DEFENSIVE MEDICINE: EVIDENCE FROM MILITARY IMMUNITY*

MICHAEL FRAKES

JONATHAN GRUBER

A major source of controversy in U.S. health care is the practice of "defensive medicine," an unintended effect of the liability system in which physicians order extraneous tests and procedures as a result of fears over medical liability. Existing evidence on the importance of this issue is quite mixed, however, partly because of the difficulty of finding a clean comparison between groups who are and are not subject to malpractice pressure. The structure of malpractice protections under the Military Health System (MHS), the $50 billion/year program that provides health care for active duty military and their dependents, provides such a comparison. Active-duty patients seeking medical treatment from military facilities cannot sue should they suffer harm as a result of negligent medical care, while protections are provided to dependents treated at military facilities and to all patients—active-duty or not—that receive care from civilian facilities. Drawing on this variation and exploiting exogenous shocks to care location choices stemming from military hospital closures pursuant to the Base Realignment and Closure process, we estimate that liability immunity reduces inpatient treatment intensity by 5% with no measurable negative effect on patient outcomes.

Word count: 17,994

JEL Codes: *I18, K13*

* Frakes (corresponding author): Duke University School of Law, 210 Science Drive, PO Box 90362, Durham, NC 27708, 919-613-7185 (email: michael.frakes@law.duke.edu). We are extremely grateful to Mike Dinneen and Daryl Crowe at the Military Health System and to Wendy Funk and Keith Hofmann at Kennell Inc. for making the MHS data available to us and for fielding endless questions. We thank Ronen Avraham for providing data on damage-cap reforms. We are also grateful to Jonathan Petkun and Chris Behrer for outstanding research assistance. We acknowledge funding from NIA grant R01AG049898.

I. INTRODUCTION

One of the most controversial aspects of the U.S. health care system is the impacts of medical malpractice pressures on patient treatments and health care costs. To be sure, the annual administrative expenses associated with the medical liability system ($4 billion per year) and the aggregate liability payments made by malpractice defendants ($5.7 billion) are perhaps trivial relative to the thee-trillion-dollar health care economy in the U.S. (Mello et al. 2010). However, of much larger concern is the potential practice of "defensive medicine," an unintended effect of the liability system in which physicians order *extraneous* tests, procedures and other services as a result of fears over medical liability. Such costs have played a central role in health care debates in recent years. A key question surrounding this debate, however, is just how large these defensive medicine costs really are.

Some have argued that defensive medicine is the major driver of excessive health care spending in the U.S. Former Congressman (and former Secretary of the Department of Health and Human Services) Tom Price suggested in 2010 that as much as 26 percent of all money spent on health care is attributable to this phenomenon.[1] Others have argued that, while it may exist, it has at most a small impact on spending.[2] The most important arbiter of this debate, the Congressional Budget Office, predicted in 2009 that a proposed aggressive package of liability reforms would lower medical costs by only 0.3% through reduced defensive medicine.

What we know thus far about the magnitude of defensive medicine has largely come from observing physician responses to various medical liability reforms that states have passed over the

[1] Press Release, Republican Representative and Republican Study Committee Chairman Tom Price (February 22, 2010), *available at* http://rsc.scalise.house.gov/news/documentsingle. aspx?DocumentID=171421.

[2] For instance, Chandra et al. (2013) stated "[the evidence] has led to a loose consensus among most economists and policy makers that defensive medicine is not an important contributor to U.S. health-care spending…" Supporting this claim of more modest impacts of liability, Mello et al. (2010) estimate that roughly $45.6 billion is spent annually on defensive medical practices, representing roughly 2 percent of total annual health care spending.

past 4 decades.[3] The typical reform has operated largely to reduce the extent of damages in the event that liability is determined—e.g., dollar caps on non-economic damages awards. While physicians face little direct financial exposure to law suits (Zeiler et al. 2009), a damage-cap reform could theoretically alter a physician's perceived liability threat to the extent caps discourage plaintiffs and plaintiffs' attorneys from filing suit in the first place (Frakes and Jena 2016). The literature, however, has produced mixed results regarding the relationship between damage caps and the frequency of suits. Moreover, self-admissions by physicians that they practice defensively is not associated with the presence of a non-economic damages cap (Carrier et al, 2010). Ultimately, it is uncertain that incremental reforms such as caps have represented a sizeable enough change in perceived or actual liability pressure from which to tease out the law's influence on behavior. Moreover, even if caps do indeed lead to reductions in perceived and actual liability risks, they are not likely to be informative on the full impacts of the liability system to the extent that caps only operate to shield physicians from a portion of claimed damages.

Recent proposals to reform the malpractice system have reached beyond remedy-focused reforms such as caps and have considered broader, more structural changes. For instance, one prominent proposal has suggested imposing liability "safe harbors" that exempt providers from malpractice risk if the treatment adheres to certain clearly delineated guidelines.[4] Predicting the impact of this new approach requires addressing a very different question: will doctors practice medicine differently if they know with greater certainty that they cannot be sued?

The structure of malpractice protections under the Military Health System (MHS) provides a novel opportunity to answer this policy relevant question. The MHS is a $50 billion program

[3] Mello and Kachalia (2011) provide a summary of these reforms.
[4] See for example H.R. 1565, the "Saving Lives, Saving Costs" Act introduced in 2017. The proposal was also included in early iterations—though not in the iteration passed on May 4, 2017—of the House bill associated with the American Health Care Act.

that provides insurance for all active duty military and their dependents. For those who seek care at Military Treatment Facilities (MTFs), this care is provided by government employees or contractors; for those who use purchased care outside of MTFs, the MHS pays for their costs through a contract with a private sector managed care plan.

Most importantly for our purposes, the MHS provides what is missing in previous studies: a true "treatment group" of patients who cannot sue for malpractice. Pursuant to a long-standing and highly controversial federal law, active-duty patients seeking medical treatment from active duty physicians at military facilities have no recourse under the law—i.e., they can sue neither the physician nor the government—should they suffer harm as a result of negligent medical care. Malpractice protections are afforded, however, to dependents and retirees treated at military facilities and to all patients—active-duty or not—that receive care from civilian facilities. By comparing those patients over which physicians are not subject to "defensive medicine" pressure to other patients over which physicians are subject to such pressure, we can identify the impact of defensive medicine pressure on practice patterns, medical costs, and patient outcomes.

Doing so requires tapping into a unique set of data that have not previously been used for health economics research, the Military Health System Data Repository (MDR), which is the main database of health records maintained by the Military Health System. Broadly, it provides incident-level claims data for both inpatient and outpatient settings, the former of which we rely upon for this analysis. Using this information, we formulate a number of health care spending and utilization metrics, in addition to quality metrics, from which we are able to investigate key questions regarding the link between liability forces and medical practices.

We use these data to develop a compelling empirical strategy that differentiates cases where physicians are and are not subject to malpractice pressure while carefully controlling for

other factors which might cause patients to be treated differently. We start with a differences-in-differences comparison of active duty versus non-active duty patients who are treated on the base versus off the base. We then address endogeneity of choice of treatment location using instruments based on location of residence, while drawing upon exogenous shifts in access to MHS bases during our sample period due to military hospital closings pursuant to recommendations by the Base Realignment and Closure Commission. We also consider comparisons within hospitals and within doctors, allowing us to compare treatment of immune versus non-immune patients by the same medical providers.

Primarily, we apply this empirical strategy to the treatment of all inpatient episodes. This provides us with a very large sample that allows us to test precisely for the impact of malpractice pressure on treatment modality and patient outcomes. To reinforce a medical liability interpretation of these findings, we separately test for the impact of liability immunity on the utilization of diagnostic and non-diagnostic procedures. In the case of diagnostics, the prevailing risks are often those concerning errors of omission—i.e., harms ensuing from failure to perform tests. Capitulating to these concerns, physicians may provide more tests. In the case of non-diagnostic procedures, the prevailing risks may be a mix of errors of omission and errors of commission—i.e, harms ensuing from failures to perform procedures and from failures in the execution of procedures. These bi-directional forces suggest that liability pressure may lead to more muted responses in the case of non-diagnostic procedures. Accordingly, we predict stronger negative effects of liability immunity in the case of diagnostics.

For further confirmation that our results reflect liability pressure, we separately investigate the case of childbirth. An influential article by Currie and MacLeod (2008) argues—counter to conventional defensive-medicine expectations—that the prevailing risks facing the marginal

mother may be as such that liability forces cause physicians to treat her *less* intensively (fewer cesarean sections). Therefore, we have a subsample of our data with precisely an opposite prediction, allowing us to distinguish malpractice pressure from other unobserved factors.

Our findings are striking. We consistently estimate that immunity from malpractice pressure is associated with lower health care utilization; our central estimate is that treatment intensity for inpatient episodes—measured in various ways—is roughly 4-5% lower when physicians are immune from such pressure. We find that these effects are stronger in the case of diagnostic as opposed to non-diagnostic procedures. And, consistent with Currie and Macleod, we find that the rate of cesarean section delivery (and other metrics of intensity in the childbirth sample) is higher when physicians are immune from malpractice pressure. In neither setting do we find any evidence that the differences in treatment intensity are associated with differential patient outcomes, although our estimates are imprecise in the case of some of the outcome metrics explored, especially in the childbirth context. Finally, we find that our effects are largest in states without pre-existing caps on non-economic or total damages for malpractice cases – further confirming our finding and allowing us to place it in the context of the previous literature.

Collectively, our results suggest that malpractice pressure is a key determinant of medical treatment patterns in the U.S. and that major reforms to the system—that is reforms beyond the more incremental, remedy-focused reforms embraced to date—could have real effects on medical costs without large effects on patient outcomes. Even absent national reforms, our findings have important implications for the MHS, one of our nation's largest (and almost certainly it's most understudied by economists) health care programs.

Our paper proceeds as follows. Part II reviews the malpractice environment and the existing literature on medical malpractice, and describes the MHS-specific structure which forms

the basis of our identification strategy. Part III discusses our unique data set. Part IV explains our empirical strategy and how we address various challenges to identification. Part V presents the results of our analysis, while Part VI concludes.

II. Previous Literature and the MHS Malpractice Environment

II.A. Background: Defensive Medicine Literature Review

As noted in the introduction, an important strand of the empirical medical malpractice literature has endeavored to quantify the extent of defensive medicine, often by assessing how practice patterns and medical expenses have changed in response to certain tort reforms. The seminal paper on this topic is Kessler and McClellan (1996), which found that tort reforms that directly reduce liability pressure—including damage caps—lead to reductions of 5 to 9 percent in medical expenditures associated with the care provided in the 1-year period following an acute myocardial infarction or new ischemic heart disease. The implication from such reductions is that higher malpractice pressure prior to the relevant reforms was inducing physicians to undergo added spending. In a follow-up study, Kessler and McClellan (2002) update this analysis to include information on managed-care penetration rates, modifying their findings to a 4-7 percent reduction in the relevant expenditures in connection with such reforms.[5]

Follow-on research has generally been less successful in extending these results, however. Sloan and Shadle (2009) and the Congressional Budget Office (2004) explored the relationship between caps and related tort reforms on the one hand and health care spending across a range of medical scenarios on the other hand but failed to replicate the cardiac-specific findings of Kessler

[5] Kessler and McClellan's analysis builds on other studies that have focused more on the first stage inquiry of whether damage caps reduce the frequency of liability claims. As discussed in the introduction, however, this literature has failed to consistently demonstrate any such reduction. See Kachalia and Mello (2011) for survey of this literature. A couple of recent studies have found that damage caps are associated with a reduction in the frequency of malpractice suits (Paik et al. 2013, Avraham 2007. Other studies, however, have found no such association (Donohue and Ho 2007, Zuckerman et al. 1990).

and McClellan (1996, 2002). Similarly, a recent study by Paik et al. (2017) focusing on the most recent wave of damage-cap adoptions finds no effect of caps on Medicare Part A spending (if anything, Paik et al. find an opposite-signed effect for Medicare Part B spending, suggesting a 4% increase in spending following cap adoptions). A related study by Moghtaderi et al. (2017) finds little to no association between caps and Medicare Part A and B spending and between caps and a range of cardiac testing rates and interventions. One exception to this recent pattern in the literature, however, is provided by Avraham and Schanzenbach (2015) who find a 3-5% reduction in intensive cardiac interventions following cap adoptions.

