NBER WORKING PAPER SERIES

HOW DO PHYSICIANS RESPOND TO MALPRACTICE ALLEGATIONS? EVIDENCE FROM FLORIDA EMERGENCY DEPARTMENTS

Caitlin Carroll
David M. Cutler
Anupam Jena

Working Paper 28330
http://www.nber.org/papers/w28330

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
January 2021, revised March 2024

We would like to thank Alice Chen, Lindsay Jacobs, Kosali Simon and Kathy Zeiler for serving as discussants and Frank Sloan for helpful comments. We also thank seminar participants at the American Economic Association Annual Meeting, the American Society of Health Economists Annual Meeting, the Electronic Health Economics Colloquium, the Empirical Health Law Conference, Harvard Medical School, the Minneapolis Federal Reserve, the Midwest Health Economics Conference, and the University of Minnesota. Caitlin Carroll acknowledges financial support from the Agency for Healthcare Research and Quality T32 trainee program and the National Science Foundation Graduate Research Fellowship. Anupam Jena acknowledges financial support from the National Institutes on Aging (1DP5OD017897).The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

How do Physicians Respond to Malpractice Allegations? Evidence from Florida Emergency Departments
Caitlin Carroll, David M. Cutler, and Anupam Jena
NBER Working Paper No. 28330
January 2021, revised March 2024
JEL No. I11,J44,K41

ABSTRACT

The general deterrence effects of malpractice laws on physician behavior have been extensively studied but may lack salience for physicians. We study the role of specific deterrence in malpractice liability by examining how physicians respond to being accused of malpractice. With the universe of data on patient care and malpractice complaints for Florida emergency physicians, we find that physicians oversee 9% fewer discharges after allegations and treat each discharge 5% more expensively. Effects are similar for paid claims and dropped accusations. Increases in treatment are generalized, i.e., not limited to conditions similar to what the physician is reported for.

Caitlin Carroll
University of Minnesota
Department of Health Policy and Management
420 Delaware Street SE, MMC 729
Minneapolis, MN 55455
carrollc@umn.edu

David M. Cutler
Department of Economics
Harvard University
1875 Cambridge Street
Cambridge, MA 02138
and NBER
dcutler@harvard.edu

Anupam Jena
Department of Health Care Policy
Harvard Medical School
180 Longwood Avenue, Door A
Boston, MA 02115
and NBER
jena@hcp.med.harvard.edu

I. 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; Congressional Budget Office, 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 and the expected health benefits are less than the cost of treatment.

Existing research on the impact of malpractice liability generally focuses on physicians' responses to state-level tort reforms. The most common reforms impose caps on non-economic damage awards. In theory, damage caps can reduce defensive medicine by decreasing the expected penalty for negligence. Evidence on the effect of state reforms is inconclusive, however, with some papers finding that cap adoption leads to spending changes (Kessler and McClellan, 1996, 2002; Avraham, Dafny and Schanzenbach, 2012; Avraham and Schanzenbach, 2015) and some papers finding evidence against (Sloan and Shadle, 2009; Paik, Black and Hyman, 2017; Congressional Budget Office, 2019; Moghtaderi, Farmer and Black, 2019). Even among the studies that find evidence of spending changes, the magnitude of the effect varies.[1]

However, the effect of tort reforms may not capture the full effect of malpractice liability. A key effect of damage caps is to decrease the amount awarded for successful malpractice claims, with weaker evidence surrounding the impact on claim frequency.[2] But most physicians are not financially liable for malpractice damages, even if they lose a suit (Zeiler et al., 2007); rather, the primary costs of being sued for malpractice are the opportunity cost of time, reputation effects and anxiety related to litigation. Thus, the main impact of malpractice liability is likely to occur from the number of claims, rather than the payout for lost claims. Further, even when tort reform affects the frequency of accusations, there may be a lag between tort reform and changes in malpractice claims (Congressional Budget Office,

[1]The seminal paper on this topic estimated a 5-9% decrease in spending following damage cap reform (Kessler and McClellan, 1996); follow up work from the same authors modified these estimates to 4-7% (Kessler and McClellan, 2002). More recent evidence tends to find more modest effects. Avraham, Dafny and Schanzenbach (2012) estimate a 2% decrease in employer-sponsored insurance premiums, while Avraham and Schanzenbach (2015) estimate decreases in Medicare spending between 2-5%.

[2]Several recent papers find that damage caps reduce claim frequency (Durrance, 2009; Paik et al., 2012; Paik, Black and Hyman, 2013), with others finding no relationship (Zuckerman, Bovbjerg and Sloan, 1990; Donohue and Ho, 2007). Congressional Budget Office (2019) finds no evidence of claim frequency reductions until three years after the law changes. A large body of evidence shows that damage cap reforms decrease claim payout amounts by 20-30%; see Kachalia and Mello (2011) and Mello and Kachalia (2016) for reviews.

2019). Changes in physician behavior may be later still.[3] This presents an identification challenge when other parts of the medical care environment change contemporaneously.

In this paper, we study how physicians respond to being accused of malpractice. This approach considers specific deterrence (response to personal experience with a citation), rather than general deterrence (ex-ante threat of litigation that is driven by malpractice laws). By examining specific deterrence, we can see how direct involvement by a physician with the malpractice system influences care provided. Because we know the exact date a claim was filed, we have accurate identification for when the malpractice episode begins.

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 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. 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 (Sloan et al., 1993; Waxman et al., 2014). Second, physician productivity is important in EDs, which have faced increased crowding (Institute of Medicine, 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 pressure on physician decisions, separately from patient choice.

[3] Carrier et al. (2010) show that physicians' perceptions of liability risk are not strongly correlated with state tort laws.

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., do physicians continue to work in the same hospital where they were reported?). 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% 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 treatment intensity among their remaining patients. Malpractice allegations lead to an increase in treatment intensity, with reported physicians increasing costs per discharge by 5% 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 accused physicians are more likely to order laboratory and radiology services, consistent with a role for malpractice pressure in driving 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 patient 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 this 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, as 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.

Related Literature: This paper makes several contributions to the literature. First, we propose and test an understudied margin of malpractice liability: response to specific accusations of wrong-doing. Relative to the existing tort reform literature, which focuses on general deterrence, we show large behavioral changes among accused physicians after realized malpractice allegations (specific deterrence). This is consistent with a growing literature that studies variation in liability outside of cap adoption, analyzing military immunity (Frakes and Gruber, 2019), jury verdicts (Lakdawalla and Seabury, 2012) and changes in the negligence standard itself (Frakes, 2013). Our work is consistent with the cap adoption literature in that physician responses to malpractice allegations are inelastic with respect to the monetary penalty: physicians respond equally to malpractice allegations with large payments and no payment. Thus, our work suggests an important role for the uninsured costs of an allegation such as time costs and reputation damage.

Second, our results shed light on mechanisms underlying physician responses to malpractice liability. Estimates of aggregate spending changes in response to liability will understate the effect of malpractice if liability 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 sepa-

[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.

rately. Our data allows us to separately identify the effect of malpractice liability on these two margins.

Third, our results relate to a broad literature on the economics of deterrence. Several studies suggest that personal experience with legal sanctions is a salient determinant of risk perceptions (Chalfin and McCrary, 2017). In the context of medical malpractice, studies of specific deterrence have generally focused on malpractice allegations against obstetricians, which the literature notes may capture both supply responses of physicians and changes in patient demand.[5] Our work makes three advances: we isolate provider supply decisions; we distinguish between positive and negative responses to allegations; and we compare responses to frivolous versus meritorious allegations. This final point is important in the context of recent policy proposals, which seek to shield physicians from malpractice if they practice within the standard of care.[6]

Finally, 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.

The paper proceeds as follows. Section II provides background on malpractice claims and Section III describes the data. Section IV discusses our empirical strategy. Section V describes our labor supply results. Section VI describes our treatment intensity results. Section VII explores heterogeneous responses to malpractice allegations across claim and physician characteristics. Section VIII discusses the aggregate impact of malpractice reports and concludes. All appendix tables and figures are included in the Online Appendix.

[5] This literature has found minimal effects 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). Stevenson, Spittal and Studdert (2013) study allegations against nursing homes and find no change in quality. Recent work has also demonstrated an association between physician labor supply and the number of paid malpractice claims (Studdert et al., 2019). One recent paper (Carlson et al., 2020) studies allegations in the ED and finds no changes in treatment intensity. However, that paper does not observe intensity at the patient level and does not address changes in labor supply. In health care more broadly, Barnett, Olenski and Jena (2017) and Howard and McCarthy (2021) study the deterrent effect of accreditation inspections and DOJ investigations, respectively, and find evidence of changing provider behavior.

[6] Evidence on the negligence standard is limited and mixed. (Frakes, 2013) finds that changes in local standards of care had meaningful effects on cardiac and obstetric services, while Waxman et al. (2014) find no effect of moving from a negligence to gross negligence standard in the context of emergency care.

II. 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 injuries often involve misdiagnosis (Sloan et al., 1993; Brown et al., 2010) and many patient injuries manifest after the patient has left the provider's care.[7] 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).[8]

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 is 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. On average, physicians lose 2.7-5 days of work time per claim (Mello et al., 2010). 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, settlement talks continue as both parties prepare for a trial.[9] The average time from report to final

[7] 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."

[8] Approximately 40% of malpractice claims result in no payment to the claimant (Studdert et al., 2006). Florida has a relatively high rate of paid claims per capita, relative to other states (Hyman et al., 2023). During our study period, noneconomic damages for ER practitioners were relatively low at $150,000 per claimant (Florida Statute 766.118(4)(a)).

[9] 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.

Panel A. Time from Injury to Report *Panel B. Time from Report to Disposition*

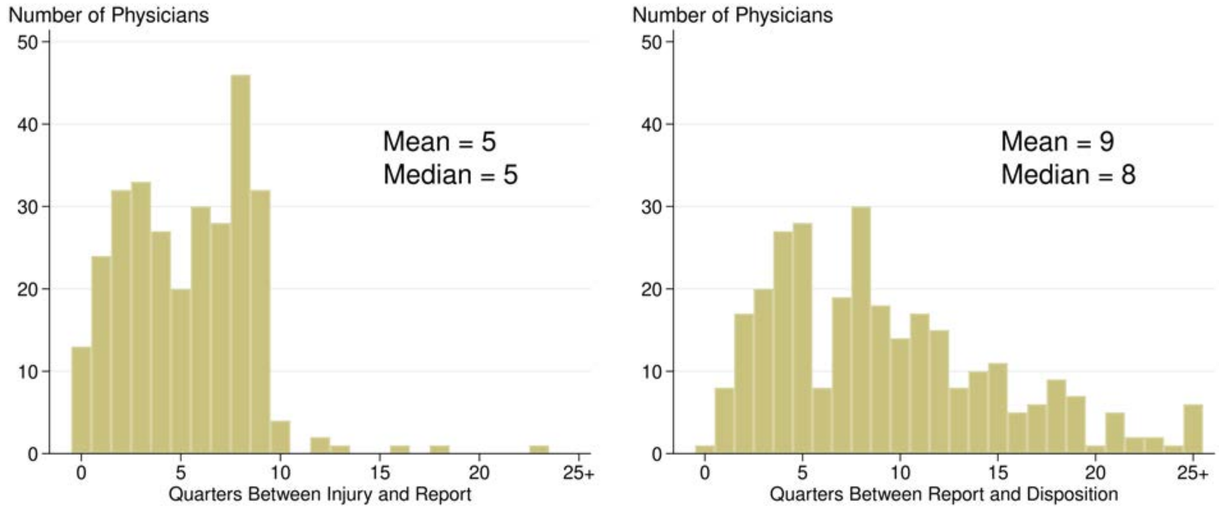


FIGURE 1: Time Between Patient Injury, Malpractice Report and Claim Disposition

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.

disposition is over two years in our sample (Figure 1, Panel B).

Nearly all physicians carry malpractice insurance.[10] Premiums have traditionally been community rated and do not rise based on personal malpractice history.[11] For hospital-based physicians, premiums are typically the responsibility of the hospital.

II.A. Conceptual Framework

We investigate positive and negative responses to malpractice allegations, which empirical work has generally considered separately. A natural question is how these effects interact. Existing conceptual models have characterized the trade-off between positive and negative responses as a physician optimization problem where patients and procedures carry heterogeneous risk (Currie and Macleod, 2008; Frakes, 2015; Paik, Black and Hyman, 2017). Positive responses can dominate where treatment itself carries little risk, as in the case of diagnostic care. Negative responses can dominate when providing treatment is risky. Thus,

[10]Physicians in Florida must carry $250,000 in malpractice insurance to have hospital staff privileges. Physicians may alternatively use secured personal assets to cover claims, but this is not common.

[11]Early evidence found limited use of experience rating for malpractice premiums (Sloan and Chepke, 2008), but there is little evidence on the current malpractice environment. Evidence from Massachusetts suggests that malpractice insurers have increased their use of risk-rating over time (Rodwin et al., 2008).

these models predict that liability can lead physicians to simultaneously increase treatment intensity among some patients and decrease it in others. In the extreme case, physicians might avoid treating high-risk patients altogether, leading to both to a decrease in labor supply and a decrease in the average severity of a physician's patient panel. From an empirical standpoint, this avoidance behavior would lead to attenuated estimates of any increase in treatment intensity (i.e., positive responses).

A second theory is that malpractice exposure affects the expected gains from spending more time with each patient, conceptualized in other contexts as a change in the shadow price of time (Tai-Seale and McGuire, 2012; Chan, 2018; Silver, 2020). For ED physicians, spending more time per patient would result in fewer patients per shift and a possible increase in treatment intensity per patient if care inputs are costly. Notably, this model does not predict differential effects across patients or procedure types, given a random draw of patients in the ED.[12] A related theory is that physicians reduce their number of shifts after a report, due to a change in the disutility of labor, a decrease in demand from hospitals or time costs associated with an allegation. A reduction in the number of shifts would decrease labor supply but would not necessarily increase treatment intensity.

III. Data

III.A. 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.[13] Each discharge record identifies the attending physician as well as the facility where the care was delivered. We also observe year and quarter time stamps, patient demographics, diagnoses and procedures, and hospital charges across different service types. Procedures are identified through documentation of Current Procedural Terminology (CPT) codes; this documentation changed in 2010 when the AHCA released a new version of the data.[14]

[12]Because patients can sue for any adverse outcome regardless of negligence, every patient treated can carry additional risk.

[13]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.

[14]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

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 Cost Reports 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 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 AII 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.[15]

jumps in radiology coding (Figure A17).

[15] We are unable to determine the share of claims omitted due to this restriction. A claimant and their

III.B. 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.[16] On the intensive margin, we study the total volume of discharges that a physician oversees in a given quarter, 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").[17]

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.[18] 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.[19] 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.[20] We also calculate the rate of one high-value imaging test:

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.

[16]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.

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

[18]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 A15.

[19]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.

[20]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,

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.

III.C. 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 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.[21] 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 placebo reported physicians who were similar to the reported physicians prior to their malpractice allegations but were not reported themselves.[22] To mirror sample restrictions on the reported physicians, the pool of potential controls is limited to physicians who we can observe for at least four years. One challenge in selecting controls is that reported physicians had different practice patterns than the full population of physicians, 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

head trauma and altered mental status.

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

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

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.[23]

Second, we implement a matched sampling procedure to choose control physicians 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, physician tenure quintiles, Medical Doctor (MD) versus Doctor of Osteopathy, physician gender, hospital type and hospital region.[24] Hospital type variables include indicators for teaching hospitals and for 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 quarter 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.

Our matching approach follows several recent empirical papers (Goldschmidt and Schmieder, 2017; Jaravel, Petkova and Bell, 2018; Smith et al., 2019; Arnold, 2022) and is grounded methodologically in Rosenbaum and Rubin (1985) and Imbens and Rubin (2015). A key advantage of matching in our setting is that it allows us to assign control physicians a synthetic event date, and compare reported versus unreported physicians over time. In particular, if a physician is reported for malpractice 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. By comparing reported physicians to controls with their own event dates, we account for such mechanical effects.

III.D. 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. The first set of columns include all physicians in our sample. For reported physicians, statistics are based on the quarter prior to the report.[25] For unreported physicians, statistics are calculated based on data from the chronological midpoint of their practice. Reported physicians were more experienced

[23] 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.

[24] We control for discharge volume trends using the average linear trend in the two years before the report and control for region using broad location bins: northern HRRs, central HRRs and southern HRRs.

[25] We cannot observe any pre-report data for physicians reported in the first quarter of 2005, so we do not include them in Table 1.

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).[26]

The second set of columns describe the balanced panel of physicians that enter into the 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.[27]

The third set of columns describe the matched analytic sample. The propensity score match identifies controls for all but two reported physicians.[28] Physician covariates are well balanced across the treatment and control group and we find no statistically significant difference in the covariate means across 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 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). As we discuss below, we show the impact of using discharge volume in the propensity score model in Table A1.

[26] Figure A5 does not include physician-quarters with bottom decile discharge volumes, where malpractice rates per discharge are high. Excluding this low volume sample, 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 allegations could decrease as volume increases if physicians learn by doing and make fewer mistakes (Hannan et al., 2003). Conversely, rates of malpractice could increase with discharge volume if physicians change their behavior when they are short on time (Chan, 2018).

[27] 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. This change results from excluding physicians with low baseline discharge volumes (described previously), who tend to have high costs per discharge.

