The Value of Specific Information: Evidence from Disruptions to the Patient- Physician Relationship

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Abstract

When a member of a work-team leaves, some information is lost to the organization. Exploiting quasi-random turnover among military physicians due to deployments, I estimate the effects of turnover on patients and other providers in the same care-team. I find that a discontinuity in primary-care leads to a 12% increase in costs driven by an increase in the use and intensity of specialty care. Overall, I find significant disruptions in care even in a context in which significant investments have been made in knowledge-management systems. This has implications for how organizations allocate tasks and manage turnover.

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1 Introduction

Organizational economics posits that organizations exist largely to coordinate tasks across individuals with different knowledge. This knowledge encompasses differences in individual skills, experiences and education, as well as idiosyncratic information regarding "the particular circumstances of time and place," as Hayek (1945) aptly put it. Economists have long recognized, however, that transferring information is costly and that the division of labor increases these coordination costs. This is further complicated by the fact that information varies in its transferability. While general information can be easily distributed across an organization, more specific information exists only within individual agents. If these agents leave or are replaced, this latter information is lost to the organization. The extent to which information loss affects individual worker productivity is unknown. In this paper I empirically estimate the impact of information loss on organizational productivity. Using quasi-random turnover of primary-care providers (PCP) within the Military Health System, I estimate the causal impact of loss of information on patients and other clinicians within the same care team.

I analyze this in the medical context as health care is a prime example both of the fragmented nature of knowledge and the associated coordination costs. Care is often fragmented across multiple clinicians (Agha et al. (2019)) and failures of coordination have been linked to $27 billion to $78 billion in waste each year (Shrank et al. (2019)). Health care is also one of several industries that are particularly reliant on case-specific information as products and services are tailored to the individual patient.[1] After the loss of a PCP, some information about the patient's particular case is lost, decreasing the probability that the new provider can diagnose a condition and increasing the probability of a referral to specialty care. Because specialty care is more expensive than primary care, this is likely to increase total costs while making the marginal referral to a specialist less appropriate.

A particular challenge of estimating the effects of information loss is that employment changes are rarely exogenous to the organization. Clinicians and other workers who move or retire may differ in unobservable ways from those that remain. Furthermore, because knowledge is non-rival, workers may take information (and patients) with them to a competitor organization. I address both of these issues through a unique data source - the Military Health System (MHS). The MHS provides a platform for patient care as well as to maintain the medical skills of active duty military health care providers (Hutter et al. (2019)). As military operational missions arise, these providers are pulled from their practices and deployed outside the United States. I use wartime deployments as a source of exogenous variation in PCP turnover in order to estimate

[1] Similar challenges exist in the legal industry and enterprise sales where products and services are tailored to the client.

the effects of information loss on patients. An additional challenge is isolating the impact of information loss from other effects of turnover. For instance, the loss of a physician from a medical practice may also result in the patient moving to a different practice. In the military setting patients affected by the discontinuity remain enrolled in the same practice with the same electronic health record, and the same insurance design.

I begin my analysis by investigating overall care utilization after the loss of a provider. In this setting, the primary care provider is the center of the clinical team and is overall responsible for coordinating a patient's care. To put this in context, consider an anecdote chronicled by a general internist (Press (2014)). A patient booked an appointment with his PCP due to pain and fever. After tests revealed a tumor, the patient saw 11 clinicians in addition to his PCP over the course of 80 days. The PCP in this tale communicated repeatedly with each of these specialists, with the patient, and with the patient's spouse. While the patient's care was fragmented across 12 providers, he received well coordinated care likely due to continuity with his PCP, who was able to maintain a full awareness of the patient's situation.

Overall, I find that a primary care provider deployment leads to a 12% ($186) increase in a patient's total cost of care in the following year, and that this increase is mostly driven by a 1.9 percentage point (6%) increase in the probability of a referral to a more specialized clinician. Consistent with a Roy model where each new referral is less well-targeted than the previous one, I find that patients are 20% (0.004 percentage points) more likely to only see the specialist once after a discontinuity. I contrast this with a sub-sample of patients who have been receiving care from multiple specialists for at least one year. Visits for these patients become approximately 5% more expensive after the loss of their primary care provider driven by an increased number of procedures for patients that require more coordination.

I provide support that information loss is the mechanism through a triple differences approach. I compare the main analysis with patients whose PCP deployed, but who had never had an appointment with that particular provider either because they did not use much primary care or happened to see another provider. I find the main effects are completely driven by patients with a relationship with their PCP. Finally, I use provider deployments as an instrumental variable in order to estimate the local average treatment effect (LATE) of a provider discontinuity on patient utilization. While deployments offer an intent to treat (ITT) estimate, not every discontinuity causes a formal change in the primary care provider. Because deployers generally return to the clinic after the mission, many patients simply won't change providers unless they undergo a health shock before or during the deployment. I use the timing of the deployment as an instrument for discontinuity in care in order to estimate the effects of changing primary care providers for those whom the deployment causes a change (i.e. compliers). For this group, costs go up by approximately 73% ($1168).

This research primarily contributes to two strands of literature. First, it contributes to the literature regarding the role of information in the firm production function. Transferring information across an organization can be costly (Garicano (2000)), even more so when the optimal solution to a problem is unknown (or unknowable) by any member of the organization Chan (2021). Information, however, varies in its cost to transmit (Jensen and Meckling (1992)). Some "general" knowledge is easily written down - for instance, a patient's blood pressure or the results of a lab test. "Specific" knowledge, in contrast, is much more costly to transfer and could include the physician's understanding of a patient's personality such as whether the patient is likely to adhere to a daily medicine and the most effective forms of communication with that patient. The management literature has linked variation in specific information to differences in firm-specific (Huckman and Pisano (2006)) and customer-specific (Clark et al. (2013)) performance. However, there is little empirical economic work estimating the value of specific information or disentangling it from other disruptions associated with turnover.

Estimating the value of specific information is critical as this information can theoretically be captured and communicated if the organization is willing to absorb the cost. This is in sharp contrast to tacit knowledge (Polanyi (1958)) such as individual skill which is infeasible to transfer outside of large-scale training programs. I offer empirical evidence of the marginal cost of losing specific information at a high level of investment (e.g. large-scale electronic health records). By considering the effect of these disruptions on other members of the team, I am also able to show that removing the primary coordinator in a team reduces the productivity of more specialized workers. This provides empirical support to Becker and Murphy's (1992) theoretical model of coordination costs and suggests that additional investment in generalized workers may also lead to increased productivity from more specialized workers, especially in project-based teams. My setting offers an opportunity to isolate the effects of turnover at the individual level while separating production loss from accounting costs. Understanding this cost contributes to our understanding of the impact of organizational turnover, especially in a knowledge-based organization.

This work also contributes to a literature detailing the challenges of coordination in health care. Previous studies have shown that discontinuity in care increases health care costs (Sabety et al. (2021)), while others have shown closer integration helps mitigate these challenges (David et al. (2013); Agha et al. (2020)); I complement these works by showing that discontinuity in care increases health care costs even in a fully integrated organization. Furthermore, I show these costs are not just driven by patients choosing more expensive sites of care, but that there are increases in the intensity of individual encounters for some patients. The Military Health System is comparable across many dimensions, including variation in care, to the civilian health care system (Bond and Schwab (2019)) and has been previously used in the economics literature to study the impact of defensive medicine (Frakes and Gruber (2019)). Using data from the MHS provides several advantages. First, previous work on discontinuity has been hampered by confounders associated with the loss of a primary care provider. For instance, patients generally change practices or forego finding a new primary care provider altogether (Sabety (2020)). New practices may have a different mix of payers offering different incentives for care. In the military setting, patients must enroll with a new PCP, all providers face the same incentive scheme, and insurance rules prevent substitution to a specialist without a PCP referral. The MHS also lets me consider the effects on a working age population, a group that has been typically understudied in the health economics literature due to a lack of publicly available data.

