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ABSTRACT

A substantial literature has studied the influence of malpractice pressure on physician behavior. However, these studies generally focus on malpractice pressure stemming from state laws that govern liability exposure, which may be unknown or not salient to physicians. We test how physicians respond to malpractice allegations made directly against them. Our sample is Emergency Department physicians in Florida, where we have the universe of data on patients and how they are treated along with a census of malpractice complaints. We find that physicians oversee 9% fewer discharges after malpractice allegations and treat each discharge 4% more expensively after an allegation. These effects are true for both allegations that result in money paid and allegations which are dropped. Further, the increase in treatment is generalized, i.e., not limited to patients with conditions similar to what the physician is reported for. The results suggest significant, if modest, impacts of malpractice claims on medical practice.

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1 Introduction

The influence of malpractice liability on physician behavior has generated considerable debate. While academic research has generally concluded that malpractice exposure has only a modest effect on providers (Chandra, Jena and Seabury, 2013; CBO, 2019), the importance of malpractice liability continues to attract attention among practitioners and policy makers (Frakes and Gruber, 2019). The core empirical question underlying this debate is whether malpractice liability leads to defensive medicine, wherein physicians alter their treatment decisions for fear of being sued, not for expected medical benefit.

Although defensive medicine has motivated a large empirical literature, existing research focuses almost exclusively on physicians' responses to state-level malpractice reforms, specifically caps on non-economic damage awards.[1] The effects of these tort reforms, however, are unlikely to capture the full impact of malpractice liability. Most physicians are not financially liable for malpractice damages (Zeiler et al., 2007) and evidence on the relationship between damage caps and suit frequency is mixed (Mello and Kachalia, 2016); thus, changing damage limits may have little direct impact on practice.[2] Moreover, not all physicians know the malpractice environment in their state and state-level policies may not be salient to physician decision-making. Empirical work shows that physicians' perceptions of liability risk are not strongly correlated with state tort laws (Carrier et al., 2010).

A different question is how physicians respond to direct accusations of malpractice. Under this premise, the key issue is whether physicians' practice patterns are influenced by their actual experience with malpractice liability, rather than the severity of potential sanctions that are determined by state laws.[3] Understanding how being accused of malpractice affects practice is also important because malpractice allegations have uninsured costs - time and emotional energy - that monetary payment by itself does not (Mello et al., 2010).

In this paper, we test how physicians change their labor supply and treatment decisions in response to accusations of malpractice made against them. Allegations of malpractice are common. Approximately 75% of physicians will face a malpractice allegation during their career and on average physicians spend 11% of their career with an open malpractice claim (Jena et al., 2011). In our sample, 3% of emergency medicine physicians are accused of

[1] See Mello and Kachalia (2016), Paik, Black and Hyman (2017) and CBO (2019) for reviews.

[2] Even when physicians lose a malpractice claim, plaintiffs' awards are typically less than the insurance limit. Premiums for malpractice insurance are community rated and do not rise based on personal malpractice history. Premiums for hospital-based physicians are often the responsibility of the hospital.

[3] A broader literature on deterrence has characterized these dynamics as general deterrence (response to threat of punishment) and specific deterrence (response to experience of punishment); there is clear evidence that risk perceptions are influenced by past experience, but less evidence that these perceptions relate to policies that govern sanctions (Chalfin and McCrary, 2017).

malpractice per year and 25% are accused at some point during the eight-year study period.

The primary challenge to studying the impact of malpractice allegations is that malpractice claims may be endogenous to physician behavior (Jena et al., 2015). We address this concern by leveraging quasi-random variation in the timing of malpractice allegations across physicians. Malpractice claims are often unexpected: negligence rarely leads to malpractice allegations and even appropriate care can trigger frivolous claims (Danzon, 1991; Studdert et al., 2006). We exploit this variation to estimate the causal effect of malpractice allegations in an event study, difference-in-differences (DD) design. We study physician practice patterns in a four-year panel around the allegations, comparing physicians who were reported for malpractice to unreported peers who were otherwise similar to the accused physicians. Our control physicians do not necessarily differ in their propensity to be negligent, given imperfections in the operation of the malpractice system, but they do differ in their exposure to malpractice allegations.

Our data come from Emergency Department (ED) physicians in Florida. We combine physician-level data on malpractice claims with the universe of ED discharges. Studying malpractice pressure in the ED has several advantages. First, uncertainty and risk surrounding patient care are high in the ED relative to other settings, elevating concerns about malpractice allegations. Second, physician productivity is important in EDs, which have faced increased crowding (IOM, 2006). Lastly, there is limited scope for patient demand responses to malpractice in the ED, since patients rarely choose their ED physician. Thus, we can isolate the impact of malpractice on physician decisions, separately from patient choice.

We consider two broad margins of physician behavior: labor supply and treatment intensity. We measure labor supply by the volume of patients seen and where the physician practices (e.g., does the physician continue to work in the same hospital where they were reported?), then measure treatment intensity by services provided. We find that physicians decrease their labor supply in response to malpractice allegations and this reduction is persistent over time. Overall, physicians who are accused of malpractice oversee 9.3% fewer discharges after malpractice allegations, relative to their unaccused peers. This decrease in labor supply is driven by responses on the intensive margin; specifically, physicians reduce the number of discharges they oversee but maintain practice in the state. However, we also find that accused physicians are more likely to stop practicing at the hospital where the alleged negligence occurred and move their practice to observably similar hospitals.

We then examine how physicians adjust their treatment intensity among their remaining patients. Malpractice allegations lead to an increase in treatment intensity, with reported physicians increasing costs per discharge by 4.3% after an allegation, relative to their peers. There is no evidence that these results are driven by differential selection of patients after

malpractice allegations. We find that increases in intensity among accused physicians are concentrated in laboratory spending, consistent with a larger role for malpractice pressure in providers' use of diagnostic care, where errors of commission are unlikely to result in injury (Baicker, Fisher and Chandra, 2007; Frakes, 2015; Frakes and Gruber, 2019).

To the extent that malpractice accusations help physicians correct errors they were making, one would expect physicians' reactions to be related to the alleged error in the treatment of the harmed patient. However, our results show that physicians change their behavior generally, rather than concentrating their behavior changes among claimant-similar patients. For example, a physician accused of malpractice for treatment of a pregnant woman would treat *all* their patients more intensively after the allegation, not only pregnant patients. Additionally, physicians have similar responses to allegations regardless of how much money is paid, including claims that were resolved in the physicians' favor. Thus, our results suggest that the additional care provided after a malpractice allegation may not stem from an intent to correct clinical decision-making, which could be welfare enhancing. Finally, we look to see whether the responses to malpractice allegations are concentrated among potential "bad apples" - physicians accused repeatedly of malpractice. Contrary to the theory, we find that responses among physicians accused multiple times are no larger than responses among physicians accused just once.

Our results are consistent with defensive medicine but are not definitive. Defensive medicine is generally defined as providing additional care in response to malpractice concerns *where the value of the care is less than the cost* (Kessler and McClellan, 1996). We do not have a way to assess the value of all the additional care provided. However, we can examine specific types of care that other researchers have characterized as high-value and low-value in emergency settings. For example, diagnostic lumbar scans have been shown to be low-value for patients presenting with uncomplicated low back pain (Schwartz et al., 2014), while chest scans are valuable for patients with a history of cancer or chronic obstructive pulmonary disease. We find that increases in treatment intensity after malpractice allegations are not limited to high-value services.

Our results have implications for system-level spending, which we take up in a discussion section. Our analysis suggests that aggregate spending *among reported physicians* decreases after malpractice allegations. However, seeing fewer patients does not have the same social cost implications as delivering additional services. Even when reported physicians reduce their labor supply, patients not treated by the accused physician will be seen by other physicians, assuming the same number of patients come to the ED. Thus, the system-level impact is driven by the increase in treatment intensity among reported physicians and by any difference in spending patterns between reported physicians and the unreported peers

who care for the reallocated patients. In back-of-the-envelope calculations, we estimate that the net effect of all allegations is a modest, 1% increase in ED spending.

This paper is related to several literatures. First, we build on a large body of work studying the influence of malpractice liability on physician behavior. This research has shown that laws governing the expected consequences of malpractice liability, such as damage caps, have modest effects on aggregate spending and little effect on the overall supply of physicians. As discussed previously, the effects of state-level tort reforms are unlikely to capture the full influence of malpractice liability. Moreover, these estimates will understate the effect of malpractice liability if malpractice pressure causes physicians to simultaneously increase spending per patient and see fewer patients.[4] The literature is aware of these phenomena (Dubay, Kaestner and Waidmann, 2001); they are termed "positive" (providing more care) and "negative" (seeing fewer patients) defensive medicine, but they are rarely studied separately. One advantage of our data is the ability to separately identify the effect of malpractice liability on these two margins. Relative to the tort reform literature, we show large behavioral changes among accused physicians.

Our results also build on a growing literature studying malpractice liability outside of tort reforms. This work has leveraged variation in liability from military immunity (Frakes and Gruber, 2019) and jury verdicts (Lakdawalla and Seabury, 2012). Closely related work has studied the effect of malpractice allegations against obstetricians, which the literature notes may capture both supply responses of physicians and changes in patient demand.[5] Our work isolates provider supply decisions, showing robust evidence of positive and negative responses to allegations, and sheds light on the mechanisms behind changes in physician behavior.

More broadly, our paper relates to a large literature on physician behavior and variation in practice styles. This literature has studied the influence of physicians' financial incentives (Clemens and Gottlieb, 2014), diagnostic decision-making (Abaluck et al., 2016; Currie and Macleod, 2017), practice environment (Chandra and Staiger, 2007; Molitor, 2018), beliefs about treatment (Cutler et al., 2019) and response to new information (Cutler, Huckman and Landrum, 2004; Dranove et al., 2004; Kolstad, 2013; Shurtz, 2013; Shapiro, 2017; Sarsons, 2019). Our results complement this literature by addressing differences in practice style due to differential exposure to the malpractice system.

[4] DD estimates of changes in aggregate spending surrounding state tort laws will likewise underestimate the impact of malpractice liability if physicians' perceptions of liability are informed by the experiences of colleagues in other states.

[5] This literature has found minimal effect of allegations on cesarean section rates (Grant and McInnes, 2004; Dranove and Watanabe, 2010; Gimm, 2010; Durrance and Hankins, 2018). Changes in the volume of deliveries have been interpreted as a supply response (Gimm, 2010) and a demand response (Dranove, Ramanarayanan and Watanabe, 2012). Recent work has also studied the association between physician labor supply and the number of paid malpractice claims (Studdert et al., 2019).

The paper proceeds as follows. Section 2 provides background on malpractice claims and Section 3 describes the data. Section 4 discusses our empirical strategy. Section 5 describes our labor supply results. Section 6 describes our treatment intensity results. Section 7 explores heterogeneous responses to malpractice allegations across claim and physician characteristics. Section 8 discusses the aggregate impact of malpractice reports and concludes.

2 Background on Malpractice Allegations

Malpractice allegations begin with a patient perceiving an injury. Importantly, patient injuries are unlikely to prompt immediate concerns about malpractice allegations. First, physicians are often not aware of the patient injury at the time of treatment. This lag is especially relevant in the context of the Emergency Department (ED), where many patient injuries manifest after the patient has left the provider's care.[6] In addition, patient injuries do not necessarily imply physician negligence. Moreover, even in cases of negligence, the majority of injuries go unreported; less than 2% of patients harmed by provider negligence pursue a malpractice claim (Danzon, 1991; Localio et al., 1991).[7]

When a patient reports an injury as malpractice, there is often a substantial lag between the injury and the report. Malpractice reports are not filed until a patient successfully recruits legal representation. Injured patients initiate malpractice claims by retaining a personal injury lawyer, who consults with an independent medical examiner to determine if there are grounds to pursue the claim. If there is sufficient evidence to build a case, the lawyer contacts the hospital and the physicians involved in the case, who in turn report the contact to their malpractice insurer (Dranove and Watanabe, 2010). This report represents the first time a physician knows with certainty that she will be facing a malpractice claim. In our sample, malpractice reports occur on average five quarters after the patient injury (Figure 1, Panel A). Almost all malpractice reports are filed within two years of the alleged negligence, consistent with statute of limitations laws in Florida (Florida Statute 95.11(4)(b)).

Once a report is filed, malpractice allegations are slow to resolve (Jena et al., 2012). Malpractice reports typically prompt settlement talks between the claimant's attorney, the physician's malpractice insurer and the hospital's risk managers. The physician is sometimes present for these negotiations, but rarely participates. In Florida, plaintiffs in medical malpractice cases must notify the defendants of an intent to file suit at least 90 days prior to the filing; this notification prompts a pre-suit discovery phase. In the event that a suit is filed,

[6] For example, one malpractice claim in our sample reads, "patient was diagnosed with a migraine and discharged. approximately [sic] 9 days later the patient was [diagnosed] with a cerebral hemorrhage."

[7] Approximately 40% of malpractice claims result in no payment to the claimant (Studdert et al., 2006).

settlement talks continue as both parties prepare for a trial.[8] The average time from report to final disposition is over two years in our sample (Figure 1, Panel B).

3 Data

3.1 Data Sources

Our analysis relies on two primary datasets. First, we use administrative discharge data from Florida's Agency for Health Care Administration (AHCA). These data contain the universe of ED discharges in Florida between 2005 and the third quarter of 2013. ED visits that result in inpatient stays at the same hospital, rather than discharges, are not included in this data.[9] Each discharge record identifies the attending physician as well as the facility where the care was delivered. We additionally observe year and quarter time stamps, patient demographics, diagnoses and procedures, and hospital charges across different service types. Procedures are identified primarily through documentation of Current Procedural Terminology (CPT) codes; this documentation changed in 2010 when the AHCA released a new version of the data.[10]

We link the discharge data with additional information on physician and hospital characteristics. Physician characteristics are from the Florida Department of Health Practitioner Profile and from Doximity, a networking service for medical professionals which has assembled a comprehensive database of physician characteristics similar to the American Medical Association Physician Masterfile; these datasets provide snapshots of provider characteristics at a point in time, including specialty, tenure and gender. Hospital characteristics are from the Dartmouth Atlas, the Florida AHCA and the Impact Files from the Centers for Medicare & Medicaid Services. These datasets provide annual information on hospital characteristics, including cost-to-charge ratios, teaching status and Hospital Referral Region (HRR). We further use the discharge data to identify (1) safety net hospitals, defined as facilities with top quartile shares of uninsured and Medicaid discharges, and (2) high-volume hospitals, defined as facilities with top quartile discharge volumes. Finally, we link discharges to the Agency for

[8] In a small minority of claims, the suit is filed before a report is issued to the malpractice insurer. This occurs in under 3% of our sample.

[9] These visits are included in the Florida inpatient data. It is not possible to follow patients across hospital settings. Discharge data does include patients who die in the ED and those discharged to another facility.

[10] In the older data release, hospitals were required to report a principal CPT code, typically an evaluation and management code, and had the option to report "other" CPT codes. About 20% of hospitals reported no radiology CPT codes. In the newer data release, the principal CPT field was eliminated. Evaluation and management CPT codes were reported in separate, required fields, and the hospital had the option to report "other" CPT codes. The 20% of hospitals not reporting radiology CPT codes prior to the change saw large jumps in radiology coding (Figure A15).

Healthcare Research and Quality's (AHRQ) Clinical Classifications Software (CCS), which categorizes discharges into related clinical groups according to listed diagnoses.

Our second primary dataset is information about physicians' malpractice histories. We use administrative data from the Florida Office of Insurance Regulation containing malpractice claims that were closed in Florida between 1994 and 2016. Each record includes the identity of the accused physician as well as the hospital where the alleged negligence occurred. When a single malpractice claim involves multiple physicians, we observe separate records for each physician. We additionally observe the date that the patient injury occurred and the date that the alleged malpractice was first reported. The patient injury date indicates when the alleged negligence occurred, even if the injury manifested after treatment had completed. The data also include a free text description of the patient injury. We use this description to categorize injuries into clinical groups. For example, we construct a "head" clinical group including claims with the key phrases such as "headache" and "stroke" in the free text field (Figure A1). See Appendix for more detail. Each record also indicates whether the claim resulted in a lawsuit, was settled out of court, was resolved in favor of the patient or the physician, and the amount of any financial compensation that was dispensed. Importantly, while our data cover all claims that resolved in favor of the patient, claims that resolved in favor of the provider are included only if there was a court approved settlement or if the malpractice insurer incurred costs of at least $5,000 in the course of investigating the claim (Florida Statute 627.912(4)). As a result, the data are likely to omit allegations that were abandoned early in the claim development or were easily refuted by malpractice insurance companies.[11]

3.2 Dependent Variable Definitions

Our first set of dependent variables focus on physician labor supply. We define total labor supply for each physician as the volume of ED discharges overseen in a given quarter. We then decompose total labor supply across extensive and intensive margins. On the extensive margin, we study whether physicians maintain any ED practice in the state, defined as overseeing one or more discharges. By this definition, discontinuation of ED practice indicates a final exit from Florida ED work and is analogous to retirement.[12] On the intensive margin, we study the total volume of discharges that a physician oversees in a given quarter,

[11] We are unable to determine the share of claims omitted due to this restriction. A claimant and their attorney might abandon a case if they discover new information that decreases the expected value of the claim; a physician's policy limits could be too low to justify filing a suit, for example. Attorneys are generally paid on a contingency basis and thus face incentives to pursue claims with high potential damages.

[12] As a counterexample, if a physician practices in the state, oversees no discharges for two quarters and then resumes practicing, this does not count as a discontinuation of practice.

conditional on maintaining any ED practice in the state. We additionally measure physician labor supply at different locations, tracking physician discharge volumes at the hospital where the patient injury occurred (hereafter the "injury hospital") and the HRR where the injury occurred (hereafter the "injury region").[13]

Our second set of dependent variables focus on treatment intensity. Our data come from hospital discharge records and include hospital charges, which we adjust using hospitals' annual cost-to-charge ratios in order to capture a measure of resource utilization.[14] We cannot observe physician charges. We study total costs at the discharge level, as well as costs in the following service categories: laboratory, radiology, and general emergency room (ER) services. These service categories are defined using revenue codes from the UB-04.[15] We report costs in 2009 dollars, adjusting for inflation using the Producer Price Index.

We further measure treatment intensity through rates of diagnostic radiology testing. We first calculate the prevalence of two low-value imaging tests: lumbar imaging among patients presenting with uncomplicated low back pain and head imaging among patients presenting with uncomplicated headache.[16] We also calculate the rate of one high-value imaging test: chest CT scans and x-rays for patients presenting with shortness of breath or chest pain who also have diagnoses of cancer or COPD. Diagnostic imaging tests are identified using ICD-9 codes and CPT codes.

3.3 Sample Construction

Our sample is Emergency Medicine (EM) specialists, who provide the majority of care in the ED (Figure A2). Because our interest is in tracking the effect of malpractice pressure over time, we restrict our sample to EM specialists who practiced in the ED for at least four quarters and oversaw at least 100 discharges during our study period. For data quality purposes, we further exclude physicians if they oversaw 3,000 discharges or more in any quarter. After these restrictions, there are approximately 2,700 EM specialists who practiced

[13] We are able to identify the injury hospital for 96% of the reported physicians in our main sample.