Other approaches which explore potentially more significant variation in the malpractice environment have found results more consistent with defensive medicine. Baicker et al. (2007) estimate that the 60 percent increase in malpractice premiums between 2000 and 2003 is associated with an increase in Medicare spending of more than $15 billion. Lakdawalla and Seabury (2011), identify the impacts of malpractice using variations in the generosity of local juries and find that the growth in malpractice payments over the last decade and a half has contributed to a 5 percent increase in the growth of medical expenditures. And Frakes (2013) and Frakes and Jena (2016) each estimate a significant sensitivity of physician behavior to an alteration of the underlying negligence standard itself; Frakes (2013) finds an increase in treatment intensity of 5-16% when the negligence standard changes so as to expect that physicians follow more intensive practice styles (with a similarly sized reduction in intensity when the law changes so as to expect the delivery of less intensive styles).[6]

Childbirth is one clinical context that has received particular attention by the defensive medicine literature. It has been well documented that obstetrics / gynecology represents one of

[6] Waxman et al. (2014), however, find little sensitivity in spending to a move from a negligence standard to a gross-negligence standard in emergency room contexts.

the higher risk specialties from a malpractice perspective. Jena et al. (2011) project that 74% of obstetricians / gynecologists will face a malpractice claim by age 45 (compared with 55% for physicians practicing in internal medicine and its sub-specialties). Currie and MacLeod (2008) estimate an *increase* in cesarean rates following damage-cap adoptions, contrary to any expectation that stronger liability fears in the absence of caps otherwise lead physicians to perform additional cesareans. They argue that there are many other reasons -- financial, convenience-related, etc.--to perform cesarean deliveries. By the time the physician reaches the marginal mother of interest for estimating the impact of liability, she may be rather healthy and in little need of cesarean delivery, in which case the prevailing concern may be one over the risks associated with the surgery itself. So liability pressure may induce less intensive treatments on the margin. Other studies, however, either find evidence more consistent with the conventional wisdom that higher liability pressure is associated with more cesarean deliveries (Dubay et al. 1999) or estimate little to no relationship between liability pressure and cesarean utilization rates (Baldwin et al. 1995, Frakes 2012).

Since the concept of defensive medicine rests on the extraneous nature of liability-induced spending, important to such studies is an evaluation of whether greater liability forces improve health care quality in conjunction with any documented changes in treatment intensity. The literature on this link has thus far produced mixed results. Kessler and McClellan (1996) estimate a trivial effect of liability reforms on (1) survival rates during the one-year period following treatment for a serious cardiac event, and (2) hospital readmission rates for repeated serious cardiac events over that period. Sloan and Shadle (2009) undertake a similar analysis and reach similar conclusions. Lakdawalla and Seabury (2011) find that higher county-level malpractice pressure leads to a modest decline in county-level mortality rates, whereas other studies of the impact of

malpractice reforms on infant mortality rates (or infant Apgar scores, which are seen as predictive of infant mortality) generally find no relationship (Klick and Stratmann 2007; Frakes 2012; Dubay et al. 1999). Currie and MacLeod (2008) find that damage caps increase preventable complications of labor and delivery. Frakes and Jena (2016), on the other hand, find little relationship between damage caps and preventable delivery complications (along with various other quality metrics).

Collectively, the above findings paint a varied picture of both the size and existence of defensive medicine. These inconsistencies in the observational literature are perhaps surprising in light of another line of studies that has directly surveyed physicians on their behaviors and that has documented notably high rates of physicians admitting that they practice defensive medicine (for example, Studdert et al. 2005). As suggested by the introduction, perhaps the source of the uncertainties in the observational literature—and the disparity between their findings and those produced by the survey literature—lies in the methodological limitations of the existing studies' focus on marginal reforms such as caps.

II.B. Background: The Military Health System

The Military Health System (MHS) is the primary insurer for all active duty military, their dependents and retirees through the TriCare program. Of the nearly 9 million patients covered by TriCare, only 20% are actively serving in the US military. TriCare is not involved in health care delivery in combat zones and operates separately from the Department of Veterans Affairs' Veterans Health Administration health service delivery system (Schoenfeld et al. 2016). For TriCare enrollees, care can be delivered in one of two ways, either directly at Military Treatment Facilities on military bases (direct care), or purchased from private providers (purchased care). For those who receive treatment at MTFs, the care is delivered primarily by a mix of providers including active duty military providers, federal government civilian employee providers, and

providers hired using contract mechanisms who work full time at the MTF. Overall, 60% of inpatient care is delivered by active-duty military.

Alternatively, TriCare enrollees can receive "purchased care" outside the MTF. This care is delivered by a network run by a contracting insurer, where patients go to private physicians who are within the insurer's contracted network, as with most privately insured in the U.S.

In principle, enrollees who live within the "catchment area" of an MTF are supposed to go to the MTF for care. This area was defined as 40 miles originally, although the military has shifted to time-based boundaries over time. But our data show clearly that this rule is not rigorously enforced during our sample period. As is clear from the data, those who live closer to an MTF are much more likely to go there, but with a more gradual fall off rather than a strong distance discontinuity.

II.C. Background: Liability Immunities in the Military

To explain the medical liability rights facing MHS beneficiaries and to demonstrate the source of liability immunities within the MHS, we separate the discussion into several scenarios depending on the type of patient receiving care and the type of physician providing care:

Active Duty Military Personnel Receiving Care from Military Providers are barred from suing the government for injuries that arise out of or in the course of active military service under *Feres v. United States*, 340 U.S. 135 (1950). It is well established that receipt of medical care by service members in military facilities has been found to be "incident to service" and thus the *Feres* doctrine bars lawsuits against the government that arise from negligent medical treatment of active duty service members. Pursuant to subsequent cases that have applied *Feres*,[7] active duty

[7] See, for example, *Bailey v. DeQuevedo*, 375 F.2d 72, 74 (3d Cir.), cert. denied, 389 U.S. 923.

personnel are likewise barred from suing military physicians in their individual capacities in the event of injuries arising from medical negligence.

Non-Active Duty Personnel Receiving Care from Military Providers The *Feres* doctrine does not bar medical liability claims brought by dependents or non-active-duty military who receive negligent care at military facilities.[8]

Patients Receiving Care from Civilian Providers at Civilian Facilities. Military beneficiaries, whether active-duty or dependents / non-active-duty, who receive care from civilian physicians at civilian facilities maintain their ability to sue such physicians under general tort rules should they receive negligent care.

All Patients Receiving Care from Civilian Providers at Military Facilities A non-trivial share of care that is delivered at Military facilities is delivered by civilian providers. The *Feres* doctrine does not prevent any active-duty patient (or non-active-duty-patient) harmed by physicians independently contracting with the Military from suing the contracting physician for medical malpractice. As such, military personnel do have recourse against someone in this circumstance—i.e., against the independently contracting physician.

While we can separate civilian physicians from active-duty physicians in our data, the difficulty in identifying scenarios in the data of this sort is that civilian providers working at Military facilities will not *necessarily* be deemed independent contractors. The distinction between independent contractors and employees—as it applies to tort law—is one that focuses on substance rather than form. If the Military hospitals exercise the same degree of oversight and control over the active duty physicians working in their facilities as they do the civilian providers

[8] See for example *Burgess v. United States,* 744 F.2d 771 (11th Cir. 1984) (serviceman's child who received post-delivery injury at army hospital successfully brought FTCA claim); *Costley v. United States,* 181 F.2d 723 (5th Cir.1950) (serviceman's wife treated negligently at army hospital during delivery of her child successfully brought FTCA claim).

within their walls, the law may not treat them differently and may thus protect the civilian providers under *Feres* as well.[9] Given this legal treatment and given that the mean workloads of civilian physicians working at Military inpatient facilities is almost identical to the mean workloads of active-duty physicians working at those facilities (according to our data), we elect in our analysis below not to exploit variations along this active-duty / civilian dimension within Military facilities.

Of course, the above differences in legal regimes are irrelevant unless understood by practicing physicians. In May 2011, we informally surveyed 13 physicians practicing within the MHS regarding their knowledge of these rules. Eleven out of those 13 indicated that they were aware of the law bearing on whether active duty or non-active duty could sue for medical liability as a result of negligent, harmful medical care received on the base. Despite this less-than-unanimous confidence regarding knowledge of such rules, all 13 physicians actually identified the correct rule when prompted via a true or false question—i.e., they identified that active duty patients could not sue in these instances, and that non-active duty could sue.

III. DATA

III.A. The MDR Data

The data for this analysis come from the Military Health System Data Repository (MDR), which is the main database of health records maintained by the MHS. Broadly, it provides incident-level claims data across a range of clinical settings and contexts. For certain methodological reasons set forth below, we focus in this paper on the inpatient records contained

[9] For an example discussion of these issues, see *Williams v United States* (2007 WL 951382) (finding that a civilian physician working at a VA hospital could be deemed an employee for the purposes of a *Feres* analysis given that the government "controlled not only [the physician's] work hours and vacation time . . . , but where he worked, who he saw, and what he did during those hours", regardless of the fact that the physician's contract labelled him as a contractor and not an employee).

within the MDR. Importantly, the MDR database collects claims information from both the direct care (at the MTF) and purchased care (off the MTF) settings. Each of these files provides information on the identity of the physician and facility and the date of the encounter, along with demographic and other information on the patient. Further, each file provides substantial details regarding the encounter itself: primarily, the diagnosis and procedure codes associated with the event, length of the inpatient stay, and other treatment intensity metrics discussed below. Moreover, the MDR database contains separate files with coverage, demographic, geographic and other information on each MHS beneficiary, which we link to the various claims records. All told, we collect these linked records for each of nearly 12 million distinct beneficiaries over an 11-year period between the beginning of 2003 and the end of 2013.

III.B. Samples

For our analysis we will predominantly focus on two samples: (1) the universe of inpatient hospital admissions and (2) the sub-universe of childbirths in our records. At other points in our analysis, however, we also consider additional clinical sub-samples.

Inpatient Admissions

While the outpatient setting has been the subject of little investigation by the medical malpractice literature to date, we follow the conventional approach of focusing on inpatient records for certain methodological reasons. Represented by more severe circumstances with higher baseline probabilities of negative outcomes, inpatient encounters arguably implicate greater liability risk and provide a greater ability to link any observed changes in health status to the encounter itself, providing us with more power by which to estimate the relationship between the liability environment and observed treatments and outcomes. Moreover, we can cleanly identify the malpractice implications for the given encounter—considering that the active duty status of the

patient will not likely change throughout the encounter, nor will the location of care (military facility versus civilian provider). Finally, one of the key methodological tools we embrace below—decisions stemming from the Base Realignment and Closure Process—are generally applicable only to hospital-based care.

While all military members, families and retirees face coverage under the MHS, the system does offer alternative insurance plans with different cost-sharing and other terms. All active-duty personnel are required to enroll in TRICARE Prime plans. While facing other alternatives, nearly 90% of the non-active duty in our records likewise choose to enroll in TRICARE Prime. To ensure comparability in insurance terms throughout our sample, we limit the analysis to those beneficiaries with TRICARE Prime coverage. Active duty and their dependents covered under PRIME face no cost-sharing obligations. We exclude retirees from our sample in that PRIME-covered retirees receiving care off the base do face some cost-sharing, compromising the difference-in-difference framework. We also focus only on the care provided at U.S. facilities and do not consider overseas encounters, where the civilian comparison analysis would otherwise be challenged. Finally, we limit our specifications to adults between 18 and 60 years of age. Since children cannot be active-duty members, this restriction ensures common age support across the four key analytical groups to our motivating difference-in-difference design. For similar reasons we drop those above the age of 60 given the disproportionately few active duty above this age. Nonetheless, in the Online Appendix, we demonstrate the robustness of the results to no age restrictions at all.

With these restrictions, our analytical sample focuses on 2,452,892 inpatient encounters, 1,130,116 (46%) of which are at MTFs and 1,322,766 (54%) of which are at civilian hospitals. Out of the MTF encounters, 553,121 (49%) are connected with active-duty patients, while 576,995

(51%) are associated with dependents of active duty. Out of the civilian encounters, 395,924 (30%) are for active-duty patients, while 926,842 (70%) are for dependents.

Obstetrics

Obstetrical care represents a sub-setting of inpatient admissions that has received special emphasis by the malpractice literature (as surveyed above). To explore the effects of liability immunities on treatments and outcomes within this commonly explored context, we estimate alternate specifications that further restrict the above sample to focus on the subsample of deliveries within the MDR inpatient database.

This alternative obstetrics sample focuses on over 1,135,640 deliveries, 502,428 (44.2%) of which occurred at MTFs and 632, 212 (55.8%) of which occurred at civilian hospitals. Out of the MTF childbirth encounters, 112,491 (22.4%) are to active duty mothers, while 389,937 (77.6%) are to non-active-duty mothers. Out of the civilian encounters, 57,003 (9%) are to active-duty mothers while 576,209 (91%) are to non-active-duty mothers.

III.C. Dependent Variables

Treatment Intensity Metrics

The first half of our defensive medicine analysis entails investigating the link between immunities from liability and the intensity by which physicians treat patients. Our primary measure of interest here is called the Relative Weighted Product or RWP, a Department of Defense-derived measure assigned to each inpatient encounter.[10] The RWP is a treatment intensity metric designed to be comparable across the direct care and purchased care inpatient settings. Using information from the indicated procedure and diagnosis codes, the Department of Defense

[10] Though our records run from 2003 to 2013, we note that the RWP metric is unavailable for most of the 2013 records. Our other intensity metrics, however, are available over the full timeframe.

assigns a Diagnosis Related Group (DRG) weight to the encounter and thereafter adjusts that weight based on the length of stay of the encounter. Even though this measure may fall short of the precise costs associated with the encounter, it is meant to provide a reliable sense of the relative costliness and utilization intensity of the actual encounters represented in the MDR database. Given that 55% of the variation across DRGs is actually explained by treatment intensity (McClellan, 1997), this measure reflects both treatment intensity and length of stay.