This paper proceeds as follows. In the next section I provide a conceptual framework for the analysis . In section 3 I detail the data and empirical specification. In section 4 I discuss the results. section 5 concludes.

2 Conceptual Framework

When the cost of acquiring knowledge is expensive, the division of labor lets workers focus on different problem sets. I build on Garicanoâs 2000 model of an organization as a partition of workers into L classes where each class has a discrete (potentially overlapping) knowledge set. Workers pay a âhelping costâ in assisting other workers with a problem. The organizational challenge is to match a problem with the worker who has the appropriate knowledge (Garicano (2000)).

I differ from Garicano in two ways. First, I break a problem into two component stages. In the first "diagnosis" stage, a worker must identify what the root cause of the problem is and in the second "treatment" stage they must fix the problem. Second, and much more substantially, I assume that an individual worker's ability to diagnose a problem is related to that particular worker's past experience and cannot be transferred to other workers. In other words, helping costs within a partition are infinite. A worker passes a problem to the next stage if they lack the requisite knowledge to either diagnose or solve a problem. This implies that some problems that are passed up the hierarchy due to inability to diagnose could be fixed at the lower level.

For simplicity I consider a two-tiered hierarchy. The first layer of the organization are the general problem-solvers. These workers decide either solve a problem or pass it up to the second level. The second layer are the specialized problem solvers. These workers either solve the problem if they have the requisite knowledge or attempt to gather new information to eventually solve the problem. In a health care setting this model reflects the relationship between primary care providers and specialists.

2.1 The Primary Care Provider

At the onset of illness or injury, the patient sees his PCP. The PCP faces the decision of whether to treat the patient, or refer to a specialist and coordinate the patient's care. Note that coordinating care is expensive for the PCP, as this time is not separately compensated. In order to treat the patient, however, the PCP must meet two necessary conditions. First, the PCP must diagnose the patient. Second, he must have the requisite skill to treat the patient's illness. If either of these conditions are not met, the PCP refers the patient to a specialist and transitions to a coordination role. While the former is dependent on the level of available information, the latter is independent of any case-specific information.

The PCP begins the appointment trying to learn about the patient's condition. Knowledge, however, is dynamic. The primary care provider can not only access information gathered during the current visit, but can also access at least a portion of information previously gathered. The more generalizable that information, the more likely the provider will have access to it regardless of a past interpersonal relationship with the patient. An example would be the results of an x-ray or patient's blood pressure at a point in time as these are both likely documented in the patient's medical record. On the other hand, specific information encompasses "the idiosyncrasies of time and place" (Hayek (1945)) and includes that which is more costly to transfer such as personal information about the patient's preferences such as their propensity to seek care and pain tolerance, but also includes previously-learned knowledge such as how much to trust each other and how to effectively communicate with each other. This type of information is generally only available within an existing relationship. This implies that the knowledge available to a provider is a function of both the visits the patient has had within the health system and the quantity of those visits that were with the current provider. After a discontinuity, this proportion goes to zero, decreasing the probability that the provider can diagnose the illness and increasing the probability of a specialty referral. Because specialty care is more expensive than primary care, this is likely to increase total costs while also making the marginal referral to a specialist less appropriate.

Figure 1 depicts these effects. The Y access denotes the expected health outcomes of a distribution of patients for a given level of information. The X access denotes levels of uncertainty in the appropriate treatment for the specific patient's condition. As uncertainty increases, information has a greater impact on the expected health outcome. The horizontal dotted line reflects a provider-specific threshold to refer a patient to specialty care. A PCP that does not believe she can achieve this a health outcome above this threshold will refer the patient to a specialist. The marginal referral triangle are patients who would have been treated by the PCP with more information, but are instead referred to a specialist.

While I present this conceptualization as provider-focused, health care decisions are often framed in a 'shared decision-making' framework Elwyn et al. (2012)) where the provider provides information and the patient elects his preferred care decision. While I focus on the PCP as the decision-maker for simplicity, the shared decision-making framework is a simple modification that yields the same predictions. For instance, the decision to refer would be the minimum expected health outcome of the patient's beliefs and the provider's beliefs.

[Figure 1 about here.]

2.2 The Specialist Clinicians

Once a PCP refers the patient, one or more specialist clinicians join the care team. The specialist, like the PCP, must first diagnose and then treat the patient. However, her choices are constrained to more or less intensive care. I assume the specialist will only provide the intensity of care needed to cure the patient. At the onset of the visit, the specialist seeks information regarding the patient's illness. In addition to the general information available to the whole care team, each clinician has access to the specific information gathered through their individual encounters, along with any other information transferred through coordination between the specialty and primary care providers. After a discontinuity, these discussions offer less information. This implies opposing effects. Specialists will treat appropriate referrals more intensively after a discontinuity in primary care. However, because the marginal referral is less appropriate, new referrals are likely to be treated less intensively on average.

3 Military Setting

I conduct this study in the context of the Military Health System (MHS). The MHS is an integrated health care system that provides care for active duty military, military retirees[2], and their family members. It is a dual system that combines care delivered in military-run clinics (direct care) and care delivered by a local network of private sector providers (purchased care). All care is paid for by the Tricare insurance benefit. Overall TRICARE covers about 9 million individuals, but does not cover care delivered in a war zone.[3] I focus my analysis on adult dependents of active-duty military. These patients tend to live near military clinics, but do not have some of the idiosyncrasies of military service such as mandatory physical training.

[2]military retirees are those who have left military service after serving long enough for their pension to vest - typically 20 years

[3]For a comprehensive review of the Military Health System, see TRI (2017).

Primary care is an ideal setting to study information loss for two reasons associated with the nature of knowledge. First, as health care costs have risen, health insurers have increasingly relied on PCP's judgement in order to limit utilization, requiring a PCP referral before authorizing payment for specialty care. Tricare Prime follows this "gatekeeper" model for primary care. Second, the PCP is responsible for coordinating a patient's care meaning the loss of a PCP is likely to impact not just the patient, but also other members of the care team.

3.1 PCP Deployments

The source of discontinuities in this study is through military provider deployments. Military providers are generally not assigned to operational (combat) units so that they can practice medicine in clinics and hospitals when not needed in combat. This serves the dual purpose of maintaining their medical skills while also providing care to Tricare beneficiaries. The military services vary in how they select providers for deployments, however none base it on patient outcomes or other quality of care metrics. Take the Army for instance[4]: At least annually, the Army reviews operational needs and submits requirements to hospitals for provider of specified specialties including primary care. Individual hospitals have discretionary power for how they choose which provider will fill these assignments. These providers are then administratively aligned with a specific operational unit while continuing in their practice. Should that unit deploy to combat, the provider leaves the practice and accompanies the unit.

3.2 Data

The data for this study primarily come from the Military Health System Data Repository (Defense Health Agency (2017)) and consist of both claims and electronic health records for military dependents enrolled in Tricare from 2008-2017. For each patient, I observe their assigned clinic and primary care provider, and the date when either of these change along with demographic details including age and gender. I measure health status using the Charlson comorbidity index (Quan et al. (2005)), a standardized score based on documented comborbidities within the medical claims. The full sample consists of 1,553,271 individuals with an average enrollment of five years. Due to military moves, changes in PCP are somewhat ubiquitous with the average patient changing providers twice during the sample.