[14] We are able to merge cost-to-charge ratios for about 90% of hospital-years. For hospitals where cost-to-charge ratios were missing for only some years, we impute missing values as the average cost-to-charge ratio at the hospital in the non-missing years. For hospitals with missing cost-to-charge ratios in all years, we use the average annual cost-to-charge ratio among all non-missing observations. We assess the sensitivity of our results to the cost-to-charge adjustment in Figure A14.

[15] General ER services encompass the 045X revenue codes. ER charges depend, for example, on whether the hospital provided EMTALA screening services only or services beyond screening.

[16] We use definitions of low-value care from Schwartz et al. (2014) and the Choosing Wisely campaign. Lumbar imaging includes x-rays, CT scans and MRIs; the population of patients presenting with low back pain excludes those with diagnoses that would warrant lumbar imaging, for example cancer, trauma, IV drug abuse and neurological trauma. Head imaging includes CT scans and MRIs; the population of patients presenting with headache excludes those with comorbidities that would warrant an image such as cancer, head trauma and altered mental status.

at some point during our study sample. Collectively, they oversaw over 43 million discharges. More information about sample construction is available in the Appendix.

We construct two cohorts of physicians to study the effect of malpractice allegations. To begin, we combine the discharge data and closed claims data to identify EM physicians who experienced a malpractice claim. Approximately 680 physicians were reported for malpractice during our study period. To create a balanced panel, we restrict our analysis to physicians who we can follow for at least two years before and after the report.[17] Functionally, this restriction limits our focus to reports that are filed between the first quarter of 2007 and the third quarter of 2011 (Figure A3). Importantly, this restriction does not imply that physicians must provide care in the ED continually for four years to be included in our sample. Because our data contain the universe of ED discharges, we can observe quarters when physicians oversaw zero discharges and count them toward the four-year sample restriction. Once a physician appears in the data, we track their labor supply until the end of the study period, including quarters where they did not oversee any discharges. This approach allows us to capture extensive margin labor supply changes such as exits from Florida ED practice.

We construct a pool of potential controls who were similar to the reported physicians prior to their malpractice allegations but were not reported themselves.[18] For each reported physician, we select controls from the group of unreported physicians who were practicing in the ED during the same time period. In addition, the pool of potential controls is limited to physicians who we can observe for at least four years that are contemporaneous with a reported physician.

One challenge in selecting controls is that reported physicians had different practice patterns than their unreported peers, even prior to the report. Specifically, reported physicians tended to oversee more discharges per quarter (Figure A4). We address this challenge in two ways. First, we drop both reported and unreported physicians with average baseline discharge volumes below the tenth percentile of volumes among reported physicians. For reported physicians, we calculate average discharge volumes using data in the two years prior to the report. For unreported physicians, we calculate average discharge volumes during their eligible match period.[19]

Second, we use a propensity score match to choose controls who had similar characteristics as the reported physicians. We perform a two-to-one caliper match with replacement based on pre-report discharge volume trends, specifically the average linear trend in the two years

[17] If a physician is reported twice during our study period, we use the date of the first report.

[18] Physicians who were reported prior to our study period are included in the potential controls.

[19] To mirror the restriction on reported physicians, we compute a rolling two-year average of discharge volumes in each quarter of the eligible period. If a physician is match eligible in multiple periods, we take the average across all eligible periods.

before the report. The match also includes the following characteristics: Medical Doctor (MD) versus Doctor of Osteopathy, tenure, gender, hospital type and hospital region.[20] Hospital type variables include an indicator for teaching hospitals and safety net hospitals. In cases where physicians practice at multiple hospitals, characteristics are based on the hospital where the physician had the plurality of their practice. To ensure that control physicians were practicing contemporaneously with the reported physician, we require an exact match on the date of the (real or placebo) report. Thus, we obtain a control group of physicians who had similar characteristics and exactly the same quarter of exposure as the reported physicians.

3.4 Descriptive Statistics of Reported Physicians and Controls

Table 1 summarizes the characteristics of reported and unreported physicians in the pre-report period, before and after the propensity score match. For reported physicians, statistics are based on the quarter prior to the report.[21] For unreported physicians, statistics are calculated based on data from the chronological midpoint of their practice. The first set of columns show that reported physicians were more experienced than the unreported physicians, less likely to be female, more likely to be in central Florida and less likely to be in teaching hospitals. 25% of EM specialists in our sample were reported for malpractice during our study period, implying that approximately 3% of EM specialists in Florida faced malpractice claims in a given year. This estimate is conservative relative to existing literature (Jena et al., 2011), likely because our closed claims database omits certain claims, as described previously.

Reported physicians oversaw more discharges than unreported physicians in the baseline period. A natural question is how malpractice risk changes as physicians see more patients. In our sample, the rate of malpractice allegations is relatively stable as physician discharge volumes increase, consistent with a mechanical increase in malpractice risk from treating additional patients (Figure A5).[22]

The second set of columns describe the balanced panel of physicians that enter into the

[20] We control for tenure quartiles during the eligible period and control for region using broad location bins: northern HRRs, central HRRs and southern HRRs.

[21] We cannot observe any pre-report data for physicians reported in the first quarter of 2005, so we do not include them in this table.

[22] Figure A5 does not include very low-volume physicians, who have high rates of malpractice. Excluding these low volume physicians, we estimate a statistically insignificant relationship between discharge volume and the rate of malpractice allegations. More generally, theory is ambiguous on the relationship between discharge volume and malpractice risk. Rates of malpractice allegations could decrease as volume increases if physicians learn by doing and are less likely to make mistakes (Hannan et al., 2003). Conversely, rates of malpractice could increase with discharge volume if physicians change their treatment patterns when they are short on time or overworked (Chan, 2018).

propensity score match. We require physicians in this panel to have two years of data before and after the report (real or placebo), reducing the number of physicians in our sample by approximately half (Figure A3). Differences between reported and unreported physicians were similar in the balanced panel, relative to the full sample of EM specialists.[23]

The third set of columns describe the matched analytic sample. The propensity score match identifies controls for all but one reported physician.[24] Physician covariates are well balanced across the treatment and control group and we find no statistically significant difference in the covariate means across the reported and unreported physicians: average tenure is similar across the two cohorts, as is the distribution of physicians by gender and across hospital characteristics. Discharge volume levels remain different across reported and unreported physicians after the match but volume trends are well matched; we discuss this topic further in Section 5.[25]

3.5 Descriptive Statistics of Malpractice Claims

Table 2 describes the malpractice allegations levied against physicians in our sample. Panel A describes physicians' malpractice histories. Consistent with previous work, multiple reports were fairly common (Studdert et al., 2016). Of the reported physicians in our sample, 26% had faced a malpractice allegation in the past and 29% faced an additional allegation after the initial report in our sample. The full distribution of reports per physician is plotted in Figure A6.

A natural question is whether physicians who experience multiple allegations are different from other providers, perhaps providing lower quality care (Studdert et al., 2019). We cannot address this question directly, but we can observe the characteristics of the complaints against physicians with single and multiple reports. In Figure A7, we compare the initial reports among physicians who go on to have multiple claims to reports among physicians with a single claim. We find few differences across the groups; initial reports among physicians with multiple claims had similar shares of suit filings, payments, and permanent injuries as reports among physicians with a single claim.[26]

[23] One exception is average costs per discharge, which were lower among reported physicians in the full sample and higher among reported physicians in the balanced panel, although neither difference is statistically significant. This change results from excluding physicians with low baseline discharge volumes (described previously), who tend to have high costs per discharge.

[24] Since we select controls with replacement, the unreported physicians in Table 1 can match to multiple reported physicians.

[25] We do not match on discharge volume levels explicitly because matching on the baseline value of an outcome variable can introduce regression to the mean into the analysis (Daw and Hatfield, 2018).

[26] Ex ante, the direction of this relationship is not clear. To the extent that multiple reports signal low quality care provision, the severity or negligence of initial claims among physicians with multiple reports could be higher than reports among physicians with a single claim. Conversely, since physicians must survive

Panel B describes how malpractice claims developed through the litigation process. The majority of reports resulted in a suit filing. In cases that included a suit, filings tended to happen relatively quickly after the report; nearly all suits were filed within two quarters of the report, while final dispositions occurred slowly over time (Figure A8). The majority of reports resulted in a payment to the claimant, with an average payment of over $200,000.[27] Panel C describes the timing and duration of malpractice claims in our sample. As discussed previously, malpractice reports are slow to resolve. The claims in our sample took nearly 3 years to reach their final disposition if a suit was filed and over a year if no suit was filed.

The bottom half of Table 2 describes the patient injuries that prompted malpractice reports in our sample. Panel D shows the distribution of patient injuries across clinical categories. The most common injuries are related to chest, head, trauma, back and abdominal conditions. While some clinical injury types occurred in proportion to the frequency of related discharges, others are more common than discharge volumes would suggest (Figure A9). Reported abdominal injuries, for example, occurred in proportion to the number of patients presenting to the ED with an abdominal condition, while reported head injuries occurred more frequently. Panel E describes the severity of patient injuries. The reports in our sample tended to be serious: approximately 80% involved a permanent injury.

Panel F shows the characteristics of hospitals where malpractice reports originated. Approximately 5% of malpractice reports occurred at teaching hospitals, proportional to the frequency of discharges in that setting (Figure A10, Panel C). About 40% of reports occurred at high volume hospitals and 26% occurred in safety net hospitals, also proportional to the frequency of discharges in these locations (Figure A10, Panels A and B).[28] Our finding on safety net hospitals is consistent with prior literature, which generally concludes that low-income patients are not more likely to sue for malpractice than high-income patients, despite physicians' perceptions to the contrary (Burstin et al., 1993; McClellan et al., 2012).

their first malpractice report in order to have a second, the severity of initial claims among physicians with multiple reports might be mechanically lower. An alternative explanation is that plaintiffs' attorneys are adept at picking winning cases, such that claims against high and low quality physicians are similar on observables.

[27] We measure payments in 2009 dollars, adjusting for inflation using the year of the disposition and using the Producer Price Index for consistency with our analysis of ED costs. The difference between the average claimant payment among all physicians ($361,000) versus our matched sample is driven by one report with an unusually high payout.

[28] High volume hospitals account for a relatively high share of discharges by definition.

4 Methods

We estimate the effect of malpractice reports on physician behavior using an event-study, difference-in-differences (DD) approach. The treatment group includes physicians who were reported for malpractice during our study period. The control group consists of observably similar physicians who practiced contemporaneously in the ED but did not experience a report during the study period, as selected by our propensity score match. This empirical strategy allows us to leverage the quasi-random timing of malpractice allegations while also controlling for any secular trends in practice patterns that occurred during our study period. We estimate the following specification:

$$\begin{aligned} Y_{jt} = & \sum_{\substack{k=-8 \\ k\neq -1}}^{8} \alpha_k * \mathbb{1}(k \textit{ qtrs since report})_{jt} + \sum_{\substack{k=-8 \\ k\neq -1}}^{8} \beta_k * \mathbb{1}(k \textit{ qtrs since report})_{jt} * \textit{Reported}_j \\ & + \delta Tenure_{jt} + \gamma_j + \tau_t + \epsilon_{jt} \end{aligned} \tag{1}$$

where Y_{jt} measures labor supply outcomes for physician j in quarter t. γ_j are a set of physician fixed effects, τ_t are year-quarter fixed effects, δ measure the time-varying effects of physician tenure, and ϵ_{jt} are error terms that allow for correlation between observations from the same physician.[29] The coefficients of interest, β_k, measure the effect of malpractice reports in the eight quarters before and after the allegation. The quarter before the report serves as the omitted category.[30] We include a set of event time indicators that apply to all physicians (α_k), as well as the set of indicators that apply only to the reported physicians (β_k). This approach gives the β_k a traditional, DD interpretation and addresses the concern that trends in physician practice patterns around a report cannot be fully explained by tenure, calendar time and physician fixed effects. In particular, if a physician is reported in time t, she is mechanically more likely to have practiced medicine in times $T \leq t$ than times $T > t$, since the physician must have been practicing at the time of the precipitating injury. The event time indicators that are common to reported and unreported physicians account for such mechanical effects.

In the course of our analysis, we estimate two variants of Equation 1. First, for brevity we specify a regression to estimate the overall, pre-post effect of malpractice reports, rather than estimating the effect separately for each quarter.[31] Second, to estimate the impact

[29] We group tenure into five-year bins.

[30] The $k = 0$ quarter is a partial exposure period. Physicians who experience reports between January 1, 2009 and March 31, 2009, for example, all have $k = 0$ in the first quarter of 2009.

[31] $Y_{jt} = \alpha * \textit{Post} + \beta * \textit{Post} * \textit{Reported} + \delta Tenure_{jt} + \gamma_j + \tau_t + \epsilon_{jt}$

of malpractice allegations on treatment intensity, we run regressions at the discharge level, rather than the physician-quarter level. This unit of analysis allows us to estimate changes in average costs per discharge, as opposed to total costs per period, and to include additional controls for patient characteristics that may influence treatment intensity: reason for visiting the ED, age, race/ethnicity, gender, insurance status and weekend care.[32]

We model costs using a log transformation to account for skewness. For service categories such as laboratory or radiology where costs can take a zero value, we employ two-part models, estimating the probability of receiving any care and then the level of costs conditional on treatment provision. We winsorize costs at the 99th percentile within each presenting condition CCS group to minimize the influence of outliers.

For our analyses of diagnostic imaging, we run regressions at the discharge level and also include hospital fixed effects interacted with an indicator for the new data version (2010 forward) to account for the change in CPT code reporting, as discussed previously. We further exclude hospitals with large increases in radiology coding across the new and old data versions.[33] In our main analysis, we exclude hospitals with (1) a 20+ percentage point increase in radiology coding rates or (2) a baseline radiology coding rate of under 1% in the old data, although we run sensitivity checks on that sample restriction (Figures A15 and A16).

5 Effects of Malpractice Reports on Physician Labor Supply

We begin by studying the effect of malpractice reports on physician labor supply. Figure 2 plots physicians' total discharge volumes before and after the report. In Panel A, we compare unadjusted discharge volumes per quarter for reported and unreported physicians. In Panel B, we plot DD coefficients from Equation 1 (β_k), which indicate the impact of the malpractice report k quarters after it occurs.

Malpractice reports had a large and persistent effect on physician labor supply. Prior to the malpractice allegations, reported physicians oversaw approximately 100 more discharges per quarter than unreported physicians. Discharge volumes were relatively stable for both reported and unreported physicians in the pre-report period. After the reports were filed, labor supply patterns of reported physicians diverged sharply from the controls. Discharge

[32]For patient covariates, we control for the reason for ED visit with CCS condition bins. We include the following age bins: 0-4, 5-17, 18-25, ten-year bins covering ages 25-84, and a bin for 85+. Controls for patient race and ethnicity include indicators for white non-Hispanic and black non-Hispanic.

[33]We assess changes in the rate of any radiology coding, not just the diagnostic radiology tests in our analysis.

volumes among reported physicians decreased by approximately 75 discharges per quarter within 6 months of the report, and remained at this new, lower level for the remainder of the study period. In contrast, average discharge volumes among unreported physicians remained stable throughout the study period. These results are summarized in Table 3, which documents the overall, pre-post effect of malpractice reports on physician labor supply. In column 1, we present estimates from our baseline model, analogous to the specification in Figure 2. Consistent with the visual evidence, we find that malpractice reports led to a reduction in labor supply of 78 discharges per quarter, or 9.3%.

We test the robustness of our results in columns 2 and 3 of Table 3. In column 2, we assess whether our results are influenced by the timing of other key events in the claim development: the patient injury, suit filing if relevant, and final disposition of the case. For reported physicians, these analyses are based on actual dates in the claim development; for matched controls, we simulate dates to mirror the elapsed time between the events and the report for the matched reported physician. Our results are largely unchanged when we include controls for these developments. We estimate a statistically significant decrease of 68 discharges per quarter after the report and estimate statistically insignificant impacts of other claim events. We find a relatively large and positive effect of the final disposition, although it is imprecisely estimated; non-parametric event-study results suggest this is driven by a recovery in labor supply immediately following the disposition (Figure A11).[34]

In column 3, we test the robustness of our results to including physician-specific time trends. This specification accounts for any differential trends between the reported and unreported physicians, but can over-correct for time-varying treatment effects (Wolfers, 2006; Goodman-Bacon, 2019). We find that our estimates are largely unchanged using this specification, estimating a 9.2% decrease in discharge volumes. Further robustness checks are available in the Appendix (Figure A12); we draw similar conclusions if we re-run our propensity score match (1) with a stricter restriction on baseline volume, (2) without matching on baseline volume trends and (3) if we select controls using a one-to-one coarsened exact match (Iacus, King and Porro, 2012).

5.1 Understanding the Decline in Physician Labor Supply

One possible driver of the labor supply decline is risk avoidance behaviors by physicians. ED physicians provide care to patients with a variety of clinical conditions, ranging from simple

[34]Figure A11 estimates changes in labor supply patterns surrounding the patient injury, suit filing and claim disposition. To model the effect of the patient injury, we re-estimate our propensity score model to choose control physicians based on characteristics in the quarter before the injury. To model the effect of the suit and the disposition, we use the main analytic sample in our original propensity score model, imputing suit and disposition dates for unreported physicians based on their matched reported physician.

ailments such as sprained ankles to high risk conditions such as chest pain. To the extent that physicians want to minimize uncertainty in response to liability pressure, malpractice reports may prompt physicians to avoid patients with high-risk clinical conditions, where an adverse outcome is more likely. In EDs where physicians assume responsibility for patients at their own discretion, reported physicians may choose to ignore patients who present with high risk clinical conditions, leaving these patients in the queue to be cared for by their peers (Chang and Obermeyer, 2020). In EDs where physicians are assigned patients in rotation, a potential strategy for avoiding complicated patients is to admit them to the inpatient setting, transferring the risk of care provision to inpatient providers. Because our data include only ED discharges, as discussed previously, an increase in admission rates by reported physicians would appear as a decrease in ED discharges.[35]

We test for risk-avoidance behavior by assessing whether physicians decreased their discharge volumes differentially for patients diagnosed with low risk, high risk and uncertain risk clinical conditions. The idea is that a change in inpatient admission rates will have larger effects on high or uncertain risk clinical conditions, relative to low acuity conditions that are inappropriate for inpatient care. Likewise, avoiding patients who present in the queue with high risk clinical conditions will also result in a decrease in high risk discharges.

We define low risk conditions to include patients under 80 years old with primary diagnoses of minor injuries, minor infections and back pain.[36] High risk discharges include patients with diagnoses of pneumonia, coronary atherosclerosis, cardiac dysrhythmias, congestive heart failure, acute cerebrovascular disease, chronic obstructive pulmonary disease, chest pain, urinary tract infection, and skin and subcutaneous tissue infections and also include all patients over 80 years old. Uncertain risk conditions include abdominal pain, headache, and other "ill-defined conditions" as identified by the AHRQ CCS. Admission rates for patients with low risk, uncertain risk and high risk conditions are 2%, 7% and 39%, respectively.[37] Our results are in Table 4, Panel A. Overall, we find little evidence of clinical risk avoidance. Malpractice reports led to statistically similar decreases in discharge volume of 9%, 12% and 9% for low, uncertain and high risk conditions, respectively.

A second issue to understand is whether physicians reduce practice or leave practice entirely. In Panels B and C, we decompose the decrease in discharge volume into extensive

[35] We cannot calculate changes in physician-specific admission rates in our data. Because the Florida inpatient data lists the attending physician overseeing *inpatient* care, and the Florida ED data lists the attending physician overseeing *ED* care, it is not possible to track admissions using physician IDs. As discussed previously, we likewise cannot track patients across hospital settings.

