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Defensive Medicine and Obstetric Practices: Evidence from the Military Health System

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We estimate the extent of defensive medicine by physicians during labor and delivery, drawing on a novel and significant source of variation in liability pressure. In particular, we embrace the no-liability counterfactual made possible by the structure of liability rules in the Military Heath System. Active-duty patients seeking treatment from military facilities cannot sue for harms resulting from negligent 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 addressing endogeneity in the choice of treatment location by estimating mother fixed-effects specifications and by exploiting exogenous shocks to care location choices stemming from base-hospital closures, we find suggestive evidence that liability immunity increases cesarean utilization and treatment intensity during childbirth with no measurable negative effect on patient outcomes.

While the medical liability system is generally premised on satisfying the objectives of compensation, corrective justice and/or deterrence, much of the attention paid by scholars and policymakers to the medical malpractice system has focused on an unintended, but potentially

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critical side effect of this system: “defensive medicine.” Defensive medicine can perhaps best be seen as deterrence gone awry (Mello and Brennan 2002), capturing situations in which fear of medical liability causes physicians to practice sub-optimal care—that is, either providing too much care (“positive” defensive medicine) or too little care (“negative” defensive medicine). Both such responses are alarming from a policy and welfare point of view. Negative defensive medicine implicates concerns over patient access to care, while positive defensive medicine implicates concerns over unnecessary health care spending (to a potentially substantial degree). Whether liability causes positive or negative defensive medicine or no defensive medicine at all is likely to depend on a number of contextual factors. Arguably foremost among those factors is the clinical setting itself. In this paper, we will revisit the topic of defensive medicine in a clinical setting that has been the most frequent target of the defensive medicine literature and that represents the most frequent reason for hospitalization in the U.S.: childbirth.

Childbirth provides an especially powerful setting by which to explore the mechanics of defensive medicine given that obstetrics / gynecology represents one of 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. Though the topic of defensive medicine in obstetric practices is well-trodden territory in the academic literature, there remains both theoretical and empirical ambiguity surrounding the question of whether, to what extent, and in what direction, physicians practice defensively during labor and delivery. In this paper, we approach this inquiry while bringing to bear a rich and novel source of data and a powerful new estimation strategy. For these purposes, we build upon our recent efforts (Frakes and Gruber 2019) and turn our focus to the Military Health System (MHS).

The crux of our methodological approach is the *Feres*-doctrine, a well-known and highly controversial rule which prohibits active-duty patients receiving medical treatment from military facilities from having any recourse should they suffer harm from negligent care—i.e., they can sue neither the government nor the treating physician in such circumstances. Importantly, however, liability recourse is available to dependents and retirees treated at military facilities and to all patients—active-duty or not—that receive care from civilian facilities. Variation in when *Feres* does and does not apply affords us the opportunity to construct a powerful methodological tool absent from traditional defensive-medicine investigations: a treatment group of patients that lack access to the medical liability system. That is, by comparing those patients over which physicians are not subject to liability pressure to other patients over which physicians are subject to such pressure, we can identify the full impact of liability forces on treatment utilization and patient outcomes. The existing literature, in contrast, has relied upon more limited forms of variation—e.g., observing differential behavior in situations whereby patients do and do not have the ability to collect pain and suffering damages above some specified cap. Not only may those studies drawing on damages caps and related tort reforms fail to inform on the full extent of liability's influence on behavior, but they also raise potential methodological concerns to the extent that there remains some ambiguity regarding the extent to which a physician's perception of liability risk varies in the presence of such limited reforms (Carrier et al. 2010).

In more specific terms, to capture a liability-immunity treatment effect, we follow Frakes and Gruber (2019)—which had embraced this approach on the full inpatient sample—and estimate a difference-in-difference design in which we compare the care received by active-duty versus non-active duty mothers delivering on the base versus off the base. To address endogeneity in the choice of on-base versus off-base care while estimating this difference-in-difference design, we draw 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. Moreover, to further address endogeneity concerns, we expand upon Frakes and Gruber (2019) and take advantage of the fact that many patients in this childbirth subsample have repeated hospitalizations over the sample period of the same clinical nature. This fact allows us to include mother fixed effects in many of our empirical specifications.

To execute this design, we rely upon a unique set of data on births: the Military Health System Data Repository (MDR). Covering 2003 to 2013, these data provide incident-level claims records of inpatient stays for beneficiaries of the MHS, including stays at both military and civilian hospitals and for active duty beneficiaries and non-active-duty beneficiaries. Using these records, we formulate a number of health care spending and utilization metrics, in addition to quality metrics.

In our previous work (Frakes and Gruber 2019), we found evidence suggesting that immunity from liability is associated with a roughly 5 percent *reduction* in inpatient treatment intensity. These previous findings are consistent with the conventional wisdom that providers practice "positive" defensive medicine—i.e., that liability pressures contribute to elevated spending. In the present paper, however, consistent with the findings of Currie and MacLeod (2008), we find evidence suggesting that immunity from liability is associated with a modest *increase* in treatment intensity in the childbirth setting, including a 4 percent increase (relative to the mean) in the rate of cesarean delivery. If anything, these results are contrary to the lay perception that liability fears cause physicians to perform additional cesarean deliveries. Moreover, consistent with a defensive—i.e., suboptimal—interpretation of these treatment-intensity findings, we do not find any evidence suggesting that immunity from liability is associated with a change in patient outcomes. However, we stress that our estimates are imprecise in the case of some of the health outcome metrics explored.

Our paper proceeds as follows. Part I reviews the existing literature on defensive medicine in obstetric practices, and describes the variation in medical liability rules pertaining the military population, which forms the basis for our identification strategy. Part II discusses our data, while Part III sets forth our empirical strategy. Part IV presents the results of our analysis. Finally, Part V concludes.

I. BACKGROUND

A. Defensive Medicine Background and Literature Review

Analysts frequently attribute defensive medicine to uncertainties in the minds of physicians as to what care standards are expected of them under the law (Frakes 2015; Craswell and Calfee 1986). Consider a physician deciding whether to perform a particular procedure in order treat a particular condition, but assume that the physician is uncertain as to whether the courts will expect that she perform the procedure in question. In the face of this uncertainty over the prevailing negligence standard governing the situation at hand, the physician may compare two scenarios under which courts may deem her negligent. (1) a situation in which she does not perform the procedure but where the courts will judge that she should have—i.e., an error of omission and (2) a situation in which she does perform the procedure but where the court judges she should not have or where the court determines that she executed the procedure without care—i.e., an error of commission. Positive defensive medicine arises when physicians, in thinking about the risks posed by the marginal patient, deem that the omission risks exceed commission risks (Currie and

MacLeod 2008), whereas negative defensive medicine arises when commission risks prevail on the margin.

Lay commentary and academic scholarship discussing the concept of defensive medicine have tended to focus on errors of omission and thus on positive defensive medicine. While the broader empirical literature on defensive medicine—i.e., not simply confined to childbirth—has produced mixed results regarding the existence and sign of defensive medicine, the tendency has been to find evidence of modest positive defensive medicine. Arguably the seminal paper here is Kessler and McClellan (1996), along with its companion paper (Kessler and McClellan 2002), which found that tort reforms that directly reduce liability pressure—including damage caps—lead to reductions of 4 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 being that stronger liability pressure contributes to greater health care utilization and spending.

A number of subsequent studies have supported the conclusions of Kessler and McClellan (1996, 2002). For instance, in recent work similarly drawing on variations in the application of the *Feres* doctrine, Frakes and Gruber (2019) found evidence suggesting that immunity from liability is associated with a roughly 5 percent reduction in treatment intensity during all inpatient stays. Other studies finding evidence of positive defensive medicine—in either cardiac spending or health care spending more broadly—include Avraham and Schanzenbach (2015) (drawing on adoptions of damage caps and related tort reforms); Avraham, Dafny and Schanzenbach (2012) (likewise drawing on damage cap adoptions and related reforms); Baicker, Fisher and Chandra (2007) (drawing on variation in insurance premiums), and Lakdawalla and Seabury (2011) (drawing on variations in the generosity of local juries). A range of other studies, however, have failed to find evidence of any change in health care utilization or spending in connection with variations in medical liability pressure. Included in this latter set of studies is Sloan and Shadle (2009); the Congressional Budget Office (2004), Paik, Black, and Hyman (2017); and Moghtaderi, Farmer and Black (2017) (in each case drawing on damages caps and related reforms).

Childbirth is one context in which malpractice scholars have theorized that physicians may capitulate to concerns over errors of commission and thus practice negative defensive medicine on the margin. Currie and MacLeod (2008), for instance, suggest that non-liability reasons behind cesarean utilization—e.g., financial motivations, convenience, etc.—may be so substantial that the marginal mother receiving cesarean delivery may carry few risk factors that would otherwise suggest a need for cesarean delivery.[1] In the face of this relatively healthy marginal mother, therefore, the prevailing liability risks may indeed be one in which the risks of committing the cesarean exceed the risks associated with vaginal delivery. Capitulating to these procedure-commission risks, physicians may avoid cesarean deliveries on the margin in the shadow of significant liability pressure. In support of these negative defensive-medicine predictions, Currie and MacLeod (2008) estimate a 1.2 percentage-point *increase* in cesarean rates following non-economic damage-cap adoptions, implying that liability forces compel *fewer* cesareans on the margin. Bertoli and Grembi (2017) find similar results when studying the effect of damages schedules in Italy.

Frakes (2012) raises a concern over Currie and MacLeod's analysis in that they draw upon a limited number of non-economic damages cap adoptions given their 1989-2001 timeframe. Using inpatient data from the National Hospital Discharge Survey and identifying off a larger number of cap adoptions, Frakes (2012) estimates a relationship between cap adoptions and cesarean rates that is relatively tightly bound around zero. Various other studies are consistent with Frakes (2012) in documenting no relationship between liability pressure and treatment intensity during childbirth, including Baldwin et al. (1995) and Sloan et al. (1997) Other studies, however, find evidence more consistent with the conventional lay-perception that higher liability pressure is associated with more (though not always substantially more) cesarean deliveries, including Localio et al. (1993), Dubay et al. 1999, Dranove and Watanabe (2010), and Grant and Mcinnes (2004).