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

Table 1: Characteristics of Physicians With and Without Malpractice Reports

	All Physicians		Eligible for Match		Matched Sample		
	Reported	Not Reported	Reported	Not Reported	Reported	Not Reported	Δ
Physicians	684	2039	297	1058	295	414	
(%)	(25%)	(75%)	(22%)	(78%)			
A. Dependent Variables							
Discharge Volume	771	623	821	753	817	733	-84
Costs/Discharge	587	627	558	542	559	542	-16
B. Physician Characteristics							
Tenure	18	16	19	17	18	18	-0
MD (%)	79	81	78	81	79	78	-1
Female (%)	19	26	19	22	19	18	-1
C. Hospital Referral Region (%)							
Northern	19	29	20	29	20	19	-0
Central	44	33	49	35	49	48	-1
Southern	37	38	31	36	31	33	1
D. Hospital Characteristics (%)							
Teaching	4	11	5	10	5	4	-1
Safety Net	25	28	25	27	24	25	0
High-Volume	40	49	44	51	44	49	5

Notes: This table reports average characteristics for Emergency Medicine specialists who were reported for malpractice and their unreported peers. 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. Costs are reported in 2009 dollars. Hospital characteristics correspond to traits of the facilities where physicians oversaw the plurality of their discharges. Differences between reported and unreported physicians in the matched sample are statistically insignificant for all covariates.

III.E. 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.[29]

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.[30] 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 27% occurred in safety net hospitals, also proportional to the frequency of discharges in these locations (Figure A10, Panels A and B).[31] Our finding on

[29]Ex ante, the direction of the relationship between multiple reports and physician quality is not clear. If multiple reports signal low quality care provision, the severity or negligence of initial claims among multi-report physicians could be higher than reports among single-report physicians. Conversely, since physicians must survive 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.

[30]We measure payments in 2009 dollars. 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.

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

Table 2: Characteristics of Malpractice Claims

	Reported Physicians		
	All Physicians	Eligible for Match	Matched Sample
Physicians	684	297	295
A. Physician Malpractice History			
Any Previous Report (%)	26	27	26
Previous Report Count	1	1	1
Any Subsequent Report (%)	29	26	26
Subsequent Report Count	1	1	1
B. Report Characteristics			
Suit Filed (%)	70	72	72
Payment to Defendant (%)	58	62	62
Non-zero Payment ($1000s)	361	221	222
Defense Costs ($1000s)	61	54	54
C. Claim Duration (Quarters)			
Injury to Disposition	14	15	15
Injury to Report	5	5	5
Report to Suit	2	2	2
Report to Disposition (without Suit)	4	4	4
Report to Disposition (with Suit)	11	11	11
D. Clinical Injury Category (%)			
Chest	18	20	20
Head	12	15	15
Trauma/Injury	7	11	11
Abdomen	8	9	9
Back	7	10	10
E. Severity of Injury (%)			
Temporary	23	21	21
Permanent	77	79	79
F. Injury Hospital Characteristics (%)			
Teaching Hospital	5	6	6
Safety Net Hospital	27	25	22
High Volume Hospital	42	49	46

Notes: This table describes average characteristics of malpractice reports against Emergency Medicine specialists. The unit of observation is a physician-quarter. Costs are reported in 2009 dollars.

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).

IV. 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. We construct counterfactual practice patterns for reported physicians (had they not experienced a report) using a control group 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.

The main challenge to identifying the causal effects of malpractice reports is that malpractice events may be endogenous to changes in physicians' labor supply or treatment intensity over time (our outcomes of interest). However, several factors alleviate this concern. First, evidence on the functioning of the malpractice system suggests that the timing of malpractice events is not strongly influenced by physician decisionmaking. As discussed previously, only 2% of patients harmed by negligence pursue a malpractice allegation (Danzon, 1991; Localio et al., 1991) and many physicians are accused of malpractice even when there is no evidence of negligence (Studdert et al., 2006). Thus, the timing of malpractice allegations depends heavily on *patients'* decisions about whether and when to sue, rather than changes over time in physicians' propensity toward negligence or clinical errors.[32] Moreover, when errors do arise in the ED, they are often a function of system-level factors (e.g., handoffs between providers, ED staffing) in addition to physicians' clinical decisions (Kachalia et al., 2007). Our empirical strategy leverages this quasi-random timing of malpractice allegations for a given physician, and identifies the effect of malpractice based on changes over time.

We estimate the following specification:

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

where Y_{jmt} measures labor supply outcomes for physician j in match m quarter t. γ_{jm} are a set of physician-match fixed effects, τ_t are year-quarter fixed effects, δ measure the time-varying effects of physician tenure (in quintiles).[33] ϵ_{jmt} are error terms that allow for

[32]Consistent with these facts, existing literature generally finds that malpractice risk is not associated with higher clinical quality of care (Mello et al., 2020). Instead, many malpractice allegations arise from breakdowns in communication between patients and providers (Roter, 2006), and depend on whether an apology was given (Kachalia et al., 2010; Ho and Liu, 2011).

[33]By including physician-match fixed effects in the model, we account for control physicians that appear in multiple events, such that the event time coefficients compare treated and control physicians matched for a particular report. Results are similar if we include physician and match effects separately or only include

correlation between observations from the same physician and from the same match (Abadie and Spiess, 2022). 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.[34] 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 set-up gives the β_k a traditional, DD interpretation.

Our approach is similar to the stacked DD approach in Jaravel, Petkova and Bell (2018), Deshpande and Li (2019) and Fadlon and Nielsen (2019); this approach is related but distinct from a traditional DD with staggered treatment timing. The main difference is that we include event time indicators that are common to reported and unreported physicians. Thus, our analysis is centered on event time, rather than calendar time, and avoids concerns about bias that can arise with staggered DDs (de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021).

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.[35] Second, to estimate the impact 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.[36]

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 1st and 99th percentiles 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

physician fixed effects.

[34] 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.

[35] $Y_{jmt} = \alpha * Post_{jmt} + \beta * Post_{jmt} * Reported_j + \delta Tenure_{jmt} + \gamma_{jm} + \tau_t + \epsilon_{jmt}$

[36] 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 non-Hispanic White patients and non-Hispanic Black patients.

data versions.[37] In our main analysis, we exclude hospitals with (1) a 20+ percentage point increase in radiology coding rates or (2) a radiology coding rate of under 1% in the old data, although we run sensitivity checks on that sample restriction (Figures A17 and A18).

V. Effects of Malpractice Reports on 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 80 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 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 72 discharges per quarter, or 8.7%.

We test the robustness of our results in the remaining columns 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 conclusions are largely unchanged when we include controls for these developments. We estimate a statistically significant decrease of 60 discharges per quarter after the report and estimate statistically insignificant impacts of other claim events. We estimate a relatively large and positive effect of the final disposition,

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

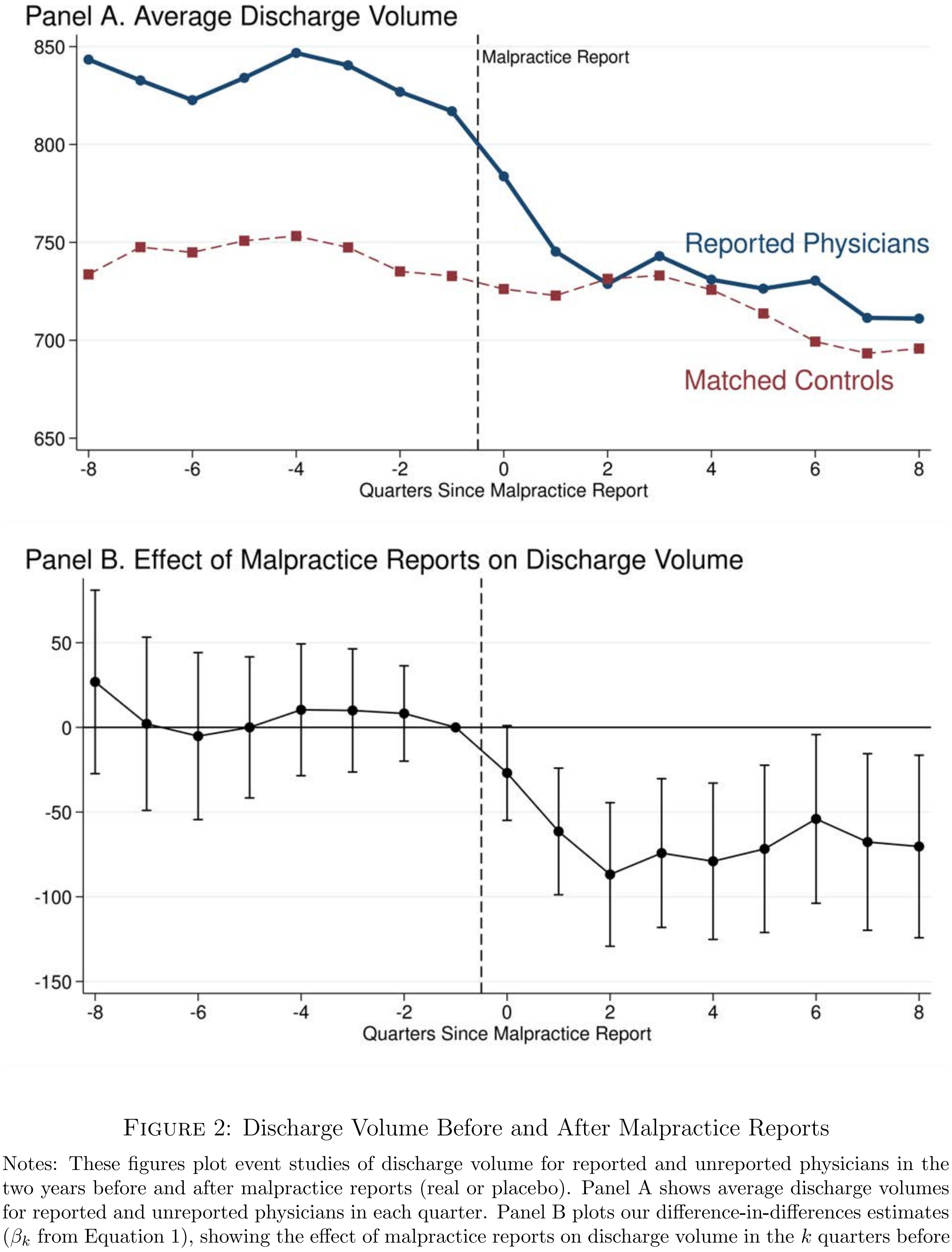


Figure 2: Discharge Volume Before and After Malpractice Reports

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-match fixed effects, controls for physician tenure and calendar time fixed effects. Standard errors are clustered by physician and match.

Table 3: Effect of Malpractice Reports on Discharge Volume

	Discharge Volume		
	(1)	(2)	(3)
Reported*Post	-72.32	-59.69	-62.90
	(20.52)	(20.30)	(21.94)
Injured*Post		0.984	
		(27.93)	
Sued*Post		-40.30	
		(26.16)	
Disposed*Post		33.84	
		(25.68)	
Observations	15045	15045	13566
Claim Timing Controls		X	
Drop High Volume			X
Number of Physicians	709	709	661
Percent Change	-8.7	-7.2	-8.3
Mean Discharge Volume	833	833	760

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 and match. All regressions include physician-match fixed effects, calendar time fixed effects, and controls for physician tenure.

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, Panel C). We find small and statistically insignificant changes around the patient injury, consistent with limited salience of such injuries in the ED (Figure A11, Panel A).[38]

In column 3, we test whether our results are driven by changes in behavior among reported physicians with especially high discharge volumes, as they may be different from unreported physicians who tend to have lower volumes on average. We exclude reported physicians with baseline discharge volumes in the top decile, as well as their matched unreported controls. We find that our results are largely unchanged when we exclude high volume physicians, estimating a decrease of 63 discharges per quarter, or 8.3%. Graphical analyses using this sample are also consistent with results from the main model (Figure A12): discharge volume

[38]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 from our original propensity score model, imputing suit and disposition dates for unreported physicians based on their matched reported physician.

trends and levels are similar across reported and unreported physicians prior the reports, but diverge sharply afterward.

Further robustness checks are available in the Appendix (Table A1). 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, (3) if we use a coarsened exact matching procedure (Iacus, King and Porro, 2012), (4) if we match on baseline volume levels in addition to the matching variables in our main model, and (5) if we use a 1-1 match without replacement and cluster standard errors at the match level (Abadie and Spiess, 2022).

V.A. 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 ailments such as sprained ankles to high risk conditions such as chest pain. If 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.[39]

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 or back pain.[40] High risk discharges include those

[39]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.

[40]Low risk conditions include the following CCS conditions: other upper respiratory infection, spondylosis,

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	-7.91	2.41	15,045	709	95	-8.3
Low Risk	-18.21	6.58	15,045	709	237	-7.7
Uncertain Risk	-9.21	2.43	15,045	709	89	-10.3
B. Probability of Any Practice						
In Any Location	-0.004	0.013	15,045	709	1.00	-0.4
In Injury Region	-0.018	0.016	13,995	685	0.89	-2.1
At Injury Hospital	-0.043	0.021	13,995	685	0.86	-5.0
At Non-Injury Hospital	0.021	0.012	13,995	685	0.81	2.6
C. Discharge Volume Given Practice						
Any Location	-65.59	17.64	14,584	708	835	-7.9
In Injury Hospital	-68.86	22.25	11,964	636	667	-10.3
In Non-Injury Hospital	-0.69	24.26	10,489	551	326	-0.2
D. Hospital Characteristics						
Teaching Hospital	-0.000	0.009	14,332	708	0.05	-0.8
Safety Net Hospital	0.014	0.016	14,332	708	0.24	5.7
High Volume Hospital	0.015	0.019	14,332	708	0.43	3.5
E. Discharge Volume by Claim Resolution Time						
Late Resolution	-85.53	22.04	14,042	650	842	-10.2
Early Resolution	-16.79	35.28	11,033	473	795	-2.1

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, focusing on quarters when physicians oversaw at least one discharge. Panel E estimates changes in discharge volume separately for claims with early versus late resolutions. All regression analyses control for physician-match fixed effects, physician tenure and calendar time fixed effects. Standard errors are clustered by physician and match.

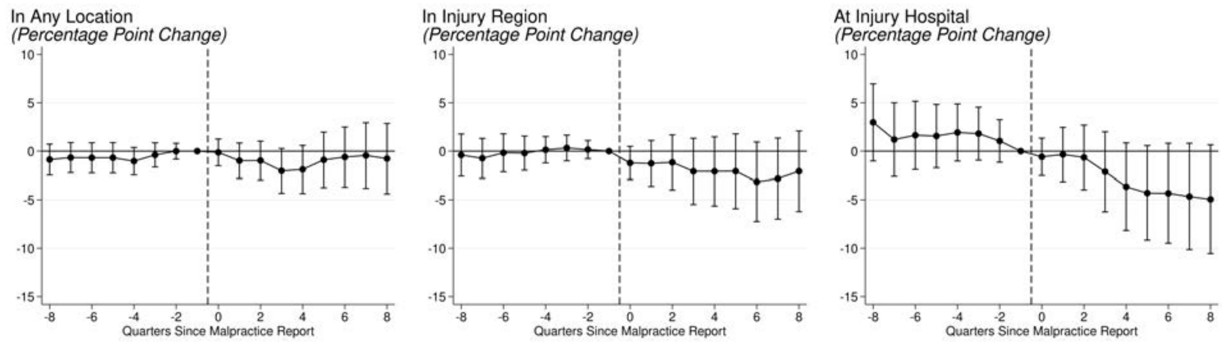
with diagnoses of pneumonia, coronary atherosclerosis, cardiac dysrhythmias, congestive heart failure, acute cerebrovascular disease, chronic obstructive pulmonary disease, chest pain, urinary tract infection, and skin and subcutaneous tissue infections and also include all patients over 80 years old. 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.[41] 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 8%, 10% and 8% for low, uncertain and high risk conditions, respectively.

A second issue to understand is whether physicians reduce practice or leave practice entirely. We decompose the decrease in discharge volume into extensive margin and intensive

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.

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

Panel A: Probability of Any Practice (Extensive Margin)



Panel B: Discharge Volume Given Practice (Intensive Margin)

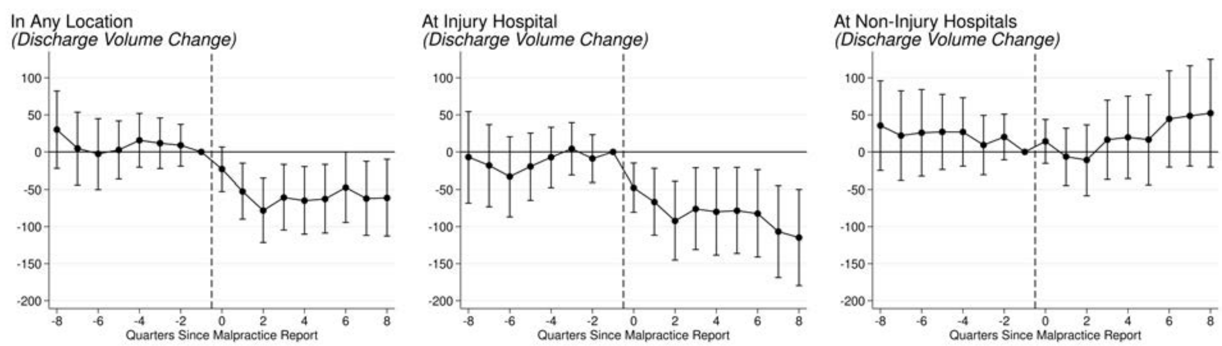


FIGURE 3: Effect of Malpractice Reports on Physician Labor Supply: Decomposition by Margin

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-match fixed effects, physician tenure and calendar time fixed effects. Standard errors are clustered by physician and match.

margin effects in Table 4 (Panels B and C) and Figure 3. Panel B of Table 4 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 of these location shifts (from hospital and from region) 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 that reported physicians were more likely to switch hospitals; the probability of practicing at the injury hospital fell by 5.0% after a malpractice report overall (Panel B of Table 4), with the largest changes at 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 66 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 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. We explore the possibility of hospital demand responses further in Figure A13. We test for differential effects of reports at EDs that employ large numbers of EM specialists (relative to patient volume), which may face fewer barriers replacing accused physicians. We find no evidence of larger decreases in discharge volume for physicians working at injury hospitals with above median staffing levels.

