Public and Private Options in Practice: The Military Health System

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JULY, 2022

Recent debates over health care reform, including in the context of the Military Health System (MHS) and Veterans Administration, highlight the dispute between public and private provision of health care services. Using novel data on childbirth claims from the MHS and drawing on the combination of plausibly exogenous patient moves and heterogeneity across bases in the availability of base hospitals, we identify the impact of receiving obstetrical care on versus off military bases. We find evidence that off-base care is associated with slightly greater resource intensity, but also notably better outcomes, suggesting marginal efficiency gains from care privatization.

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Health systems throughout the world typically consist of some mix of public and private health care delivery. In the U.S., most health care is delivered privately, albeit sometimes under contract with public payers, but health care for the U.S. military and veterans has historically been delivered largely through public systems. In many European nations, health care is typically delivered by public systems, while individuals can often "top up" their care using private arrangements with (often the same) providers. But one thing that appears common to all systems is a constant sense of dissatisfaction with the mix. For example, in the UK, the Prime Minister faces pressure to undo the increased tendering of private contracts that began in 2012 (Hussain 2020). On the other hand, in the United States, former President Trump signed the Veterans Affairs' Mission Act, allowed some veterans to receive care from community providers outside of public VA Medical Centers – and was opposed by many stakeholders, including the former head of the Veteran Affairs Department David Shulkin, who has argued that the VA has special expertise in treating certain issues (Shulkin 2018).

Therefore, understanding the implications of shifting care from public to private health systems, and vice versa, is a key issue worldwide. But this has been a difficult issue to study because the populations using these different systems are quite heterogeneous – and when individuals have a choice between systems, they may do so in a way which is correlated with unobserved determinants of health.

Perhaps for this reason, there is little work of which we are aware comparing delivery between public and private systems of care (particularly in the U.S).[2] There is a large literature that compares public versus private *financing* of care, such as through the Medicare Advantage program versus

[2] The one exception is a contemporaneous paper by Card, Chan, and Taylor (2020) which compares those who receive care inside or outside of the VA health care system. We discuss the comparison between our results and theirs below.

traditional Medicare (e.g. Duggan, Gruber and Vabson, 2018; Curto *et al.*, 2019), or through private Medicaid HMOs versus government direct contracting with physicians (e.g. Geruso *et al.*, 2019). But the actual care delivered through these different financing mechanisms are often at similar sets of providers, so that these differences primarily reflect contracting and care management differences across the public and private sectors.

Closer in spirit are a number of papers that compare delivery of care at publicly owned versus privately owned hospitals; for example, Villa and Kane (2012) find that privatizing public hospitals in the United States results in increased occupancy, shorter stays, and the loss of unprofitable services. Various related studies in the international context use survey instruments to compare outcomes at public and private hospitals, finding that private facilities have both higher prices and higher quality.[3] But these international investigations are often subject to selection bias, with papers using propensity matching (Bjorvatn, 2018) and instrumental variables strategies (Pérotin *et al.*, 2013) finding substantial patient differences across the types of providers and little causal difference in quality.[4] Methodological challenges aside, inquiries into public-versus-private ownership of a particular hospital are unable to speak to the impact of an entirely public-versus-private *system* of care, which includes the entirety of outpatient and inpatient care, and which incorporates different payment mechanisms and care management strategies.

[3] See for example Andaleeb (2000), Camilleri and O'Callaghan (1998), Taner and Antony (2006), and Yesilada and Direktor (2010).

[4] Systematic reviews by Tiemann *et al.* (2012) and Eggleston *et al.* (2008) conclude that existing studies do not demonstrate a convincing gap between private and public facilities.

Surprisingly, the best context for studying a mixed public-private system may be in the U.S.: the Military Health System (MHS). The MHS is a $50 billion/year program that provides care to active-duty military, their dependents, and military retirees, covering over 9 million eligible individuals. Crucially, MHS beneficiaries have access to government-owned and run facilities on military bases, as well as private providers that are contracted to the military through an insurance company. Care is split roughly equally between the two sources; 49 percent of outpatient encounters and more than 67 percent of hospitalizations for MHS beneficiaries take place with private providers (TRICARE Management Activity, 2020).

The proper division of care on and off military bases is a source of controversy and policy action. In particular, the MHS announced plans in 2019 to repurpose roughly 17,000 medical providers (Philpott, 2019)—a development that is likely to push MHS beneficiaries to receive care from civilian facilities—and the Pentagon's 2021 budget called for transitioning roughly 200,000 non-active-duty beneficiaries to receiving care from civilian providers (Kime, 2020). Yet despite these policy actions, there is no convincing evidence as to whether moving care to civilian providers raises or lowers costs, or improves or deteriorates outcomes.

In this paper, we provide such evidence. This evidence is immediately relevant to the $50 billion MHS, one of our nation's largest health care programs. But it may also have important implications for other systems in the U.S.; more broadly, it speaks to the rigorous and ongoing debate between expanding or contracting the role of the private sector in delivering care in the U.S.[5]

[5] For instance, in a recent New York Times commentary, Aaron Carroll makes on argument on the contraction side of this debate, suggesting that the U.S. can learn "a thing or two" from Singapore's "largely privately financed public delivery system," the "opposite of what we have in the United States (Carroll, 2019).

A key feature of the military allows us to create a quasi-experimental analysis of the impact of systems of care on patient treatment and outcomes: plausibly exogenous moves. Moves across military bases are dictated by the needs of the military and are not solely determined by the preferences of individuals which might be endogenous to their tastes for medical care. Preferences may only be expressed within the set of available bases, which are dictated by the combination of rank, occupation, and time (Lleras-Muney, 2010). Combining beneficiary moves with another key feature of the military context—i.e., variation across bases in the availability of base hospitals—provides us with exogenous variation in access to a public system of care. A further advantage of studying the MHS is that there is a large sample of individuals who themselves receive care both on and off base for a major medical intervention: childbirth. This allows us to hold patient characteristics constant in comparing care delivered from different sources, as well as to take advantage of a detailed set of quality measures facilitated by the childbirth setting. During our sample period there are 776,074 births to 590,353 dependents of active-duty military, and 43.63% of children are delivered at MHS bases.

To carry out this study, we turn to an innovative data set that has been little used by economists, the Military Health System Data Repository data. These data provide complete medical claims for all military personnel and their dependents over the 2003-2013 period. Critically, they provide detail on care delivered on and off military bases.

Our analysis follows the approach of Finkelstein *et al*. (2016) in a discrete event framework. We consider every mother in our sample who gives birth at least twice. We then restrict our sample to all mothers who move to a new base between births, and we compare those mothers who see a change in the availability of military hospitals. That is, we compare mothers who move but both before *and* after the move do (or do not) have a military hospital nearby, to mothers who move and who see a change in their nearby access to military hospitals. In this way, we control for both underlying mother characteristics, as well as any effect of moves per se. Following Finkelstein *et al*. (2016), we provide evidence that our estimates may be causal by examining the previous births for all such mothers. We begin with a transparent presentation of mean birth outcomes among movers, and carry this through to simple differences-in-differences estimates of the reduced form. We then move to an instrumental variables (IV) framework which allows us to carry out overidentification tests to confirm that the results are similar for mothers moving either closer or further from base hospitals when they give birth.

We find that mothers delivering off-base use more resources than those delivering on-base; total resource utilization appears to be about 2.2% higher for those using the private- rather than public-care system, primarily driven by higher Cesarean section rates off-base. At the same time, we find that the quality of care appears to be higher for mothers delivering and receiving prenatal care off-base. We find that mothers and babies receiving off-base care have fewer severe complications. There is also a substantially lower rate of unplanned readmissions within 30 days of discharge. Our results suggest that, at current levels, shifting childbirth from on- to off-base is likely to be cost effective.

Our paper proceeds as follows. Section I provides background on the MHS and a comparison to the Veterans Health Administration (VHA). Section II discusses our data, while Section III describes our empirical strategy. Section IV shows results, and Section V concludes.

Part I: Institutional Background

The Military Health System (MHS) is the primary insurer for all active-duty military, their dependents, and many military retirees through the TRICARE program. Of the nearly 9 million patients covered by TRICARE, only 20% are actively serving in the US military. TRICARE is not involved in health care delivery in combat zones and operates separately from the Department of Veterans Affairs' Veterans Health Administration (Schoenfeld *et al.* 2017). For TRICARE enrollees, care can be delivered in one of two ways: either directly at Military Treatment Facilities (MTFs) on military bases (direct care), or purchased from private providers (purchased care).

The 42 MTF hospitals in the U.S. provide both outpatient and inpatient care to TRICARE enrollees.[6] MTF care represents the classic public delivery mechanism. All resources are owned by one of the branches of the U.S. military. Individual MTFs are managed by active-duty military officers who draw a salary that is not tied to the performance of their MTF. Their incentives to perform well come from a sense of duty as service-members and physicians, and from a desire to advance their careers and attain positions of greater responsibility. Medical departments or services, such as OBGYN, are led by physicians or other providers as appointed by the MTF commander (HQDA, 2004).

For those treated at MTFs, the care is delivered by a mix of providers including active-duty military providers (49 percent), federal civilian employee providers (40 percent), and providers hired using contract mechanisms who work full time at the MTF (11 percent) (Defense Health Program, 2018). Providers are primarily salaried with payment not explicitly tied to either quantity or quality of care delivered.[7] In almost every aspect, care at MTFs approximates the type of publicly provided care delivered in the UK and other systems with public health care delivery.

Alternatively, TRICARE enrollees can receive "purchased care" outside the MTF. This care is delivered by a network run by a contracting insurer—United HealthCare, in the case of our sample

[6] An additional 73 locations provide solely outpatient care. All bases in our data include at least an outpatient clinic.
[7] Military providers are on a military pay scale, while civilian providers are on the federal General Schedule (GS); both pay systems include base pay, and local cost of living adjustments. Contractors are flexibly hired to meet local needs. The contracting process solicits competitive bids to provide a service for an annual salary—once the contract is awarded the MTF commander does not have authority to alter its terms or to provide additional awards (DoD, 2019).

period—where patients go to private providers within the insurer's contracted network. This purchased care system very much parallels the public purchase of private care, such as through the Medicare Advantage program. However, contractors for TRICARE are not paid a capitated rate per beneficiary per month. They are instead paid for their services and manage payment to providers on a per-claim basis. Therefore, by comparing direct to purchased care, we are comparing care provided directly by the government to care provided by private contractors paid by the government.

Providers under purchased care are paid Medicare rates, and all inpatient stays are subject to bundling into a single Diagnosis Related Group (DRG) based on age, sex, diagnoses, procedures, and discharge status using the Medicare Grouper. Individual discharges may also be further reimbursed for high cost and length-of-stay outliers. The TRICARE Reimbursement Manual notes that, while this system is functionally identical to Medicare's prospective payment system, weights and payments may vary because of the characteristics and needs of the beneficiaries (OASD, 2018). While Medicare is associated with people over the age of 65, it also provides care for Americans with permanent disabilities or end-stage renal disease. According to *Medicare.org*, "there are over 1 million female Medicare recipients under the age of 65." Therefore, Medicare rates are applicable to childbirth services.

The network of MTFs is maintained primarily to ensure military readiness. The MHS is organized with three principal goals: to maintain the health of uniformed personnel, to provide medical professionals that can deploy worldwide, and to provide a medical benefit to active-duty personnel, retirees, and their families. In practice, the families of uniformed personnel require more medical procedures, and this is reflected in the composition of the most common directly provided and purchased procedures. In 2013, six of the seven most common procedures provided at MTFs were related to delivery. From most to least common, these were vaccination, circumcision, repair of obstetric laceration, Cesarean section, manually assisted delivery, and two different procedures for the induction of labor. Similarly, four of the top seven privately provided procedures purchased by the MHS were related to childbirth. In order these were: manually assisted delivery, total knee replacement, Cesarean section, circumcision, vaccination, repair of obstetric laceration, and insertion of a stent. In terms of volume, 12,400 C-sections were performed on-base in 2013, while 20,400 were purchased from private providers by the MHS.

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

Importantly, moves are not under the control of active-duty enrollees. Moves within the military are driven by Department of Defense personnel management strategies, motivated by staffing needs across units. As personnel leave or are promoted, units must be reorganized so that each formation has the appropriate composition. Official policy is that these decisions are made according to "current qualifications and ability to fill a valid requirement," and this takes priority over all other considerations, including individual preferences or time at current locations. Furthermore, assignments "will not be influenced by the employment, school enrollment, volunteer activities, or health of a Service member's family member" (DoD 2015). Several papers have demonstrated that the timing and frequency of relocations is not subject to soldier preferences, including Lyle (2006), Lleras-Muney (2010), Carter and Skimmyhorn (2017), and Carter and Wozniak (2018). While the choice of location may be influenced by Soldier preferences, these choices are constrained by the timing of the move, and the sponsor's rank and occupation. Accordingly, we follow Lleras-Muney (2010) in controlling for time, rank, and occupation throughout our estimates. Importantly, we argue only that moves across bases are orthogonal to family health-care system preferences. Moves within a catchment area may be endogenous since they are unlikely to be driven by personnel management needs; we therefore exclude such moves from our analysis. A related concern is the relationship between distance to the nearest MTF and preferences over health care delivery, since all mothers in our sample exercise a degree of control over their residential locations relative to the base of assignment. To allay concerns, we control for distance to the base and its square in all of our main specifications.