As robustness checks, we also use as outcome measures certain alternative treatment metrics (which are effectively components of the RWP measure): (1) number of inpatient bed days, (2) number of procedures performed and (3) the incidence of any procedure performed. In the obstetrics analysis, we also use the incidence of a cesarean delivery as an indicator for intensive treatment.

Moreover, a critical component to our empirical analysis focuses on exploring the impacts of liability immunity on the utilization of diagnostic procedures. For these purposes, we divide the sample into a number of major diagnostic categories—e.g., respiratory care, cardiac and circulatory care, gastrointestinal care, etc.—and then surveyed the medical literature to assess the most common diagnostic tests utilized within each such category. For each such subsample, we then create indicator variables for whether the patient received any diagnostic procedure among the set of common procedures applicable to that category. For those diagnoses not specifically investigated, we create a residual diagnostic category, with respect to which we create an indicator variable for whether any of the diagnostic tools used in the other categories collectively are employed (which largely captures the usage of radiological imaging on these residual admission types). Finally, we also create a pooled any-diagnostic indicator variable across the full sample.[11]

[11] In the Online Appendix, we indicate which tests we code for in the case of each diagnostic category. Importantly, we note that we do not include the labor and delivery admissions in the diagnostic analysis. To flag diagnostic tests within the MDR, we need to explore both ICD-9 and CPT

To serve as a comparison point for our diagnostic-testing analysis, we also explore the effects of liability immunity on the incidence of any-non-diagnostic procedure. We divide the sample into major diagnostic categories and create indicator variables for the incidence of any procedure other than the diagnostic procedures flagged above.

Health Care Quality Metrics

Also critical to our defensive medicine analysis is an assessment of the link between liability pressures and health care quality. For these purposes, we construct a number of quality indicators commonly employed in the health economics literature and in the assessment of medical institutions. For the full inpatient sample, we construct the following indicators: (1) the incidence of mortality within 90 days following discharge (mean of 0.5%), (2) the incidence of an unplanned hospital readmission within 30 days (mean of 4.0%),[12] and (3) the incidence of an adverse event, as captured by the presence of Patient Safety Indicators (PSIs) promulgated by the Agency for Health Care Research and Quality or AHRQ (mean of 1.5%).[13] For the purposes of brevity, we focus this quality analysis on the full inpatient sample; however, we extend this analysis in the Online Appendix to consider a range of obstetrical quality metrics—e.g., infant and neonatal mortality, among others.

IV. Empirical Strategy

Our core empirical strategy exploits the fact that there is differential malpractice pressure across types of patients and site of care. In particular, active duty patients treated on the base may not sue for negligent care and thus pose no liability pressure, while other patients treated at the

procedure codes across the various MDR files. The prominent diagnostic tool used in deliveries is fetal monitoring; however, perhaps due now to the common nature of this tool, fetal monitoring is built into the CPT codes indicating the delivery itself, leaving it difficult to separately identify the use of fetal monitoring within our data. Diagnostic ultrasounds are sometimes performed during the time of delivery; though the incidence is far too low to warrant using ultrasounds as a proxy for childbirth diagnostics.

[12] To determine an unplanned readmission, we follow the approach used by the Centers for Medicare & Medicaid Services.

[13] As defined by the AHRQ, PSIs are "a set of indicators providing information on potential in hospital complications and adverse events following surgeries, procedures, and childbirth."

same site may sue. At the same time, both groups may sue and thus pose liability threats when treated off the base. This set of facts naturally motivates a difference-in-difference specification:

$$(1)\ Y_{ije} = \alpha + \theta ACTIVE_DUTY_{ie} + \gamma ON\text{-}BASE_{je} + \beta ACTIVE_DUTY_{ie} X\ ON\text{-}BASE_{je} + \delta \mathbf{X_{ie}} \{+ \mu_j\} + \{\lambda_k\} + \varepsilon$$

Where i denotes individual patients, j denotes providers,[14] e denotes episodes, and k denotes zip-codes (based on patient residence); individual patients may have multiple episodes over the 11 years of our data. Y is some measure of patient treatment. ACTIVE_DUTY is a dummy variable for whether the individual is active duty at the time of the treatment; ON-BASE is a dummy variable for whether the individual is treated on the base; X is a set of individual-episode specific controls (age dummies, patient sex, year dummies, pay-grade-level dummies, Charlson comorbidity scores for the patient at the time of the encounter and primary diagnosis-code dummies associated with the encounter, based on the first 2 digits of the primary ICD-9-CM diagnosis code). We cluster our standard errors at the MTF catchment-area level to capture any correlations between patient characteristics or treatment decisions within base catchment areas.[15]

While a straightforward representation of the variation in malpractice environment facing patients, this approach raises four potential empirical concerns.

IV.A. Addressing Patient Case Mix Concerns

First, there may be differential selection into military treatment facilities (MTFs) by active vs. non-active duty patients. For example, it may be that the active duty patients treated on the

[14] In our analyses, we will consider separately hospital fixed effects and physician fixed effects.
[15] For these purposes, we cluster based on catchment area assignments as of the beginning of the sample. This creates consistent assignments in the face of the base-hospital closings that transpire over the sample, which impact actual catchment area assignments. Since catchment areas effectively overlap with MTF hospitals, this clustering approach turns out to be very similar to hospital level clustering. Nonetheless, we note that our results are robust to hospital clustering instead.

base are particularly sick relative to active duty patients treated off base, while there is no such differential for dependents.

To explore this further, Table I shows the covariate balance in our difference-in-difference approach. There is clearly a lack of balance along most covariates individually, most notably in gender. The last row of the table conducts a more omnibus balance analysis that attempts to explore covariate balance collectively, an exercise that demonstrates only a modest degree of overall covariate imbalance, if any. For these purposes, we draw on our main treatment intensity metric—the Relative Weighted Product (RWP)—and explore balance in a log RWP measure that is predicted on all of the available covariates. We find that our treatment group is predicted to have RWP levels that are 2% higher in this DD framework, which both biases *against* our ultimate conclusion and is nonetheless statistically indistinguishable from zero (while also being less than half the size of our ultimate treatment effects). Nonetheless, we will control for all of these covariates in the regression framework.

We can further address this point by using an important quasi-experiment that took place during our sample period: the closing of a number of MTFs. In 1995 and 2005, the Base Realignment and Closure (BRAC) Commission—after recommendations from the Pentagon—proposed a set of bases to be either or closed or restructured, recommendations that were thereafter approved by the respective President at that time. The closings and restructurings themselves took place over the ensuing years. Pursuant to the decisions by the 1995 and 2005 BRAC Commissions (mostly the latter), 11 MTF hospitals were closed over the course of our sample period, causing nearly 20% of the sample to experience a change in the distance to the closest MTF.[16]

[16] We list the affected hospitals in the Online Appendix. Four additional hospitals were closed over the sample period—affecting only a small portion of our sample—due to other central decisionmaking processes within the respective military branch. We include these closings in our analysis, though the results are essentially unchanged when excluding these latter bases.

While the hospitals themselves closed pursuant to this process, each of the bases affected by these BRAC-induced MTF closings continued to operate to some degree following the BRAC event—i.e., the bases themselves were realigned rather than closed. As such, these events did not cause MHS beneficiaries to move away in masses. We demonstrate this in Column 1 of Table II. The coefficient of the post-closing indicator suggests little change in the number of non-active duty in a catchment area following the closing of a base hospital in that area, though the coefficient on the interaction term shows that the closings led to a 12% decline in the number of active duty, relative to non-active duty, in the catchment area. But there appears to be no relative change in the underlying health mix of those who stay near the base: we find in Columns 2 and 3 of Table II that MTF closings have no effect on the age of active duty relative to non-active duty and on the log of predicted RWP (based on the available covariates).[17]

Rather than affecting the composition of MHS beneficiaries living in the affected regions, the key effect of these MTF closings is to provide an exogenous shift in the access of patients to inpatient direct care on the base. This is illustrated clearly in Figure I. In this figure, we focus on those zip codes that experienced a base-closure induced change in the distance to the closest MTF during the sample. The top line of this figure is for the period of time prior to the base closing. As we see with this line, the distance to the closest MTF (the X-axis) has a strong relationship with the likelihood of using an MTF for inpatient care versus going to a civilian hospital (the Y-axis). For those zip codes that are within 0 and 10 miles from the closest MTF, nearly 60 percent of admissions take place at an MTF; for those zip codes that are greater than 40 miles away from the closest MTF, less than 10 percent of admission take place at an MTF.

[17] To the extent there are residual concerns that the BRAC process induces beneficiaries to move in a way that compromise comparability in patient case mix, we also estimate a specification in the Online Appendix that limits the sample to those MHS beneficiaries that do not move at all throughout the sample period. The estimated impacts of liability immunity do not change in connection with this sample restriction.

In the period of time following the base closing, as shown by the lower line, all of these zip codes effectively become more alike—that is, they all become distant from the closest MTF. Even those zip codes that were initially within 10 miles of the closest MTF, the average distance to the closest MTF among post-closing encounters is over 50 miles. As such, following the closings, we would expect that the MTF-usage gradient across these groups of zip codes will flatten and will do so at a relatively low rate of MTF usage. Figure I is consistent with these expectations. While encounters in those zip codes initially close to a base took place at base hospitals nearly 60 percent of the time prior to the closing, encounters in those same zip codes took place at a base hospital (generally at the next closest MTF) less than 20 percent of the time following the closing.

This exogenous shift from MTF inpatient care to civilian inpatient care allows us to measure the effect of MTF care free of the endogeneity of MTF choice. We do so in two ways. First, we use a straightforward differences-in-differences strategy which compares the treatment of active duty and non-active duty patients who initially live near a base, interacted with a variable indicating the closing of that base's MTF. That is, we examine how care differs between active duty and non-active duty patients when they are denied access to hospital care on a nearby base. This implies an exogenous removal of malpractice protection for one group of patients relative to another, allowing us to model the effect on relative spending and outcomes.

Second, we incorporate the variation afforded by base-hospital closings more directly into equation (1) by forming a series of binary instruments representing distance of residence from the MTF (based on zip-code centroids): less than 10 miles from a military hospital, 10-20 miles away, 20-30 miles away, 30-40 miles away and 40+ miles away. We then instrument for ON-BASE and ACTVIE_DUTY X ON-BASE using these various distance bins and the interactions of each distance bin with the ACTIVE_DUTY indicator. And we include a set of zip code fixed effects,

λ_k, so that any fixed locational effects are controlled for. In this way, we achieve identification of the ON-BASE and ACTIVE-DUTY X ON-BASE coefficients only from those enrollees whose distance to the closest MTF exogenously changed due to BRAC-induced hospital closures. More specifically, we implement this IV approach via two-stage least squares, where the first stages are represented by:

$$(2)\ ON\text{-}BASE_{ije} = \alpha + \theta_1 ACTIVE_DUTY_{ie} + \delta_1 \mathbf{X_{ie}} + \eta_1 \mathbf{DIST_{ie}} + \phi_1 \mathbf{DIST_{ie}}\ X\ ACTIVE_DUTY_{ie} + \lambda_k \{+ \mu_j\} + \varepsilon$$

$$(3)\ ON\text{-}BASE_{ije}\ X\ ACTIVE_DUTY_{ie} = \alpha + \theta_2 ACTIVE_DUTY_{ie} + \delta_2 \mathbf{X_{ie}} + \eta_2 \mathbf{DIST_{ie}} + \phi_2 \mathbf{DIST_{ie}}\ X\ ACTIVE_DUTY_{ie} + \pi_k \{+ \rho_j\} + \varepsilon$$

where $\mathbf{DIST_{ie}}$ represents a series of dummy variables capturing the distance bins identified above—e.g., residence within 10 miles of an MTF. Equation (1) above then captures the second stage of this 2SLS analysis. While Figure 1 (discussed above) graphically depicts the essence behind these first stage regressions, equations (2) and (3) formalize this analysis and capture the extent to which MTF utilization is associated (non-parametrically) with how far patients live from the closest MTF, again focusing on variation in these distances induced by base-hospital closings. The models include zip code fixed effects to control for general differences across enrollees who live at difference distances from the MHS. We present the full first stage results in the Online Appendix. We note here, however, that MTF utilization (and its interaction with active-duty status) are strongly determined by these distance-based instruments, with a Cragg-Donald F-statistic on the instruments of 1960, well beyond conventional critical thresholds (Stock and Yogo, 2005).