[36] Low risk conditions include the following CCS conditions: other upper respiratory infection, spondylosis, joint disorders, fracture of upper limb, sprains and strains, open wounds of head, neck and trunk, open wounds of extremities, superficial injury, burns, and poisoning by nonmedicinal substances.

[37] Authors' calculations from HCUPnet; does not account for the age restriction on low-risk conditions.

margin and intensive margin effects. Panel B shows the impact of malpractice reports on the probability of maintaining any ED practice after an allegation. There is no evidence that physicians left practice in response to malpractice reports.

We further study shifts in physician practice location, testing whether reported physicians maintained practice in the same hospital or geographic region in which the initial injury occurred. We model both location shifts conditional on maintaining ED practice in the state. For reported physicians, these analyses are based on the actual location of the patient injury in their claim; for matched controls, we define the injury hospital and region according to the hospital where they had the plurality of their practice during the quarter of the simulated patient injury. We find modest evidence that physicians moved to a new geographic market (change = 3.1%; standard error = 1.8%). Further, reported physicians were more likely to switch hospitals; the probability of practicing at the injury hospital fell by 5.2% percentage points after a malpractice report overall, with larger changes by the end of the study period (Panel A of Figure 3).

In Panel C of Table 4, we model changes in physicians' labor supply on the intensive margin, estimating changes in discharge volumes conditional on maintaining practice. Our overall estimate of labor supply reductions is entirely driven by a sizable intensive margin response to liability pressure. After malpractice allegations, reported physicians decreased their labor supply by about 80 patients per quarter on the intensive margin. We also find large decreases in discharge volumes at the injury hospital, conditional on maintaining practice in that location. These intensive margin labor supply patterns are plotted non-parametrically in Panel B of Figure 3.

One question raised by our results is whether physicians chose to reduce their workloads in response to increased malpractice risk, or if they faced reduced demand from hospitals that would normally employ their services. We cannot address this issue directly but provide two pieces of related evidence. First, we test whether physicians move away from relatively demanding practice environments, specifically EDs with relatively high discharge volumes per shift and EDs housed within teaching hospitals. Second, we test whether physicians move away from EDs housed within safety net hospitals, where the perceived risk of malpractice liability is high. The results of our analysis are in Panel D of Table 4. Our dependent variables track the share of physicians' practice at high volume hospitals, safety net hospitals and teaching hospitals. There is no evidence that physicians move systematically to different practice environments after a report.

A final issue is whether physicians decreased their labor supply because they were spending time defending themselves against the malpractice allegation. We cannot observe time costs directly in our data, but the magnitude of our labor supply decrease would imply sub-

stantially larger time costs than found in previous research, which estimates 2.7-5 days of lost work time per claim (Mello et al., 2010). Nevertheless, as described below, we provide indirect evidence that the decrease in labor supply is related to the resolution of the claim and the associated reduction in both time and psychic costs.

First, we test for differential effects of malpractice reports that vary in their time to claim resolution. In Panel E of Table 4, we split the reported physicians into separate treatment groups according to resolution time and model labor supply changes for each group relative to the full control group. Early resolution claims include those that were settled before or during the pre-suit period, as well as abandoned claims. We find that malpractice reports led to statistically different changes in labor supply across physicians facing early and late resolution claims; we estimate a 2% decrease in discharge volume (standard error = 4.5%) among early resolution claims and an 11% (standard error = 2.6%) decrease in labor supply among late resolution claims. In addition, we find that physicians with shorter claim durations experienced a sharp decrease in discharge immediately after the report, followed by a recovery in labor supply (Figure A13). Physicians with longer claims, on the other hand, were slower to decrease discharge volumes after a report but had no recovery in labor supply by the end of the study period. These results are consistent with our finding that labor supply rebounds somewhat after the final disposition of the case (Table 3 and Figure A11).

Overall, our results are most consistent with physicians taking on fewer shifts or working more slowly during any given shift. To put the size of this decrease in context, ED physicians typically work 3-5 shifts per week and individual shifts cover approximately 15 patients on average (Joseph et al., 2018; Silver, 2019).[38] A decrease of 78 discharges per quarter, therefore, equates to approximately 5 fewer shifts per quarter (out of about 48), or 1.5 fewer patients per shift.[39]

6 Effect of Malpractice Reports on Treatment Intensity

In this section, we study the effect of malpractice reports on the intensity of treatment provided by physicians. Figure 4 estimates the impact of malpractice reports on average costs per patient discharged, plotting unadjusted costs for reported physicians and controls over

[38] EM physician workloads vary across settings, but physician surveys suggest that full-time work requires approximately 30 clinical hours per week (Katz, 2014).

[39] We are not able to observe or construct shifts using our data, which do not include time stamps beyond year and quarter indicators. Working less has potential financial consequences for EM physicians, many of whom have at least some productivity-based compensation (Kane, 2015).

event time (Panel A) and DD estimates from our event study model (Panel B). Prior to the report, average costs and cost trends were similar between physicians with and without malpractice reports. After the report, average costs increased relatively more among physicians with malpractice reports.

Table 5 documents the overall effect of malpractice reports on costs in the two years after malpractice allegations.[40] Total costs increased by 4.3% among reported physicians after the report, relative to unreported physicians. In column 2, we include controls for the timing of the injury, suit filing and final disposition. We estimate an increase in costs of 3.2% after the report and smaller, statistically insignificant changes after other claim events. Column 3 shows results including physician time trends; the magnitude of our estimate decreases to 2.0% under this specification and is significant at the 10% level. As discussed previously, unit-specific time trends can over-control for time-varying treatment effects. If we use a two-step strategy to partial out group-specific time trends based on only pre-report data, analogous to the approach in Goodman-Bacon (2019), we estimate a treatment effect of 4.1%.[41] Further robustness checks are available in the Appendix (Figure A14); we estimate similar effects of malpractice allegations on treatment intensity when we model charges instead of costs (Panel A), add hospital fixed effects to the regression (Panel B), model linear costs rather than the log transformation (Panel C) and model costs using a one-part generalized linear model (GLM) with a log-link function (Panel D).

One concern about our treatment intensity analyses is that they are affected by patient selection by reported physicians, specifically that reported physicians avoid complicated and potentially expensive patients following a malpractice report. Given that we find little systematic change in discharge volumes across patient types (Panel A of Table 4), there is limited scope for such selection. Moreover, to the extent that reported physicians seek out straightforward, inexpensive cases after a report, our sample of remaining discharges will tend to grow less complex after the report and our estimates of the increase in costs will be attenuated.

6.1 Understanding the Increase in Treatment Intensity

A natural inquiry is to understand the types of care and patients for which physicians increase spending after a malpractice report. Panel A of Table 6 estimates the effect of malpractice reports on radiology costs and laboratory costs, as well as general ER costs, which vary across

[40] The sample in Table 5 includes one fewer physician than in Table 3. This physician does not oversee any discharges during the study period so they contribute to the labor supply estimates but not the treatment intensity estimates.

[41] This approach does not correct standard errors in the second stage.

discharges from screening services only to higher levels of care (AHCA, 2011). We estimate relatively large changes in laboratory costs following allegations. Malpractice reports led to a 0.8 percentage point, or 1.7%, increase in the probability of receiving laboratory treatment. Conditional on having any laboratory services, laboratory costs increased by 4.2%. These results are consistent with a larger role for defensive medicine in diagnostic care, where risks associated with providing that care are low (Baicker, Fisher and Chandra, 2007; Frakes, 2015; Frakes and Gruber, 2019).

Panels B and C examine the impacts of malpractice reports on treatment intensity by two measures of clinical severity. The first, shown in Panel B, sorts presenting conditions by the low risk, high risk, and uncertain risk categories discussed previously (Section 5.1). There are statistically similar increases in costs across all three risk groups. Panel C considers conditions divided according to the coefficient of variation in costs per discharge within a CCS group-payer pair. The intuition behind this analysis is that the coefficient of variation may serve as a proxy for the extent to which care is protocolized (when the coefficient of variation may be low) versus subject to clinical uncertainty and varying treatment approaches (when the coefficient of variation may be high). Responses to malpractice reports are statistically similar across the coefficient of variation quartiles. One concern about this analysis is that we are missing patients who are admitted to the hospital. To the extent that reported physicians are differentially increasing admission rates among high-risk patients, relative to low risk patients, our results will understate the relationship between clinical severity and treatment intensity. Our results in Table 4, however, suggest the scope for such selection is small.

Lastly, in Panel D, we examine the impact of malpractice reports on rates of diagnostic imaging, studying whether testing rates changed differentially across high- and low-value services. There is little evidence that increases in treatment intensity were limited to high-value care. We find no change in the use of high-value chest imaging after malpractice reports. We estimate no change in the use of low-value lumbar imaging and a marginally insignificant 1.9 percentage point (standard error = 1.0), or 4.5% increase, in the use of low-value head imaging tests. Non-parametric event study results suggest that the increase in low-value head imaging began prior to the malpractice reports (Figure A16, Panel A). We draw a similar conclusion using an alternate sample restriction to account for the change in CPT coding across data versions (Figure A16, Panel B).

7 Heterogeneous Effects of Malpractice Reports

In this section, we examine heterogeneous responses to malpractice reports. Of particular importance is whether physicians treat patients more intensively if they are clinically similar to the patient who filed the report. To study these dynamics, we identify discharges that are clinically similar and dissimilar to the claimant in each physician's case. For example, if a physician was accused of malpractice for their treatment of a pregnant woman, we compare their responses among subsequent pregnant patients versus all other patients. For the reported physicians, the clinical category corresponds to the patient injury in their actual claim. For the unreported physicians, the clinical category corresponds to the claim of their matched, reported physician. Panel A of Figure 5 and Table A1 examine the impact of malpractice reports across clinically similar and dissimilar patients. Treatment intensity responses to malpractice reports are statistically similar across clinical similarity groups. We likewise find that physicians decreased labor supply proportionately among patients that resemble the patient in their case. There is no evidence that physicians "learned" to avoid patients who are most like the one for which the claim was filed.

We examine responses to malpractice reports by other measures of heterogeneity as well. In Panels B-F of Figure 5, we split the reported physicians into separate treatment groups according to the characteristic of interest and estimate changes relative to the full control group. Panel B considers whether physician responses to malpractice reports were greater when more money was paid, generally an indicator of greater negligent damage.[42] There are statistically similar decreases in labor supply among physicians facing claims that resolved in their favor without a payment to the claimant, relative to physicians facing claims resulting in large payments to the claimant. Treatment intensity responses are likewise large and statistically significant even among physicians with $0 claims. Panel C sorts claims by the severity of patient injury: permanent or temporary. We find no meaningful difference in spending or labor supply responses of reported physicians across injury severity.

In Panels D-F, we examine whether the impact of malpractice reports varies according to physician characteristics, rather than claim characteristics. Panel D considers physicians divided by tenure. Labor supply responses are statistically similar across tenure groups, although the estimated change in discharge volume is relatively small for physicians with less than 10 years of practice. Treatment intensity effects are likewise similar for physicians with varying experience levels. In Panel E, we find no differential responses to malpractice across female versus male physicians.

[42] Studdert et al. (2006) find that 73% of meritorious claims result in compensation and 28% of frivolous claims result in compensation.

Lastly, in Panel F, we test for differential changes in behavior among potential poor performers: physicians with multiple malpractice allegations made against them.[43] There are no meaningful differences in labor supply or spending among physicians with repeated claims, relative to physicians with a single claim. Next, we use an alternative definition of potential "bad apple" physicians: those with multiple *paid* claims (Studdert et al., 2016). In Table A1, we find no change in labor supply among physicians with multiple paid claims, but a large decrease among physicians without multiple payments; these labor supply changes are different from each other (p = 0.037), suggesting that physicians' labor supply responses to malpractice allegations may attenuate with repeated exposure. Treatment intensity responses are large and statistically similar across the subgroups.

8 Discussion and Conclusion

Our results show that malpractice allegations led physicians to decrease their labor supply and to increase treatment intensity for patients discharged from the ED. Thus, we provide evidence that malpractice pressure led simultaneously to positive and negative responses in physician behavior. For the *individual, reported physician*, these two effects are offsetting. A 9.3% reduction in cases seen, coupled with a 4.3% increase in costs per case, leads to a net decrease in resource use of approximately 5.5% (standard error = 2.8%) after allegations.[44]

In an analysis of the aggregate spending change among *all physicians*, the reduction in labor supply is not a reduction in costs per se, assuming the same number of patients go to the ED.[45] Reported physicians may see fewer patients, but the patients they do not see will be treated by other physicians. For these cases, the change in aggregate spending depends on the differential treatment patterns of reported versus unreported physicians. Prior to the malpractice allegations, reported physicians treated patients more intensively than their matched peers. The difference in spending was a statistically insignificant 1%

[43] By definition, we estimate the effect of repeated claims among physicians who survive to have a second report, who may be different from other physicians in unobservable ways. If the system weeds out poor performers through the first claim, for example, then physicians with repeat claims in our sample could be higher quality. Two facts from our analysis suggest the scope for such selection is small. First, we estimate small, statistically insignificant extensive margin effects of malpractice allegations (Table 3). Second, we find statistically similar decreases in labor supply across physicians with $0 claims and high payout claims (Figure 5, Panel B). Nevertheless, we note this potential selection as a caveat to our analyses.

[44] The percent decrease equals to $1 - [(1 + \hat{\beta}^{labor}) * (1 + \hat{\beta}^{costs})]$, where $\hat{\beta}^{labor}$ is our estimate of the labor supply decline and $\hat{\beta}^{costs}$ is our estimate of the treatment intensity increase.

[45] We find little evidence that faster accumulation of reports within an ED was associated with decreases in discharge volume (Table A2). EDs experienced an average of 3 malpractice reports against EM specialists during our study period (Figure A17). As shown in Figure A10, hospitals with fewer reports tended to be smaller, with similar report rates as larger hospitals. Approximately 60 out of 220 EDs had no reports against EM specialists, although they had reports against physicians in other specialties.

greater intensity in the pre-report period, after controlling for hospital fixed effects, calendar time fixed effects and patient characteristics (Table A3).[46] Taking this point estimate into account, the net effect of the malpractice reports on aggregate spending is the 4.3% increase for patients who are seen by the reported physicians less the 1% decline for patients who are seen by other physicians, for a net change of about 3.8% (standard error = 1.4%).

Patients who were seen by the reported physicians in the pre-report period account for approximately 30% of discharges (Table 1). Thus, the net effect of all malpractice claims is a 1.1% increase in ED spending overall. Our results, therefore, suggest that malpractice accusations can have an important influence on medical practice patterns, though the associated dollar amount is relatively modest. These calculations are sensitive to the assumption that patients are reallocated from reported physicians to unreported physicians with similar spending patterns, as in our matched sample. Our estimates would be notably larger if patients were reallocated to inpatient physicians, for example.[47]

A natural question is how our estimates compare to the previous literature. Existing research studying the impact of tort reforms on claim frequency is mixed. On the high end, recent evidence from the CBO (2019) suggests that damage caps reduce malpractice claims by about 20%. Our results imply that a 20% reduction in claims would lead to a reduction in ED costs of 0.2%.[48] Existing work on tort reforms and spending is likewise mixed, with recent reviews cautioning against firm conclusions; Mello and Kachalia (2016) and CBO (2019) both argue that existing evidence does not support any conclusion about the impact of non-economic damage caps on overall health care spending. Earlier meta-analyses estimated modest impacts of defensive medicine on spending at 2% (Mello et al., 2010) and 0.3% (CBO, 2009).[49] Our results likewise suggest a modest change in overall spending, but imply a larger role for malpractice liability in influencing physician behavior.

Our results are suggestive of defensive medicine, but do not prove it. Defensive medicine is generally defined as additional care where the value of the care is less than the cost. In this case, it is hard to measure the value of all additional care provided. However, several

[46] We model pre-report spending differences in the full pre-period and the quarter before the report, estimating insignificant differences of 0.7% and 0.8%, respectively.

[47] In 2005, spending on inpatient admissions that originated from the ED was about 10 times higher than spending on ED discharges, controlling for hospital fixed effects, calendar time fixed effects and patient characteristics. If 10% of the labor supply decrease could be explained by an increase in hospital admissions, our estimated net change in spending would approximately triple.

[48] With a 20% reduction in claims, our results imply that malpractice reports would affect approximately 24% of all discharges and lead to a 0.9% increase in ED spending (3.8%*0.24)

[49] These analyses mainly relied on variation in spending from tort reforms. The seminal papers on this topic (Kessler and McClellan, 1996, 2002) estimated decreases in spending on the order of 5%, while some of the strongest recent evidence suggests that damage caps actually *increase* spending by about 2% (Paik, Black and Hyman, 2017).

factors suggest that the added value is likely small. We find that physicians respond even to frivolous allegations, implying that malpractice allegations have effects outside of deterring low quality care. In addition, care increases for all patients, not just those similar to the case for which the physician was sued. Finally, we find modest evidence of increases in low-value care, specifically diagnostic radiology tests that have little benefit for patients.

Our results suggest several areas for future work. First, we have not attempted to assess spillover effects of malpractice allegations on other physicians. Even physicians who are not accused of malpractice may change their behavior in response to an accusation of a peer, for example if they learn about their colleague's legal issues. Second, we show that physicians tend to leave the hospital where the patient injury occurred, but we cannot say whether this was the physician's decision or the hospital's decision. It would be useful to study how malpractice pressure influences the physician-hospital relationship and other hospital management decisions. Third, we study the impact of malpractice allegations in a specific clinical area and the results may not be applicable to other contexts.

As malpractice reforms continue to stir debate, our results suggest that malpractice allegations can have a meaningful impact on physician behavior, leading to fewer cases seen and more intensive care for each patient. These results raise the possibility that more recent proposals for malpractice reform, such as safe harbors for physicians that follow clinical guidelines, may be more promising than damage caps if they change the claim process.

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Tables and Figures

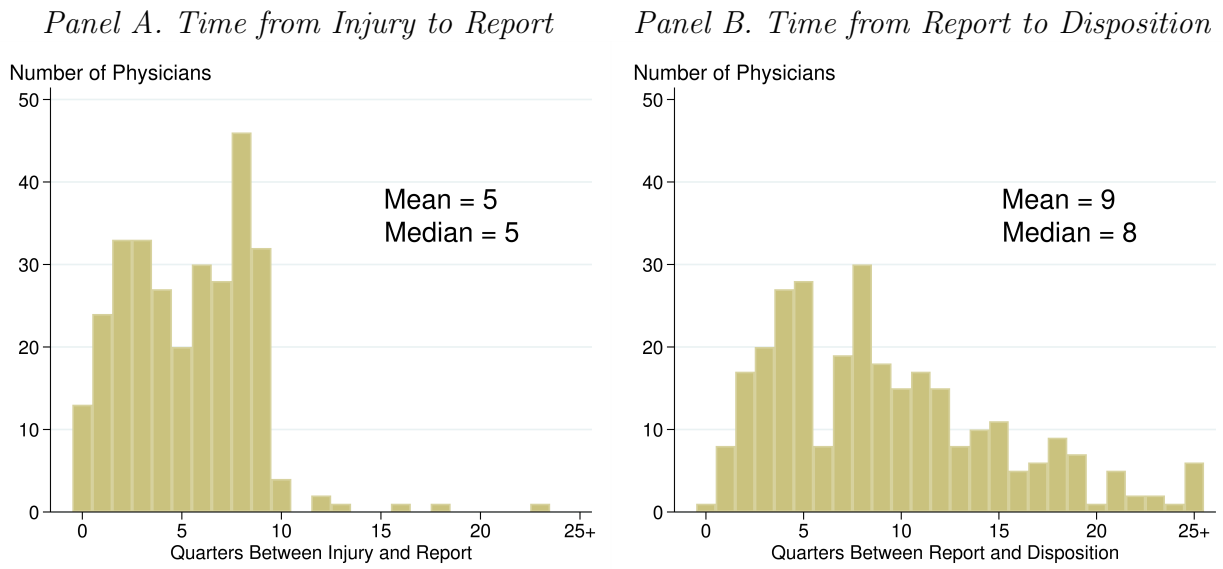


Figure 1: Time Between Patient Injury, Malpractice Report and Claim Disposition

Notes: These figures plot histograms of the time elapsed between key events in malpractice claim development. Panel A shows the quarters elapsed between the patient injury and the malpractice report. Panel B shows the quarters elapsed between the malpractice report and the final disposition of the claim. The sample covers malpractice reports against Emergency Medicine specialists. For scaling purposes, the horizontal axis is truncated at 25 quarters.