Part II: Data

Our data come from the Military Health System Data Repository, a comprehensive set of health records collected at MTFs and from civilian providers reimbursed under TRICARE. The data are comprised of several separate repositories, including administrative eligibility files and separate sets of claims data for direct and purchased care, both inpatient and outpatient. While TRICARE does not pay claims for direct care, the data are structured as if they were claims and include detailed information such as physician identifiers, diagnosis and procedure codes, length of hospitalization, and measures of treatment intensity. The sample period covers fiscal years 2003 through 2013.

In our comparison of the care provided at MTF hospitals versus civilian facilities, we focus on inpatient care for childbirth, for a number of reasons. First, we have a very large sample of births, as this is the most common type of hospital admission for the MHS population. Second, many women in the sample period deliver more than once, allowing us to control for unobservable heterogeneity by focusing on within-mother differences in outcomes across births. Third, the delivery context affords us the ability to explore both birth-specific measures of treatment intensity, like the prevalence of C-sections, and detailed health outcomes, such as preventable complications. Finally, admission for birth is nearly universal among pregnant women, minimizing the potential for selection into the sample on the dimension of whether to receive care at all.

We construct a sample consisting of the wives of active-duty service-members to facilitate the comparability of on and off base care. This sample limitation is important for two reasons. First, we exclude active-duty service members (who account for 21 percent of births) because, as Frakes and Gruber (2019, 2020) document, they face different medical malpractice liability environments which would limit our ability to extrapolate our findings to other settings. We use the active-duty partner's rank, occupation, and race since these characteristics are not in the data for dependents. Second, we remove retirees and their dependents (who account for only 3.5 percent of births) since under TRICARE rules they have different rules for cost sharing on- and off-base. We also remove all transfers from on to off-base or vice versa.

After restricting to mothers who give birth at least twice in our sample window, and who move across military installations between births (for reasons discussed below), we have a sample of 87,005 women who are admitted for labor and delivery 182,779 times. Out of these admissions, 55 percent are off-base, while 45 percent are at MTF hospitals.

We use several variables to measure treatment intensity. The most important is Relative Weighted Product (RWP), an encounter-specific metric created by the MHS to compare the intensities of inpatient resource use across the direct- and purchased-care systems. This measure is a function of both the DRG weight associated with the admission and the length of the inpatient stay. A RAND study found an 80 percent correlation between RWP and cost for surgical cases (Farley *et al.* 1999). The use of this RWP measure is necessary because, while the MHS records cost allocations to each inpatient stay both on and off the base, they only record charges for stays off the base.[8] In addition to RWP, we consider separately the rate of C-sections, the number of additional (non-C-section) procedures, the length of stay for mothers (bed days) and newborns (bassinet days), and the number of diagnostic procedures.[9]

In addition to treatment intensity, we consider several measures of care quality and patient outcomes. First, we consider a measure indicating the presence of any condition from a list of severe complications, and a similar indicator for any of a list of preventable complications. Complications are very common during childbirth, occurring in more than two-thirds of admissions in our sample. For example, the single most common is trauma or lacerations of the perineum, occurring in 40 percent of admissions. However, this complication is rarely present in cases of delivery by C-section. Therefore we construct indicators for severe and preventable complications. Severe complications are a set of dangerous complications identified by the World Health Organization, including for example severe preeclampsia, a potentially life-threatening condition. Other complications classified as severe are third stage hemorrhage, rupture of uterus, and sepsis. Note that these complications are not prevented by C-sections. Preventable complications include bleeding problems, such as post-partum hemorrhage and post-partum coagulopathy, and several conditions of labor such as unusually fast delivery, called precipitate delivery, or unusually long labor. Some of these latter preventable complications can be

[8] We winsorize RWP at the 99th percentile to reduce the influence of outliers.

[9] Specifically, we follow the list of diagnostic procedures coded in Frakes and Gruber's (2019) analysis of diagnostic testing during MHS inpatient stays. As explained in Frakes and Gruber (2019), some form of continuous electronic fetal heart monitoring has become so common during labor and delivery now that the accessible CPT codes in the MDR database do not allow us to distinguish deliveries with and without this particular diagnostic tool. Accordingly, our diagnostic analysis can best be seen as capturing diagnostic tools other than continuous electronic fetal heart monitoring.

mitigated using C-sections. Severe and preventable complications are not mutually exclusive—bleeding problems are considered both severe and preventable. We also consider a measure called severe acute maternal morbidity, or SAMM. SAMM consists of life-threatening conditions for the mother such as "embolism, acute renal failure, stroke, acute myocardial infarction," and others. Callaghan *et al*. (2012), who developed an updated version of the measure, estimate that SAMM is roughly 100 times more common than death. Maternal mortality outcomes were, fortunately, too rare to offer precise estimates.

We are, however, able to measure neonatal mortality. Typically, infant mortality is defined as death during the first 28 days after birth. To consider a longer-term outcome, we also measure infant mortality within one year of birth. Third, we look at unplanned readmissions of the mother within 30 days of discharge. Unplanned readmissions are inpatient admissions to short-term acute care hospitals within 30 days of discharge from an "index admission" (CMS 2016).[10] We also consider unplanned readmissions of the child within 30 days and within one year of discharge. Given the importance of child health to understanding quality of care, we also consider *any* hospital admission for the child within the same time frames.

One disadvantage of limiting our analysis to birth admissions is that pre-hospitalization care—prenatal care—affects birth outcomes. This is potentially problematic to the extent women may receive on-base prenatal care and deliver off-base, or vice versa. Indeed, there is a sizeable share of such

[10] We use a slightly modified version of the CMS "Planned Readmission Algorithm (Version 5.0)." We consider all admissions for birth to be index admissions. We exclude readmissions on the same day, where CMS would include these if the primary diagnosis was different. If there is more than one readmission within 30 days, only the first is considered a readmission. Therefore we have a dummy variable for each birth admission that indicates whether the *mother* has an unplanned readmission. A planned readmission is "defined as a non-acute readmission for a scheduled procedure," and a few procedures are always classified as planned, including organ transplants, chemotherapy, and rehabilitation. It is important to note that admissions for either acute conditions or for complications are never categorized as planned.

"mixed care". Of women who live within 40 miles of an MTF and who deliver on-base and for whom we can clearly identify pre-natal care, 22% get at least some of their pre-natal care off-base; conversely, of women who live more than 40 miles from an MTF and deliver off-base, 20% get at least some of their pre-natal care on-base.[11] We do not have a separate instrument for receipt of pre-natal versus delivery care, so we are treating these as a package test of public-versus-private delivery. Below, we show separately results for the three-quarters of our sample for whom all pre-natal and childbirth care is delivered within the same system. We also attempt to assess the relative role of these two factors by looking at outcomes mostly determined by pre-hospital care (low birthweight and premature births) and by looking at mothers who move during pregnancy.

Sample Generalizability

To understand the nature of selection into our sample, we compare the women in our sample to those in the National Health Interview Survey (NHIS) using similar sample selection criteria—married, recent mothers from the same time periods. The results are presented in Table A1. In comparison to the NHIS sample, we find that women in our sample are on average three years younger, and less than half as likely to have a baby with low birth weight. We also find that the husbands of women in our sample are less likely to be White, more likely to be Black, and less likely to have a four-year college degree, as proxied by status as a military officer.

Part III: Empirical Strategy

[11] This is partly because there are a number of military bases that don't have an MTF but do have some medical services.

The objective of this paper is to determine whether treatment intensity and quality differ between the “direct” (public) and “purchased-care” (private) systems—i.e., between MTF-based care / management and private-based care / management. We frame this empirical comparison by considering off-base private care the treatment, making those who give birth at MTFs the untreated group. The key inferential challenge for this exercise is that selection to purchased care is non-random. Simply comparing outcomes of direct and purchased-care encounters will be confounded by observable and non-observable differences between patients.

Finkelstein *et al.* (2016) develop an approach to address this concern in the context of Medicare spending. They face a similar challenge: they are interested in determining the extent to which geographic area differences in spending are due to true differences in area characteristics as opposed to the selection of patients who choose to live in those areas. Their approach is to create a sample of Medicare movers and to compare the change in movers spending based on the differences in average area spending in their ex-ante and ex-post locations. In their context, the concern is that such moves are endogenous – e.g. that unobservably sicker people will move to high spending areas. To address this, Finkelstein *et al*. (2016) study the outcomes of movers before they move – showing that those who move to more and less expensive areas are quite similar before the move but differ thereafter in ways correlated with area expense. We can apply this same approach to the context of discrete medical events - with the further advantages that the timing of military moves is exogenous, and the set of locations is constrained by military requirements.

Fundamentally, both approaches rely on the logic of differences-in-differences; as Finkelstein *et al.* state, the “key identifying assumption is that such differential trends do not vary systematically with the migrant’s origin and destination.” However, our context has several significant advantages when compared to Finkelstein *et al.* (2016). First, and perhaps most important, we do not need to assume that moves to one location type have the same effect on individuals from more than one other location type. We consider only one treatment (private care) relative to one untreated control (public care), while Finkelstein *et al.* (2016) estimate 305 separate healthcare market treatments. Second, we do not rely on non-movers to identify effects on movers. Finkelstein *et al.* must assume that the place effects that are relevant for movers are the same as those that are relevant for non-movers. Finally, given our sample restrictions, we know *a priori* that moves in our data are induced by military relocations, which gives us a plausibly exogenous (non-health-related) source of moves across location types. By contrast, Finkelstein *et al.* "cannot allow for shocks to utilization that coincide exactly with the timing of the move and that are correlated with utilization in the origin and destination," for example, newly diagnosed patients could respond to illness by moving to areas with higher utilization. We will show that, conditional solely on initial location type, we appear to have conditional random assignment to subsequent location types.

In particular, let $Y_{it}(d)$ be potential outcome Y for individual i in period t, given treatment status d, where $d = 0$ if a woman lives within 40 miles of an MTF hospital, and $d = 1$ for women who live more than 40 miles away from an MTF hospital.[12] For any outcome Y, we can measure the impact of losing access to an MTF hospital as $Y_{i1}(1) - Y_{i0}(0)$. Likewise, we can define the impact of gaining access to an MTF hospital as $Y_{i1}(0) - Y_{i0}(1)$.

[12] We utilize this 40-mile specification as the baseline to account for situations in which mothers live close to two bases. If the closest base does not have an MTF hospital but the second closest-base does, and the mother still lives within 40 miles of the MTF hospital on this second-closest base, we treat this scenario as one in which the mother has access to an MTF hospital. Alternatively, as discussed below, we specify location as taking on a value of 1 if the base closest to where the mother lives has an MTF. We find similar results across both approaches.

There are two fundamental threats to identification in this context. The first is that movers and non-movers are not comparable. As we argued above, moves are assigned by the military and should not be endogenous to individual characteristics once we have controlled for time, occupation, and rank. But it is conceivable that pregnancy could impact the timing of moves. Moreover, it is possible that moving itself impacts birth outcomes. We can readily address both of these concerns by restricting our analysis only to mothers who move between births.

The second is the potential endogeneity of move location, despite the institutional factors that suggest exogenous move timing. To assess this possibility, we pursue the discrete analogy to the event-study approach of Finkelstein *et al*. (2016) and examine the *previous* birth of movers. That is, if those movers who are high risk are systematically moved to a base with an available base hospital, then this should show up in those same mothers having higher-risk first births.

On a final note regarding potential endogeneity of moves, we restrict our inquiry to mothers with multiple births who move between births to a new base location. That is, the variation we capture in whether patients live within 40 miles of a base hospital derives from moves across bases that differ in their availability of a base hospital, rather than from internal moves within bases that cross this 40-mile threshold. Such within-base moves are more likely to be endogenous with respect to treatment decisions since they are not tied to military reassignments.

Given these sample restrictions, we have four groups:

(a) mothers who move from a base close to an MTF hospital to a different base close to an MTF hospital

(b) mothers who move from a base close to an MTF hospital to a base far from an MTF hospital

(c) mothers who move from a base far from an MTF hospital to a different base far from an

MTF hospital

(d) mothers who move from a base far from an MTF hospital to a base close to an MTF hospital

To transparently illustrate the power of this approach, we begin by showing simple comparisons of our key measures for these four sets of possible move combinations. We start with the sample of mothers whose first birth occurs when they live within 40 miles of an MTF hospital; within this sample, we then compare those whose second birth takes place after they move within 40 miles of a different MTF hospital, group (a), to those whose second birth takes place after they move more than 40 miles away from any MTF hospital, group (b). Likewise, we consider a sample of mothers whose first birth occurs when they live more than 40 miles from an MTF hospital; within this sample, we then compare those whose second birth takes place after they move more than 40 miles from a different MTF hospital, group (c), to those whose second birth takes place after they move to within 40 miles of an MTF hospital, group (d). We also estimate the parameters implied by these comparisons using the following differences-in-differences specification:

(1) $$Y = \alpha \cdot Switched + \gamma \cdot SecondBirth + \lambda \cdot Offbase \times SecondBirth + \varepsilon$$

where Y is the outcome variable, $Switched$ is an indicator for having moved from a base with an MTF hospital to one without an MTF hospital or vice-versa, second birth is an indicator for the post-move birth, and λ is the parameter identified by this simple difference-in-differences. We argue that λ is causally identified, and that this is the reduced form for the instrumental variables specification that follows.