I identify provider's military status and deployments through Contingency Tracking System (CTS) and

[4]The Army changed this system after the sample period, though they still do not deploy providers based on any quality of care metrics

military personnel master files provided by the Defense Manpower Data Center (Defense Manpower Data Center (2017a);Defense Manpower Data Center (2017b)). The CTS is a database that records when a service-member arrives and finishes an overseas deployment in support of combat operations in either Iraq or Afghanistan or nearby countries supporting those wars. The Master File lists demographic data for all active duty service members.

Using the claims data, I construct a number of utilization and intensity of care measures. Primarily, I focus on the cost of care following a discontinuity. Military claims do not include prices so I apply Medicare rules to these claims with two major exceptions. First, I don't geographically adjust so that costs are not driven by regional price differences. Second, I don't include medical equipment or pharmaceuticals as I want to capture provider workload. Equipment can be very expensive but not indicative of the intensity of care. Average spending is about $1600/year.

Second, I consider the types of care a patient uses each quarter. I categorize care as primary care, specialty care, emergency care, or inpatient admissions. While primary care and specialty care utilization denote changes in specialization, the latter two provide insight into whether these changes potentially prevent adverse events. Finally, I consider variation within an individual appointment. I measure this using the total cost of the appointment, as well as the specific procedures listed in each visit as coded using the Common Procedural Terminology (CPT) system. CPT codes are primarily used for billing and include up to three evaluation and management (E&M) codes and up to 10 non-E&M procedure codes. E&M codes are meant to denote the discussion and decision-making portion of the visit, while procedure codes are meant to capture anything else including tests and procedures performed. E&M codes have five levels of complexity. I dichotomize the complexity of a visit by coding E&M levels 4 and 5 as "complex."

Sample Construction

The unbalanced full sample is not well suited to the two-way fixed effects difference in differences design for several reasons. As Goodman-Bacon (2018) points out, the staggered timing difference in difference estimator is a weighted average of all possible two by two estimators. When treatment effects vary over time, those who are treated early end up receiving higher weights and potentially biasing the results. Second, the traditional approach does not account for individual time trends uncorrelated with calendar time. This is particularly likely in health care where going to the doctor once may be predictive of going again (e.g. for a follow-up visit).

I address these concerns by creating a balanced analysis sample through a blocking and exact matching strategy. First, I block by the quarter-year time period. For each quarter, I limit the data to patients whose PCP deploys one year later[5], have been enrolled for at least one year with the same provider, and who remain enrolled in Tricare Prime for at least two more years (i.e. one year after the PCP deploys). I subsequently refer to this point in time as the "match quarter." I then match to the control group based on the length of relationship (in quarter-years) with their assigned PCP and assign a match quarter - relationship length identifier. By requiring the same initial relationship length between treated units and their matched controls I am able to control for any lingering effects from a previous discontinuity in care which would jeopardize the parallel trends assumption. While I do not restrict to a specific number of matches per treated unit, I do restrict control group patients to one matching group. For instance if patient A is in the data as a control for a two year relationship in the first quarter of 2010 and a three year relationship in the first quarter of 2011, I randomize which one of these groups to keep. While not econometrically required, this simplifies the analysis. I further limit the control group to patients that are never treated in the data.

In the final step, I drop two groups of patients. First, I drop patients whose provider deploys but is not gone long enough to reasonably cause a discontinuity in care. Analysis shows deployments that are less than 6 months are less likely to lead to a discontinuity in care. I use deployments that are one to three months as a falsification test later in the paper. I also drop patients who do not have an appointment with their assigned PCP between one year prior to the match quarter and the deployment quarter (two years overall), as by definition there is little specific information to lose [6]. I return to this group in a triple differences analysis that provides further support for the information loss mechanism. I also relax these restrictions in the instrumental variables approach since 'non-compliers' should not affect the local average treatment effect. The final sample consists of 361,765 individuals over 3,255,885 patient-quarters. Table 1 lists the sample size reductions from each step.

[Table 1 about here.]

Empirical Design

My primarily analysis uses a difference in differences approach that treats a physician deployment as a discrete event. Because the treatments occur at different time periods for different individuals I use a two-way fixed effects model modified based on recent advances. I estimate the models using ordinary least

[5] In order to observe any pre-treatment effects, I only consider patients as treated if they were assigned to the provider one year prior to the deployment date. I chose one year because this is an upper bound on when a PCP may learn about an impending deployment.

[6] They may have *some* specific information from coordinating care, but likely much less

squares. Because spending is highly skewed, I conduct a log transformation, adding one dollar to each quarter in order to deal with any zero-spending. I show the results are robust to alternate transformations and distributional assumptions later in the paper.

Identification Strategy

The key identifying assumption for a difference in differences approach is that both the treatment and control groups would have followed parallel trends in the outcome variables if it were not for the discontinuity in care. For this assumption to hold, the timing of the deployment should not be correlated with the patient's health or physician's performance prior to the deployment. While this assumption is inherently untestable, we can provide suggestive evidence by looking at pre-trends. I use an event-study methodology to evaluate whether trends in the treatment and control groups were parallel prior to the physician deployment. The event study takes the form:

$$Y_{ipjt} = \beta_{Q=t-t^*} + \gamma^* X_{pt} + \theta_i + \delta_{jt} + \epsilon_{ipjt} \tag{1}$$

where y_{ipjt} represents an outcome of interest for individual i, assigned to PCP p, part of matched-group j at time t and Q is the quarter relative to the quarter of the physician deployment t^*. β_1 represents the relative difference between the treatment and control groups at each point in time. X_{pt} is a vector of two time-varying controls. First, I control for whether the the patient's PCP in time t is a physician or an advanced practice provider (APP) such as a Nurse Practitioner or Physician Assistant. It's possible that moving from a physician to an APP could lead to more referrals biasing the results since these providers could have less specialized skill. Second, I control for whether the patient's assigned PCP is active duty military since active duty providers may differ from civilian providers in observable ways. I return to these provider differences in the alternative explanations section of the paper. Note that most standard patient-level controls are collinear with the fixed effects given that I've restricted the analysis to two years of data. θ represents a vector of individual fixed-effects and δ_{jt} represent the quarter-year fixed-effects interacted with the relationship-group and the quarter on which the group was matched as described above. This cohort time fixed effect controls for any differential trends based on the timing of treatment or previous discontinuities. Finally, I cluster the standard errors by the patient to account for any serial correlation of the error terms.

In the main analysis, I estimate the intent to treat (ITT) effect of a discontinuity in care using equation 2 below.

$$Y_{ipjt} = \alpha + \beta_1^* I(t > deployment) + \gamma^* X_{pt} + \theta_i + \delta_{jt} + \epsilon_{ipjt} \tag{2}$$

where I is an indicator for the treatment group and all else is the same as in equation 1. β_1 is the coefficient of interest that indicates the relative change for the treatment group after the provider deploys. Because I observe the date the PCP arrives overseas, but not the date that they leave the clinic, I omit the deployment quarter from these regressions.

While the provider deployment provides an *intent to treat*, not every patient changes providers after a deployment. The MHS as an enterprise does not automatically transfer patients when a provider deploys, though individual clinics vary in their approach to these patients. Some patients simply do not seek care and are less affected by the provider deployment. Other patients may come in and see whomever is available in the clinic. In this section I use the timing of the deployment as an instrumental variable in order to obtain the effects of the deployment specifically on those who change primary care providers due to the deployment (e.g. *compliers*). This local average treatment effect (LATE) scales the OLS estimates by the probability of changing providers due to the deployment.