Of course, the concept of defensive medicine rests on sub-optimal responses to liability—that is, defensive medicine captures situations in which liability pressure causes physicians to do more or less treatments but without any improvements in patient outcomes. Accordingly, critical to a defensive medicine analysis is also an exploration of the link between health outcomes and liability pressures. Similar to the utilization-based findings, the literature has produced mixed results on this front, as we now summarize, focusing for brevity purposes on the childbirth context. Most studies find little to no impact of malpractice reforms on infant mortality rates, infant Apgar

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[1] Underlying this observation is also a model of patient triage, where physicians will first deliver via cesarean section to those mothers in greatest need of cesarean delivery and will subsequently make the cesarean decision on the mother with the next most serious set of risk factors. After considering true clinical motivations, financial motivations, and convenience-based motivations (due to the scheduling benefits of cesarean delivery), physicians may have reached the point where the marginal cesarean decision is deep into this triage process and thus in arguably little need of cesarean delivery given her risk factors.

scores, or on other birth outcomes (Klick and Stratmann 2007, Frakes 2012, Dubay et al. 1999, Yang et al. 2009, and, across most of their specifications, Sloan et al. 1995). Currie and MacLeod (2008) do find that damage caps increase preventable complications of labor and delivery. Iizuka (2013) finds similar outcomes in the case of collateral source rule reforms and punitive damages caps, but not in the case of non-economic damages caps. Frakes and Jena (2016), on the other hand, find little relationship between damage caps and preventable delivery complications. Zabinski and Black (2019) find higher rates of birth-related Patient Safety Indicators following damage cap adoptions.

B. Background: The Military Health System

In our recent work (Frakes and Gruber 2019), we provide a background on the Military Health System (MHS) and on the relevant variations in medical liability rules pertaining to beneficiaries of this system. In this subsection, we simply summarize the key points from this discussion, directing readers to our prior paper for more details.

To begin, the MHS is the primary insurer for all active duty military, their dependents and retirees through the TriCare program. For MHS beneficiaries, 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 at civilian facilities (purchased care). Whether patients receive care on the base versus off the base is largely a function of where patients live insofar as those living close to the base are expected to receive care at military facilities and those living far from the base are expected to receive care off the base. We demonstrate this distance gradient below.

At the heart of our estimation strategy is the comparison in the care delivered during medical encounters when patients do and do not have medical liability recourse. The starting point for this approach derives from *Feres v. United States*, 340 U.S. 135 (1950). Under *Feres* and subsequent cases, the 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 and against the providing physicians that arise from negligent medical treatment of active duty service members. Accordingly, the key treatment group for our analysis—where treatment captures immunity from liability—is represented by the active-duty military personnel receiving care from military facilities. Under *Feres* and subsequent cases, however, liability recourse for negligently provided care is available to the following groups of MHS beneficiaries, who will serve as the key control groups in our analysis: (1) non-active duty personnel receiving care from military providers, and (2) all patients—whether active duty or non-active duty—receiving care from civilian facilities.[2]

In Part III below, we provide additional detail regarding how we compare the care provide to beneficiaries in the treatment and control groups in order to illuminate the effects of immunity from liability, following Frakes and Gruber (2019). We note at the outset, however, that we also expand upon Frakes and Gruber (2019) in the present analysis by estimating specifications with mother fixed effects, such that we do not simply make comparisons *across* individuals falling into these treatment and control categories, but instead also compare the care provided *within* the same individual over time as they fall into both the treatment and control groups at different points.[3]

Finally, for these variations in the application of the *Feres* doctrine to be relevant determinants of provider behavior, providers must naturally be aware of such rules. As reported in our prior work, we verified through an informal survey at the beginning of our project (prior to data collection) that military physicians are well aware of the *Feres* doctrine and implications.

II. DATA

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 and with various data fields for each encounter, including those bearing on the associated diagnosis and procedure codes, length of the inpatient stay, and other treatment intensity metrics discussed below. The records cover 2003 to 2013. Importantly, these records include details on deliveries at Military Treatment Facilities (MTFs) and at civilian hospitals.

Our focus is on the child-birth-related inpatient records from the MDR. The obstetrics sample covers 1,016,606 deliveries, 44% of which occurred at MTFs and 56% of which occurred

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[2] One may question how the law views the immunity status of the child being delivered by an active duty mother on the base. The mother has no liability recourse should she be harmed by negligent care. But, does the child have liability recourse considering that the child is not technically "active-duty"? The dominant legal treatment of this situation is indeed to nonetheless extend immunity status to cover harm to either the mother or the child so long as the child is still in-utero when the harm occurs, as the harm to the child in such instances is deemed derivative of some harm imposed on the active-duty mother. *Ortiz v. United States Evans Army Cmty. Hosp.*, 786 F.3d 817 (10th Cir. 2015)

[3] In Frakes and Gruber (2019), we did not estimate individual fixed effects specifications throughout given the low rate by which beneficiaries across the full inpatient sample are hospitalized more than once during the sample period and given that those repeat hospitalizations will often be for different medical conditions, creating an arguably awkward within-individual analysis. Nonetheless, in our prior work, we did include a footnote in which we reported the results of a beneficiary fixed effects specification and demonstrate the robustness of our key findings to this approach.

at civilian hospitals. Out of the MTF childbirth encounters, 24% are to active duty mothers, while 76% are to non-active-duty mothers. Out of the civilian encounters, 10% are to active-duty mothers while 90% are to non-active-duty mothers.

While all MHS beneficiaries are covered under the system, there is some variability in the insurance plans that beneficiaries hold. 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, we limit the analysis to those beneficiaries with TRICARE Prime coverage. Some beneficiaries in the birth sample are on "retirement status" for purposes of their MHS eligibility. We exclude such beneficiaries from the sample given that retirees face differential cost sharing when being treated on the base versus off the base, which would otherwise confound our difference-in-difference framework. Finally, to ensure age overlap between our active-duty and non-activity-duty comparison groups, we limit our specifications to those over 18 years of age.

B. Dependent Variables

Treatment Intensity Metrics

Consistent with Frakes and Gruber (2019), our primary measure of interest is called the Relative Weighted Product or RWP, a Department of Defense-derived measure of treatment intensity assigned to each inpatient encounter.[4] Designed to be comparable across the direct care and purchased care inpatient records, the RWP metric is derived after assigning a Diagnosis Related Group (DRG) weight to the encounter and thereafter adjusting that weight based on the length of stay of the encounter. Given the length of stay adjustment and given that 55% of the variation across DRGs is explained by treatment intensity (McClellan, 1997), this measure is informative of the relative costliness of the respective encounter.

We also use as outcome measures certain alternative treatment metrics. Foremost among these alternatives is the metric of strongest interest to the childbirth literature to date: the incidence of a cesarean delivery. With this cesarean specification, we are effectively modeling the decision between a costly and intensive approach to delivery—i.e., via cesarean section—and a less-costly, less-intensive approach—i.e., vaginal delivery. In yet other alternatives, we use the following

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[4] 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.

treatment intensity metrics: (1) number of inpatient bed days and (2) number of procedures performed.

Health Care Quality Metrics

Critical to our defensive medicine analysis is an estimation of the relationship between liability pressures and health care quality. For these purposes, we construct a number of quality indicators specifically tailored for the labor and delivery context: (1) the incidence of neonatal mortality—i.e., mortality within the newborn's first 28 days, (2) incidence of neonatal birth trauma, following the relevant Patient Safety Indicators (PSI) promulgated by the Agency for Health Care Research and Quality (AHRQ)—e.g., incidence of subdural and cerebral hemorrhage to neonate, (3) incidence of maternal trauma, likewise following the relevant PSI promulgated by AHRQ—e.g., third degree laceration, and (4) the incidence of any one of the following delivery complications: fetal distress, excessive bleeding, precipitous labor, prolonged labor or dysfunctional labor. We select this latter list of complications as potential outcomes of interest following Currie and MacLeod (2008), which identified them as "preventable" in nature. We acknowledge, however, that it may be difficult to attribute any null effects respecting these complication markers to weak liability effects or to the possibility that such complications are not strongly preventable in the first place. Table 1 provides summary statistics for the key treatment intensity and health outcome metrics.

III. EMPIRICAL STRATEGY

The core of our methodological strategy draws upon the variations in medical liability rules discussed in Part I.B above, whereby active duty patients treated at the base have no liability recourse, while other patient types—e.g., active duty treated off the base and non-active duty treated at either location—do have medical liability recourse. These variations motivate the following difference-in-difference specification, estimated using the sample of childbirths in the inpatient MDR records discussed above:

$$(1)\ Y_{ije} = \alpha + \theta ACTIVE\_DUTY_{ie} + \gamma ON\text{-}BASE_{je} + \beta ACTIVE\_DUTY_{ie}\, X\ ON\text{-}BASE_{je} + \delta \mathbf{X_{ie}}$$

$$\{+\ \mu_j\}\ \{+\ \lambda_k\}\ \{+\ \eta_i\} + \varepsilon$$

Where i denotes individual patients / mothers, j denotes providers,[5] e denotes childbirth episodes, and k denotes zip-codes (based on patient residence). Y is some measure of patient treatment—as above, we will estimate specifications covering a range of treatment intensity and health care quality metrics. ACTIVE_DUTY is a dummy variable for whether the individual is active duty at the time of the delivery; ON-BASE is a dummy variable for whether the delivery occurs at a base hospital; **X** is a set of individual-episode specific controls (age dummies, year dummies, pay-grade-level dummies, Charlson comorbidity scores for the patient at the time of the delivery and indicator variables for the incidence of the following non-preventable delivery complications: previous cesarean delivery, multiple births, breech presentation, cephalo-pelvic disproportion, placenta previa, abruptio placentae, and cord prolapse.[6] One of the advantages of investigating a specific clinical context such as childbirth—relative to the broad-based inpatient inquiry in Frakes and Gruber (2019)—is the ability to control for risk factors of this nature that are more specifically tailored to the precise clinical context, thereby enhancing the ability to ensure comparability between those patients in the relevant treatment and control groups.[7]

Though the difference-in-difference specification set forth in equation (1) captures a straightforward treatment of the variation in the malpractice environment facing mothers in the MHS, this approach does raise various potential empirical concerns. Frakes and Gruber (2019) sets forth a more thorough treatment of these concerns and of our relevant methodological responses. Rather than revisiting this entire analysis in full depth, this section will summarize these concerns and methodological responses, while tailoring this discussion to the childbirth-specific context at focus in this paper.