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 substantially 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 reports led to a decrease of 17 discharges per quarter (2.1%) among physicians with early resolution claims versus 86 discharges per quarter (10.2%) for late resolution claims (difference = 69, standard error = 38). In addition, we find that physicians with shorter claims experienced a sharp decrease in discharges immediately after the report, followed by a recovery in labor supply (Figure A14, Panel A). Physicians with longer claims, on the other hand, had no recovery in labor supply. We draw similar conclusions if we follow physicians for three years after the report (instead of two), suggesting a long-lasting effect of malpractice allegations (Figure A14, Panel B). These results are consistent with our finding that labor supply rebounds somewhat after the final disposition of the case (Table 3 and Figure A11, Panel C).

Overall, our results are most consistent with physicians taking on fewer shifts after a malpractice report 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, 2020).[42] A decrease of 72 discharges per quarter, therefore, equates to approximately 5 fewer shifts per quarter (out of about 48), or 1.5 fewer patients per shift.[43]

VI. 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 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.

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

[43] We are not able to observe or construct shifts using our data, which do not include time stamps beyond year and quarter indicators. Prior work suggests that physician decrease work hours by 3% for a 10% increase in malpractice pressure (Helland and Showalter, 2009). Working less has potential financial consequences for EM physicians, many of whom have at least some productivity-based compensation (Kane, 2015).

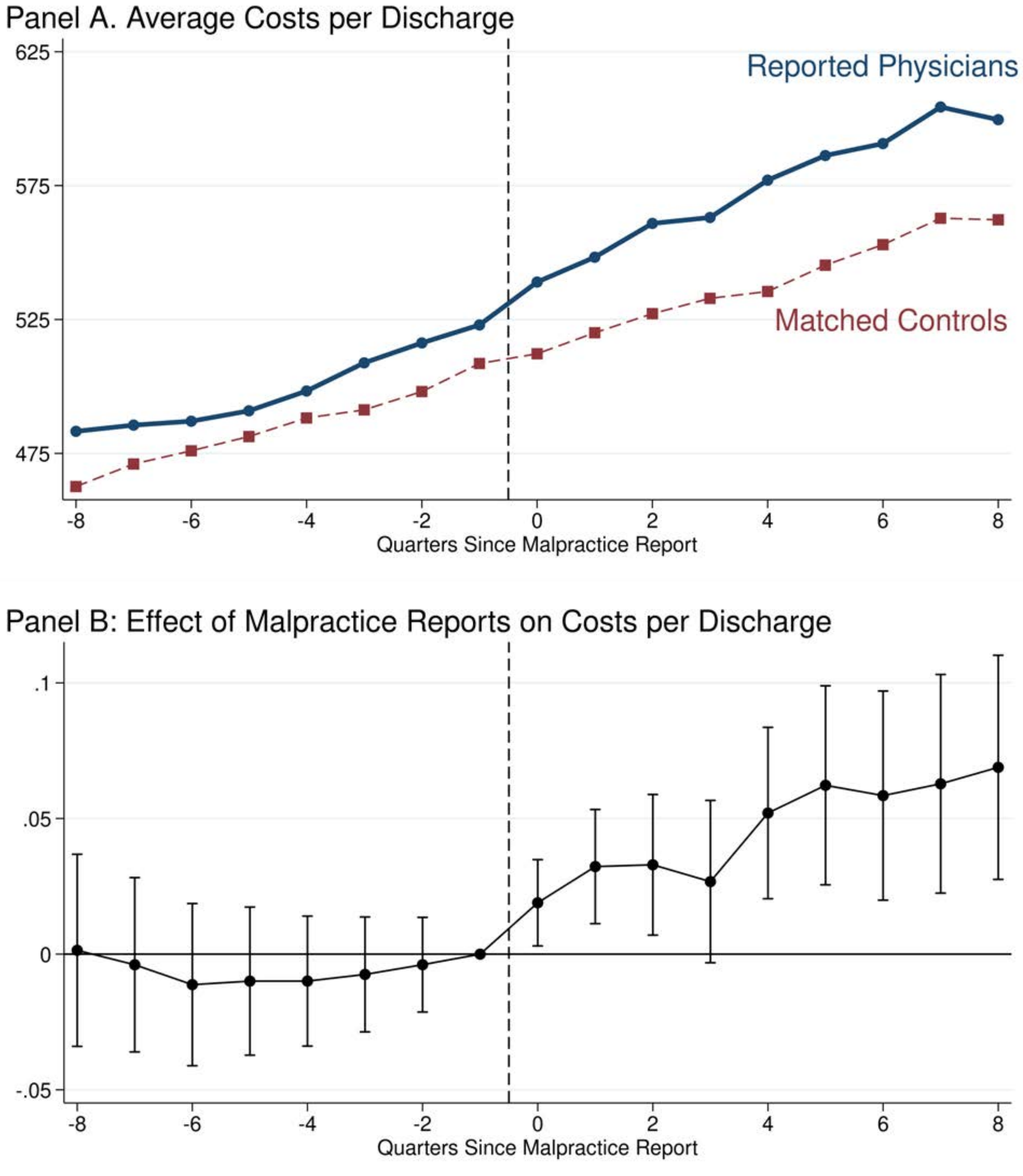


Figure 4: Costs per Discharge Before and After Malpractice Reports

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-match fixed effects, controls for 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 by physician and match. Costs are reported in 2009 dollars.

Table 5: Effect of Malpractice Reports on Costs per Discharge

	Ln(Costs)		
	(1)	(2)	(3)
Reported*Post	0.0510	0.0328	0.0484
	(0.0145)	(0.0126)	(0.0147)
Injured*Post		0.00488	
		(0.0169)	
Sued*Post		0.0210	
		(0.0193)	
Disposed*Post		0.0245	
		(0.0203)	
Observations	11201373	11201373	9815287
Claim Timing Controls		X	
Drop High Volume			X
Number of Physicians	708	708	660
Mean Costs	499	499	519

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 and match. All regressions include controls for physician-match 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.

Table 5 documents the overall effect of malpractice reports on costs in the two years after malpractice allegations.[44] Total costs increased by 5.1% 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 3.3% after the report and smaller, statistically insignificant changes after other claim events. Column 3 shows results when we exclude reported physicians with baseline discharge volumes in the highest decline (and their matches). Our results are similar using this specification, with an estimated increase in costs of 4.8%. Further robustness checks are available in the Appendix. We estimate similar effects of malpractice allegations when we use five alternate matching procedures, as described previously (Table A2). We also draw similar conclusions if we model charges instead of costs (Figure A15, Panel A), add hospital fixed effects to the regression (Panel B) and model linear costs rather than the log transformation (Panel C).

[44]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.

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. Nevertheless, we explore this issue further in Figure A16, re-estimating our treatment intensity analysis with discharge costs standardized to the median cost in its presenting condition-age group-payer bin. We estimate small and statistically insignificant changes in standardized costs, suggesting that physicians are not shifting their practice to patients with different cost types. Moreover, if reported physicians seek out straightforward, inexpensive cases, our sample of remaining discharges will grow less complex and our estimates of the increase in costs will be understated relative to the true effect.

VI.A. 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 discharges that include screening services only versus higher levels of care. We find a generalized increase in spending across service types. Reports led to an increase in general ER costs and to increased use of radiology and laboratory services. We estimate a 3.0% and 2.4% increase in the probability of receiving radiology and laboratory treatments, respectively. These results are consistent with a role for defensive medicine in driving use of diagnostic care, where the risk of service provision low (Baicker, Fisher and Chandra, 2007; Frakes, 2015; Frakes and Gruber, 2019).

Panels B through D examine the impacts of malpractice reports on treatment intensity by three measures of clinical severity. The first, in Panel B, sorts presenting conditions by the low risk, high risk, and uncertain risk categories discussed previously (Section V.A). There are statistically similar increases in costs across all three risk groups. Panel C considers conditions divided according to the coefficient of variation (CV) in costs per discharge within a CCS group-payer pair. The intuition behind this analysis is that the CV may serve as a proxy for the extent to which care is protocolized (when the CV 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 CV quartiles.

In Panel D, we consider patients who visited the ED for a "nondeferrable" condition, where treatment cannot be delayed. Because discretionary care is more likely to occur during the week, (Card, Dobkin and Maestas, 2009; Doyle, Graves and Gruber, 2017), we

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	0.014	0.006	11,201,373	708	0.46
Ln(Radiology Costs)	-0.031	0.017	5,270,876	708	378
Any Laboratory Costs	0.011	0.003	11,201,373	708	0.46
Ln(Laboratory Costs)	0.032	0.021	5,256,980	708	195
Ln(Emergency Room Costs)	0.064	0.019	11,177,229	708	162
B. Ln(Costs) by Risk Type					
Low	0.046	0.016	1,668,230	708	374
High	0.047	0.017	758,391	708	621
Uncertain	0.060	0.014	2,183,733	708	768
C. Ln(Costs) by Protocol Uncertainty					
Bottom Quartile	0.040	0.014	2,797,065	708	810
Middle Quartiles	0.058	0.014	5,861,431	708	429
Top Quartile	0.048	0.019	2,542,865	708	309
D. Ln(Costs) by Acuity					
Nondeferrable	0.079	0.016	858,687	708	451
E. Diagnostic Imaging Tests					
High-Value Chest Imaging Test	-0.022	0.016	39,510	539	0.69
Low-Value Lumbar Imaging Test	0.013	0.009	278,484	564	0.31
Low-Value Head Imaging Test	0.022	0.011	220,014	567	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), acuity of presenting condition (Panel D) and high-value versus low-value care (Panel E). All regression analyses include controls for physician-match fixed effects, physician tenure and calendar time fixed effects, as well as patient age, sex, race, presenting condition, visit day and insurance status. Panel E also includes hospital by data-version fixed effects. Standard errors are clustered by physician and match. Costs are reported in 2009 dollars.

identify nondeferrable conditions as those where weekend visits are equally likely to weekday visits.[45] We estimate an increase in costs among nondeferrable conditions of 7.9%, which is statistically similar to the overall change in treatment intensity, albeit larger in magnitude.

Overall, we find little evidence that increases in treatment intensity were restricted to clinically complicated cases (Panels B-D). One concern about these clinical severity analyses is that we are missing patients who are admitted to the hospital. To the extent that reported

[45] We include conditions as nondeferrable if the difference between their weekend admission rate and 2/7 is in the bottom 10% across all ED discharges. We do not study analogous changes in the volume of patients discharged with nondeferrable conditions because changes could reflect both responses to clinical severity and how many weekend shifts a physician works, making interpretation difficult.

physicians differentially increase 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 E, 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 statistically meaningful change in the use of high-value chest imaging after malpractice reports. We estimate no statistical change in the use of low-value lumbar imaging but a significant 2.2 percentage point (standard error = 1.1), or 5.2%, 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 A18, Panel A). We draw similar conclusions using an alternate sample restriction to account for the change in CPT coding across data versions (Figure A17, Panel B of Figure A18).

VII. 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 patient, 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 Panel A of Table A3 examine the impact of malpractice reports across clinical similarity groups. Treatment intensity responses to malpractice reports are statistically and economically similar among clinically similar patients (6.1%, standard error = 1.9%) versus dissimilar patients (5.4%, standard error = 1.6%). We likewise find that physicians decreased labor supply proportionately among patients who resembled the patient in their case versus patients who did not (7.6% versus 8.4%). Thus, there is no evidence that physicians "learned" to avoid patients who are most like the one for which the claim was filed.

In Panels B-C of Figure 5, we examine responses to malpractice reports by two other claim characteristics. 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

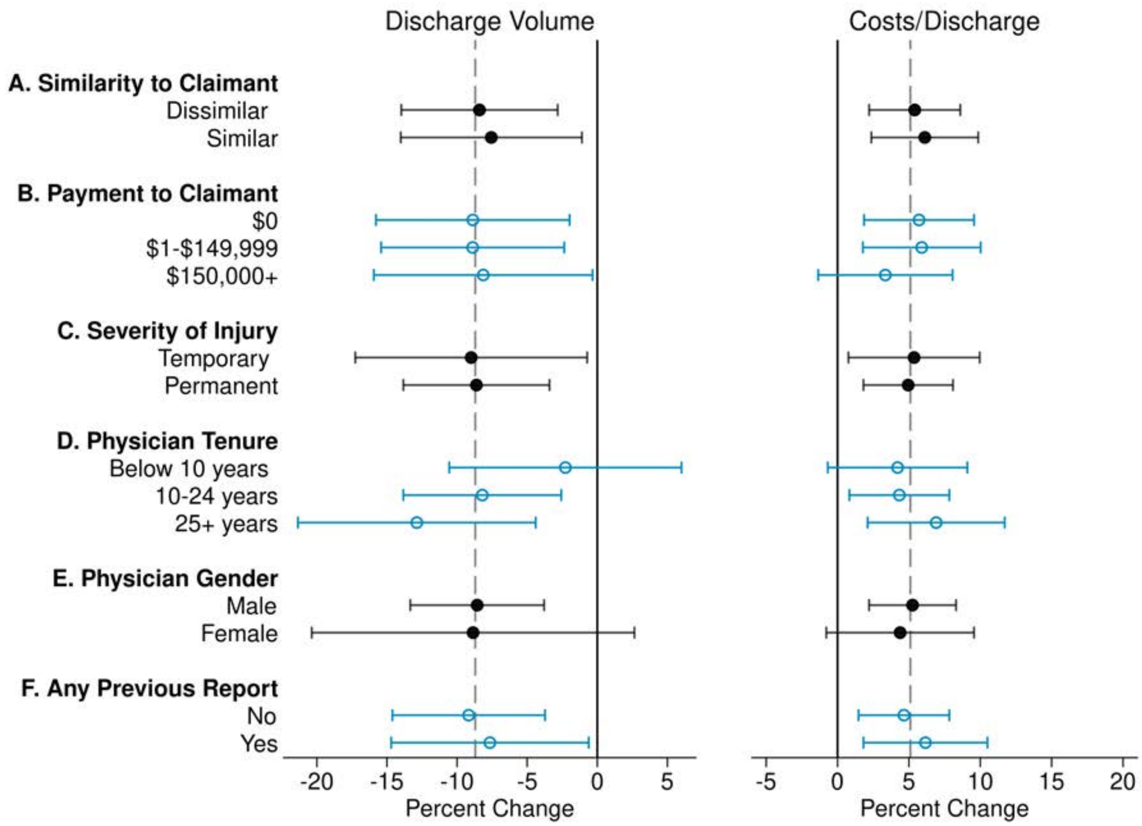


Figure 5: Heterogeneous Effects of Malpractice Reports

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-match 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 by physician and match.

was paid, generally an indicator of greater negligent damage.[46] There are statistically and economically similar decreases in labor supply among physicians facing claims that resolved in their favor without a payment to the claimant (8.9%, standard error = 3.5%), relative to physicians facing claims resulting in large payments to the claimant (8.1%, standard error = 4.0%). Treatment intensity responses are likewise large and statistically significant even among physicians with $0 claims (5.7%, standard error = 2.0%). 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.

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

In Panels D-F, we study whether the impact of malpractice reports varies according to physician characteristics, rather than claim characteristics. Panel D considers physicians divided by tenure. The estimated decrease in discharge volume is relatively small for physicians with less than 10 years of practice (2.3%, standard error = 4.2%) versus 25+ years of practice (12.9%, standard error = 4.3%), though labor supply responses are statistically similar across groups (p = 0.07). Treatment intensity effects are also similar for physicians with varying experience levels. In Panel E, we find that responses to malpractice reports are similar across female versus male physicians.

Lastly, in Panel F, we test for differential changes in behavior among potential poor performers: physicians with multiple malpractice allegations made against them.[47] There are no meaningful differences in labor supply or spending among physicians with repeated claims, relative to physicians with a single claim. In Table A3, we use an alternative definition of potential "bad apple" physicians: those with multiple *paid* claims (Studdert et al., 2016). We find no change in labor supply among physicians with multiple paid claims (0.3%, standard error =4.9%), but a large decrease among physicians without multiple payments (9.9%, standard error = 2.6%); these labor supply changes are economically and statistically different from each other (p = 0.038), suggesting that physicians' labor supply responses to malpractice allegations may attenuate with repeated exposure. Treatment intensity responses are statistically similar across the subgroups, though the effect among physicians with multiple paid claims is larger in magnitude.

VIII. 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 8.7% reduction in cases seen, coupled with a 5.1% increase in costs per case, leads to a net decrease in resource use of approximately 4.0% (standard error = 2.9%) after allegations.[48]

[47]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 malpractice 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.

[48]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.