Of course, our approach has one other key difference from Finkelstein *et al*. (2016), which is that we are only measuring an intent-to-treat effect, since some of our movers will be close to military hospitals but still use non-military providers. To address this, we can implement an instrumental variables strategy. In particular, the reduced form is the following formalization of our simple intent-to-treat:

(2) $$\Delta Y_{it} = \alpha_i + \gamma_t + \beta \Delta D_{it} + \delta D_{it-1} + \eta \Delta X_{it} + \mu Z_i + \varepsilon_{it}$$

where ΔY_{it} is the change in outcome Y for individual *i* at period *t* relative to period $t-1$. ΔD=0 for groups (a) and (c), $\Delta D = 1$ for group (b), and $\Delta D = -1$ for group (d). We include fixed effects for calendar year, γ_t, and control for time-varying observable birth characteristics (as of the post-move birth) in X_{it}: age group, sponsor's pay grade. In a specification check, as discussed below, X_{it} also includes dummies for various maternal risk-factors. We also control for time-invariant characteristics (occupation and race of the sponsor) using Z_i. This reduced form is causally identified under the usual parallel trends assumption between groups (a) and (b) and between groups (c) and (d).[13]

Given this framework, we can estimate second stage estimates of the form:

(3) $$\Delta Y_{it} = \alpha_i + \gamma_t + \beta \Delta O_{it} + \delta O_{it-1} + \eta \Delta X_{it} + \mu Z_i + \varepsilon_{it}$$

where ΔO_{it} is equal to 1 for women who give birth off-base in period $t = 1$ and on-base in period $t = 0$. ΔO_{it} is equal to -1 for women who give birth at an MTF in period $t = 1$, but who give birth off-base in period $t = 0$. To address the fact that choice of delivery location is endogenous, we instrument the endogenous regressor ΔO in the equation above. To identify the coefficient of interest, β, we use two instruments:

$\mathbf{1}(\Delta D = 1)$ is an indicator for moving from within 40 miles of an MTF to a new location more than 40 miles from an MTF.

[13] Under these assumptions, Hull's Proposition 1 (2018) states that the "binary treatment mover regression identifies a convex combination of average treatment effects, across time and the two mover groups."

$\mathbf{1}(\Delta D = -1)$ is an indicator for moving from a location that is more than 40 miles from an MTF to a new location within 40 miles of an MTF.

These instruments use the plausibly exogenous variation in MTF access from move location as an instrument for the actual location of the birth. Moreover, the restriction of our model to incorporate symmetric responses to moves both away from and towards military facilities allows us to implement an overidentification test of our estimates.

For this instrumental variable estimate to be causally identified, we must assume that the exclusion restriction holds. In our context this means that the only way that a military-reassignment-induced change in distance from an MTF hospital affects a change in birth outcomes is by causing a change in birth location on- or off-base – and not other area factors that might drive outcomes when individuals move. To assess this issue, Table A2 compares key features of the catchment areas with and without MTFs available, using both information on births from vital statistics, and population from the American Community Survey (ACS). We find that birth characteristics are very similar across the two types of areas, with very similar age at birth and rates of prematurity, and slight differences in low birth weight. The areas with MTFs do have significantly lower rates of tobacco use, which would suggest if anything that outcomes should be better in those areas (and not worse as we find). As far as population characteristics, the areas with MTFs are slightly higher income, higher share black, and have higher education and lower poverty rates. The major differences, not surprisingly, is a higher share of the population with TRICARE insurance and a higher share of veterans, and that the areas with MTFs are significantly larger with more births.

While area characteristics may differ, TRICARE beneficiaries are unique in that their access to comprehensive health insurance is unaffected by moves, and the active-duty sponsor's compensation is also unaffected by location.[14] Additionally, for the majority of couples, the active-duty husband is the main income provider. During this period, roughly 60% of military wives did not work, and "80% of military wives [earned] less than $30,000" (Rapino and Beckhusen, 2013). We are also reassured by the robustness of our results to controlling for the volume of area surgical inpatient cases.[15] Thus, not only are area differences modest, military families are insulated from some of the effects of moves relative to their civilian counterparts.

We show the first stage for this IV model in Table A3. Both instruments are highly significant with nearly equal and opposite signs. The F-statistic for the set of instruments is 575, well above the standard suggested by Lee *et al*. (2020). The first stage itself shows us that nearly two-thirds of the sample are compliers with the instrument. We can also use the first stage to estimate the proportion of the treated that are compliers—this is a relatively high 62%. By assumption, the remainder of the sample is split between always-takers (who would always give birth off-base), and never-takers (who would always find a way to give birth on-base, even if they must travel to a MTF hospital). We assume no defiers. Such individuals would only give birth off-base if the nearest base had an MTF-hospital. Such behavior would not only be implausible, but it would also violate the monotonicity of our instrument.

We further characterize compliers in Table 1. We can see that compliers have slightly fewer bed days and lower utilization. In large part, this is because compliers have a C-section rate that is almost three percentage points below average. Importantly, compliers have higher rates of low birth weight,

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[14] While base pay is entirely unaffected, supplemental pays such as Basic Allowance for Housing (BAH) and Cost of Living Adjustment (COLA) are intended to equalize disposable income regardless of area cost of living.
[15] See Table A8 for this specification. All of our results are robust except for the reduction in unplanned readmissions, which is no longer statistically significant when controlling for area surgical volume.

and higher rates of infant mortality measured at both 28 days and at one year. We can also see that compliers are slightly older, are almost nine percentage points less likely to have White sponsors and are three percentage points more likely to have Black sponsors.

Whether complier effects are relevant to policy discussions depends on the policy space under consideration. Most relevant is the discussion of marginal capacity changes at MTFs which shift some births across MTF and outside hospitals. For this discussion, it is unlikely that always-takers or never-takers would be affected, so that our complier estimate is the appropriate one. On the other hand, for more significant changes such as shutting down delivery services at MTFs, the total effect would be the sum of the effects for compliers and for never-takers, but since the majority of this sample would be compliers, it would likely be fairly similar to what we estimate here.

We also consider two alternate specifications in Table A5.[16] The first uses the change in whether the mother lives close to an MTF hospital as a single instrument. That is to say, we use ΔD directly, which has possible values of 1, 0, and -1. Results are nearly identical to those with two instruments. The second specification includes separate coefficients on indicators for inclusion in group (b) and group (d). This estimate uses the same two instruments as our main specification but relaxes the imposition of symmetric effects on move types. The effect of moving to or from a private hospital appears largely symmetric, especially the effects on utilization as measured by bed days, relative weighted product, and C-sections. We further examine heterogeneity between move types in Table A6.

Part IV: Results

[16] The first stages for these alternate specifications are shown in Table A4—these are also highly significant.

Mean Outcomes

As discussed above, we restrict the analysis to mothers with only two births who move between births to a new base location. Tables 2A and 2B illustrate balance for our sample at the first birth before moving. This table has rows for each first-birth outcome, broken into sections for utilization and health outcomes. The first four columns focus on those whose first birth occurs when they live within 40 miles of an MTF hospital. In the first two columns, we compare first-birth means of the indicated measures for mothers whose second birth takes place after they move within 40 miles of a different MTF hospital (group (a)) with those for mothers whose second birth takes place after they move more than 40 miles away from any MTF hospital (group (b)); the third and fourth columns shows the difference in the relevant first-birth outcome between the two groups and its t-statistic. Likewise, in the second set of four columns, we consider the sample of mothers whose first birth occurs when they live more than 40 miles from an MTF hospital, and compare first-birth outcomes of those whose second birth takes place after they move more than 40 miles from a different MTF hospital (group (c)), to those whose second birth takes place after they move within 40 miles of any MTF hospital (group (d)). Finally, the last column shows the t-statistic on a test of the difference between the two estimates in the third and seventh columns. Given the multiple hypotheses being tested in this table, we compare these t-statistics to sup-t critical values as suggested by Olea and Plagborg-Møller (2019).

This table is generally consistent with balance between our treatment and comparison groups in the first birth before moving. Consider for example a key measure of outcomes, Cesarean section deliveries. The C-section rate for first births is almost equal for those who live near an MTF hospital for that first birth and then move, regardless of whether that moves takes them to a new location near an MTF hospital (23.6%) or to one not near an MTF hospital (23.2%); the difference of 0.4% has a t-statistic that is only half of the critical value for significance at the five percent level. Likewise, the C-section rate is nearly equal for those whose first birth occurs when they live in a location that is far from an MTF and then move, regardless of whether that move takes them to a new location far from an MTF hospital (25.1%) or to one that is near to an MTF hospital (25.4%); the difference of 0.3% has a t-statistic that is less than one, well short of statistical significance. And these two estimates are very similar, with the t-statistic on the difference between the estimates smaller still than either difference alone. It is notable that the estimates are higher for the first birth for those whose first birth is not near an MTF, but this cannot be interpreted causally since initial location is potentially endogenous.

The evidence of balance is consistent for all measures of utilization in the case of the first comparison, where we focus on those who live near an MTF for the first birth and then move. In the case of the second comparison, we do find significant first-birth differences for several measures (bed days, other procedures and diagnostics); however, in two of these measures, the differences in first-birth means are opposite in direction to the corresponding differences in second births, suggesting that any selection bias arising in who moves may bias *against* our ultimate findings.

The next panel focuses on outcomes. We find that outcomes are balanced for the first birth when that birth occurs at a base with an MTF. Most outcomes are also balanced for first births occurring at bases without MTFs, except for hospital admissions in the first year of life. Babies born to mothers who subsequently move to bases with MTFs are admitted to the hospital at somewhat higher rates, though there is no difference within the first 30 days of discharge after birth. The difference in later hospitalizations could reflect differences in underlying health, or it could be a result of the subsequent move to a base with an MTF.

Tables 3A and 3B carry out a parallel analysis, but now focused on second births to provide our, arguably, causal estimates of delivering off-base. The results here are significant for most measures of utilization and many outcomes, suggesting higher rates of utilization when deliveries are off-base. For example, for C-section delivery, we estimate that among those who live near an MTF hospital for that first birth and then move, if they move to another location near an MTF hospital, their C-section rate for the second birth is 27.7%, but if they move to a new location that is not near an MTF hospital, their C-section rate is a much higher 29.5%; the difference of 1.9% is highly significant. Likewise, for those whose first birth occurs when they live in a location that is far from an MTF hospital, we find that if they move to a new location that is also far from an MTF hospital, their C-section rate is 29.8%, whereas if they move to a new location that is near an MTF hospital, their C-section rate is a much lower 28.4%; the difference of 1.4% is also significant, even accounting for multiple hypothesis testing.

For utilization, where there was either no significant differences in first births or differences in the opposite direction, there is now significance in every single case for second births, other than for bassinet days in the second comparison. This provides clear evidence that when mothers give birth without access to an MTF hospital, their utilization is significantly higher.

For outcomes, we find highly significant outcomes for both comparisons for all measures of complications suggesting lower rates of complications when deliveries are off-base. The impacts are sizeable, with the rate of severe complication being less than two-thirds as large for those who move away from an MTF hospital rather than staying near an MTF hospital, and almost one-third lower for those who stay away from an MTF hospital rather than moving close to one. We do not find any significant impact on infant mortality, although this is partly due to imprecision; the estimates imply a large reduction in both 28-day and one-year infant mortality for those moving away from an MTF hospital. And we see a large and significant reduction in unplanned readmissions for those who move away from an MTF hospital or those who stay away from an MTF hospital.

Overall, this clear and transparent presentation of the data yields two lessons. First, for those mothers who move, an observation of first-birth characteristics poses few concerns over selection bias compromising our investigation into the effects of off-base care. That is to say: post-move access to type of care (public or private) does not consistently predict pre-move outcomes. Second, the evidence suggests that living far from an MTF hospital leads to higher rates of utilization, and potentially better outcomes as well.

If moves to and from bases indeed provide an exogenous source of variation in MTF hospital access, then one would tend to expect estimates of similar size but opposite magnitude when drawing separately on moves away from MTF hospitals and on moves towards MTF hospitals. In Column 9 of Table 3B, we offer a preliminary test of this prediction. With respect to various measures—e.g., C-sections—we cannot reject a symmetrical response (in absolute value terms) between a move that provides MTF hospital access and a move that removes it. However, in other cases—e.g., severe complications—we can reject same absolute effect sizes between these two types, generally finding slightly larger absolute effect sizes when drawing on moves that bring patients farther away from MTF hospitals. In the regression analysis and full IV analysis set forth below, we will formalize this overidentification test, allowing for the inclusion of both covariates and for different first-stage magnitudes across move types.

Regression Analysis

Table 4 presents the results of estimating equation (1) above. We first estimate equation (1) for the sample of mothers whose first birth occurs while assigned to a base with an MTF hospital. These estimates are shown in column 2. We then estimate the same equation for the sample who first give birth while assigned to a base without an MTF hospital. Following this formalization of the simple reduced form implied by Tables 3A and 3B, we estimate equation (2). We first show OLS estimates, using the endogenous choice of delivering on versus off base. To the extent that this decision is driven solely by location, then the OLS estimates can be interpreted causally; but to the extent that individuals choose their delivery location based on underlying factors correlated with maternal or infant health, these estimates may be biased. We then show IV estimates, along with the over-identification statistic from the two instruments that arise from our two types of comparisons.

The reduced form differences-in-differences estimates illustrate the impact that our two move types have. In particular, bed days, C-sections, and overall utilization are higher in both cases. Note that the increase in utilization is not statistically significant for the group who first give birth without MTF hospital access. However, the presence of severe or preventable complications and the occurrence of unplanned readmissions are lowered for off-base births in both cases. For those starting at a base with an MTF hospital, children's readmissions and hospitalizations are also lowered, at both the 30-day and the 1-year mark. The OLS estimates indicate highly significant impacts of delivering off-base on utilization and outcomes, consistent with the findings from Tables 2 and 3. There are sizeable and significant increases in all utilization measures other than additional (non-C-section) procedures. There are also sizeable and significant reductions in severe and preventable complications and unplanned readmission. The results are generally consistent with the conclusions from Tables 2 and 3: receiving care off-base leads to higher utilization but better outcomes. Infant mortality and hospitalizations, both unplanned and otherwise, within 30 days and 1-year of discharge are lower for babies that are born off-base, although these differences are not statistically significant.