Table 1: Characteristics of Physicians With and Without Malpractice Reports, Before and After Propensity Score Match

	All Physicians		Eligible for Match		Matched Sample		
	Reported	Unreported	Reported	Unreported	Reported	Unreported	Δ
Number of Physicians	684	2039	297	1071	296	433	
(%)	(25%)	(75%)	(22%)	(78%)			
A. Dependent Variables							
Discharge Volume	771.3	623.3	820.6	752.6	821.0	703.0	-118.9***
Costs/Discharge	625.4	657.5	619.5	603.2	620.3	596.9	-23.4
B. Physician Characteristics							
Tenure	18.1	15.6	18.6	16.9	18.5	19.2	0.6
MD (%)	78.7	80.9	78.5	81.1	78.7	80.2	1.7
Female (%)	18.7	26.0	19.2	22.2	19.3	18.8	-0.5
C. Hospital Referral Region (%)							
Northern	19.4	28.6	19.5	28.4	19.6	19.6	0.0
Central	43.6	33.3	49.5	34.9	49.3	49.3	-0.1
Southern	37.0	38.1	31.0	36.7	31.1	31.1	0.1
D. Hospital Characteristics (%)							
Teaching	3.7	10.7	4.7	9.8	4.7	4.9	0.2
Safety Net Hospital	25.1	27.7	24.6	27.1	24.7	23.6	-1.0
High-Volume Hospital	39.6	49.4	44.4	51.3	44.6	49.0	4.3

Notes: This table reports summary statistics for Emergency Medicine specialists who were reported for malpractice and their unreported peers. The statistics are calculated using data from 2005 to 2013. The table reports means where the unit of observation is a physician-quarter, using data from the quarter before the (real or placebo) report. In the pre-match samples, statistics for unreported physicians are calculated using data from the chronological midpoint of their practice. Physicians are eligible for the propensity score match if we can observe them for two years surrounding their (real or placebo) report, as described in Section 3.3. Costs are reported in 2009 dollars. Hospital characteristics correspond to traits of the facilities where physicians oversaw the plurality of their discharges. Statistical significance of differences between reported and unreported physicians in the matched sample indicated as follows: * p<.05; ** p<.01; *** p<0.001.

Table 2: Characteristics of Malpractice Claims, Before and After Propensity Score Match

	Reported Physicians		
	All Physicians	Eligible for Match	Matched Sample
Number of Physicians	684	297	296
A. Physician Malpractice History			
Any Previous Report (%)	26.0	26.6	26.7
Previous Report Count	1.4	1.4	1.4
Any Subsequent Report (%)	29.1	26.3	26.4
Subsequent Report Count	1.3	1.2	1.2
B. Report Characteristics			
Suit Filed (%)	69.6	72.4	72.3
Payment to Defendant (%)	58.2	62.0	61.8
Non-zero Payment ($1000s)	361.1	220.8	221.4
Defense Costs ($1000s)	61.4	54.0	54.2
C. Claim Duration (Quarters)			
Injury to Disposition	14.1	14.5	14.5
Injury to Report	5.2	5.3	5.3
Report to Suit	2.3	2.3	2.4
Report to Disposition (without Suit)	4.3	4.1	4.1
Report to Disposition (with Suit)	10.9	11.2	11.2
D. Clinical Injury Category (%)			
Chest	17.7	20.2	19.9
Head	12.4	15.2	15.2
Trauma/Injury	6.6	10.8	10.8
Abdomen	7.7	9.4	9.5
Back	7.3	10.1	10.1
E. Severity of Injury (%)			
Temporary	22.5	20.9	20.9
Permanent	77.5	79.1	79.1
F. Injury Hospital Characteristics (%)			
Teaching Hospital	5.2	6.7	6.8
Safety Net Hospital	25.7	24.0	24.4
High Volume Hospital	41.1	48.1	48.8

Notes: This table reports summary statistics for malpractice reports against Emergency Medicine specialists. The table reports means where the unit of observation is a physician-quarter. The statistics are calculated using data from 2005 to 2013. Costs are reported in 2009 dollars.

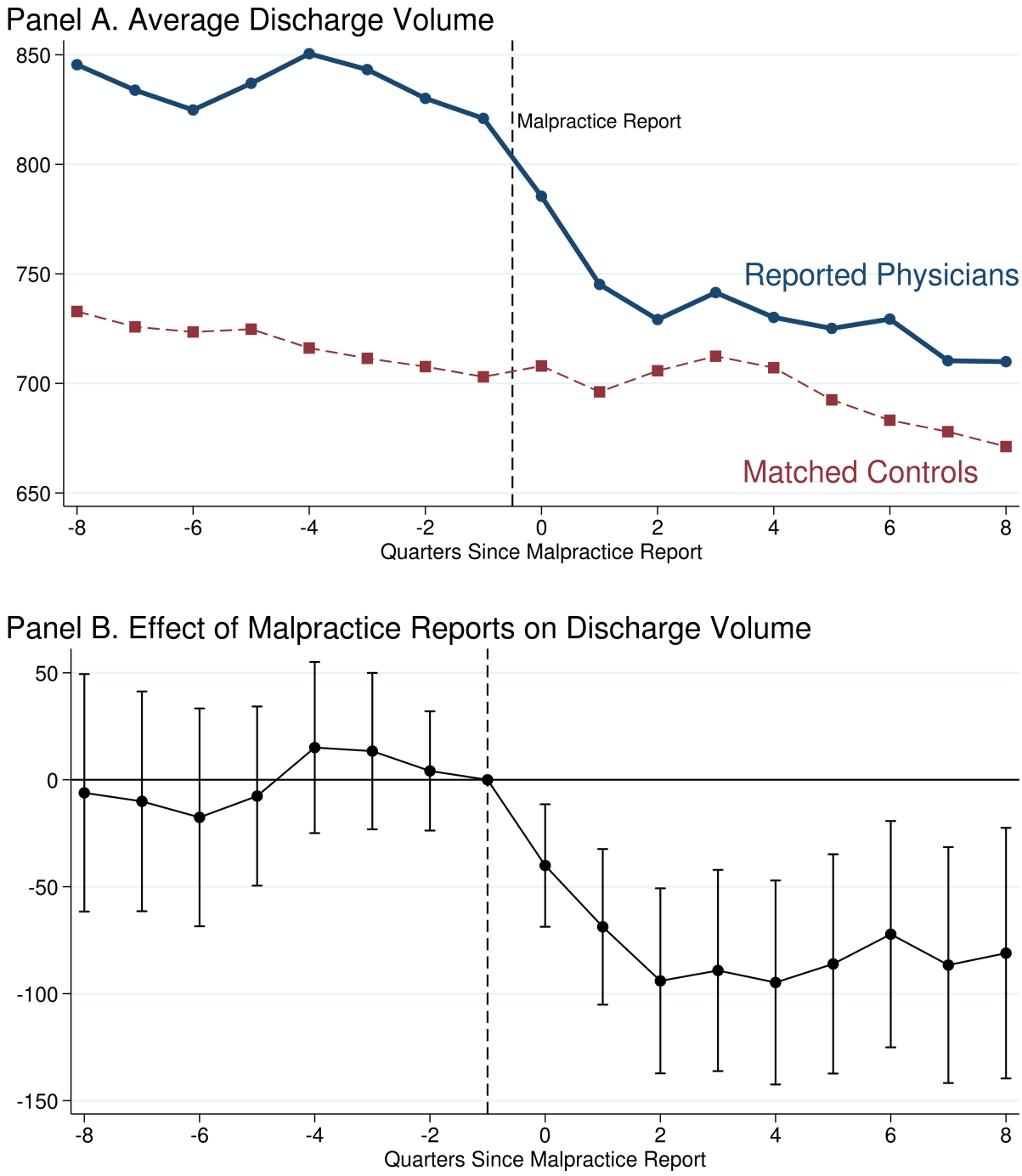


Figure 2: Discharge Volume Before and After Malpractice Reports

Notes: These figures plot event studies of discharge volume for reported and unreported physicians in the two years before and after malpractice reports (real or placebo). Panel A shows average discharge volumes for reported and unreported physicians in each quarter. Panel B plots our difference-in-differences estimates (β_k from Equation 1), showing the effect of malpractice reports on discharge volume in the k quarters before and after the report. Regression analysis includes physician fixed effects, physician tenure and calendar time fixed effects. Standard errors are clustered at the physician level.

Table 3: Effect of Malpractice Reports on Discharge Volume

	Discharge Volume		
	(1)	(2)	(3)
Reported*Post	-78.03***	-68.06**	-76.62***
	(21.62)	(20.97)	(18.76)
Injured*Post		14.80	
		(25.93)	
Sued*Post		-50.50	
		(26.16)	
Disposed*Post		45.26	
		(27.14)	
Observations	15079	15079	15079
Claim Timing Controls		X	
Physician*Time Trends			X
Physicians	729	729	729
Percent Change	-9.3	-8.1	-9.2
Mean Discharge Volume	836	836	836

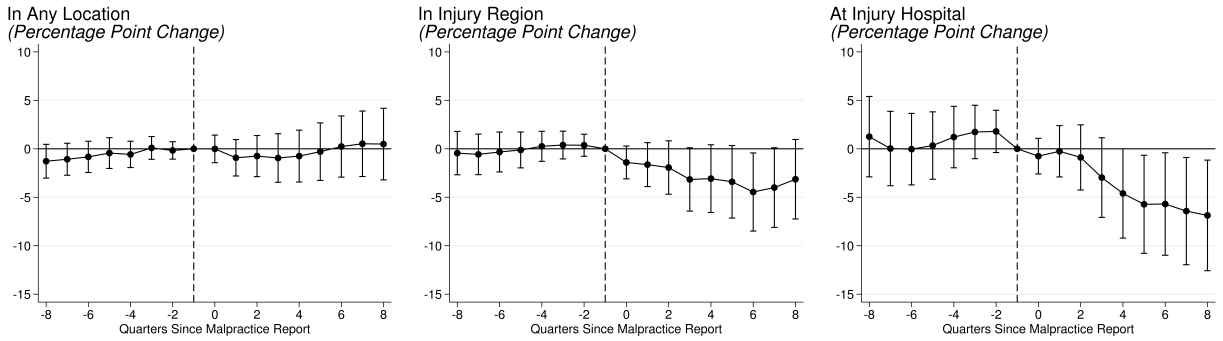
Notes: This table presents difference-in-differences estimates of the effect of malpractice reports on physicians' discharge volumes per quarter. Each column reports coefficients from a separate regression, with standard errors clustered by physician in parentheses. All regressions include physician fixed effects, calendar time fixed effects, and controls for physician tenure. * p<.05; ** p<.01; *** p<0.001.

Table 4: Effect of Malpractice Reports on Physician Labor Supply: Decomposition by Margin

	Coefficient	Standard Error	Observations	Physicians	Dependent Variable Mean	Percent Change
A. Discharge Volume by Risk Type						
High Risk	-8.51**	2.59	15,079	729	95	-8.9
Low Risk	-20.88**	6.66	15,079	729	237	-8.8
Uncertain Risk	-10.93***	2.71	15,079	729	90	-12.2
B. Probability of Any Practice						
In Any Location	0.003	0.013	15,079	729	1.00	0.3
In Injury Region	-0.028	0.016	13,958	708	0.89	-3.1
At Injury Hospital	-0.044*	0.022	13,958	708	0.86	-5.2
At Non-Injury Hospital	0.029*	0.013	13,958	708	0.81	3.5
C. Discharge Volume Given Practice						
Any Location	-79.73***	19.23	14,545	728	838	-9.5
In Injury Hospital	-86.47***	24.08	11,787	664	670	-12.9
In Non-Injury Hospital	-4.04	25.65	10,576	572	324	-1.2
D. Hospital Characteristics						
Teaching Hospital	-0.003	0.009	14,219	728	0.05	-6.5
Safety Net Hospital	0.009	0.017	14,219	728	0.24	3.6
High Volume Hospital	0.015	0.019	14,219	728	0.43	3.6
E. Discharge Volume by Claim Resolution Time						
Late Resolution	-93.71***	23.39	14,076	670	846	-11.1
Early Resolution	-13.28	36.16	11,050	492	795	-1.7

Notes: This table presents difference-in-differences estimates of the effect of malpractice reports on physician labor supply. Each row reports coefficients from a separate regression. Panel A estimates changes in discharge volume across patients with varying clinical risk. Panels B and C decompose the labor supply change into extensive margin (Panel B) and intensive margin (Panel C) effects. Panel D estimates the share of physicians' practice at different hospital types. Panel E estimates changes in discharge volume separately for claims with early versus late resolutions. All regression analyses control for physician fixed effects, physician tenure and calendar time fixed effects. Standard errors are clustered at the physician level. * p<.05; ** p<.01; *** p<0.001

Panel A: Probability of Any Practice (Extensive Margin)



Panel B: Discharge Volume Given Practice (Intensive Margin)

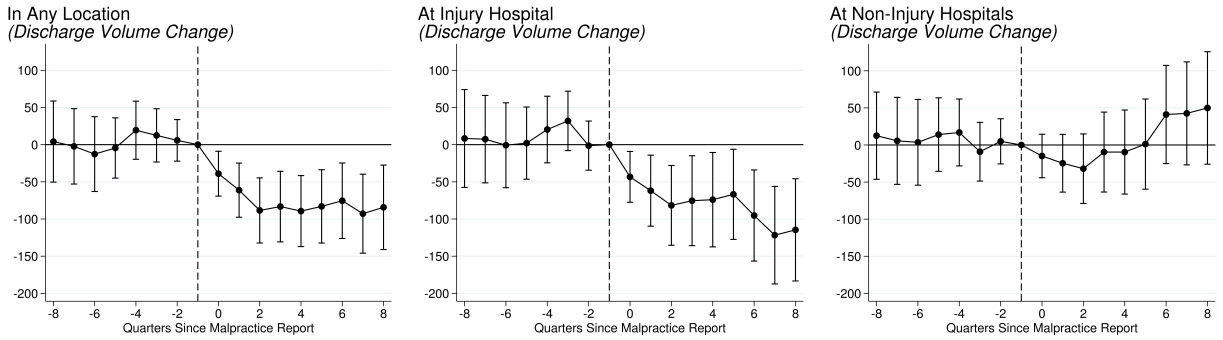


Figure 3: Effect of Malpractice Reports on Physician Labor Supply: Decomposition by Margin

Notes: These figures show the effects of malpractice reports on physicians' labor supply. Panel A plots changes in labor supply on the extensive margin, estimating whether reported physicians are less likely to continue practicing (1) in any location, (2) in the same hospital referral region where the patient injury occurred or (3) in the same hospital where the patient injury occurred. Panel B shows changes in labor supply on the intensive margin, estimating whether reported physicians reduce the volume of discharges they oversee across locations, conditional on maintaining practice in that location. All regressions include physician fixed effects, physician tenure and calendar time fixed effects. Standard errors are clustered at the physician level.

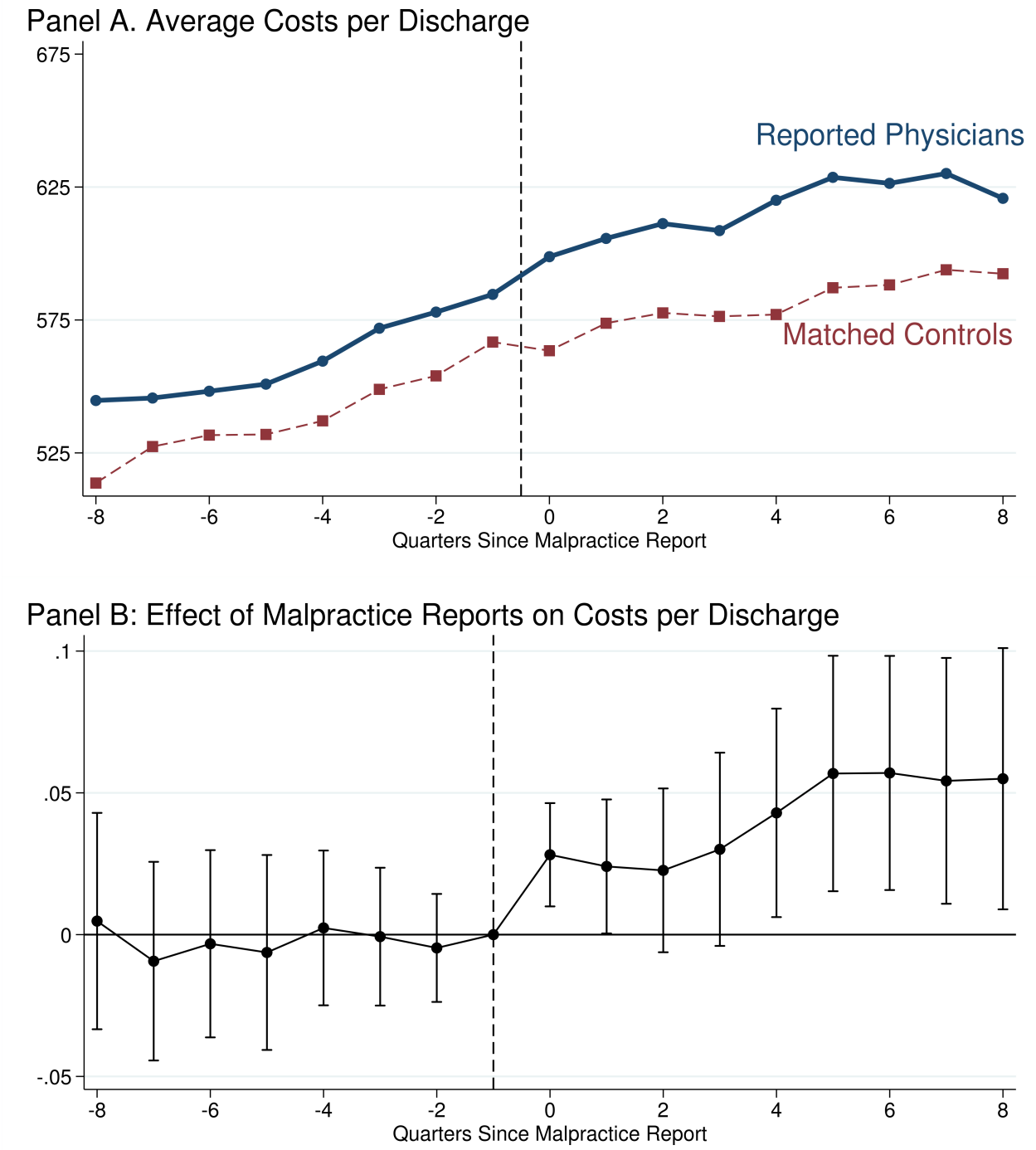


Figure 4: Costs per Discharge Before and After Malpractice Reports

Notes: These figures plot event studies of costs per discharge for reported and unreported physicians in the two years before and after malpractice reports. Panel A shows average costs per discharge for reported and unreported physicians in each quarter. Panel B plots our difference-in-differences estimates, showing the effect of malpractice reports on costs per discharge in the 8 quarters before and after the report. Costs are modeled in logs. Regression analysis includes physician fixed effects, physician tenure and calendar time fixed effects, as well as controls for patient age, sex, race, presenting condition, visit day and insurance status. Standard errors are clustered at the physician level. Costs are reported in 2009 dollars.

