nearby areas. Here, we show that limiting our control group to these more-similar districts also produces identical results, as shown by Figure A7 and Table A15.

In summary, these additional tests validate our original empirical strategy and show that our results are remarkably robust across different control designs.

Figure A5: Effect of Litigation on Ridership, Not-Yet-Treated as Controls

(a) Civil Enforcement (b) Criminal Enforcement



Notes: All rides are non-emergency 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 $\theta^{bal}_{es}(e; 23)$ for $e \in [-24, 23]$ using methods from Callaway and Sant'Anna (2021) with the control group limited to not-yet-treated districts. The treatment date is the earliest enforcement action of the relevant type in the district. Standard errors are obtained using Callaway and Sant'anna's bootstrap-based procedure. Error bars represent 95% confidence intervals.

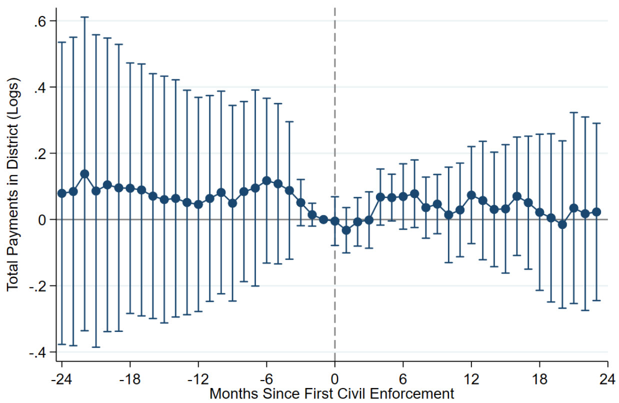
Table A15: Effect of Litigation on Ridership, Bordering Districts as Controls

	Civil		Criminal	
	(1) Total Ride Payments (Log)	(2) Total Rides (Log)	(3) Total Ride Payments (Log)	(4) Total Rides (Log)
Enforcement	0.0192	0.0312	-0.230^{+}	-0.268*
	(0.0727)	(0.0597)	(0.134)	(0.107)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	12.04	6.779	11.41	6.301
Observations	5160	5160	5256	5256

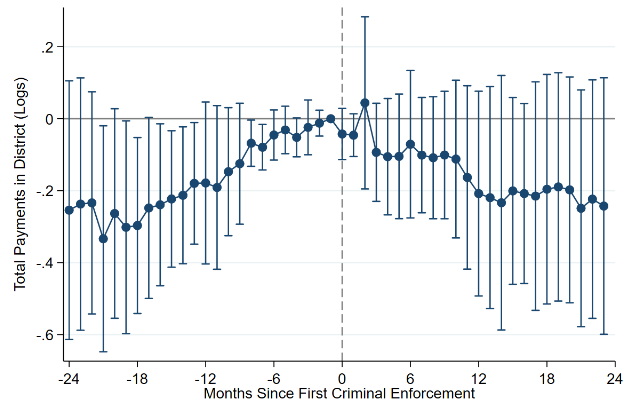
Notes: Estimates of β from equation (2). All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables are transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district–month. The sample is limited to districts subject to the relevant enforcement type and geographically bordering districts. 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% levels, respectively.

Figure A6: Effect of Litigation on Ridership, Event Study

(a) Civil Enforcement



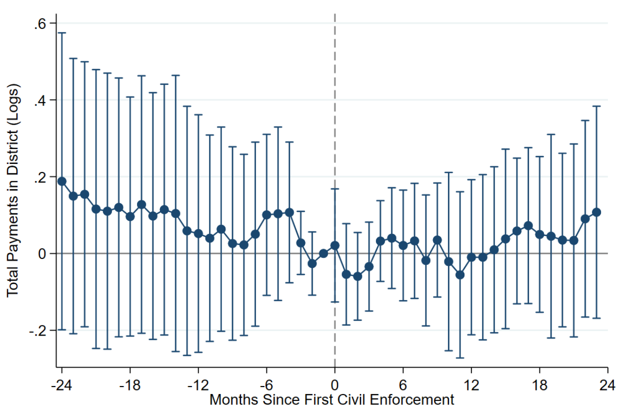
(b) Criminal Enforcement



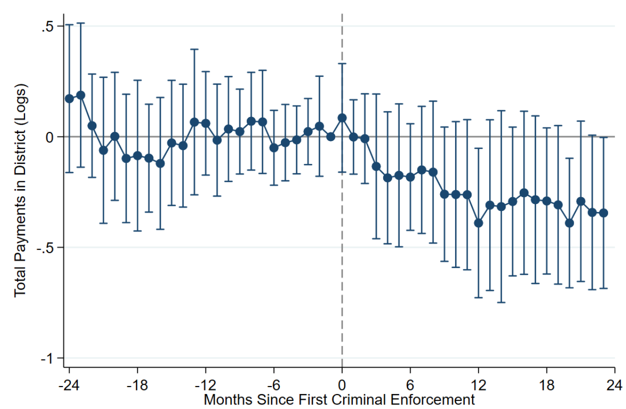
Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (12). Dependent variable is total payments for non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data transformed by adding 1 and taking the natural log. These data include rides from 2003–2017. An observation is a district–month. The sample is limited to districts subject to the relevant enforcement type. 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 represent pointwise 95% confidence intervals.

Figure A7: Effect of Litigation on Ridership, Bordering Districts as Controls

(a) Civil Enforcement



(b) Criminal Enforcement



Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is total payments for non-emergency 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 sample is limited to districts subject to the relevant enforcement type and geographically bordering districts. 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 represent pointwise 95% confidence intervals.

E Heterogeneity in Treatment Effects

In this appendix, we report results on potential dimensions of heterogeneity in the effect of prior authorization and criminal and civil litigation.

First, we demonstrate that the effects of the first and second waves of prior authorization were remarkably similar. This is an important point given that while the first-wave states were selected due to high utilization, the second-wave states (which as shown in Appendix C are very similar to control states) were chosen due to geographic proximity. To do this, we estimate

$$Y_{dt} = \sum_{e=-K}^{-2} \beta_e T_{dt}(e) + \beta \sum_{e=0}^{L} T_{dt}(e) + \sum_{e=-K}^{-2} \gamma_e T_{dt}(e) \times Het_d + \gamma \sum_{e=0}^{L} T_{dt}(e) \times Het_d + \alpha_d + \alpha_t + \Gamma X_{dt} + \varepsilon_{dt}, \tag{13}$$

where Het_d gives the district characteristic along which we allow for heterogeneity in the effect. In this case, it is an indicator for whether the district was first exposed to prior authorization in January 2015.

As shown by Table A16 and Figure A8, while the first wave of prior authorization had a larger effect on the level of spending and ridership than the second wave, the effect of each wave is qualitatively and statistically the same when the dependent variable is measured in logs. The states subject to the second wave of prior authorization were chosen for reasons exogenous to the level of fraud (their geographic proximity to the first-wave states), and in those regions prior authorization leads to the same percentage change in spending and utilization.

Next, we show that prior authorization reduced fraud regardless of whether a district had previously experienced litigation. As shown in Table A17 prior authorization was successful when the threat of litigation had been present and even in cases even when it had been realized, indicating that in this context, regulation was successful at eliminating much of the fraud that had proliferated when litigation was the only means of deterrence. That fraud rose under the threat of, and even under the active pursuit of litigation before being significantly curtailed by prior authorization, we feel comfortable with the paper's main point that increasing litigation was less effective than adding regulation at eliminating fraud.

Table A16: Effect of Prior Authorization by Wave

	(1) Total Ride Payments (Log)	(2) Total Ride Payments	(3) Total Rides (Log)	(4) Total Rides
Prior Authorization	-1.209***	-1751047.6+	-1.207***	-8886.1+
	(0.306)	(981301.3)	(0.261)	(4913.8)
Prior Auth. × Second Wave	0.144	1585860.6	0.468	8097.6
	(0.585)	(976344.7)	(0.333)	(4890.0)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	9.934	415286.7	5.357	2005.3
Observations	7272	7272	7272	7272

Notes: Estimates of β and γ from equation (13), where Het_d is an indicator for whether the district was first exposed to prior authorization in January 2015. All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables 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% levels, respectively.

Table A17: Effect of Prior Authorization by Litigation Status

	(1) Total Ride Payments (Log)	(2) Total Ride Payments	(3) Total Rides (Log)	(4) Total Rides	(5) Active Firms (Log)	(6) Active Firms
Prior Authorization	-1.316*	-115764.5*	-0.918***	-579.6*	-0.250***	-4.449*
	(0.581)	(46032.6)	(0.240)	(231.6)	(0.0672)	(1.700)
Prior Auth. × Litigation	0.401	-1295691.0+	0.0147	-6521.0+	-0.0780	-19.81+
	(0.602)	(706903.7)	(0.320)	(3560.7)	(0.124)	(10.64)
Year-Month FE	1	1	1	1	1	1
District FE	1	1	1	1	1	1
Dep. Var. Mean	9.934	415286.7	5.357	2005.3	2.152	17.23
Observations	7272	7272	7272	7272	6336	6336

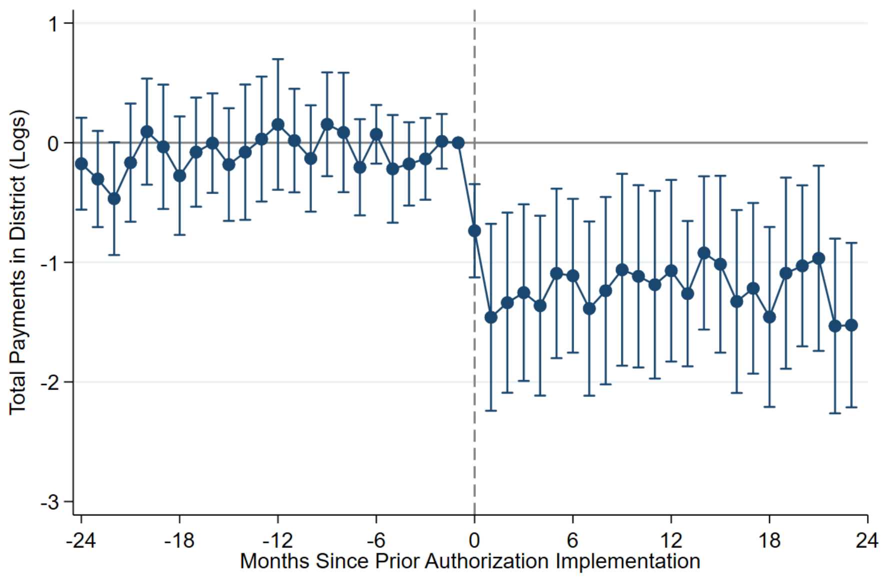
Notes: Estimates of β and γ from equation (13), where Het_d is an indicator for whether the district was ever subject to litigation. All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Dependent variables in columns (1), (3), and (5) 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% levels, respectively.

Next, we turn to potential dimensions of heterogeneity in the effect of litigation. In particular, we consider heterogeneity that may indicate endogenous enforcement decisions or timing. We find no evidence of such heterogeneity. For instance, we first note that the timing of litigation is uncorrelated with the effect of litigation, which is inconsistent with prosecutors picking the low-hanging fruit first and avoiding possibly ineffective litigation. Table A18 shows that there is no heterogeneity in the effect of litigation by whether the litigation occurred earlier or later in time.