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.[49] 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% greater intensity in the pre-report period, after controlling for hospital-quarter fixed effects and patient characteristics (Table A5).[50] Taking this point estimate into account, the net effect of the malpractice reports on aggregate spending is the 5.1% 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 4.6% (standard error = 1.3%).

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 1.4% 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 in the same ED. Our estimates would be notably larger if patients were reallocated to inpatient physicians, for example.[51]

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. Recent evidence from the Congressional Budget Office (2019) suggests that damage caps have no effect on the volume of malpractice claims in the first two years after enactment but decrease malpractice claims by about 20% after three or more years. Our results imply that a 20% reduction in claims would lead to a reduction in ED costs of 0.3%.[52] Existing work on tort reforms

[49] We find little evidence that faster accumulation of reports within an ED was associated with decreases in discharge volume (Table A4). EDs experienced an average of 3 malpractice reports against EM specialists during our study period (Figure A19). 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.

[50] We model pre-report spending differences among physicians with at least ten discharges in a hospital-quarter cell. Estimated differences in spending are greater at 2.7% and 3.7% if we impose restrictions of 50 discharges and 100 discharges per hospital-quarter cell, respectively.

[51] In 2005, spending on inpatient admissions that originated from the ED was about 9 times higher than spending on ED discharges, controlling for hospital-quarter 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.

[52] With a 20% reduction in claims, our results imply that malpractice reports would affect approximately 24% of all discharges and lead to a 1.1% increase in ED spending (4.6%*0.24)

and spending is likewise mixed, with recent reviews cautioning against firm conclusions; Mello and Kachalia (2016) and Congressional Budget Office (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% (Congressional Budget Office, 2009).[53] Our results likewise suggest a modest change in overall spending, but imply a larger role for malpractice liability in influencing physician behavior.

A close study to ours examines the effect of malpractice suits among EM physicians who were in a national, physician-owned ED group and finds no changes in treatment intensity (Carlson et al., 2020). This paper observes a smaller sample of physicians than in our work (65 versus 296) and defines a shorter panel around the malpractice event (8 months versus 4 years). Treatment intensity is defined differently than in our paper – average monthly intensity as opposed to intensity per discharge – and regression analyses are unable to control for underlying patient characteristics. When we run our analysis following this approach – defining shorter panel requirements around malpractice suits (rather than reports) and analyzing average treatment intensity results at the quarter level – we estimate a statistically insignificant increase in treatment intensity of 0.8% (standard error = 1.1%).[54] Short panel requirements around the malpractice events are important to the diverging results.

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 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. For economic policy, 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.

We further find that responses to malpractice allegations are generalized, not restricted to patients with high-risk conditions. Thus, our results are not well-aligned with models of physician behavior in which malpractice pressure changes physicians' treatment decisions

[53]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).

[54]Carlson et al. (2020) estimate a decrease of 0.5% (standard error = 1%).

according to the risk of care provision (Currie and Macleod, 2008; Frakes, 2015; Paik, Black and Hyman, 2017). In models of this form, physicians would treat fewer high risk patients, for example, which we do not observe. Instead, our results are more consistent with a model of behavior in which malpractice allegations uniformly increase the expected gains from spending more time with each patient or increase physicians' disutility of labor – i.e., a model where malpractice allegations change the shadow price of physicians' time (Tai-Seale and McGuire, 2012; Chan, 2018; Silver, 2020). In these models, physicians would respond to malpractice pressure by seeing fewer patients and treating all patients more intensively, as we find they do. Of note, the muted relationship between malpractice allegations and patient risk may be specific to the ED setting, where physicians have limited scope to select their patients.

Our results suggest several areas for future work. First, we do not attempt to calculate the welfare effects of malpractice allegations, which likely involve spillover effects of accusations onto unreported physicians. 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. Even without spillover effects, the reallocation of patients across physicians can affect welfare if reported and unreported physicians vary in their productivity, and we do not attempt to value this reallocation. 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 – the ED – and the results may not be applicable to other contexts.

References

Abadie, Alberto, and Jann Spiess. 2022. "Robust Post-Matching Inference." *Journal of the American Statistical Association*, 117(538): 983–995.

Abaluck, Jason, Leila Agha, Chris Kabrhel, Ali Raja, and Arjun Venkatesh. 2016. "The Determinants of Productivity in Medical Testing: Intensity and Allocation of Care." *American Economic Review*, 106(12): 3730–3764.

Arnold, David. 2022. "The Impact of Privatization of State-Owned Enterprises on Workers." *American Economic Journal: Applied Economics*, 14(4): 343–80.

Avraham, Ronen, and Max Schanzenbach. 2015. "The impact of tort reform on intensity of treatment: Evidence from heart patients." *J of Health Economics*, 39: 273–288.

Avraham, Ronen, Leemore Dafny, and Max Schanzenbach. 2012. "The Impact of Tort Reform on Employer-Sponsored Health Insurance Premiums." *Journal of Law, Economics, and Organization*, 28(4): 657–686.

Baicker, Katherine, Elliot Fisher, and Amitabh Chandra. 2007. "Malpractice Liability Costs And The Practice Of Medicine In The Medicare Program." *Health Affairs*, 26(3): 841–852.

Barnett, Michael L., Andrew R. Olenski, and Anupam B. Jena. 2017. "Patient Mortality During Unannounced Accreditation Surveys at US Hospitals." *JAMA Internal Medicine*, 177(5): 693–700.

Brown, Terrence, Melissa McCarthy, Gabor Kelen, and Frederick Levy. 2010. "An Epidemiologic Study of Closed Emergency Department Malpractice Claims in a National Database of Physician Malpractice Insurers." *Academic Emerg Med*, 17(5): 553–560.

Burstin, Hellen, William Johnson, Stuart Lipsitz, and Troyen Brennan. 1993. "Do the Poor Sue More? A Case-Control Study of Malpractice Claims and Socioeconomic Status." *Journal of the American Medical Association*, 270(14): 1697–1701.

Callaway, Brantly, and Pedro H.C. Sant'Anna. 2021. "Difference-in-Differences with multiple time periods." *Journal of Econometrics*, 225(2): 200–230.

Card, David, Carlos Dobkin, and Nicole Maestas. 2009. "Does Medicare Save Lives?" *The Quarterly Journal of Economics*, 124(2): 597–636.

Carlson, Jestin, Krista Foster, Bernard Black, Jesse Pines, Christopher Corbit, and Arvind Venkat. 2020. "Emergency Physician Practice Changes After Being Named in a Malpractice Claim." *Annals of Emergency Medicine*, 75(2): 221–235.

Carrier, Emily, James Reschovsky, Michelle Mello, Ralph Mayrell, and Michelle Katz. 2010. "Physicians' Fears Of Malpractice Lawsuits Are Not Assuaged By Tort Reforms." *Health Affairs*, 29(9): 1585–1592.

Chalfin, Aaron, and Justin McCrary. 2017. "Criminal Deterrence: A Review of the Literature." *Journal of Economic Literature*, 55(1): 5–48.

Chan, David. 2018. "The Efficiency of Slacking Off: Evidence from the Emergency Department." *Econometrica*, 86(3): 997–1030.

Chandra, Amitabh, and Douglas Staiger. 2007. "Productivity spillovers in health care: Evidence from the treatment of heart attacks." *J of Political Economy*, 115(1): 103–140.

Chandra, Amitabh, Anupam Jena, and Seth Seabury. 2013. "Defensive Medicine May Be Costlier Than It Seems." *Wall Street Journal*, A13, Feb. 7.

Chang, Cindy, and Ziad Obermeyer. 2020. "Association of Clinical Characteristics With Variation in Emergency Physician Preferences for Patients." *Journal of the American Medical Association: Network Open*, 3(1).

Clemens, Jeffrey, and Joshua Gottlieb. 2014. "Do Physicians' Financial Incentives Affect Medical Treatment and Patient Health?" *American Economic Review*, 104(4).

Congressional Budget Office. 2009. "Letter to Senator Orrin G. Hatch."

Congressional Budget Office. 2019. "How Do Changes in Medical Malpractice Liability Laws Affect Health Care Spending and the Federal Budget?"

Currie, Janet, and Bentley Macleod. 2008. "First Do No Harm? Tort Reform and Birth Outcomes." *Quarterly Journal of Economics*, 123(2): 795–830.

Currie, Janet, and Bentley Macleod. 2017. "Diagnosing Expertise: Human Capital, Decision Making, and Performance among Physicians." *J of Labor Econ*, 35(1): 1–43.

Cutler, David, Jonathan Skinner, Ariel Dora Stern, and David Wennberg. 2019. "Physician Beliefs and Patient Preferences: A New Look at Regional Variation in Health Care Spending." *American Economic Journal: Economic Policy*, 11(1): 192–221.

Cutler, David, Robert Huckman, and Mary Beth Landrum. 2004. "The Role of Information in Medical Markets: An Analysis of Publicly Reported Outcomes in Cardiac Surgery." *American Economic Review*, 94(2): 342–346.

Danzon, Patricia. 1991. "Liability for Medical Malpractice." *Journal of Economic Perspectives*, 5(3): 51–69.

Daw, Jamie, and Laura Hatfield. 2018. "Matching and Regression to the Mean in Difference-in-Differences Analysis." *Health Services Review*, 53(6): 4138–4156.

de Chaisemartin, Clément, and Xavier D'Haultfœuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110(9): 2964–96.

Deshpande, Manasi, and Yue Li. 2019. "Who Is Screened Out? Application Costs and the Targeting of Disability Programs." *American Economic Journal: Economic Policy*, 11(4): 213–48.

Donohue, John, and Daniel Ho. 2007. "The Impact of Damage Caps on Malpractice Claims: Randomized Inference with Difference-in-Differences." *J Emp Legal Studies*, 4(1): 69–102.

Doyle, Joseph J., John A. Graves, and Jonathan Gruber. 2017. "Uncovering waste in US healthcare: Evidence from ambulance referral patterns." *J Hlth Econ*, 54: 25–39.

Dranove, David, and Yasutora Watanabe. 2010. "Influence and Deterrence: How Obstetricians Respond to Litigation against Themselves and Their Colleagues." *American Law and Economics Review*, 82(1): 69–94.

Dranove, David, Dan Kessler, Mark McClellan, and Mark Satterthwaite. 2004. "Is More Information Better? The Effects of 'Report Cards' On Health Care Providers." *Journal of Political Economy*, 111(3): 555–588.

Dranove, David, Subramaniam Ramanarayanan, and Yasutora Watanabe. 2012. "Delivering Bad News: Market Responses to Negligence." *The Journal of Law and Economics*, 55(1): 1–25.

Dubay, Lisa, Robert Kaestner, and Timothy Waidmann. 2001. "The impact of malpractice fears on cesarean section rates." *J of Health Economics*, 20(4): 591–611.

Durrance, Christine Piette. 2009. "Noneconomic Damage Caps and Medical Malpractice Claim Frequency: A Policy Endogeneity Approach." *The Journal of Law, Economics, and Organization*, 26(3): 569–591.

Durrance, Christine Piette, and Scott Hankins. 2018. "Medical Malpractice Liability Exposure and OB/GYN Physician Delivery Decisions." *Health Services Research*, 53(4): 2633–2650.

Fadlon, Itzik, and Torben Nielsen. 2019. "Family Health Behaviors." *American Economic Review*, 109(9): 3162–3169.

Frakes, Michael. 2013. "The Impact of Medical Liability Standards on Regional Variations in Physician Behavior: Evidence from the Adoption of National-Standard Rules." *American Economic Review*, 103(1): 257–76.

Frakes, Michael. 2015. "The Surprising Relevance of Medical Malpractice Law." *The University of Chicago Law Review*, 82: 317–392.

Frakes, Michael, and Jonathan Gruber. 2019. "Defensive Medicine: Evidence from Military Immunity." *American Economic Journal: Economic Policy*, 11(3): 197–231.

Gimm, Grant. 2010. "The Impact of Malpractice Liability Claims on Obstetrical Practice Patterns." *Health Services Research*, 45(1): 192–211.

Goldschmidt, Deborah, and Johannes F. Schmieder. 2017. "The Rise of Domestic Outsourcing and the Evolution of the German Wage Structure." *The Quarterly Journal of Economics*, 132(3): 1165–1217.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*, 225(2): 254–277. Themed Issue: Treatment Effect 1.

Grant, Darren, and Malayne McInnes. 2004. "Practice Experience and the Incidence of Cesarean Delivery: Physician-Level Longitudinal Analysis." *Inquiry*, 41: 170–88.

Hannan, Edward, Chuntao Wu, Thomas Ryan, Edward Bennett, Alfred T. Culliford, Alan Gold, Jeffrey P. Hartman, O. Wayne Isom, Robert Jones, Barbara McNeil, Eric Rose, and Valavanur Subramanian. 2003. "Do Hospitals and Surgeons with Higher Coronary Artery Bypass Graft Surgery Volumes Still Have Lower Risk-Adjusted Mortality Rates?" *Circulation*, 108(7): 795–801.

Helland, Eric, and Mark H. Showalter. 2009. "The Impact of Liability on the Physician Labor Market." *The Journal of Law and Economics*, 52(4): 635–663.

Ho, Benjamin, and Elaine Liu. 2011. "What's an Apology Worth? Decomposing the Effect of Apologies on Medical Malpractice Payments Using State Apology Laws." *Journal of Empirical Legal Studies*, 8(s1): 179–199.

Howard, David H., and Ian McCarthy. 2021. "Deterrence effects of antifraud and abuse enforcement in health care." *Journal of Health Economics*, 75: 102405.

Hyman, David A., Joshua Lerner, David J. Magid, and Bernard Black. 2023. "Association of Past and Future Paid Medical Malpractice Claims." *JAMA Health Forum*, 4(2): e225436–e225436.

Iacus, Stefano, Gary King, and Giuseppe Porro. 2012. "Causal Inference Without Balance Checking: Coarsened Exact Matching." *Political Analysis*, 20(1): 1–24.

Imbens, Guido W., and Donald B. Rubin. 2015. *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction.* Cambridge University Press.

Institute of Medicine. 2006. "The Future of Emergency Care in the United States Health System."

Jaravel, Xavier, Neviana Petkova, and Alex Bell. 2018. "Team-Specific Capital and Innovation." *American Economic Review*, 108(4-5): 1034–73.

Jena, Anupam, Amitabh Chandra, Darius Lakdawalla, and Seth Seabury. 2012. "Outcomes of Medical Malpractice Litigation Against US Physicians." *Archives of Internal Medicine*, 172(11): 892–894.

Jena, Anupam, Lena Schoemaker, Jay Bhattacharya, and Seth Seabury. 2015. "Physician spending and subsequent risk of malpractice claims: observational study." *The BMJ*, 351(h5516).

Jena, Anupam, Seth Seabury, Darius Lakdawalla, and Amitabh Chandra. 2011. "Malpractice risk according to physician specialty." *NEJM*, 365(7): 629–36.

Joseph, Joshua, Samuel Davis, Elissa Wilker, Matthew Wong, Ori Litvak, Stephen Traub, Larry Nathanson, and Leon Sanchez. 2018. "Modelling attending physician productivity in the emergency department: a multicentre study." *Emerg Med J*, 35: 317–322.

Kachalia, Allen, and Michelle Mello. 2011. "New Directions in Medical Liability Reform." *New England Journal of Medicine*, 364(16): 1564–1572.

Kachalia, Allen, Samuel Kaufman, Richard Boothman, Susan Anderson, Kathleen Welch, Sanjay Saint, and Mary Rogers. 2010. "Liability Claims and Costs Before and After Implementation of a Medical Error Disclosure Program." *Annals of Internal Medicine*, 153(4): 213–221.

Kachalia, Allen, Tejal Gandhi, Ann Puopolo, Catherine Yoon, Eric Thomas, Richard Griffey, Troy Brennan, and David Studdert. 2007. "Missed and delayed diagnoses in the emergency department: a study of closed malpractice claims from 4 liability insurers." *Annals of Emergency Medicine*, 49(2): 196–205.

Kane, Carol. 2015. "How are Physicians Paid? A Detailed Look at the Methods Used to Compensate Physicians in Different Practice Types and Specialties."

Katz, Barb. 2014. "How Much Do Emergency Physicians Make? A National Job Market Survey." American College of Emergency Physicians.

Kessler, Daniel, and Mark McClellan. 1996. "Do Doctors Practice Defensive Medicine?" *Quarterly Journal of Economics*, 111(2): 353–390.

Kessler, Daniel, and Mark McClellan. 2002. "How Liability Law affects Medical Productivity." *Journal of Health Economics*, 21(6): 931–955.

Kolstad, Jonathan. 2013. "Information and Quality When Motivation is Intrinsic: Evidence from Surgeon Report Cards." *American Economic Review*, 103(7): 2875–2910.

Lakdawalla, Darius, and Seth Seabury. 2012. "The Welfare Effects of Medical Malpractice Liability." *International Review of Law and Economics*, 32(4): 356–369.

Localio, A. Russell, Ann Lawthers, Troyen Brennan, Nan Laird, Liesi Hebert, Lynn Peterson, Joseph Newhouse, Paul Weiler, and Howard Hiatt. 1991. "Relation between Malpractice Claims and Adverse Events Due to Negligence - Results of the Harvard Medical Practice Study III." *New England Journal of Medicine*, 325: 245–251.

McClellan, Frank, Augustus White, Ramona Jimenez, and Sherin Fahmy. 2012. "Do Poor People Sue Doctors More Frequently? Confronting Unconscious Bias and the Role of Cultural Competency." *Clin Orthop Relat Res*, 470(5): 1393–1397.