Table 5: Effect of Malpractice Reports on Costs per Discharge

	Ln(Costs)		
	(1)	(2)	(3)
Reported*Post	0.0426**	0.0324*	0.0203
	(0.0155)	(0.0142)	(0.0121)
Injured*Post		-0.0118	
		(0.0177)	
Sued*Post		0.0281	
		(0.0202)	
Disposed*Post		0.00213	
		(0.0238)	
Observations	10954628	10954628	10954628
Claim Timing Controls		X	
Physician*Time Trends			X
Physicians	728	728	728
Mean Costs	560	560	560

Notes: This table presents difference-in-differences estimates of the effect of malpractice reports on costs per discharge. Each column reports coefficients from a separate regression, with standard errors clustered by physician in parentheses. All regressions include controls for physician fixed effects, calendar time fixed effects and physician tenure, as well as patient age, sex, race, presenting condition, visit day and insurance status. Costs are reported in 2009 dollars. * p<.05; ** p<.01; *** p<0.001.

Table 6: Effect of Malpractice Reports on Treatment Intensity: Decomposition by Margin

	Coefficient	Standard Error	Observations	Physicians	Dependent Variable Mean
A. Service Category					
Any Radiology Costs	0.015**	0.005	10,954,628	728	0.46
Ln(Radiology Costs)	-0.024	0.017	5,112,233	728	422
Any Laboratory Costs	0.008*	0.003	10,954,628	728	0.46
Ln(Laboratory Costs)	0.042*	0.018	5,183,163	728	217
Ln(Emergency Room Costs)	0.039*	0.018	10,899,092	728	183
B. Ln(Costs) by Discharge Complexity					
Low	0.043*	0.017	1,616,238	728	417
High	0.041*	0.017	745,612	728	694
Uncertain	0.054***	0.016	2,165,290	728	866
C. Ln(Costs) by Protocol Uncertainty					
Bottom Quartile	0.033*	0.015	2,719,659	728	912
Middle Quartiles	0.050**	0.015	5,617,291	728	484
Top Quartile	0.039	0.020	2,617,678	728	349
D. Diagnostic Imaging Tests					
High-Value Chest Imaging Test	-0.017	0.017	38,262	558	0.69
Low-Value Lumbar Imaging Test	0.011	0.008	264,292	579	0.31
Low-Value Head Imaging Test	0.019	0.010	215,035	579	0.42

Notes: This table presents difference-in-differences estimates of the effect of malpractice reports on physicians' treatment intensity. Each row reports estimates from a separate regression. We study changes in treatment intensity across service types (Panel A), discharge complexity (Panel B), the certainty of treatment protocol (Panel C), and high-value versus low-value care (Panel D). All regression analyses include controls for physician fixed effects, physician tenure and calendar time fixed effects, as well as patient age, sex, race, presenting condition, visit day and insurance status. Standard errors are clustered at the physician level. Costs are reported in 2009 dollars. * p<.05; ** p<.01; *** p<0.001.

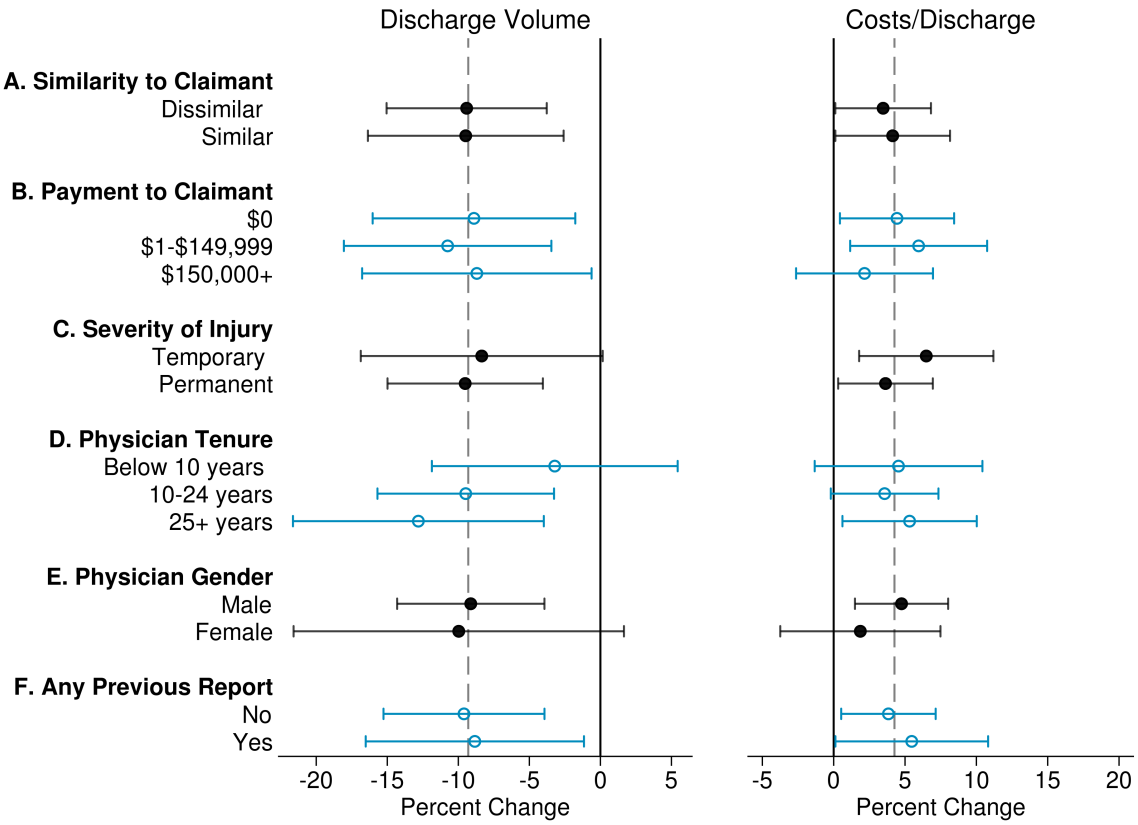


Figure 5: Heterogeneous Effects of Malpractice Reports

Notes: This figure presents difference-in-differences estimates of the effect of malpractice reports across different claim types (Panels A-C) and physician characteristics (Panels D-F). Each row reports estimates from a separate regression. Panel A uses the subset of data where claimant similarity is well defined. Panels B-F use data from the full analytic sample. All regression analyses include controls for physician fixed effects, physician tenure and calendar time fixed effects; analyses of costs per discharge also control for patient age, sex, race, presenting condition, visit day and insurance status. Costs are modeled in 2009 dollars. Standard errors are clustered at the physician level.

Appendix

A1 Sample Construction

Our analysis uses Emergency Department (ED) discharge data between 2005 and the third quarter of 2013. This data covers approximately 56 million discharges. We focus our attention on discharges that were overseen by physicians, rather than nurses or other provider types. The overwhelming majority of discharges in our sample are overseen by physicians and this inclusion criteria reduces our sample size by less than 2%.

We merge the ED data with information on physician characteristics from two sources: the Florida Department of Health (DOH) Practitioner Profile data and Doximity data. We use four snapshots from the 2014 Florida DOH Practitioner data. First, the Licensee Profile Master Table provides information on the year that each provider began practice. Second, the Education History Supplement describes medical school graduation and attendance dates. Third, the Certifications Supplemental File identifies physicians' specialty certifications. Fourth, we get additional information about specialty from the Professional & Postgraduate Training Supplemental File, specifically whether each physician did a postgraduate program in Emergency Medicine (EM). The Doximity data includes information about physician specialty, tenure and gender. Doximity identifies physicians by National Provider Identifier (NPI), which we link to Florida state license numbers using the NPI to State License Crosswalk dataset available through the National Bureau of Economic Research (NBER), as well as crosswalk information that is available in later years of discharge data.

We categorize physicians as EM specialists if they have an EM specialization listed in the Certifications Supplemental File, the Training Supplemental File or in Doximity. We define tenure as the number of years elapsed from the following dates, in sequence: (1) final medical school attendance date in the DOH data, (2) medical school graduation date from the DOH data and (3) medical school graduation date from Doximity. We categorize physician gender using the Doximity data and the Florida DOH Practitioner Profile Master Table, matching first names with gender frequency from the Social Security Administration. When we cannot identify specialty, tenure or gender from the sources above, we hand code physician characteristics based on online public profiles.

We restrict our sample to EM specialists; these physicians oversee the vast majority of care in the ED, accounting for approximately 80% of discharges in our sample (Figure A2). Non-EM specialists oversee the highest share of patients at low-volume EDs.

A2 Malpractice Claims Data

We use closed malpractice claims from the Florida Office of Insurance Regulation (FLOIR). We merge this data with the ED discharge data according to the reported physician's license number and the date of the malpractice report. We take a number of steps to clean the FLOIR data. First, a small minority of physicians in the malpractice data have multiple reports listed on the same day. Such multiple claim records can occur, for example, if a physician is covered by two insurance companies during their claim. For physicians with multiple reports on the same day between 2005 and 2013, we clean the data using the following steps. First, if the claim records match in all fields except for payments (N=30), we sum payments together to create a single report.[50] Second, if cases match except for the payments and disposition date (N=5), we verify that the claims were filed by separate insurers, then aggregate payments to most recent filing to create a single report. After completing these steps, there were 6 remaining physicians with multiple reports on the same day. These claims had a variety of differences across records and we hand coded information to create a single report. For example, some claims were filed prematurely, then reopened once a suit was filed, sometimes with a different insurer. In these cases, we create a single report using the nonmissing suit date, the disposition date from the latest filing, summed payments, and nonmissing data from either field where available.

After cleaning records among physicians who have multiple reports listed on the same day, we turn to physicians who have multiple reports on two days in the same quarter (N=11). These records pose a challenge because our merge with the ED discharge data requires that we have unique records at the physician-report quarter level. While the quarter of the report is well-defined for these physicians, the two reports may vary in their payment levels and other claim characteristics. For these physicians, we define the injury and suit dates to the be the earlier across the two reports and we define the disposition date to be the later date. We define the payment levels according to the maximum insurer payment and legal fee across the two claims. In the event that the same-quarter reports have different injury hospitals or clinical characteristics, we redefine the value to be null.

We take two other minor steps to clean the data. First, we count missing values for indemnity payments and defense costs as zeros. Defense costs include loss adjustment expenses paid to the defense counsel and do not include standard contingency fees. Second, there is one malpractice case that reports defense costs of over $20 million. This amount

[50] Payment fields include legal fees as well as the indemnity payment made to the claimant. Other fields include the injury, suit and disposition date, the injury location, the severity of the injury and the clinical type of the injury.

is over 20 times the next highest value among all malpractice claims in our sample and far exceeds the indemnity payment in this case of approximately $600,000. As such, we recode the defense costs to missing for this case.

A2.1 Categorization of Malpractice Claims into Clinical Groups

We categorize malpractice claims into clinical groups according to the description of the patient injury. This categorization requires two steps (Figure A1). First, we categorize claimant injuries into clinical groups using a key phrase search. Each clinical group broadly corresponds to a system of the human body. For example, the Head clinical group includes claims with the key phrases "headache" and "stroke." It also includes key phrases that point to non-specific head conditions such as "neurological assessment." Some injury descriptions contain only key phrases that point to non-specific symptoms such as "respiratory distress" or "nausea." We generally do not assign these non-specific key phrases to clinical groups. We make an exception for "slurred speech," which is strongly associated with stroke and therefore included in the Head category. Overall, we are able to assign approximately 80% of claimant injuries to clinical groups in our final analytic sample.

Some malpractice claims correspond to multiple clinical groups, for example a claimant presenting with both chest pain and headache. We assign such claims to clinical groups using a step-wise hierarchy, creating a one-to-one matching from malpractice claims to clinical groups. We assign claims to the following clinical groups in sequence: Pregnancy, Circulatory, Respiratory, Head, Abdomen, Back, Infection, Injury, Psychiatric and Male reproductive system. Injuries associated with the head and back are assigned to those clinical groups, respectively. Injuries not associated with the Head or Back clinical groups (e.g. "hand injury") are assigned to the Injury clinical category.

The second step is to match each clinical injury group to related ED discharges. We link claims to related discharges using Clinical Classification Software (CCS) from the Healthcare Cost and Utilization Project (HCUP). Under the CCS, single level CCS codes group diagnoses into approximately 300 mutually exclusive groups (e.g. acute myocardial infarction, essential hypertension). Multi-level CCS codes assign each of the single-level CCS groups to broader clinical categories (e.g. diseases of the heart).

For each key phrase that points to a specific condition, we match to discharges in the associated multi-level CCS group. For example, the key phrase "stroke" is matched to the multi-level CCS category for cerebrovascular disease. Some key phrases point to a condition in the multi-level CCS group for symptoms, signs, and ill-defined conditions. In these cases, we match the key phrase to diagnoses under the single-level CCS, rather than all ill-defined

conditions. If the key phrase points to a non-specific condition ("neurological assessment"), we do not link to a CCS code. For key phrases that point to a symptom ("slurred speech"), we match to related codes from the International Classification of Diseases, Ninth Revision (ICD-9).

We match injury-related key phrases in a different manner. When a key phrase indicates an injury, we link it to fractures, wounds and other injury types in the same clinical group using both CCS codes and ICD-9 codes. For example, the key phrase "head inj" is matched with CCS codes for skull and face fractures (228) and intracranial injury (233), as well as ICD-9 codes for nonspecific head injuries (959.0x) and open wounds of the head (870.xx-874.xx). For the Injury category, related diagnoses include trauma-related joint disorders (225), fractures other than head and spine (226, 229, 230, 231 except spine), open wounds of the extremities (236), superficial injuries (239), sprains and strains (232), crushing injury (234) and nonspecific minor injuries (959.2x-959.7x).

Importantly, we aggregate related discharges to the clinical group level. For example, the Head clinical group contains key phrases that match the multi-level CCS groups for headache and cerebrovascular disease. For a physician with the key phrase "migraine," their similar discharges include diagnoses that fall into either multi-level CCS category, even though their key phrase points to only one (headache).

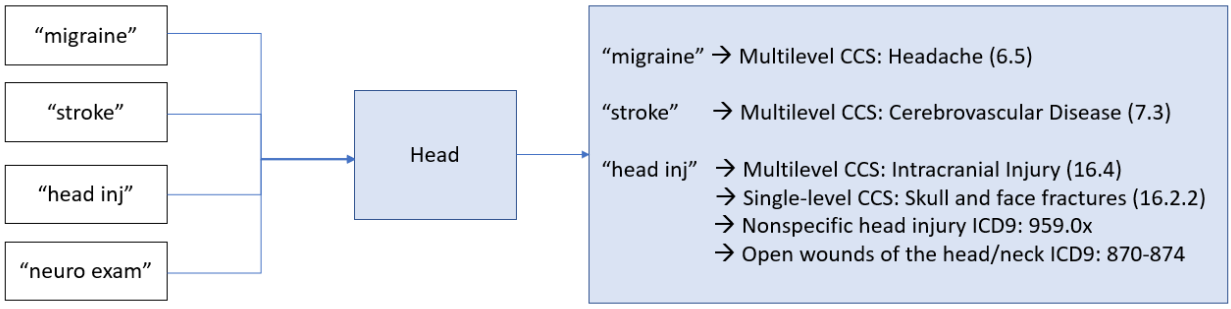


Figure A1: Claim Categorization Example

Notes: This figure shows an example of how malpractice claims are assigned to clinical groups and matched related discharges. This example is illustrative and not complete; the "Head" category contains more key phrases and links to additional related diagnoses.

A3 Appendix Figures

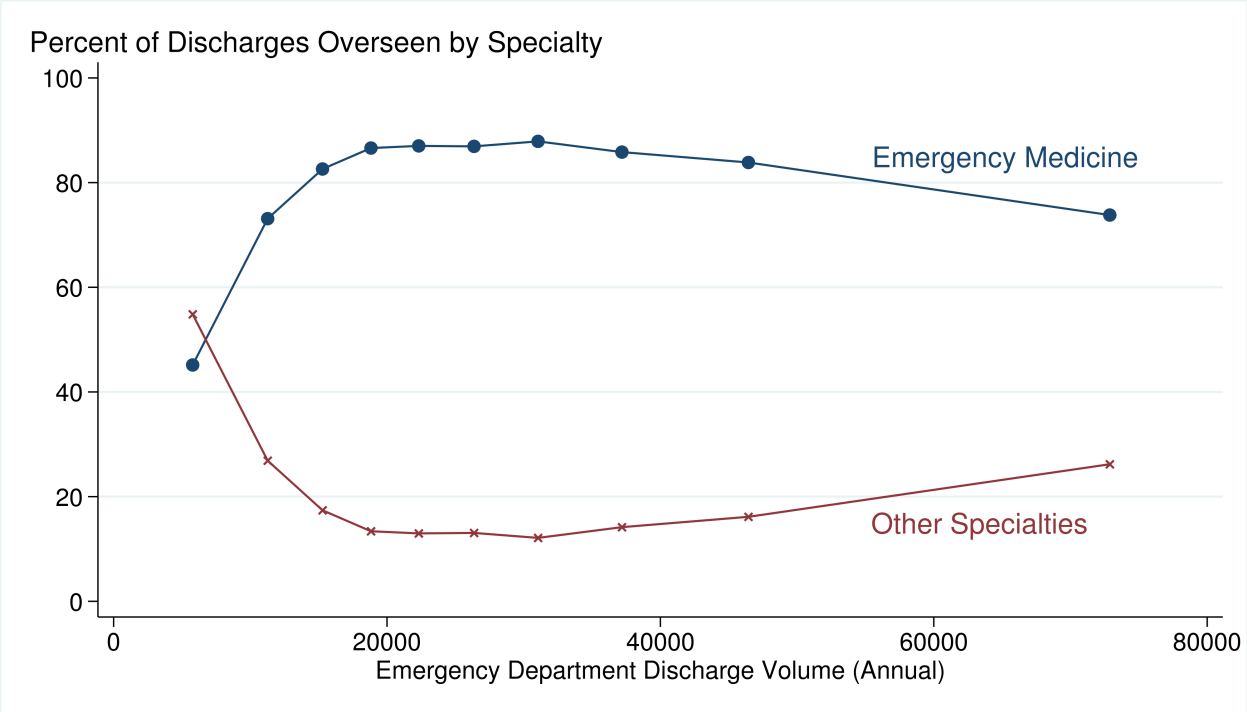


Figure A2: Percent of Discharges Overseen by Emergency Medicine Specialists, by Emergency Department Size

Notes: This figure estimates the relationship between Emergency Department (ED) discharge volumes and the share of discharges overseen by Emergency Medicine (EM) specialists. The graph is a non-parametric binned scatter plot of the share of discharges in each specialty versus the underlying annual discharge volume of the ED. The solid blue circles plot the EM specialty share. The red X's plot the non-EM share. We bin specialty shares into 10 equal-sized bins and plot the mean shares within each bin.

