The Effects of Combat Deployments on Veterans' Outcomes*

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Abstract

As millions of soldiers deployed to Iraq or Afghanistan between 2001 and 2021, Veteran Affairs Disability Compensation payments quadrupled and the veteran suicide rate rose rapidly. We estimate causal contribution of combat deployments to declining veteran well-being. Deployments increase injuries, combat deaths, and disability compensation, but we find limited effects on suicide, deaths of despair, financial health, incarceration, or education. Our estimates suggest that deployment cannot explain either the recent rise in disability payments, which is more likely driven by policy changes, or the surge in noncombat deaths, which is better explained by shifts in observable characteristics of soldiers.

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War is a pervasive global phenomenon. Soldiers exposed to combat experience potentially life-altering physical, mental, and behavioral consequences that may linger long after they return from the battlefield. These unique dangers also raise important and politically fraught questions about how to compensate soldiers for, and insure them against, the risks of combat. Answering these questions requires understanding the impact of conflict on a range of veterans' outcomes, including physical and mental disability, mortality and suicide, and antisocial behavior.

Concerns about the risks of war are particularly salient in the United States, where almost 2 million servicemembers deployed to Iraq or Afghanistan following September 11, 2001 (Bilmes, 2021). Over this period, the outcomes of U.S. veterans deteriorated. As shown in Figure 1, the age- and sex-adjusted suicide rate of veterans rose nearly twice as fast as non-veterans, while real annual Veterans Affairs Disability Compensation (VADC) payments per living veteran rose from $900 to $4,700, a figure 10 times larger per eligible beneficiary than Social Security Disability Insurance (Autor and Duggan, 2006; US Department of Veterans Affairs, 2023).

Many lay the blame for these worrisome trends on the long-run behavioral and health consequences of combat (e.g. Stiglitz and Bilmes, 2008; Tanielian and Jaycox, 2008; Bilmes, 2021).[1] A majority of the public remains concerned that not enough is being done for veterans harmed by war (Frankovic, 2021). However, attributing deteriorating veteran outcomes to the causal effects of combat exposure is difficult because of many other changes over this period. In response to recruiting shortfalls over the 2000s, for example, the Army permitted soldiers to enlist with lower Armed Forces Qualification Test (AFQT) scores and granted more waivers for prior felony convictions (Department of Defense, 2020; Murphy, 2019). Policy changes also eased access to VADC (McMahon et al., 2009; Department of Veterans Affairs, 2010; Public Law 112-56, 2013).

This paper estimates the causal effect of combat deployments on veterans' VADC, noncombat deaths, including deaths of despair and suicides, and other key measures of well-being, such as criminal misconduct, educational attainment, and financial health. Unlike prior literature that relies on survey-based retrospective questions about military service, we construct a novel dataset that matches numerous military and non-military administrative records. The data permit us both to measure effects over long time horizons and to understand how trends in veteran outcomes have been impacted by changes in combat deployments, shifts in the observable characteristics of soldiers, and potential changes in policy.

Despite our detailed data, identifying the causal effect of combat deployments remains difficult be-

[1]This extends to policy makers as well. For example, President Biden has said, "A lot of our veterans and their families have gone through hell—deployment after deployment, months and years away from their families; missed birthdays, anniversaries; empty chairs at holidays; financial struggles; divorces; loss of limbs; traumatic brain injury; posttraumatic stress. We see it in the struggles many have when they come home... The cost of war they will carry with them their whole lives (Biden, 2021).

cause soldiers are not deployed at random. For example, unit commanders may prefer to bring their best soldiers to war and leave the rest behind. Soldiers with extenuating family or other circumstances may also remain in a rear-detachment and not deploy. To overcome these challenges, our empirical strategy leverages the quasi-random assignment of newly recruited soldiers to units. Soldiers are assigned to brigades by career managers using a limited set of observable characteristics. The command-and-control nature of the Army ensures that low-rank, first-term soldiers have virtually no ability to influence career managers' decisions. By conditioning on the appropriate set of covariates, we can compare soldiers assigned "as-good-as randomly" to near-identical units. Due to their staggered deployment cycles, some units deploy early on in the soldier's contract, while others deploy later or not at all. As a result, our strategy isolates variation in both whether soldiers deploy and for how long that is orthogonal to their observed and unobserved characteristics.

Our first finding is that combat deployments substantially increase VADC payments. An average 10 month deployment increases any VADC receipt by 9.4 percentage points (pp) and annual compensation by $2,602 eight years after enlistment. Some of this increase reflects the dangers of warfighting. Deployment causes a 4.4 pp increase in the probability of being wounded in combat and a 2.6 pp increase in the likelihood of having a formally documented health condition that limits the soldier's ability to continue serving in the Army. Some injuries are—tragically—fatal. We find an average 10 month deployment increases all-cause mortality by 0.53 pp (30% of the mean) within 8 years. Yet, direct injuries alone do not account for the large increases in disability compensation. Other channels, including physical overuse and psychological trauma from deployment, as well as the potential for the deployment experience to ease eligibility requirements, likely also contribute to the large observed VADC effects.

While deaths and injury from warfighting may be mechanically connected to deployment, noncombat deaths, including suicides and drug overdoses, are not. Our point estimates suggest an average deployment has limited effects on these outcomes. The estimated effect on overall noncombat deaths within eight years of enlistment is 0.05 pp. For deaths of despair, which primarily comprise suicide and drug or alcohol-related deaths, the estimated effect is 0.002 pp. As a result, deaths that occur as a direct result of combat explain 91% of the overall 0.53 pp mortality effect. However, estimated effects on noncombat deaths are relatively imprecise. Nintey-five percent confidence intervals cannot rule out a 0.40 pp increase overall (32% of the mean) and a 0.27 pp increase in deaths of despair (34% of the mean).