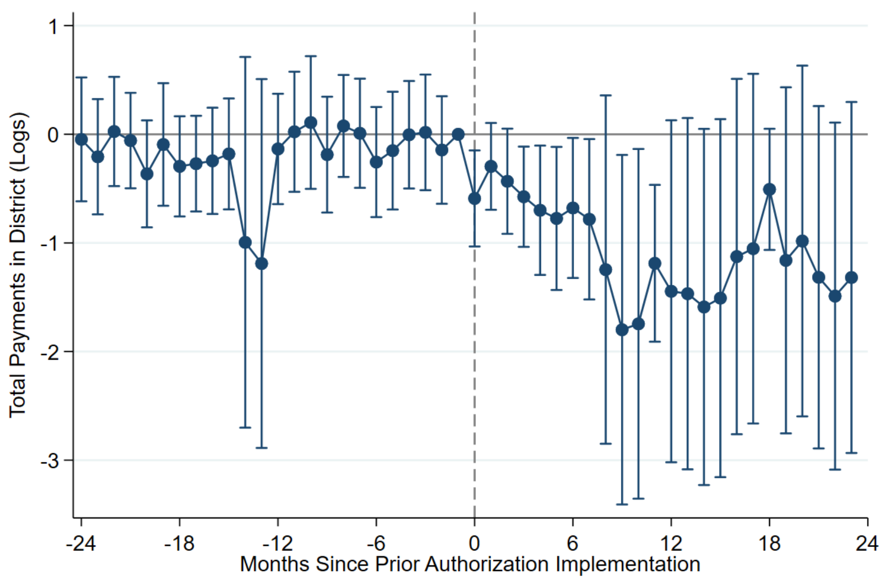
Second, the effect of litigation is not correlated with the baseline level of ridership or spending in the district prior to the litigation being realized. Table A19 shows that the effect of litigation

Figure A8: Effect of Prior Authorization by Wave

(a) Wave 1



(b) Wave 2



Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is total payments for non-emergency 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. In panel (a), the sample is limited to districts first subject to prior authorization in December 2014 or not exposed to prior authorization during our sample. In panel (b), the sample is limited to districts first subject to prior authorization in January 2015 or not exposed to prior authorization during our sample. Standard errors are clustered at the district level. Error bars represent pointwise 95% confidence intervals.

Table A18: Heterogeneity in Effect of Litigation by Enforcement Date

	Civil		Criminal	
	(1) Total Ride Payments (Log)	(2) Total Rides (Log)	(3) Total Ride Payments (Log)	(4) Total Rides (Log)
Enforcement	-0.0426	0.0255	-0.211*	-0.281**
	(0.110)	(0.0664)	(0.0983)	(0.0891)
Enforcement × Enforcement Date	0.000406	0.000401	-0.00390	-0.00429
	(0.00407)	(0.00224)	(0.00332)	(0.00303)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	9.221	4.835	9.354	4.928
Observations	14160	14160	14436	14436

Notes: Estimates of β and γ from equation (13), where Het_d is the number of months from the average date of litigation nationwide to the start of litigation in that district for the relevant type of litigation. All rides are non-emergency 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. Standard errors are clustered at the district level. $^+$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

does not differ by the ridership and spending in the district at the beginning of our sample. This lack of heterogeneity indicates a lack of strategic enforcement, which bolsters the argument that litigation timing is quasi-random.

Table A19: Heterogeneity in Effect of Litigation by Baseline Ridership and Spending

	Civil		Criminal	
	(1) Total Ride Payments (Log)	(2) Total Rides (Log)	(3) Total Ride Payments (Log)	(4) Total Rides (Log)
Enforcement	-0.0222	0.0372	-0.142	-0.219*
	(0.104)	(0.0595)	(0.0883)	(0.0851)
Enforcement × Baseline	0.0375	0.0276	-0.0284	-0.0206
	(0.0612)	(0.0391)	(0.0997)	(0.102)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	9.483	4.975	9.577	5.049
Observations	13620	13620	14028	14028

Notes: Estimates of β and γ from equation (13), where Het_d is the average value of the dependent variable in the district from 2003–2005, with the district-wide average normalized to zero. All rides are non-emergency 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. Standard errors are clustered at the district level. $^+$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

Next, in Table A20, we present evidence that when the DOJ litigates more cases in a district is not associated with greater deterrence effects. To do this, we estimate equation (2) allowing for treatment effect heterogeneity by various measures of "enforcement intensity": each distinct number of cases, whether there were multiple cases, and linearly in the number of cases. We find no statistically significant evidence of heterogeneity in the effect of criminal litigation by the number of cases litigated in the district, while we find some evidence that districts in which more civil cases were litigated saw a smaller decline in payments than those with fewer cases. These results suggest that the first case in a district has the largest deterrence effect, perhaps because it sends a stronger signal about the risk of being detected than subsequent cases do or indicates more vigilant law enforcement in the region. In addition, very few districts have multiple cases, with approximately 70% of the districts that have any criminal litigation having only one or two criminal lawsuits, while for districts subject to civil litigation, the corresponding number is almost 90%. The fact that criminal litigation shows the strongest impact from the first case indicates that, while the fraudulent behavior was always illegal, firms respond to cases actually being pursued.

Table A20: Heterogeneity in Effect of Litigation by Number of Cases

	Criminal			Civil		
	(1) Total Ride Payments (Log)	(2) Total Ride Payments (Log)	(3) Total Ride Payments (Log)	(4) Total Ride Payments (Log)	(5) Total Ride Payments (Log)	(6) Total Ride Payments (Log)
Enforcement	-0.403*	-0.296+	-0.403*	-0.0738	-0.226	-0.0736
	(0.163)	(0.161)	(0.163)	(0.125)	(0.214)	(0.124)
Enforcement × 2 Cases	0.495*			-0.0362		
	(0.209)			(0.234)		
Enforcement × 3 Cases	0.444+			0.355+		
	(0.254)			(0.194)		
Enforcement × 4 Cases	0.247					
	(0.261)					
Enforcement × 6 Cases	0.370+					
	(0.219)					
Enforcement × 10 Cases	0.240					
	(0.274)					
Enforcement × Intensity		0.0307			0.142	
		(0.0313)			(0.113)	
Enforcement × Intensity			0.381*			0.160
			(0.189)			(0.228)
Intensity Measure	Discrete	Linear	Multiple	Discrete	Linear	Multiple
Year-Month FE	1	1	1	1	1	1
District FE	1	1	1	1	1	1
Dep. Var. Mean	9.354	9.354	9.354	9.221	9.221	9.221
Observations	14436	14436	14436	14160	14160	14160

Notes: Estimates of β and γ from equation (13), where Het_d is a measure of the number of cases observed in the district. Specifications in columns (1) and (4) allow treatment effect heterogeneity by each level of cases observed in the data with the "Intensity" variable indicating two cases. Specifications in columns (2) and (5) allow treatment effect heterogeneity by whether there were multiple cases in the district. Specifications in columns (3) and (6) allow the treatment effect to varying linearly in the number of cases, with the "Enforcement" coefficient capturing the estimated effect of a district being subject to any litigation but having zero cases. All rides are non-emergency 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. Standard errors are clustered at the district level. +, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

Finally, similar to the prior authorization results in Table A17, we note that the effect of litigation also did not differ by whether the district would eventually be subject to prior authorization, as shown by Table A21.

Table A21: Effect of Litigation by Eventual Prior Authorization Status

	Civil		Criminal	
	(1) Total Ride Payments (Log)	(2) Total Rides (Log)	(3) Total Ride Payments (Log)	(4) Total Rides (Log)
Enforcement	-0.0460	0.0556	-0.232^{+}	-0.239*
	(0.106)	(0.0670)	(0.135)	(0.117)
Enforcement × Prior Auth.	0.0164	-0.149	0.0620	-0.123
	(0.313)	(0.172)	(0.220)	(0.212)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	9.221	4.835	9.354	4.928
Observations	14160	14160	14436	14436

Notes: Estimates of β and γ from equation (13), where Het_d is an indicator for whether the district is ever subject to prior authorization. All rides are non-emergency 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. Standard errors are clustered at the district level. $^{+}$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

F 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 A9 that our estimate of the large effect of prior authorization on rides is robust at the firm–month and patient–month levels using traditional TWFE methods. As a placebo test, we also show in Table A22 that prior authorization had no impact on the number of emergency rides. Next, Figure A10 shows the effect of prior authorization on claim denial rates at the firm level.

Table A22: Effect of Prior Authorization on Emergency Ambulance Spending

	(1) Payments for Emergency Rides (Log)	(2) Payments for Emergency Rides	(3) Total Emergency Rides (Log)	(4) Total Emergency Rides
Prior Authorization	-0.0164 (0.0445)	4412.0 (3648.9)	-0.000333 (0.0242)	11.53 (9.353)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	11.13	120302.6	5.289	327.0
Observations	7272	7272	7272	7272

Notes: Estimates of β from equation (2). All rides are emergency ambulance transports observed in the USRDS data. Dependent variables in columns (2) and (4) are transformed by adding 1 and taking the natural log. These data include rides from 2011–2017. 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% levels, respectively.

Table A23 shows what happens to riders in the month after their ambulance company exits the market by whether the firm exited before or coincident with the implementation of prior authorization. One potential 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 A23 shows what happens to riders in the month after their ambulance company exits the market. Compared to the patients in column (1) who rode with ambulance companies that exited before prior authorization (i.e., exits not induced by the anti-fraud regulation), those in column (2) 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 with another firm. That is, patients riding with ambulance companies that exited immediately after prior authorization were not more likely to miss a month of dialysis than a typical patient whose ambulance company exited before prior authorization. Taken together, these results suggest prior authorization for non-emergency ambulance rides did not adversely affect patients' health: patients continue receiving treatment at the same rate as before and do not see an increase in hospitalizations or mortality.

Table A23: Summary Statistics for Riders of Exiting Firms by Prior Authorization Status

	Period When Firm Exits	
	Pre-Prior Auth.	At Prior Auth.
Continues Riding	0.651	0.097
Is Treated without Riding	0.278	0.849
Dies This Month	0.029	0.029
Is Hospitalized This Month	0.023	0.010
Is Not Treated Next Month	0.019	0.015
Observations	835	517

Notes: The sample is limited to patients who 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. The "at prior authorization" period corresponds to one month before and after the implementation of prior authorization. Rows represent shares of patients in mutually exclusive categories of the patient's activity in the following month.

Finally, we present estimates of the paperwork costs of prior authorization. To do this, we follow Brot-Goldberg et al. (2022), a recent paper about prior authorization used to eliminate wasteful pharmaceutical prescriptions, and calibrate the paperwork costs for firms remaining in the market using estimates of the cost of prior authorization from Bukstein et al. (2006), Goldstein et al. (2010), for Affordable Quality Healthcare (2014), and Carlisle et al. (2020) along with the more extreme costs Brot-Goldberg et al. (2022) propose. We combine these possible costs per submission with the number of submissions reported by Centers for Medicare and Medicaid Services (2020a) for each year of the prior authorization program. We scale these to the district-month level for comparison with our estimates of the reductions in spending from prior authorization. Table A24 gives the range of potential paperwork costs for each year of the program. This table shows that even the under the most extreme assumptions on the potential costs of prior authorization, the paperwork costs are far outweighed by the savings to Medicare, which we estimate to be $736,000 per district-month.