A. Addressing Patient Case Mix Concerns

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[5] In our analyses, we will consider separately hospital fixed effects and physician fixed effects, though in some specifications, we include both together.

[6] Following Currie and MacLeod (2008), we view these complications as covariates and not as outcome measures. Previous cesarean delivery, multiple deliveries and breech presentation are especially strong determinants of cesarean deliveries. In the primary specifications, we control for the incidence of these factors. In alternative, unreported specifications (with full results available upon request), we estimate the incidence of "primary" cesarean deliveries, as that term is specified by the Agency for Health Care Research and Quality. Primary cesarean deliveries are meant to reflect more discretionary cesareans and simply exclude mothers with these three key determinants from the analysis altogether. If anything, our results are only larger in magnitude when taking this alternative approach. For instance, in our baseline difference in difference specification (Column 1 of Table 4), we estimate a coefficient of the key interaction variable of 0.019. When focusing on the "primary" cesarean delivery specification, we estimate a coefficient of the difference-in-difference interaction variable of 0.018. In percent terms, this effect is larger considering a baseline primary cesarean rate of 0.16 and an overall cesarean rate of 0.27.

[7] 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. 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.

To begin, even though care location—on-base versus off-base—is predominantly a function of distance from the base, one may be concerned with endogeneity over patient choice in whether to go on-base or off-base for delivery, where such choice may be associated with unobservable characteristics of the relevant mother's health status. To provide a preliminary assessment of this concern, Table 2 shows the degree of covariate balance in our difference-in-difference approach, focusing on our key covariates. There is clearly a lack of balance along some of the observables individually. In Panel B, we present a more omnibus covariate balance analysis that attempts to explore balance in a collective sense. 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. The first row of Panel B demonstrates that, through our DD framework, our treatment group is predicted to have RWP levels that are 1% lower, which at least biases *against* our ultimate conclusion. In any event, we will begin to account for these concerns by controlling for all of these covariates in the regression framework.

We can further address these concerns with two additional approaches. First, we include mother fixed effects, η_i, such that we identify the above specification based on mothers who deliver more than once and that either deliver both on and off the base at least once or that deliver at least once while on active-duty status and once while on non-active-duty status. Roughly 29 percent of mothers in the sample deliver more than once during the sample, with 9 percent of the mothers in the sample doing so at least once on the base and once off the base and with roughly 1.5 percent of the mothers in the sample doing so at least once while on active-duty status and once while on non-active duty status. As demonstrated by the second row of Panel B of Table 2, covariate balance improves with this addition of mother effects—that is, when we estimate the difference-in-difference specification on a log RWP measure predicted based on the covariates, we find no relationship between treatment status and this aggregate reflection of the covariates.

In addition, we attempt to address endogeneity in the choice of on-base care by using an important quasi-experiment that took place during our sample period: the closing of 11 MTF hospitals pursuant to decisions by the 1995 and 2005 Base Realignment and Closure (BRAC) Commissions (after recommendations from the Pentagon). Altogether, these closings caused nearly 20% of the sample to experience a change in the distance to the closest MTF.[8] Importantly, while the MTF hospitals themselves closed, each of the bases affected by these BRAC-induced MTF closings continued to operate 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. Rather, as we discuss below the key effect of these closings is to change where mothers receive their deliveries—on the base versus off. Moreover, even if the BRAC events do cause some change in patient residence choice, Frakes and Gruber (2019) demonstrate that there is no change in the compositional health mix of the active duty relative to the non-active duty that continue to live near the base following the BRAC event.

Again, the key effect of the MTF closing is in driving those mothers who live close to a base from using an MTF for delivery to using a civilian hospital for delivery—i.e., a plausibly exogenous shift in the access of patients to inpatient care on the base. We illustrate this access effect in Figure 1. In this figure, we focus only on those zip codes that are affected by a base closure—i.e., that experienced a base-closure induced change in the distance to the closest MTF—and then demonstrate how the likelihood that a delivery occurs at a base hospital varies as a function of the distance between the base hospital and the patient's location of residence. The top line shows the relevant distance gradient for the period of time prior to the base closing. As is evident, this gradient is steep, with a strong drop off in MTF usage as the distance from the base increases. In the period of time following the base closing, these zip codes effectively become more alike—that is, they all tend to be distant from the closest MTF (with a few exceptions where the next nearest MTF hospital after a closing was roughly 20 miles away). As such, following an MTF closing, we would expect that the MTF-usage gradient will roughly flatten and will do so at a relatively low rate of MTF usage. The bottom line of Figure I is consistent with these expectations.

The variation afforded by both the distance gradient in MTF usage and the change in that gradient driven by base hospital closures motivates an instrumental variables solution to the concern over the endogeneity of the choice to deliver on the base. That is, to estimate the effect of our treatment status of interest—active duty delivering on the base—we incorporate this distance-related variation directly into equation (1) by forming a series of binary instruments

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[8] Frakes and Gruber (2019) list the affected hospitals. We note that 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.

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 distance bins and the interactions of each distance bin with the ACTIVE_DUTY indicator. With this baseline IV estimate, we address endogeneity in MTF choice by drawing on variation in the distance between the relevant patient's residence and the location of the closest MTF. Of course, this specification does not fully resolve concerns that unobservable characteristics of patients may bear on how close to the base MTF they initially decide to live. Accordingly, we proceed to estimate a variant of this IV specification where we include a set of zip code fixed effects, λ_k, such that 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 (that is, we achieve identification from the flattening out of this distance gradient).[9] While patients may have some degree of choice over whether to reside close to an MTF, they have less choice over the BRAC's decision to remove that MTF (and thus less choice over a within-zip-code change in the distance to the closest MTF). We implement this IV approach via two-stage least squares, where the first stages are represented by:

$$(2)\ \mathit{ON\text{-}BASE}_{ije} = \alpha + \theta_1\mathit{ACTIVE\_DUTY}_{ie} + \delta_1\mathbf{X}_{ie} + \eta_1\mathbf{DIST}_{ie} +$$

$$\phi_1\,\mathbf{DIST}_{ie}\ X\ \mathit{ACTIVE\_DUTY}_{ie}\ \ \{+ \mu_j\}\ \{+ \lambda_k\}\ \{+ \eta_i\} + \varepsilon$$

$$(3)\ \ \mathit{ON\text{-}BASE}_{ije}\ X\ \mathit{ACTIVE\_DUTY}_{ie} = \alpha + \theta_2\mathit{ACTIVE\_DUTY}_{ie} + \delta_2\mathbf{X}_{ie} + \eta_2\mathbf{DIST}_{ie} +$$

$$\phi_2\,\mathbf{DIST}_{ie}\ X\ \mathit{ACTIVE\_DUTY}_{ie}\ \ \{+ \pi_{j\}}\ \{+ \rho_j\}\ [+ \Delta_i\} + \varepsilon$$

where $\mathbf{DIST}_{ie}$ represents a series of dummy variables capturing the distance bins identified above. Equation (1) above then captures the second stage of this 2SLS analysis. While Figure 1 graphically depicts the essence behind these first stage regressions, we formally present these first

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[9] Within zip-codes, changes over time in the distance between the zip-code centroid and the closest MTF are driven only by MTF closings. Hence, the essence of our base-hospital-closure IV strategy is to draw on variations over time within-zip-codes in the distance to the closest MTF. This approach also flexibly accounts for the fact that not all base-hospital closures are alike. Some leave the next closest MTF very far away, whereas others leave the next closest MTF within driving distance. This approach allows us to draw on base hospital closures as a shock to patient access to on-base care, while parametrizing those closures in a way reflective of their true impact on this access (based on how close they leave the next option).

stage results in Table 3. As demonstrated by the reported F-statistics, MTF utilization and its interaction with active-duty status are strongly determined by these distance-based instruments.

The estimated 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. Furthermore, we also estimate IV specifications of this nature that include mother fixed effects to account for fixed differences across individual mothers. On a final note, as demonstrated by the last row of Table 2, covariate balance also improves through the estimation of this IV specification with mother fixed effects (as expected). When estimating this specification, we find a near-zero relationship between treatment status and predicted treatment intensity (with predictions based off the full set of covariates).

B. *Addressing Provider Selection Concerns*

The second concern with our motivating difference-in-difference design is that the medical providers serving the treatment group may differ from those serving the various control groups. We address this concern by adding provider fixed effects (μ_j) in some specifications, doing so separately for physicians and hospitals across alternative specifications (with a particular focus on hospital effects, given certain limitations with the physician data available to us).[10]. In this light, we compare active-duty and non-active-duty patients treated by the same provider.

Though allowing us to confront certain omitted variables concerns, this approach does raise some interpretation difficulties if the immunities from liability surrounding the treatment group "spillover" and affect the care provided to the non-immune patients. Consider, for instance, a situation, in which liability fears arising from the non-immune patient group contribute to a physician's overall "defensive" practice style, but where she consistently practices this style in the face of both immune and non-immune patients. In the face of liability-effect spillovers of this nature, the inclusion of provider-specific effects may attenuate the estimated treatment effects.