To better understand the potential adverse effects of deployment, we conduct two additional analyses. First, we exploit the fact that some soldiers are assigned to brigades that experience more intense and regular violence while deployed to analyze whether more dangerous deployments lead to worse outcomes. Soldiers assigned to brigades with higher casualty rates have increased risk for

combat death and injury and receive more VADC. However, we find that they are no more likely to die outside of combat. Importantly, large variation in casualty rates among units that deploy results in substantial power advantages relative to our analysis of the effects of the average deployment. For example, we can rule out that a standard deviation increase in casualty rates increases noncombat mortality within eight years by 0.09 pp (7% of the mean) and deaths of despair by 0.02 pp (3% of the mean) conditional on deploying for the same length of time.

Second, we explore how deployment affects other measures of well-being. While we find modest increases in separation from the Army that are comparable in magnitude to the increases in service-limiting injuries, we find no statistically significant evidence that deployments cause soldiers to be removed from service for misconduct or to be incarcerated. We find precisely estimated null effects on credit scores. We can rule out even modest declines in the probability of obtaining a college degree. Additionally, soldiers exposed to more violence on deployments of the same duration do not have worse misconduct, incarceration, credit, or educational attainment outcomes. These results are consistent with deployment having limited long-run adverse effects beyond death and injury directly resulting from combat, a finding that may reflect the extensive network of post-service support available to U.S. veterans.

We conclude by revisiting the striking trends in veterans' outcomes that have been the focus of public attention. We decompose between-cohort changes in average outcomes into components attributable to the causal effects of deployment, changes in soldiers' observable characteristics, and all other factors. The results show that deployment cannot explain trends in outcomes not directly related to war-fighting, such as noncombat deaths. However, observable factors such as AFQT scores and moral conduct waivers *are* closely connected to changes in these outcomes. Shifts in observables explain at least a third of the between cohort variation in noncombat death. This result suggests that some of the worrying trends in veterans' well-being are best explained by changes in who was allowed to serve rather than the effect of war itself.

The results also show that while deployment explains a large portion of the early 2000s increase in VADC receipt, more recently VADC and deployment have decoupled. The most recent cohorts of soldiers have some of the highest levels of VADC *and* the lowest deployment risk. When these later cohorts did deploy, they also faced substantially lower risk of death and injury. And yet, deployment causes significantly *larger* increases in VADC receipt for later cohorts than it did for earlier cohorts. In contrast, effects on Social Security Disability Insurance (SSDI) show no such pattern. A host of changes to VADC regulation and policy aimed at expanding the program likely explain its recent surge.

Despite a large, multidisciplinary literature on the effects of military service, causal evidence on

modern combat deployments remains scarce. Several papers explore the effects of compulsory and voluntary military service on earnings, education, disability, and mortality in the U.S. and elsewhere.[2] Related research has focused on specific aspects of service, highlighting both potential benefits (e.g., Barr, 2019; Barr et al., 2021; Wilson and Kizer, 1997; Breznitz, 2005; Borgschulte and Martorell, 2018) and risks, perhaps none more salient than combat. Numerous studies, primarily published in medical journals, have focused on links between deployment and health, including psychological and cognitive injuries.[3] Other work has linked combat deployments to divorce, alcohol use, domestic violence, and crime (Jacobson et al., 2008; Rohlfs, 2010; Negrusa et al., 2014; Anderson and Rees, 2015; Cesur and Sabia, 2016; Cesur et al., 2016; Hjalmarsson and Lindquist, 2019; Cesur et al., 2022).[4] Most of the analyses connecting combat deployments to well-being rely on survey data and observational research designs. We extend this important work by using high quality administrative data on soldiers' outcomes and by leveraging quasi-random variation in the soldier-to-unit assignment mechanism that, in our view, supports stronger claims to causality than previous research.

Our results also speak to a large literature, mostly studying developing countries, on the varied legacies of exposure to violence among civilians and armed group participants. These studies explore how war affects a variety of outcomes, including social behavior, voting, preferences, future violent acts, education, and financial decisions (e.g. Blattman, 2009; Blattman and Annan, 2010; Chamarbagwala and Morán, 2011; Leon, 2012; Callen et al., 2014; Bauer et al., 2016; Brown and Velásquez, 2017; Moya, 2018; Jakiela and Ozier, 2019; Brown et al., 2019; Couttenier et al., 2019; Brück et al., 2019; Blumenstock et al., 2021). We present new evidence on this question from the perspective of professional soldiers from the United States fighting abroad. Even for the most violent deployments, we find limited effects outside of combat death/injury and VADC, suggesting that the context in which one is exposed to violence (e.g., who is exposed, in what capacity, and the support networks they can access) likely matters.

Lastly, we contribute to a nascent literature on VADC, a program designed to insure U.S. soldiers against injuries incurred or aggravated while in service.[5] With real annual expenditures increasing from $28 billion to $112 billion since 2002, VADC is now 85% as large as SSDI despite only

[2]See, for example, Angrist (1990); Imbens and Klaauw (1995); Angrist (1998); Angrist et al. (2010, 2011); Card and Cardoso (2012); Bingley et al. (2020); Greenberg et al. (2022).

[3]See, for example Hoge et al. (2006); Milliken et al. (2007); Seal et al. (2007); Tanielian and Jaycox (2008); Gade and Wenger (2011); Cesur et al. (2013); Loughran and Heaton (2013); Bilmes (2021).

[4]Other studies explore how combat deployments impact families and children (Angrist and Johnson IV, 2000; Lyle, 2006; Engel et al., 2010). Bäckström and Hanes (2023) find no effect of Swedish peacekeeping deployments on earnings after accounting for selection.

[5]While studies have examined how VADC receipt impacts Vietnam-era veterans' labor supply (Autor et al., 2016; Coile et al., 2021) and veterans' health (Silver and Zhang, 2022), much less is known on the effects of recent combat deployments on disability (Heaton et al., 2012; Sabia and Skimmyhorn, 2023).

covering a fraction of SSDI's population. While VADC's growth has spurred calls for reforms (Washington Post Editorial Board, 2023) and the program has been cited as one that could generate large budgetary savings if reformed (Congressional Budget Office, 2022), it has received relatively little attention from economists, in sharp contrast to the large literature exploring potential reasons for the growth of SSDI in the 1990s and 2000s (see, for example, Black et al., 2002; Autor and Duggan, 2003, 2006; Duggan and Imberman, 2009; Von Wachter et al., 2011; Burkhauser and Daly, 2012; Mueller et al., 2016).[6] The limited research on VADC could reflect a tendency to attribute its unparalleled growth to the costs of insuring veterans against the risk of deployments to Iraq and Afghanistan, as suggested by Stiglitz and Bilmes (2008), Edwards (2014), and others. Our results suggest that the large, long-term cost of compensating veterans has at least in part been driven by policy choices rather than the direct effects of combat.