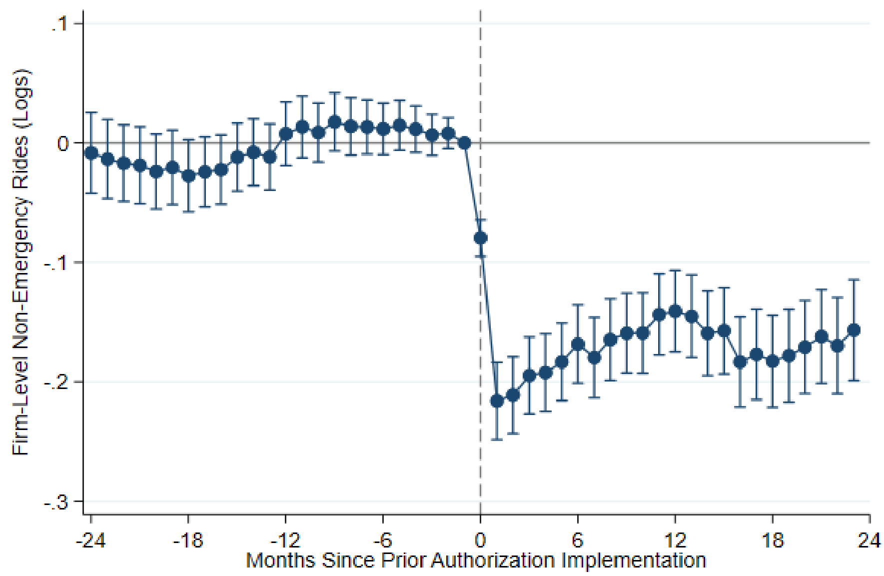
Table A24: Estimates of Paperwork Costs of Prior Authorization

	Year of Program				
Cost per Request	Year 1	Year 2	Year 3	Year 4	Year 5
$11.62	$3,478.06	$1,477.81	$1,564.12	$1,422.16	$1,405.12
$18.19	$5,444.57	$2,313.36	$2,448.48	$2,226.25	$2,199.58
$21.72	$6,501.16	$2,762.30	$2,923.63	$2,658.29	$2,626.43
$22.48	$6,728.64	$2,858.96	$3,025.93	$2,751.30	$2,718.33
$31.30	$9,368.61	$3,980.66	$4,213.15	$3,830.77	$3,784.87
$50	$14,965.83	$6,358.89	$6,730.28	$6,119.44	$6,046.11
$100	$29,931.67	$12,717.78	$13,460.56	$12,238.89	$12,092.22
$200	$59,863.33	$25,435.56	$26,921.11	$24,477.78	$24,184.44
Requests per District-Month	299.32	127.18	134.61	122.39	120.92

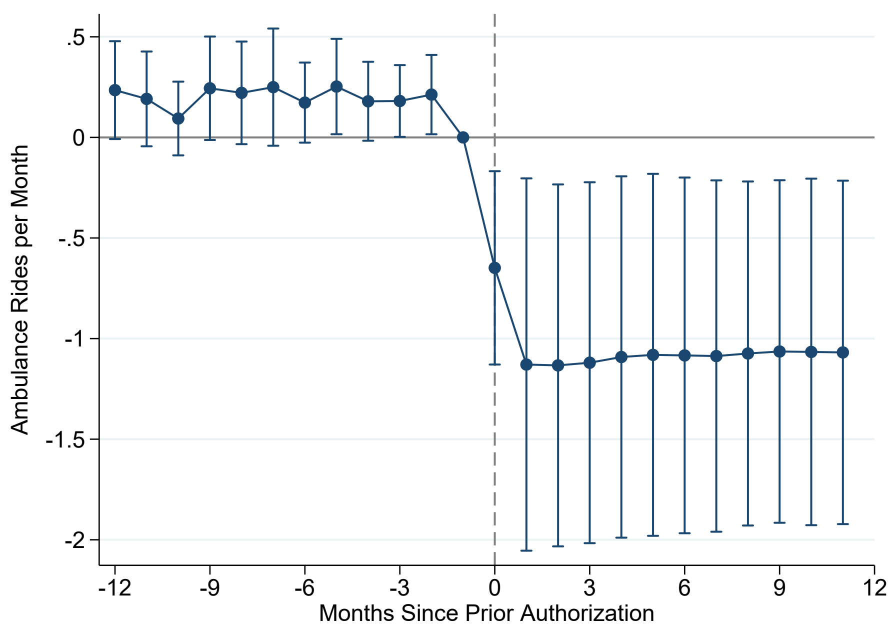
Notes: Estimates of the total monthly costs of submitting prior authorization requests per jurisdiction. Possible costs come from Bukstein et al. (2006), Goldstein et al. (2010), for Affordable Quality Healthcare (2014), Carlisle et al. (2020), and Brot-Goldberg et al. (2022). Number of requests come from Centers for Medicare and Medicaid Services (2020a).

Figure A9: Effect of Prior Authorization on Ridership

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



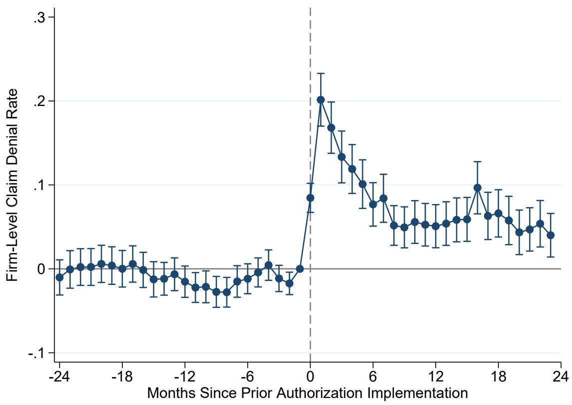
(b) Patient-Level Effect on Non-emergency Dialysis Rides



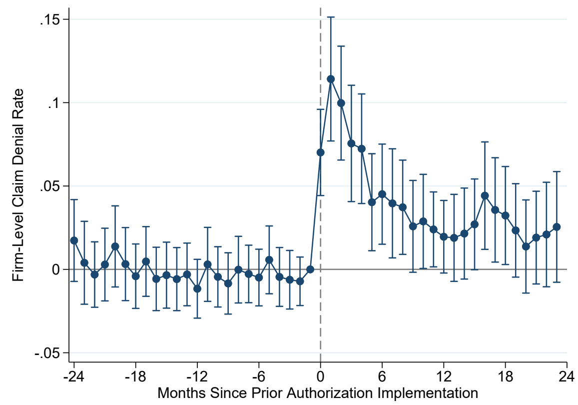
Notes: All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Error bars represent 95% confidence intervals. Panel (a) gives estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1) and includes rides from 2012–2017, where 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 β_e for $e \in [-12, 11]/\{-1\}$ from equation (3) and includes data from 2011–2017, where 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 district level.

Figure A10: Claim Denial Rates

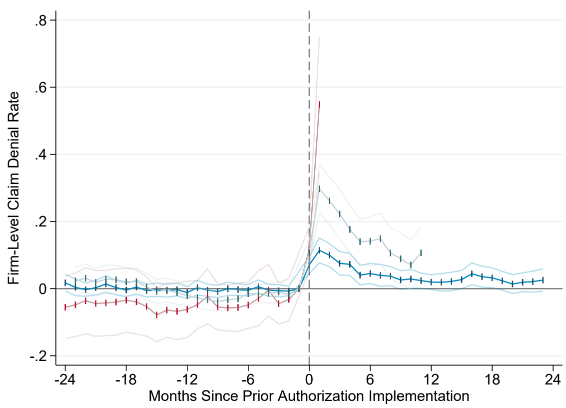
(a) All Firms



(b) Surviving Firms



(c) By Exit Date



Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). Dependent variable is the share of claims for non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data that are not paid any positive amount. These data include rides from 2011–2017. An observation is a district–month. Standard errors are clustered at the firm level. Error bars represent pointwise 95% confidence intervals. Panel (a) includes all firms, and panel (b) includes only firms that provide at least one ride in each of the 48 months in the sample. Panel (c) presents estimates for firms that provide rides in each of the 24 months in the pre-period and that continue providing rides in each of the 24 months in the post-period (blue line), permanently exit the dialysis market in the first two months of prior authorization (red line), or do not permanently exit in the first two months of prior authorization but do not provide rides for the entire post-period (green line).

G More Results on the Effects of Litigation

In this appendix, we present additional results on the effects of litigation that we refer to throughout the paper.

First, 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 districts bordering those subject to enforcement with those neither bordering districts subject to enforcement nor subject to enforcement themselves. Table A25 indicates that there is no detectable impact of civil or criminal enforcement on the total number of rides or payments in neighboring districts.

Table A25: 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	-0.0669	0.0324	-0.143	-0.0267
	(0.197)	(0.0881)	(0.199)	(0.104)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Dep. Var. Mean	7.793	3.976	8.450	4.403
Observations	7692	7692	9096	9096

Notes: Estimates of β from equation (2). All rides are non-emergency 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 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% levels, respectively.

Next, we demonstrate how the distribution of firms by their share of rides that were non-emergency dialysis rides changed following litigation. Figure A11 demonstrates that, unlike for prior authorization, there were no large changes in the distribution following litigation. It is important to note, though, that the total number of firms are much larger in the three years after litigation than before due to the uninterrupted growth in the number of firms. By contrast, prior authorization led to a large reduction in the number of active firms.

Tables A26 through A29 show the effects of criminal and civil enforcement actions on patient health outcomes. In parallel with the discussion about the health effects of prior authorization in Section 5.2, we estimate equation 4 using patient health outcomes as dependent variables and civil and criminal litigation as treatments. In the first two tables the only evidence of any health effects is a potentially small uptick in dialysis sessions received by patients each month, suggesting at minimum that patient health is not harmed by civil or criminal enforcement. Tables A28 and A29 repeat this exercise with a focus on frequent riders—those with at least 100 non-emergency ambulance rides to dialysis in the non-prior authorization regime—and confirm a lack of harm.

Table A26: Effect of Civil Enforcement on Adherence and Adverse Events

	(1) Dialysis Sessions	(2) Mortality	(3) All-Cause Hosp.	(4) Fluid Hosp.
Civil Enforcement	-0.0123	-0.000215	0.00333**	0.000736
	(0.0147)	(0.000639)	(0.00121)	(0.000784)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Pat/Fac Controls	1	1	1	1
Facility FE	1	1	1	1
R-squared	0.000439	0.00554	0.0174	0.00570
Dep. Var. Mean	12.17	0.0112	0.133	0.0134
Observations	24036101	24036101	24036101	24036101

Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2003–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and 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% levels, respectively.

Next, Figures A12, A13, A14, and A15 along with Tables A30 and A31, explore the potential impact of criminal and civil litigation on the selection of patients taking non-emergency rides to dialysis facilities. Overall, these show patient selection effects that are much smaller than what we find for prior authorization, though generally in the same direction.