As we will demonstrate when presenting the results below, we find only a minor impact, if any, to our point estimates when including provider effects, which either suggests that both

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[10] There are two key limitations with the physician fixed effects approach: (1) we are assigning physicians 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.

provider selection and spillovers are moderately inconsequential phenomena or that they operate in different directions and thus roughly offset each other. In the Appendix, we attempt to shed light on this uncertainty by testing for the presence of spillovers of this nature. Though these tests are subject to certain methodological challenges—as we discuss in the Appendix—this analysis finds no evidence of treatment spillovers to the non-active-duty patients; however, we cannot confidently rule out the existence of spillovers. In any event, this is not detrimental to our analysis in that, if treatment spillovers do exist, the results from the provided fixed effects specifications presented below can simply be seen as a lower bound for the full extent of defensive practices exhibited by physicians (since spillover effects would simply attenuate the presented treatment effects).

C. Addressing Mechanism Concerns

The final concern we face is that the relationship that we identify—the differential treatment of active-duty patients versus non-active-duty patients on the base versus off—reflects more than just differences in the availability of liability recourse, a concern that is not fully ameliorated by the base closure and mother-fixed-effects approaches. Frakes and Gruber (2019) discuss several possible threats of this nature—e.g., (1) perhaps the Department of Defense places pressure on MTFs to treat active duty patients non-intensively in order to encourage their more immediate return to work or (2) perhaps active-duty patients themselves are reluctant to appear weak in front of their own—i.e., military providers—and my thus refuse recommended interventions.[11]

To begin this discussion, it is important to note that most plausible non-liability mechanisms that might explain a differential level of care for the treatment group—such as the two possible mechanisms identified above—are stories that suggest a possible *lower* level of care intensity for the treatment group (active duty on the base). Accordingly, these concerns are arguably weaker in the present paper given that our findings cut in the opposite direction—that is, we find a higher level of care intensity for the treatment group.

Second, we note that Frakes and Gruber (2019) offered certain additional empirical tests to support a liability interpretation of the estimated treatment effects. Included among these

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[11] Frakes and Gruber (2019) also address certain related concerns—e.g., perhaps the way in which on base providers differentially treat healthy and unhealthy patients differs from the way in which off-base providers differentially treat healthy and unhealthy patients, a concern that may be relevant for our population in that the active duty group is generally healthier than the non-active-duty.

additional tests was an exercise in which Frakes and Gruber separately estimate the relationship between treatment status and health care utilization for diagnostic procedures and for therapeutic, non-diagnostic procedures. They predict a stronger degree of positive defensive medicine in the case of diagnostic procedures—i.e., they predict that liability pressures cause more diagnostic screens and thus that immunity from liability causes fewer screens. Underlying this prediction is an acknowledgement that the prevailing liability concerns in the face of diagnostic tests are likely to be those over errors of omission as opposed to errors of commission (considering that there are few liability concerns stemming from performing diagnostic tests). In the case of non-diagnostic procedures, however, physicians are likely to face a mix of concerns over errors of omission and errors of commission, in which case one would either predict a more muted positive defensive medicine response or even a negative defensive medicine response. Frakes and Gruber found that this was indeed the case across every single clinical subsample that they explored. Insofar as these predictions derived from a model of defensive medicine, these findings supported a liability interpretation of their estimated treatment effects.

Unfortunately, we cannot extend this diagnostic / non-diagnostic analysis to the childbirth context for reasons indicated in Frakes and Gruber (2019)—that is, because the key diagnostic tool that is utilized at the time of childbirth—fetal monitoring—is not separately identified with the CPT procedure codes in the childbirth records in the MDR database (perhaps due to their pervasive use in so many modern deliveries). Nonetheless, we flag this analysis from Frakes and Gruber (2019) to lend general support to the identification strategy that is common across both papers.

However, we can and do attempt to support a liability interpretation of our findings by considering the second empirical exercise in this regard put forth by Frakes and Gruber (2019), whereby we separately test the relationship between immunity treatment status and health care practices during childbirth in two sets of states: (1) those without caps on non-economic damages awards (and/or total damages awards) and (2) those with caps on non-economic damages awards (and/or total damages awards). To the extent that damages caps already effectively immunize providers for some portion of the possible damages that may be levied against them, one would predict a weaker effect of fully immunizing providers from liability (in terms of reduced defensive medicine) in states that have already capped damages awards.

D. Addressing Sample Selection Concerns

One concern in defensive medicine analyses using inpatient data is that medical liability pressures may affect both the choice of hospitalization in the first instance and the choice of care intensity conditional on hospitalization, thereby implicating selection concerns in those specifications attempting to estimate the latter of these two effects. In our prior attempt to estimate the effect of liability forces on treatment intensities during inpatient stays (Frakes and Gruber 2019), we appeased these concerns by finding no evidence of a relationship between liability immunity status and the incidence of hospitalization. The present childbirth analysis affords an even cleaner resolution of this selection concern. After all, the no-hospitalization option is not heavily relied upon in the childbirth context.

IV. RESULTS

A. Preliminary Difference-in-Difference Results

We begin in Panel A of Table 4 by showing the impact of immunity from liability on patient treatment intensity during childbirth, as captured by the Relative Weighted Product (logged) for the relevant delivery. The first column shows the results from the Ordinary Least Squares (OLS) difference-in-difference specification with various patient-level covariates included. For the purposes of brevity, we note at the outset that we do not present the coefficients of the maternal risk factors and other covariates across all of the specifications estimated in Table 4, though we do show the coefficients for all such variables from this baseline OLS specification in Table A1 of the Appendix.[12] The key coefficient of interest in Table 4 is captured by the coefficient of the Active-Duty-Patient-by-MTF interaction term, a finding which suggests that immunity from liability—i.e., our "treatment" status—is associated with a 1.6 log point increase in treatment intensity. This estimate, in turn, is suggestive of a story in which greater liability pressure is associated with a decrease in the intensity of care provided to mothers during childbirth—i.e., with a story of negative defensive medicine—a finding that is counter to the conventional lay wisdom of positive defensive medicine during childbirth though consistent with the findings of Currie and MacLeod (2008).

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[12] Those covariate estimates are as expected—i.e., strong positive correlations between treatment intensity and Charlson combordity, previous cesarean delivery, multiple births, breech presentation and each of the other coded non-preventable risk factors.

In the first column of Panel B of Table 4, we estimate an identical specification but instead using the incidence of cesarean delivery as the relevant dependent variable. Our findings are consistent with the Relative Weighted Product results—that is, we find that our immunity treatment status is associated with a 1.9 percentage point increase in the incidence of cesarean delivery, a roughly 7 percent increase relative to the mean.

B. Mother Fixed Effects and Provider Fixed Effects

In Column 2 of Table 4, we build on the above OLS specification and demonstrate that this difference-in-difference finding is robust to the inclusion of mother fixed effects, providing us with a flexible approach to account (at least partially) for differences in patient status between those who deliver on and off the base. We note, however, that the magnitude of the estimated treatment effect falls slightly with this inclusion to suggest a 0.9 log-point increase in the Relative Weighted Product in association with immunity from liability (Panel A) and a 1.2 percentage-point increase in the incidence of cesarean delivery in association with the immunity treatment status (Panel B). In Columns 3 and 4, we likewise demonstrate the robustness of these findings to the inclusion of provider effects, specifications that allow us to demonstrate the relative effects within providers in their treatment of active duty versus non-active duty patients (and how these relative effects differ between encounters on and off the base). Looking separately at hospital effects and physician effects, we estimate, respectively, a 1.0 and a 0.8 log-point increase in the Relative Weighted Product in association with treatment status (Panel A), along with a 1.1 and 0.9 percentage-point increase in the incidence of cesarean delivery (Panel B). In Column 5, we likewise demonstrate the robustness of the findings to the inclusion of zip-code fixed effects, whereas in Column 6, we do the same while including each of mother, hospital, physician and zip-code effects all together.

It is important to note that the estimates across Table 4 (within the respective panel) are statistically indistinguishable from one another. This suggests among other things that the inclusion of provider effects has no discernable impact on the estimated treatment effect (especially once mother fixed effects are accounted for), which suggests that there is either little provider-based selection or that there is an offsetting effect between provider selection and patient-level spillovers within providers, as discussed in Part III above. Again, however, in the Appendix, we do not find any evidence suggesting that the immunity treatment effects on the active duty spillover to influence the care that providers deliver to non-active-duty patients.

C. *Distance-based Instruments and Base Hospital Closure Analysis*

A key concern with this difference-in-difference strategy, as discussed in Part III, is with the potential endogeneity of the choice over whether to deliver at an MTF or at a civilian hospital. Mother fixed effects partially address this concern, insofar as they allow us to make comparisons between on and off base care within the same person. However, mother fixed effects alone are imperfect in that even the same mother may face different delivery circumstances across her various births in ways that cannot be perfectly accounted for by the covariates. In this subsection, we address residual care-location endogeneity concerns by estimating various instrumental variables specifications that draw on variation in proximity to on-base care.

We start in Column 1 of Table 5 by estimating the differential effect of using an MTF for labor and delivery for active duty relative to non-active duty, where we instrument the MTF choice with a series of dummy variables signifying various bins for the distance between the relevant patient's residence and the closest MTF hospital (based on zip-code centroids). Starting with an IV specification of this nature that includes neither mother fixed effects nor provider effects, we find that our treatment status—that is, deliveries to active duty mothers at MTFs—are associated with a higher level of treatment intensity during childbirth. In particular we estimate that treatment status is associated with a roughly 1.5 log-point increase in the Relative Weighted Product (Panel A) and with a 1.4 percentage-point increase in the incidence of cesarean delivery, or a roughly 5 percent increase relative to the mean cesarean rate (Panel B). This estimate falls slightly in magnitude—to a 0.9 log-point effect in the case of the Relative Weighted Product and to a 0.8 percentage-point effect in the case of cesarean delivery—with the inclusion of mother effects to this IV specification (Column 2). This estimate remains virtually unchanged when further adding hospital effects (Column 3) and both hospital and provider effects (Column 4).