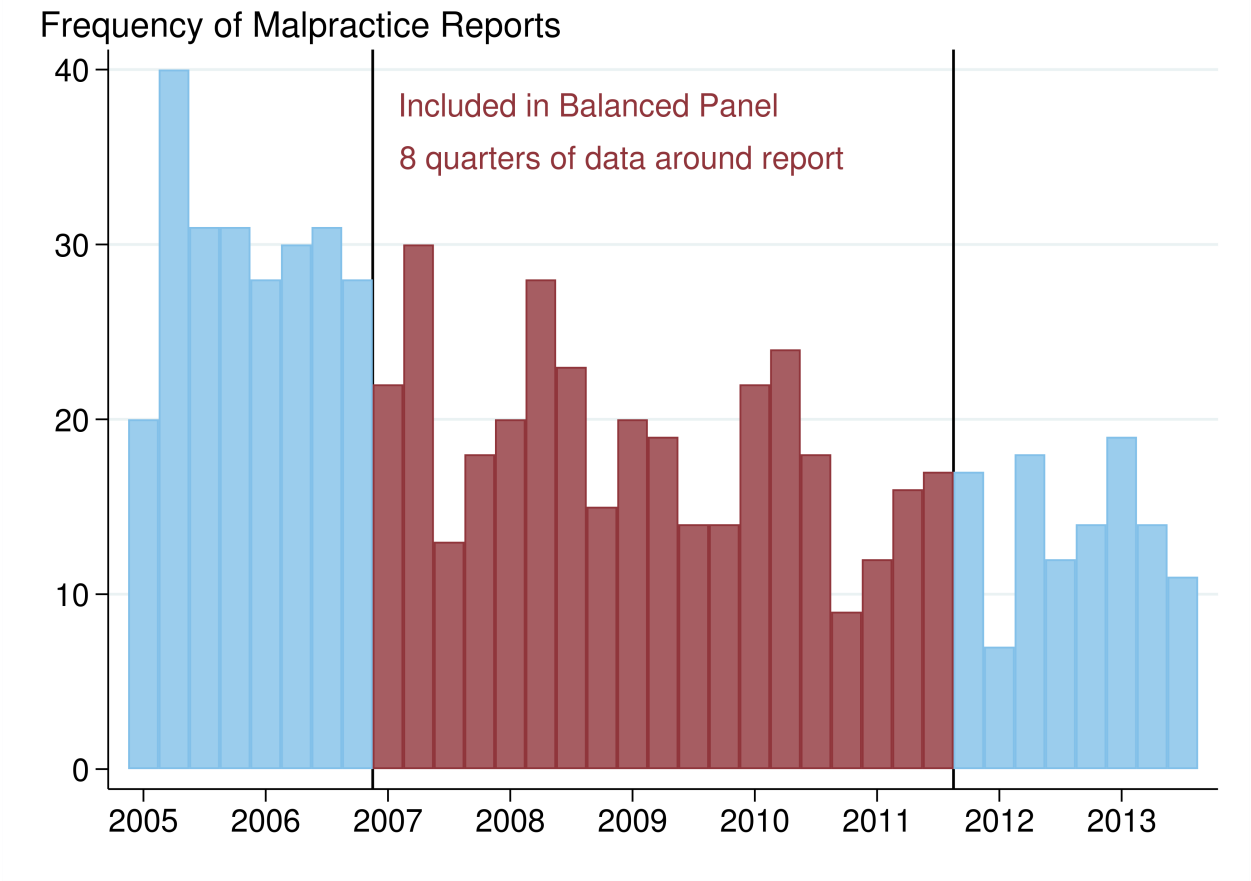


Figure A3: Malpractice Reports per Quarter

Notes: This figure plots a histogram of the number of malpractice reports filed per quarter. Red bars indicate reports in time periods that are included in our final analytic sample: those with at least 8 quarters of data before and after the report. Blue bars indicate reports in time periods that are not eligible for our final sample.

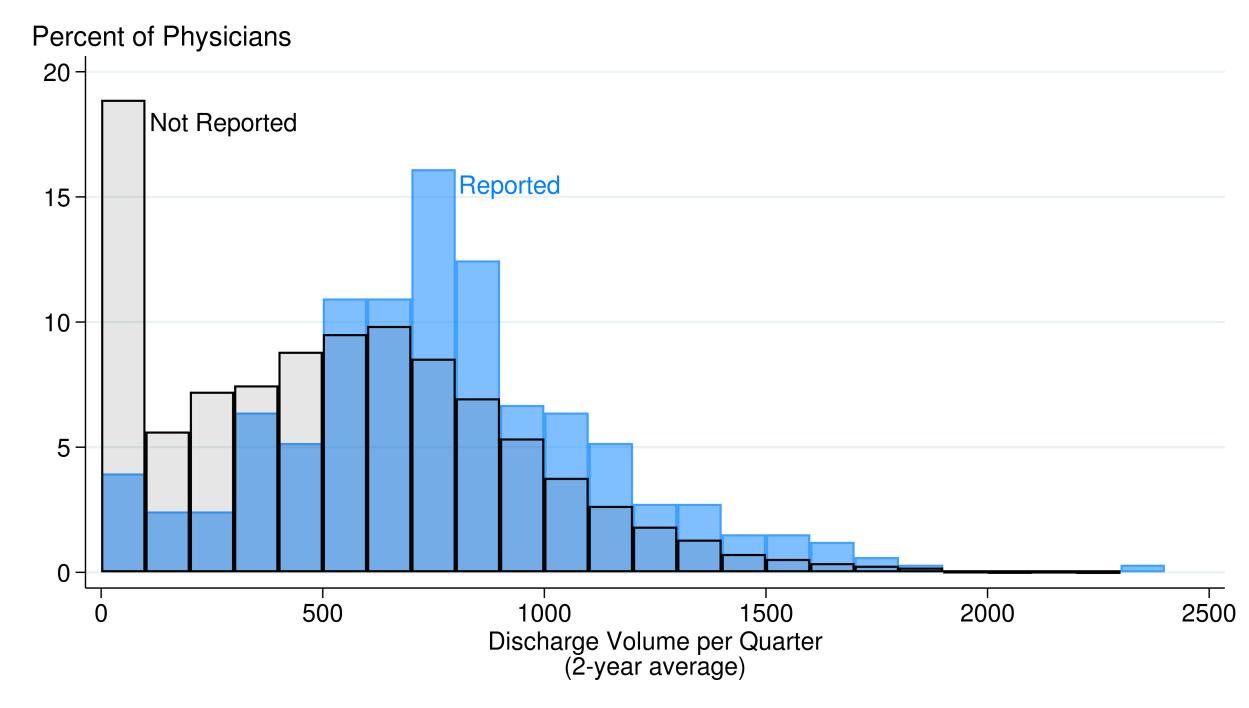


Figure A4: Discharge Volume Among Reported and Unreported Physicians

Notes: This figure estimates the relationship between physicians' discharge volumes and experience with malpractice reports. We plot a histogram of average discharge volumes per quarter separately for reported and unreported physicians. Discharge volumes for reported physicians are plotted using blue bars; discharge volumes for unreported physicians are marked using grey bars with black outlines. Discharge volumes are calculated as two-year rolling averages. Discharge volumes for reported physicians are calculated using data in the quarter before their report and the seven previous quarters. Discharge volumes for unreported physicians are calculated using data from their eligible match period, as described in Section 3.3.

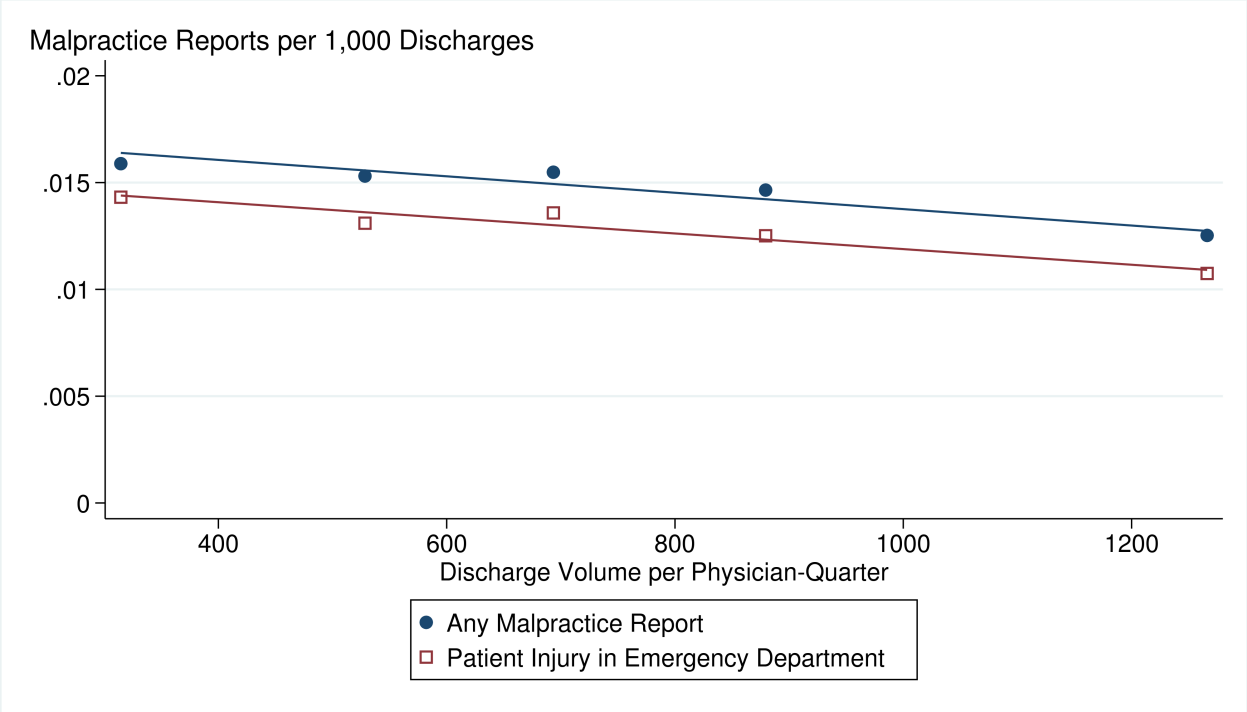


Figure A5: Malpractice Reports per Discharge

Notes: This figure estimates the relationship between physician discharge volume per quarter and the probability of being reported for malpractice. The graph is a non-parametric binned scatter plot of the rate of reports per 1,000 discharges versus the underlying quarterly discharge volume of each physician. We bin discharge volumes into 5 equal-sized bins and plot the mean malpractice rate within each bin. We calculate two malpractice rates depending on the location of the patient injury which precipitated the report. The hollow, red squares plot the rate of malpractice reports that specifically list the Emergency Department as the injury location. The blue circles plot the rate of any malpractice report against an Emergency Medicine physician. For scaling purposes, we drop physician-quarters in the bottom 10 percent of discharge volumes. We drop data from reported physicians in the post-report period.

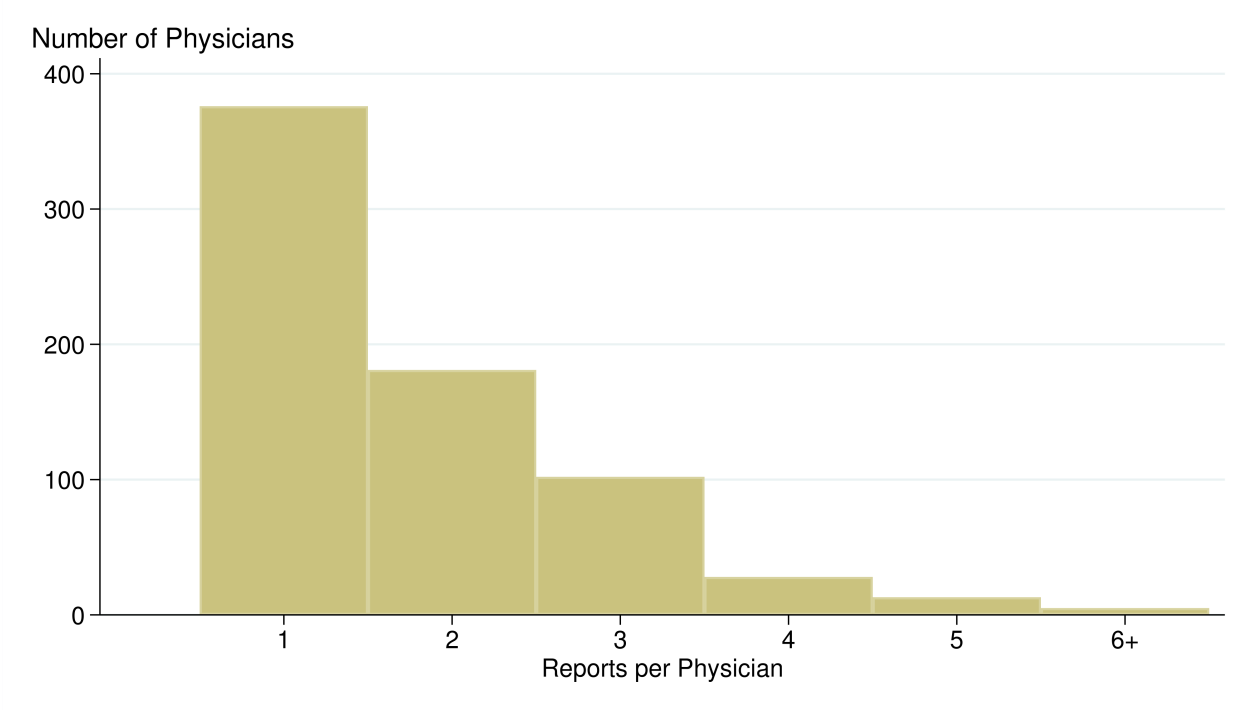


Figure A6: Malpractice Reports per Physician

Notes: This figure plots a histogram of the number of malpractice reports filed against Emergency Medicine specialists in our sample, conditional on having at least one report. For scaling purposes, the horizontal axis is truncated at six reports.

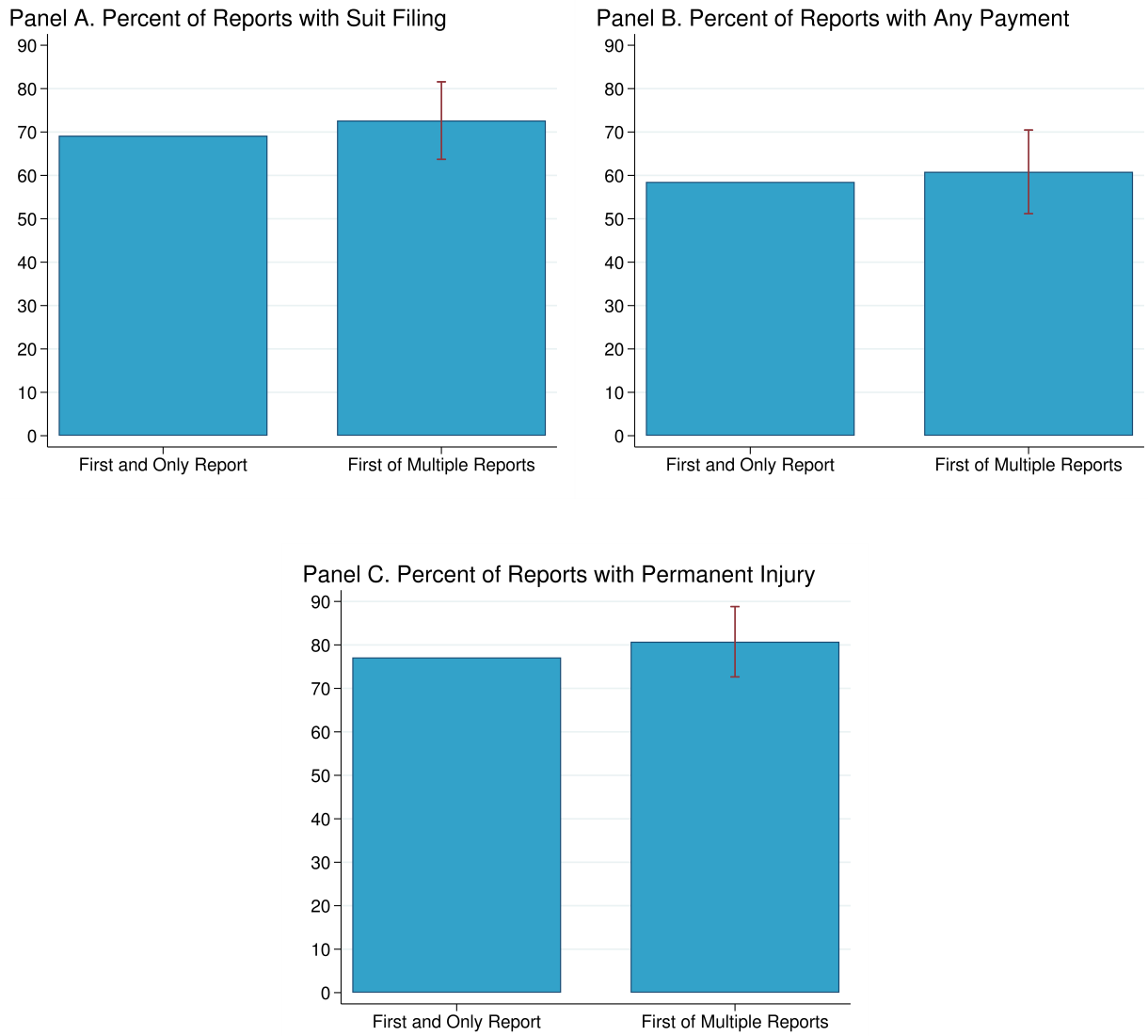


Figure A7: Malpractice Report Characteristics among Physicians with a Single versus Multiple Reports

Notes: These figures plot the characteristics of physicians' first malpractice reports, stratified by whether or not the physicians go on to have other reports filed against them. In each panel, the left column describes physicians that experience only one report. The right column describes physicians who experience multiple reports. The error bars indicate the difference in means across the two groups, controlling for differences in underlying tenure. Panels A, B and C plot the share of reports that include a suit, a non-zero payment and a permanent injury, respectively.