The fifth column of Table 4 shows IV results. The utilization results are once again generally positive, albeit somewhat weaker than with OLS. We find that the total resource measure rises by 0.011, which is roughly 2.2% of the sample mean; that is, overall treatment intensity rises by 2.2%. There is a significant rise of 0.12 bed days, which is more than 5% of the sample mean. Bassinet days rise as well, but the coefficient is much lower than under OLS and is not significant. As with OLS, there is a positive but insignificant impact on other procedures, while diagnostic tests rise by 0.052, which is an effect more than 50% of the sample mean.

Our results are consistent with much of the impacts we see being driven by higher rates of Cesarean delivery off-base. The rate of Cesarean section delivery is 2.8 percentage points higher off-base, which is 10% of the sample mean. The average admission with a C-section has a RWP 57% higher than that with no C-section. Therefore, our observed change in Cesarean delivery alone implies a 1.7% increase in RWP. The remaining increase in treatment intensity can be attributed to increased length of stay.

The outcome results are also generally sizeable and significant, indicating across the board reductions in bad outcomes. The probability of severe birth complication is reduced by 0.021, which is more than 55% of the sample mean. Importantly, the rate of severe birth complications is comparable for admissions with probable C-sections and admissions where C-section is unlikely. We explore this sample split fully in Table 7. The reductions in preventable complications of 8.4 percentage points amount to more than 40% of the sample mean. Note that, while many preventable complications can be mitigated by C-section, the percentage point reduction in preventable complications exceeds the percentage point increase in C-sections, implying that additional measures are lowering the rate of preventable complications for off-base deliveries. Severe Acute Maternal Morbidity (SAMM) has a negative point estimate equivalent to 17% of the mean rate, but this is not statistically significant.

Unplanned readmissions fall by 5 per 1000, which is about 40% of the sample mean. Infant mortality falls substantially as well, and by even more than indicated by OLS, but the coefficient is smaller than its standard error. Infant mortality at one year, however, has a positive point estimate, but this estimate is one-third of its standard error. The lack of concrete evidence on mortality complicates the interpretation of on and off-base quality.

In most cases, our IV estimates have the same sign as the OLS estimates, but a smaller magnitude. This implies that the OLS estimates are partially a result of selection bias. For example, the OLS estimate suggests that C-sections are 3.6 percentage points more prevalent for off-base admissions, while the IV estimate is 2.8 percentage points. If mothers with a higher propensity for C-sections are also more likely to go off-base, then this will bias the OLS estimate upward. A notable exception to this pattern is the reduction in severe and preventable complications. Here the magnitude is similar or even greater, suggesting that there is little effect of selection bias on these outcome measures. Reductions in hospitalizations are also larger in the IV specification, suggesting that selection bias is working in the opposite direction—that is to say, babies with a higher propensity for hospitalization are more likely to be born off-base. This is consistent with their mothers having a higher propensity for C-sections if we consider hospitalization a combined measure of health and of utilization.

These sizeable outcome results imply that the delivery of care off-base is cost effective. For example, the average amount allowed for C-section versus non-C-section deliveries off-base is

$2,422.39 (in 2020 dollars). Multiplied by the increased rate of C-sections, this is $70.25 per admission. At the same time, the average readmission cost for our sample is $15,563. A 0.5% reduction in readmissions therefore saves $78.26 per delivery – larger than the costs of increased utilization. This ignores any additional health benefits from fewer complications, and any cost savings related to lower rates of hospitalization for the child after the delivery. This cost-effectiveness comparison does not account for the possibility of chronic respiratory diseases or other health problems following the first year of birth, which may have differing welfare implications.[17]

The next column shows the p-value of the overidentification test for this IV estimation. In essence, the null hypothesis for this test is that we identify the same parameter when using moves away from MTF hospitals as an instrument and when using moves towards an MTF hospital as an instrument. Encouragingly, in every case but two, the overidentification test passes; there are marginal rejections at the 10% level for severe complications and for unplanned child hospitalizations within one year.

Specification Checks

We consider two specification checks of these results in Table 5. First, in columns 2 and 3, we assess the impact of including controls for diagnoses. These are all conditions that would generally necessitate a C-section, including the following: previous C-section, and change in breech presentation, multiple birth, umbilical cord prolapse, placenta previa, and placental abruption. This allows us to capture any observable health differences across the comparison groups, but at the same time the

[17] See Card et al. (2020) for evidence on this point. We were unable to find evidence of chronic illness in the children born to compliers in our sample, perhaps due to insufficient power.

coding of these conditions is done by the treating physician and may itself be endogenous to source of care. In any case, including these controls has virtually no impact on the results.

Second, in columns 5 & 6, we parallel the analysis from Tables 2A and 2B by looking at prior births. That is, we estimate equation (2), but change the outcome to a level, rather than a first difference, and we use the level of the outcome from the previous birth admission. Strikingly, we find *no* significant coefficients for previous births, and most of these tests pass their associated overidentification tests. These findings demonstrate sample balance at first births among movers, regardless of their ultimate moving destination. Consistent with the conclusions we drew from Tables 2A and 2B, these findings ease concerns over selection bias in the composition of moves associated with changes in MTF access.

We consider two additional specification checks in Table 6. First, we consider an alternative way of assessing “closeness”. A strong finding in the health economics literature is that individuals are much more likely to go to the hospital nearest to them (e.g. McClellan *et al*, 1994). We therefore recreate our instrument not based on whether there is an MTF hospital within 40 miles, but rather whether the nearest base has an MTF hospital. While correlated with our primary instrument, it is not collinear, with a correlation coefficient of 0.79. Yet, as the next two columns show, the results are quite similar.

Finally, we focus explicitly on the 75% of our sample that receives both pre-natal and delivery care from the same system, as opposed to those receiving mixed care. As the final columns of Table 6 show, such a restriction strengthens our findings. In particular, measures of utilization are uniformly higher, including a 3.7 percentage point increase in C-sections, relative to the 2.8 percentage point increase from our main results. This raises the natural question of the relative importance of pre-natal versus delivery care, which we address in the following section.

Mechanisms

The results so far demonstrate that delivering at an off-base hospital leads to higher utilization but arguably better outcomes. In this section, we explore some mechanisms that might be driving this result.

Testing the C-section explanation

There is an impressive increase in C-section rates for mothers who deliver in private hospitals that can account for much of the cost increase that we see – but can it account for the better outcomes? The literature on C-sections and outcomes is mixed (Card, Fenizia & Silver, 2020).

While we can't condition our results on receipt of a C-section, which is endogenous, we can split the sample on the individual mother's probability of having a C-section at the current birth admission. This probability is calculated using the fitted values from a linear probability model that uses calendar year, age group, sponsor's pay grade and race, and medical factors including previous C-section, breech presentation, multiple birth, umbilical cord prolapse, placenta previa, and placental abruption.[18] We consider the first and fourth quartiles of individual C-section probability. Of those in the lowest probability group, 6.2% ultimately do have a C-section, while those in the higher probability group have

[18] Given the dependence of second birth delivery type on first birth delivery type, we also explore the dependence of our results solely on previous C-section and split by whether the mother had a prior C-section in Tables A11 and A12. Here we see that 90% of mothers with previous C-section undergo C-sections, and that off-base admissions increase the proportion by 6.6 percentage points relative to on-base admissions. The implication is that provider willingness to perform vaginal births after previous C-sections is higher for on-base admissions.

an 88.3% C-section rate. See Figure 1 for an illustration of the effect of off-base admission on C-sections for each quartile. While C-section rates are raised for off-base admissions in each quartile, the quartile with the largest individual propensity for C-section also has by far the largest off-base effect.

The results for these quartiles are shown in Table 7. Our estimates show that higher utilization is driven in part by higher C-section rates off-base. Increased bed days are concentrated among the group with a higher probability of C-section. Similarly, the increase in RWP is concentrated in this group. However, the group with a lower probability of C-section experiences an increase in (non-C-section) procedures for off-base birth admissions. Notably, the reductions in preventable complications are large and statistically significant for both groups, though these are reduced more for the group with a lower C-section propensity. In contrast to Ranjit *et al.* (2017), who use patient observables to adjust on-and off-base comparisons and find that “purchased care had higher odds of SAMM [severe acute maternal morbidity] complications for cesarean delivery,” we find that, among mothers with a high probability of C-section, SAMM is reduced by nearly one percentage point, or more than one third of the mean. We also find that potential reductions in infant mortality are concentrated in the group with a higher probability of C-section, though this is not statistically significant. In sum, our most convincing reductions in preventable complications and unplanned readmissions are not concentrated among those with a high propensity for C-sections.

We can further this exploration by using area variation in the difference between C-section rates on and off base. Figure 2 shows the difference in risk-adjusted C-section rates between each specific MTF and the (birth weighted average) of all private hospitals used by those living in the MTF catchment area.[19] As expected, the mass of this distribution lies below zero, with lower C-section rates at MTF bases, but there is a wide distribution. If our effect is driven by areas with a larger differential, then it suggests that it is indeed the higher rates of C-section in the private sector that is causing our result. The fourth quartile of this distribution includes areas with no difference in average risk-adjusted C-section rates for MTFs and area hospitals. Importantly, for this quartile there is precisely no difference in C-sections caused by admission to off-base hospitals.[20]

Table 8 shows the results of our main estimating equation separately for areas in the first and fourth quartiles of the risk-adjusted C-section rate differential distribution. We find that most utilization results are in fact higher in areas where the rate differential is larger. Compared to our main results, results for bed days, RWP, number of procedures and number of diagnostic procedures, as well as the probability of C-section are all larger for the first quartile, which has a more extreme rate difference. When we look at quality measures, we find that unplanned child readmissions are substantially less likely off-base in areas with a larger C-section rate differential. However, we find that complication rates, including severe and preventable complications, are lowered by similar amounts in both the first and the fourth quartiles. This implies that higher utilization off-base is driven by higher C-section rates, but that higher C-section rates are not the only determinant of better quality in the private sector.

<u>Pre-Natal Care</u>

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[19] The risk-adjusted rate is calculated by subtracting the expected rate from the observed rate. The expected rate is calculated using fitted values from a regression including calendar year, race, pay grade, prior C-section, breech presentation, multiple birth, cord prolapse, placenta previa, placental abruption, and age group.

[20] See Figure 3 for an illustration of how this effect varies across quartiles.

As noted earlier, one open question is whether this result is driven by changes in prenatal care or by changes in hospital care. We attempt to separate these mechanisms in two ways in Table 9. First, we look at measures of infant outcomes that should be driven primarily by prenatal care: low birth weight and premature births, which are any births occurring prior to 37 weeks gestational age. For both of these outcomes, the primary medical determinants are related to prenatal care, with hospital decisions having little impact. In the first three columns of Table 9, we show the results of estimating equation (2) for these alternative measures. Despite finding strong impacts on a variety of outcome measures in Tables 4 and 5, we find no effect on these measures, suggesting that the impacts documented in Tables 4 and 5 are not arising from changes in prenatal care. At the same time, even if some of the impact is arising through prenatal care, we may simply not have enough power to detect effects on these outcomes.

We therefore complement this approach with a second approach in Columns 4—9 of Table 9: examining separately outcomes for mothers who move during versus before pregnancy. Just as the location of moving is exogenous subject to rank, occupation, and year, the timing of moving should be exogenous as well. Therefore, if the impacts we estimate are determined by prenatal care differences, we should see weaker impacts for those who move during pregnancy. In fact, the results are quite comparable, with the notable exception of basinett days, which are longer for those who moved before pregnancy. There is no pattern suggesting that either move timing results in consistently weaker or stronger effects. This suggests once again that it is hospital care differences, not prenatal care differences, that are driving our results.

<u>Differences in Treating Physicians</u>

A second question is what makes on-base hospital care different. There are a wide variety of differences between MTF hospitals and non-MTF hospitals, ranging from the types of doctors that practice there to the management to the compensation structure. We cannot decompose all these alternatives; however, we do attempt to shed light on whether our findings arise due to one obvious difference between on-base and off-base doctors: the fact that much of the on-base physician workforce are active-duty military. Active-duty doctors may differ from civilian providers—whether civilian providers working on-base or off—for various reasons, including their career progression concerns, the chance that they may be deployed to combat zones, the frequency by which they may be reassigned locations and their general professional dispositions.

To explore this matter, we exploit the fact that care at MTF hospitals is provided by a mix of active-duty physicians and contracted private sector physicians. We then test for differential off-base effects (estimating our main model) depending on the share of on-base birth admissions in a given year and a given MTF that have an attending provider who is active-duty military; specifically, we split the on-base sample by above- and below-median active-duty OBGYN share. This share ranges from zero to one in our data, with a median of 0.55. Variation is due to a variety of factors, including the moves of military physicians, who are relocated by the Department of Defense every several years, as well as specialty-specific retention and the needs and preferences of each military branch. For example, Army facilities average 42% military OBGYNs, while Navy facilities have a higher average of 54%. Importantly, this proportion is not a function of the quantity of procedures performed each year; there is little correlation between this share and number of inpatient deliveries.