Table A27: Effect of Criminal Enforcement on Adherence and Adverse Events

	(1) Dialysis Sessions	(2) Mortality	(3) All-Cause Hosp.	(4) Fluid Hosp.
Criminal Enforcement	0.0680^{+}	-0.0000251	-0.00243	-0.000166
	(0.0373)	(0.000725)	(0.00234)	(0.000603)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Pat/Fac Controls	1	1	1	1
Facility FE	1	1	1	1
R-squared	0.000446	0.00547	0.0175	0.00577
Dep. Var. Mean	12.17	0.0111	0.134	0.0136
Observations	26173113	26173113	26173113	26173113

Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2003–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and 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% levels, respectively.

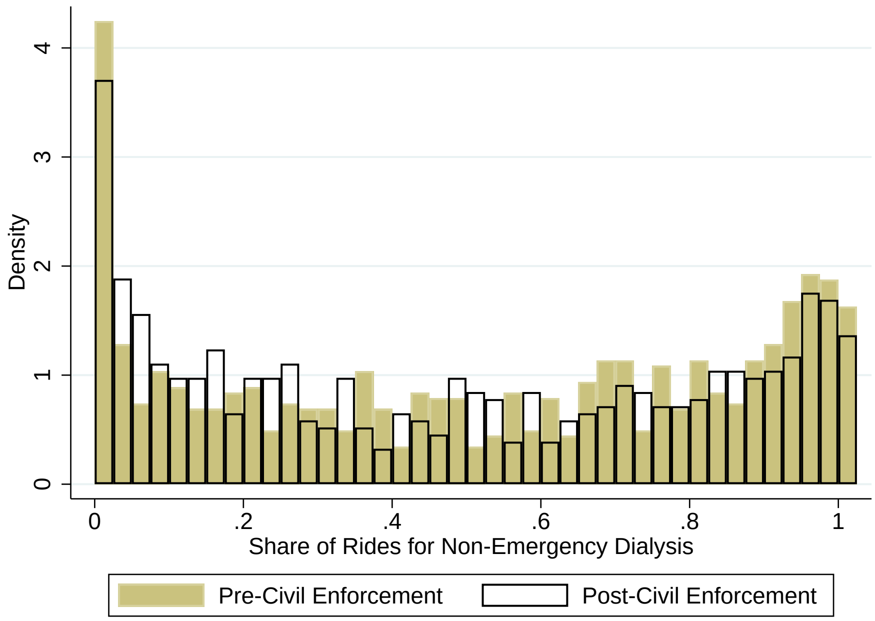
Table A28: Effect of Civil Enforcement on Frequent Riders

	(1) Dialysis Sessions	(2) Mortality	(3) All-Cause Hosp.	(4) Fluid Hosp.
Civil Enforcement	-0.187^{+}	0.000965	0.00976^{*}	0.00183
	(0.112)	(0.00160)	(0.00431)	(0.00237)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Pat/Fac Controls	1	1	1	1
Facility FE	1	1	1	1
R-squared	0.00138	0.00858	0.0221	0.0103
Dep. Var. Mean	12.10	0.0102	0.178	0.0165
Observations	1599317	1599317	1599317	1599317

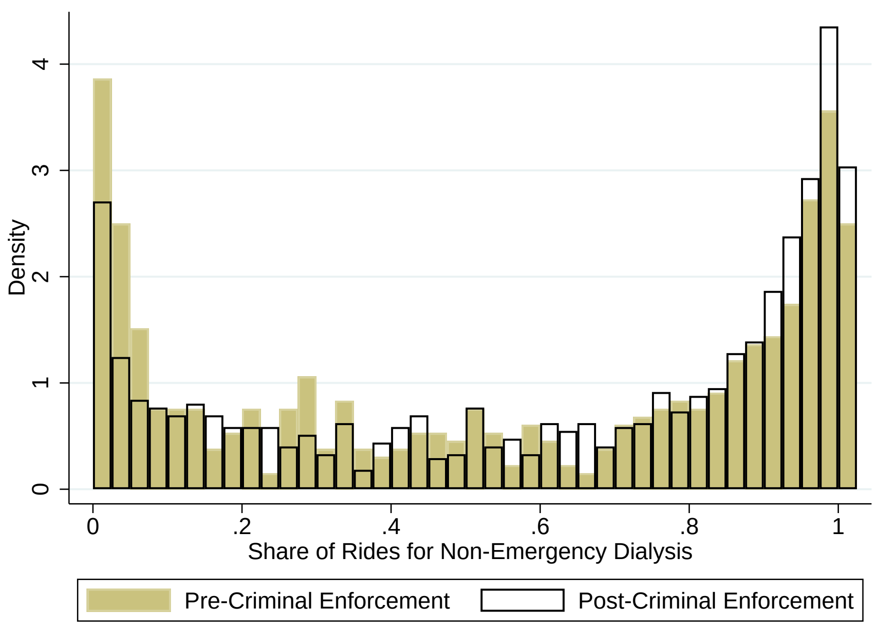
Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2003–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and facility characteristics including chain ownership status, demographic characteristics 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 who took at least 100 non-emergency ambulance rides to dialysis under the pre-litigation regime. Standard errors clustered at the district level are given in parentheses. $^{+}$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

Figure A11: Effect of Litigation on Firm Specialization

(a) Civil Enforcement



(b) Criminal Enforcement



Notes: Figures give the distribution of ambulance firms that served dialysis patients from in the three years before and after criminal and civil litigation in districts subject to each form of enforcement. A firm's pre-litigation non-emergency share is determined by the share of total rides given by the firm in the 36 months before the start of litigation in that district that were non-emergency rides between a dialysis treatment facility and a patient's residence. The post-litigation share is the same share for the 36 months following the start of litigation. Firms that gave no non-emergency dialysis rides in the relevant period are excluded.

Table A29: Effect of Criminal Enforcement on Frequent Riders

	(1) Dialysis Sessions	(2) Mortality	(3) All-Cause Hosp.	(4) Fluid Hosp.
Criminal Enforcement	-0.0568	-0.00418	0.0413	-0.0157
	(0.531)	(0.0102)	(0.0330)	(0.0134)
Year-Month FE	1	1	1	1
District FE	1	1	1	1
Pat/Fac Controls	1	1	1	1
Facility FE	1	1	1	1
R-squared	0.00156	0.00755	0.0224	0.0102
Dep. Var. Mean	12.07	0.00975	0.173	0.0157
Observations	1781027	1781027	1781027	1781027

Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2003–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and 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 who took at least 100 non-emergency ambulance rides to dialysis under the pre-litigation regime. Standard errors clustered at the district level are given in parentheses. $^{+}$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

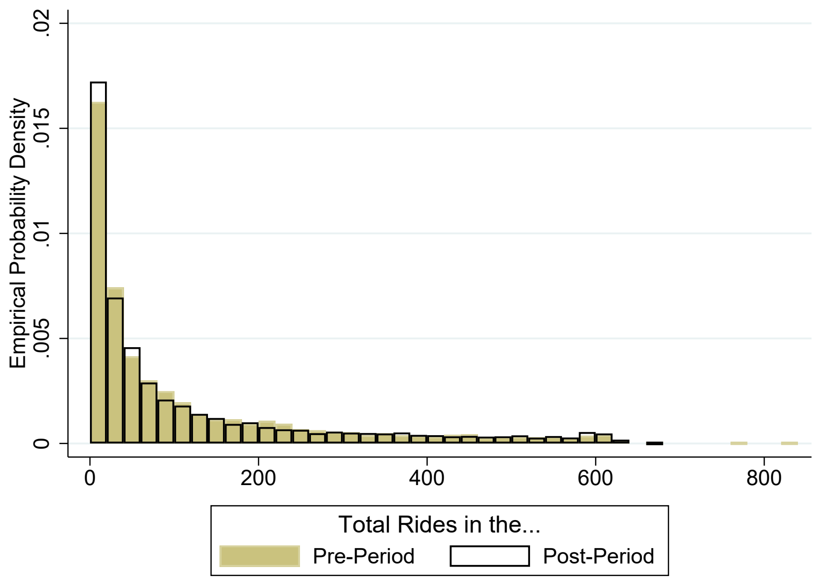
Table A30: Effect of Civil Enforcement on Patient Selection

	(1) Rides Next Month	(2) Hospitalizations	(3) Mortality
Civil Enforcement	0.0136	-0.00485	0.00628*
	(0.0111)	(0.00977)	(0.00270)
Year-Month FE	1	1	1
District FE	1	1	1
Pat/Fac Controls	1	1	1
Facility FE	1	1	1
R-squared	0.0968	0.0420	0.0248
Dep. Var. Mean	0.831	0.268	0.0381
Observations	885681	885681	885681

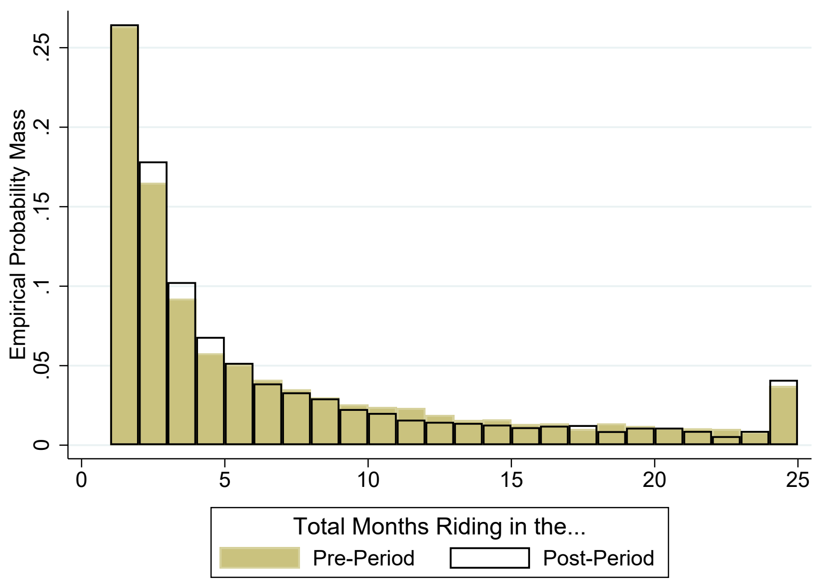
Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2003–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and facility characteristics including chain ownership status, demographic characteristics of the ZIP code, and whether the facility is freestanding or hospital based. The dependent variable in column (1) is an indicator for whether the patient rides in the following month. The dependent variable in column (2) is an indicator for whether the patient is hospitalized in the same month in which he or she is observed to be riding. The dependent variable in column (3) is an indicator for whether a patient dies in the same month that he or she is observed to be riding. Sample is limited to patient–months in which the patient receives at least one non-emergency 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% levels, respectively.