Mello, Michelle, Amitabh Chandra, Atul Gawande, and David Studdert. 2010. "National Costs Of The Medical Liability System." *Health Affairs*, 29(9): 1569–1577.

Mello, Michelle, and Allen Kachalia. 2016. "Medical Malpractice: Evidence on Reform Alternatives and Claims Involving Elderly Patients."

Mello, Michelle M., Michael D. Frakes, Erik Blumenkranz, and David M. Studdert. 2020. "Malpractice Liability and Health Care Quality: A Review." *JAMA*, 323(4): 352–366.

Moghtaderi, Ali, Steven Farmer, and Bernard Black. 2019. "Damage Caps and Defensive Medicine: Reexamination with Patient Level Data." *Journal of Empirical Legal Studies*, 16(1): 26–68.

Molitor, David. 2018. "The Evolution of Physician Practice Styles: Evidence from Cardiologist Migration." *American Economic Journal: Economic Policy*, 10(1): 326–356.

Paik, Myungho, Bernard Black, and David Hyman. 2013. "The Receding Tide of Medical Malpractice Litigation: Part 2." *J of Empirical Legal Studies*, 10(4): 639–669.

Paik, Myungho, Bernard Black, and David Hyman. 2017. "Damage Caps and Defensive Medicine, Revisited." *Journal of Health Economics*, 51: 84–97.

Paik, Myungho, Bernard Black, David Hyman, William Sage, and Charles Silver. 2012. "How Do the Elderly Fare in Medical Malpractice Litigation, Before and After Tort Reform? Evidence from Texas." *American Law and Economics Review*, 14(2): 561–600.

Rodwin, Marc A., Hak J. Chang, Melissa M. Ozaeta, and Richard J. Omar. 2008. "Malpractice Premiums In Massachusetts, A High-Risk State: 1975 To 2005." *Health Affairs*, 27(3): 835–844.

Rosenbaum, Paul R., and Donald B. Rubin. 1985. "Constructing a Control Group Using Multivariate Matched Sampling Methods That Incorporate the Propensity Score." *The American Statistician*, 39(1): 33–38.

Roter, Debra. 2006. "The Patient-Physician Relationship and its Implications for Malpractice Litigation." *Journal of Health Policy and Law*, 9(7).

Sarsons, Heather. 2019. "Interpreting Signals in the Labor Market: Evidence from Medical Referrals." *Working Paper*.

Schwartz, Aaron, Bruce Langon, Adam Elshaug, Michael Chernew, and J. Michael McWilliams. 2014. "Measuring Low-Value Care in Medicare." *Journal of the American Medical Association*, 174(7): 1067–1076.

Shapiro, Bradley. 2017. "Informational Shocks, Off-Label Prescribing, and the Effects of Physician Detailing." *Management Science*, 64(12): 5925–5945.

Shurtz, Ity. 2013. "The impact of medical errors on physician behavior: Evidence from malpractice litigation." *Journal of Health Economics*, 32(2): 331–340.

Silver, David. 2020. "Haste or Waste? Peer Pressure and Productivity in the Emergency Department." *The Review of Economic Studies.*

Sloan, Frank, and John Shadle. 2009. "Is there empirical evidence for "Defensive Medicine"? A reassessment." *Journal of Health Economics*, 28(2): 481–491.

Sloan, Frank, and Lindsey Chepke. 2008. *Medical Malpractice.* Cambridge, Massachuetts:The MIT Press.

Sloan, Frank, Penny Githens, Ellen Clayton, Gerald Hickson, Douglas Gentile, and David Partlett. 1993. *Liability for Medical Malpractice.* Chicago, Illinois:University of Chicago Press.

Smith, Matthew, Danny Yagan, Owen Zidar, and Eric Zwick. 2019. "Capitalists in the Twenty-First Century." *The Quarterly Journal of Economics*, 134(4): 1675–1745.

Stevenson, David, Matthew Spittal, and David Studdert. 2013. "Does litigation increase or decrease health care quality?: a national study of negligence claims against nursing homes." *Medical Care*, 51(5): 430–436.

Studdert, David, Marie Bismark, Michelle Mello, Harnam Singh, and Matthew Spittal. 2016. "Prevalence and Characteristics of Physicians Prone to Malpractice Claims." *New England Journal of Medicine*, 374: 354–362.

Studdert, David, Matthew Spittal, Yifan Zhang, Derek Wilkenson, Harnam Singh, and Michelle Mello. 2019. "Changes in Practice among Physicians with Malpractice Claims." *New England Journal of Medicine*, 380: 1247–1255.

Studdert, David, Michelle Mello, Atul Gawande, Tejal Gandhi, Allen Kachalia, Catherine Yoon, Ann Louise Puopolo, and Troyen Brennan. 2006. "Claims, Errors, and Compensation Payments in Medical Malpractice Litigation." *New England Journal of Medicine*, 354: 2024–2033.

Sun, Liyang, and Sarah Abraham. 2020. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics.*

Tai-Seale, Ming, and Thomas McGuire. 2012. "Time is up: increasing shadow price of time in primary-care office visits." *Health Economics*, 21(4): 457–476.

Waxman, Daniel, Michael Greenberg, M. Susan Ridgely, Arthur Kellermann, and Paul Heaton. 2014. "The Effect of Malpractice Reform on Emergency Department Care." *New England Journal of Medicine*, 371(16): 1518–1525.

Zeiler, Kathryn, Charles Silver, Bernard Black, Davie Hyman, and William Sage. 2007. ""Physicians' Insurance Limits and Malpractice Payments: Evidence from Texas Closed Claims: 1990–2003." *Journal of Legal Studies*, 36(S2): S9–45.

Zuckerman, Stephen, Randall Bovbjerg, and Frank Sloan. 1990. "Effects of Tort Reforms and Other Factors on Medical Malpractice Insurance Premiums." *Inquiry*, 27(2): 167–182.

For Online Publication: Appendix

AI 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.

AII 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.[55] 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 is over 20 times the next highest value among all malpractice claims in our sample and far

[55] 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.

exceeds the indemnity payment in this case of approximately $600,000. As such, we recode the defense costs to missing for this case.

AIIA. 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).

AIII Appendix Tables and Figures

Table A1: Robustness Checks: Effect of Malpractice Reports on Discharge Volume, with Alternative Matching Procedures

	Main Sample	Alternative Matches				
	(1) Main	(2) Drop Low Volume	(3) No Trend	(4) CEM	(5) Baseline Volume	(6) No Replacement
Reported*Post	-72.32	-66.75	-50.78	-64.95	-45.29	-52.84
	(20.52)	(22.70)	(21.87)	(23.18)	(19.61)	(24.65)
Observations	15045	12597	15096	9894	15130	8772
Number of Physicians	709	582	690	536	711	516
Matched Reported Physicians	295	247	296	291	297	258
Percent Change	-8.7	-7.3	-6.1	-7.8	-5.4	-6.4
Mean Discharge Volume	833	914	836	830	834	820

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. Except for column 6, all regressions control for physician-match fixed effects, physician tenure and calendar time fixed effects, and standard errors are clustered by physician and by match. Column 1 repeats the results of our main model. In columns 2-6, we estimate our model using alternate matching approaches. In column 2, we impose a stricter volume restriction on the sample of physicians who are eligible for our match, dropping physicians with baseline volumes below the 25th percentile volume among reported physicians (instead of the 10th percentile). In column 3, we drop baseline volume trends as a predictor in the match. In column 4, we select controls using a one-to-one coarsened exact match; we do not impose an exact match on quarter (as in our main model) in order to increase the number of physicians with matches. In column 5, we match on baseline discharge volume in addition to the covariates in our main model. In column 6, we run a 1-1 match without replacement. This specification includes physician fixed effects (instead of physician-match fixed effects) and clusters standard errors at the match level.

Table A2: Robustness Checks: Effect of Malpractice Reports on Costs per Discharge, with Alternative Matching Procedures

	Main Sample	Alternative Matches				
	(1)	(2)	(3)	(4)	(5)	(6)
	Main	Drop Low Volume	No Trend	CEM	Baseline Volume	No Replacement
Reported*Post	0.0510	0.0428	0.0456	0.0471	0.0300	0.0563
	(0.0145)	(0.0145)	(0.0141)	(0.0173)	(0.0143)	(0.0180)
Observations	11201373	10611771	11082211	7322163	11572636	6436615
Number of Physicians	708	582	688	533	710	513
Matched Reported Physicians	295	247	296	291	297	258
Average Costs	499	487	500	502	499	500

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. Except for column 6, all regressions control for physician-match fixed effects, physician tenure and calendar time fixed effects, and standard errors are clustered by physician and by match. Column 1 repeats the results of our main model. In columns 2-6, we estimate our model using alternate matching approaches. In column 2, we impose a stricter volume restriction on the sample of physicians who are eligible for our match, dropping physicians with baseline volumes below the 25th percentile volume among reported physicians (instead of the 10th percentile). In column 3, we drop baseline volume trends as a predictor in the match. In column 4, we select controls using a one-to-one coarsened exact match; we do not impose an exact match on quarter (as in our main model) in order to increase the number of physicians with matches. In column 5, we match on baseline discharge volume in addition to the covariates in our main model. In column 6, we run a 1-1 match without replacement. This specification includes physician fixed effects (instead of physician-match fixed effects) and clusters standard errors at the match level.

Table A3: 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	-64.70	21.92	12,036	591	770	-8.4
Similar	-3.31	1.44	15,045	709	44	-7.6
B. Payment to Claimant						
$0	-75.09	29.80	11,951	527	846	-8.9
$1-$149,999	-72.90	27.34	11,594	506	821	-8.9
$150,000+	-67.45	33.00	11,560	504	829	-8.1
C. Severity of Claimant Injury						
Temporary	-68.79	32.29	11,084	476	765	-9.0
Permanent	-73.33	22.62	13,991	647	851	-8.6
D. Tenure						
Below 10 years	-18.36	34.11	10,965	469	808	-2.3
10-24 years	-70.30	24.63	12,648	568	858	-8.2
25+ years	-103.64	34.84	11,492	500	805	-12.9
E. Gender						
Male	-72.76	20.69	14,076	652	850	-8.6
Female	-67.62	44.82	10,999	471	763	-8.9
F. Previous Report						
No	-77.65	23.52	13,719	631	847	-9.2
Yes	-60.80	28.56	11,356	492	794	-7.7
G. Multiple Paid Claims						
No	-82.83	21.51	14,467	675	837	-9.9
Yes	2.21	39.41	10,608	448	803	0.3

Table continues on next page.

Table A3: (continued) Effect of Malpractice Reports by Claim and Physician Characteristics

Panel B: Costs per Discharge

	Coefficient	Standard Error	Observations	Physicians	Dependent Variable Mean	Percent Change
Ln(Costs)						
A. Similarity to Claimant						
Dissimilar	0.054	0.016	8,379,132	591	496	
Similar	0.061	0.019	586,291	591	593	
B. Payment to Claimant						
$0	0.057	0.020	8,819,890	526	512	
$1-$149,999	0.059	0.021	8,495,780	505	479	
$150,000+	0.034	0.024	8,482,107	503	503	
C. Severity of Claimant Injury						
Temporary	0.054	0.023	8,052,112	475	508	
Permanent	0.049	0.016	10,447,463	646	497	
D. Tenure						
Below 10 years	0.042	0.025	8,030,530	468	536	
10-24 years	0.043	0.018	9,398,938	567	486	
25+ years	0.069	0.024	8,368,309	499	501	
E. Gender						
Male	0.053	0.016	10,512,064	651	504	
Female	0.044	0.026	7,987,511	470	477	
F. Previous Report						
No	0.047	0.016	10,210,945	630	493	
Yes	0.062	0.022	8,288,630	491	517	
G. Multiple Paid Claims						
No	0.046	0.015	10,746,332	674	499	
Yes	0.083	0.029	7,753,243	447	502	

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-match 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 by physician and match. Costs are reported in 2009 dollars.

Table A4: 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 (0.00540)	-36.34 (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 III.D. 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.

Table A5: Cost Differential Between Reported and Unreported Physicians in the Pre-Report Period

	Log(Costs)		
	(1) 10+ discharges	(2) 50+ discharges	(3) 100+ discharges
Reported	0.0103	0.0274	0.0374
	(0.0113)	(0.0113)	(0.0112)
Observations	4366006	4229876	4106252
Mean of Dep. Var.	525	525	525

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 hospital. Samples include physicians with a minimum number of discharges per hospital-quarter, with the minimum specified in the column heading. The pre-report period includes the quarter prior to the report. All regressions include hospital-quarter fixed effects and controls for patient age, sex, race, presenting condition, visit day and insurance status. Costs are reported in 2009 dollars.

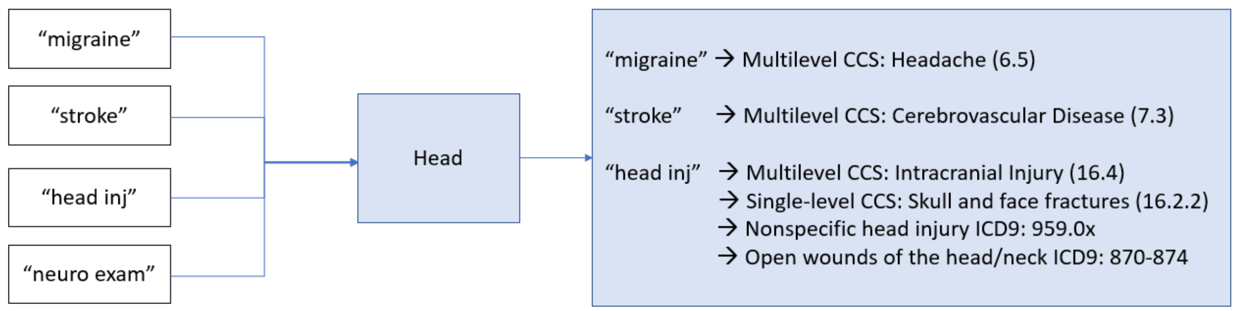


FIGURE A1:
Claim Categorization Example

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.

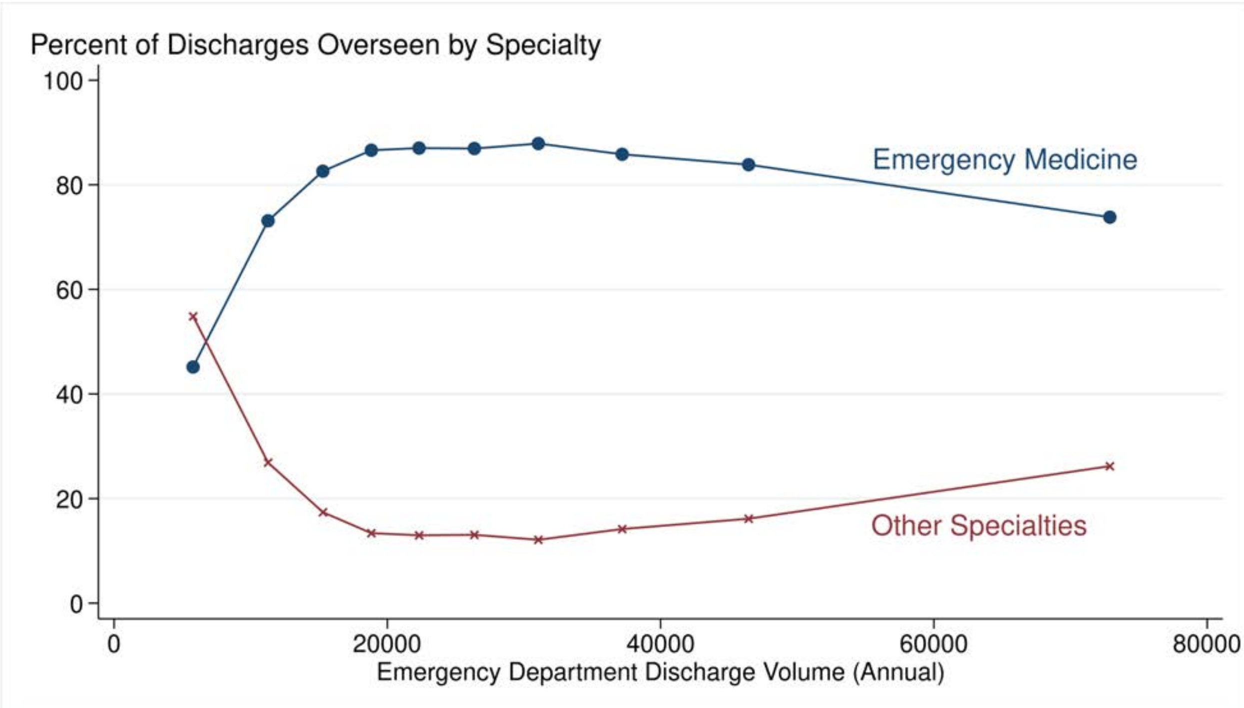


Figure A2:
Percent of Discharges Overseen by Emergency Medicine Specialists

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.