The results from this sample split are shown in Table 10. In fact, the evidence is consistent with the notion that our effects are larger at MTF with a larger share of active-duty OBGYNs. Most results are stronger in those cases, including a much higher response of unplanned readmissions. None of the differences are statistically significant, but the results suggest that differences in physician practice styles may be driving our results.

Differences in Hospital Volume

Another potential explanation is that hospitals with a higher throughput of births will tend to have better outcomes. We test this explanation in Table A8 with controls for hospital volume. We use the annual number of surgical inpatients in 2004 as a proxy for hospital volume throughout the period considered and find that results are robust to this control. We also consider the total annual volume of inpatient surgical cases in the 40-mile area surrounding the base, and again find that results are robust.

Heterogeneity

Heterogeneity by Move Type

Since we are leveraging two move types, we can consider these separately. In spirit, this is similar to the split in Tables 2 and 3 by the starting location. That is to say, the control group for mothers who move away from an MTF are mothers who move from a base with an MTF to another base with an MTF. Similarly, the control group for mothers who move to a base with an MTF from a base without one are those mothers who move between two bases that lack a hospital-level MTF.

In Table A6 we make these comparisons separately and find quite similar results across the two types of moves. We do find a significant difference in the effect of move type on hospitalizations for children in the first year following discharge, which are lowered more for mothers moving away from MTF hospitals; additionally, while 30-day readmissions are lower in both cases, this difference is only significant for mothers that move away from bases without MTF hospitals. Overall, however, our most important results, including overall utilization, increased bed days, diagnostic procedures, and C-sections, and reductions in severe and preventable complications, appear to be symmetric.

Comparison to Card, Chan, and Taylor (2020)

A new working paper by Card, Chan, and Taylor (2020) undertakes similar analysis to ours in the context of Veteran's Administration (VA) hospitals. They consider a different population, patients receiving emergency care, and follow Doyle *et al*. (2015) in using exogenous variation in the propensity of ambulance companies to bring patients to VA versus private hospitals. Their findings are similar in some ways and different in others. While they do find higher resource utilization in private hospitals, the welfare consequences are ambiguous. Indeed, they suggest that VA hospitals provide superior care to eligible patients.

Their work differs from ours in several ways. First, the patient population is substantially different. We focus on dependents of active-duty military personnel treated through the MHS. The Veterans Health Administration (VHA) provides direct and purchased care to military veterans. The VHA does not typically serve the families of veterans, though some are eligible for specific events or as caregivers. Veterans are eligible for VHA services if they served honorably for at least two years, and have a service-connected disability; there are additional criteria based on service-connected disability ratings, income levels relative to geographically-adjusted limits, and combat experience or other service-connected trauma. Therefore, VHA clients are older, poorer, and more likely to suffer from disabilities than the average American. By comparison, the patient population served by the MHS is on average young and healthy; for example, 48% of VHA beneficiaries are over age 65, while only 24% of TRICARE beneficiaries use TRICARE-for-Life (Farmer *et al.* 2018).[21]

Part V: Conclusions

Health care in all developed nations is delivered by a mix of public and private systems. A central question for health care reform around the world is therefore the proper mix of public-versus-private delivery. But addressing this question is challenging since it requires comparing individuals across different health care systems who choose their location of care and therefore may differ in unobservable ways.

By studying the treatment of childbirth within the MHS, we can potentially address this shortcoming. The MHS is the largest broadly-mixed health care delivery system in the U.S., providing us with a large sample of births and excellent data with which to study treatments and outcomes. Drawing on births from the same mother before and after exogenous military moves, along with heterogeneity across bases in the availability of base hospitals, we are able to provide convincing estimates of the impact of private-versus-public delivery systems. We find that delivering off-base leads to higher treatment intensity – but better outcomes. The magnitudes of our findings imply that the better off-base care is cost effective.

This is a particularly timely finding given policy debates in the U.S. around the proper role of public-versus-private systems. In the near term, the MHS is planning to greatly reduce manpower at public facilities, while the Veterans Administration is considering allowing much broader use of private

[21] Our data do not include ambulance company information for on-base admissions. We also do not have sufficient data to learn much about emergency department admissions because there are only 302 such cases at MTF hospitals in our sample.

providers to deliver care. More broadly, discussions of moving from a mixed public-private system to a purely public system have become much more prominent in the U.S. Our findings provide some initial evidence that can help inform these debates.

There are a variety of important next steps to further understand public-versus-private delivery of health care in the U.S. and around the world. Extending this work to other types of treatments is an obvious extension, although this likely requires alternative identification strategies given the infrequency of multiple comparable treatments for the same patient. Addressing this issue in an international context may be particularly important given the strong role played by public-versus-private health care systems in other nations.

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Table 1: Descriptive Statistics

	Mean	SD	25th Pctl	75th Pctl	2nd Birth Mean	2nd Birth Complier Mean
Bed Days	2.315	1.890	2	3	2.159	2.097
Bassinet Days	2.467	4.535	1	2	2.361	2.142
RWP	0.499	0.154	0.392	0.695	0.503	0.490
Procedures	2.256	1.263	1	3	2.134	2.191
Diagnostics	0.085	0.748	0	0	0.100	0.106
C-Section	0.262	0.440	0	1	0.282	0.252
Severe Complication	0.041	0.199	0	0	0.036	0.031
Preventable Complication	0.223	0.416	0	0	0.189	0.210
Severe Acute Maternal Morbidity	0.011	0.103	0	0	0.012	0.013
28-Day Infant Mortality (per 1,000)	1.118	33.251	0	0	0.804	0.935
1-Year Infant Mortality (per 1,000)	2.126	45.777	0	0	1.576	2.266
Unplanned Readmission (per 1,000)	12.372	110.540	0	0	12.089	10.499
Unplanned Child Readmission (per 1,000)	37.855	190.846	0	0	36.080	33.891
Any Child Hospitalization within 30 Days (per 1,000)	55.886	229.702	0	0	53.917	55.197
1-Year Unplanned Child Hospitalization (per 1,000)	85.562	279.717	0	0	84.343	97.857
Any 1-Year Child Hospitalization (per 1,000)	103.472	304.576	0	0	102.091	119.412
Low Birth Weight	0.033	0.178	0	0	0.031	0.034
Premature	0.057	0.232	0	0	0.057	0.050
Age	26.933	4.844	23	30	28.454	28.654
White	0.777	0.416	1	1	0.779	0.693
Black	0.104	0.305	0	0	0.102	0.132
N Admissions	182,777				95,774	
N Mothers	87,004					

Notes: The sample consists of mothers who give birth at least twice, and who move across bases between births. *Bed Days* is the mother's length of stay. *Bassinet Days* is the child's length of stay, or the average length of stay for a multiple birth. *RWP* is the Relative Weighted Product, a measure designed by the Military Health System (MHS) to capture relative treatment intensity. *Procedures* is the count of non-diagnostic procedure codes. *Diagnostics* is the number of purely diagnostic procedures. *C-Section* is a dummy equal to one for mothers who gave birth via Cesarean section. *Severe Complication* is a dummy equal to one when an admission includes a diagnosis for any of the following conditions: postpartum hemorrhage, severe pre-eclampsia, eclampsia, rupture of uterus, sepsis, or septicemia. *Preventable Complication* is a dummy equal to one when admission includes a diagnosis for any of the following conditions: fetal distress affecting management of mother, abnormality in fetal heart rate or rhythm, postpartum hemorrhage, long labor, uterine inertia, precipitate labor, or shoulder dystocia. *Severe Acute Maternal Morbidity (SAMM)* indicates conditions such as hemorrhage, embolism, acute renal failure, stroke, and heart attack. *Infant Mortality* is missing when mothers are not covered by TRICARE. *Unplanned Readmission* is categorized according to the CMS algorithm. *Low Birth Weight* is an indicator for birth weight below 5 pounds, 8 ounces (2,500 grams). *Premature* is an indicator for delivery prior to 37 weeks gestational age. *Age* is the age of the mother in years at the time of delivery. *White* and *Black* are indicators for the race of the active-duty sponsor.

Table 2A: First Birth Means Starting with MTF Access

	Starting with MTF Access			
	(1)	(2)	(3)	(4)
	MTF/ MTF	MTF/ No MTF	Difference	t-stat
Panel 1: Utilization				
Bed Days	2.535	2.530	-0.005	0.262
Bassinet Days	2.596	2.613	0.018	0.388
RWP	0.496	0.496	-0.001	0.478
Procedures	2.492	2.486	-0.005	0.389
Diagnostics	0.062	0.059	-0.003	0.513
C-Section	0.236	0.232	-0.004	1.088
Panel 2: Outcomes				
Severe Complication	0.053	0.056	0.004	1.757
Preventable Complication	0.292	0.287	-0.005	1.222
Severe Acute Maternal Morbidity	0.010	0.010	0.000	0.366
28-Day Infant Mortality (per 1,000)	1.420	1.462	0.042	0.116
1-Year Infant Mortality (per 1,000)	2.543	2.775	0.232	0.472
Unplanned Readmission (per 1,000)	13.876	15.372	1.496	1.332
Unplanned Child Readmission (per 1,000)	40.705	42.311	1.606	0.837
30-Day Child Hosp. (per 1,000)	60.493	62.194	1.701	0.738
1-Year Unplanned Child Hosp. (per 1,000)	88.807	90.795	1.988	0.722
Any 1-Year Child Hosp. (per 1,000)	108.266	111.018	2.752	0.916
Sup-T Critical Value (0.05)				2.153
Share Giving Birth Off-Base	0.266	0.267		
N	31,671	17,581		

Notes: Column 1 shows means for first-birth outcomes for mothers who lived within 40 miles of an MTF hospital, and who moved to within 40 miles of an MTF hospital at a different base at the time of their second birth. Column 2 shows first-birth means for mothers who also lived within 40 miles of an MTF hospital, but who moved to a different base and were no longer within 40 miles of an MTF hospital at the time of their second birth. Column 3 lists the difference between these groups. Column 4 gives the t-statistic from a t-test where the null hypothesis is that the means in column 1 and column 2 are the same. The sup-t critical values reported at the bottom adjust for multiple hypothesis testing as suggested by Olea and Plagborg-Møller (2019).

Table 2B: First Birth Means Starting without MTF Access

	Starting Without MTF Access				(3)+(7)=0
	(5)	(6)	(7)	(8)	(9)
	No MTF/ No MTF	No MTF/ MTF	Difference	t-stat	t-stat
Panel 1: Utilization					
Bed Days	2.475	2.519	0.045	2.465*	1.767
Bassinet Days	2.620	2.622	0.002	0.046	0.372
RWP	0.497	0.498	0.001	0.316	0.130
Procedures	2.316	2.289	-0.027	2.020	2.007
Diagnostics	0.068	0.088	0.020	2.879*	2.285*
C-Section	0.251	0.254	0.003	0.719	0.287
Panel 2: Outcomes					
Severe Complication	0.041	0.040	-0.001	0.312	1.348
Preventable Complication	0.233	0.225	-0.008	1.687	2.506*
Severe Acute Maternal Morbidity	0.008	0.010	0.002	2.095	2.128
28-Day Infant Mortality (per 1,000)	1.286	1.223	-0.063	0.161	0.035
1-Year Infant Mortality (per 1,000)	3.025	2.194	-0.831	1.480	0.888
Unplanned Readmission (per 1,000)	10.304	11.125	0.820	0.731	1.803
Unplanned Child Readmission (per 1,000)	36.819	38.574	1.755	0.820	1.439
30-Day Child Hosp. (per 1,000)	53.166	55.085	1.919	0.757	1.295
1-Year Unplanned Child Hosp. (per 1,000)	78.160	85.546	7.386	2.394*	2.700*
Any 1-Year Child Hosp. (per 1,000)	94.112	101.807	7.696	2.306*	2.787*
Sup-T Critical Value (0.05)				2.168	2.154
Share Giving Birth Off-Base	0.971	0.971			
N	14,967	19,820			

Notes: Columns 5 and 6 show first-birth outcome means for mothers who did not live within 40 miles of an MTF hospital. Mothers in column 5 moved to a different base, but remained more than 40 miles away from an MTF hospital, while those in column 6 moved to within 40 miles of an MTF hospital at a different base by the time of their second birth. Column 7 lists the difference between the means in columns 5 and 6, while column 8 reports the t-statistic for a t-test where the null hypothesis is that there is no difference. The t-statistics reported in column 9 are from a t-test where the null hypothesis is that the sum of columns 3 (from Table 2A) and 7 is zero. The sup-t critical values reported at the bottom adjust for multiple hypothesis testing as suggested by Olea and Plagborg-Møller (2019).