Figure A12: Histogram of Ridership Among Riders - Civil Enforcement

(a) Total Rides Taken



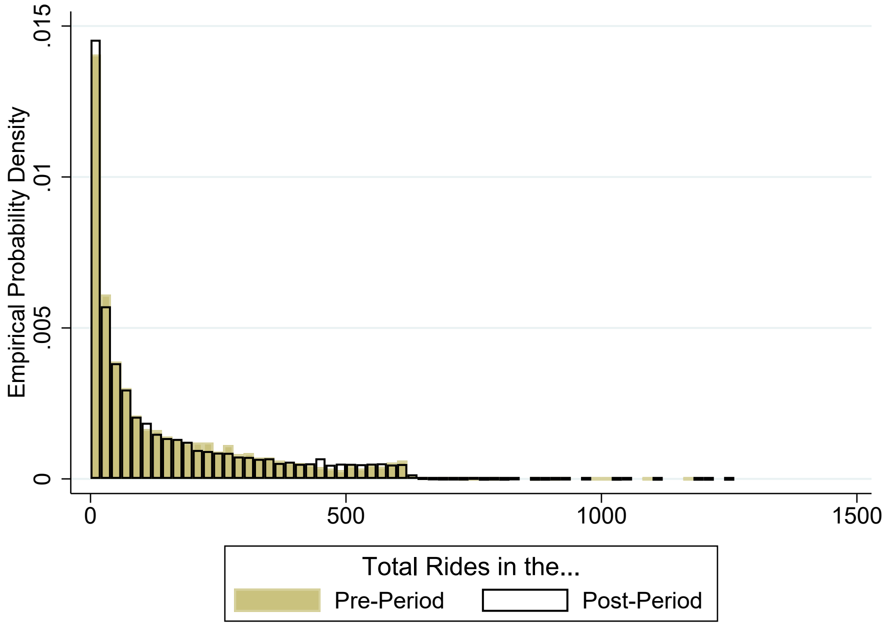
(b) Months with at Least One Ride Taken



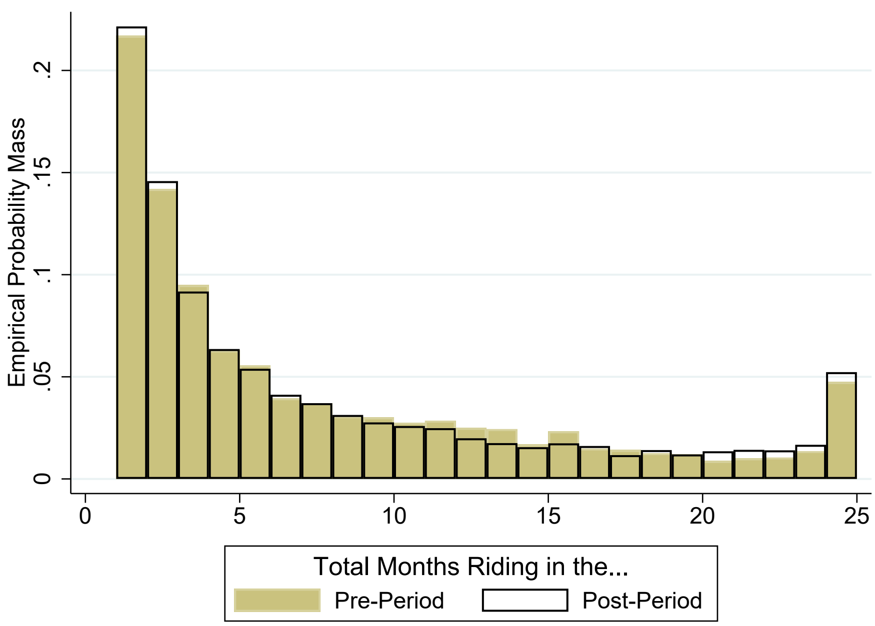
Notes: Panel (a) gives histograms of total rides taken by patients in districts subject to civil enforcement in the 24 months before and after the first complaint in the district. Panel (b) gives analogous histograms for the total number of months in which the patient takes at least one ride. All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the 20% sample of all Medicare beneficiaries.

Figure A13: Histogram of Ridership Among Riders - Criminal Enforcement

(a) Total Rides Taken



(b) Months with at Least One Ride Taken



Notes: Panel (a) gives histograms of total rides taken by patients in districts subject to criminal enforcement in the 24 months before and after the first indictment in the district. Panel (b) gives analogous histograms for the total number of months in which the patient takes at least one ride. All rides are non-emergency basic life support rides between a dialysis facility and a patient's home observed in the 20% sample of all Medicare beneficiaries.

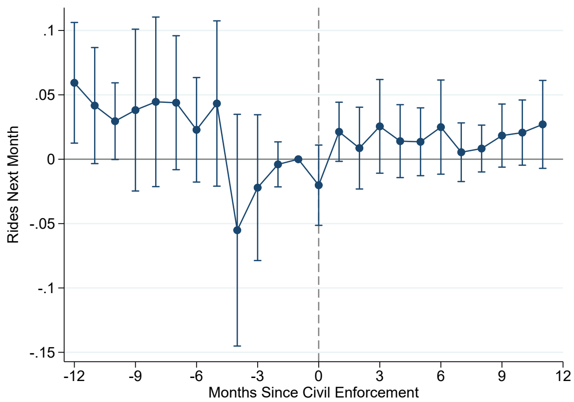
Table A31: Effect of Criminal Enforcement on Patient Selection

	(1) Rides Next Month	(2) Hospitalizations	(3) Mortality
Criminal Enforcement	-0.00802 (0.00717)	0.00538 (0.00595)	0.00549 (0.00406)
Year-Month FE	1	1	1
District FE	1	1	1
Pat/Fac Controls	1	1	1
Facility FE	1	1	1
R-squared	0.0955	0.0414	0.0232
Dep. Var. Mean	0.834	0.266	0.0379
Observations	1033299	1033299	1033299

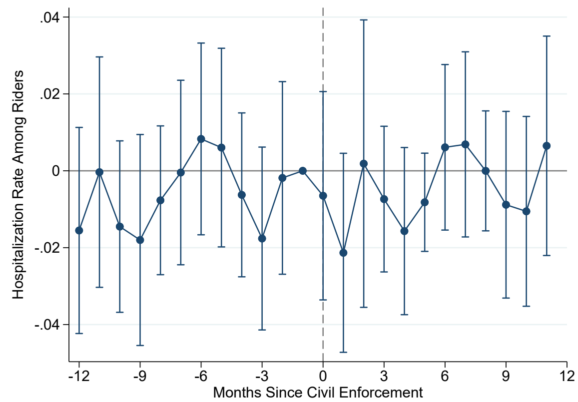
Notes: Table gives estimates of β from equation (4) at the patient–month level. Data are from 2003–2017. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and facility characteristics including chain ownership status, demographic characteristics of the ZIP code, and whether the facility is freestanding or hospital based. The dependent variable in column (1) is an indicator for whether the patient rides in the following month. The dependent variable in column (2) is an indicator for whether the patient is hospitalized in the same month in which he or she is observed to be riding. The dependent variable in column (3) is an indicator for whether a patient dies in the same month that he or she is observed to be riding. Sample is limited to patient–months in which the patient receives at least one non-emergency 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% levels, respectively.

Figure A14: Effect of Civil Enforcement on Patient Selection

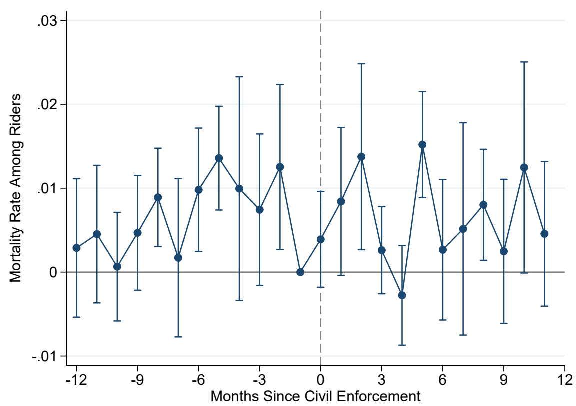
(a) Share of Riders Riding Next Month



(b) Hospitalization Rate Among Riders



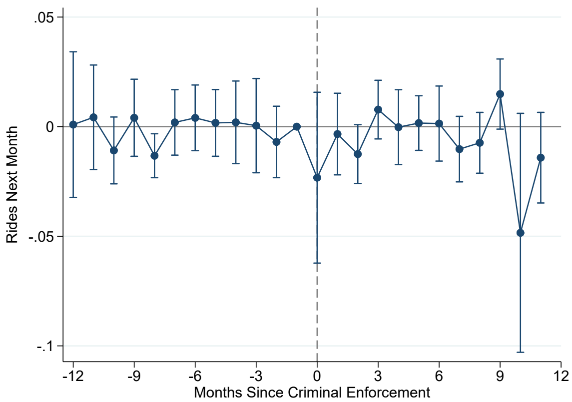
(c) Mortality Rate Among Riders



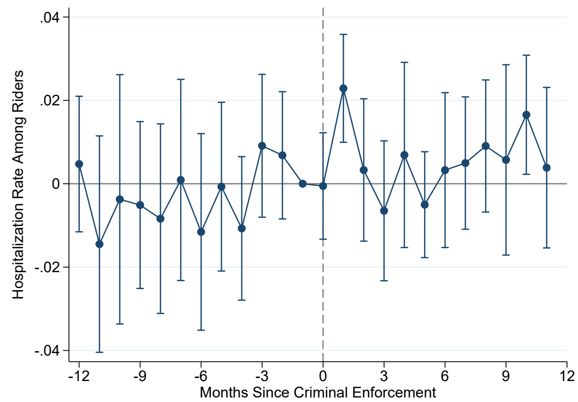
Notes: Estimates of β_e for $e \in [-12, 11]/\{-1\}$ from equation (3). These data include rides from 2003–2017. An observation is a patient–month. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and facility characteristics including chain 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-emergency dialysis ambulance ride. Standard errors are clustered at the district level. Error bars represent pointwise 95% confidence intervals.

Figure A15: Effect of Criminal Enforcement on Patient Selection

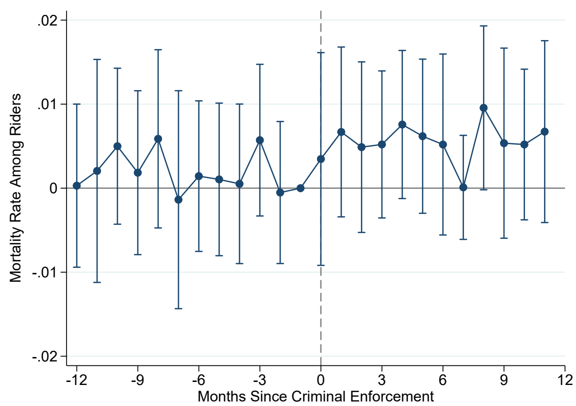
(a) Share of Riders Riding Next Month



(b) Hospitalization Rate Among Riders



(c) Mortality Rate Among Riders



Notes: Estimates of β_e for $e \in [-12, 11]/\{-1\}$ from equation (3). These data include rides from 2003–2017. An observation is a patient–month. Controls include incident patient characteristics, age, and tenure on dialysis as well as facility fixed effects and 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-emergency dialysis ambulance ride. Standard errors are clustered at the district level. Error bars represent pointwise 95% confidence intervals.

H Direct Comparison of Regulation and Litigation

In this appendix, we present further evidence of the difference in effectiveness of the prior authorization regulation and criminal and civil litigation by directly comparing the effects of each treatment. First, we recreate Figure 5 for the number of rides and active firms. Figure A16 presents these results and makes clear the difference in magnitudes of the effects of each treatment.

We also compare the estimated effect of each intervention more formally. To conduct this exercise, we perform a Z-test of the difference between the coefficients on civil litigation, criminal litigation, and prior authorization from their respective regressions. Table A32 reports p-values for the difference in estimated effects of each treatment. The results indicate that the effects of criminal litigation, civil litigation, and prior authorization are statistically significantly different from one another. We can reject that criminal litigation and prior authorization had the same effect on total ride payments at the 5% level, and on total rides at the 1% level.