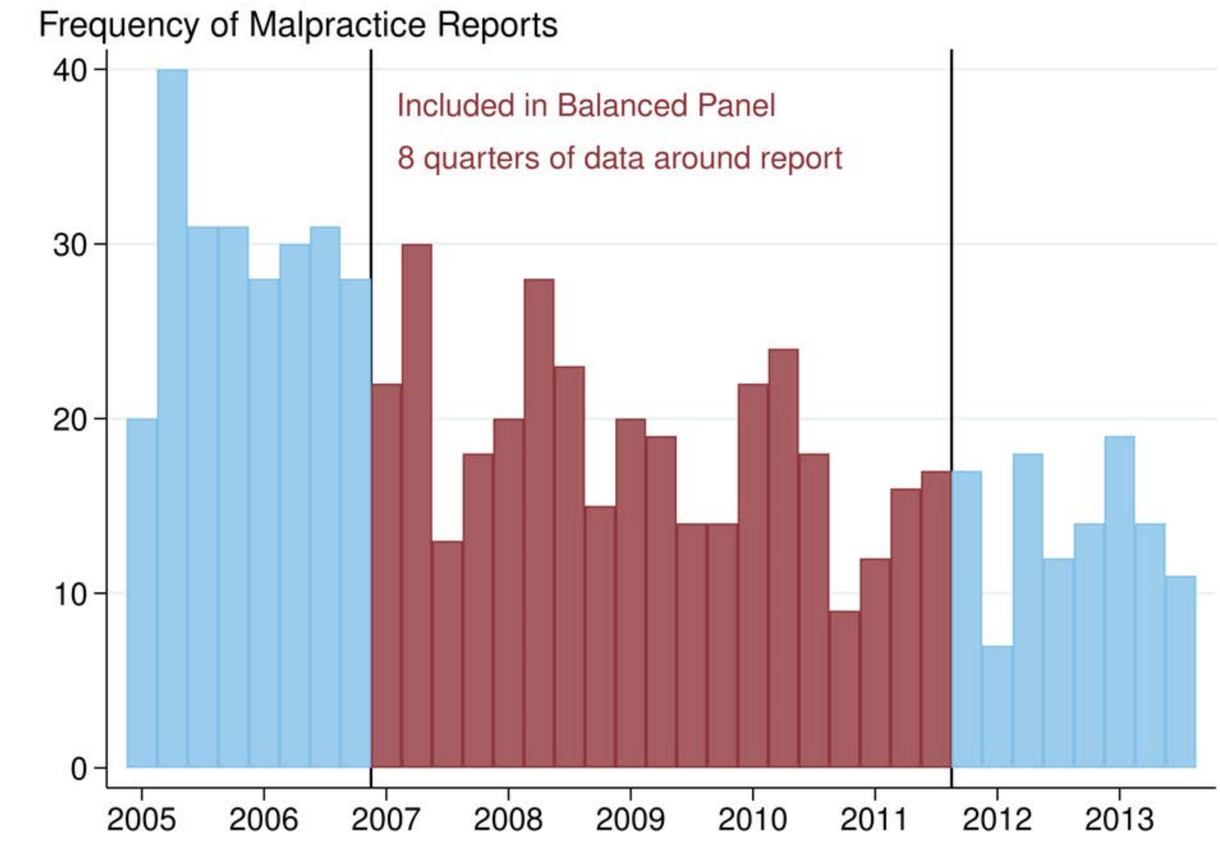


Figure A3:
Malpractice Reports per Quarter

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.

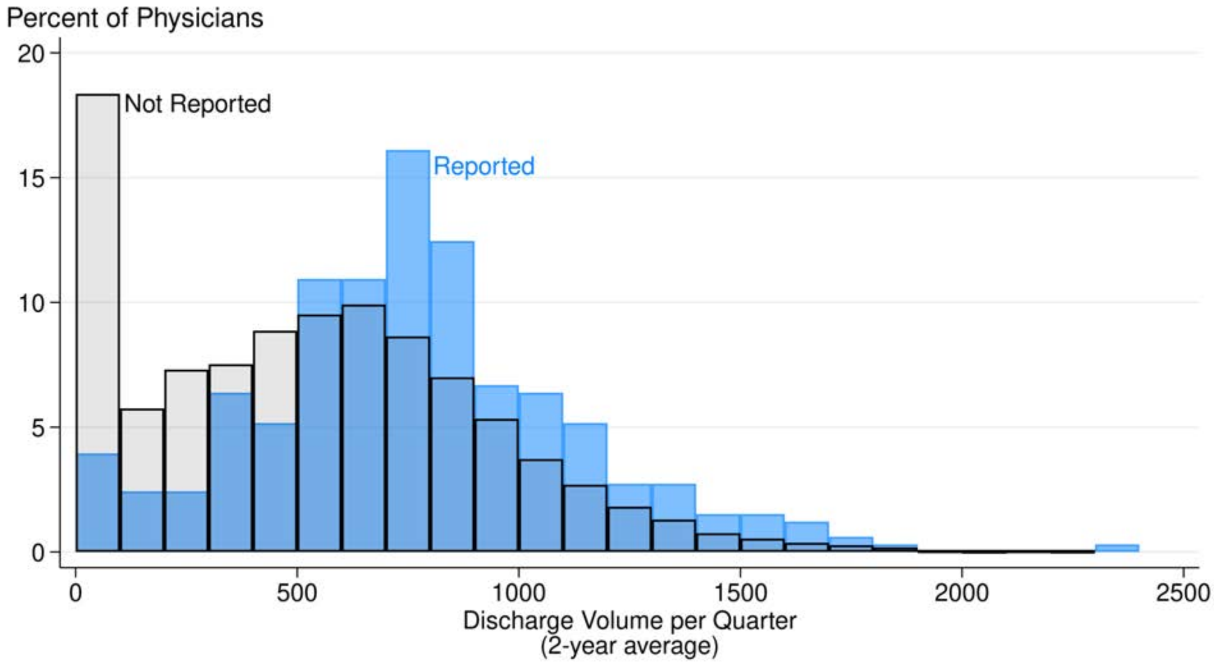


Figure A4:
Discharge Volume Among Reported and Unreported Physicians

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 III.

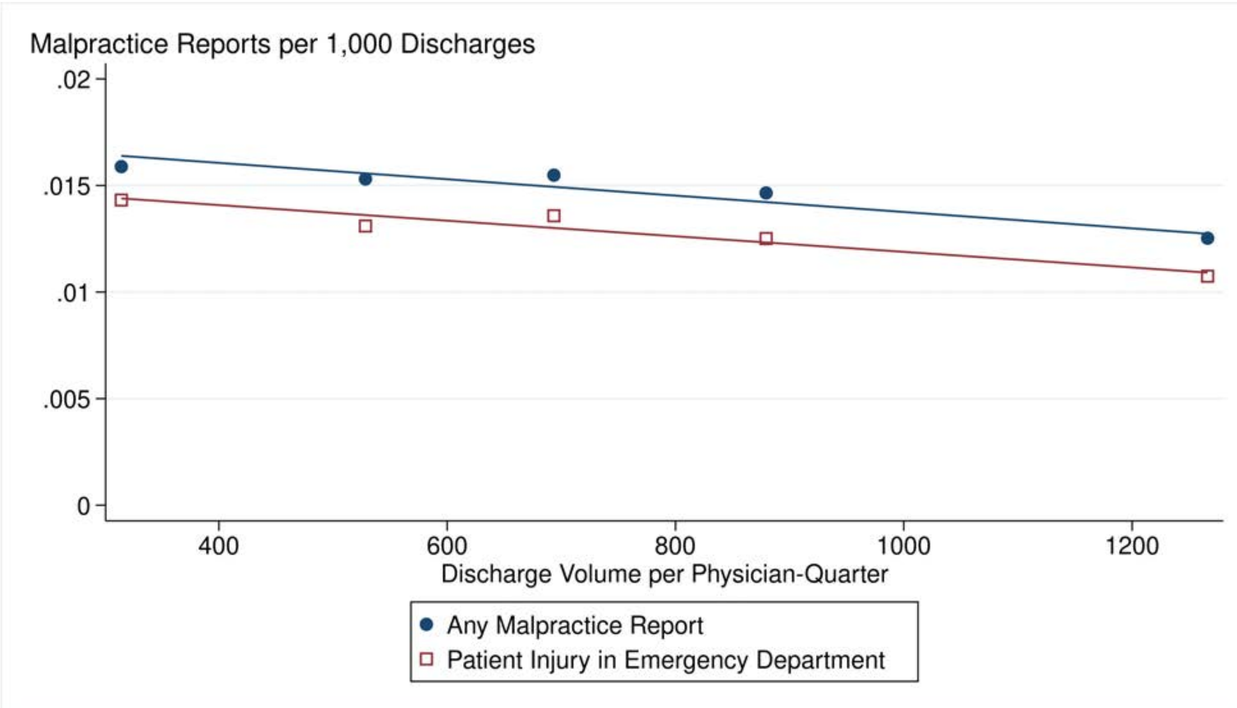


Figure A5:
Malpractice Reports per Discharge

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.

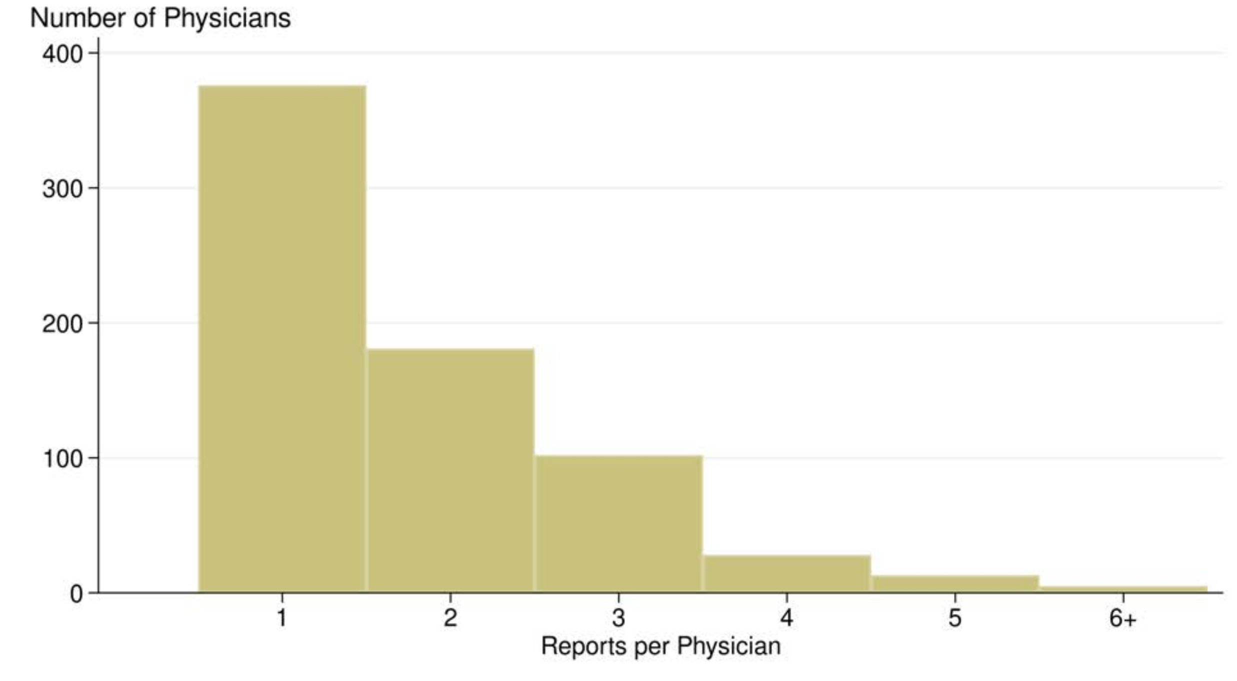


Figure A6:
Malpractice Reports per Physician

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.

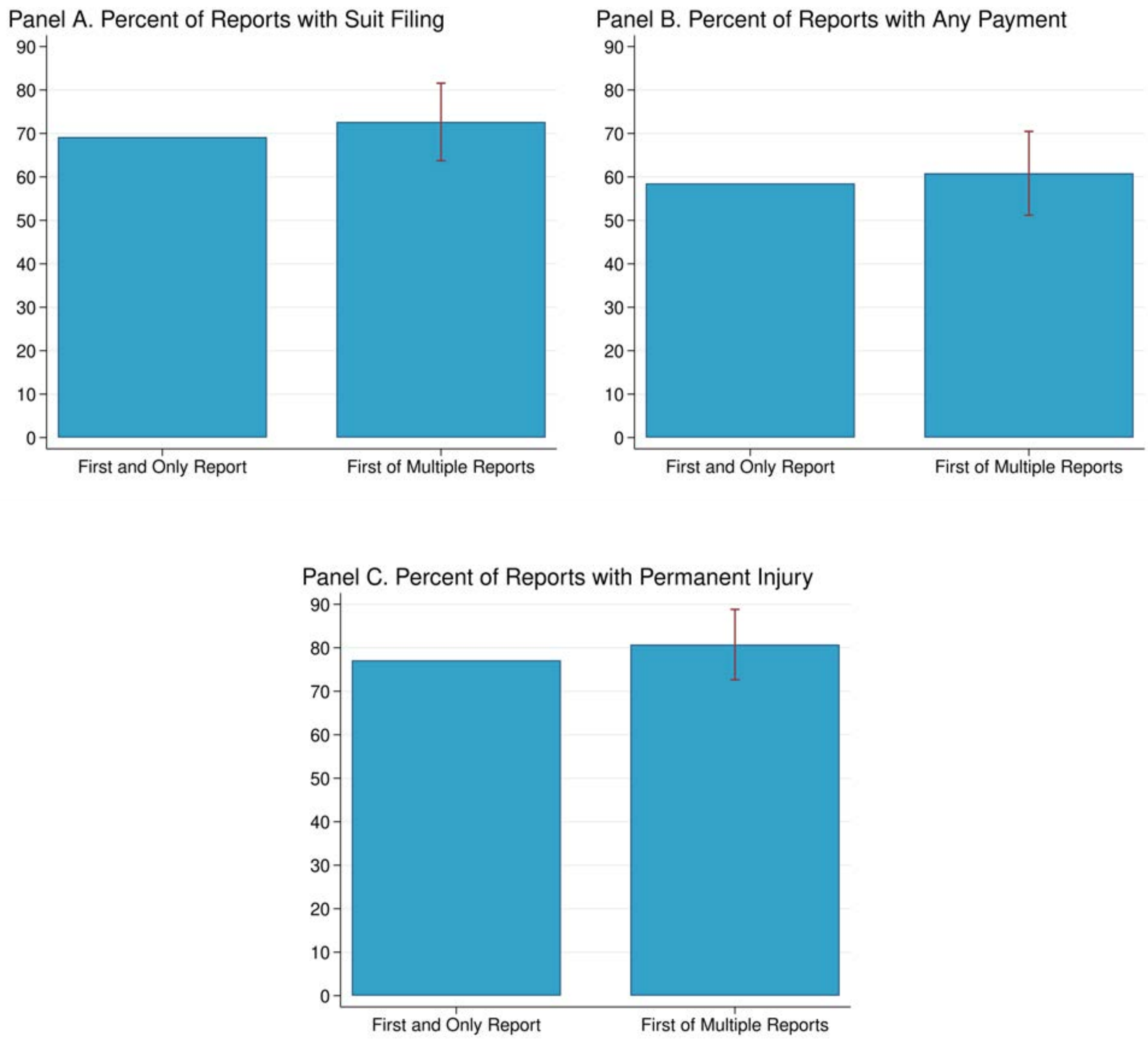


Figure A7:
Malpractice Report Characteristics among Physicians with a Single versus Multiple Reports

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.

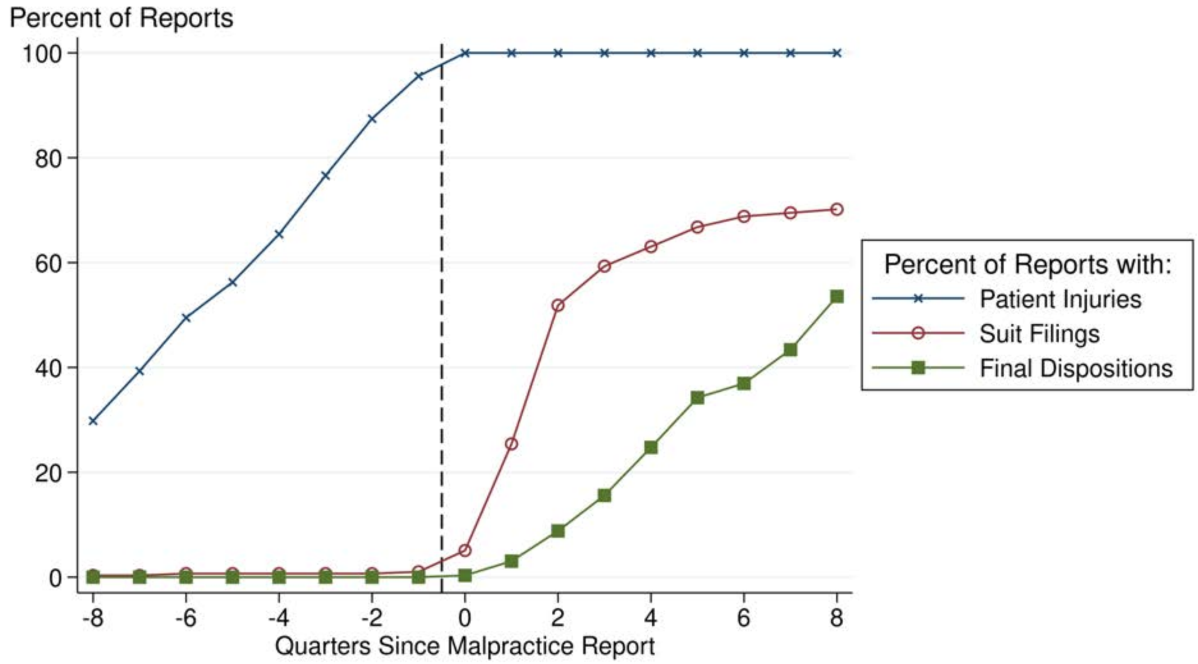


Figure A8:
Development of Malpractice Claims Before and After the Report

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.