Because patients may change providers shortly before or after a deployment, I use indicators for each time period around a deployment as an instrument for the actual discontinuity in care. I estimate the first stage as:

$$D_{ipjt} = \beta_{Z=t-t^*} + \gamma^* X_{pt} + \theta_i + \delta_{jt} + \epsilon_{ipjt} \tag{3}$$

where D represents an individual in match-group j being in the first year of a relationship with a new provider in time t. Z is a vector of indicators for the 8 consecutive time periods relative to the deployment, beginning with the time period after the match quarter.

The first stage predicted values for D_{ipjt} are then substituted into the following equation

$$Y_{ipjt} = \beta \hat{D} + \gamma^* X_{pt} + \theta_i + \delta_{jt} + \epsilon_{ipjt} \tag{4}$$

Standard errors are adjusted for the two-stage least squares approach and clustered by individual patient.

In order to provide evidence that loss of information is the mechanism, I take a triple differences approach leveraging a particular organizational idiosyncrasy. While patients are assigned a specific PCP, they can book an appointment with any PCP within the practice. I leverage patients who, for likely-endogenous reasons such as appointment availability, have never had an appointment with their PCP prior to the deployment.

This could be because they didn't use any care or because they happened to see someone else in the practice when they did use care. Overall, about 1/3rd of patients fall into this category. With the assumption that this decision is orthogonal to the physician deployment, I estimate equation 3.

$$
\begin{aligned}
Y_{ipjt} = \alpha + \beta_1^{*} I(t > deployment) + \beta_2 * (t > deployment) * (visits > 0) \\
\beta_3^{*} I(t \geq deployment) * (visits > 0) + \gamma^{*} X_{pt} + \theta_i + \delta_{jt} + \epsilon_{ipjt}
\end{aligned} \tag{5}
$$

β_1 in this equation is the effect of the treatment group in the post period. β_2 is the effect of those who have met their provider in the post period compared to those who never met their provider. Finally coefficient β_3 indicates the triple interaction of the treatment indicator, the post-period indicator and having met the provider. This can be interpreted as the relative effect of the deployment for those who had some specific information to lose compared to those who were treated but didn't have any information to lose.

Finally, I consider what occurs within individual encounters using the following equation:

$$
Y_{ipjt} = \alpha + \beta_1^{*} I(t > deployment) + \gamma^{*} X_{it} + \theta_p + \delta_{jt} + \epsilon_{ipjt} \tag{6}
$$

The index p here refers to the encounter provider rather than the assigned PCP. This is similar to equation 2, however I swap the patient fixed-effect for a provider fixed effect so that we identify differences in how the provider treats these patients compared to the provider's other patients. This eliminates any differences driven by provider specialty or skill level. Because I no longer include the patient fixed effect I add patient covariates including age, age squared, gender, and charlson comorbidity score.

Results

I begin this section by presenting evidence that provider deployments do in fact lead to changes in a patient's primary care provider. I test for this using equation 1 with the outcome being an indicator for having a different primary care provider than in the matching period. Figure 2 shows an event-study of the probability of having changed primary care providers since the matching index quarter in the periods around a deployment. The slight decrease two to three quarters prior to the deployment are a function of the deployment process. Physicians who have been assigned to deploy cannot be moved between hospitals reducing the chance for a discontinuity based on a provider moving. This means a patient assigned to this physician is *slightly* less likely to have a discontinuity in care from the time of assignment until right before the provider deploys. We then observe an uptick in changing primary care providers in the quarter prior to the deployment. This may be due to observing the date that a provider arrives overseas but not the date they left the clinic.

[Figure 2 about here.]

In order to interpret the results as the affect of a discontinuity in care, the deployments must be quasi-random and must only affect outcomes through their impact on discontinuity of care. This would be violated if, for example, patients begin seeing providers of lower average quality after a deployment or if providers change their practice style prior to the deployments. Table 2 panel A displays the results of a regression of patient demographics in the year prior to the match-quarter on an indicator for undergoing a primary care provider deployment. The regression does not include any controls but does include clinic and cohort-time fixed effects.

[Table 2 about here.]

The treatment group tends to be about a year and a half older, has fractional more comorbidities, and tilts a bit more female. Panel B shows differences in utilization using the same regression. While there are small differences, none of these are economically meaningful and all are explained by the observable demographic differences. Panel C repeats the utilization regression but adds in controls for age, (linear and quadratic) gender and Charlson score. There are no economic or statistically significant differences between the groups.

Table 3 shows the results of a series of regressions of practice intensity on an indicator for whether a provider deploys within the sample window. The regressions don't include any controls but do include clinic and cohort-time fixed-effects. Deployers and non-deployers appear to practice in similar fashion. Both groups spend about the same per primary care appointment, refer to specialists at about the same rate, and code about the same number of CPT procedures per encounter. Deployers code complex evaluation and management codes a bit less often. Finally, I check for changes in practice patterns by modifying equation 6. I regress each appointment intensity indicator on an indicator for being within one year of a deployment and include provider and match cohort fixed effects. Table 10 shows the results of these regressions. There's no evidence of an economically meaningful change in practice patterns for any of the measures, with only the number of coded procedures being statistically significant.

[Table 3 about here.]

Next, I present evidence of parallel pretrends in patient cost and utilization. Figure 3a shows changes in the natural log of total dollars spent per quarter across the year before and after the discontinuity. The estimates are stable in the quarters prior to the deployment indicating parallel pre-trends and providing support for the parallel trends assumption. Cost goes up as the PCP departs indicating that there is an effect of the discontinuity on total cost of care. Figure 3b shows the probability of any specialty care appointment. The trend is similar to the spending outcome with a sharp increase after the provider leaves.

The linear probability model event study for primary care shown in 3c is interesting as the effects appear somewhat delayed. This is logical if discontinuities are unlikely to lead to a patient requiring primary care on the extensive margin. A discontinuity in care isn't likely to bring on an illness or injury, but conditional on that illness or injury a patient may be likely to use more or more expensive care, especially if there is a primary care follow-up after a specialty appointment. This is supported by the event study in figure 3d which shows the probability of using multiple primary care appointments in a quarter.

[Figure 3 about here.]

Table 4 presents the difference in differences results for each of the three main utilization variables. Column 1 shows the effect of a physician deployment on the log of spending in the post period. The coefficient indicates about a 12% percent or an approximately $186 increase over the full year. Column 2 shows the effects of the discontinuity on the probability of using a specialty care appointment. The 1.9 percentage point increase on a base of 0.33 indicates that specialty care utilization goes up by about 6% consistent with the theoretical prediction. Column 3 shows a 1 percentage point increase in primary care. This is contrary to previous literature using Medicare data that has shown a drop in primary care after a discontinuity (Van Walraven et al. (2010); Sabety et al. (2021)). This could be due to the gatekeeping model where a patient cannot seek specialty care without first seeking primary care. While I estimate changes in spending using a log $+1$ transformation in the main results. I present an alternative hyperbolic sine transformation in appendix table 11 column 1. The specialty and primary care results are based on linear probability models, however, I allow for different distributional assumptions and present alternative poisson models for these outcomes in appendix table 11 columns two and three. Poisson models require some variation in the outcome, meaning that individuals who do not use that type of care over the two year period are dropped from the regressions. However, the magnitude of the estimates are substantially similar for specialty care and much larger for primary care - about a 6% increase in each. This providers further support that the effects on primary care are on the intensive margin rather than the extensive margin.

[Table 4 about here.]

Table 5 presents linear probability models for whether a patient ends up in either the emergency department or is admitted to a hospital as an inpatient. The coefficients on both are extremely small and statistically indistinguishable from zero. While I cannot definitively rule out a longer-term effect, I do not find evidence that the increased use of outpatient care is preventing any negative events.

[Table 5 about here.]