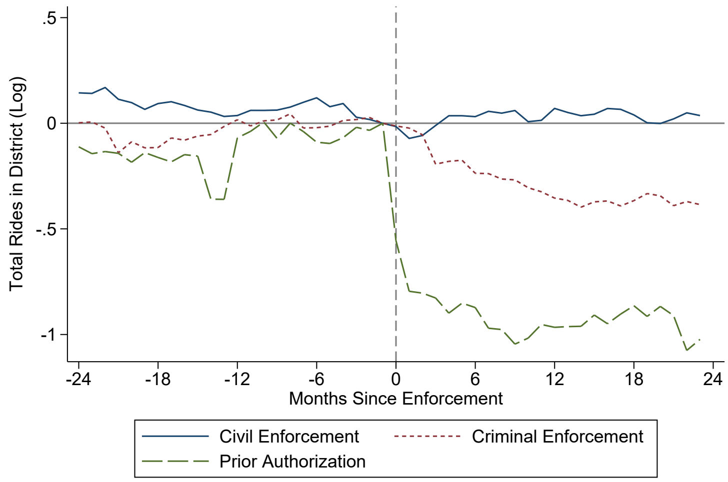
Table A32: P-Value of Difference in Effect of Treatment

	Civil vs. Criminal	Civil vs. Prior Auth.	Criminal vs. Prior Auth.
Total Ride Payments (Log)	0.226	0.002	0.017
Total Rides (Log)	0.005	0.000	0.001
Active Firms (Log)	0.475	0.000	0.014

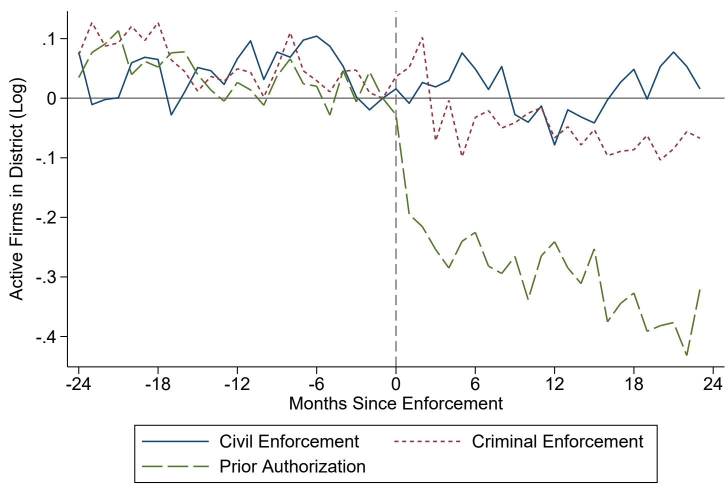
Notes: P-values of the difference in the estimates of β from equation (2) using a Z-test. The interventions being compared are given by the column title, while the dependent variable is given in the row title. P-values are calculated using standard errors clustered at the district level.

Figure A16: Comparison of Effects of Regulation and Litigation

(a) Total Rides



(b) Active Firms



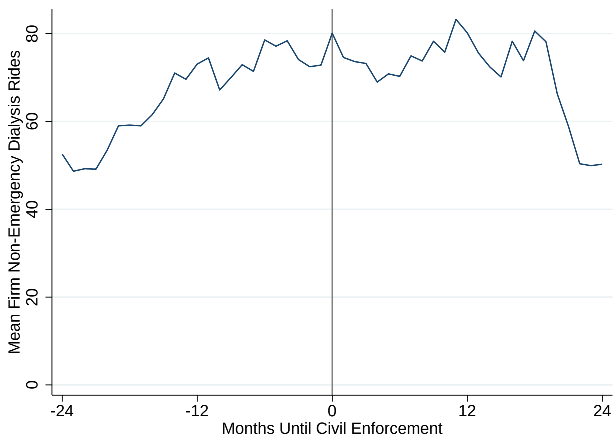
Notes: Estimates of β_e for $e \in [-24, 23]/\{-1\}$ from equation (1). In panel (a), the data include rides from 2003–2017, and the dependent variable is total non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data transformed by adding 1 and taking the natural log. In panel (b), the dependent variable is the number of firms observed supplying at least one such ride, similarly transformed, where the estimates of the effect of prior authorization rely on data from 2012–2017 while the estimates of the effect of civil and criminal litigation rely on data from a 20% sample of all Medicare beneficiaries from 2007–2019. An observation is a district–month. The treatment date is the earliest enforcement action of the relevant type in the district.

I Firms Subject to Enforcement

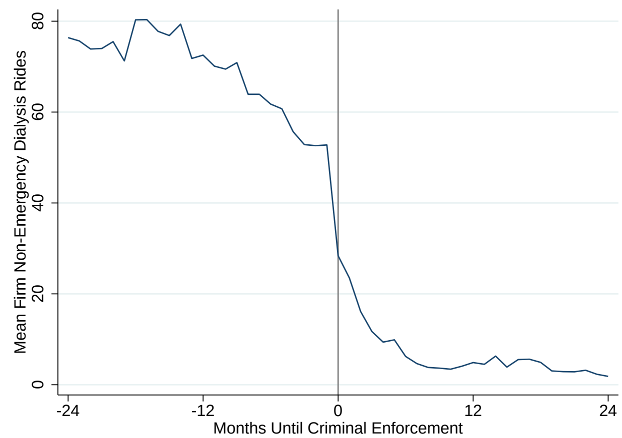
In this appendix, we highlight the effect of enforcement on the firms subject to it. First, we show that criminal and civil litigation had different effects on the firms subject to enforcement that mirror their impacts on the wider market, as shown in Figure A17.

Figure A17: Estimates of Incapacitation Effect

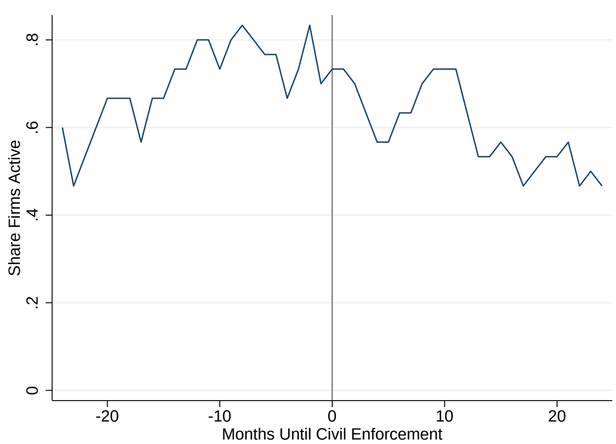
(a) Effect of Civil Enforcement on Firm Rides



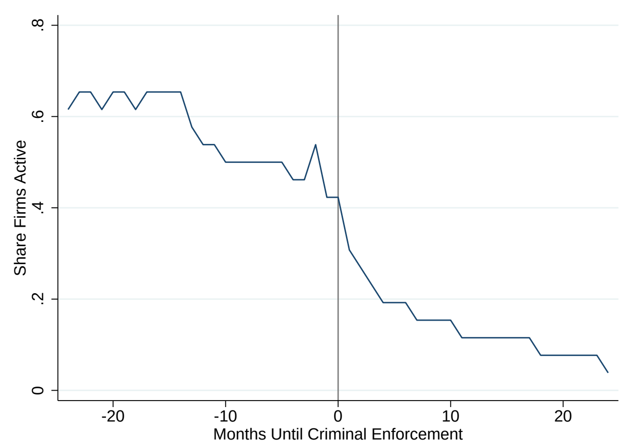
(b) Effect of Criminal Enforcement on Firm Rides



(c) Effect of Civil Enforcement on Firm Active Status



(d) Effect of Criminal Enforcement on Firm Active Status



Notes: Outcomes for firms subject to civil or criminal enforcement in the 24 months before and after complaint or indictment date. Panels (a) and (b) present the average monthly number of rides given, while panels (c) and (d) report the probability of giving at least one ride in the month. These data come from a 20% sample of all Medicare beneficiaries and include rides from 2007–2019. An observation is a firm–month.

One hypothesized reason why litigation had a limited effect is that many firms participating in fraudulent activity can quickly exit with minimal loss when they perceive the costs of the fraud to be increasing. This fly-by-night character of fraudulent firms complicates both fraud detection and resource recovery. To understand whether this phenomenon contributes to the relative effectiveness of prior authorization, we extend our analysis by showing that the increased likelihood of exit is especially pronounced among firms with Medicare revenue concentrated in non-emergency ambulance services whereas firms with substantial revenue streams from services beyond non-emergency ambulance services were less likely to exit the non-emergency market.

To show this, we combine our firm-level data on activity in the non-emergency dialysis market with publicly available data on firms' revenues from all Medicare patients, available at the firm–state–year level.[31] We then classify firms that gave at least one non-emergency dialysis ride in 2014 into quartiles by their Medicare payments from sources other than provision of non-emergency rides to dialysis patients. Finally, we estimate a triple-difference specification comparing the change in various outcomes following prior authorization by the outside revenue of the firm. More explicitly, we estimate

$$\begin{aligned} (14) \qquad Y_{jsy} &= \gamma_1 Post_y + \alpha_{1,s} + \beta_1 Treat_{js} \times Post_y \\ &+ \sum_{q \in \{2,\ldots,4\}} Q(q)_j \times (\beta_q Treat_{js} \times Post_y + \gamma_q Post_y + \alpha_{q,s}) + \varepsilon_{jsy}, \end{aligned}$$

where Y_{jst} is a firm-level outcome for firm j in state s in year y. $Post_y$ is an indicator for the observation being for the year 2015, $Treat_{js}$ is an indicator for the firm being located in a state subject to prior authorization in 2015, and α_s is a series of state fixed effects. $Q(q)_j$ is a series of indicators for firm j having 2014 revenue from activities other than providing non-emergency rides to dialysis patients in quartile q. The first coefficient of interest is β_1, which reports the differential change from 2014 to 2015 for first-quartile firms in states exposed to prior authorization relative to those in other states. The other coefficients of interest are β_2, β_3, and β_4, which report the differential change for second-, third-, and fourth-quartile firms relative to

[31]These data can be found at `https://data.cms.gov/provider-summary-by-type-of-service/medicare-physician-other-practitioners/medicare-physician-other-practitioners-by-provider-and-service/data`.

the differential change reported by β_1.

Table A33 reports these coefficients. Firms with the least revenue from non-dialysis activities experienced large reductions in revenue and increases in the likelihood of exit, relative to those with less exposure to the prior authorization–targeted activity. Among these fly-by-night firms, almost 35 percentage points more exited the non-emergency dialysis market, and almost 40 percentage points more completely shut down relative to firms with the most revenue from other sources. Among the firms that did not exit, their total revenue decreased by 68%, and their dialysis revenue fell by 91% relative to the year-to-year change experienced by firms less exposed to prior authorization.

Firms with substantial non-dialysis revenues were still affected by prior authorization, but to a much lesser degree. These results indicate that while all firms were affected by prior authorization, it most negatively affected firms without substantial revenue streams beyond non-emergency ambulance services. This is consistent with the regulation sweeping out fly-by-night firms that can easily start up and shut down to provide only ambulance taxi services.