1 Data and summary statistics

1.1 Administrative military personnel records and outcome data

Our data combine Army personnel records with administrative data on disability and mortality, allowing us to observe outcomes that extend beyond an individual's time in the military. We also link to additional data with national coverage of criminal, credit, and education outcomes. Our Army data include soldier demographic characteristics, Armed Forces Qualification Test (AFQT) scores, education levels, and home of record information determined at the time a soldier enlists, as well as a monthly panel of assignment data (i.e., assignment location, brigade of assignment) that extends through the last month of a soldier's service. We determine deployment status from Army pay records that indicate receipt of Imminent Danger Pay (also known as Hostile Fire Pay), which is only paid to soldiers who serve in a combat zone. Although our records do not reveal the precise location of deployments, combat deployments to locations other than Iraq and Afghanistan were incredibly rare for soldiers assigned to brigade combat teams during the period we study.

Our disability data combine payments for Veterans Affairs Disability Compensation (VADC) with payments for Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI). All payment amounts reflect annual payments adjusted for inflation to 2020 USD using the CPI-U. Mortality data from the National Death Index (NDI) contain the date and cause of death for deceased soldiers. We also link to incarceration data from LexisNexis and Army records,

[6]Exceptions include Angrist et al. (2010), who attribute the differential impact of Vietnam-era service on federal transfer income among lower skilled white men to the relative attractiveness of VADC for this group; Autor et al. (2016), who show that by 2006, the 2001 Agent Orange policy decision increased VADC enrollment by 5 pp among Vietnam-era veterans who served in theater relative to Vietnam-era veterans who did not serve in theater; and Coile et al. (2021), who find evidence that growth from Agent Orange policies continued beyond 2006.

additional misconduct and criminal outcomes from Army personnel and criminal records, credit data from Experian, and post-secondary education data from the National Student Clearinghouse. All of our outcomes extend through 2019 except for credit and education outcomes, which extend through 2020. Appendix B.1 contains additional information on our data.

1.2 Sample construction

Our unit of observation is a first-term soldier. Our baseline estimation sample consists of first-term enlisted soldiers assigned to a brigade combat team (BCT) between 2005 and 2015. BCTs have been the Army's predominant maneuver fighting force since 2005, the same year that the Army began assigning personnel to brigades instead of larger, divisional units. Restricting the sample to soldiers assigned to BCTs excludes soldiers assigned to training units that rarely deploy or to support units that have highly heterogeneous experiences while in garrison. We exclude soldiers assigned to BCTs in overseas locations outside the United States. Since our identification strategy compares soldiers in the same military occupation assigned to different BCTs in the same location and year, we necessarily exclude soldiers assigned to locations with a single BCT. Finally, we restrict our sample to male soldiers.[7] Our final analysis sample consists of 157,415 soldiers assigned to one of between 20 and 35 active BCTs, depending on the year.[8]

1.3 Summary statistics

Table 1 presents summary statistics. Column (1) describes the universe of first-term soldiers under standard enlistment contracts who arrived at their first unit between the years of 2001-2015. Column (2) reports averages after restricting to male soldiers who arrived at their first units between 2005 and 2015. Column (3) shows averages for our estimation sample. Relative to the average first-term male soldier who enlisted between 2005 and 2015, our sample is less likely to be Black, has lower average levels of education, and has slightly lower average AFQT scores. These differences are largely driven by occupational differences that emerge from our restriction to soldiers whose initial assignment is to a brigade combat team. Panel (b), for example, shows that 64% of soldiers in the estimation sample work in combat occupations compared to only 43% of male soldiers assigned to all units between 2005 and 2015.

Column (4) restricts our estimation sample to the subset of soldiers who did not deploy. Column

[7] Women constitute roughly nine percent of first-term soldiers in BCTs. We exclude them because during the period we study women were precluded from serving in combat occupations. Prior to 2012, women assigned to BCTs were only permitted to be assigned to Brigade headquarters or support battalions. Women are also not permitted to deploy while pregnant, and our data do not permit us to observe all cases of pregnancy. Nevertheless, including women in our sample yields similar estimates (see Section 3.6).

[8] See Table A.1. Appendix B.2 contains additional details on sample construction.

(5) restricts to soldiers who deployed. These columns reveal that soldiers who deploy are less likely to be Black, less likely to be Hispanic, have lower levels of education, and have higher AFQT scores. What drives this selection? First, soldiers in combat occupations are more likely to deploy. This is true even conditional on initially being assigned to a BCT. Many soldiers rotate to a new unit after two to three years, and soldiers in noncombat jobs are more likely to rotate to a unit with a low propensity to deploy. Second, changes in deployment rates over time may be correlated with changes in the composition of soldiers. Finally, when BCTs deploy, unit commanders have discretion in determining which soldiers to leave behind in garrison as a "rear-detachment" that facilitates administrative matters. Soldiers who perform poorly in training or misbehave are more likely to remain in the rear guard. We also expect that soldiers with extenuating personal or family circumstances may be more likely to be left behind. Differences in the observed and unobserved characteristics of soldiers who do and do not deploy motivates the instrumental variables (IV) strategy we describe next.