While the difference in differences results focused on the intent to treat, table 6 shows the local average treatment effect of a provider discontinuity from the instrumental variables approach specified in equations 4 and 5. For patients whom the deployment causes a discontinuity (i.e. "compliers"), the results are much starker - exponentiating the outcome I observe a 73% increase in the total cost of care driven by approximately a 35% relative increases in specialty care. Panel A shows the IV results for the main sample. In Panel B I relax the exclusion restrictions of requiring a visit between the patient and the provider in the two years leading up to the post-period. Including these patients should not affect the LATE as these patients are likely 'non-compliers' - lower utilizers who do not change PCP's due to the deployment. As expected the coefficients for the models are nearly identical. Finally, in panel C I also relax the no short deployments restriction and get an almost identical coefficient on spending. The coefficient on the probability of a specialty visit care in column two is a bit lower, and the coefficient on the probability of primary care use in column three is a bit higher. This could be due to the fact that short deployments can cause a discontinuity, but the magnitude of information loss is much lower. However, none of the estimates are significantly different across the three samples. That I find similar results regardless of these restrictions suggests that the effects are not driven by these sampling decisions, and that the results are driven by 'compliers' - those whom change providers when their PCP deploys.

[Table 6 about here.]

Mechanism

In this section I present suggestive evidence that information loss is the mechanism driving the main results. I begin by presenting the results of a difference in difference in differences model that compares the effects of a provider deployments for those that have a recent (within two years of the deployment) visit with their PCP to those who do not. Table 7 displays the results of these regressions. The *discontinuous relationship* estimate is the coefficient on the triple interaction in equation 5. The estimates are slightly lower but substantially similar to then main difference in differences results.

[Table 7 about here.]

Next I consider the impact on specialist encounters. The conceptual framework suggests that information loss could potentially manifest in two different ways. First, primary care provider referrals to specialists will be less appropriate. Second, appropriate specialist visits will be more intensive. For this analysis, I restrict the sample to specialty appointments within military hospitals where we observe more information about the individual specialty provider and where coordination between specialty and primary care physicians is more likely to occur. I exclude from this analysis any visits that are not paid for individually (e.g. post-surgical follow-ups or pregnancies that are paid with a "global payment" designed to cover all pre-natal care as well as the delivery). I also exclude visits that do not require evaluation and management (e.g. an appointment to have an MRI).

I begin the analysis by considering whether primary care providers are referring patients to a new specialty or back to a clinic in which the patient was previously seen. I define a new specialty as one in which the patient was not seen in the year prior to the match. For each patient-specialty clinic grouping, I also observe whether the patient is seen in that clinic multiple times or only once. The intuition is that a patient that goes to a new clinic only once may represent a less appropriate referral compared to a patient that requires follow-up care within the specialty clinic. I estimate the change in probability for each of these measures - a new specialty visit and a single-visit referral - using equation 2. Table 8 presents the results of these regressions. The coefficient in column 1 indicates a 9% (0.009 pp) relative increase in the probability of a visit in a new specialty clinic - about 1.5 times the magnitude of the overall increase in the probability of specialty care utilization. Column 2 indicates a 20% (0.004 pp) relative increase in the probability of only having one encounter in a specialty clinic. Together, these findings suggest that the increase in specialty care is not the result of patients reverting to existing specialty relationships and support the prediction that much of the increase in specialty care is driven by less appropriate referrals.

[Table 8 about here.]

Next, I look within these specialty encounters for evidence of a change in intensity of care by the specialty provider. I use equation 6 to estimate any changes in the intensity of care along three measures. Primarily, I consider spending within the visit. Second, I consider whether the provider codes a more complex evaluation and management code (indicated by a 4 or 5 level code). Finally, I consider whether there is a change in the number of procedures conducted in the encounter. Table 9 panel A presents the results of these regressions applied to the full sample of specialty encounters. I find no evidence of an effect of the discontinuity in primary care on these specialty encounters.

I then subset the group along two margins in which theory would predict an effect. First, I consider patients who see a provider with whom they have a long-term relationship. I define long-term relationship as a provider with whom the patient had an encounter in the year leading up to the match-quarter. Second, I consider any specialist visit for patients who have encounters with multiple specialists in the year leading up to the match quarter. These patients are likely to require more coordination of care. Table 6 presents the results of these regressions in panels B & C respectively. Both groups have a similar increase in spending per visit, although this is imprecisely measured especially for the long-term relationship sample. However, there is a difference in what is driving the increased cost. For long-term relationships, this is driven by an 18% increase in the probability of a level 4 or level 5 evaluation and management code indicating that these providers are exerting more effort in the discussion portion of the visit. The results shown in panel C, however, suggest that for those who require more coordination, the cost is driven by an approximately 5% increase in the number of procedures performed. The difference between these groups makes sense for a couple reasons. One, patients returning to specialists whom they have been seeing for years may have already undergone the procedures that provider commonly performs. These specialist providers may end up providing some of the management the PCP was formerly providing, consistent with previous findings (Sabety (2020)). Two, patients with higher coordination needs may have more uncertainty in their diagnoses resulting in more tests and procedures when information is decreased.

[Table 9 about here.]

Alternative Explanations

While the results so far have shown the effects of a deployment, there could be several reasons why a deployment causes a change in utilization aside from information loss. First, the loss of a primary care provider could simply make all other providers in the clinic busier and less attentive to a focal patient's care. Second, the type of provider a patient is sent to could drive the results. Patients are more likely to be matched to either a civilian providers or an advanced practice provider, each of which may differ in their quality from active duty physicians. While I control for provider-type in the main estimation, I also run a series of regressions limiting the sample to those with the same provider type in the pre and post periods.

Table 12 shows the results of four robustness checks designed to test these alternative explanations. In panel A, I add in a clinic-quarter fixed effect to the main specification. This fixed effect is intended to take out any variation in a clinic's capabilities. For instance if the loss of a PCP to a deployment limits the overall capacity of the clinic. The magnitude of the effects are not significantly different from the main results implying that the results are not driven by some time-varying effect on the clinic. In panel B, I display the results from limiting patients to an active duty military provider in the pre and post-period. The results are a bit attenuated with about a 7% increase in costs, though the sample size is much smaller . In panel C, I present the results from eliminating anyone who switches to a purchased care (private sector) primary care provider. The spending coefficient is about 8%, again slightly lower but in line with the main results. Finally, in column four I restrict the analysis to those who have physicians rather than advanced practice providers in the pre and post period. The results here indicate about a 14% increase in spending, slightly higher than the main results. As the choice of a physician versus an advanced practice provider may not be random, these are potentially higher intensity patients. The change in probability of a specialty encounter is remarkably consistent across the samples - about 1.6 percentage point increase, slightly lower than the main results. Despite differences in sample size and precision, the four robustness checks offer evidence that the results are stable and are not driven by these alternative explanations.

Finally, I run a falsification test using short deployments to provide supporting evidence that discontinuity in care is in fact driving the results. These are operational assignments where the provider returns within three months. I expect that there is little information lost when the provider is only gone for a short time period. table 13 presents the results of running equation three on a sample that includes all deployments less than 90 days and excludes any longer deployments. There are no significant effects across the three main utilization measures. This provides additional support that the deployment itself does not cause an effect, but that the effect is generated through the impact of the deployment on discontinuous care and loss of specific information.

4 Discussion and Conclusions

In this paper I study the effects of losing specific information on productivity using quasi-random discontinuity in patient care. I apply a difference in differences model to the Military context where primary care providers are pulled from their practices in the midst of treating and coordinating care for a panel of patients. By considering overall utilization and within specialty visit variation, I am able to provide new information on the value of specific information.