Importantly, the resulting coefficients from this IV specification can be compared directly to our estimates of the difference-in-difference specification indicated in equation (1) and can

provide us an estimate of the magnitude of the effect of malpractice immunity on treatment intensity and outcomes.[18]

IV.B. Addressing Provider Selection Concerns

The second concern with our basic framework is that medical providers—be it physicians or hospitals—who serve the treatment group may be different in some systematic way from providers that serve other groups; for example, physicians who tend to treat active duty patients at MHS facilities may be particularly high skilled physicians. We can address this concern by introducing a provider-specific fixed effect (μ_j) in some models, doing so separately for physicians and hospitals across alternative specifications.[19] This controls for general provider-specific treatment styles, by comparing the outcomes of the treatment group compared to control groups treated by the same provider. While we show results for both hospital effects and physician effects below, where space limits us in certain tables to adopt a primary approach, we elect to focus on hospital effects, given certain limitations with the physician data available to us.[20]

This provider fixed-effects approach does, however, raise some interpretation difficulties if there are patient treatment "spillovers" between immune and non-immune patients. For example, the threat of liability from some patients may contribute to a physician's overall practice style that they exhibit to both active and non-active-duty patients. In this event, if physicians do practice defensively, then some amount of the estimated treatment effect picked up in the active-

[18] In the Online Appendix, we estimate similar IV specifications that simply instrument the ON-BASE and ON-BASE X ACTIVE indicator variables with indicator variables for the incidence of a hospital closing and for the interaction of this closing with the ACTIVE indicator. The distance-based IV specification differs slightly in spirit from a model that relies on a binary hospital-closing indicator in that it offers a natural way to capture the difference in kind between the following two hypothetical closings: one MTF closing that leaves the next-closest alternative MTF at only 15 miles away versus another MTF closing that leaves the next-closest alternative MTF greater than 100 miles away. The latter type of closing would be expected to more strongly drive the affected population to use civilian facilities. In the end, however, this distinction makes little difference as the binary approach generates nearly identical results.

[19] In unreported regressions, we also estimate specifications with both physician and hospital fixed effects together. The results are nearly identical to the separate findings that we report below.

[20] There are two key limitations with the provider effects: (1) we are assigning providers based on the attending physician overseeing the inpatient stay, though we acknowledge that inpatient stays often involve the care provided by a larger number of physicians and (2) the MDR records do not provide consistent physician identifier codes across the direct care and purchased care settings that would allow us to observe given doctors operating both on and off the base. With this latter limitation in mind, the entire sample includes roughly 38,000 unique physicians.

duty/non-active-duty differential inherent in the above design may derive from across provider variation, or within-provider variation over time, in the share of active-duty patients. In this event, the inclusion of provider fixed effects could attenuate true treatment effects. As such, changes in estimated treatment effects with the inclusion of provider effects may be attributed to the presence of spillovers and/or to provider selection (as discussed above).

By way of preview, our results below demonstrate only a minor impact if any from including provider effects, which either suggests that both provider selection and spillovers are moderately inconsequential phenomena or that they operate in different directions and thus roughly offset each other. In a partial attempt to resolve this uncertainty, we test for markers suggestive of spillovers in the Online Appendix.[21] These tests are subject to certain selection concerns of their own, however, limiting the force of any conclusions we can draw.[22] Nonetheless, this analysis does suggest some degree of within-physician spillovers. Our analysis of within-hospital spillovers is rather noisy and inconclusive, however. Ultimately, while the provider effects specifications estimated below may allow us to account for provider selection concerns, the results may only constitute a lower bound of the full impacts of liability immunity depending on the extent of spillovers that actually exist.

IV.C. Addressing Sample Selection Concerns

Our third concern with the difference-in-difference strategy is the fact that we are conditioning on hospital utilization. If the differential malpractice pressure impacts the odds of entry into the hospital, this could change the mix of patients that we see in a way that potentially

[21] In short, we examine how the treatment of non-active duty patients varies with the share of a physician's (or hospital's) caseload that is active duty. If there are spillovers, then the higher is a physician's (or hospital's) share of active duty patients, the less intense should be their treatment of non-active duty patients.

[22] It is possible that when a provider sees relatively fewer non-active-duty, the remaining non-active-duty could be relatively more sick, a possibility that would bias against finding evidence of spillover effects. We look for suggestions of this selection in the Online Appendix by regressing predicted treatment intensity (based on a regression of the relevant intensity metric on the covariates) on the active-duty patient share of the provider, focusing on the sample of on-base encounters for non-active-duty patients. We find mixed evidence supporting these selection concerns.

biases the results. This is clearly less of a concern in the case of the delivery sub-sample, since nearly all deliveries occur in hospitals. To address this concern in the face of the full inpatient sample, we take advantage of the fact that we have data on the full beneficiary population of the MHS and thus possess knowledge on who does and does not receive inpatient care in the first place. With this information, we directly show in Column 4 of Table II that there is no differential selection into or out of the hospital by our treatment group in the face of base closing. In particular, that column shows that the odds of using the hospital among the full population in the catchment area of any base is unrelated to the interaction of active duty and base closing.

In the alternative, we address this concern by excluding deferrable medical conditions. That is, following Card, Dobkin, and Maestas (2009) and Doyle et al. (2015), we focus on admissions associated with diagnosis fields whose weekend admission rates are close to 2/7,[23] reflecting admissions types in which physicians have less discretion over the admission decision itself, thereby further appeasing these particular sample selection concerns.

IV.D. Addressing Mechanism Concerns

The final concern we face is that the core effect that we identify—the differential treatment of active duty patients versus non-active duty patients on base, compared to that same differential off base—captures more than just differences in malpractice pressure. To be clear, this would require some reason (other than differential malpractice pressure) for less intensive treatment of active duty patients *only* when at the base that does *not* apply to non-active duty patients. If such a reason exists, there is no clear sign to the bias to our results.

[23] Specifically, we exclude admission whose primary diagnosis codes on average throughout the sample exhibit weekend shares less than 20%, reflecting admission types in which admitting physicians likely have either a substantial or a modest amount of discretion over the admission decision. The results are robust to other thresholds. We chose a 20% threshold as this only amounts to excluding a little over half of the sample, leaving behind a still robust sample size. .

Nevertheless, we take several approaches in reinforcing a liability interpretation of our findings. To begin, the case of childbirth provides a useful check on this alternative explanation, since the predicted effect of defensive medicine in childbirth from Currie and Macleod (2008) is the opposite of what we would expect for other inpatient care (e.g. that defensive medicine leads to less intensive childbirth treatment, while it leads to more intensive treatment of other inpatient illnesses). As discussed above, this prediction essentially stems from the notion that the marginal mother receiving a cesarean delivery may be rather in little need of a cesarean, in which case the liability risks most pressing in her case are those respecting errors in committing a cesarean delivery itself. Therefore, if we find opposite signed effects for childbirth and other inpatient treatments, it is directly consistent with the defensive medicine explanation and not alternatives.

Beyond the childbirth context, the prior literature provides little guidance by which to form similar predictions regarding the relative sign (and magnitude) of defensive medicine *across* other diagnoses types. However, *within* any given diagnosis type, one can at least more readily predict that physicians will tend to exhibit positive defensive medicine—increasing procedure utilization in response to liability fears—to a stronger degree in the case of diagnostic procedures relative to non-diagnostic procedures (Frakes 2015). This is because for any given diagnostic procedure, the prevailing liability risk is likely to be one concerning errors of omission—e.g., failing to perform a necessary test. Responding to these concerns, physicians will tend to increase the amount of tests they perform. However, in the face of any given non-diagnostic procedure, a physician may face some degree of concern over liability risks stemming from errors of commission—e.g., botching a procedure—and errors of omission—e.g., failing to perform a needed a procedure. On the one hand, these risks may encourage physicians to perform more procedures, while on the other hand, they may discourage higher treatment intensity. Accordingly, the net influence of the law

is likely to be more attenuated in this latter context. To the extent that we indeed find this predicted pattern of evidence, it is further supportive of a liability interpretation of the primary findings.[24]

Finally, we separately estimate our key specification in two distinct sub-samples: (1) states without caps on non-economic damages (pain and suffering) and total damages awards and (2) states with either non-economic damages caps or total damages caps.[25] By capping damages at some specified level, the relevant jurisdictions are already effectively immunizing providers from some portion of the potential damages award. As such, cap adoptions should absorb some portion of the overall impact of immunizing providers from liability. Accordingly, we would predict a smaller impact of liability immunities in states that have already imposed pain and suffering caps or total damages caps. Of course, just how much of a difference we would expect is subject to much uncertainty given our review of the literature above. Nonetheless, given that recent meta-analyses on defensive medicine predict some overall degree of aggregate spending reductions in response to caps (for instance, Mello et al. 2010), we do predict a difference—even if minor—in effects in states with and without caps; and if we do find such a difference, it will further reinforce the liability explanation over alternative mechanisms behind a reduction in treatment intensity for active duty being treated at MTFs.

V. RESULTS

V.A. Inpatient Treatment Intensity Analysis – Difference-in-Difference Results

[24] Consistent with these views, the malpractice literature to date has generally found stronger evidence of defensive medicine in the diagnostic procedure context. For instance, Baicker et al. (2007) estimate that the association between malpractice awards / premiums and Medicare spending is nearly twice as strong in the case of imaging services relative to other Medicare services. A similar conclusion was reached in Office of Technology and Assessment (1994). With these views in mind, other malpractice studies appear to take as given that diagnostic utilization is only relevant for positive defensive medicine, whereas other procedures may implicate both positive and negative defensive medicine. For instance, when surveying physicians as to whether they practice either kind of defensive medicine, Studdert et al. (2005) only discuss diagnostic testing when defining positive defensive medicine for survey respondents.

[25] In a simulation analysis, Hyman et al. (2009) demonstrate that both non-economic damages caps and total damages caps cut off a meaningful percentage of jury verdicts in medical liability cases, with total caps arguably reducing damages awards the most. We receive data on caps from the Database of State Tort Law Reforms, 5th Edition compiled by Ronen Abraham (Avraham 2014).

We begin by showing the impact of malpractice pressure on patient treatment intensity. Table III shows the results for Relative Weighted Product (logged). The first column shows the results from the Ordinary Least Squares (OLS) difference-in-difference specification with various patient-level covariates included: patient age dummies, sex, pay-grade levels, Charlson comorbidity scores, and primary diagnosis fixed effects. The key difference-in-difference coefficient of interest is captured by the coefficient of the Active-Duty-Patient-by-MTF interaction term, which suggests that immunity from liability—i.e., our "treatment"—is associated with a 5.0 log-point decrease in treatment intensity.

The coefficient of the MTF variables does evidence baseline differences in utilization levels across MTF and civilian care locations—indicative of lower intensity at military facilities, all else equal. The interaction term, however, suggests that these MTF-civilian differences are notably more pronounced in the case of active duty patients—i.e., the group that lacks medical liability recourse when treated at MTFs. As such, this finding implies that treatment intensity at the MTF might have been greater had active duty patients possessed the right to sue in the event of harm arising from negligent care. Other coefficients in the regression have their expected signs, with higher comorbidity enrollees garnering more utilization, and variability across pay grade.[26]

In the second and third columns of Table III, we address concerns over provider selection. For instance, as noted earlier, it is possible that active duty patients choose civilian hospitals (or MTFs) who treat more or less intensively than the civilian hospitals (or MTFs) chosen more frequently by non-active duty patients. We attempt to control for this by including hospital fixed effects (Column 2) and physician fixed effects (Column 3). In this specification, we thus show the

[26] In yet another specification, we include the local unemployment rate as an additional regressor (at the county-year level). The estimated coefficient of the *MTF X ACTIVE_DUTY* interaction variable remains virtually unchanged with this addition The estimated coefficient of the unemployment rate variable itself is near zero (and statistically indistinguishable from zero).

relative effects within providers in their treatment of active duty versus non-active duty patients (and how such relative effects differ when looking at encounters on the base versus off). With this addition, the coefficient of the active duty-on-base interaction remains very similar at -0.054 and -0.039 across these two columns; moreover, these several estimates are statistically indistinguishable from each other, suggesting either little or no provider-based selection or an offsetting influence of provider selection and patient-level spillovers within providers. As discussed in Part IV, should patient-level spillovers exist, our results can be seen as a lower bound for the full treatment effects stemming from liability immunity.

V.B. Inpatient Treatment Intensity – Base Closings Analysis

A major concern with this strategy noted above was the fact that choice of treatment at the MTF is endogenous. To address this, we turn to our closings analysis. We start with a simple presentation of this analysis and then more comprehensively build it into the estimation framework just discussed.

To begin, we estimate a difference-in-difference specification where we form a post-closings dummy variable which equals 0 for all zip codes not affected by a closing (i.e., whose closest-MTF distances do not change throughout the sample period) and which equals 0 for the affected zip codes in the time period prior to the closing, while equaling 1 for the affected zip codes post-closing. We then interact this variable with the active-duty dummy. Since closings push beneficiaries to use civilian hospitals as opposed to MTFs, we expect to observe an opposite-signed coefficient of this interaction variable relative to the coefficients estimated in Table III. A positive coefficient of the post-closings-active-duty interaction suggests that treatment intensity increases for active duty relative to non-active duty as care moves off the base—i.e., to an environment where active-duty attain liability rights.