2 Identifying the causal effects of deployment

2.1 "Faces-to-spaces": the soldier-unit assignment mechanism

After completing initial training, soldiers are assigned to their first unit primarily based on Army personnel requirements (Army Regulation 614-200). The starting point of this process is the Army's demand for soldiers, as determined by personnel structure documents that identify the number of soldiers required in each unit within each occupation and rank, and senior leader guidance on how to manage personnel shortages. The U.S. Army's Human Resources Command (HRC) compares the demand for soldiers to the current supply, expected attrition, and expected training requirements to project entry-level soldier vacancies at the brigade-by-occupation level 7 to 18 months into the future.[9]

The"faces-to-spaces" system matches first-term soldiers to projected vacancies given a soldier's Military Occupational Speciality (MOS) and occasionally soldier-specific factors (e.g., if a soldier is married to another servicemember he will often be assigned to the same location as his spouse). Soldiers are permitted to submit a short list of preferences over specific duty-stations (e.g., Fort Carson, CO, or Fort Bliss, TX).[10] But given the hierarchical, command-and-control nature of military service, low-ranking, first-term soldiers have virtually no ability to influence which specific

[9] Army units have the following structure: Corps ⇒ Division ⇒ Brigade ⇒ Battalion ⇒ Company. Since 2005, HRC has managed personnel assignments at the brigade-by-occupation-by-rank level.

[10] Specific details on the assignment process come from Army Regulation 614-200, knowledge acquired through conversations with HRC officials, and the first-hand work experience of a member of this research team (Kyle Greenberg, who was recently assigned to HRC for his military assignment).

brigade they are assigned within a given duty-station. Variation is driven by idiosyncratic choices made by the HRC career manager, who must make hundreds of assignment decision each month in addition to their other duties. As a result, brigade assignments are as good as random conditional on MOS, duty-station, contract term-length, and arrival time.[11] In support of this claim, we show evidence of balance on observable characteristics in Section 2.5.

2.2 The ARFORGEN model

Beginning in 2004, Army leadership implemented the Army Force Generation (ARFORGEN) model that established a cycle of training, deployment, and reset for all Army brigade combat teams (United States Army, 2011; Johnson et al., 2012). The purpose of ARFORGEN was to sustain the warfighting capability of the all-volunteer force during extended periods of conflict in Iraq and Afghanistan. To this end, a key goal of ARFORGEN was to ensure BCTs were self-sustaining and interchangeable to facilitate unit replacements during combat operations (Johnson et al., 2012). The Army accomplished this by standardizing stateside (pre-deployment) training regimens and unit structures through a process known as "modularity".

ARFORGEN also aimed to create a regular, cyclical deployment timeline designed to ensure all BCTs had sufficient time to prepare for combat deployments and to rest and recuperate upon return. While official Army orders directing where and when units are to deploy are classified documents, our data allow us to confirm a cyclical, although not entirely predictable, pattern in the share of soldiers within a BCT who were deployed at any one time (see Figure A.1 for an example). As a general rule, the majority of soldiers assigned to a BCT would deploy for 9-15 months, followed by anywhere from one to five years stateside.

2.3 Research design

Our rich data make it straightforward to account for changes in observable characteristics across cohorts and selection into different military occupations. However, simple controls for soldier characteristics will not isolate the causal effects of combat deployments because soldiers are not randomly sent to war. As discussed in Section 1.3, even within unit and occupation, commanders may elect to bring their best soldiers overseas.

To overcome the endogenous selection into deployment, we use an instrumental variables (IV) approach that exploits BCT-level variation in deployment. At each point in time, each BCT within

[11] Contract term-length refers to the length of time a soldier commits to serve during his initial enlistment. Term-lengths influence soldiers' unit of assignment because they mechanically influence how long a soldier serves in the Army (absent unexpected attrition), thus influencing a unit's projected vacancies.

a duty-station will have a different likelihood of being deployed in the short and medium term due to the ARFORGEN deployment cycle. Soldiers randomly assigned to a BCT that is about to deploy will be more likely to deploy, and spend more total time deployed, than a soldier assigned to a BCT that has just returned from overseas.

To illustrate the variation exploited by our research design, consider a stylized example. Suppose Private Bruhn and Private Greenberg are both newly recruited soldiers that enlisted in the Water Treatment Specialist occupation. They arrive at their first assigned duty-station, Fort Drum, New York, in 2005. Private Bruhn is assigned to the First BCT, which will not deploy until 2008; Private Greenberg is assigned to the Second BCT, which will deploy within the next calendar year. The institutional details of the soldier-unit assignment procedure imply that which soldier was assigned to which brigade is as-good-as random. Thus we can compare Privates Bruhn's and Greenberg's outcomes to estimate the causal effect of assignment to Second Brigade relative to First Brigade. If the primary way brigade assignment affects outcomes is through exposure to deployment—a point we discuss further below—then brigade assignment can be used as an instrument for deployment.

2.4 Empirical implementation

We implement the research design using the following two-stage least squares (2SLS) model:

$$Y_i = \delta_{k(i)} + \beta D_i + \epsilon_i \tag{1}$$

$$D_i = \omega_{k(i)} + \pi Z_i + u_i \tag{2}$$

Here Y_i is the outcome of soldier i measured at a specific time horizon relative to year of assignment at first duty-station; $\delta_{k(i)}$ and $\omega_{k(i)}$ are duty-station by job by year of arrival by term-length fixed effects.[12]

Our treatment variable, D_i, measures the number of months that soldier i spent deployed within three years of arrival at their initial duty-station. Three years is the modal enlistment term length and hence a natural period over which to capture a first-term soldier's deployment exposure. Measuring deployment over other time horizons (e.g., eight years) yields similar results, as discussed below.

[12]There is some ambiguity regarding the relevant time horizon that assignment officers consider when making soldier-unit matches. Our preferred model uses a relatively large window to leverage as much variation as possible. However, we obtain similar results using quarter of arrival instead. Moreover, including covariates such as demographics, AFQT, any moral waiver, high school graduate, family status, ASVAB composite line scores, and medical and drug testing results does not impact our estimates, consistent with the balance results discussed below.