In Columns 5—8, we replicate the progression estimated in Columns 1–4 but with one important addition: the inclusion of zip-code effects. With these effects included, we draw on changes within given zip-codes over time in the distance between that zip-code centroid and the closest MTF hospital. These within-zip-code changes arise from base hospital closings during the sample period pursuant to the BRAC process. In this light, we estimate the differential effect of MTF care on active relative to non-active duty mothers, while identifying on variation in MTF usage that is driven by MTF closing events that push affected beneficiaries to deliver on the base versus off. In each case, we estimate between a 0.8 – 1.0 log-point increase in the Relative

Weighted Product and a 0.8 to 1.3 percentage point increase (or a 3—5 percent increase relative to the mean) in the incidence of cesarean delivery in connection with treatment status (though the Relative Weighted Product specification estimated in Column 5 with only mother fixed effects is estimated with slightly less precision).

Encouragingly, the estimates from the IV specifications—both based on distance alone and based on base-hospital-closings-driven changes in distance—mirror those from the OLS specifications in terms of sign and magnitude. All told, these findings are consistent with a story in which immunity from liability is associated with an increase in treatment intensity, which, in turn, implies that greater liability pressure is associated with less intensive care during childbirth. Though these IV specifications and the inclusion of mother fixed effects allow us to account for unobservable patient characteristics that may be specific to treatment status, we do remind the reader of our discussion in Part III.C above and caution that a causal interpretation of these findings may also be challenged by consideration of other mechanisms behind a differential level of treatment provided to active duty patients on the base—that is, for reasons other than the evident fact that this group poses no liability threat whatsoever. Beyond the points raised above (e.g., that most of the plausible non-liability mechanisms of this nature work in the direction opposite to our findings), we further support a liability interpretation of these findings by assessing whether the estimated relationship between liability immunity and treatment intensity is weaker in jurisdictions that have already effectively immunized some portion of liability by capping damages awards.

We show the results of this exercise in Table 6, where we estimate the full-control IV specification from Table 5 (Column 8) but where we interact the key difference-in-difference coefficient with an indicator for the presence of a damages cap (non-economic and / or total damages cap), while also including the other necessary interaction terms. Frakes and Gruber (2019) had estimated a similar specification across the full inpatient sample and found a 2 log-point lower effect of immunity from liability on the Relative Weighted Product in states with caps, relative to a baseline 5 log-point effect. The analysis was just powered enough to detect this 2 log-point difference with a 10 percent level of confidence. In the present childbirth analysis, the point estimate of the relevant interaction term (the difference-in-difference variable interacted with the damages cap indicator) likewise suggests that the effect of immunity from liability is weaker in states with a damages cap, in particular a 0.9 log-point lower effect in the Relative Weighted Product specification. However, this interaction coefficient is not statistically distinguishable from zero, which is perhaps not surprising given that we were only able to detect a marginally significant difference of this nature in the full inpatient sample in the face of a higher baseline effect. With the cesarean delivery analysis, however (Column 2 of Table 6), we indeed estimate a statistically significant interaction term of relevance. That is, we find that relative to a baseline 2.2 percentage-point increase in cesarean rates in connection with the immunity treatment status in states without caps, immunity from liability appears to be associated with a roughly 2.0 percentage-point lower effect in states that have already capped damages awards.

In the cesarean context, this suggests that the biggest effect—in terms of reducing defensive medicine—appears to come from imposing non-economic and / or total damages caps, with less subsequent impact from fully immunizing beyond that point. Incidentally, this analysis also establishes a novel method for estimating the effect of a damages cap on treatment intensity (Frakes and Gruber 2019), thereby suggesting that caps are associated with a 2 percentage-point increase in cesarean rates. Perhaps more importantly for the present purposes, however, the analysis from Table 6 supports a liability interpretation of the immunity results presented in Tables 4 and 5 (as distinct from non-liability mechanisms that may be behind an increase in treatment intensity for the active duty on the base during childbirth) insofar as one would not expect to observe these differential damages-caps results in the face of plausible non-liability explanations.

D. Alternative Treatment Intensity Metrics

For our primary analysis, we have focused on two treatment intensity metrics: the Relative Weighted Product (RWP) and the incidence of cesarean delivery. Combining length-of-stay with a cost-reflective measure of the procedures performed during the encounter, the RWP measure is our most refined and inclusive intensity metric. The cesarean analysis focuses on arguably the single most cost-intensive decision that providers make during childbirth. In Table A2 of the Appendix, we consider two alternative treatment intensity metrics, albeit less informative of total treatment intensity relative to our primary metrics. These include: (1) the total number of procedures and (2) the total number of delivery bed days. We find that the number-of-procedures results are similar in magnitude to the RWP and cesarean findings; however, we do not find an association between our liability immunity treatment status and the number of bed days associated with the delivery.

E. Heath Care Outcomes Analysis

The analysis thus far has demonstrated evidence consistent with a story in which immunity from liability increase treatment intensity during childbirth, which in turn implies that stronger liability pressures discourages more intensive approaches to childbirth on the margin. At this point in the analysis, the welfare impacts of these responses are unclear. The decrease in intensity stemming from liability pressure may not constitute true negative "defensive" medicine if this behavioral response is associated with improved patient outcomes. In this sub-section, we accordingly explore whether immunity from liability is associated with an alteration of certain health care outcomes specific to childbirth: (1) certain potentially preventable delivery complications (e.g., fetal distress, dysfunctional labor, etc.), (2) neo-natal death, (3) neonatal trauma and (4) maternal trauma. We note at the outset that the most reliable analysis here is likely with respect to our first measure—i.e., the incidence of certain delivery complications—as this incidence occurs over roughly 24 percent of the deliveries in the sample, leaving us with a high baseline rate over which to test the effect of immunity from liability. The incidence of neonatal trauma and neonatal mortality, in contrast, are very low over the sample period, challenging our ability to test the effects of immunity on such rates with a comfortable level of precision.

Rather than present the full set of specifications indicated in Tables 4 and 5 for each of these outcome measures, we present results for only a subset of these specifications. Included in this subset is the full-control base-closure IV specification that instruments on-base care with the distance-from-MTF bins and that includes, zip-code, mother, hospital and provider fixed effects. Nonetheless, given our concerns over statistical precision in the face of low baseline rates, we also show results from three other specifications that do not include the mother, hospital and physician fixed effects: (1) the baseline OLS specification, (2) the IV specification using the distance-from-MTF bins and (3) the IV specification with distance bins and zip-code fixed effects (i.e., the hospital-closing-based IV specification). We spread the results of these specifications across Tables 8 and 9, where Table 8 focuses on delivery complications and neonatal mortality and Table 9 focuses on neonatal trauma and maternal trauma.

As demonstrated by Table 8, we find no evidence of a relationship between the immunity treatment group and the incidence of any one of the following complications: fetal distress, excessive bleeding, prolonged labor or dysfunctional labor. Moreover, the estimated difference-in-difference coefficient in the delivery complication specification is relatively tightly bound around zero, with the bottom end of the 95 percent confidence interval suggesting a -7 to a -10 percent effect relative to the mean delivery complication rate (depending on the specification) and with the top end of the 95 percent confidence interval suggesting only a 2 to 6 percent effect relative to the mean. Accordingly, this analysis supports a defensive interpretation of the results from Tables 4—6 in that we appear to find an impact of liability pressure on observables rates of treatment utilization with little consequence to quality.

In the case of neonatal mortality and neonatal trauma, we likewise fail to find a relationship between immunity from liability and the respective outcome, though the results are much less precise. For instance, in the baseline OLS specification for the neonatal mortality analysis, the 95 percent confidence interval for the difference-in-difference coefficient—expressed as a fraction of the relevant mean—ranges from -0.52 to 0.36. The confidence intervals expands considerably in the full-control base-closure-driven IV specification. Accordingly, we cannot rule out large effects of immunity from liability on neonatal mortality. The neonatal trauma results are slightly more precise, though even there, we cannot rule out that immunity from liability leads to as much as a 14-65 percent (relative to the mean) increase in neonatal trauma rates.

Interestingly, counter to a story in which liability pressure causes better outcomes and immunity from liability leads to deterioration in health care status, we find an opposite signed effect in the case of maternal trauma. That is, we find immunity from liability is associated with a decline in the incidence of maternal trauma, a finding that, in turn, implies that greater liability pressure is associated with a worsening of maternal trauma rates. This finding is not inconsistent with a defensive medicine story insofar as it only reinforces that liability pressure may lead to suboptimal care. However, even though we run into fewer concerns over statistical precision in this case given that baseline rates of maternal trauma are over 3 percent in our sample, we do stress that this maternal-trauma exercise arguably suffers from sample selection concerns to the extent that it is estimated only over the vaginal delivery sample and to the extent that immunity from liability has already been shown to impact the incidence of cesarean delivery.

All told, there is little evidence by which to suggest that immunity from liability leads to a worsening of health care quality and health care outcomes. Accordingly, the evidence tends to suggest that the treatment intensity results depicted above may indeed be defensive in nature and not merely a reflection of optimal deterrence in practice.