Consistent with these expectations, we do indeed estimate a positive relationship between treatment intensity (log RWP) and the post-closings-active-duty interaction variable. We present the results of this exercise in Figure II and in Table A2 of the Online Appendix. We start in Table A2 by showing results for a difference-in-difference specification that focuses only on zip codes that were initially close to a base hospital (within 40 miles at the beginning of the sample) and that were affected by a base hospital closing—i.e., zip codes whose closest-MTF variable changed during the sample due to a closing. We then proceed across the various columns of Table A2 to incrementally enrichen this naïve specification, adding zip-code effects and provider effects, while also including as controls encounters in zip codes unaffected by MTF closings. The findings are notably stable across each specification, with the most complete approach suggesting a 3.4 log-point increase in RWP for active duty relative to non-active duty in the post-closing relative to the pre-closing period.

In Figure II, we confront the base closings analysis more rigorously in an event-study framework. We modify the specification from Table A2 to include leads and lags of both the closing variable and the interaction term of interest, and plot the latter set of interactions over time. The estimated coefficients of the leads of this interaction term allow us to test whether the divergence in treatment intensity between active duty and non-active duty that we estimate in connection with MTF closings, in fact, began to materialize prior to the closing, an effect that would otherwise cast some doubt on a causal interpretation of the base-hospital-closure findings. This falsification exercise sounds no alarms, with each of the estimated lead coefficients being close to 0 in magnitude and statistically indistinguishable from 0. This finding suggests that the treatment intensity delivered to active duty would have trended in the same manner as it would for the non-active duty but for the closings. Following the moment of the closing, however, we begin

to observe an increase in log RWP for active duty relative to non-active duty, consistent with a causal effect of liability immunity.

While this framework clearly illustrates the impact of closings on the treatment of active duty versus non-active duty patients, the magnitudes cannot be directly compared to the difference-in-difference coefficient of interest from Table III. To create a comparable estimate of the bottom line impact of immunity on treatment, we add additional columns to Table III to estimate the closings-based IV strategy discussed in Part IV. This specification allows us to control for endogeneity of use of the base hospital (since it draws on closings to exogenously shift access to base hospitals insofar as it draws on closings to induce within-zip-code changes in distances to the closest MTF). With this IV specification, we find that liability immunity is associated with a 5 log-point reduction in treatment intensity. When adding either hospital of physician fixed effects, these finding change very little. Overall, the pattern of IV results is nearly identical to the OLS results presented in Columns 1 – 3.

Thus far, our results have focused only on the Relative Weighted Product, the Department of Defense derived intensity metric designed to be comparable across direct and purchased care settings. To explore matters further, in Table IV, we show the results for the following alternative measures of treatment intensity: hospital length of stay (logged), the incidence of any procedure being performed during the encounter, and the number of procedures administered during the encounter (logged). For the purposes of brevity, we show only our IV estimates with hospital fixed effects (though the results are robust to the inclusion of physician fixed effects and to the estimation of OLS specifications). Consistent with the RWP findings, the results from Table IV indicate that liability immunity is associated with significant reductions—ranging from approximately 4 to 5 percent—in each of these alternative metrics.

On a final note regarding Table III, in Table A7 of the Online Appendix, we estimate an analogous table that focuses on a subset of admissions for medical conditions over which physicians have less discretion over whether or not to admit the patient to the hospital in the first place (as discussed in Part IV). The resulting sample size is slightly less than 1/2 of the primary analytical sample. With this more restrictive approach, we continue to estimate a negative association between liability immunity and treatment intensity, though at a magnitude that is roughly 1-2 percentage points lower in absolute terms than the corresponding estimates in Table III.[27]

V.C. Diagnostic Utilization Analysis

In Table V, we separately explore the impacts of liability immunity on the incidence of the utilization of any diagnostic procedure (Panel A) and on any non-diagnostic procedure (Panel B). As indicated in Part IV, this exercise is helpful in separating a medical liability interpretation of our findings from alternative mechanisms unrelated to liability. Under a medical liability story, one would predict stronger negative impacts of liability immunity in the case of diagnostic tests. Given the specificity of many such tests to the particular clinical setting—e.g., esophagogastroduodenoscopy in the case of gastrointestinal encounters—we first perform this analysis separately across a range of the most common Major Diagnostic Categories represented in our data, with each column in Table V representing a different category. In the second-to-last column, we consider a residual category of the remaining diagnostic categories pooled and in the

[27] The Online Appendix also subjects this IV analysis to certain additional specification checks, including one that excludes all injury-related admissions in an attempt to exclude workplace injures which may represent a form of non-comparability between active duty and non-active duty. We also demonstrate that the results are virtually identical when including a control for the distance to the closest hospital of any kind (civilian or MTF), a factor that also changes in connection with base-hospital closures. The almost complete insensitivity of the results to this control is perhaps unsurprising given that the primary specification is designed to capture the manner in which closures otherwise differentially affect active duty and non-active duty. Any additional general impacts of closures on treatment patterns should be accounted for through the differencing calculations themselves. Finally, we also demonstrate the robustness of the findings to inclusion of controls for catchment area assignments that are updated over time following the BRAC process.

final column, we pool over all diagnostic categories.[28] For the purposes of brevity, we present only the results of the IV specification with hospital fixed effects, though the resulting pattern of coefficients is robust to the estimation of OLS specifications and to the inclusion of physician fixed effects.

First of all, we note that across every diagnostic category in the case of the diagnostic testing specifications, we document a negative coefficient for the *ON-BASE X ACTIVE_DUTY* interaction variable suggesting a negative impact of medical liability immunity on the incidence of utilization of any diagnostic procedure. The magnitudes of these negative coefficients—as a percentage of the relevant mean diagnostic rates—are considerable, ranging from 3% to a 67%. In the case of the non-diagnostic-testing specifications, however, we estimate coefficients of the key interaction variable that are much closer to zero across each diagnostic category. Only two such estimates are marginally significant—in the case of orthopedic and gastrointestinal admissions—and, in those cases, the point estimates are actually small and positive suggesting an increase in non-diagnostic procedure use in connection with immunity from liability. All told, across each and every category, the effect of liability immunity on procedure utilization rates is more strongly negative in the case of diagnostic testing relative to non-diagnostic procedures.

In the final pooled column, we find a roughly 22 percent decrease in the incidence of diagnostic testing in connection with liability immunity, which is meaningfully and statistically different from the roughly 1 percent increase in the incidence of any non-diagnostic procedures in association with liability immunity. Ultimately, it appears that the negative effects of liability immunity on the incidence of procedure utilization documented in Table IV are driven by diagnostic testing behaviors. Accordingly, these results imply that the increases in procedure

[28] For the reasons stated earlier (regarding the inability to fully identify instances of fetal monitoring), we perform these analysis on all contexts other than labor and delivery.

utilization resulting from malpractice pressure—i.e., "positive" defensive medicine—are likely to be concentrated on such behaviors.

While this pattern of stronger "positive" defensive medicine in the case of diagnostic testing is intuitive under theories of defensive medicine, one would not necessarily expect such an outcome under alternative explanations for the general observation of lower treatment intensity for active duty patients being cared for on the base. As such, this pattern of results arguably reinforces a liability interpretation of the findings.

V.D. Childbirth Analysis

The diagnostic analysis above separates our inquiry into clinical situations whereby we would expect to observe a greater concern over errors of omission—e.g., failing to perform a procedure—versus errors of commission—e.g., harms arising from the execution of that procedure. The direction of any defensive medicine effect—positive or negative—will depend upon which of these concerns dominates on the margin. In this sub-section, we extend this thought process further, but now, instead of focusing on different types of procedures within admission categories, we turn our attention to comparisons across different types of inpatient admissions. In particular, we compare the full inpatient sample results from Table III to a sub-sample of admission types that the previous literature (Currie and MacLeod 2008) has identified likely presents greater concerns on the margin over errors of commission: childbirth.

In Table VI, we estimate the same progression of treatment intensity specifications estimated in Table III, but in the context of the labor and delivery sub-sample (OLS and IV specifications, in each case, with and without provider effects). Consistent with Currie and Macleod's (2008) prediction that the marginal mother faces a greater risk associated with the execution of a cesarean delivery than she does over proceeding with a vaginal delivery, we find

evidence that a reduction in liability pressure is associated with an increase in treatment intensity. Specifically, the estimated coefficient of the interaction between the MTF variable and the active-duty variable implies a 0.8 – 1.6 log-point increase in the Relative Weighted Product in connection with liability immunity.

These RWP estimates correspond with estimates from specifications that use other measures of treatment intensity, as demonstrated in Table VII. Here, we estimate our richest model (IV with hospital fixed effects) for four measures of treatment intensity: the incidence of cesarean section delivery, total bed days (logged), the incidence of any procedure greater than 1 (i.e., the incidence of any procedure other than the delivery itself), and the number of procedures (logged). In each case, we estimate positive and significant coefficients, illustrating once again that immunity from malpractice is leading to more intensive treatment of childbirth. In particular, the cesarean results suggest a 0.008—or a roughly 4%--increase in cesarean utilization in connection with liability immunity.

The fact that we observe opposite signed effects in this delivery context—as expected under a medical liability story—complements the diagnostic procedure analysis in lending further support to a medical liability mechanism underlying the set of results presented above.

V.E. Patient Outcomes

We have shown that removing malpractice pressure substantially reduces treatment intensity across the overall inpatient sample. But the welfare impacts of this finding are far from clear. If this reduced treatment intensity significantly worsens patient outcomes, then malpractice pressure may be a productive way of ensuring that quality care is delivered to patients. But if the reduced treatment has no effect on outcomes, or if it even improves outcomes, this would argue that the "defensive medicine" that we see in non-immune environments is wasteful.

As discussed in the data section, we consider several measures of patient outcomes in our inpatient sample analysis.[29] Table VIII shows the results for each measure, again focusing for brevity purposes on the IV specification with hospital fixed effects (as before, with the results being similar in the omitted OLS and other alternative specifications). In every case in Table VIII, we fail to find evidence suggesting that liability immunity worsens health care outcomes and quality—i.e., that malpractice pressure improves outcomes. To be sure, each individual result is somewhat noisy and, in the case of the two mortality outcomes and patient safety indicators, we cannot necessarily rule out that liability pressure improves outcomes. We show this in the remaining rows of the Table, which show the confidence intervals in absolute and percentage terms. For example, while we estimate that the treatment group sees a reduction in mortality, even while receiving less care, we can't rule out that there is an increase in mortality of 0.08 percentage points (or 17%).

Nonetheless, the consistency in estimates across the different quality metrics supports a general inference that the liability system does not substantially improve health care quality. Together with the treatment intensity results for the full inpatient sample from Tables III-V, the quality results from Table VIII are consistent with a defensive, sub-optimal response in physician behavior to liability fears, where liability forces induce greater spending with no appreciable improvement in quality.

In the case of unplanned hospital readmissions, we actually estimate a negative coefficient of the key interaction variable, which may tend to suggest that stronger liability pressure actually worsens health outcomes, only reinforcing the defensive interpretation of the full set of results.

[29] Though we still only consider a limited number of outcomes, our investigation into the health outcome impacts of medical liability covers a wider variety of indicators than that explored by most studies in the literature. For instance, Kessler and McClellan (QJE 1996) only consider mortality and readmissions.

However, we caution that readmissions may not constitute a pure outcome metric and may also reflect a subsequent clinical decision on the part of physicians—i.e., whether to readmit. As such, the negative impact of liability immunity on readmission outcomes may, in part, reflect a story in which liability pressure causes physicians to readmit marginal cases, a finding that itself may further a defensive medicine interpretation of our findings (as discussed in Part III above).[30]

In Table A3 of the Online Appendix, we evaluate the impact of liability immunity on a number of health outcomes in the obstetrics context. As with Table VIII, we find that liability immunity is either associated with an improvement in outcomes (in the case of maternal trauma rates) or associated with no discernable changes in outcomes (though these null results are quite noisy in the case of case of neonatal mortality, which is perhaps unsurprising given the low baseline neonatal mortality rates). As before, to the extent that the change in treatment patterns brought about by liability immunity is not accompanied by deterioration in clinical quality, the findings are consistent with a "defensive" response to liability fears.

V.F. Generalizability Analysis

A concern with this analysis is the degree to which these results may generalize beyond the MHS setting. At the outset of raising this concern, we note that our analysis itself respects the influence of medical liability forces within a $50 billion / year system, a program of a scale and importance that arguably merits independent interest.

That being said, there may be reason to believe that our results may represent a lower bound for the impacts that liability immunities may have for the medical system at large. For instance, Kessler and McClellan (2002) argue that defensive medicine may be stronger in fee-for-service environments because physicians are reimbursed for any additional precautionary tests and

[30] It is worth noting, however, that we find no evidence of a general relationship between liability pressure and the likelihood of hospitalization, so the negative effect on readmissions would not reflect a pure mechanical impact of fewer admissions.

procedures that liability fears may encourage them to undertake - in contrast with settings like MHS facilities where physicians face salaried payment structures (or where they face other restraints through managed-care structures).