The findings indicate that loss of information creates about 12% additional cost per patient and that this is driven by a much higher 73% increase for those most affected. These effects are particularly relevant given the high investment in health information technology by many health systems where any provider can see generalizable information regarding the patient. I also find that a primary care discontinuity has an effect on other members of the care team. Examining specialty encounters, I find that new referrals become less appropriate while care of existing patients becomes more costly. This is consistent with the both the literature on specific information (Jensen and Meckling (1992)) and work on coordination costs, including Becker and Murphy's (1992) theoretical model that showed coordination costs limit the extent of the market, and recent empirical work on fragmentation of care (Agha et al. (2019) and provider team relationships (Agha et al. (2021)).

This study has important implications for health care where many recent policies and organizational innovations have been focused on reducing coordination costs. There is a substantial tradeoff, however, in that policies focused on decreasing loss of information may limit the accumulation of new information. For instance, Electronic Health Records are designed to expand access to generalizable information, yet may take the provider's attention away from the patient resulting in less accumulation of new, specific information. Another example is the patient centered medical home (PCMH) model. This complex model is designed to promote sharing of information about a patient across a group of providers. However, the patient's relationship with an individual provider may be weakened. This could conceivably mitigate the effects of discontinuity by reducing continuity overall. It's ambiguous whether this can improve outcomes.

Beyond the health care context, this work has implications for how organizations organize and allocate tasks. For instance, a firm may weigh the benefits of repeated interactions between team-members and a customer with the potential loss that occurs when a team-member departs. While it's beyond the scope of this paper to offer specific recommendations, firms may consider policies and organizational structures that promote knowledge management and sharing of information. If specific information is contained within individuals, firms may adopt policies that provide more opportunities for 'warm handoffs' in which some specific information can be shared prior to the team-member departing. Firms may also adopt technology that reduces the cost of information transfer. Finally, effective policies may want to not only address information about the customer, but also information regarding the best way to coordinate within the team.

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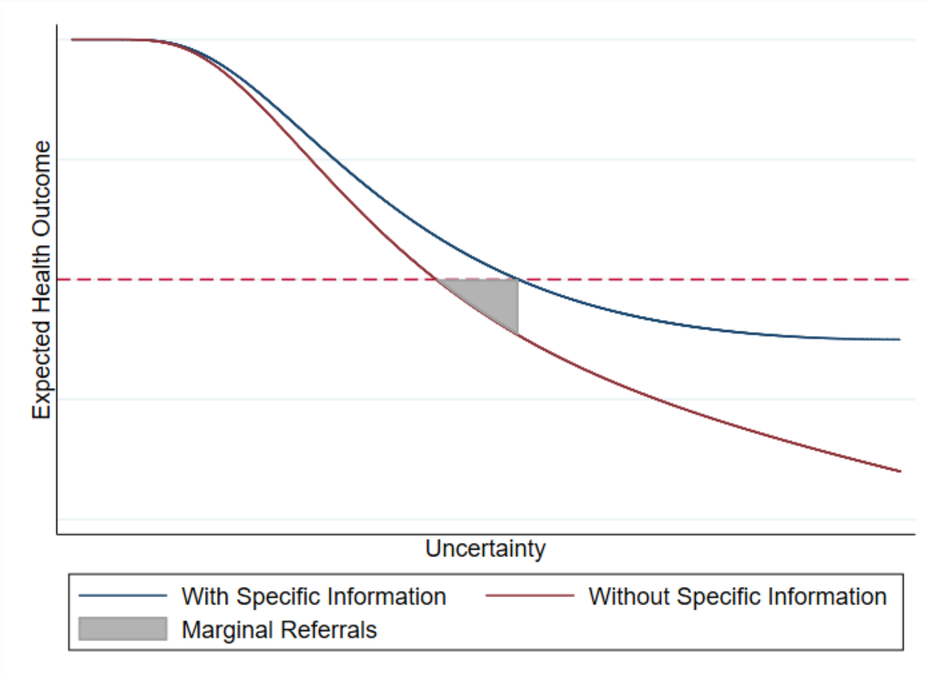
A Appendix

[Table 10 about here.]

[Table 11 about here.]

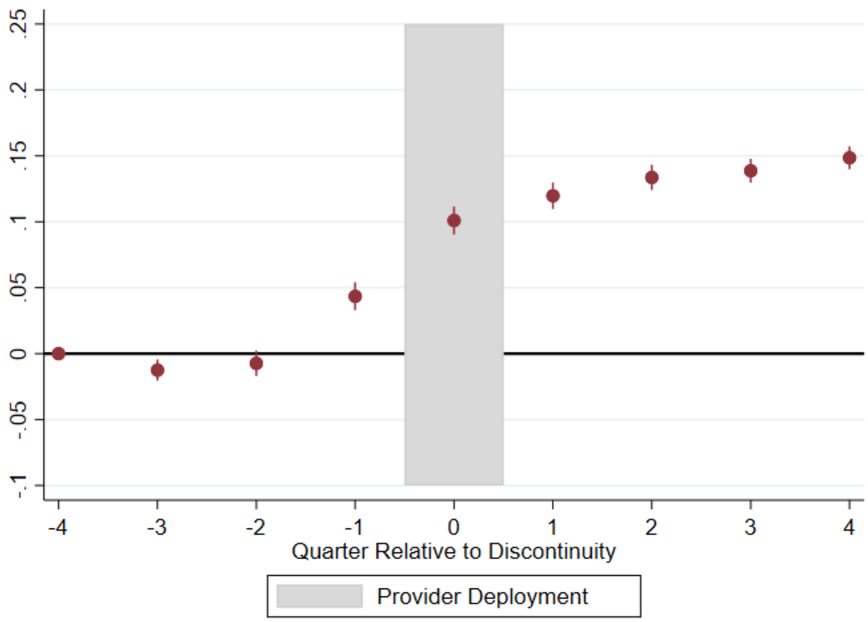
[Table 12 about here.]

[Table 13 about here.]



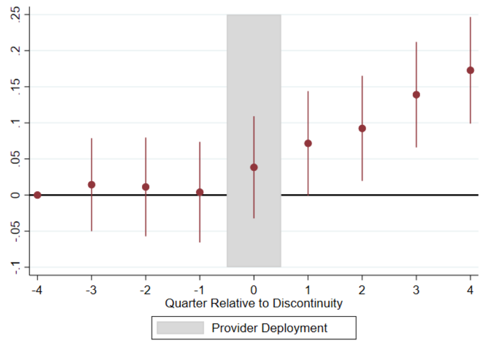
Note: Author's depiction of the relationship between treatment uncertainty and information. The dotted line reflects a primary care provider's (PCP's) threshold for referring a patient to specialty care. Patients whose expected outcome is above this line are treated by the PCP. Patients whose expected outcome is below this line are referred to a specialist. The grey box denotes patients that are treated by the PCP when information is available, but referred to a specialist without this information.

Figure 1: Information as an Input in Production of Health

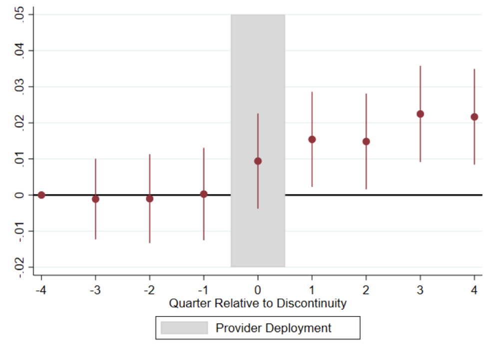


Note: Event study of the the cumulative probability that a patient has changed primary care providers since the match period using equation 1. Each point is the coefficient for the interaction of the elapsed-time quarter and treatment group indicator for quarter-years relative to the provider deployment. The match quarter is the omitted quarter.