V. CONCLUSION

Observers, including policymakers, are frequently concerned with the bottom-line—that is, just how much additional spending overall is attributable to medical liability fears. Nonetheless, it is arguably important to dig deeper than these overall effects and understand how defensive medicine operates in more specific clinical arenas. After all, if positive defense medicine exists in some contexts and negative in others, then just focusing on overall spending impacts may mask what is otherwise a much larger deviation from optimal medical practices and thus a much larger potential impact on social welfare. Focusing specifically on childbirth is a natural first step in this context-specific endeavor given that labor and delivery constitutes the most frequent reason for hospitalization in the U.S. Given this scale, even if the findings from the childbirth setting do not generalize to other settings, they undoubtedly carry independent interest.

In focusing on labor and delivery encounters, we indeed find evidence suggesting that medical liability shapes physician behavior, specifically that liability pressures may be contributing to negative defensive medicine in childbirth care, in contrast with the positive defensive medicine that Frakes and Gruber (2019) have found to be characteristic of inpatient care on average. To be sure, our estimated effect sizes are somewhat modest—e.g., a roughly 1 percentage-point (or a roughly 4 percent relative to the mean) higher cesarean rate in association with liability immunity. Though our full analysis suggests that this estimate is consistent with a true liability effect, the magnitude is arguably small relative to other sources of variation in cesarean rates, as is perhaps evident from other findings in this paper itself. For instance, this liability effect is smaller than the roughly 1.6 percentage-point differential in cesarean rates between on-base and off-base deliveries (as presented in Column 5 of Table 4). Moreover, when focusing on the distribution of mean cesarean rates across Military Treatment Facilities (weighted by number of deliveries), we observe a standard deviation of roughly 2.9 percentage points, suggesting that the estimated negative defensive-medicine effect is modest in comparison with the degree of area variations in cesarean rates.

We further acknowledge and caution that our findings are subject to some amount of generalizability concerns (in addition to the internal validity concerns and caveats already addressed above), considering that we have investigated this question using a treatment group consisting of active duty patients receiving care at military facilities. The impacts of immunizing providers more broadly may differ from those presented above considering that liability's effects may interact with various patient and care-system characteristics.

Generalizability concerns aside, our analysis has allowed us to explore the impacts of medical liability pressure during labor and delivery care while drawing on a more fundamental form of variation in the law than that explored by the existing literature to date. By creating an unprecedented counterfactual whereby liability recourse is not available at all, our methodological design allows us to explore the full extent of defensive medicine. While much of the existing literature claims to identify the magnitude of defensive medicine through the exploration of the effects of various tort reforms—e.g., caps—such reforms necessarily only capture part of the story insofar as they leave intact much of the liability threat itself.

In part, this limitation of the existing literature is methodological in nature, as it leaves analysts with less power to identify liability's influence. Furthermore, the marginal nature of these reforms limits the extent of the policy implications that can be drawn from such studies. After all, the effects of caps may only speak to the degree of liability pressure that is indeed reduced by caps. The results from the present study, on the other hand, can be seen as placing an upper bound on the full impacts that may derive from next-generation reforms to the liability system that more fundamentally target the sources of defensive behavior—for instance, reforms that reduce providers' uncertainties over the standards of care that they face. In fact, policymakers continue to express interest in an uncertainty-reducing reform of this nature—e.g., liability "safe harbors" that exempt providers from malpractice risk if the treatment adheres to certain clearly delineated guidelines.[13]

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[13] 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.

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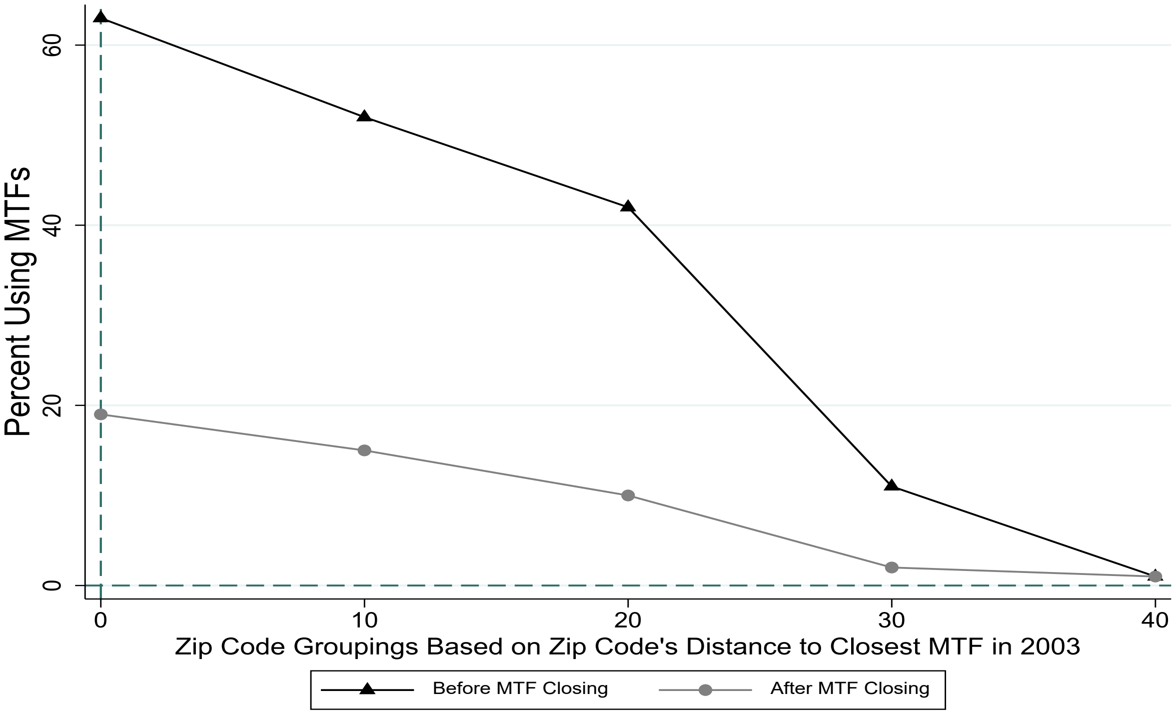
Figure 1: Percent Delivering On-base as a Function of Distance from Base, before and after Closing of Nearby MTF Hospital



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 labor and delivery 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.

Table 1: Summary Statistics

	(1)
Utilization Metrics	
Relative Weighted Product	0.51 (0.23)
Bed Days	2.50 (2.57)
Number of Procedures	2.00 (1.41)
Cesarean Delivery	0.27 (0.45)
Health Care Quality Metrics	
Neonatal Mortality (28-Day, per 1,000 people)	1.338 (36.553)
Neonatal Birth Trauma (per 1,000 people)	4.127 (64.105)
Maternal Trauma (Among Vaginal Delivery Sample, per 100 people)	3.435 (18.211)
Incidence of Certain Delivery Complications (Percent of births involving either fetal distress, excessive bleeding, precipitous labor, prolonged labor and dysfunctional labor)	24.495 (43.007)
N	1,029,197

Source: 2003-2013 Military Health System Data Repository.

Table 2: Covariate Balance Analysis in Childbirth / Delivery 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					
Charlson Comorbidity Score	0.03 (0.18)	0.06 (0.25)	0.02 (0.15)	0.04 (0.21)	-0.01** (0.00)
(Omitted: Junior Enlisted)					
Senior Enlisted	0.33 (0.47)	0.43 (0.50)	0.41 (0.49)	0.42 (0.49)	-0.09*** (0.001)
Warrant Officer	0.01 (0.04)	0.01 (0.10)	0.01 (0.07)	0.01 (0.1.5)	0.00 (0.00)
Junior Officer	0.09 (0.28)	0.11 (0.32)	0.09 (0.29)	0.11 (0.32)	-0.01 (0.01)
Senior Officer	0.04 (0.19)	0.05 (0.22)	0.04 (0.20)	0.05 (0.22)	0.00 (0.00)
Pay-Grade Missing	0.39 (0.49)	0.18 (0.38)	0.54 (0.50)	0.27 (0.44)	-0.06*** (0.02)
Maternal age	25.75 (5.03)	26.50 (5.12)	26.66 (5.20)	26.45 (5.19)	-0.96*** (0.20)
Previous Cesarean Delivery	0.09 (0.29)	0.13 (0.34)	0.10 (0.30)	0.14 (0.34)	-0.01*** (0.00)
Multiple Deliveries (e.g., Twins)	0.01 (0.12)	0.01 (0.11)	0.02 (0.15)	0.02 (0.14)	-0.00** (0.00)
Breech Presentation	0.03 (0.18)	0.03 (0.18)	0.04 (0.19)	0.04 (0.19)	-0.00*** (0.00)
Panel B. Evaluating Covariate Balance Collectively					
Predicted Log Relative Weighted Product (based on regression of log of relative weighted product on covariates)	-0.74 (0.15)	-0.72 (0.16)	-0.71 (0.17)	-0.71 (0.17)	-0.01*** (0.00)
Predicted Log Relative Weighted Product. Final column including mother fixed effects	-	-	-	-	-0.00 (0.00)
Predicted Log Relative Weighted Product. Final column including mother fixed effects and instrumenting for on-base care	-	-	-	-	-0.00 (0.00)

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 3: First Stage Estimates. Effect of Patient Distance from Base (and its Interaction with Active-Duty Status) on the Incidence of an On-Base Delivery (and its Interaction with Active-Duty Status)

	(3)	*(4)*
	ON_BASE	ACTIVE_DUTY X ON_BASE
Omitted: Absolute Distance < 10 Miles		
Absolute Distance 10-20 miles	-0.071***	0.001**
	(0.019)	(0.000)
Absolute Distance 20-30 miles	-0.367***	0.003***
	(0.038)	(0.000)
Absolute Distance 30-40 miles	-0.626***	0.004***
	(0.028)	(0.000)
Absolute Distance 40+ miles	-0.725***	0.004***
	(0.020)	(0.000)
Omitted: ACTIVE_DUTY X Absolute Distance < 10 Miles		
ACTIVE_DUTY X Absolute Distance 10-20 miles	0.070***	-0.009
	(0.013)	(0.017)
ACTIVE_DUTY X Absolute Distance 20-30 miles	0.199***	-0.191***
	(0.043)	(0.050)
ACTIVE_DUTY X Absolute Distance 30-40 miles	0.054	-0.603***
	(0.056)	(0.078)
ACTIVE_DUTY X Absolute Distance 40+ miles	-0.109***	-0.878***
	(0.015)	(0.018)
N	1,016,606	1,016,606
F-Statistics for Test of Joint Significant of Instruments	416.00	941.61
P-value of F-statistic	0.00	0.00