In one final exercise, we therefore attempt to shed some empirical light on this generalizability discussion – in a fashion which can further provide confidence in the causal role that malpractice pressure is playing in driving our results. In particular, we utilize the primary empirical methodology from this paper to separately explore the impact of immunizing medical encounters from liability in states with caps on non-economic or total damages awards and in states without such caps. The difference in such estimates should illuminate the degree to which caps themselves influence treatment intensity.

As background, note that medical liability itself represents an aggregation of damages that a negligently harmed patient may recover, including economic damages (lost wages, medical expenses, etc.) and non-economic damages (e.g, pain and suffering). Accordingly, one can arguably decompose the effects of immunizing medical encounters from liability in states that otherwise place no limitations on damages awards into two parts: (1) the effects of immunizing providers from paying non-economic damages (or any damages) above some limit—i.e., the effects from imposing a damages cap and (2) the effects of immunizing providers from having to pay along the remainder of the damages spectrum—that is, all damages below the cap.[31] Importantly, our methodological framework allows us to estimate the second of these effects—by exploring the relationship between liability immunity and treatment intensity in states that have already imposed caps on damages awards—while also allowing us to estimate the sum of both the

[31] As above, physicians themselves may be indifferent to the damages level in a direct sense given the nature of their insurance coverage. What drives their defensive impulses may instead be the reputation and psychological damage that results from being sued in the first place. Damages are relevant to these mechanisms to the extent potential damages impact the likelihood of plaintiffs (and their attorneys) filing suit in the first place (Frakes and Jena 2016). Arguably, the economic damages and the non-economic damages below the cap are most critical in impacting these plaintiff incentives. However, damages levels above the cap may also impact the likelihood of suit (Paik et al. 2013).

first and second effects—by exploring the relationship between liability immunity and treatment intensity in states that have not capped damages. Therefore, since we can estimate the aggregate effect and the second effect, we can isolate the first effect—that is, the impact of caps on treatment intensity—by taking the difference between the impacts of immunity in states with and without caps.[32]

We present the results of this exercise in Table IX. The first two columns of the Table show our basic instrumental variables estimation (using base closings), estimated separately for two samples of states: those without non-economic damages caps and total damages caps (column 1) and states with caps on either non-economic damages or total damages (column 2). We estimate a 6.5% reduction in RWP in the states without caps, compared to a 4.4% reduction in RWP in states with caps. The implication is that the effects of caps on treatment intensity roughly equals 2.1%. In Column 3, we show this comparison more explicitly by estimating a specification over all states that interacts the main *ON-BASE X ACTIVE_DUTY* variable with an indicator variable for the presence of a cap (and includes the other necessary double interaction terms). In this specification, the coefficient of the triple interaction term suggests a 2.1 log-point differential between cap and non-cap states (with the non-cap states evidencing a stronger negative effect), though only significant at the 10% level.

Interestingly, this magnitude falls in between two recent meta-analyses of defensive medicine studies. First of these analyses is Mello at al. (2010), which uses results predominantly from damage-cap-focused studies to predict that the portion of defensive medicine attributable to caps (and similar reforms) contributes to a roughly 5% increase in hospital spending and a roughly

[32] While claims by dependents against military facilities will be brought under the Federal Tort Claims Act, the substantive law applying in such instances will be the prevailing state laws of the relevant location, in which event state damage cap requirements will apply. The same will be true of any suit brought against civilian providers providing care off the base.

2% increase in overall healthcare spending. Second is the CBO study discussed in the Introduction which predicts that a package of liability reforms—foremost among them being non-economic damages caps—would lead to a roughly 0.3% decrease in overall healthcare spending.[33]

Accordingly, the comparability of our damage-cap findings to those deriving from investigations on other patient populations suggests that our investigation into the impacts of liability immunity among the military health population may at least roughly generalize to the population at large. Moreover, our observation of stronger impacts of immunity in states without caps is consistent with the causal interpretation of our findings as the result of liability pressure and not some alternative explanation.

VI. DISCUSSION AND CONCLUSION

Many within the public sphere and within the medical community have long identified the medical liability system—and in particular the defensive impulses that stem from that system—as a significant and sizeable contributor to the nation's overall level of health care spending. Empirical scholars of the medical liability system have cast doubts on the strength of this conventional wisdom given the mixed and generally modest evidence of defensive medicine that they have produced (and the CBO has agreed). However, the variations within the liability environment that the existing studies have drawn upon to paint this uncertain picture are arguably marginal in nature—e.g., comparing physician behavior before and after tort reforms that maintain the structure of the liability system but that simply place a cap on the amount of pain and suffering awards that successful plaintiffs may keep. Therefore, it is possible that the variation exploited by the literature to date to make such determinations cannot speak to the full impacts of liability

[33] The implicit estimate of the effect of caps on spending in the CBO analysis is actually higher than this amount, however, because they are simulating the impact of a nationwide adoption of these reforms, even though nearly 55% of states had already adopted caps. Even when scaling their prediction up with this in mind (that is, roughly doubling it), the CBO estimates are likely still notably below that of Mello et al.'s predictions.

pressures on behavior, as Mello et al. (2010) note in their recent discussion of the overall impacts of defensive medicine. Moreover, these studies largely do not capture the variation that is required to analyze the most recent proposals for reform of our malpractice system, which provides physician "safe harbors" for following clinical guidelines.

In this paper, we capture variation along a dimension more relevant to current debates: comparing environments where liability is and is not a possibility at all, taking advantage of a long-standing doctrine applied to active duty members of the U.S. military. We use a novel data set to explore one of the nation's largest (and least studied) health care programs, the Military Health System. The immunity from malpractice for active duty doctors treating active duty patients on the base, but not for other groups, provides a natural comparison for understanding the effect of malpractice immunity. Moreover, the large spate of base-hospital closings (due to the BRAC process) over our period allow us to augment our analysis with an IV strategy that exploits exogenous shifts in the availability of MTF hospitals at which active duty physicians are immune. And the availability of hospitals and physicians who treat both active and non-active duty patients allows us to control for selection of hospitals and physician by patients.

We conclude from our empirical analysis that there is strong evidence of defensive medicine. When patients are unable to pursue malpractice remedies, they are treated 5% less intensively. This result shows up in a variety of indicators of care intensity. In addition, we find consistent evidence that the outcomes of immune patients are no worse, and perhaps even better, than those patients who have a malpractice remedy. These results therefore strongly suggest that policies which broadly immunize physicians from malpractice remedies will lower medical spending without harming patients.

The implications of our findings for malpractice reform policy depend very much on the details of the policy. On the one hand, our results do *not* imply that marginal changes to the malpractice environment such as damage caps will move us fully in the direction capturing these potential reductions in defensive medicine, though, as we discuss in Part V(E), they may take us part of the way. On the other hand, a "safe harbor" policy with guidelines that are set at the "natural" point of treatment intensity (e.g. the point at which patients would be treated if they could not sue) could yield results similar to those that we estimate here.

Guidelines which are set at the prevailing norm, however, which incorporates existing defensive medicine effects, have an ambiguous effect. Guidelines of this latter nature may provide certainty against lawsuit, which should reduce defensive medicine pressure; however, that certainty may come at the cost of excessive medical delivery among those below the prevailing standard.

DUKE UNIVERSITY AND NATIONAL BUREAU OF ECONOMIC RESEARCH

MASSACHUSETTS INSTITUTE OF TECHNOLOGY AND NATIONAL BUREAU OF ECONOMIC RESEARCH

REFERENCES

Avraham, Ronen, "An Empirical Study of the Impact of Tort Reforms on Medical Malpractice Settlement Payments," *Journal of Legal Studies* 36(S2) (2007), S183-S229

Avraham, Ronen, "Database of State Tort Law Reforms (5th)," University of Texas Law and Economics Research Paper No. e555 (2014).

Avraham, Ronen, and Max Schanzenbach, "The Impact of Tort Reform on Intensity of Treatment: Evidence from Heart Patients," *Journal of Health Economics* (39) (2015), 273-88.

Baicker, Katherine, Elliott S. Fisher, and Amitabh Chandra, "Malpractice Liability Costs And The Practice Of Medicine In The Medicare Program," *Health Affairs* 26(3) (2007), 841-52.

Baldwin, Laura-Mae, L. Gary Hart, Michael Lloyd, Meredith Fordyce, and Roget Rosenblatt, "Defensive medicine and obstetrics," *JAMA* 274(20) (1995), 1606-10.

Card, David, Carlos Dobkin, and Nicole Maestas, "Does Medicare Save Lives?" *Quarterly Journal of Economics* 24 (2009), 597-636.

Carrier, Emily R., James D. Reschovsky, Michelle M. Mello, Ralph C. Mayrell, and David Katz, "Physicians' Fears of Malpractice Lawsuits Are Not Assuaged By Tort Reforms," *Health Affairs* 29(9) (2010), 1585-92.

Congressional Budget Office (CBO), Economic and Budget Issue Brief: Limiting Tort Liability for Medical Malpractice, January 8, 2004.

Cragg, John G., and Stephen G. Donald, "Testing Identifiability and Specification in Instrumental Variable Models," *Econometric Theory* 9 (1993), 222-240.

Currie, Janet, and W. Bentley MacLeod, "First Do No Harm? Tort Reform and Birth Outcomes," *Quarterly Journal of Economics* 123 (2008), 795-830.

Doyle, Joseph, John Graves, Jonathan Gruber, and Samuel Kleiner, "Measuring Returns to Hospital Care: Evidence from Ambulance Referral Patterns," Journal of Political Economy 123(1) (2015), 170-214.

Dubay, Lisa, Robert Kaestner, and Timothy Waidmann. "The Impact of Malpractice Fears on Cesarean Section Rates," *Journal of Health Economics* 18 (1999), 491-522.

Jena, Anupam, Seth Seabury, Darius Lakdawalla, and Amitabh Chandra, "Malpractice Risk according to Physician Specialty," *New England Journal of Medicine* 365 (2011), 629-636.

Frakes, Michael, "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) (2013), 257-76.

Frakes, Michael, "The Surprising Relevance of Medical Malpractice Law," *University of Chicago Law Review* 82(1) (2015), 317-91.

Frakes, Michael, and Anupam B. Jena, "Does medical malpractice law improve health care quality?," *Journal of Public Economics* 143 (2016), 142-58.

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

Hyman, David, Bernard Black, Charles Silver and William Sage, "Estimating the Effect of Damages Caps in Medical Malpractice Cases: Evidence from Texas," *Journal of Legal Analysis* 1(1) (2009), 355-409.

Kessler, Daniel, and Mark McClellan, "Do Doctors Practice Defensive Medicine?," *Quarterly Journal of Economics* 111 (1996), 353-90.

Kessler, Daniel, and Mark McClellan, "Malpractice law and health care reform: optimal liability policy in an era of managed care," *Journal of Public Economics* 84(2) (2002), 175-97

Klick, Jonathan, and Thomas Stratmann, "Medical Malpractice Reform and Physicians in High-Risk Specialties," *Journal of Legal Studies* 36 (2007), S121-142.

Lakdawalla, Darius, and Seth Seabury, "The Welfare Effects of Medical Malpractice Liability," *International Review of Law and Economics* 32(4) (2013), 356-69.

McClellan, Mark, "Hospital Reimbursement Incentives: An Empirical Analysis," Journal of Economics and Management Strategy 6(1) (1997): 91-128.

Mello, Michelle M., Amitabh Chandra, Atul A. Gawande, and David M. Studdert, "National Costs of the Medical Liability System," *Health Affairs* 29 (2010), 1569-77.

Mello, Michelle M. and Allen Kachalia, Evaluation of options for medical malpractice system reform: a report to the Medicare Payment Advisory Commission, (MedPAC, 2010).

Mello, Michelle M. and Allen Kachalia, Medical Malpractice: Evidence on Reform Alternatives and Claims Involving Elderly Patients, A Report to the Medicare Payment Advisory Commission, (MEDPAC 2016).

Moghtaderi, Ali, Steven Farmer, and Bernard Black, "Damage Caps and Defensive Medicine: Reexamination with Patient Level Data," working paper available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2816969 (2017).

Office of Technology and Assessment, *Defensive Medicine and Medical Malpractice* (1994).

Paik, Myungho, Bernard Black and David Hyman, "The Receding Tide of Medical Malpractice Litigation Part 2: Effect of Damage Caps," *Journal of Empirical Legal Studies* 10(4) (2013), 639-69.

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

Schoenfeld, Andrew J., Wei Jiang, Mitchell B. Harris, Zara Cooper, Tracey Koehlmoos, Peter A. Learn, Joel S. Weissman, and Adil H. Haider, "Association Between Race and Postoperative Outcomes in a Universally Insured Population Versus Patients in the State of California," *Annals of Surgery* (2016), E-Publish Ahead of Print:1. doi:10.1097/SLA.0000000000001958.

Seabury, Seth A., Amitabh Chandra, Darius Lakdawalla, and Anupam B. Jena, "On Average, Physicians Spend Nearly 11 Percent of Their 40-Year Careers With an Open, Unresolved Malpractice Claim," *Health Affairs* 32(1) (2013), 111-19.