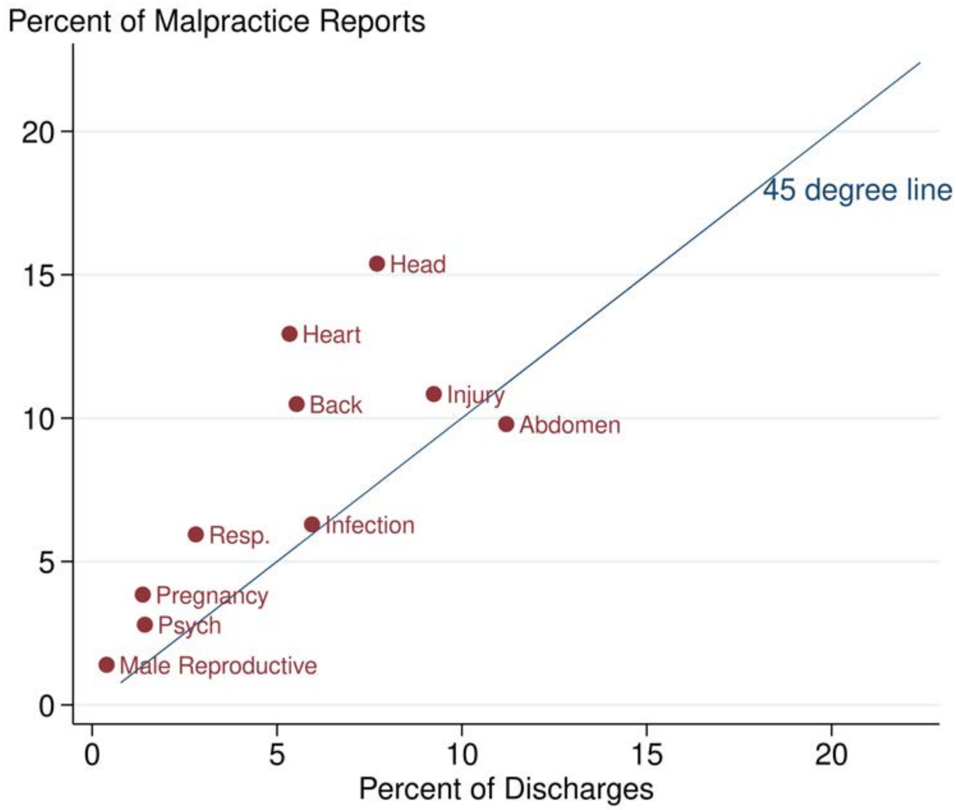
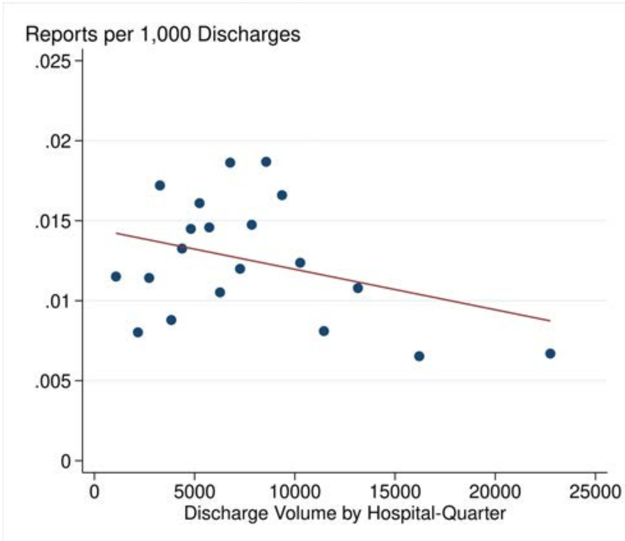


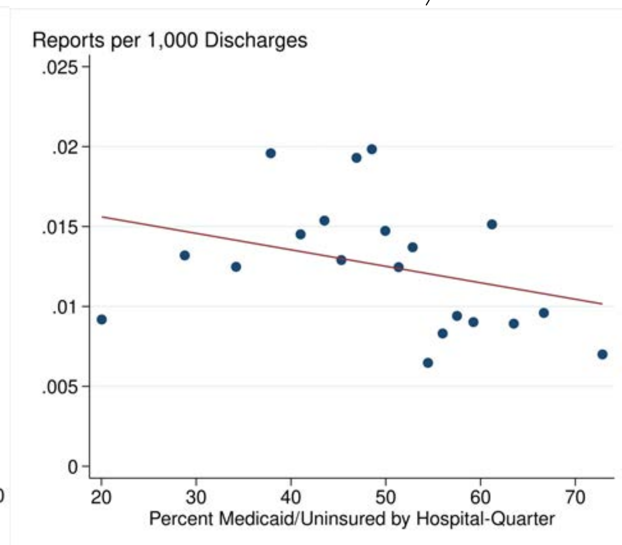
Figure A9:
Frequency of Malpractice Reports by Clinical Type

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

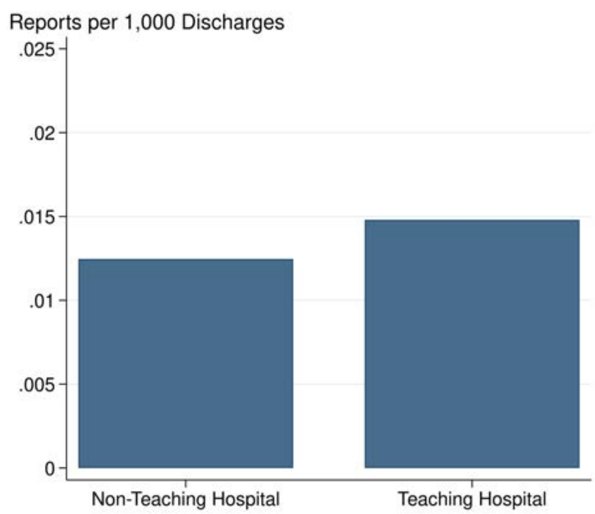
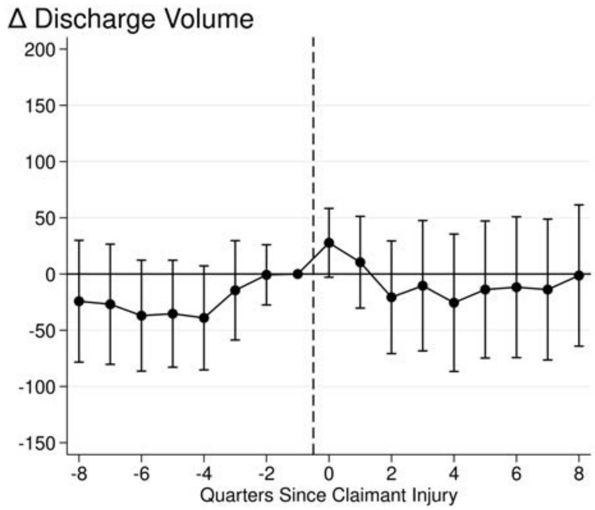


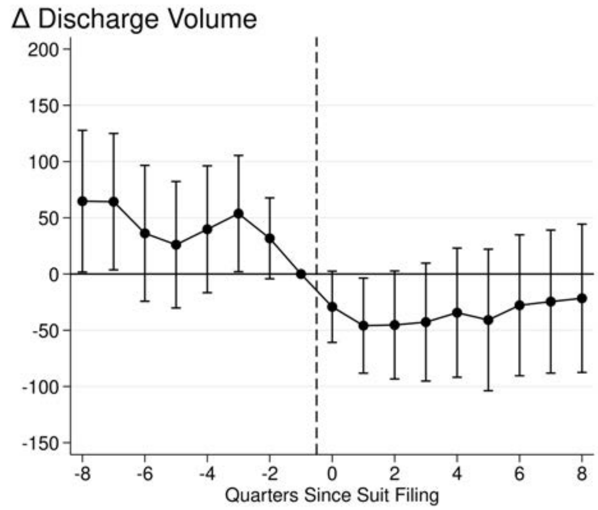
FIGURE A10:
Prevalence of Malpractice Reports Across Emergency Department Types

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.

Panel A. Claimant Injury



Panel B. Suit Filing



Panel C. Final Disposition

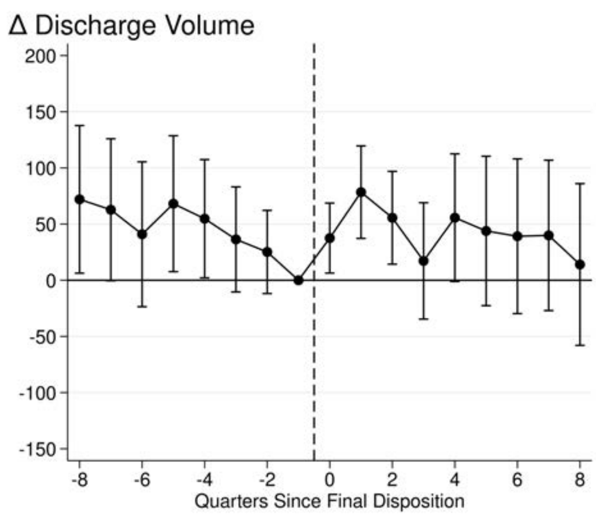


Figure A11:
Changes in Discharge Volume and the Development of the Malpractice Claim

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.

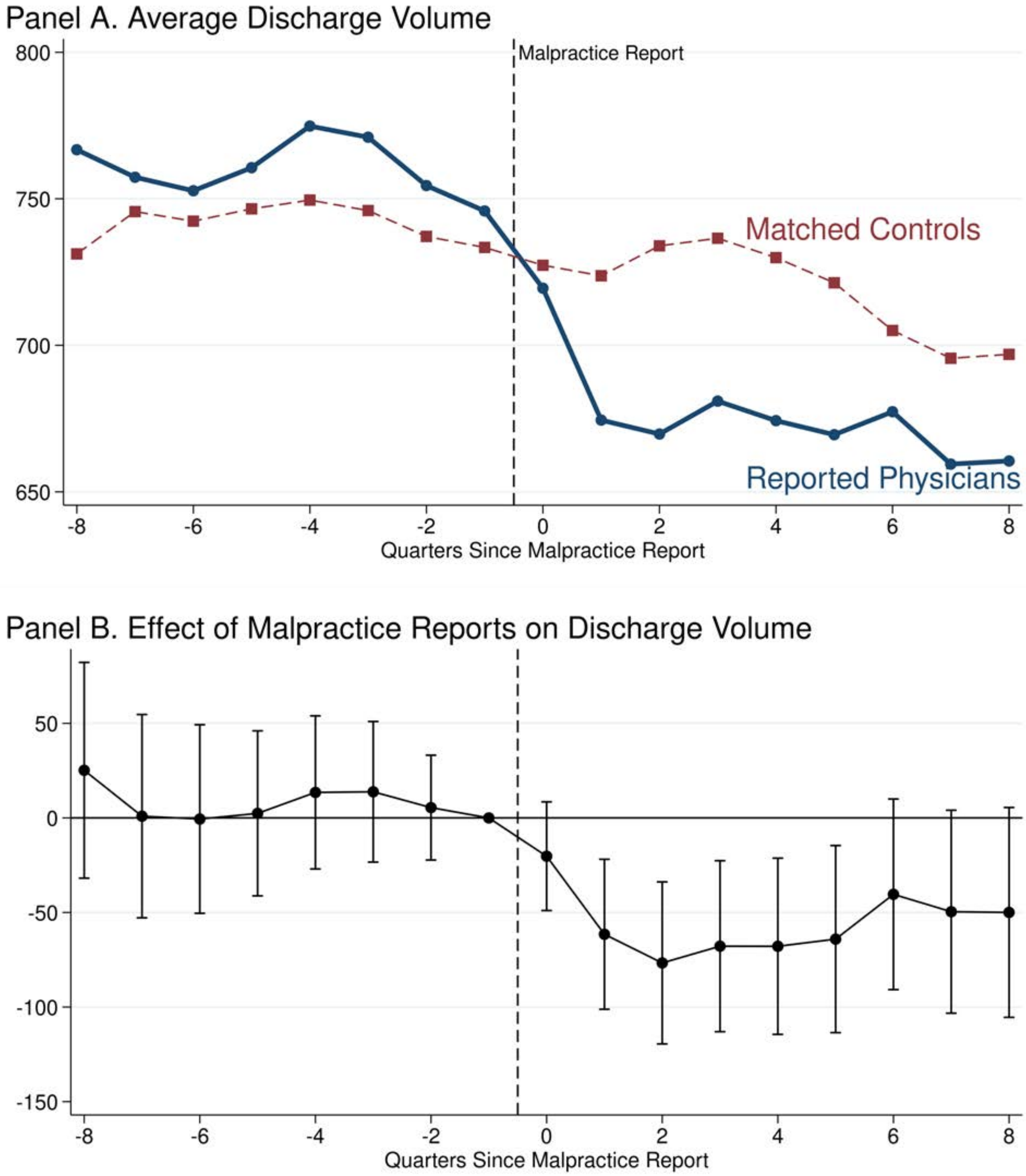
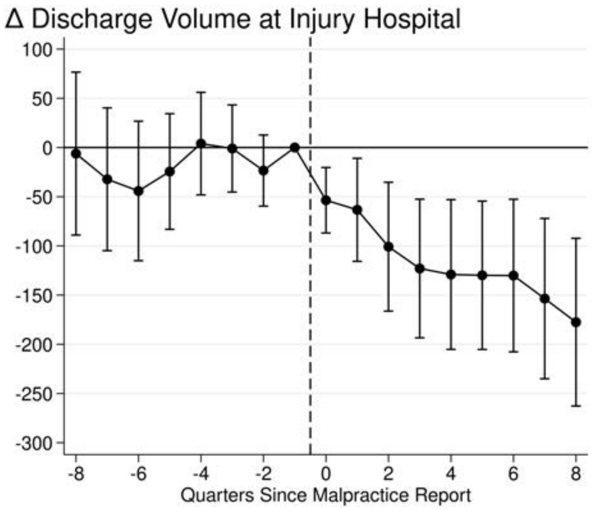


Figure A12: Discharge Volume Before and After Malpractice Reports

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-match fixed effects, controls for physician tenure and calendar time fixed effects. Standard errors are clustered by physician and match.

Panel A. Low Staffing Hospitals



Panel B. High Staffing Hospitals

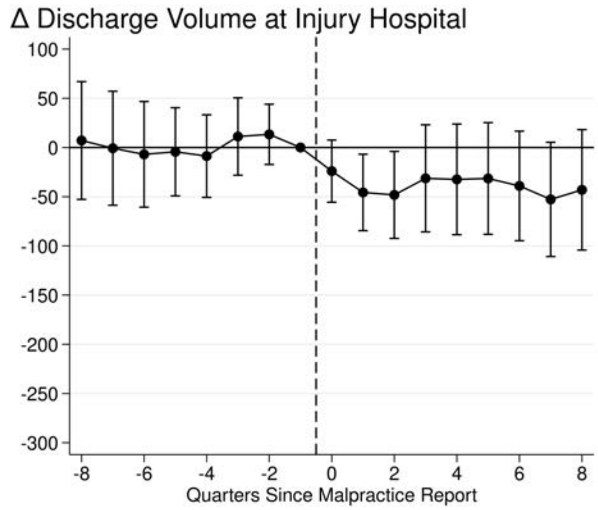
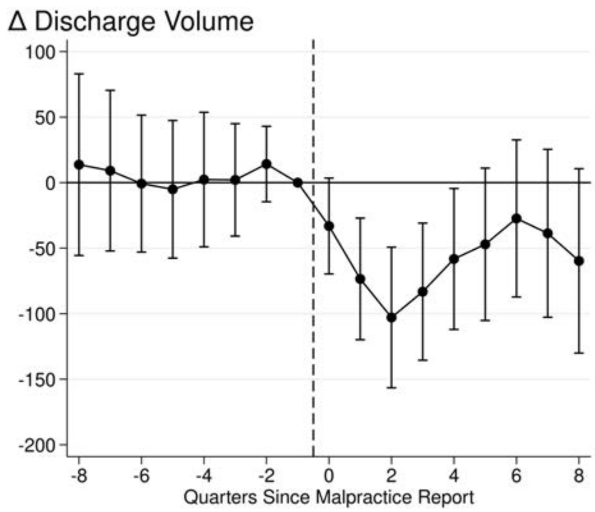


FIGURE A13:
Effect of Malpractice Reports on Discharge Volume, by Injury Hospital Staffing Levels

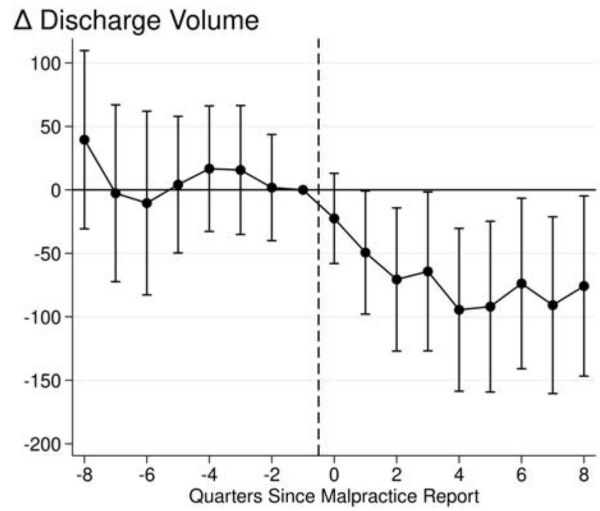
Notes: These figures plot event studies of physician discharge volumes at the hospital where the patient injury occurred before and after malpractice reports. The panels separate reported physicians into two groups according to the staffing level of the injury hospital and show changes relative to the full control group. Low staffing hospitals include those with below median numbers of unique EM specialists per discharge in the pre-report period.