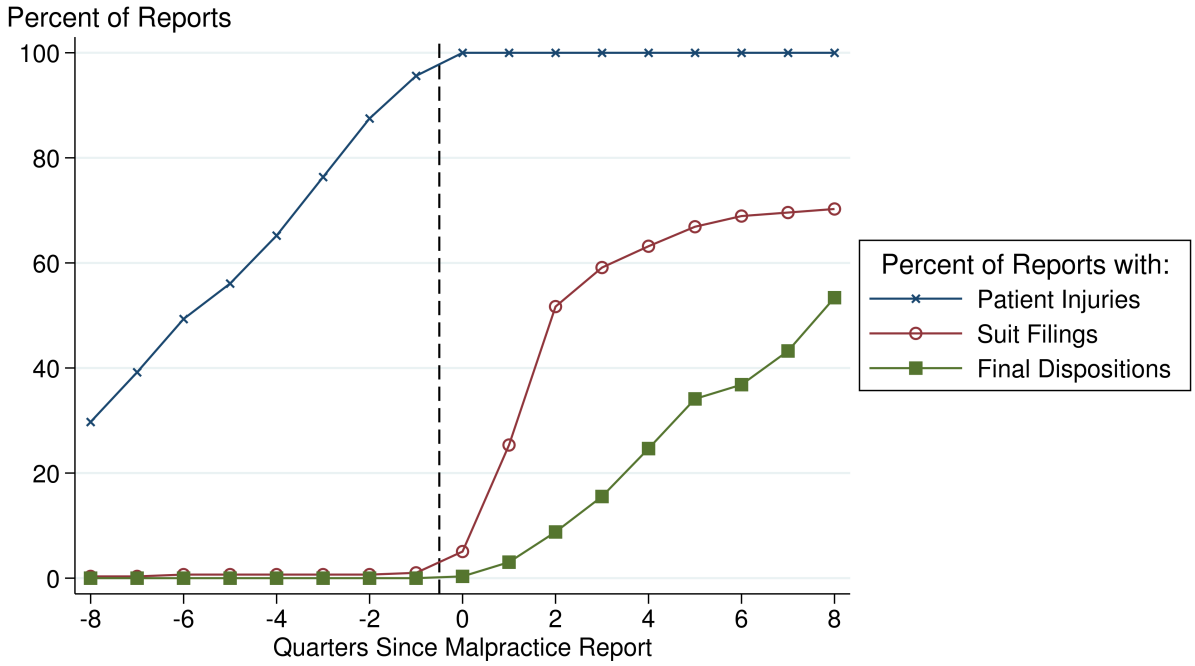


Figure A8: Development of Malpractice Claims Before and After the Report

Notes: This figure plots an event study of malpractice claim development relative to the timing of the initial report. The opaque squares plot the percent of malpractice reports that have reached a final disposition. The hollow circles plot the share of malpractice reports where a suit has been filed. The X's indicate the share of reports where the precipitating injury has occurred.

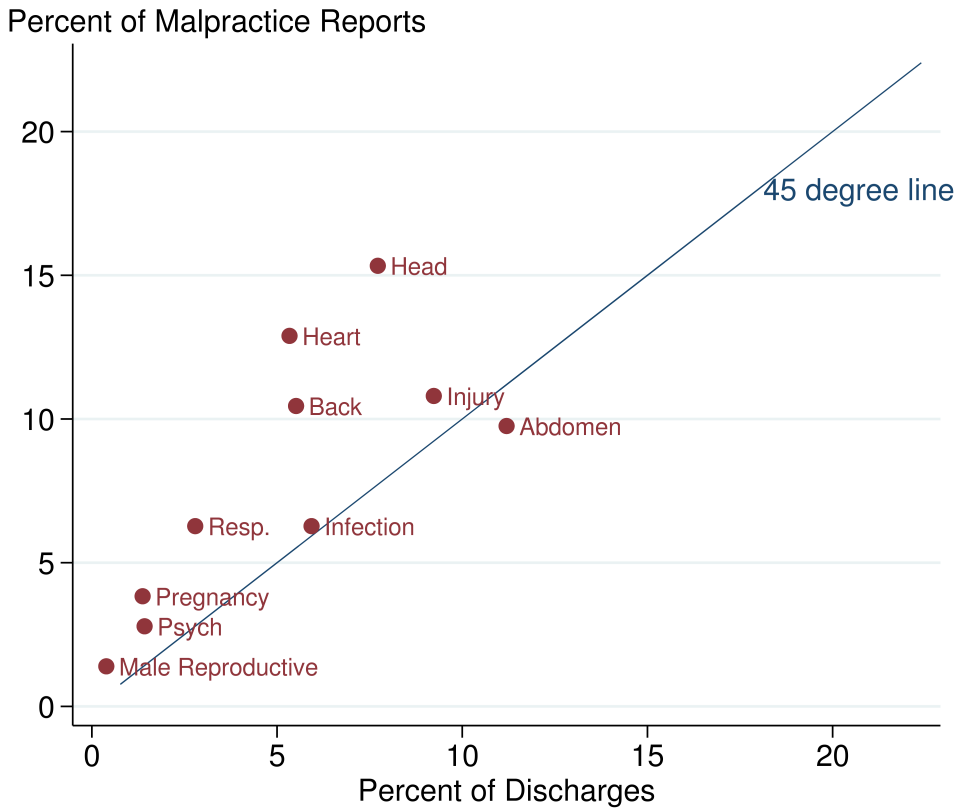
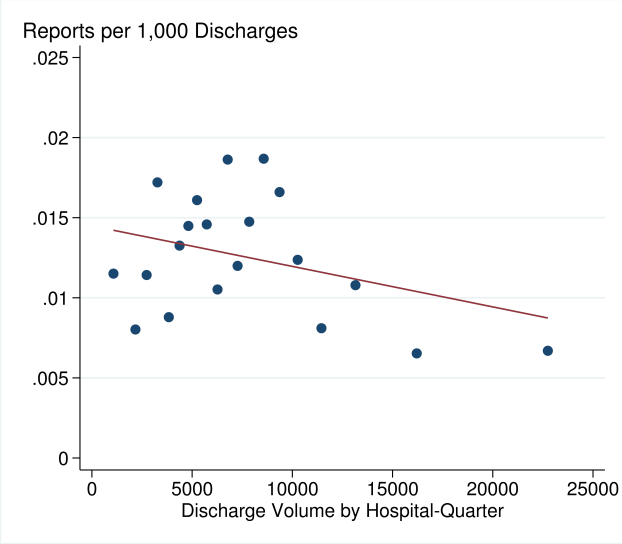


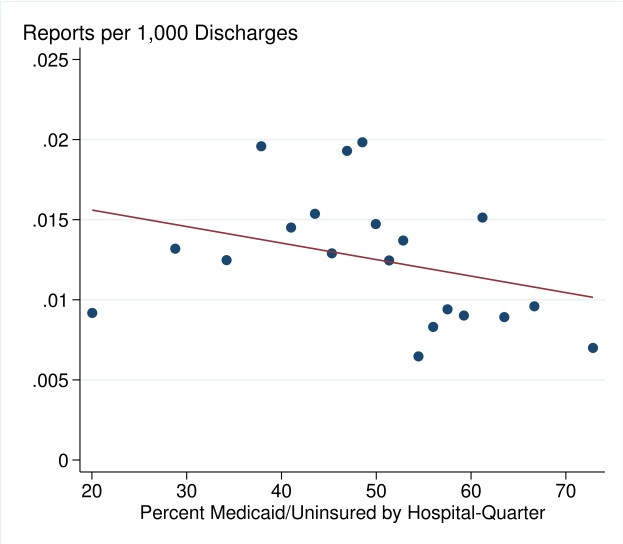
Figure A9: Frequency of Malpractice Reports by Clinical Type

Notes: This figure estimates the frequency of malpractice reports relative to the frequency of related discharges in the Emergency Department. The vertical axis describes the share of malpractice reports that fall into each clinical category. The horizontal axis shows the share of discharges that fall into the equivalent clinical category. Discharge shares are based on data from reported physicians in the quarter in which the patient injury occurred.

Panel A. Emergency Department Volume



Panel B. Percent Medicaid/Uninsured



Panel C. Hospital Teaching Status

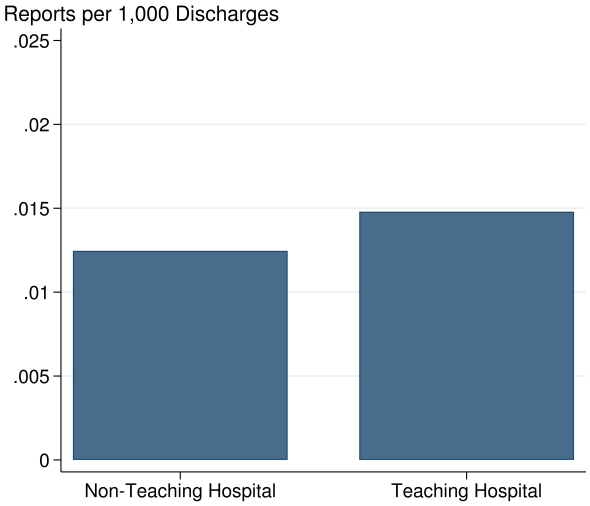


Figure A10: Prevalence of Malpractice Reports Across Emergency Department Types

Notes: These figures plot the prevalence of malpractice reports across Emergency Departments (ED). Panel A is a non-parametric binned scatter plot of the rate of malpractice reports per 1,000 discharges versus the underlying quarterly discharge volume of the ED. Panel B is a non-parametric binned scatter plot of the rate of malpractice reports per 1,000 discharges versus the share of Medicaid/uninsured discharges at the ED. For each panel, we bin the horizontal axis into 20 equal-sized bins and plot the mean malpractice rate within each bin. Panel C is a bar graph plotting the rate of malpractice reports per 1,000 discharges by the teaching status of the hospital.

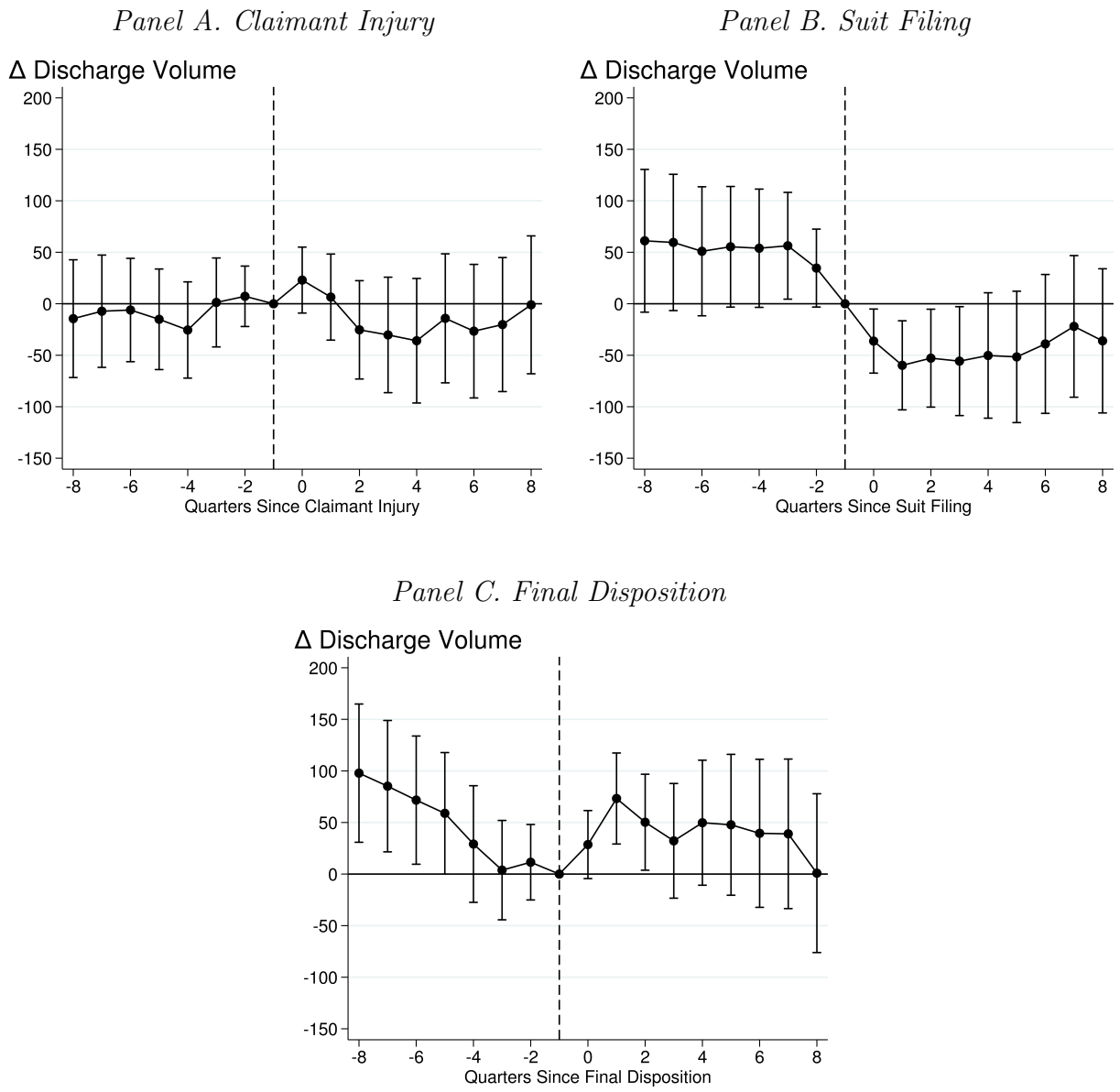


Figure A11: Changes in Discharge Volume and the Development of the Malpractice Claim

Notes: This figure plots event studies of physician discharge volume before and after key events in the development of the malpractice claim other than the malpractice report (Figure 2). Panels A, B and C plot changes in discharge volume over time relative to the timing of the claimant injury, the suit filing where relevant, and the final disposition of the claim, respectively.

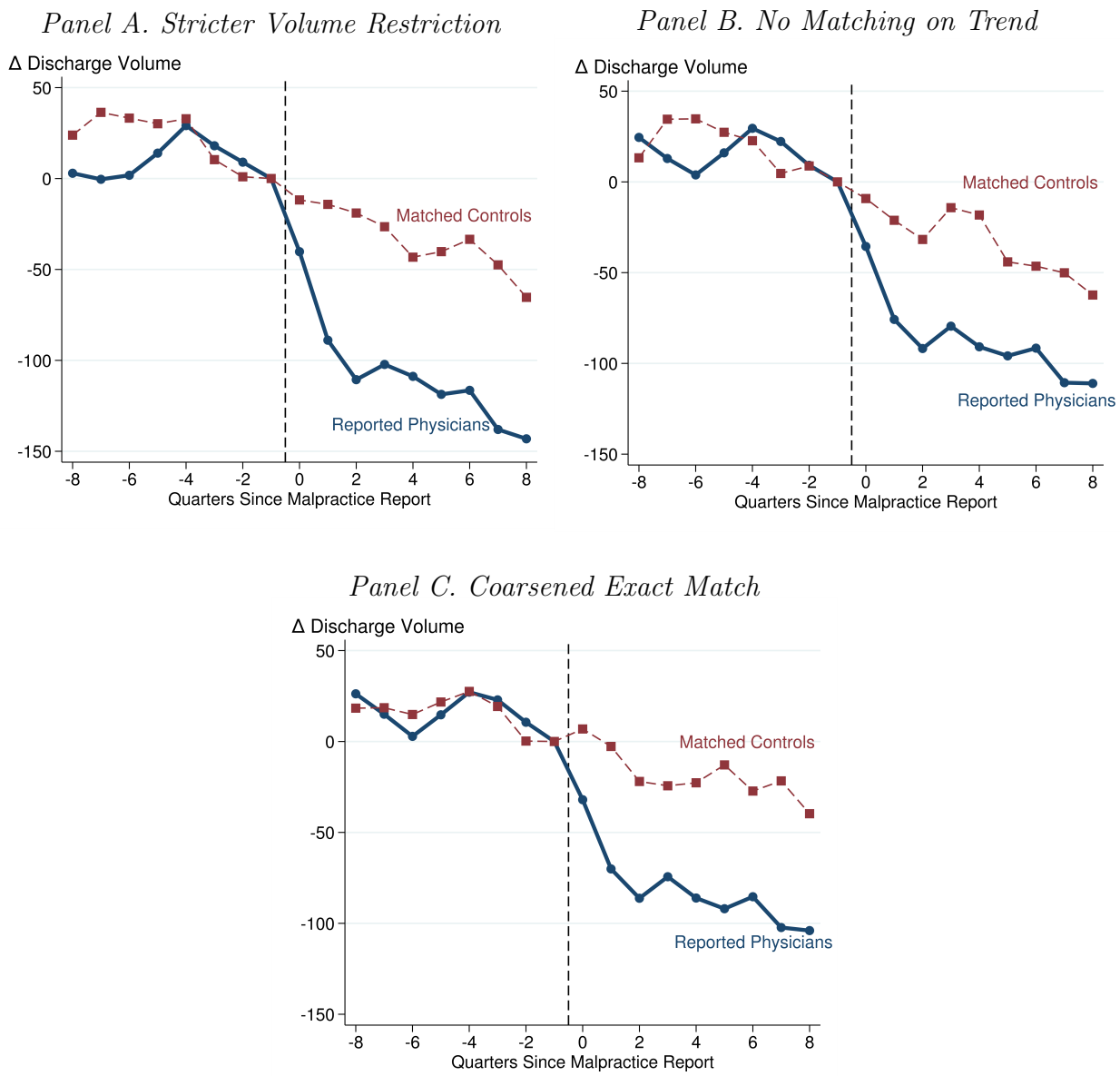


Figure A12: Effect of Malpractice Reports on Discharge Volume: Alternate Matching Approaches

Notes: These figures plot event studies of physician discharge volumes before and after malpractice reports, relative to unreported physicians selected through alternative matching approaches. To facilitate comparison, we plot all discharge volumes as absolute changes relative to the quarter prior to the report. In Panel A, we select controls using a propensity score match that mirrors our main approach, except that we impose a stricter volume restriction on the sample, dropping physicians with baseline volumes below the 25th percentile volume among reported physicians (instead of the 10th percentile). In Panel B, we select controls using a propensity score match as in our main model, except that we do not include baseline volume trends as a predictor in the match. In Panel C, we select controls using a one-to-one coarsened exact match.

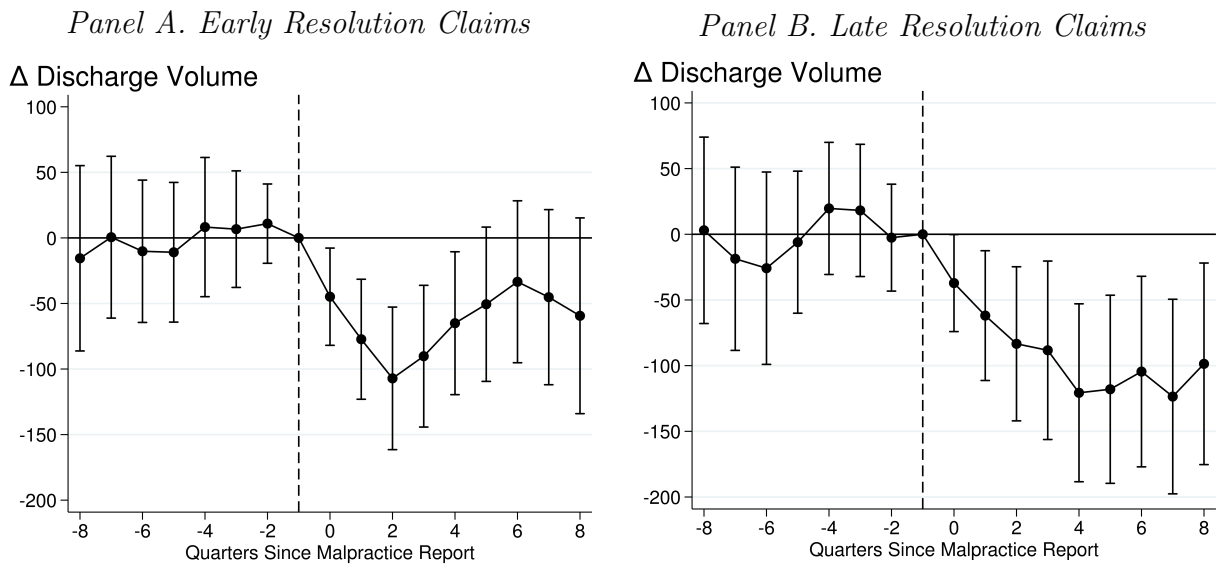


Figure A13: Effect of Malpractice Reports on Discharge Volume, by Claim Duration

Notes: These figures plot event studies of physician discharge volumes before and after malpractice reports. The panels separate reported physicians into two groups according to the duration of the malpractice claim and show changes relative to the full control group. Early resolution claims include those that were resolved during the pre-suit period and claims that were abandoned.