The instrument Z_i is the leave-out mean of D_i for all soldiers in our sample other than i assigned to the same brigade in the same quarter:

$$Z_i = \frac{1}{n_{bq} - 1} \sum_{\ell \in N_{bq(-i)}} D_\ell \tag{3}$$

where $N_{bq(-i)}$ is the set of all soldiers other than i assigned to brigade b during quarter q and $n_{bq} = |N_{bq}|$ is the total number of soldiers assigned to brigade b during quarter q. We report heteroskedasticity-robust standard errors (White, 1980).

The coefficient β on D_i captures the causal effects of combat deployment. Because D_i is an ordered treatment, in the absence of controls and under standard assumptions validated below, β identifies a weighted average of effects of exposure to different "doses" of treatment for potentially overlapping sets of compliers, an estimand known as the "Average Causal Response" or ACR (Angrist and Imbens, 1995). For a binary instrument $\tilde{Z}_i$, it can be written in potential outcome notation as:

$$\frac{E[Y_i|\tilde{Z}_i = 1] - E[Y_i|\tilde{Z}_i = 0]}{E[D_i|\tilde{Z}_i = 1] - E[D_i|\tilde{Z}_i = 0]} = \sum_{d=1}^{\bar{D}} \omega_d E[Y_i(d) - Y_i(d-1)|D_i(1) \geq d > D_i(0)] \tag{4}$$

where $\omega_d = \frac{\Pr(D_i(1) \geq d > D_i(0))}{\sum_{l=1}^{\bar{D}} \Pr(D_i(1) \geq l > D_i(0))}$. With a multi-valued instrument, the estimand averages these averages of causal effects along the support of the instrument.

We therefore interpret our estimates as capturing a combination of extensive (i.e., no deployment vs. some) and intensive margin (i.e., some deployment vs. more) effects.[13] Figure A.2 reports estimates of the weights ω_d put on each dose. While it is impossible to separately identify the individual dosage effects in the ACR without strong assumptions (Rose and Shem-Tov, 2021), a natural question is *how much* of our variation comes from the extensive margin vs. intensive margin. This quantity is not point identified, but it can be bounded using the procedure described in the appendix of Garin et al. (2023). We find that at least 46% of our variation—and possibly as much as 89%—is on the extensive margin. Rose and Shem-Tov (2021) show it is also possible to estimate untreated complier means for extensive margin compliers, i.e., $E[Y_i(0)|D_i(1) > D_i(0) = 0]$. We use these estimates to characterize averages outcomes under no deployment.

We therefore view our 2SLS estimates using length of deployment as the endogenous variable as

[13] Our specification also includes a saturated set of control variables in the form of the fixed effects, but no interactions between the instrument and controls. Such models capture positively weighted averages of causal effects for compliers when (roughly) the first stage effect has the same sign for all covariate groups (Blandhol et al., 2022), a natural restriction in our setting.

informative about the effects of "any deployment" relative to none. Throughout the analysis, we refer to estimated causal effects—scaled by a factor 10 to capture the mean deployment length—as the impact of an average deployment. However, we also explore multiple-endogenous variable models that allow for non-linear effects of deployment. As we show below, these models find similar implied effects of receiving 10 versus zero months of deployment to our re-scaled primary 2SLS estimates.

2.5 Instrument validity

Table 2 presents evidence to support the assumption that soldiers are as-good-as-randomly assigned to brigades conditional on our set of fixed effects. Column (1) reports estimates of Equation 2 where the outcome has been replaced with pre-treatment soldier characteristics. The leave-out months deployed instrument is not correlated with individual covariates and does not jointly predict them (the p-value of a joint F-test is 0.27). Figure 2 presents further evidence of covariate balance by non-parametrically regressing predicted months deployed, formed using a regression on all available exogenous covariates, on the instrument. Variation in the instrument itself is plotted in the histogram.[14] We see no relationship between our instrument and predicted months deployed, despite a strong relationship with actual months deployed.

Column (2) of Table 2 presents naive OLS regressions of pre-treatment soldier characteristics on the months-deployed treatment variable and our baseline set of fixed effects. In contrast to the balance on our leave-out months deployed instrument, actual months-deployed is strongly conditionally correlated with soldiers' characteristics. Among soldiers in the same occupation, duty-station, term-length, and arrival year group, those with high school diplomas and higher AFQT scores deploy for longer on average, while soldiers who are married, have more dependents, and who are older deploy less. These patterns of observable selection are consistent with non-random deployment even within occupation and unit, potentially as a result of commanders' ability to select which soldiers to bring to war.

For BCT assignment to serve as a valid instrument, it also must satisfy an exclusion restriction.[15] In our setting, exclusion requires assignment to different BCTs to affect outcomes only through the quantity of time spent deployed. While it is possible that individual BCTs may also directly affect outcomes independent of deployment, we view exclusion as a reasonable assumption in this context for several reasons. First, BCTs are designed to be interchangeable units, and the Army's

[14] Both variables are residualized on the duty-station by job by initial assignment period by term-length fixed effects from our primary specification, with their sample means added back in for interpretability.

[15] In a heterogeneous effects framework, we also require monotonicity, which implies that no soldiers would find a way to spend more time deployed if assigned to a brigade with no pending deployment than they would if assigned to a brigade with a pending deployment.

ARFORGEN system highly standardized the stateside training sequence soldiers received as they prepared for their next deployment, as discussed in Section 2.2. By comparing soldiers assigned to different brigades within the same duty-station, our identification strategy ensures soldiers have nearly identical stateside training environments regardless of their unit assignment.

Second, the process of equipping and training units for deployment requires a well-established cycle that is difficult to deviate from for both logistical and political reasons. The same brigade experiences varying deployment propensities over time as it progresses through this cycle, making it unlikely that our effects are driven by the impact of assignment to particular BCTs that persistently deployed more than others and may differ in other ways (e.g., in unit culture). The cyclical nature of deployment also means that the Army is not picking its best or worst units to deploy (something that is unlikely to occur in any case given the lack of unit-level performance data).