NOTES: robust standard errors corrected for within-catchment-area correlation in the error term are reported in parentheses. All regressions included year fixed effects, zip-code fixed effects and controls for patient age (dummies), Charlson comorbidity, paygrade (dummies), primary diagnosis dummies and the incidence of various non-preventable delivery complications. *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 4: Relationship between Medical Liability Immunity and Childbirth Delivery Treatment Intensity

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Panel A. Dependent variable = Relative Weighted Product, logged						
MTF	-0.006	-0.009***	-	-	-0.016***	-
	(0.004)	(0.003)			(0.006)	
Active Duty Patient	0.014***	-0.001	0.016***	0.016***	0.016**	-0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Active Duty Patient X MTF	**0.016*** **	**0.009*** **	**0.010*** **	**0.008*** **	**0.010*** **	**0.008*** **
	(0.003)	**(0.003)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.003)**
N	928386	928386	928386	928386	928386	928386
Panel B. Dependent variable = incidence of cesarean delivery (mean = 0.27)						
MTF	-0.015***	-0.015***	-	-	-0.020***	-
	(0.005)	(0.003)			(0.007)	
Active Duty Patient	0.021***	-0.007	0.025***	0.023***	0.023***	-0.007*
	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.004)
***Active Duty Patient X MTF**	**0.019*** **	**0.012*** **	**0.011*** **	**0.009*** **	**0.012*** **	**0.010*** **
	(0.004)	**(0.004)**	**(0.003)**	**(0.003)**	**(0.003)**	**(0.004)**
N	1016606	1016606	1016606	1016606	1016606	1016606
Hospital Fixed Effects	NO	NO	YES	NO	NO	YES
Physician Fixed Effects	NO	NO	NO	YES	NO	YES
Mother Fixed Effects	NO	YES	NO	NO	NO	YES
Zip code Fixed Effects?	NO	NO	NO	NO	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 non-preventable 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 5: Relationship between Medical Liability Immunity and Childbirth Delivery Treatment Intensity, Instrumental Variables Estimates

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Panel A. Dependent variable = Relative Weighted Product, logged								
MTF	0.000	-0.007***	-	-	0.016	-0.017	-	-
	(0.004)	(0.002)			(0.016)	(0.033)		
Active Duty Patient	0.013***	-0.002	-0.001	-0.002	0.013***	-0.001	-0.002	-0.003
	(0.002)	(0.004)	(0.004)	(0.004)	(0.002)	(0.004)	(0.004)	(0.004)
Active Duty Patient X	**0.015*****	**0.009****	**0.009****	**0.008****	**0.008****	**0.010**	**0.010****	**0.009****
MTF	**(0.003)**	**(0.004)**	**(0.004)**	**(0.004)**	**(0.003)**	**(0.006)**	**(0.004)**	**(0.004)**
N	928386	928386	928386	928386	928386	928386	928386	928386
Panel B. Dependent variable = incidence of cesarean delivery (mean = 0.27)								
MTF	-0.009	-0.016***	-	-	-0.005	-0.046**	-	-
	(0.007)	(0.002)			(0.019)	(0.021)		
Active Duty Patient	0.022***	-0.005	-0.006	-0.005	0.024***	-0.005	-0.007	-0.006
	(0.002)	(0.004)	(0.004)	(0.004)	(0.002)	(0.004)	(0.004)	(0.004)
Active Duty Patient X	**0.014*****	**0.008****	**0.009****	**0.007**	**0.008***	**0.013****	**0.010****	**0.008***
MTF	**(0.004)**	**(0.004)**	**(0.004)**	**(0.004)**	**(0.004)**	**(0.005)**	**(0.004)**	**(0.004)**
N	1016606	1016606	1016606	1016606	1016606	1016606	1016606	1016606
Instrument MTF with Distance Binds (from residence to closest MTF)?	YES	YES	YES	YES	YES	YES	YES	YES
Hospital Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES
Physician Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Mother Fixed Effects	NO	YES	NO	YES	NO	YES	YES	YES
Zip code Fixed Effects?	NO	NO	NO	NO	YES	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 non-preventable 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 6: Relationship between Medical Liability Immunity and Childbirth Treatment Intensity, interacting Immunity Treatment Status with the Incidence of a Damages Cap

	(1)	*(2)*
	Relative Weighted Product (Logged)	Incidence of Cesarean Delivery (Mean = 0.27)
Active Duty Patient	0.002	-0.011**
	(0.005)	(0.004)
Active Duty Patient X Incidence of Damages Cap	-0.022*	-0.026**
	(0.012)	(0.011)
Active Duty Patient X MTF	0.016**	0.022***
	(0.007)	(0.007)
MTF X Incidence of Damages Cap	-0.025**	-0.026**
	(0.011)	(0.011)
Active Duty Patient X MTF X Incidence of Damages Cap	**-0.009**	**-0.020****
	(0.008)	**(0.088)**
N	928386	1016606

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, zip-code fixed effects, mother fixed effects, hospital and physician fixed effects, primary diagnosis code fixed effects, and controls for patient age (dummies), Charlson comorbidity, paygrade (dummies) and various non-preventable delivery risk factors. MTF (and the various interaction terms involving 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 distance dummies and the other relevant terms in the respective interaction effect). The coefficient of the MTF indicator is dropped due to the inclusion of provider fixed effects. 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 7: Relationship between Medical Liability Immunity and Delivery Complications and Neonatal Mortality

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	INCIDENCE OF DELIVERY COMPLICATION (COEFFICIENTS MULTIPLIED BY 100)				INCIDENCE OF NEONATAL MORTALITY (COEFFICIENTS MULTIPLIED BY 1,000)			
MTF	8.414*** (1.516)	9.139*** (1.679)	9.963*** (3.105)	-	-0.046 (0.141)	0.762*** (0.250)	0.242 (1.414)-	-
Active Duty Patient	3.839*** (0.318)	3.557*** (0.298)	3.252*** (0.285)	3.310*** (0.871)	0.397* (0.216)	0.224 (0.233)	0.023 (0.209)	-2.222*** (0.768)
Active Duty Patient X MTF	**-0.610** **(0.573)**	**-0.477** **(0.702)**	**-0.359** **(0.617)**	**-0.404** **(0.996)**	**-0.109** **(0.295)**	**-0.160** **(0.302)**	**0.216** **(0.361)**	**1.349** **(0.937)**
95% Confidence Interval for Interaction Coefficient	[-1.758, 0.538]	[-1.885, 0.931]	[-1.596, 0.877]	[-2.400, 1.591]	[-0.701, 0.483]	[-0.767, 0.447]	[-0.507, 0.938]	[-0.530, 3.222]
95% Confidence Interval as a Fraction of Mean of Dependent Variable	[-0.07, 0.02]	[-0.08, 0.04]	[-0.07, 0.04]	[-0.10, 0.06]	[-0.52, 0.36]	[-0.57, 0.33]	[-0.39, 0.69]	[-0.39, 2.39]
N	1016606	1016606	1016606	1016606	1016606	1016606	1016606	1016606
Instrument MTF with Distance Binds (from residence to closest MTF)?	NO	YES	YES	YES	NO	YES	YES	YES
Hospital Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Physician Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Mother Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Zip code Fixed Effects?	NO	NO	YES	YES	NO	NO	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 non-preventable delivery risk factors, along with the primary diagnosis code (dummies). Delivery complications comprising the dependent variable in Columns 1-4 include those identified as "preventable" by Currie and MacLeod (2008): fetal distress, excessive bleeding, precipitous labor, prolonged labor and dysfunctional labor. *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 8: Relationship between Medical Liability Immunity and Neonatal and Maternal Trauma

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	Incidence of Neonatal Trauma (Coefficients Multiplied by 1000)				Incidence of Maternal Trauma (Coefficients Multiplied by 100)			
MTF	3.400*** (0.590)	3.672*** (0.640)	4.416 (5.766)	-	-0.492*** (0.143)	**-0.724***** **(0.182)**	-0.03 (0.938)	-
Active Duty Patient	0.295 (0.257)	0.462 (0.294)	0.458 (0.454)	0.603 (0.975)	0.781*** (0.151)	0.871*** (0.179)	0.818*** (0.162)	1.081** (0.513)
Active Duty Patient X MTF	**-0.030** **(0.419)**	**-0.385** **(0.482)**	**-0.521** **(0.881)**	**0.264** **(1.219)**	**-0.456**** **(0.173)**	**-0.508***** **(0.215)**	**-0.735***** **(0.207)**	**-1.084*** **(0.544)**
95% Confidence Interval for Interaction Coefficient	[-0.871, 0.809]	[-1.351, 0.581]	[-2.288, 1.246]	[-2.179, 2.707]	[-0.803, -0.109]	[-0.933, -0.068]	[-1.152, -0.320]	[-2.175, 0.008]
95% Confidence Interval as a Fraction of Mean of Dependent Variable	[-0.21, 0.19]	[-0.33, 0.14]	[-0.55, 0.30]	[-0.53, 0.65]	[-0.23, -0.03]	[-0.20, -0.02]	[-0.33, -0.09]	[-0.63, 0.00]
N	1016606	1016606	1016606	1016606	736,756	736,756	736,756	736,756
Instrument MTF with Distance Binds (from residence to closest MTF)?	NO	YES	YES	YES	NO	YES	YES	YES
Hospital Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Physician Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Mother Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Zip code Fixed Effects?	NO	NO	YES	YES	NO	NO	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 non-preventable delivery risk factors, along with the primary diagnosis code (dummies). *Source*: 2003-2013 Military Health System Data Repository.