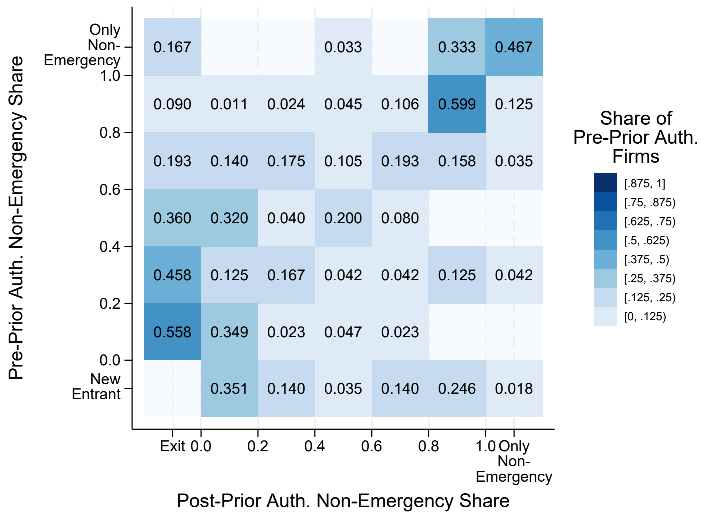
Table A33: Effect of Prior Authorization by Outside Revenue

	(1) Total Revenue (Log)	(2) Non-Emergency Dialysis Revenue (Log)	(3) Exits Non-Emergency Dialysis Market	(4) Completely Exits
Prior Authorization	-1.140***	-2.358***	0.393***	0.431***
	(0.219)	(0.434)	(0.0395)	(0.0524)
Prior Auth. × Size Quartile 2	0.351*	0.746**	-0.154*	-0.224**
	(0.170)	(0.261)	(0.0655)	(0.0748)
Prior Auth. × Size Quartile 3	0.111	0.632	-0.241*	-0.327***
	(0.331)	(0.442)	(0.0956)	(0.0598)
Prior Auth. × Size Quartile 4	0.810**	1.418***	-0.342***	-0.396***
	(0.238)	(0.359)	(0.0643)	(0.0506)
Quartile-by-State FE	1	1	1	1
Quartile-by-Year FE	1	1	1	1
Dep. Var. Mean	13.38	11.01	0.121	0.0571
Observations	3199	2975	3396	3396

Notes: Estimates of $\beta_1, \ldots, \beta_4$ from equation (14). Quartiles are based on the difference between a firm's 2014 total Medicare revenue and revenue from providing non-emergency basic life support rides between a dialysis facility and a patient's home observed in the USRDS data. Exit from the non-emergency dialysis market indicates that we observe no non-emergency rides to dialysis beneficiaries in 2015, while complete exit indicates that we observe no Medicare revenue in the public use data. Years included in the data are 2014 and 2015. An observation is a firm–state–year. The sample is limited to firms that provided at least one non-emergency dialysis ride in 2014. Standard errors are clustered at the state level. $^{+}$, *, ** and *** indicate significance at the 10%, 5%, 1% and 0.1% levels, respectively.

Related evidence is that, aside from leading fly-by-night firms to shut down, prior authorization led to specialization, as shown in Figure A18. This figure focuses only on rides given to dialysis patients and shows that prior authorization led firms for which non-emergency rides constituted a small share of their revenue to exit this segment of the market while those that had previously focused on providing non-emergency ambulance rides to dialysis beneficiaries specialized in this service even further.

Figure A18: Change in Distribution of Firms by Initial Share of Non-emergency Rides



Notes: Figure presents the share of firms with pre–prior authorization non-emergency 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's providing no non-emergency dialysis rides in the relevant period while non-emergency-only firms performed no emergency or non-dialysis non-emergency rides for dialysis beneficiaries.

J Model Details

In this appendix, we give additional details on the theoretical implications and our empirical calibration of the model presented in Section 6.

J.1 Theoretical Implications

We begin by demonstrating that limited liability reduced the effectiveness of civil litigation relative to criminal litigation. Note that without limited liability, the firm commits fraud if and only if

$$(1 - 3P_{Crim} - 3P_{Civil})R(Reg) > C(Reg) + P_{Crim}J.$$

If the heterogeneity across firms is in the level of assets, then either all firms would commit fraud or no firms without limited liability would commit fraud. If the heterogeneity is in the cost of jail J, then the share of firms that do not face limited liability that commit fraud would be given by

$$F_{J|Assets \geq 3R(Reg)}\left(\frac{(1 - 3P_{Crim} - 3P_{Civil})R(Reg) - C(Reg)}{P_{Crim}}\right),$$

where $F_{J|Assets \geq 3R(Reg)}$ is the distribution of jail costs conditional on having no limited liability.

With limited liability, the firm commits fraud if and only if

$$R(Reg) - C(Reg) > (P_{Crim} + P_{Civil})Assets + P_{Crim}J.$$

With homogeneous jail costs, the share of firms facing limited liability that commit fraud is given by

$$\frac{F_{Assets}\left(\frac{R(Reg) - C(Reg) - P_{Crim}J}{P_{Crim} + P_{Civil}}\right)}{F_{Assets}(3R(Reg))},$$

where F_{Assets} is the distribution of firm assets. With homogeneous assets, the share of firms facing limited liability that commit fraud is given by

$$F_{J|Assets<3R(Reg)}\left(\frac{R(Reg)-C(Reg)-(P_{Crim}+P_{Civil})Assets}{P_{Crim}}\right),$$

where $F_{J|Assets<3R(Reg)}$ is the distribution of jail costs conditional on facing limited liability.

Suppose that the costs of jail are homogeneous and, in light of our evidence that all prosecuted firms faced limited liability, that only firms facing limited liability commit fraud. Then the share of firms that commit fraud is given by $F_{Assets}(\Xi)$ where

$$\Xi \equiv \frac{R(Reg)-C(Reg)-P_{Crim}J}{P_{Crim}+P_{Civil}}$$

is the highest level of assets for which a firm will commit fraud.

We see that this threshold is decreasing in civil enforcement and even more strongly decreasing in criminal enforcement:

$$\frac{\partial \Xi}{\partial P_{Civ}} = -\frac{R(Reg)-C(Reg)-P_{Crim}J}{(P_{Crim}+P_{Civil})^2} < 0$$

$$\frac{\partial \Xi}{\partial P_{Crim}} = -\frac{R(Reg)-C(Reg)+P_{Civil}J}{(P_{Crim}+P_{Civil})^2} < \frac{\partial \Xi}{\partial P_{Civ}} < 0$$

Furthermore, this threshold is also decreasing in prior authorization:

$$\frac{\partial \Xi}{\partial Reg} = \frac{\frac{\partial R}{\partial Reg}-\frac{\partial C}{\partial Reg}}{P_{Crim}+P_{Civil}} < 0.$$

One minor complication is that imposing the regulation also lowers the threshold for facing limited liability because $3R(Reg)$ is decreasing in regulation. However, under the assumption that without limited liability, firms do not commit fraud, this change does not affect the share of firms committing fraud. Furthermore, the reduction in Ξ is greater than the reduction in $3R(Reg)$, meaning that all firms with assets below Ξ continue to face limited liability. We can see this by noting that $\frac{\partial \Xi}{\partial Reg} < \frac{\partial 3R(Reg)}{\partial Reg}$ if and only if

$$\frac{\partial C}{\partial Reg} > (1 - 3P_{Crim} - 3P_{Civil})\frac{\partial R}{\partial Reg}.$$

The right-hand side is positive and the left-hand side is negative because $1 - 3P_{Crim} - 3P_{Civil}$ is greater than $\frac{C(Reg)+P_{Crim}J}{R(Reg)}$, which is positive, by the assumption that firms not facing limited liability do not commit fraud in the absence of prior authorization.

J.2 Calibration

Next, we explain in detail how we arrive at our estimates for the parameter values discussed in Section 6.

- F_{Civ} - We are able to determine recovery amounts for 27 cases. Across these cases, the average amount recovered to date is \$1,158,241.92, which is 51% of the total amount owed in these cases. Notably, for 10 cases the entire amount owed was paid, while for 11 cases less than 20% of owed amount has been recovered, indicating that for some firms, liability is extremely limited.

- P_{Crim} - From 2007–2014, there were 25 cases against 26–28 firms (by name or NPIs, respectively). We estimate that during this period there were (approximately) 4,598 firms active, which we arrive at by adjusting the 20% sample of claims to account for the likelihood of failing to observe a firm. We do so by noting that, for each firm that is observed serving n patients in the 20% sample, the expected number of unobserved firms of that size is given by $(1 - 0.2)^n$. A reasonable estimate of the share of firms that were fraudulent is the share that exit after prior authorization, or 25% (from Table 6). This results in an estimated number of fraudulent firms of 1,150. Dividing the number of firms subject to litigation (using the NPI as the relevant number of firms) by this estimate of the number of fraudulent firms results in a probability of detection P_{Crim} of 2.4%.

- P_{Civ} - From 2007–2014, there were 12 cases against 44–45 firms (by NPI or name, respectively). During this period, we estimate that there were approximately 1,150 fraudulent firms active, implying a probability of detection P_{Civ} of 3.8%.

- $C(Reg)$ - While we do not know costs without prior authorization, we can calibrate their increase using the paperwork costs implied by previous estimates. These range from \$11.62 to \$31.30 per submission, or up to \$89.43 per successful submission if 65% of prior authorization requests are rejected, as was the case in 2015 (Centers for Medicare and Medicaid Services, 2020a). In our data from 2007 to the start of prior authorization, the average firm in our data serves (approximately) 39 patients over their entire tenure, implying a total cost of prior authorization of up to \$3,488 per firm. Note that this calculation speaks to only a small part of the costs of operating a fraudulent firm ("ambulances," fraudster time, etc.). These should not change in response to enforcement, but they are important for understanding why litigation is insufficient at baseline.

- $R(Reg)$ - From 2007–2017, we observe 65 firms subject to litigation in the NBER data. The total payments to these firms were \$69,912,034.63 (in the 20% sample). This means that the payments per firm were, on average, \$5,377,848.82. Note that this may be inflated because these are the firms that were caught (which may have received unusually high payments) or deflated because these firms were caught and so incapacitated from generating as much fraudulent revenue as they expected. This revenue changed under prior authorization because of the change in the probability a claim is rejected. The claim denial rate in the year before prior authorization implementation in the wave 1 states was 5.73% before increasing to 22.68% in January 2015 (see Figure 10). Limiting the sample to firms that exited at the implementation of prior authorization, these denial rates are 8.14% and 52.47%. Centers for Medicare and Medicaid Services (2020a) reports that over the first five years of the prior authorization program, 43% of prior authorization requests were rejected. This implies that under prior authorization, firm revenue was nearly 70% lower $(1 + \frac{0.0814-(0.43+(1-0.43)\times 0.5247)}{1-0.0814} = 0.295)$.