Sloan, Frank A., and John H. Shadle, "Is there empirical evidence for `Defensive Medicine'? A reassessment," *Journal of Health Economics* 28 (2009), 481-91.

Stock, James, and Motohiro Yogo, "Testing for Weak Instruments in Linear IV Regression," in *Identification and Inference for Econometric Models* (ed. Andrews, Cambridge University Press) (2005): 80-108.

Studdert, David M., Michelle M. Mello, William M. Sage, Catherine M. DesRoches, Jordon Peugh, Kinga Zapert, and Troyen Brennan, "Defensive medicine among high-risk specialist physicians in a volatile malpractice environment," JAMA 293(21) (2005), 2609-17.

Waxman, Daniel, Michael Greenberg, M. Susan Ridgely, Arthur Kellerman, and Paul Heaton. "The Effect of Malpractice Reform on Emergency Department Care," *New England Journal of Medicine* 371(16) (2014), 1518-25.

Zeiler, Kathryn, Charles Silver, Bernard Black, David Hyman and William Sage, "Physicians' Insurance Limits and Malpractice Payments: Evidence from Texas Closed Claims: 1990-2003." *Journal of Legal Studies* 36(2) (2007), S9-S45.

Zuckerman, Stephen, Randall Bovbjerg, and Frank Sloan, "Effects of tort reforms and other factors on medical malpractice insurance premiums," *Inquiry* 27(2) (1990), 167-82.

TABLE I

Covariate Balance Analysis in Full Inpatient Sample: Summary Statistics for Covariates by Patient Category

	(1)	*(2)*	*(3)*	*(4)*	*(5)*
	MEANS (STANDARD DEVIATIONS) OF INDICATED VARIABLES				
	ON-BASE		OFF-BASE		DIFFERENCE-IN-DIFFERENCE FOR INDICATED VARIABLE
	ACTIVE-DUTY	NON-ACTIVE DUTY	ACTIVE-DUTY	NON-ACTIVE-DUTY	
Panel A: Evaluating Covariate Balance by Individual Covariates					
Male	0.61 (0.49)	0.02 (0.14)	0.68 (0.47)	0.04 (0.10)	-0.06*** (0.02)
Charlson Comorbidity Score	0.13 (0.60)	0.17 (0.60)	0.22 (0.79)	0.22 (0.73)	-0.03*** (0.01)
(Omitted: Junior Enlisted)					
Senior Enlisted	0.27 (0.44)	0.40 (0.49)	0.36 (0.48)	0.46 (0.50)	-0.03*** (0.01)
Warrant Officer	0.01 (0.09)	0.01 (0.11)	0.01 (0.11)	0.02 (0.14)	0.00 (0.00)
Junior Officer	0.04 (0.19)	0.08 (0.27)	0.03 (0.17)	0.05 (0.22)	-0.02*** (0.00)
Senior Officer	0.04 (0.20)	0.06 (0.22)	0.06 (0.23)	0.09 (0.28)	0.01*** (0.00)
Pay-Grade Missing	0.27 (0.44)	0.18 (0.38)	0.40 (0.49)	0.27 (0.44)	-0.05*** (0.01)
(Omitted: Age < 30 Years Old)					
Age 30-40 Years Old	0.22 (0.42)	0.29 (0.45)	0.25 (0.44)	0.29 (0.45)	-0.03*** (0.01)
Age 40-50 Years Old	0.10 (0.31)	0.07 (0.26)	0.15 (0.35)	0.09 (0.28)	-0.03*** (0.01)
Age 50-60 Years Old	0.02 (0.15)	0.01 (0.09)	0.04 (0.19)	0.01 (0.11)	-0.01*** (0.00)
Panel B. Evaluating Covariate Balance Collectively					
Predicted Log Relative Weighted Product (based on regression of log of relative weighted product on covariates)	-0.29 (0.47)	-0.53 (0.35)	-0.26 (0.49)	-0.49 (0.38)	0.02 (0.02)

Notes: Standard errors in Column 5 are corrected for within-catchment area correlation in the error term (based on catchment areas as they are defined at the beginning of the sample, prior to any base closings) and are reported in parentheses. Pay-grade status is based on the officer/enlisted status of the household sponsor. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE II

Effects of Base Closures on Resident Population Counts, Resident Case Mix and Rates of Hospitalization

	(1)	(2)	(3)	(4)
	LOG NUMBER OF PEOPLE IN ORIGINAL CATCHMENT AREA (AGGREGATED SAMPLE OF MHS BENEFICIARIES)	LOG MEAN AGE OF MHS BENEFICIARY (AGGREGATED SAMPLE OF MHS BENEFICIARIES)	LOG PREDICTED RELATIVE WEIGHTED PRODUCT (INDIVIDUAL SAMPLE OF INPATIENT ADMISSIONS)	LIKELIHOOD OF HOSPITALIZATION WITHIN A YEAR (INDIVIDUAL SAMPLE OF MHS BENEFICIARIES), COEFFICIENT MULTIPLIED BY 100)
Active	0.69***	0.51***	0.21***	-0.71***
	(0.15)	(0.01)	(0.01)	(0.13)
Post-Closing	0.01	-0.01	-0.00	-0.31
	(0.03)	(0.01)	(0.01)	(0.23)
Active * Post-Closing	**-0.12*** **	**0.01**	**-0.00**	**0.00**
	(0.04)	**(0.01)**	**(0.01)**	**(0.29)**

Notes: robust standard errors corrected for within-original-catchment area correlation in the error term are reported in parentheses. Post-Closing equals 0 for all zip codes unaffected by a base closure (that is, all zip codes whose closest-MTF measure was not altered by a base closure) and 0 for years prior to the closure for those zip codes affected by a base closure. Post-Closing equals 1 for years after closure for those zip codes affected by a base closure. Original catchment areas signify geographic areas of beneficiary residence, where regions are grouped according to the MHS Catchment Area Number assigned to that region at the beginning of the sample. The regressions in Column 3 and 4 includes zip code and year fixed effects. The regressions in Columns 1 and 2 are aggregated to the catchment-year level and include original catchment area and year fixed effects; while drawing on information from the full MHS beneficiary file regardless of receipt of inpatient care. The regression in Column 4 is organized at the individual-beneficiary-by-year level. Regressions include regions unaffected by a base closure as a control. Regressions are also limited to regions that were within 40 miles of an MTF hospital as of the beginning of the sample period. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE III

Relationship between Medical Liability Immunity and Treatment Intensity (Relative Weighted Product, Logged)

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
MTF	-0.047***	-	-	0.008	-	-
	(0.004)			(0.025)		
Active Duty Patient	0.038***	0.036***	0.028***	0.028***	0.034***	0.026***
	(0.006)	(0.006)	(0.005)	(0.010)	(0.005)	(0.005)
Active Duty Patient X MTF	**-0.050*** **	**-0.054*** **	**-0.039*** **	**-0.050*** **	**-0.049*** **	**-0.035*** **
	(0.008)	**(0.008)**	**(0.006)**	**(0.009)**	**(0.007)**	**(0.007)**
Male	0.012***	0.011***	0.016***	0.011***	0.009***	0.016***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.011)
Charlson Comorbidity Score	0.067***	0.058***	0.059***	0.068***	0.058***	0.059***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
(Omitted: Junior Enlisted)						
Senior Enlisted	-0.007***	-0.006***	-0.006***	0.005	-0.005***	-0.006***
	(0.001)	(0.002)	(0.001)	(0.005)	(0.001)	(0.001)
Warrant Officer	-0.011***	-0.012***	-0.013***	0.003	-0.013***	-0.013***
	(0.003)	(0.004)	(0.003)	(0.007)	(0.004)	(0.003)
Junior Officer	-0.030***	-0.033***	-0.031***	-0.022***	-0.029***	-0.028***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Senior Officer	-0.029***	-0.037***	-0.033***	-0.011*	-0.029***	-0.028***
	(0.002)	(0.003)	(0.002)	(0.006)	(0.002)	(0.002)
(Estimated Coefficients for Age and Primary Diagnosis Dummies Not Listed)						
N	2027632	2026725	2020794	2022723	2021787	2015831
Hospital Fixed Effects	NO	YES	NO	NO	YES	NO
Physician Fixed Effects	NO	NO	YES	NO	NO	YES
Instrument for MTF & Active Duty Patient X MTF?	NO	NO	NO	YES	YES	YES
Zip code Fixed Effects?	NO	NO	NO	YES	YES	YES

Notes: robust standard errors corrected for within-catchment-area correlation in the error term are reported in parentheses (based on original catchment area designations). All regressions include year fixed effects and controls for patient age (dummies), sex, Charlson comorbidity and paygrade (dummies), along with primary diagnosis code dummies. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE IV

Relationship between Medical Liability Immunity and Various Alternative Treatment Intensity Metrics among Full Inpatient Sample

	(1)	*(2)*	*(3)*
	TOTAL BED DAYS (LOGGED)	INCIDENCE OF ANY PROCEDURE	NUMBER OF PROCEDURES (LOGGED)
Active Duty Patient	0.072***	0.011***	0.034***
	(0.010)	(0.004)	(0.012)
Active Duty Patient X MTF	**-0.053*** **	**-0.028*** **	**-0.052*** **
	(0.018)	**(0.009)**	**(0.019)**
N	2402313	2411597	1881708
Interaction Coefficient as a Fraction of Mean of Dependent Variable, For Non-Logged Dependent Variables	**-**	**-0.04**	**-**

Notes: robust standard errors corrected for within-catchment-area correlation in the error term are reported in parentheses (based on original catchment area designations). All regressions include year fixed effects, primary diagnosis code fixed effects, hospital fixed effects, zip-code fixed effects and controls for patient age (dummies), sex, Charlson comorbidity and paygrade (dummies). MTF and Active-Duty-Patient X MTF are instrumented by a series of dummy variables capturing different distance bins between a patient's residence and the closest MTF along with the interaction between such dummies and the active-duty-patient dummy. The coefficient of the MTF indicator is dropped due to the inclusion of provider fixed effects. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE V

Effect of Liability Immunity on Diagnostic-Procedure Utilization and Non-Diagnostic-Procedure Utilization, Separately Across Different Admission Types

	(1) Cardiac / Circulatory Admission	(2) Respiratory Admission	(3) Orthopedic Admission	(4) Gastrointestinal Admission	(5) Neuro-logical Admission	(6) Mental Health Admission	(7) Endocrine, Nutritional and Metabolic Admission	(8) Other Non-Childbirth Admission	(9) Pooled Sample: All Non-Childbirth Admissions
Panel A: Dependent Variable = Incidence of Appropriate Diagnostic Test for Indicated Admission Type									
Active	0.015*	0.031***	0.030***	-0.008	0.033***	0.004*	0.061***	0.027***	0.038***
	(0.008)	(0.013)	(0.007)	(0.008)	(0.009)	(0.002)	(0.014)	(0.004)	(0.003)
MTF * Active	-0.022*	-0.059***	-0.056***	-0.018*	-0.053**	-0.026*	-0.066***	-0.059***	-0.067***
	(0.013)	(0.022)	(0.015)	(0.011)	(0.020)	(0.015)	(0.018)	(0.007)	(0.005)
Interaction Coefficient as a Fraction of Mean of Dependent Variable	-0.03	-0.15	-0.16	-0.04	-0.09	-0.55	-0.67	-0.31	-0.22
Panel B: Dependent Variable = Incidence of Any Non-Diagnostic Procedure									
Active	0.004	0.003	0.019***	-0.001	0.008	-0.005	-0.145***	-0.008*	-0.005*
	(0.010)	(0.012)	(0.006)	(0.006)	(0.008)	(0.003)	(0.020)	(0.004)	(0.003)
MTF * Active	0.010	-0.027	0.026*	0.015*	0.007	0.033	0.009	0.005	0.003
	(0.017)	(0.025)	(0.015)	(0.008)	(0.016)	(0.025)	(0.030)	(0.010)	(0.008)
Interaction Coefficient as a Fraction of Mean of Dependent Variable	0.02	-0.07	0.03	0.03	0.02	0.30	0.01	0.08	0.01

Notes: robust standard errors corrected for within-catchment-area correlation in the error term are reported in parentheses (based on original catchment area designations). All regressions include year fixed effects, primary diagnosis code fixed effects, hospital fixed effects, zip-code fixed effects and controls for patient age (dummies), sex, Charlson comorbidity and paygrade (dummies). MTF and Active-Duty-Patient X MTF are instrumented by a series of dummy variables capturing different distance bins between a patient's residence and the closest MTF along with the interaction between such dummies and the active-duty-patient dummy. The coefficient of the MTF indicator is dropped due to the inclusion of hospital fixed effects. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE VI

Relationship between Medical Liability Immunity and Childbirth Delivery Treatment Intensity (Relative Weighted Product, Logged)