Table 3A: Second Birth Means

	Starting with MTF Access			
	(1)	(2)	(3)	(4)
	MTF/ MTF	MTF/ No MTF	Difference	t-stat
Panel 1:				
Utilization				
Bed Days	2.132	2.225	0.092	5.192*
Bassinet Days	2.294	2.433	0.139	3.460*
RWP	0.501	0.509	0.008	5.205*
Procedures	2.122	2.183	0.062	5.564*
Diagnostics	0.084	0.122	0.038	4.977*
C-Section	0.277	0.295	0.019	4.422*
Panel 2: Outcomes				
Severe Complication	0.041	0.026	-0.015	8.411*
Preventable Complication	0.214	0.151	-0.063	17.070*
Severe Acute Maternal Morbidity	0.013	0.011	-0.002	1.954
28-Day Infant Mortality (per 1,000)	0.948	0.780	-0.168	0.587
1-Year Infant Mortality (per 1,000)	1.749	1.680	-0.069	0.174
Unplanned Readmission (per 1,000)	13.179	10.731	-2.448	2.356*
Unplanned Child Readmission (per 1,000)	38.314	32.363	-5.951	3.301*
30-Day Child Hosp. (per 1,000)	59.012	48.031	-10.981	4.997*
1-Year Unplanned Child Hosp. (per 1,000)	89.333	80.275	-9.058	3.346*
Any 1-Year Child Hosp. (per 1,000)	109.814	95.761	-14.052	4.764*
Sup-T Critical Value (0.05)				2.153
Share Giving Birth Off-Base	0.297	0.983		
N	31,671	17,581		

Notes: Column 1 shows means for second-birth outcomes for mothers who lived within 40 miles of an MTF hospital at the time of first birth, and who moved to within 40 miles of an MTF hospital at a different base. Column 2 shows second-birth outcome means for mothers who also lived within 40 miles of an MTF hospital at the time of first birth, but who moved to a different base and were no longer within 40 miles of an MTF hospital. Column 3 lists the difference between these groups. Column 4 gives the t-statistic from a t-test where the null hypothesis is that the means in column 1 and column 2 are the same. The sup-t critical values reported at the bottom adjust for multiple hypothesis testing as suggested by Olea and Plagborg-Møller (2019).

Table 3B: Second Birth Means

	Starting Without MTF Access				(3)+(7)=0
	(5)	(6)	(7)	(8)	(9)
	No MTF/ No MTF	No MTF/ MTF	Difference	t-stat	t-stat
Panel 1:					
Utilization					
Bed Days	2.211	2.143	-0.068	3.230*	1.541
Bassinet Days	2.443	2.392	-0.051	0.997	1.963
RWP	0.508	0.504	-0.005	2.534*	2.219*
Procedures	2.186	2.097	-0.089	6.969*	1.315
Diagnostics	0.111	0.088	-0.023	2.636*	2.019
C-Section	0.298	0.284	-0.014	2.867*	1.272
Panel 2: Outcomes					
Severe Complication	0.031	0.039	0.008	3.980*	3.731*
Preventable Complication	0.150	0.205	0.055	13.118*	3.454*
Severe Acute Maternal Morbidity	0.010	0.011	0.001	1.294	0.618
28-Day Infant Mortality (per 1,000)	1.056	0.728	-0.327	1.023	1.412
1-Year Infant Mortality (per 1,000)	1.619	1.613	-0.006	0.014	0.166
Unplanned Readmission (per 1,000)	8.246	14.010	5.764	4.977*	2.236*
Unplanned Child Readmission (per 1,000)	32.737	36.725	3.988	1.956	1.173
30-Day Child Hosp. (per 1,000)	46.217	55.865	9.648	3.935*	0.968
1-Year Unplanned Child Hosp. (per 1,000)	78.767	84.803	6.036	1.981	1.202
Any 1-Year Child Hosp. (per 1,000)	92.645	104.181	11.536	3.486*	1.135
Sup-T Critical Value (0.05)				2.172	2.153
Share Giving Birth Off-Base	0.989	0.318			
N	14,967	19,820			

Notes: Columns 5 and 6 show second-birth outcome means for mothers who did not live within 40 miles of an MTF hospital at the time of first birth. Mothers in column 5 moved to a different base, but remained more than 40 miles away from an MTF hospital, while those in column 6 moved to within 40 miles of an MTF hospital at a different base. Column 7 lists the difference between the means in columns 5 and 6, while column 8 reports the t-statistic for a t-test where the null hypothesis is that there is no difference. The t-statistics reported in column 9 are from a t-test where the null hypothesis is that the sum of columns 3 (from Table 3A) and 7 is zero. The sup-t critical values reported at the bottom adjust for multiple hypothesis testing as suggested by Olea and Plagborg-Møller (2019).

Table 4: Differenced Outcomes

		MTF First	No MTF First	OLS	IV	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	λ	λ	β	β	p-value
Bed Days	2.159	0.097**	0.112***	0.229***	0.116**	0.562
		(0.040)	(0.038)	(0.037)	(0.055)	
Bassinet Days	2.361	0.121	0.053	0.478***	0.089	0.961
		(0.079)	(0.080)	(0.075)	(0.106)	
RWP	0.503	0.009***	0.005	0.014***	0.011***	0.392
		(0.003)	(0.003)	(0.002)	(0.003)	
Procedures	2.134	0.067	0.061	0.032	0.137	0.640
		(0.074)	(0.077)	(0.074)	(0.111)	
Diagnostics	0.100	0.041***	0.043***	0.109***	0.052***	0.732
		(0.011)	(0.015)	(0.010)	(0.018)	
C-Section	0.282	0.023***	0.017*	0.036***	0.028***	0.365
		(0.008)	(0.009)	(0.004)	(0.005)	
Severe Complication	0.036	-0.018***	-0.009***	-0.018***	-0.021***	0.051
		(0.003)	(0.003)	(0.003)	(0.004)	
Preventable Complication	0.189	-0.058***	-0.062***	-0.078***	-0.084***	0.748
		(0.014)	(0.014)	(0.017)	(0.019)	
Severe Acute Maternal Morbidity	0.012	-0.002	0.001	0.001	-0.002	0.162
		(0.002)	(0.001)	(0.001)	(0.002)	
28-Day Infant Mortality (per 1k)	0.804	-0.210	0.264	-0.028	-0.152	0.922
		(0.440)	(0.534)	(0.291)	(0.751)	
1-Year Infant Mortality (per 1k)	1.576	-0.301	-0.825	-0.358	0.320	0.691
		(0.585)	(0.747)	(0.413)	(0.977)	
Unplanned Readmissions (per 1k)	12.089	-3.944***	-4.944***	-5.934***	-5.210**	0.834
		(1.378)	(1.753)	(1.416)	(2.247)	
Unplanned Child Readmission (per 1k)	36.080	-7.557***	-2.233	-3.189	-4.368	0.246
		(2.463)	(3.596)	(3.169)	(5.339)	
30-Day Child Hospitalization (per 1k)	53.917	-12.683***	-7.729*	-5.880	-16.682***	0.777
		(3.295)	(4.393)	(3.741)	(5.958)	
1-Year Unplanned Child Hospitalization (per 1k)	84.343	-11.046**	1.350	-4.043	-6.058	0.068
		(4.583)	(5.539)	(3.971)	(6.038)	
Any 1-Year Child Hospitalization (per 1k)	102.091	-16.804***	-3.840	-7.020	-18.754***	0.175
		(4.704)	(6.344)	(4.207)	(6.694)	
N		74,320	93,758	95,773	95,773	

Notes: Column 1 shows mean outcomes for the second birth. Outcomes in columns 2 and 3 are current values, and outcomes in columns 4-6 are the first difference between the current and prior birth admission. All mothers in the sample moved across bases between births. Columns 2 and 3 show simple difference-in-difference estimates for the effect of living greater than 40 miles from an MTF hospital for each move type—either to or away from an MTF hospital. This is a version of the reduced form for our IV estimates and includes no controls. Column 4 shows OLS estimates of the effect of a change in off-base admission between the current and prior birth. Possible values for the change in off-base are 1 (for a change from on to off-base), 0 (for no change), and -1 (for a change from off to on base). This specification includes an indicator for whether the prior birth took place off base. Column 5 shows 2SLS estimates where the change in off-base admission is instrumented with indicators for a move from MTF hospital to no MTF hospital, or from no MTF hospital to MTF hospital. MTF hospital is defined as the zip code of residence being within 40 miles of a hospital-level MTF with inpatient services. All moves are across bases. Column 6 reports the p-value for Hansen's J statistic, where the null hypothesis is that both instruments identify the same parameter. The previous birth location indicator is also instrumented with an indicator for whether the mother lived within 40 miles of an MTF hospital. Specifications in columns 4-6 include controls for calendar year, sponsor's occupation and race, and change in age group, sponsor's pay grade, and distance to the nearest base and its square. Standard errors are clustered at the base level.

Table 5: Risk Factor Controls and Lagged Outcomes

		Baseline	Risk Factor Controls		Lagged Outcomes	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	β	β	p-value	β	p-value
Bed Days	2.159	0.116**	0.125**	0.505	-0.026	0.521
		(0.055)	(0.056)		(0.023)	
Bassinet Days	2.361	0.089	0.110	1.000	0.115	0.715
		(0.106)	(0.105)		(0.071)	
RWP	0.503	0.011***	0.011***	0.149	0.000	0.039
		(0.003)	(0.003)		(0.002)	
Procedures	2.134	0.137	0.138	0.680	-0.009	0.068
		(0.111)	(0.111)		(0.026)	
Diagnostics	0.100	0.052***	0.055***	0.772	-0.004	0.049
		(0.018)	(0.018)		(0.009)	
C-section	0.282	0.028***	0.029***	0.146	-0.001	0.038
		(0.005)	(0.005)		(0.007)	
Severe Complication	0.036	-0.021***	-0.021***	0.050	0.004	0.683
		(0.004)	(0.004)		(0.003)	
Preventable Complication	0.189	-0.084***	-0.084***	0.983	0.001	0.144
		(0.019)	(0.019)		(0.005)	
Severe Acute Maternal	0.012	-0.002	-0.002	0.171	0.000	0.329
Morbidity		(0.002)	(0.002)		(0.001)	
28-Day Infant Mortality	0.804	-0.152	-0.136	0.931	-0.003	0.551
(per 1,000)		(0.751)	(0.749)		(0.587)	
1-Year Infant Mortality	1.576	0.320	0.336	0.698	-0.021	0.341
(per 1,000)		(0.977)	(0.982)		(0.712)	
Unplanned Readmissions	12.089	-5.210**	-5.150**	0.857	0.143	0.485
(per 1,000)		(2.247)	(2.247)		(1.191)	
Unplanned Child	36.080	-4.368	-4.372	0.252	0.726	0.393
Readmission (per 1,000)		(5.339)	(5.369)		(2.601)	
30-Day Child	53.917	-16.682***	-16.188***	0.760	1.348	0.439
Hospitalization (per 1k)		(5.958)	(6.012)		(2.762)	
1-Year Unplanned Child	84.343	-6.058	-6.000	0.069	-3.718	0.022
Hospitalization (per 1k)		(6.038)	(6.061)		(4.075)	
Any 1-Year Child	102.091	-18.754***	-18.211***	0.169	-3.381	0.029
Hospitalization (per 1k)		(6.694)	(6.698)		(4.040)	
N		95,773	95,773		95,773	

Notes: All specifications except in columns 5-6 use the first difference. Coefficients estimate the effect of an off-base birth relative to an on-base birth. The sample includes military wives who moved across bases between births. All regressions include controls for calendar year, sponsor's occupation and race, and change in age group, sponsor's pay grade, and distance to the nearest base and its square. Column 2 shows our baseline IV estimates from Table 4. Column 3 adds indicators for previous C-section, and for change in breech presentation, multiple birth, umbilical cord prolapse, placenta previa, or placental abruption. Outcomes in column 5 are the level from the previous birth. Standard errors are clustered at the base level. P-values are for a Hansen's J statistic where the null hypothesis is that both instruments identify the same parameter.

Table 6: Alternate Instrument and Single System

		Baseline	Alternate Instrument		One System	
	(1) Mean	(2) β	(3) β	(4) p-value	(5) β	(6) p-value
Bed Days	2.159	0.116** (0.055)	0.202*** (0.075)	0.350	0.185*** (0.055)	0.977
Bassinet Days	2.361	0.089 (0.106)	-0.003 (0.140)	0.343	0.263** (0.110)	0.410
RWP	0.503	0.011*** (0.003)	0.010** (0.004)	0.322	0.015*** (0.003)	0.554
Procedures	2.134	0.137 (0.111)	0.253 (0.189)	0.357	0.165 (0.116)	0.831
Diagnostics	0.100	0.052*** (0.018)	0.046* (0.025)	0.948	0.081*** (0.016)	0.956
C-section	0.282	0.028*** (0.005)	0.027*** (0.008)	0.732	0.037*** (0.005)	0.465
Severe Complication	0.036	-0.021*** (0.004)	-0.017** (0.007)	0.881	-0.018*** (0.005)	0.056
Preventable Complication	0.189	-0.084*** (0.019)	-0.058 (0.036)	0.855	-0.084*** (0.020)	0.639
Severe Acute Maternal Morbidity	0.012	-0.002 (0.002)	-0.001 (0.003)	0.600	-0.001 (0.002)	0.314
28-Day Infant Mortality (per 1,000)	0.804	-0.152 (0.751)	-0.410 (0.924)	0.285	-0.349 (0.775)	0.964
1-Year Infant Mortality (per 1,000)	1.576	0.320 (0.977)	0.394 (1.328)	0.052	0.445 (1.030)	0.882
Unplanned Readmissions (per 1,000)	12.089	-5.210** (2.247)	-1.136 (3.419)	0.524	-5.869** (2.698)	0.566
Unplanned Child Readmission (per 1,000)	36.080	-4.368 (5.339)	-13.694* (8.090)	0.083	-7.917 (5.090)	0.251
30-Day Child Hospitalization (per 1k)	53.917	-16.682*** (5.958)	-35.785*** (11.407)	0.196	-19.151*** (6.011)	0.562
1-Year Unplanned Child Hospitalization (per 1k)	84.343	-6.058 (6.038)	-21.550* (12.197)	0.092	-8.225 (6.150)	0.095
Any 1-Year Child Hospitalization (per 1k)	102.091	-18.754*** (6.694)	-41.176*** (14.459)	0.101	-19.072*** (6.665)	0.172
N		95,773	95,773		71,361	

Notes: All specifications except in columns 5-6 use the first difference. Coefficients estimate the effect of an off-base birth relative to an on-base birth. The sample includes military wives who moved across bases between births. Column 2 shows our baseline IV estimates from Table 4. Column 3 instruments with the change in whether the nearest base has an MTF hospital, regardless of distance to that base. Column 5 includes women who received all prenatal care and delivered in the same system. All regressions include controls for calendar year, sponsor's occupation and race, and change in age group, sponsor's pay grade, and distance to the nearest base and its square. Standard errors are clustered at the base level. P-values are for a Hansen's J statistic where the null hypothesis is that both instruments identify the same parameter.