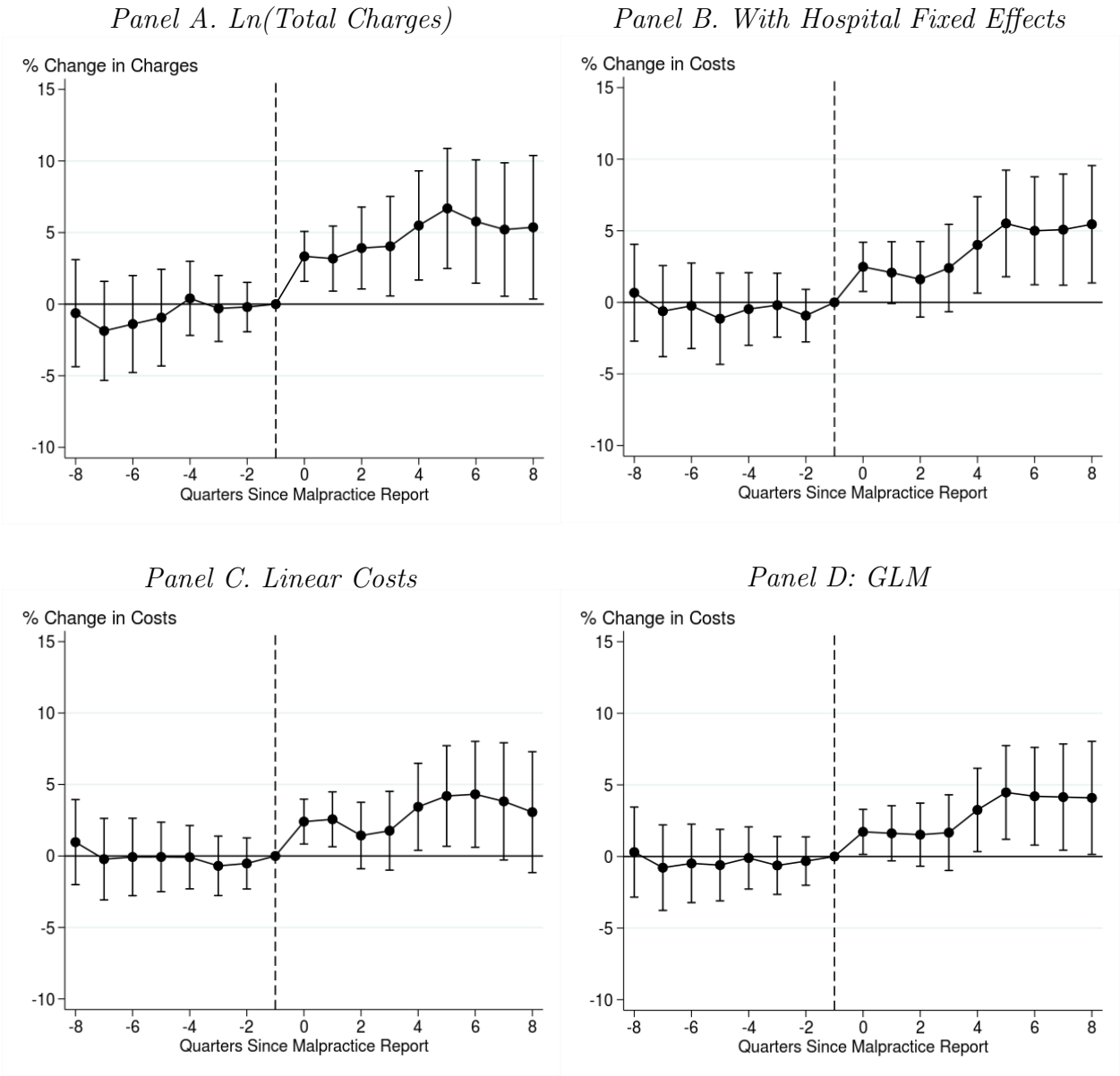


Figure A14: Effect of Malpractice Reports on Costs per Discharge: Alternate Specifications

Notes: These figures plot event studies of treatment intensity before and after malpractice reports using four alternative specifications to our main model. Panel A estimates changes in charges rather than costs. Panel B includes controls for hospital fixed effects. Panel C models linear costs rather than the log transformation. Panel D models costs using a one-part GLM model with a log link function. Costs are modeled in 2009 dollars.

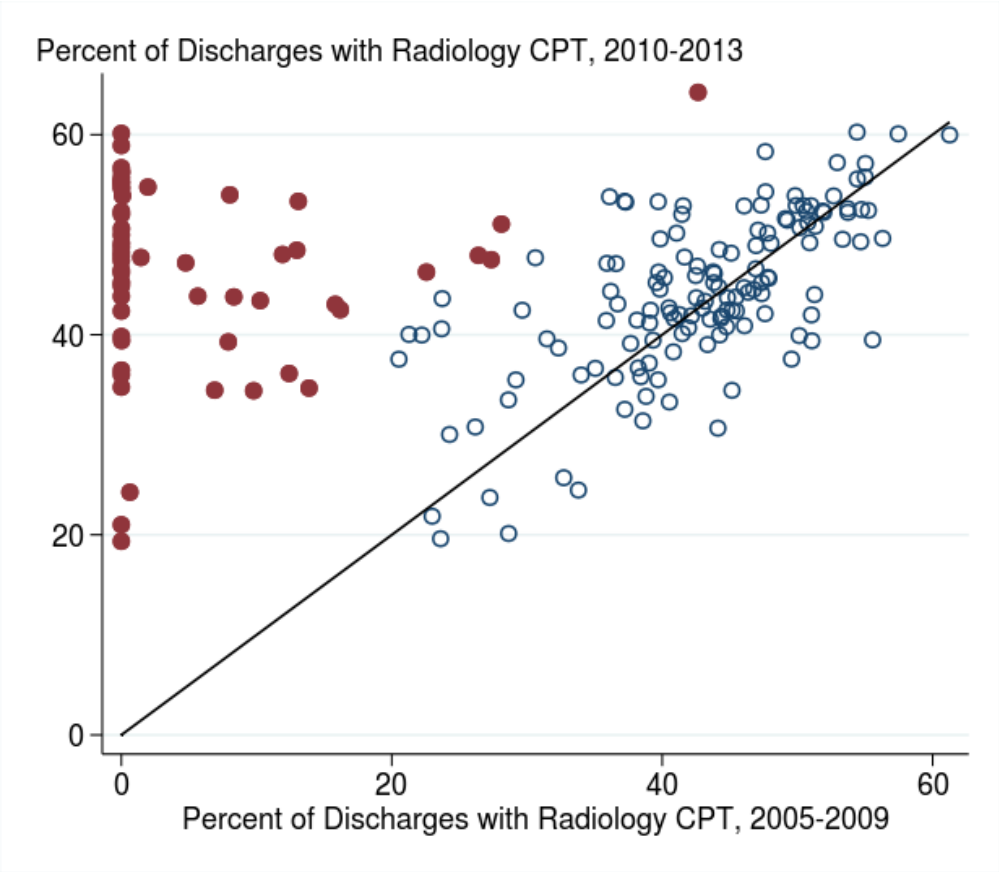


Figure A15: Rate of Radiology Coding Across Discharge Data Versions

Notes: This figure is a scatter plot of the rate of radiology coding in the older version of the discharge data (2005-2009) versus the newer version (2010-2013). Each dot is a hospital. The vertical axis describes the share of discharges in the newer data that have any radiology CPT code. The horizontal axis describes the share of discharges in the older data that have any radiology CPT code. The hollow circles indicate the hospitals that are included in the main specification of our testing analysis. The solid red circles show hospitals that are excluded. The solid black line is a 45 degree line.

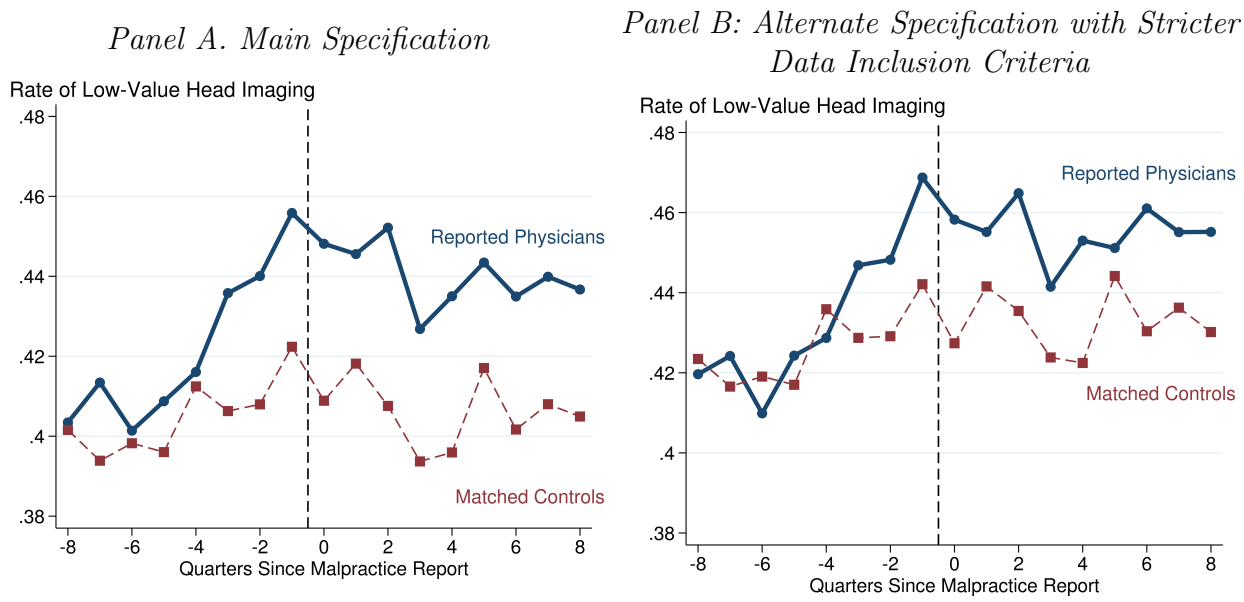


Figure A16: Effect of Malpractice Reports on Low-Value Head Imaging

Notes: These figures plot event studies of low-value head imaging rates before and after malpractice reports. Low-value head imaging includes head CT scans and MRIs among patients presenting with an uncomplicated headache. The main specification excludes hospitals with (1) a 20 percentage point or more difference in radiology coding between the new data version (2010-2013) and old data version (2005-2009) or (2) a radiology coding rate of under 1 percent in the old data version. The alternate specification excludes hospitals with (1) a 10 percentage point or more difference in radiology coding between the new and old data versions or (2) a radiology coding rate of under 10 percent in the baseline period.

Table A1: Effect of Malpractice Reports by Claim and Physician Characteristics
Panel A: Discharge Volume

	Coefficient	Standard Error	Observations	Physicians	Dependent Variable Mean	Percent Change
Discharge Volume						
A. Similarity to Claimant						
Dissimilar	-72.79**	22.24	12,070	595	773	-9.4
Similar	-4.19**	1.55	15,079	729	44	-9.5
B. Payment to Claimant						
$0	-75.27*	30.77	11,968	546	846	-8.9
$1-$149,999	-89.18**	30.90	11,628	526	830	-10.8
$150,000+	-72.11*	34.17	11,577	523	829	-8.7
C. Severity of Claimant Injury						
Temporary	-63.90	33.25	11,101	495	765	-8.3
Permanent	-81.29***	23.84	14,025	667	854	-9.5
D. Tenure						
Below 10 years	-25.93	35.64	10,982	488	808	-3.2
10-24 years	-81.26**	27.19	12,665	587	858	-9.5
25+ years	-104.41**	36.71	11,526	520	815	-12.8
E. Gender						
Male	-77.80***	22.55	14,110	672	853	-9.1
Female	-76.10	45.29	11,016	490	763	-10.0
F. Previous Report						
No	-81.30***	24.48	13,736	650	847	-9.6
Yes	-71.15*	31.55	11,390	512	805	-8.8
G. Multiple Paid Claims						
No	-88.33***	22.66	14,501	695	840	-10.5
Yes	-4.37	40.03	10,625	467	803	-0.5

Table continues on next page.

Table A1: Effect of Malpractice Reports by Claim and Physician Characteristics *(continued)*
Panel B: Costs per Discharge

	Coefficient	Standard Error	Observations	Physicians	Dependent Variable Mean	Percent Change
Ln(Costs)						
A. Similarity to Claimant						
Dissimilar	0.035*	0.017	8,293,443	595	560	
Similar	0.041*	0.021	591,842	595	672	
B. Payment to Claimant						
$0	0.044*	0.020	8,565,483	545	576	
$1-$149,999	0.060*	0.024	8,256,488	525	547	
$150,000+	0.022	0.024	8,227,713	522	553	
C. Severity of Claimant Injury						
Temporary	0.065**	0.024	7,798,454	494	571	
Permanent	0.036*	0.017	10,203,702	666	558	
D. Tenure						
Below 10 years	0.045	0.030	7,778,196	487	602	
10-24 years	0.036	0.019	9,140,806	586	543	
25+ years	0.053*	0.024	8,130,682	519	567	
E. Gender						
Male	0.048**	0.017	10,267,835	671	564	
Female	0.019	0.029	7,734,321	489	544	
F. Previous Report						
No	0.038*	0.017	9,949,298	649	550	
Yes	0.055*	0.027	8,052,858	511	590	
G. Multiple Paid Claims						
No	0.04*	0.02	10,501,108	694	560	
Yes	0.06	0.03	7,501,048	466	564	

Notes: This table estimates the effect of malpractice reports across different claim types and physician characteristics. Panel A models discharge volume and Panel B models costs per discharge. Each row represents the result of a separate regression. All regression analyses control for physician fixed effects, physician tenure and calendar time fixed effects. Analyses of costs per discharge additionally control for patient age, sex, race, presenting condition, visit day and insurance status. Standard errors are clustered at the physician level. Costs are reported in 2009 dollars. * p<.05; ** p<.01; *** p<0.001

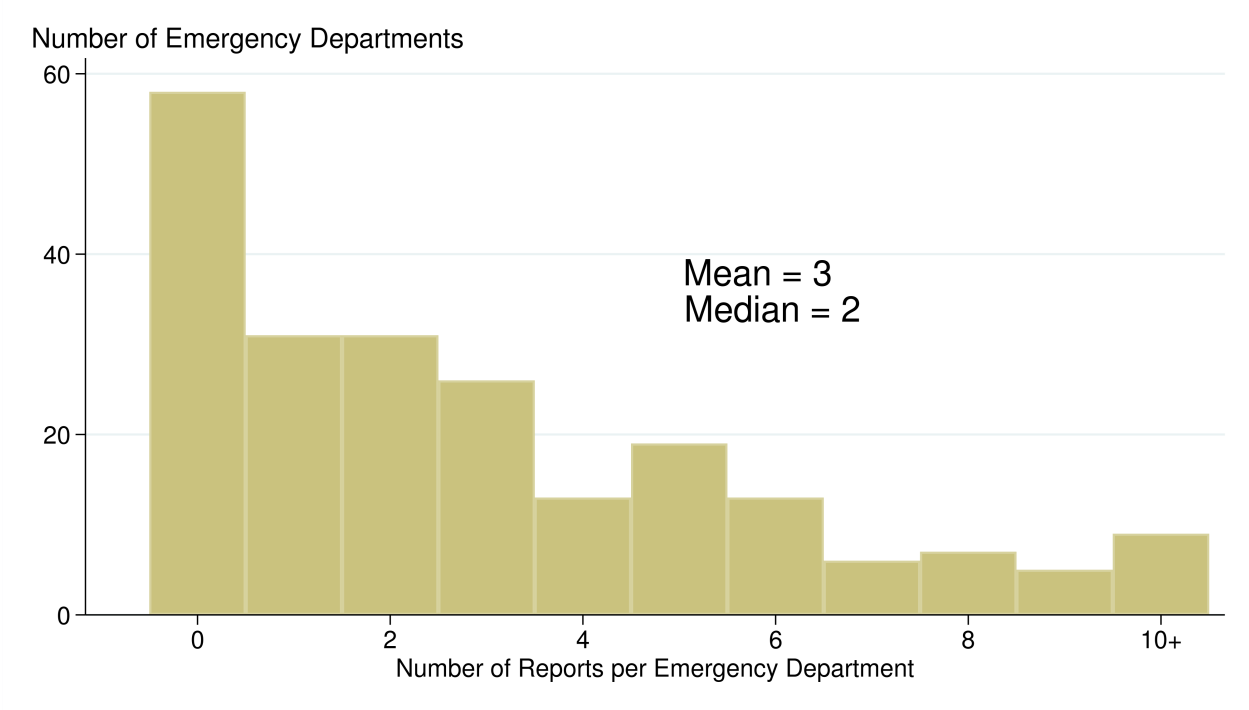


Figure A17: Number of Malpractice Reports per Emergency Department

Notes: This figure plots a histogram of the number of reports per Emergency Department. The sample covers malpractice reports against the Emergency Medicine specialists included in our final analytic sample, as discussed in Section 3.4. For scaling purposes, the horizontal axis is truncated at 10 reports.

Table A2: Association Between Malpractice Reports and Discharge Volume at the Emergency Department Facility Level

	Emergency Department Discharge Volume	
	(1) Ln(Volume)	(2) Volume
Cumulative Reports in Emergency Department	0.000350	-36.34
	(0.00540)	(59.28)
Observations	6347	6347
Emergency Departments	212	212
Mean Emergency Department Volume	7775	7775

Notes: This table estimates total Emergency Department discharge volume per quarter as a function of the number of malpractice reports at the facility in the past year (not including the contemporaneous quarter). The sample covers malpractice reports against Emergency Medicine specialists included in our final analytic sample, as discussed in Section 3.4. Column 1 models the log of discharge volume while column 2 models linear discharge volume. All models include hospital fixed effects and calendar time fixed effects. * p<.05; ** p<.01; *** p<0.001.

Table A3: Cost Differential Between Reported and Unreported Physicians in the Pre-Report Period

	Full Pre-Report Period	Quarter Before Report
	(1)	(2)
Reported	0.00734	0.000810
	(0.0163)	(0.0188)
Observations	5345808	654128
Mean of Dep. Var.	560	585

Notes: This table estimates the pre-report cost differential between reported and unreported physicians. Each column reports coefficients from a separate regression, with standard errors clustered by physician in parentheses. The full pre-report period includes the two years prior to the report. All regressions include hospital fixed effects, physician tenure and calendar time fixed effects, as well as patient age, sex, race, presenting condition, visit day and insurance status. Costs are reported in 2009 dollars. * p<.05; ** p<.01; *** p<0.001.