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
MTF	-0.008** (0.004)	-	-	0.015 (0.015)	-	-
Active Duty Patient	0.014*** (0.002)	0.016*** (0.002)	0.015*** (0.002)	0.014*** (0.002)	0.014*** (0.002)	0.014*** (0.001)
Active Duty Patient X MTF	**0.016*** (0.003)**	**0.010*** (0.002)**	**0.008*** (0.002)**	**0.008** (0.003)**	**0.010*** (0.002)**	**0.008*** (0.002)**
N	936391	936147	934762	930897	930614	929214
Hospital Fixed Effects	NO	YES	NO	NO	YES	NO
Physician Fixed Effects	NO	NO	YES	NO	NO	YES
Instrument for MTF & Active Duty Patient X MTF?	NO	NO	NO	YES	YES	YES
Zip code Fixed Effects?	NO	NO	NO	YES	YES	YES

Notes: robust standard errors corrected for within-catchment-area correlation in the error term are reported in parentheses (based on original catchment area designations). All regressions included year fixed effects and controls for patient age (dummies), Charlson comorbidity, paygrade (dummies) and various delivery risk factors, along with the primary diagnosis code (dummies). *Source*: 2003-2013 Military Health System Data Repository. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE VII

Relationship between Medical Liability Immunity and Various Alternative Treatment Intensity Metrics among Childbirth Delivery Sample

	(1)	*(2)*	*(3)*	*(4)*
	INCIDENCE OF CESAREAN DELIVERY	TOTAL BED DAYS (LOGGED)	NUMBER OF PROCEDURES (LOGGED)	INCIDENCE OF GREATER THAN 1 PROCEDURE
Active Duty Patient	0.025***	0.047***	-0.006*	-0.005*
	(0.002)	(0.003)	(0.003)	(0.002)
Active Duty Patient X MTF	**0.008****	**0.016*****	**0.024*****	**0.011*****
	(0.003)	**(0.005)**	**(0.005)**	**(0.004)**
N	1019225	1018538	1019225	1019225
Interaction Coefficient as a Fraction of Mean of Dependent Variable, For Non-Logged Dependent Variables	0.04	-	-	0.02

Notes: robust standard errors corrected for within-catchment-area correlation in the error term are reported in parentheses (based on original catchment area designations). All regressions include year fixed effects, primary diagnosis code fixed effects, hospital fixed effects, zip-code fixed effects and controls for patient age (dummies), Charlson comorbidity, paygrade (dummies) and various delivery risk factors. MTF and Active-Duty-Patient X MTF are instrumented by a series of dummy variables capturing different distance bins between a patient's residence and the closest MTF along with the interaction between such dummies and the active-duty-patient dummy. The coefficient of the MTF indicator is dropped due to the inclusion of provider fixed effects. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE VIII

Relationship between Medical Liability Immunity and Various Health Care Quality Metrics among Full Inpatient Sample

	(1)	*(2)*	*(3)*	*(4)*
	INCIDENCE OF MORTALITY WITHIN 90 DAYS OF DISCHARGE (COEFFICIENTS MULTIPLIED BY 100)	INCIDENCE OF MORTALITY WITHIN 1 YEAR OF DISCHARGE (COEFFICIENTS MULTIPLIED BY 100)	INCIDENCE OF UNPLANNED HOSPITAL READMISSION WITHIN 30 DAYS (COEFFICIENTS MULTIPLIED BY 100)	INCIDENCE OF PATIENT SAFETY INDICATOR (COEFFICIENTS MULTIPLIED BY 100)
Active Duty Patient	0.11***	0.07	-0.48***	0.13**
	(0.04)	(0.06)	(0.10)	(0.06)
Active Duty Patient X MTF	**-0.03**	**0.02**	**-0.35****	**0.06**
	(0.06)	**(0.10)**	**(0.15)**	**(0.08)**
95% Confidence Interval for Interaction Variable	[-0.16, 0.08]	[-0.17, 0.23]	[-0.65,-0.06]	[-0.10, 0.23]
95% Confidence Interval as Fraction of Mean of Dependent Variable	[-0.33, 0.17]	[-0.19, 0.26]	[-0.16, -0.01]	[-0.06, 0.13]
N	2195151	2195151	1996766	2324561

Notes: robust standard errors corrected for within-catchment-area correlation in the error term are reported in parentheses (based on original catchment area designations). All regressions include year fixed effects, primary diagnosis code fixed effects, hospital fixed effects, zip-code fixed effects and controls for patient age (dummies), sex, Charlson comorbidity and paygrade (dummies). MTF and Active-Duty-Patient X MTF are instrumented by a series of dummy variables capturing different distance bins between a patient's residence and the closest MTF along with the interaction between such dummies and the active-duty-patient dummy. The coefficient of the MTF indicator is dropped due to the inclusion of provider fixed effects. The mortality specifications exclude observations from 2013 to provide time to follow patients post discharge. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

TABLE IX

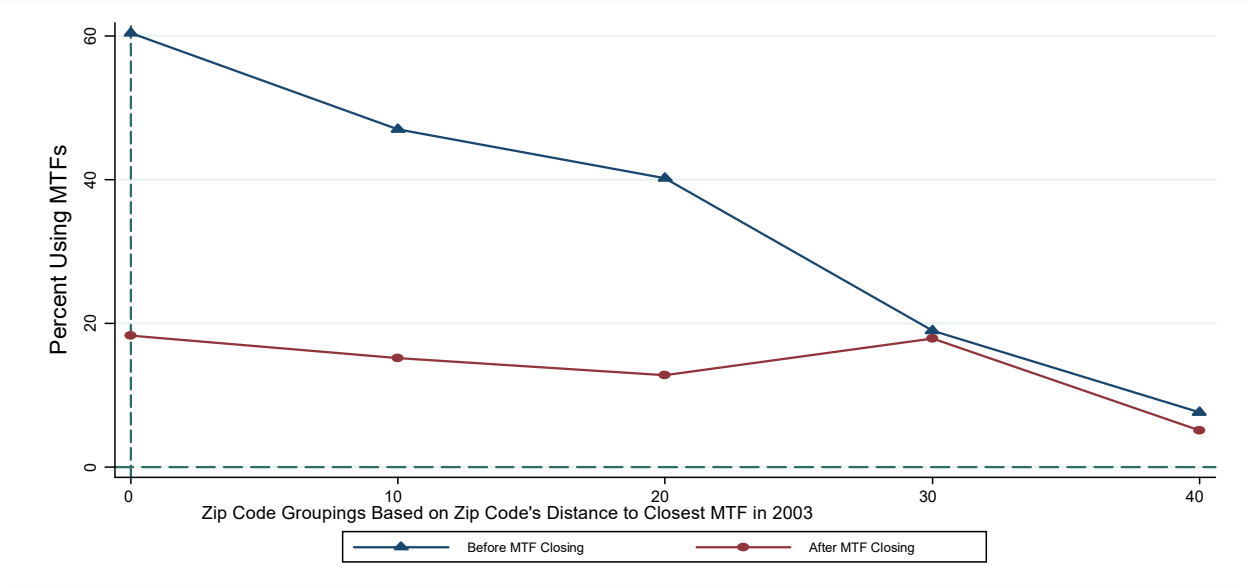
Relationship between Medical Liability Immunity and Treatment Intensity (Relative Weighted Product, Logged), Separately for States with and without Damages Caps

	(1)	*(2)*	
	STATE-YEAR CELLS WITHOUT CAPS (HIGHER LIABILITY PRESSURE STATES)	STATE-YEAR CELLS WITH CAPS (LOWER LIABILITY PRESSURE STATES)	ALL STATES
Active Duty Patient	0.035***	0.035***	0.035***
	(0.004)	(0.004)	(0.007)
Active Duty Patient X MTF	**-0.065*** **	**-0.044*** **	-0.065***
	(0.007)	**(0.005)**	(0.011)
Active Duty Patient X MTF X Incidence of Damages Cap	**-**	**-**	**0.021*** **(0.012)**
N	634491	1385258	2021787

Notes: robust standard errors corrected for within-catchment-area correlation in the error term are reported in parentheses. The estimated coefficients of the caps indicator and its interaction with MTF and with the active-duty patient indicator are not shown for purposes of brevity. All regressions include year fixed effects, primary diagnosis code fixed effects, hospital fixed effects, zip-code fixed effects and controls for patient age (dummies), sex, Charlson comorbidity and paygrade (dummies). MTF and Active-Duty-Patient X MTF are instrumented by a series of dummy variables capturing different distance bins between a patient's residence and the closest MTF along with the interaction between such dummies and the active-duty-patient dummy. The coefficient of the MTF indicator is dropped due to the inclusion of provider fixed effects. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

FIGURE I

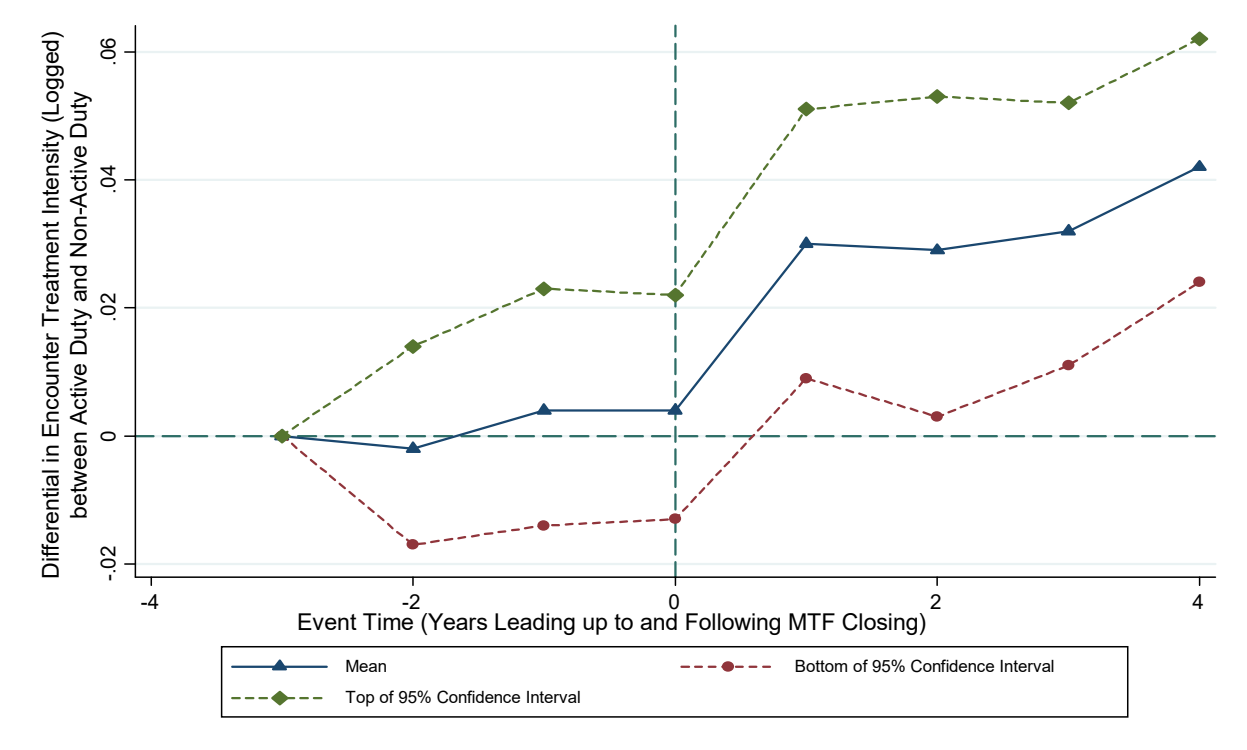
Effect of MTF Closings on Likelihoods of Using Any MTF for Inpatient Care in Regions Affected by Base Closings



Notes: the top line focuses on those encounters in zip codes affected by a base-hospital closing in the period of time prior to the closing. The bottom line focuses on those encounters in zip codes affected by a base-hospital closure in the period of time following the closing. In each case, the indicated point represents the percent of encounters that occur at an MTF as opposed to a civilian hospital, where this percentage is calculated for each of the following groups of zip codes based on their distance to the closest MTF at the beginning of the sample (i.e., in 2003): (1) 0-10 Miles (represented by 0 in the graph), (2) 10-20 Miles (represented by 10 in the graph), (3) 20-30 Miles (represented by 20 in the graph), (4) 30-40 Miles (represented by 30 in the graph), and (5) 40+ Miles (represented by 40 in the graph). Source: 2003-2013 Military Health System Data Repository.

FIGURE II

Differential Effect of MTF Closings on Relative Weighted Product (Logged) for Active Duty Relative to Non-Active Duty: Event-Study Analysis



Notes: this figure plots coefficients from a regression that extends the specification estimated in Column 4 of Table A2 to include leads and lags of the closing variable and of the interaction between closing and active-duty-patient status (where the relevant lead variable equals 1 in the indicated period of time and 0 in all other periods). The points in the graph represent the coefficients of the set of leads and logs of the interaction variables. The period of time prior to 3 years before a closing is the reference period. The last time period represents all years beyond 3 years after the time of closing. Source: 2003-2013 Military Health System Data Repository.