Table 7: Heterogeneity by Individual Probability of C-Section

	1st Quartile (P < 7.09%)			4th Quartile (P ≥ 43.54%)		
	(1) Mean	(2) β	(3) p-value	(4) Mean	(5) β	(6) p-value
Bed Days	1.919	-0.038	0.279	2.635	0.423***	1.000
		(0.058)			(0.099)	
Bassinet Days	1.967	0.025	0.641	3.137	0.062	0.419
		(0.114)			(0.293)	
RWP	0.431	0.003	0.842	0.693	0.023***	0.991
		(0.004)			(0.005)	
Procedures	2.169	0.302**	0.634	1.913	-0.178	0.062
		(0.131)			(0.108)	
Diagnostics	0.063	0.027	0.523	0.173	0.086*	0.562
		(0.021)			(0.043)	
C-Section	0.062	0.007	0.746	0.883	0.069***	0.686
		(0.007)			(0.011)	
Severe Complication	0.037	-0.010	0.452	0.038	-0.025***	0.174
		(0.006)			(0.008)	
Preventable Complication	0.223	-0.090***	0.484	0.096	-0.047***	0.977
		(0.027)			(0.015)	
Severe Acute Maternal Morbidity	0.007	-0.001	0.534	0.023	-0.008	0.301
		(0.002)			(0.006)	
28-Day Infant Mortality	0.520	1.043	0.655	1.196	-1.979	0.825
(per 1,000)		(0.998)			(1.578)	
1-Year Infant Mortality	1.431	1.747	0.488	1.848	-1.281	0.263
(per 1,000)		(1.498)			(2.228)	
Unplanned Readmissions	8.194	-8.419***	0.653	18.872	-6.483	0.380
(per 1,000)		(2.963)			(5.408)	
Unplanned Child Readmission (per	37.348	0.513	0.608	33.140	-3.484	0.076
1,000)		(8.495)			(6.017)	
30-Day Child Hospitalization (per	50.482	-3.707	0.823	60.640	-22.916**	0.822
1,000)		(8.811)			(10.655)	
1-Year Unplanned Child	86.174	2.122	0.724	84.648	-3.886	0.024
Hospitalization (per 1,000)		(10.949)			(9.961)	
Any 1-Year Child Hospitalization	99.358	-3.092	0.849	110.884	-14.298	0.289
(per 1,000)		(11.757)			(12.518)	
N		23,942			23,947	

Notes: All estimates considered in this table are the first difference between the current and prior birth admission. Coefficients are for the effect of an off-base birth and are instrumented using moves away from or to areas with an MTF hospital. The sample includes civilian women married to military personnel who moved across bases between the previous and the current birth. Means are shown for the current birth. The first quartile includes birth admissions where the C-section probability is below 7.09%, while the fourth quartile includes those where a C-section is likely to occur at least 43.54% of the time. This probability is the predicted value from a linear probability model using calendar year, age group, distance to hospital, distance to hospital squared, change in sponsor's pay grade and race, prior C-section, breech presentation, multiple birth, umbilical cord prolapse, placenta previa, and placental abruption. We use the preferred instrumented specification and include controls for calendar year, sponsor's occupation and race, and change in age group, sponsor's pay grade, and distance to the nearest base and its square. Standard errors are clustered at the base level. P-values in columns 3 and 6 are for a Hansen's J statistic where the null hypothesis is that both instruments identify the same parameter.

Table 8: Heterogeneity by Risk-Adjusted C-Section Rate Difference for Area MTFs and Hospitals

	1st Quartile (Rate Diff. < -0.050)			4th Quartile (Rate Diff. ≥ -0.006)		
	(1) Mean	(2) β	(3) p-value	(4) Mean	(5) β	(6) p-value
Bed Days	2.117	0.144 (0.130)	0.274	2.227	0.085 (0.099)	0.533
Bassinet Days	2.390	0.089 (0.264)	0.054	2.422	-0.107 (0.216)	0.392
RWP	0.502	0.020*** (0.004)	0.801	0.505	0.002 (0.007)	0.672
Procedures	1.954	0.363** (0.156)	0.195	2.212	0.111 (0.186)	0.931
Diagnostics	0.104	0.044 (0.042)	0.043	0.097	0.028 (0.029)	0.616
C-Section	0.285	0.048*** (0.008)	0.329	0.285	-0.001 (0.012)	0.952
Severe Complication	0.034	-0.021** (0.009)	0.987	0.032	-0.024*** (0.007)	0.065
Preventable Complication	0.175	-0.071*** (0.027)	0.562	0.173	-0.099*** (0.026)	0.073
Severe Acute Maternal Morbidity	0.011	-0.002 (0.005)	0.778	0.012	0.002 (0.003)	0.018
28-Day Infant Mort. (per 1,000)	0.569	-0.990 (1.460)	0.459	1.022	0.519 (1.800)	0.430
1-Year Infant Mort. (per 1,000)	1.184	-0.907 (1.050)	0.672	1.877	1.001 (2.586)	0.982
Unplanned Readmits (per 1,000)	12.347	-3.197 (3.308)	0.426	10.499	0.055 (6.364)	0.737
Unplanned Child Readmission (per 1,000)	37.786	-17.386** (6.970)	0.420	35.506	16.328 (10.553)	0.416
30-Day Child Hospitalization (per 1,000)	57.259	-31.281*** (5.717)	0.157	52.460	-1.052 (11.894)	0.796
1-Year Unplanned Child Hospitalization (per 1,000)	90.485	-28.995*** (10.547)	0.115	82.192	17.969* (10.088)	0.344
Any 1-Year Child Hospitalization (per 1,000)	110.600	-46.326*** (10.400)	0.059	98.535	3.700 (11.257)	0.643
N		22,873			24,974	

Notes: All estimates considered in this table are the first difference between the current and prior birth admission. Coefficients are for the effect of an off-base birth and are instrumented using moves away from or to areas with an MTF hospital. The sample includes civilian women married to military personnel who moved across bases between the previous and the current birth. Mean outcomes are for the current birth. The first quartile includes birth admissions where the difference in C-section probability between the nearest MTF and area private hospitals is less than minus 5.0%, while the fourth quartile includes those where this difference is greater than or equal to -0.6%. See Figure 2 for a histogram of the distribution of area rate differences. This probability is risk adjusted using predicted values from a linear probability model using calendar year, age group, distance to hospital, distance to hospital squared, change in sponsor's pay grade and race, prior C-section, breech presentation, multiple birth, umbilical cord prolapse, placenta previa, and placental abruption. All regressions use the preferred instrumented specification and include controls for calendar year, sponsor's occupation and race, and change in age group, sponsor's pay grade, and distance to the nearest base and its square. Standard errors are clustered at the base level. P-values in columns 3 and 6 are for a Hansen's J statistic where the null hypothesis is that both instruments identify the same parameter.

Table 9: Prenatal Care Tests

	All			Moved In Pregnancy			Moved Before Pregnancy		
	(1) Mean	(2) β	(3) p-value	(4) Mean	(5) β	(6) p-value	(7) Mean	(8) β	(9) p-value
Low Birth Weight	0.031	0.000	0.973	0.032	-0.003	0.828	0.030	0.002	0.853
		(0.003)			(0.005)			(0.004)	
Premature	0.057	0.000	0.836	0.059	-0.004	0.867	0.056	0.003	0.751
		(0.004)			(0.007)			(0.005)	
Bed Days				2.171	0.144**	0.916	2.149	0.101	0.588
					(0.063)			(0.063)	
Bassinet Days				2.395	-0.084	0.657	2.331	0.255*	0.907
					(0.125)			(0.142)	
RWP				0.502	0.012***	0.307	0.504	0.009**	0.902
					(0.003)			(0.004)	
Procedures				2.140	0.135	0.454	2.129	0.143	0.963
					(0.119)			(0.110)	
Diagnostics				0.106	0.064***	0.797	0.096	0.043	0.574
					(0.023)			(0.026)	
C-Section				0.275	0.027***	0.069	0.288	0.029***	0.557
					(0.006)			(0.007)	
Severe Complication				0.034	-0.018***	0.358	0.037	-0.022***	0.105
					(0.005)			(0.005)	
Preventable Complication				0.188	-0.079***	0.601	0.191	-0.084***	0.945
					(0.021)			(0.020)	
Severe Acute Maternal Morbidity				0.011	-0.002	0.890	0.012	-0.002	0.074
					(0.003)			(0.002)	
28-Day Infant Mort. (per 1,000)				0.814	-0.448	0.896	0.796	0.005	0.806
					(1.118)			(0.891)	
1-Year Infant Mort. (per 1,000)				1.570	-1.185	0.526	1.581	1.599	0.924
					(1.436)			(1.299)	
Unplanned Readmits (per 1,000)				11.632	-3.963	0.838	12.492	-6.232**	0.951
					(3.534)			(2.447)	
Unplanned Child Readmit (per 1,000)				37.321	-1.089	0.606	34.992	-7.415	0.100
					(6.548)			(6.149)	
30-Day Child Hosp. (per 1,000)				56.035	-17.479**	0.723	52.058	-16.047**	0.326
					(6.731)			(7.137)	
1-Year Unplanned Child Hosp. (per 1,000)				89.201	-10.549	0.424	80.084	-3.517	0.029
					(7.678)			(7.621)	
Any 1-Year Child Hosp. (per 1,000)				107.789	-25.461***	0.860	97.093	-13.739*	0.047
					(8.121)			(8.170)	
N		95,773			44,777			50,996	

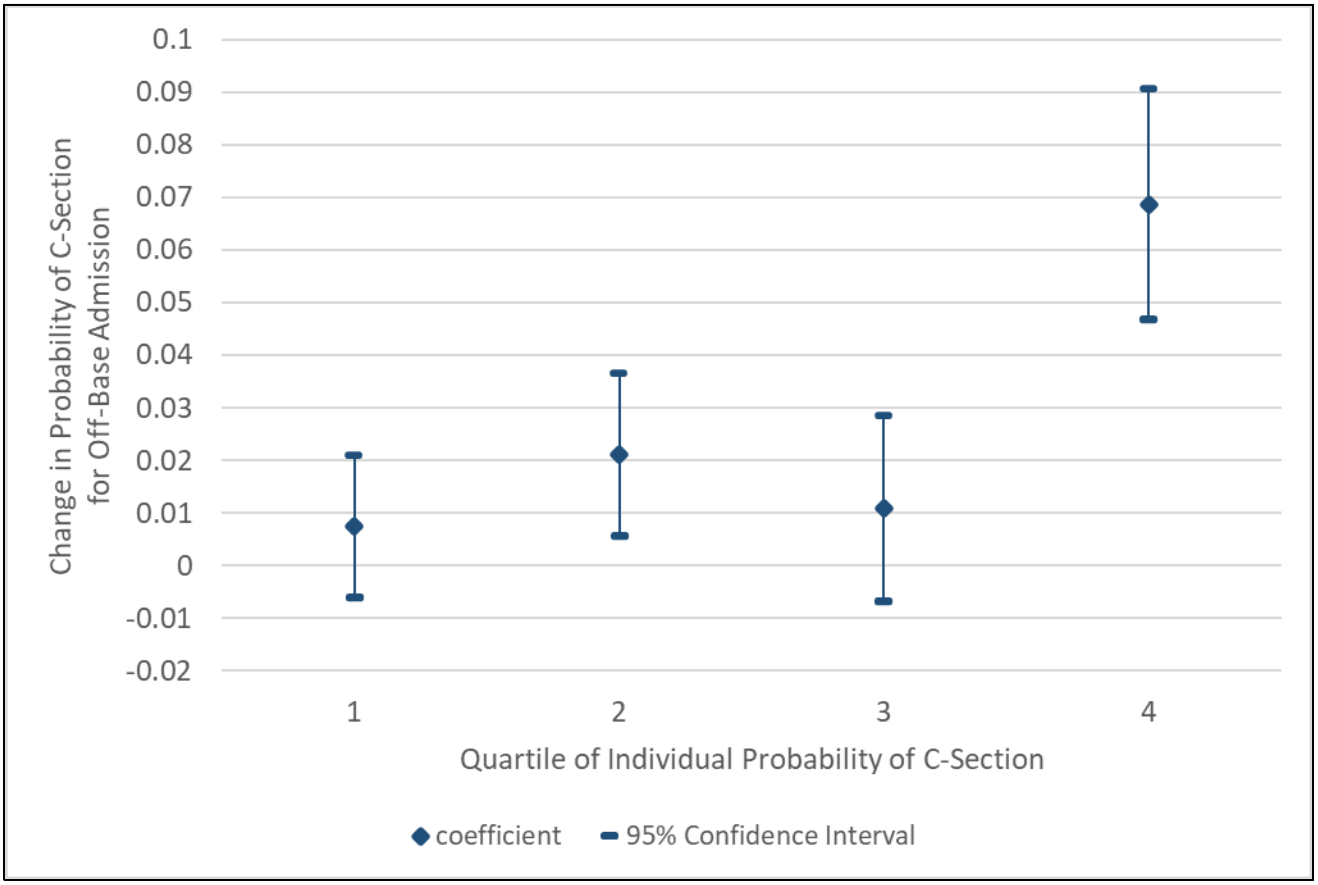
Notes: All estimates considered in this table use the first difference between the current and prior birth admission. Coefficients are for the effect of an off-base birth and are instrumented using moves away from or to areas with an MTF hospital. The sample includes civilian women married to military personnel who moved across bases between the previous and the current birth. Mean outcomes are for the current birth. Columns 2 and 3 estimate the effect of off-base admission on the probability of low birth weight or premature birth using our main specification on the full sample. The remaining columns split the sample into women who moved across (not within) bases during their pregnancy, and women who moved prior to their pregnancy. Moves are defined as changes in the nearest base measured using zip code. All regressions include controls for calendar year, sponsor's occupation and race, and change in age group, sponsor's pay grade, and distance to the nearest base and its square. Standard errors are clustered at the base level. P-values in columns 3, 6, and 9 are for a Hansen's J statistic where the null hypothesis is that both instruments identify the same parameter.