Using these calibrated parameter values, fraud is profitable under reasonable bounds on uncalibrated parameters without prior authorization and unprofitable when prior authorization is in place. Plugging the calibrated parameter values into equation (5), we have that firms commit fraud in the absence of prior authorization if

$$R(0) - C(0) > (P_{Crim} + P_{Civ})F_{Civ} + P_{Crim}J$$

$$5377848.82 - C(0) > (0.024 + 0.038)(1158241.92) + 0.024J$$

$$5306037.82 > C(0) + 0.024J.$$

This means that ignoring the potential jail costs, fraud is profitable as long as the firm has a profit margin of greater than $\frac{5377848.82-5306037.82}{5306037.82} = 0.014$, or 1.4%. Given the low cost of acquiring and staffing an ambulance that does not provide the billed-for services, this is quite likely to be the case. Furthermore, note that the expected liability for fraud is only $(0.024+0.038)(1158241.92)+0.024J = 71811.00 + 0.24J$. Without very high jail costs, this figure is likely much smaller than the financial gains from fraud.

Similarly, firms commit fraud under prior authorization if

$$R(1) - C(1) > (P_{Crim} + P_{Civ})F_{Civ} + P_{Crim}J$$

$$5377848.82 \times 0.295 - C(0) - 3488 > 71811.00 + 0.024J$$

$$1511166.40 > C(0) + 0.024J.$$

This means that even ignoring the potential jail costs, the firm finds fraud unprofitable as long as it has a profit margin less than $\frac{5377848.82-1511166.40}{1511166.40} = 2.559$, or 255.9%, without prior authorization. This high profit margin is unlikely, meaning that prior authorization makes fraud unprofitable.

Finally, note that because of the limited liability the firms face, even a civil enforcement probability of 1 would not be as large a deterrent of fraud as prior authorization is. To see this, note that

$$R(0) - C(0) - F_{Civ} - P_{Crim}J > R(1) - C(1) - (P_{Crim} + P_{Civ})F_{Civ} - P_{Crim}J$$

$$5377848.8 - 1158241.92 > 1511166.40$$

$$4219606.88 > 1511166.40.$$

This indicates that, because recoveries are limited, facing ex post liability will (in our context) never be as effective as preventing the money from being paid out in the first place.

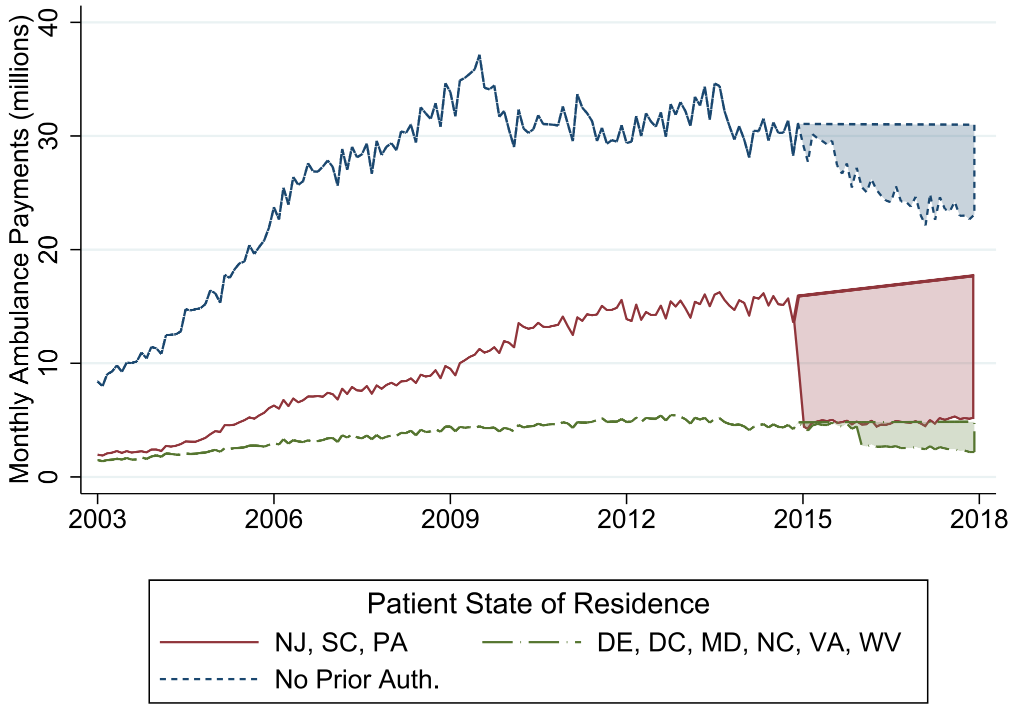
K Counterfactual Spending

In this appendix, we describe the back-of-the-envelope calculation that we perform to arrive at our estimate of counterfactual savings had Medicare implemented prior authorization earlier. First, we estimate a linear trend in spending for each of three groups of states: those subject to prior authorization in December 2014, those subject to prior authorization in January 2016, and those not subject to prior authorization in our data. We estimate this trend using data from November 2009 to November 2014. We then project this trend to the end of our data. This yields our estimate of the counterfactual level of spending had prior authorization not been implemented at all. We estimate that the implementation of prior authorization saved Medicare $703 million on 3.7 million rides relative to this counterfactual. Figure A19 shows this counterfactual and the savings that Medicare accrued graphically.

Next, we estimate the mean level of spending in each of the three groups of states in 2003 and 2004. We take this to be the counterfactual level of spending had prior authorization been in place throughout our sample and the large growth in ridership not occurred. We estimate that relative to this counterfactual, Medicare spent an additional $4.1 billion on 17.5 million rides. Figure A20 shows this counterfactual and the amount of excess Medicare spending realized graphically.

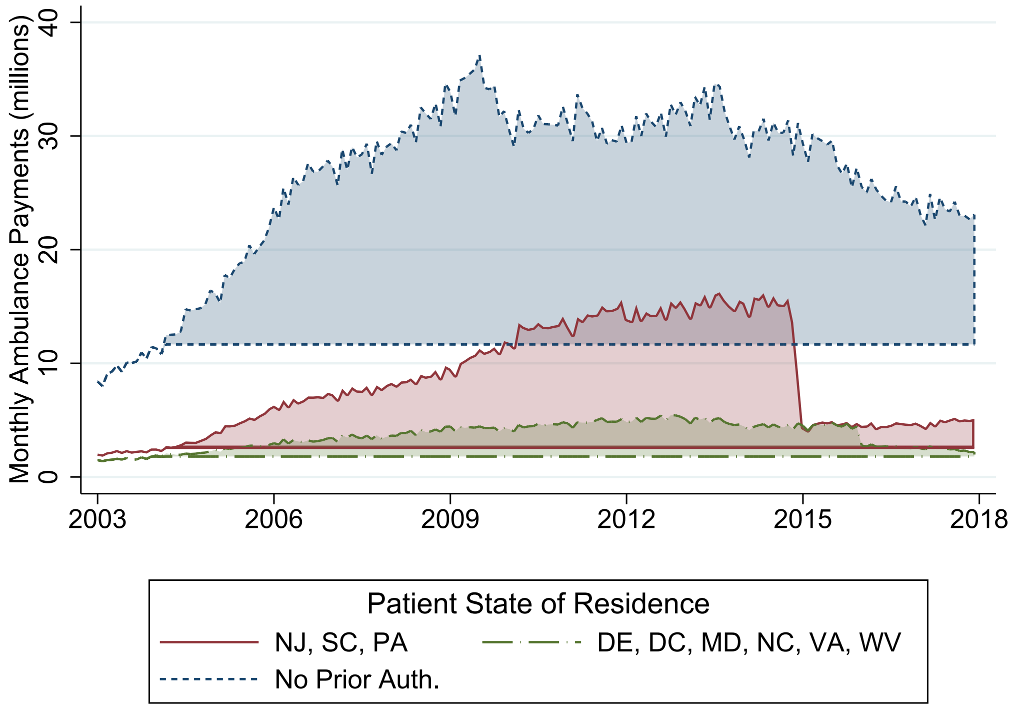
Finally, we add these two sums together, obtaining the amount of money that Medicare would have saved by implementing prior authorization in 2003 relative to not implementing it at all during our sample period: $4.8 billion and 21.2 million rides. Figure A21 shows this amount graphically for all states aggregated together.

Figure A19: Counterfactual Spending without Prior Authorization



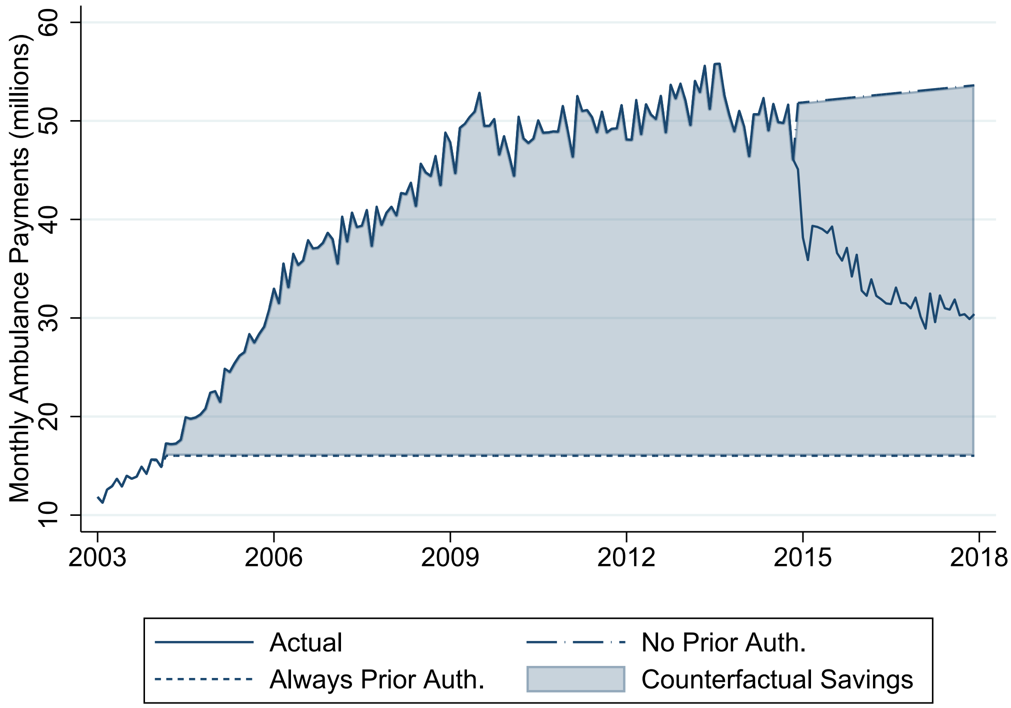
Notes: For each of the three lines, the vertical axis measures total Medicare payments for non-emergency dialysis rides in the represented states. Sample includes non-emergency basic life support ambulance rides from a dialysis facility to a place of residence for dialysis patients from 2003–2017. State determined by the transported patient's residence. Projections represent a linear projection of the trend in spending in each set of states from November 2009 to November 2014.

Figure A20: Counterfactual Spending with Prior Authorization Throughout Sample Period



Notes: For each of the three lines, the vertical axis measures total Medicare payments for non-emergency dialysis rides in the represented states. Sample includes non-emergency 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. Projections represent the mean level of spending in each of the three groups of states in 2003 and 2004.

Figure A21: Total Counterfactual Savings from Prior Authorization



Notes: The figure present the total Medicare payments for non-emergency dialysis rides. Sample includes non-emergency basic life support ambulance rides from a dialysis facility to a place of residence for dialysis patients from 2003–2017. The short-dashed line gives the mean level of spending in 2003 and 2004 while the long-dashed and dotted line gives a linear projection of the trend in spending from November 2009 to November 2014.