For these reasons, we believe that interpreting our 2SLS estimates as treatment effects of deployment is reasonable. However, we also report reduced form estimates of Equation (2) for our main outcomes in Table A.2. Due to high compliance rates, these reduced form estimates are only slightly smaller in magnitude than the 2SLS estimates that follow. The reduced form estimates can be interpreted as the effect of being assigned to a BCT where a large share of other first-term enlisted soldiers deploy regardless of whether exclusion is satisfied. We also study the reduced form effects of indicators for assignment to each BCT as a test for whether BCT assignment affects outcomes *at all*, regardless of the causal channel. Finally, our empirical strategy can also be viewed as a "judge fixed effects" design, with the BCT-by-quarter interactions serving as the "judges." To further support the validity of the instrument, we perform the tests proposed by Frandsen et al. (2023) and detect no violations, as shown in Table A.3.

2.6 First stage

Figure 2 shows that the relationship between the instrument and total months deployed within three years of arrival is approximately linear and precisely estimated; the coefficient from a linear regression is 0.961 (s.e. $= 0.005$), indicating that assignment to a BCT with longer peer deployment translates roughly one-for-one into a soldier's own expected time deployed. By contrast, predicted deployment formed using a regression of deployment on all available covariates is flat over the full support of the instrument, supporting the claim to as-good-as-random assignment discussed above. Moreover, Figure A.3 shows that the instrument also meaningfully shifts the likelihood of *any* deployment within three years of arrival.

Panel (a) of Figure 3 shows the dynamics underlying the first-stage effect in Figure 2 by plotting the relationship between the instrument and months deployed within varying horizons of arrival.

The shape and shade of the points changes at 48 months post-arrival, when our sample goes from being balanced to unbalanced.[16] Initially, effects are are small since soldiers have only been in the Army for several months and have had limited opportunities to deploy. Large differences then emerge. Eight years after assignment the first stage coefficient remains close to one, implying that initial exposure effects are highly persistent.

Initial BCT assignment is also strongly correlated with exposure to combat. Figure 3 panels (b)-(d) report 2SLS estimates of the effects of deployment on combat-related outcomes such as suffering a combat injury (being "Wounded in Action," or WIA), suffering a serious combat injury, and being killed in combat.[17] Point estimates are scaled by 10 to reflect the effect of a ten-month deployment, which is roughly the average number of months deployed during the first three years of service among soldiers in our sample who ever deployed (9.87 months, column (5) of Table 1). All binary outcomes are expressed in percentages, so Figure 3 panel (b), for example, suggests that an average 10 month deployment increases the probability of having any recorded combat injury 8 years after arrival by 4.43 pp. Across outcomes, the results clearly show deployment strongly affects exposure to combat, violence, and injuries.

3 Causal effects of deployment

This section presents three sets of results on the effects of combat deployment. We begin with impacts on VADC and examine whether they are driven by injuries sustained in war, more general physical and mental trauma, or deployment itself facilitating access to the program. We then estimate impacts on mortality and noncombat deaths. Finally, we examine additional outcomes such as misconduct, incarceration, credit, and educational attainment.

3.1 Disability compensation

Figure 4 panels (a) and (b) plots estimates of the causal effect of deployment on whether the soldier receives any VADC and total VADC payments (in 2020 USD) in the most recent calendar

[16] For example, since we do not observe most outcomes beyond December 2019, our estimates at five years after arrival exclude any soldiers who arrived at their first operational assignment after December 2014.

[17] The definitions of these outcomes follow Department of Veterans Affairs (2008). WIA is defined as an injury resulting from adversarial action. Serious combat injuries are defined as an injury from adversarial action that is life-threatening or life-altering, or where death is possible within 72 hours. Combat deaths include soldiers identified as "Killed in Action" (KIA) in official casualty records from the Defense Casualty Analysis System (94% of total), soldiers who die in Iraq, Afghanistan, or Kuwait as a result of unspecified vehicle accidents (ICD-10 code V899; 4%), noncommercial aircraft accidents (ICD-10 code V958; 2%), or explosions of blasting or other materials (ICD-10 code W40; $<$1%), and soldiers identified in the NDI as dying from war that are not recorded as KIA in casualty records (ICD-10 codes Y35 and Y36; $<$ 1%). 99% of KIA deaths from casualty records are also identified as war deaths in the NDI.

year before the time indicated on the x-axis. As above, the point estimates are scaled up by a factor of 10 to reflect the effect of an average deployment. Effects are initially small as soldiers remain in service. They increase rapidly three or four years after arrival, however. By eight years after arrival, deployment causes large increases in both the likelihood of receiving any VADC and in total dollars received.

Table 3 reports corresponding point estimates for these and other outcomes at select time horizons. Panel (a) displays results for any and total annual VADC receipt in the most recent calendar year. The table also reports analogous results for all disability programs we observe (VADC, SSDI, and SSI). As with Figure 4, we have scaled the coefficients by a factor of 10 so that the point estimate can be interpreted as the causal effect of an average-length deployment. All binary outcomes are expressed in percentages. We use the same conventions for all subsequent tables.

Eight-years after arrival at first duty-station, deployment increases the likelihood of receiving any VADC by 9.4 pp off a base of 37% and increases total VADC payments by $2,602 dollars per year, which is 42% of the amount paid to the average soldier in our sample. Deployment increases SSDI and SSI payments by $426, the vast majority of which is driven from SSDI rather than SSI. Total annual disability payments from all three programs thus increase by $3,029, with VADC accounting for the bulk of this effect.

Part of the effects of deployment on VADC likely reflects the fact that sending soldiers into conflict results in injuries that require long term care and hence qualify soldiers for compensation. We explore this directly in panel (b) of Table 3, which presents effects on various indicators of trauma that occur during combat or military service more generally. Consistent with the results reported in Figure 3, deployment increases the likelihood that a soldier suffers a combat death within eight years by 0.48 pp and increases the likelihood that a soldier experiences a combat injury by 4.4 pp The magnitude of these effects is meaningful. For example, a 0.48 pp increase is 38% of the mean noncombat death rate (1.25 pp).