Panel A: Main Analysis Panel (Two Years Before and After Report)

Early Resolution Claims

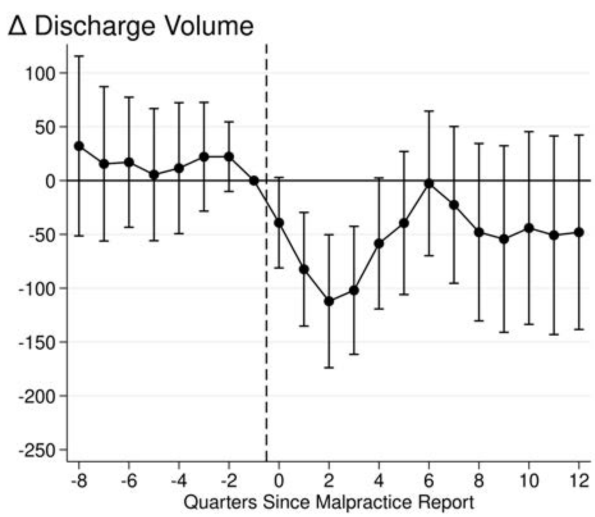


Late Resolution Claims



Panel B: Extended Panel (Three Years After and Two Years Before Report)

Early Resolution Claims



Late Resolution Claims

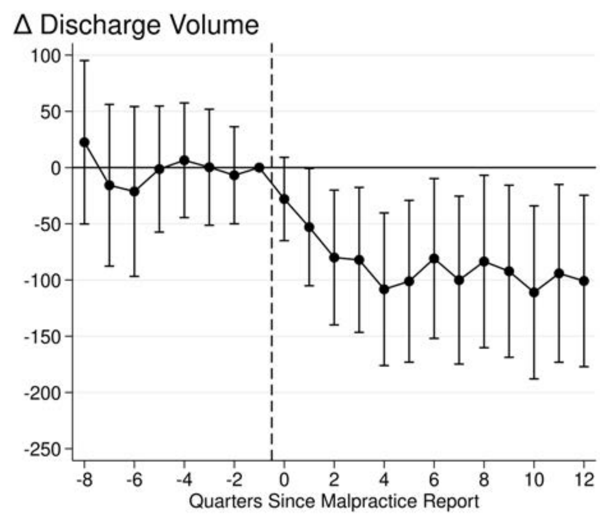
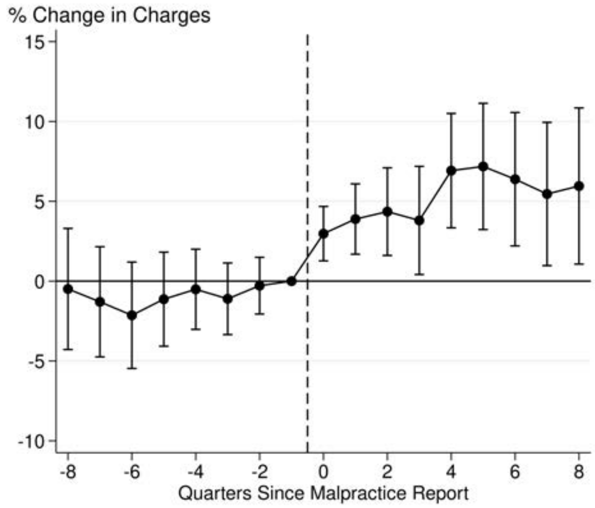


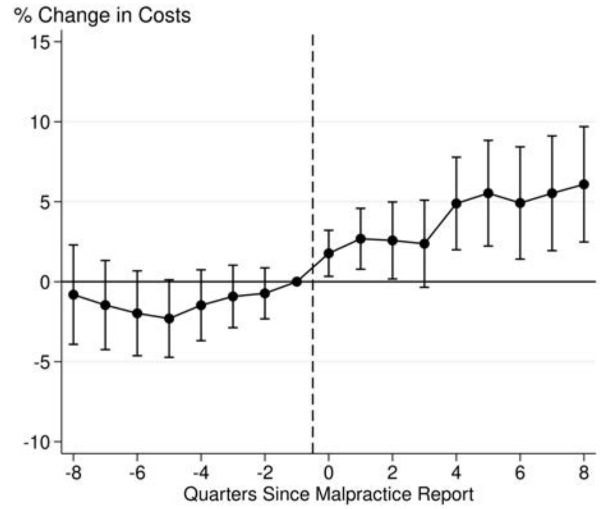
Figure A14:
Effect of Malpractice Reports on Discharge Volume, by Claim Duration

Notes: These figures plot event studies of physician discharge volumes before and after malpractice reports. The figures 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. Panel A uses the main analytic sample, following physicians for two years before and after reports. Panel B uses an extended panel, subsetting to physicians who can be followed for at least three years after the report.

Panel A. Ln(Total Charges)



Panel B. With Hospital Fixed Effects



Panel C. Linear Costs

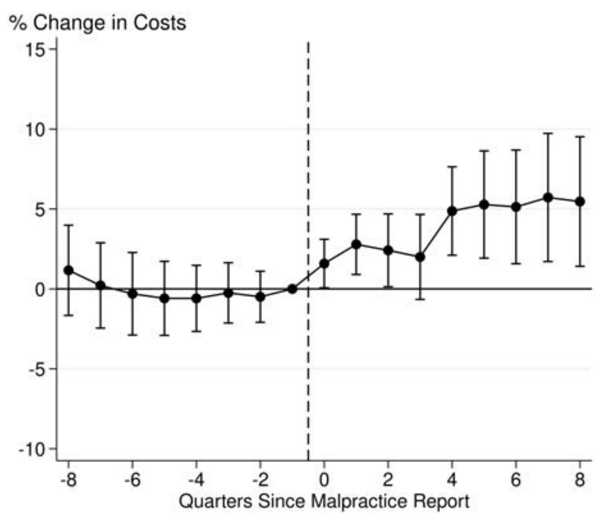


Figure A15:
Effect of Malpractice Reports on Costs per Discharge: Alternate Specifications

Notes: These figures plot event studies of treatment intensity before and after malpractice reports using four alternative specifications to our main model. All regressions control for physician-match fixed effects, physician tenure, calendar time fixed effects and patient characteristics. 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. Costs and charges are modeled in 2009 dollars.

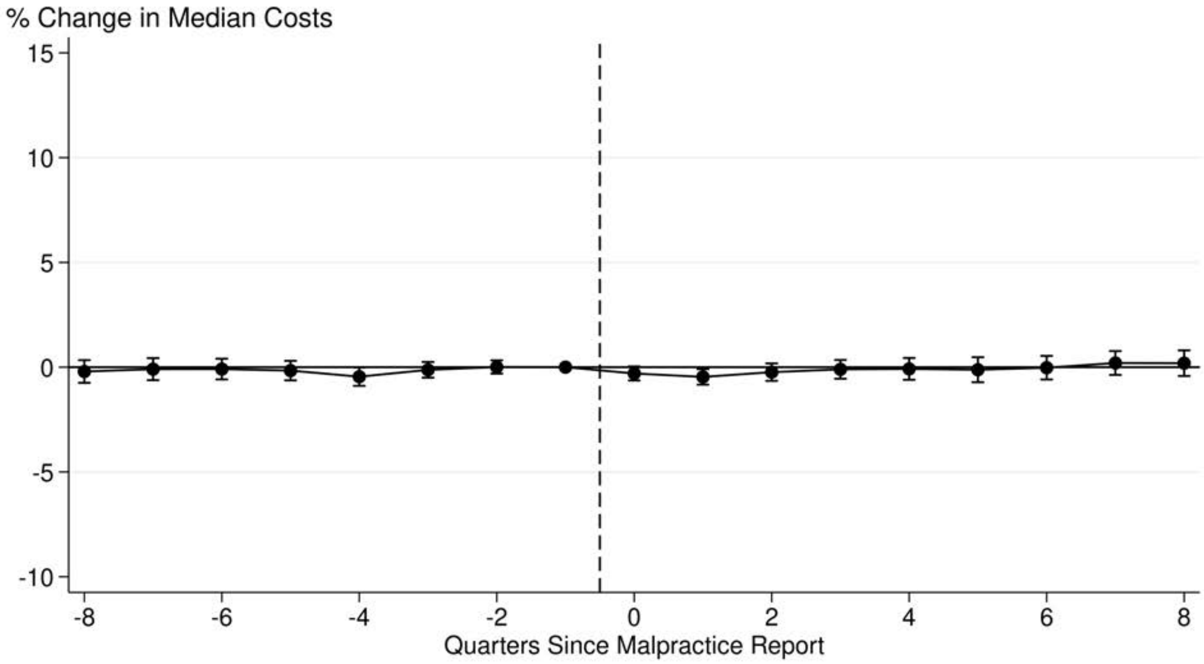


Figure A16:
Effect of Malpractice Reports on Median Costs per Discharge

Notes: This figure plots an event study of median discharge costs before and after malpractice reports. Each discharge is standardized to the median cost in its presenting condition-age group-payer bin. Costs are modeled in 2009 dollars.

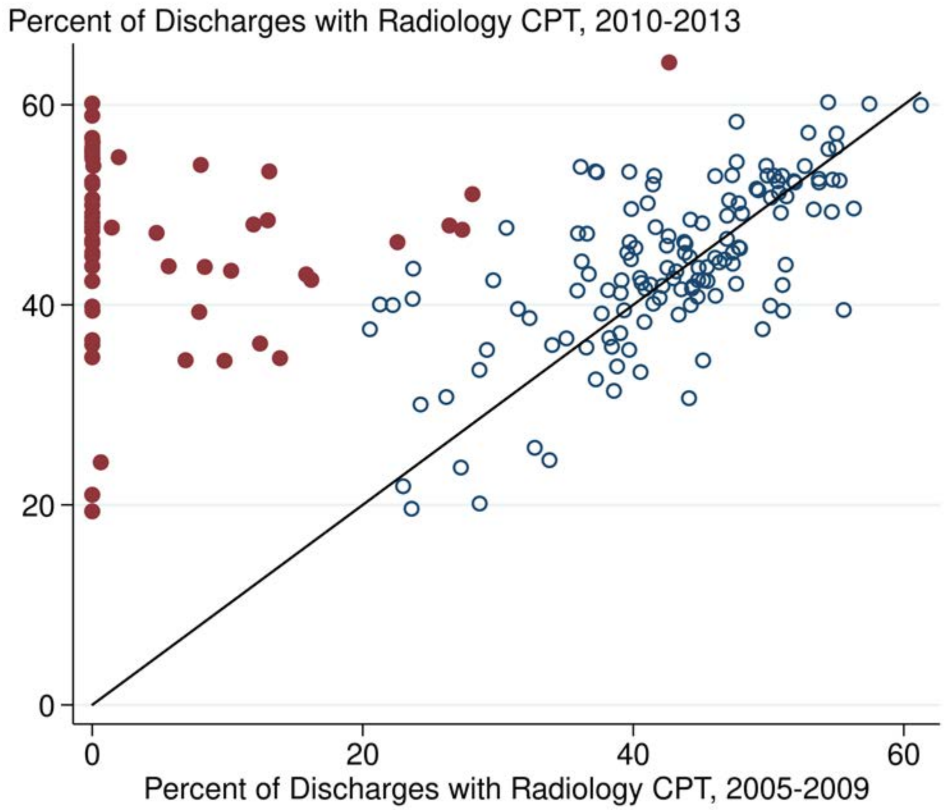


Figure A17:
Rate of Radiology Coding Across Discharge Data Versions

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.

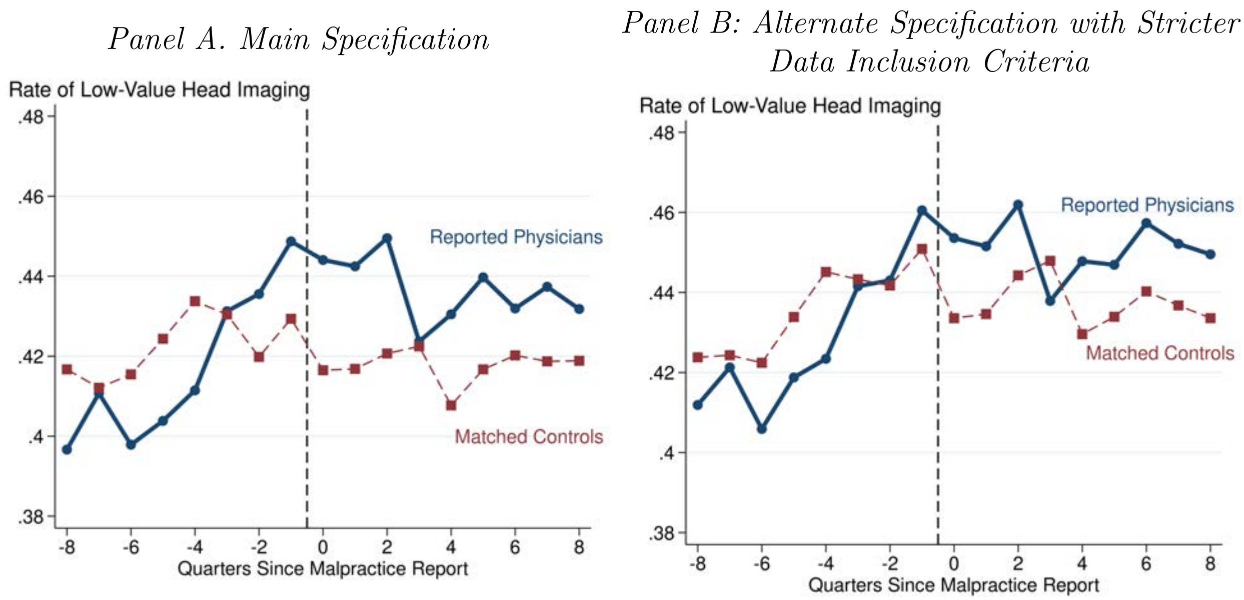


Figure A18:
Effect of Malpractice Reports on Low-Value Head Imaging

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.

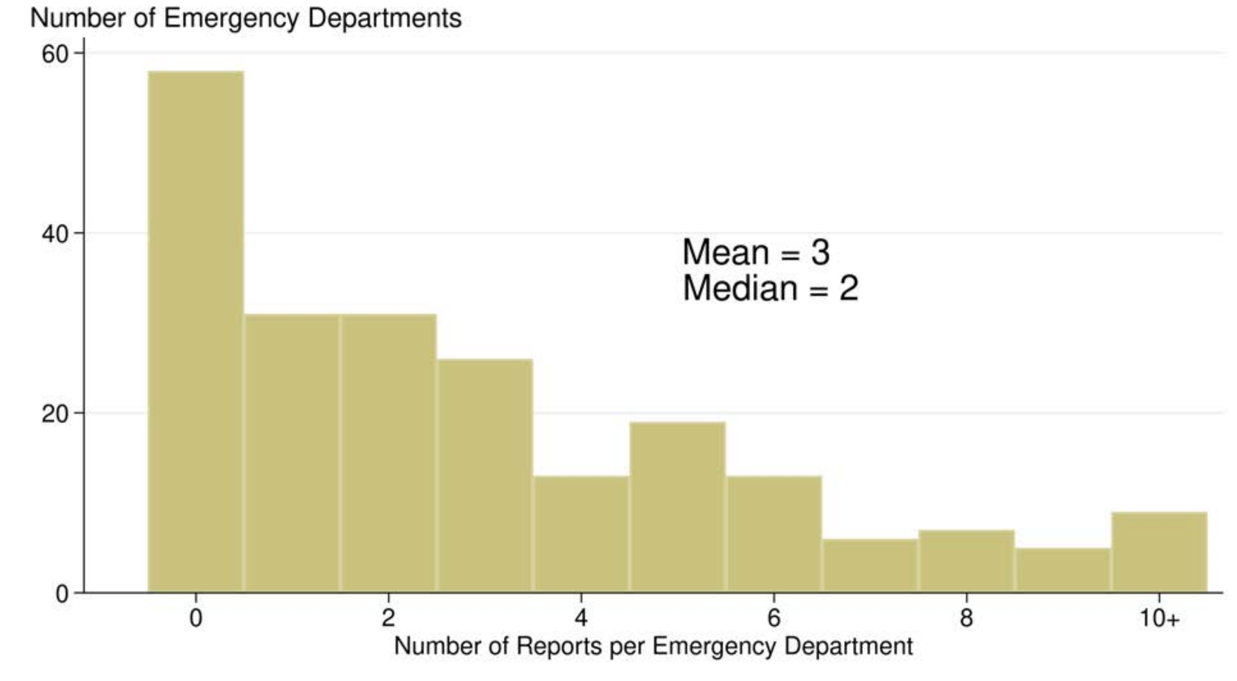


Figure A19:
Number of Malpractice Reports per Emergency Department

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 III.D. For scaling purposes, the horizontal axis is truncated at 10 reports.