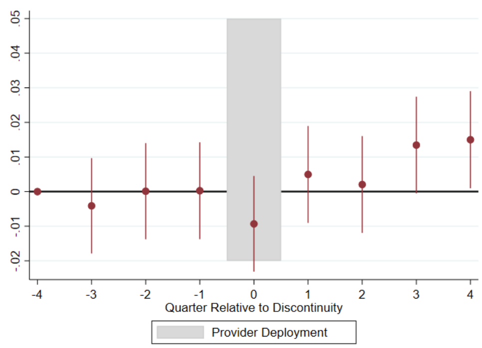
Figure 2: Event Study of Probability of Different Primary Care Provider than Match Period.



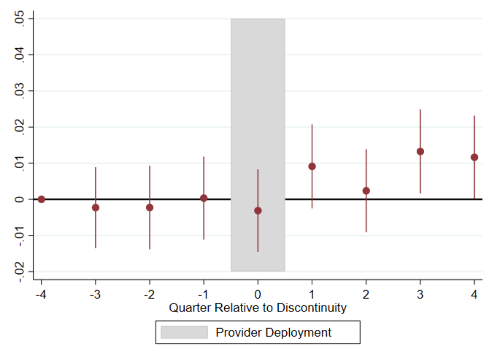
(a) Log of Spending



(b) Probability of a Specialty Care Visit



(c) Probability of a Primary Care Visit



(d) Probability of Multiple Primary Care Visits

Note:
Event studies display the coefficient on the interaction of the elapsed-time quarter and treatment group indicator for each quarter relative to the provider deployment as described in equation 1. The quarter prior to the deployment is the omitted quarter for all figures. 3a shows the change in the log of total spending. 3b displays the change in the probability of at least one specialty visit. 3c and 3d show the changes in the probability of at least one primary visit and at least two primary care visits respectively. Regressions include controls for whether the provider is a physician and whether the provider is active duty along with patient and cohort-time fixed effects. Standard errors are clustered by patient.

Figure 3: Event Studies of Utilization Measures

Table 1: Sample Construction

	Unique Patients	Patient-Quarters
Full Data	1,553,271	26,461,009
Matched Sampled	548,454	4,936,086
After Short-Deployment Restriction	536,451	4,828,059
After PCP Relationship Restriction	361,765	3,255,885

Note: This tables lists the sample size after each restriction is imposed. Difference in differences and triple differences samples will have fewer observations due to dropping the treatment quarter in these regressions.

Table 2: Comparison of Patients in the Analysis Sample Who Do and Do Not Undergo A Deployment Related Discontinuity in Primary Care

	(1) Sample Mean	(2) Coefficient On Treated	(3) Std Error
Panel A - Demographics			
Age	30.25	1.524	(0.086)
Gender - Female	0.92	0.017	(0.003)
Charlson Comborbidity Score	0.18	0.021	(0.005)
Panel B - Annual Utilization			
Total Spend	$1637	$58.59	($65.28)
Specialist Visits	5.34	0.282	(0.10)
Primary care Visit	3.89	0.104	(0.048)
Emergency Dept Visits	1.31	0.001	(0.037)
Inpatient Admissions	0.17	0.001	(0.005)
Panel C - Annual Utilization with Controls			
Total Spend	$1637	-$8.02	($62.79)
Specialist Visits	5.34	0.03	(0.098)
Primary care Visit	3.89	-0.02	(0.046)
Emergency Dept Visits	1.31	0.033	(0.036)
Inpatient Admissions	0.17	0.001	(0.005)
N - Unique Patients			
Control Group	353,178		
Treatment Group	8,587		

Note: Coefficients from a regression of the dependent variable on an indicator for whether the patient is in the treatment group. Panel A&B include a clinic and cohort-time fixed-effect with no controls. Panel C also includes demographic controls including age, age squared, gender, and Charlson Comorbidity Score. Differences are estimated for the year prior to the match-quarter.

Table 3: Primary Care Encounter Intensity

	(1) Sample Mean	(2) Coefficient On Deployer	(3) Std Error
Cost per Encounter	\$59.57	-\$0.57	(0.488)
Probability of a Referral	0.07	-0.000	(0.001)
Complex Evaluation	0.34	-0.021	(0.012)
Number of Procedures	1.13	0.016	(0.007)
N - Providers			
Control Group Providers	5,501		
Treatment Group Providers	410		
Total Encounters	1,714,400		

Note: Coefficients from a regression of the dependent variable on an indicator for whether the provider deploys in the analysis sample and a clinic and match cohort fixed-effect as described in the text. Cost per encounter is based on Medicare rates applied to the Military Direct Care System. Probability of a referral is calculated by assigning any specialty visit to the previous primary care visit if the visit occurred within 1 year. Complex evaluation is the probability that the provider codes an encounter using a 4 or 5 level evaluation and management code. Number of procedures is the total number of Common Procedural Technology (CPT) codes listed on the medical claim. Regression includes any primary care encounter by a primary care provider who has at least one patient from his or her panel in the analysis sample during the match period. I restrict the regressions to encounters prior to a provider deployment. This is not inclusive of all primary-care encounters.

Table 4: Main Results - Utilization

	(1) Log of Spending	(2) Probability of Specialty Encounter	(3) Probability of Primary Care Encounter
Discontinuous Care	0.111	0.019	0.010
	(0.022)	(0.004)	(0.004)
Sample Mean	$396.41	0.33	0.44
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
N	3,247,298	3,247,298	3,247,298

Note: Results of estimating equation two. Discontinuous Care coefficient is the interaction of the treatment group identifier and the post period identifier. Controls include whether the Primary Care Provider (PCP) in that quarter is active duty military, and whether the PCP is a physician or advanced practice provider. All regressions include individual and cohort-time fixed effects as described in the text. Standard errors are clustered by patient. The deployment quarter is omitted from these regressions.

Table 5: Main Results - Outcomes

	(1) Probability of Emergency Department Encounter	(2) Probability of Inpatient Admission
Discontinuous Care	0.001	0.001
	(0.003)	(0.001)
Sample Mean	0.12	0.04
Controls	Yes	Yes
Fixed Effects	Yes	Yes
N	3,247,298	3,247,298

Note: Results of estimating equation 2. Discontinuous Care coefficient is the interaction of the treatment group identifier and the post period identifier. Controls include whether the Primary Care Provider (PCP) in that quarter is active duty military, and whether the PCP is a physician or advanced practice provider. All regressions include individual and cohort-time fixed effects as described in the text. Standard errors are clustered by patient. The deployment quarter is omitted from these regressions.

Table 6: IV Results - Utilization

	(1) Log of of Spending	(2) Probability of Specialty Encounter	(3) Probability of Primary Care Encounter
Panel A - Main Sample			
Discontinuous Care	0.552	0.116	0.031
	(0.164)	(0.032)	(0.029)
First Stage F-Stat	210.10	210.10	210.10
Sample Mean	$396.41	0.33	0.44
N	3,247,298	3,247,298	3,247,298
Panel B - No Relationship Restriction			
Discontinuous Care	0.598	0.104	0.050
	(0.167)	(0.031)	(0.028)
First Stage F-Stat	187.17	187.17	187.17
Sample Mean	$346.77	0.29	0.37
N	4,828,059	4,828,059	4,828,059
Panel C - No Restrictions			
Discontinuous Care	0.533	0.061	0.076
	(0.117)	(0.021)	(0.019)
First Stage F-Stat	203.23	203.23	203.23
Sample Mean	$347.11	0.29	0.37
N	4,936,086	4,936,086	4,936,086
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes

Note: Results of estimating equations three and four. The elapsed time relative to a deployment instruments for being in the year (4 quarters) after changing primary care providers. The coefficient on discontinuous care is the local average treatment effect (LATE). The main sample restricts to patients who have at least one primary care visit with their Primary Care Provider (PCP) in the two years prior to the deployment (or equivalent for the match group) period, and whose provider deploys for at least 6 months. In Panel B, I relax the restriction to have at least one primary care visit with the PCP. In Panel C, I also relax the deployment length restriction. Controls include whether the Primary Care Provider (PCP) in that quarter is active duty military, and whether the PCP is a physician or advanced practice provider. All regressions include individual and cohort-time fixed effects as described in the text. Standard errors are clustered by patient.