Since combat injuries only capture injuries during combat deployments, they cannot affect soldiers who do not deploy. We therefore turn to Army medical personnel records to better understand the impact of combat deployments on all injuries. Specifically, "Any Army Profile" is an indicator that equals one if the soldier has a "medical profile," which is formal documentation of a temporary or permanent medical condition that limits the soldier's ability to perform assigned duties. Only 43% of soldiers in our sample who experienced combat injuries also received a medical profile, suggesting that many combat injuries do not substantially limit a soldier's physical performance. We also explore the impact of deployments on receipt of a "significant profile," an indicator that equals one if the soldier has a medical profile that the Army deems severe enough to limit their

ability to continue to serve.[18]

In the short term, deploying reduces the likelihood that a soldier receives any medical profile (-.76 pp within the first two years of arrival). This is likely due to the fact that soldiers with certain types of profiles are barred from deploying. Thus commanders will often require soldiers with medical profiles to be medically re-evaluated in the run up to deployment to ensure that any temporary profiles are removed as soon as the underlying issue has cleared up. However, by eight years after arrival the average deployment causes an 1.71 pp increase in the likelihood of having any medical profile, a 7% increase relative to mean profile rates (25.5%). The average deployment also increases the likelihood of having a significant medical profile by 2.62 pp, a 17% increase relative to mean significant profile rates (15.0%).

To what extent can effects on injuries alone explain our estimated disability impacts? Table A.4 investigates the association between injuries and future VADC receipt. Among those who deploy, a combat injury in the first term is associated with a 24.45 pp increase in VADC receipt and an $8,663 increase in VADC payments. Applying this estimate to the 4.43 pp increase in combat injuries caused by deployment by year eight, we would expect combat injuries to explain around a 1.08 pp increase in any VADC receipt and a $384 increase in payments. We reach similar conclusions when using significant Army profiles instead of combat injuries. As such, injuries sustained in combat plausibly explain only a small portion of the overall effect of deployment on VADC.

Table A.5 further explores the drivers of VADC receipt by showing how deployment affects VADC receipt for the top five most common conditions for veterans of the Global War on Terror (US Department of Veterans Affairs, 2022). Veterans can have multiple conditions associated with their VADC, so effects do not necessarily sum to the total effect. Deployment has the largest effects on receiving any VADC with a tinnitus (ringing in the ears) diagnosis and any VADC with PTSD, with the latter being particularly large (a 12.82 pp increase or 77% of the mean).

Some of the effects of deployment on VADC may also reflect the fact that soldiers cannot receive disability while on active duty and that deployment increases long-run separation from the Army. Table A.6 shows that eight years after arrival, deployment increases separation by 2.6 pp (relative to a mean of 83%). However, even under extreme assumptions, separation can only account for a relatively small portion of the estimated VADC effects. If, for example, the additional 2.6 pp of soldiers who separate as a result of deployment received the 99th percentile of VADC payments ($46,000) by year eight and otherwise would not have received VADC at all, effects on separation

[18] See Department of the Army Pamphlet 40-502, "Medical Readiness Procedures" for a formal description of temporary and permanent medical profiles, as well as a medical profile functional guide the Army uses to distinguish between temporary and permanent profiles. Roughly 91% of soldiers in our sample with a significant profile at the end of their fourth year of service were no longer in the Army two years later.

would explain 2.6 pp of the 9.42 pp effect on VADC receipt and $1,196 of the $2,602 effect.

Taken together, these results suggest that deployment increases VADC eight years after arrival in part due to increased separations and in part due to combat-related injuries, but, predominantly, due to conditions not tied to a specific injury recorded in our data. These conditions may be the consequences of physical overuse or psychological harm resulting from deployment. However, it is also possible that the experience of deployment increases VADC receipt by directly increasing access. VADC is available to soldiers for any illness or injury that can be connected to their military service. Section 3.304(d) of Title 38 of the Combined Federal Registry (CFR) explicitly states that "satisfactory lay or other evidence that an injury or disease was incurred or aggravated in combat will be accepted as sufficient proof of service connection," implying that serving in a combat zone can make it easier for veterans to meet the required threshold of evidence. This is particularly true for PTSD claims, where stressors linked to combat or a veteran's fear of hostile military or terrorist activity consistent with the veteran's service are sufficient (38 CFR § 3.304(f)). Other channels, including, information dissemination, additional screening, peer effects, or changes in expectations about the probability of a successful claim may also disproportionately encourage soldiers who have deployed to apply for and receive VADC.

3.2 Noncombat deaths and deaths-of-despair

Figure 5 displays results for mortality outcomes derived from the National Death Index at various time horizons. The figure reports results for all-cause mortality, deaths due to combat, all noncombat deaths, and specific subcategories of noncombat deaths including deaths of despair, suicide and drug or alcohol-related deaths,[19] and deaths resulting from motor-vehicle accidents, assault, and all other causes. We find large and statistically significant effects of deployment on overall mortality. Within two years of arrival at first duty-station, the average deployment causes a 0.50 pp increase in deaths. This effect remains stable over longer horizons.[20]

Over 90% of this effect is due to deaths resulting from combat (0.48 pp at eight years after arrival). By contrast, the impacts of deployment on overall noncombat death and its subcategories are substantially smaller and not statistically significant. Eight years after arrival, point estimates imply that deployment increases noncombat deaths by 0.05 pp overall, or roughly 4% of the mean (1.25 pp), has no effect on deaths of despair, and, if anything, a slight negative effect on suicides (-0.02 pp). However, because mortality is a rare outcome among the individuals in our sample,

[19] 98% of deaths of despair are suicides or deaths resulting from drugs or alcohol (or both). The remaining 2% are firearm deaths resulting from undetermined intent.

[20] Table A.7 reports the point estimates and standard errors associated with Figure 5 and the overall mean after eight years from enlistment.

who are typically around the age of 22 when they arrive at their first duty-station, these results are not estimated with sufficient precision to rule out meaningful adverse effects. For example, 95% confidence intervals only allow us to rule out that deployment increases noncombat deaths within eight years by 0.40 pp, which is 32% of the mean.