Table 10: Proportion Active-Duty OBGYNs

	Below Median			Above Median		
	(1) Mean	(2) β	(3) p-value	(4) Mean	(5) β	(6) p-value
Bed Days	2.119	0.169** (0.070)	0.049	2.189	0.095 (0.070)	0.404
Bassinet Days	2.348	0.092 (0.167)	0.955	2.359	0.150 (0.115)	0.740
RWP	0.502	0.010** (0.005)	0.205	0.503	0.013*** (0.003)	0.651
Procedures	2.078	0.227* (0.133)	0.188	2.182	0.074 (0.121)	0.028
Diagnostics	0.093	0.061** (0.025)	0.030	0.103	0.051** (0.022)	0.223
C-Section	0.281	0.025*** (0.007)	0.271	0.281	0.035*** (0.007)	0.441
Severe Complication	0.036	-0.023*** (0.007)	0.394	0.036	-0.019*** (0.005)	0.115
Preventable Complication	0.193	-0.075** (0.031)	0.569	0.190	-0.094*** (0.016)	0.820
Severe Acute Maternal Morbidity	0.011	-0.002 (0.003)	0.930	0.012	-0.002 (0.003)	0.138
28-Day Infant Mortality (per 1,000)	0.881	-0.104 (1.181)	0.627	0.724	-0.145 (0.994)	0.397
1-Year Infant Mortality (per 1,000)	1.509	0.530 (1.472)	0.949	1.603	0.658 (1.258)	0.917
Unplanned Readmits (per 1,000)	11.098	-2.683 (3.898)	0.712	12.957	-8.643*** (2.731)	0.385
Unplanned Child Readmit (per 1,000)	36.998	-1.664 (8.322)	0.372	35.932	-5.117 (4.605)	0.686
30-Day Child Hosp. (per 1,000)	54.553	-12.686 (9.900)	0.985	54.393	-16.073*** (5.407)	0.850
1-Year Unplanned Child Hosp. (per 1,000)	86.615	-2.250 (7.771)	0.159	82.892	-8.561 (7.937)	0.214
Any 1-Year Child Hosp. (per 1,000)	103.743	-14.205 (9.878)	0.313	101.438	-19.554** (8.398)	0.337
N		41,348			50,281	

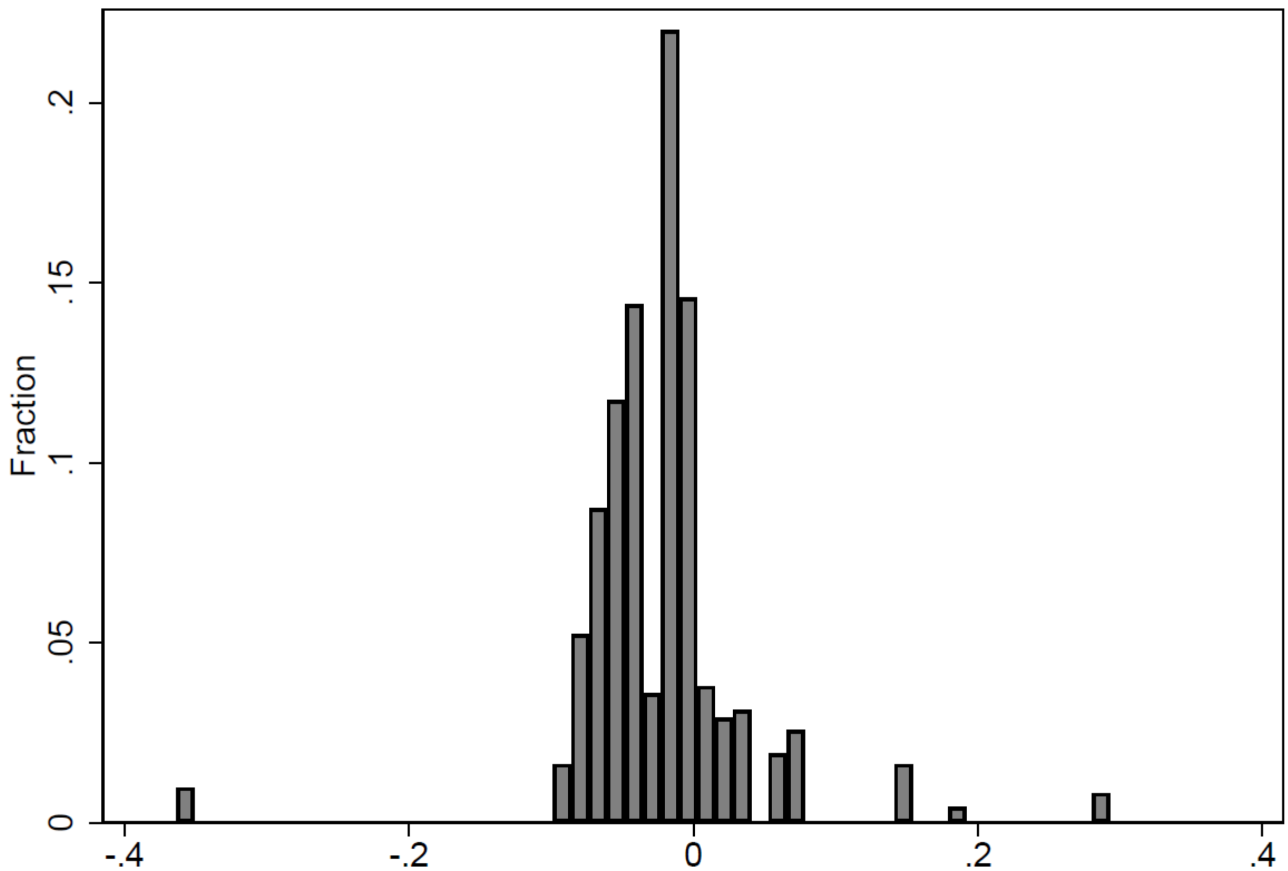
Notes: All estimates considered in this table are the first difference between the current and prior birth admission. Coefficients are for the effect of an off-base birth and are instrumented using moves away from or to areas with an MTF hospital. The sample includes civilian women married to military personnel who moved across bases between the previous and the current birth. Mean outcomes are shown for the current birth. The sample is split by the prevalence of active-duty military OBGYNs at the nearest MTF hospital. The prevalence of active-duty military OBGYNs is measured as the proportion of birth admissions where the attending physician is on active-duty. The median is 54%. All regressions include controls for calendar year, sponsor's occupation and race, and change in age group, sponsor's pay grade, and distance to the nearest base and its square. Standard errors are clustered at the base level. P-values in columns 3 and 6 are for a Hansen's J statistic where the null hypothesis is that both instruments identify the same parameter.

Figure 1: Effect of Off-Base Admission on C-Section by Quartile of Individual Probability of C-Section



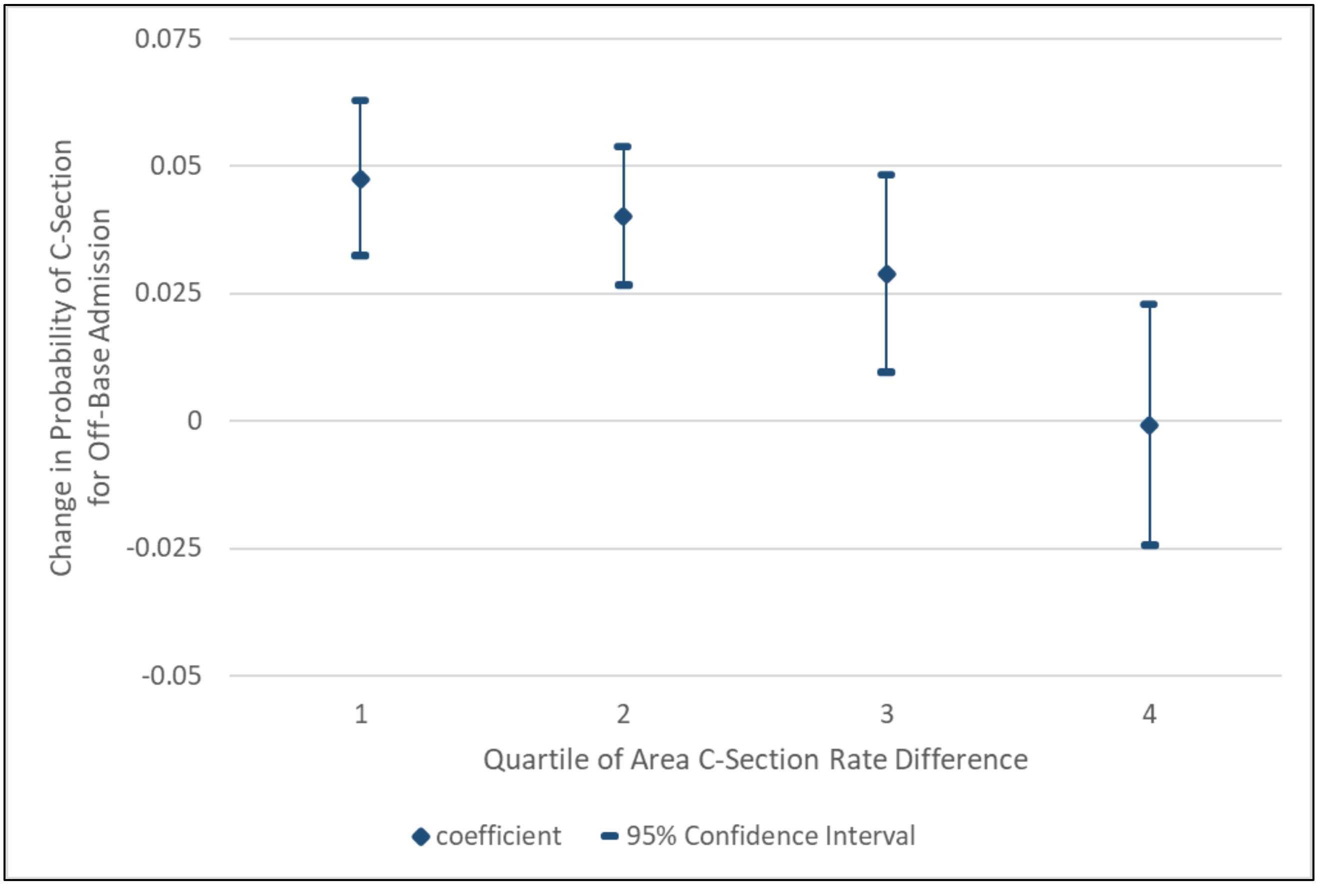
Notes: This figure shows the effect of Off-Base admission using our preferred specification for each of four quartiles of C-section probability. C-Section probability is calculated using a linear probability model fitted on calendar year, race, prior C-section, and change in pay grade, breech presentation, multiple birth, cord prolapse, placenta previa, placental abruption, distance to the base and its square, and age group. The quartile cut points are: 7.09%, 8.58%, and 43.54%. See Table 7 for results for quartiles 1 and 4.

Figure 2: Distribution of Area C-Section Rate Differences



Notes: This figure shows a histogram of the risk-adjusted C-section rate at each MTF minus the risk-adjusted weighted average C-section rate for all private hospitals used by women for whom the nearest MTF is the same. A negative value indicates that the rate at the MTF is lower than the rate at surrounding private hospitals. The risk-adjusted rate is the difference between the observed rate and the expected rate. The expected rate is calculated using fitted values from a regression including calendar year, race, prior C-section, and change in pay grade, distance to the base and its square, breech presentation, multiple birth, cord prolapse, placenta previa, placental abruption, and age group.

Figure 3: Effect of Off-Base Admission on C-Section by Quartile of Area C-Section Rate Difference



Notes: This figure shows the effect of Off-Base admission using our preferred specification for each of four quartiles of area risk-adjusted C-section rate difference. A negative value indicates that the C-section rate at the MTF is lower than the rate at surrounding private hospitals. The risk-adjusted rate is calculated by subtracting the expected rate from the observed rate. The expected rate is calculated using fitted values from a regression including calendar year, race, and change in distance from the base and its square, pay grade, prior C-section, breech presentation, multiple birth, cord prolapse, placenta previa, placental abruption, and age group. See Table 8 for results for quartiles 1 and 4. The quartile cut points are: -0.050, -0.022, and -0.006.