Table 7: Results of Triple Difference

	(1) Log of of Spending	(2) Probability of Specialty Encounter	(3) Probability of Primary Care Encounter
Post-Deployment	0.023	0.003	-0.002
	(0.027)	(0.005)	(0.004)
Discontinuous- Relationship	0.083	0.015	0.010
	(0.035)	(0.006)	(0.006)
Sample Mean	$346.77	0.29	0.37
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
N	4,291,608	4,291,608	4,291,608

Note: Results of estimating equation five. Post-Deployment coefficient is the interaction of the treatment group identifier and the post period identifier. regardless of whether the patient has a relationship with the provider. Discontinuous relationship is the triple interaction of whether a patient who has a relationship with the provider, whether the provider deploys, and an indicator for the post-deployment period. Controls include whether the Primary Care Provider (PCP) in that quarter is active duty military, and whether the PCP is a physician or advanced practice provider. All regressions include individual and cohort-time fixed effects as described in the text. Standard errors are clustered by patient. The deployment quarter is omitted from these regressions.

Table 8: Specialty Referrals

	(1) New Specialty Encounter	(2) Single Specialty Encounter
Discontinuous Care	0.009	0.004
	(0.002)	(0.002)
Sample Mean	0.10	0.02
Controls	Yes	Yes
Fixed Effects	Yes	Yes
N	3,247,298	3,247,298

Note: Results of estimating equation two. Discontinuous Care coefficient is the interaction of the treatment group identifier and the post period identifier. Column one is a linear probability model indicating whether a patient was seen in a specialty clinic (e.g. dermatology, ob-gyn) in which they are not seen in the year prior to the match-quarter. Single encounters refers to the probability that a patient visits a specialty clinic with no follow-up visit in the data as of one-year after the deployment event. Controls include whether the Primary Care Provider (PCP) in that quarter is active duty military, and whether the PCP is a physician or advanced practice provider. All regressions include individual and cohort-time fixed effects as described in the text. Standard errors are clustered by patient. The deployment quarter is omitted from these regressions.

Table 9: Impact on Specialty Encounters

	(1) Log of Spending	(2) Complex Evaluation	(3) Number of Procedures
Panel A - Main Sample			
Discontinuity in Primary Care	0.009	0.003	0.011
	(0.014)	(0.009)	(0.019)
Sample Mean	$81.58	0.277	1.71
N	585,857	585,857	585,857
Panel B - Existing Relationship			
Discontinuity in Primary Care	0.047	0.054	-0.004
	(0.050)	(0.037)	(0.066)
Sample Mean	$78.28	0.298	1.68
N	139,622	139,622	139,622
Panel C - Coordination Required			
Discontinuity in Primary Care	0.054	0.001	0.080
	(0.030)	(0.019)	(0.041)
Sample Mean	$82.78	0.301	1.74
N	179,801	179,801	179,801
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes

Note: Results of estimating equation six. Observations are at the specialty encounter level for all patients in the analytical sample being seen for specialty care in the military direct care system on an outpatient basis. Panel A shows results for all patients in the main sample. Panel B restricts the sample to encounters between a patient and a specialty provider who have at least one encounter together in the year leading up to the match quarter. Panel C restricts the sample to patients who have at least one encounter with two or more specialty providers in the year leading up to the match quarter.

Table 10: Primary Care Physician Anticipatory Effects

	(1) Log of Spending	(2) Probability of Referral	(3) Complex Evaluation	(4) Number of Procedures
Anticipatory Period	-0.010	0.002	0.001	-0.021
	(0.010)	(0.003)	(0.008)	(0.008)
Sample Mean	$59.58	0.07	0.34	1.13
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes
N	1,714,386	1,714,386	1,714,386	1,714,386

Note: Results a regression of the dependent variable on an indicator for whether the provider is within 4 quarters of a deployment. Regressions provider and cohort-time fixed effects as described in the text. Controls include patient age, patient age squared, patient gender and patient co-morbidity score. Regression includes any primary care encounter with at least one evaluation and management code, conducted by a primary care provider who has at least one patient from his or her panel in the analysis sample during the match-period. This is not inclusive of all primary-care encounters.

Table 11: Alternative Estimation of Main Results

	(1) Inverse Hyperbolic Sine of Spending	(2) Number of Specialty Encounters	(3) Number of Primary Care Encounters
DiD Coefficient	0.123	0.051	0.057
	(0.024)	(0.021)	(0.014)
Model:	OLS	Poisson	Poisson
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
N	3,247,298	2,484,388	2,875,144

Note: Results of estimating equation 2 using alternative transformation and distributional assumptions. Column 1 transforms total dollars using the inverse hyperbolic sine rather than a log + 1 method. Column 2 estimates the effects of a discontinuity on specialty care using a poisson model. Column 3 does likewise for primary care visits. All regressions include a individual and cohort-time fixed effect as described in the text. Standard errors are clustered by patient.

Table 12: Robustness Checks

	(1) Log of Spending	(2) Probability of Specialty Encounter
Panel A - Clinic-Quarter Fixed Effect		
Discontinuous Care	0.103	0.016
	(0.023)	(0.004)
Sample Mean	$396.41	0.33
N	3,247,298	3,247,298
Panel B - Active Duty Only		
Discontinuous Care	0.065	0.016
	(0.028)	(0.005)
Sample Mean	$403.08	0.33
N	826,397	826,397
Panel C - Direct Care Only		
Discontinuous Care	0.080	0.016
	(0.024)	(0.004)
Sample Mean	$377.56	0.32
N	2,810,132	2,810,132
Panel D - Physician Only		
Discontinuous Care	0.133	0.017
	(0.031)	(0.006)
Sample Mean	$399.60	0.32
N	1,427,645	1,427,645
Controls	No	No
Fixed Effects	Yes	Yes

Note: Results of estimating equation two with adjustments and sample restrictions. Panel A adds a clinic-quarter fixed effect to the equation. Panel B omits patients who ever have a non-active duty military primary care provider. Panel C omits patients who are ever managed in the private sector. Panel D omits patients managed by an advanced practice provider.

Table 13: Results of Falsification Test

	(1) Log of of Spending	(2) Probability of Specialty Encounter	(3) Probability of Primary Care Encounter
Discontinuous Care	0.020	0.004	0.008
	(0.053)	(0.010)	(0.009)
Sample Mean	$396	0.33	0.44
Controls	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes
N	3,190,874	3,190,874	3,190,874

Note: Results of estimating equation two to deployments that last between one and three months and omitting longer deployments. Discontinuous Care coefficient is the interaction of the treatment group identifier and the post period identifier. Controls include whether the Primary Care Provider (PCP) in that quarter is active duty military, and whether the PCP is a physician or advanced practice provider. All regressions include individual and cohort-time fixed effects as described in the text. Standard errors are clustered by patient. The deployment quarter is omitted from these regressions.