Appendix

Table A1: Relationship between Medical Liability Immunity and Childbirth Delivery Treatment Intensity (Relative Weighted Product, Logged)

	(1)
MTF	-0.006 (0.004)
Active Duty Patient	0.014*** (0.002)
Active Duty Patient X MTF	**0.016***** **(0.003)**
Charlson Comorbidity Score	0.070*** (0.002)
(Omitted: Junior Enlisted)	-
Senior Enlisted	-0.006 (0.001)
Warrant Officer	-0.012*** (0.004)
Junior Officer	-0.025*** (0.002)
Senior Officer	-0.038*** (0.002)
Previous Cesarean Delivery	0.344*** (0.005)
Multiple Deliveries	0.222*** (0.005)
Breech Presentation	0.320*** (0.008)
Cephalopelvic disproportion	0.448*** (0.004)
Cord prolapse	0.295*** (0.010)
Placenta Previa	0.376*** (0.009)
Abruptio Placentae	0.250*** (0.004)
N	928386
Hospital Fixed Effects	NO
Physician Fixed Effects	NO
Mother Fixed Effects	NO
Zip code Fixed Effects?	NO

NOTES: This table replicates the specification from Column 1 of Panel A of Table 4 in the text. The addition is simply to show the coefficient estimates for various of the included covariates. Not shown are the coefficients for the estimated age dummies, primary diagnosis code dummies and year 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 A2: Relationship between Medical Liability Immunity and Alternative Childbirth Treatment Intensity Metrics: Instrumental Variables Estimates

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Panel A. Dependent variable = number of procedures (logged)								
MTF	0.038	0.019	-	-	0.286***	0.188	-	-
	(0.053)	(0.037)			(0.130)	(0.126)		
Active Duty Patient	0.003	0.009	0.006	0.009	-0.013*	0.004	0.011	0.015
	(0.010)	(0.015)	(0.0103)	(0.013)	(0.007)	(0.012)	(0.013)	(0.013)
Active Duty Patient X MTF	**0.012**	**0.044****	**0.047*****	**0.035*****	**0.001**	**0.033**	**0.039****	**0.028***
	(0.022)	**(0.022)**	**(0.016)**	**(0.015)**	**(0.021)**	**(0.022)**	**(0.015)**	**(0.015)**
N	1016606	1016606	1016606	1016606	1016606	1016606	1016606	1016606
Panel B. Dependent variable = number of bed days (logged)								
MTF	-0.021	-0.028**	-	-	0.133**	0.105	-	-
	(0.023)	(0.014)			(0.064)	(0.088)		
Active Duty Patient	0.044***	0.064***	0.080***	0.081	0.040	0.075***	0.074***	0.074***
	(0.005)	(0.008)	(0.007)	(0.007)	(0.004)	(0.008)	(0.007)	(0.007)
Active Duty Patient X MTF	**0.038*****	**0.022****	**-0.001**	**-0.005**	**-0.003**	**-0.010**	**0.001**	**-0.002**
	(0.009)	**(0.009)**	**(0.008)**	**(0.008)**	**(0.011)**	**(0.011)**	**(0.008)**	**(0.008)**
N	1015920	1015920	1015920	1015920	1015920	1015920	1015920	1015920
Instrument MTF with Distance Binds (from residence to closest MTF)?	YES	YES	YES	YES	YES	YES	YES	YES
Hospital Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES
Physician Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Mother Fixed Effects	NO	YES	NO	YES	NO	YES	YES	YES
Zip code Fixed Effects?	NO	NO	NO	NO	YES	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 non-preventable 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.

Treatment Effects Spillover Analysis

In this section, we follow the analysis from Frakes and Gruber (2018) and explore whether the liability immunity treatment effect possibly spills over to likewise affect the control group. That is, consider a physician on the base who treats both active duty and non-active duty patients. The active duty patients have no liability recourse. Assume that the physician wishes to provide a consistent practice style to all of her patients; however, assume that she will develop this practice style while considering her aggregate liability exposure. In other words, the more active duty patients the physician treats, the less liability pressure she will perceive and the less she may practice defensively in the face of all of her patients. Behavior of this sort can be characterized as the treatment effect spilling over to influence the control group.

Now, it could be the case that physicians do not behave like this and simply differentially treat the immune group and the non-immune group. Or, the truth could be somewhere in between—that is, there is differential treatment plus some amount of spillovers. In this section, we will look for some empirical markers of spillover effects to illuminate this discussion. Should spillover effects exist, then the estimates from the provider fixed effects specifications in the text can be seen as a lower bound of the true effects of immunities from liability.

To test for evidence of patient spillovers, we implement an empirical strategy whereby we examine how the treatment of non-active duty patients on the base varies with the share of a base provider's caseload that is active duty. If there are spillovers, then the higher is a provider's share of active duty patients (for on-base physicians), the more intense should be their treatment of non-active duty patients—at least assuming that providers practice negative defensive medicine as the analysis from the text tends to suggest in the labor and delivery context. The results of this exercise are shown in Table A3. Panel A focuses on spillovers within physicians, whereas Panel B focuses on spillovers within hospitals. In each case, we regress treatment intensity (log of Relative Weighted Product or the incidence of a cesarean delivery) on the provider's active-duty share, while controlling for each of the covariates indicated in equation (1) of the text and while including provider fixed effects. The latter fixed effects ensure that we draw on within-provider changes over time in their active-duty patient shares in order to test for patient-level spillovers. We perform all of this analysis using records from the direct care (on-base) encounters.

Ultimately, under this approach, we do not find evidence indicative of spillovers within physicians or within hospitals. The results are somewhat imprecise, however, and we cannot rule out some amount of spillover effects. This approach is also not perfect as it is possible that when a physician sees relatively fewer non-active-duty, the remaining non-active-duty could be relatively more sick or healthy, a possibility that would impose bias in the above analysis.

We test for markers of any such selection in Table A4. For these purposes, for each non-active-duty patient, we form predicted values of the indicated treatment intensity metric based on an initial regression of such metrics on the numerous covariates employed in our primary analysis. We then regress those predicted values on the active-duty patient share of the provider, along with provider fixed effects. In the case of physician-level spillovers, we indeed find evidence suggesting some degree of selection. That is, we find that predicted log Relative Weighted Product is about 2.4 log points lower—reflecting an arguably healthier non-active duty patient of interest—following an increase from 0 to 1 in a physician's active-duty patient share (note, however, that we find no such markers of selection issues in the hospital spillovers analysis). If this physician spillovers selection finding also portends selection on unobservables, this would tend to bias against finding evidence of spillover effects (assuming negative defensive medicine). In other words, despite the findings from Table A3, it remains possible that treatment effect spillovers exist. Again, however, if that is the case, that only suggests that our provider fixed effects findings from the text are merely lower bounds of the full defensive medicine effects.

Note that the results from this analysis are not sensitive to the use of regression weights from the number of patients treated by each of the relevant physicians (in Panel A) or hospitals (in Panel B) (i.e., weights for the denominator in the patient share metrics).

Table A3: Patient Spillover Analysis: Relationship between Treatment Intensity and Active-Duty Patient Share of Provider, focusing on Care Delivered to Non-Active Duty Patients on the Base

	(1)	*(2)*
	Log Relative Weighted Product	Incidence of Cesarean Delivery
Panel A: Physician-Level Spillover Analysis. Outcome variable equals the indicated metric.		
Active Duty Patient Share of Physician	-0.017 (0.015)	-0.031 (0.021)
N	326,923	359,609
Panel B: Hospital-Level Spillover Analysis. Dependent variable equals the indicated metric.		
Active Duty Patient Share of Hospital	0.001 (0.057)	-0.052 (0.102)
N	326,923	359,609

NOTES: robust standard errors corrected for within-physician correlation in the error term are reported in parentheses. All regressions include year and primary diagnosis code fixed effects, and controls for patient age (dummies), Charlson comorbidity, paygrade (dummies), and various non-preventable delivery risk factors. This analysis is confined to records from the direct-care setting. The active-duty patient share of the physician or hospital is calculated annually. Specifications in Panel A include physician fixed effects, thereby drawing on within-physician variation over time in active-duty patient shares. Specifications in Panel B include hospital fixed effects, thereby drawing on within-hospital variation over time in active-duty patient shares.

Table A4: Testing for Selection Effects in Patient Spillover Analysis: Relationship between Predicted Treatment Intensity (Based on Observable Covariates) and Active-Duty Patient Share of Provider, focusing on Care Delivered to Non-Active Duty Patients on the Base

	(1)	*(2)*
	Log Relative Weighted Product	Incidence of Cesarean Delivery
Panel A: Physician-Level Spillover Selection Analysis. Dependent variable equals the predicted value of the indicated metric, based on a regression of that metric on the set of covariates used in Panel A.		
Active Duty Patient Share of Physician (Unit = 1 Standard Deviation)	-0.024** (0.010)	-0.034* (0.017)
Panel B: Hospital-Level Spillover Selection Analysis. Dependent variable equals the predicted value of the indicated metric, based on a regression of that metric on the set of covariates used in Panel C.		
Active Duty Patient Share of Hospital (Unit = 1 Standard Deviation)	-0.019 (0.038)	-0.021 (0.062)

NOTES: robust standard errors corrected for within-physician correlation in the error term are reported in parentheses. The active-duty patient share of the physician or hospital is calculated annually. Specifications in Panel A physician fixed effects, thereby drawing on within-physician variation over time in active-duty patient shares. Specifications in Panels B include hospital fixed effects, thereby drawing on within-hospital variation over time in active-duty patient shares. Specifications include year effects. This analysis is confined to records from the direct-care setting. *** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

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