Despite these wide confidence intervals, several additional pieces of evidence suggest the effects of deployment on noncombat deaths are unlikely to be large. First, in the remainder of this section we show that deployments have no adverse impacts on other outcomes for which we can estimate effects more precisely. These results demonstrate that if deployment increases noncombat deaths in ways we are simply not powered to detect, these effects are not associated with a broader deterioration in veterans' outcomes. Second, Section 4 shows more precise evidence that exposure to violence while deployed does not increase noncombat deaths. These results imply that any impact of deployment on noncombat deaths does not likely flow through exposure to combat and violence itself. Finally, in Section 5 we show that cohort trends in noncombat deaths are better explained by selection than effects of deployment. These results provide an alternative explanation for why veterans' noncombat deaths surged since the wars in Iraq and Afghanistan began.

3.3 Misconduct, credit scores, and education

Panel (a) of Table 4 explores whether deployment causes soldiers to be separated from the Army for misconduct or incarcerated at any point during or after military service. Within two years of arrival at first duty-station, deployment reduces separation for misconduct, but this is almost certainly due to mechanical and administrative impacts of deployment. Soldiers may have less opportunity to misbehave while deployed and commanders will often defer Army separation proceedings until after the unit has returned stateside. Consistent with this idea, effects on ever separated for misconduct quickly revert to nearly zero by year four and remain at zero eight years after arrival at first duty-station. These estimates are precise: we can rule out increases in misconduct separations larger than 1.4% of the mean. Although less precise, results for incarceration suggest that deployment increases incarcerations by a statistically insignificant 0.1 percentage point (4% of the mean) within eight years. Consistent with null effects on antisocial behavior, the results reported in Table A.8 show that while soldiers remain in the Army, deployment does not increase demotions or the probability of becoming the subject of military investigations.[21]

Panel (b) of Table 4 shows that deployment has a precise null effect on Vantage credit scores from the Experian credit bureau. We consider the Vantage credit score to be an omnibus measure of financial health, but report on additional credit outcomes and on national foreclosure outcomes

[21] Results on demotions and military criminal investigations suffer from a censoring problem because we only observe these outcomes while soldiers are in the military, which Table A.6 shows is also affected by deployment.

from LexisNexis in Table A.9. Since we only have access to credit bureau data at two points in time (June 2017 and December 2020), panel (b) of Table 4 only reports credit score results as of these two dates pooled across all enlistment cohorts. The point estimate from 2020 indicates that deployment increases Vantage scores by 1.91 points on average, which is small relative to the mean score of 655 and the standard deviation of 92 within our sample. Taking the lower bound of the 95% confidence interval, we can rule out that deployment decreases credit scores by more than 0.1% of the mean (less than 1% of a standard deviation).

Finally, panel (c) of Table 4 shows that deployment may have a small, positive effect on ever having enrolled in college by year six, but this effect is indistinguishable from zero by year eight. Similarly, deployment appears to have no effect on earning an associate's degree or higher.[22] These estimates are precise enough to rule out that a ten month deployment decreases college enrollment by 0.5 pp (1% of the sample mean) and decreases degree attainment by 0.26 pp (3% of the mean).

3.4 Discussion

What explains the null effects of deployment on deaths outside of combat, misconduct separations, incarceration, education outcomes, and credit scores? One possibility is deployments simply have limited average effects on these medium- and long-run outcomes given current support networks and resources for veterans. While deployments cause trauma, they do so for a subset of those who deploy, and the physical or mental harm incurred among this subset need not necessarily lead to severe consequences on the dimensions measured, especially if given adequate support.

We also explore whether remaining stateside for many months waiting to deploy worsens outcomes, which would cause us to underestimate the harmful effects of deployment, but find evidence arguing against this hypothesis. As noted in Section 3.3, for example, deployment appears to reduce separation for misconduct in the short-run. To better understand these patterns, Figure A.4 plots average outcomes **within** a specific quarter after arrival among compliers left stateside for the duration of their contract (i.e., $E[Y_i(0)|D_i(1) > D_i(0) = 0]$). In a separate series, we add the treatment effect of a 10 month deployment to these means to represent outcomes of a typical deployer. For non-deployers, most non-violent felonies and misconduct separations occur within 6 quarters of arrival. Among deployers, the same pattern happens 1.5-3 years later. There is no evidence of steadily increasing misconduct rates for non-deployers as they remain on post. Thus short-run dynamic effects on misconduct primarily reflect shifting behavior across time without

[22] For consistency with credit outcomes, Table A.10 reports results on these and additional education outcomes as of June 2017 and December 2020. These results show deployment may have a small, positive effect on college enrollment but no effect on earning an associate's or bachelors degree by 2020.

changing its long-run prevalence. This suggests soldiers behave similarly across treatments, but deployment simply delays the consequences of this behavior or potentially some of the behavior itself.

Another possibility is that other positive consequences of deployment offset its adverse effects. While many have focused on the link between deployment and PTSD (Hoge et al., 2004, 2006; Seal et al., 2007; Tanielian and Jaycox, 2008), others have suggested that deployment could also increase skills, for example by building resilience or discipline (Pietrzak et al., 2010; Tsai et al., 2015), or by improving career opportunities inside or outside of the military (Hall et al., 2014; Parker et al., 2019). Table A.11 shows evidence of a modest increase in Army promotion rates due to deployment (despite increased separations) in our sample, though this effect could partly be due to reasons other than improved skills such as increased military awards linked to deployment.

Deployment also increases both earnings during service (through extra combat-related pay and tax deductions) and, importantly, VADC compensation. While Table A.11 shows that a 10 month-deployment increases cumulative Army pay over the first four years of service by around $5,000, or 3% of the mean,[23] the same deployment increases VADC by $2,600 *annually*. This extra compensation may offset any negative effects on the outcomes we measure. We view this explanation as less likely, however. Although the few studies that examine the link between VADC and health find positive impacts, there is limited evidence of improvements in mortality (Autor et al., 2016; Trivedi et al., 2022). Moreover, Silver and Zhang (2022) find that VADC payments improve self-reported health and decrease food insecurity and homelessness, but have no impact on mortality, blood pressure, HbA1c glucose levels, body mass index, major depressive disorder, and alcohol and substance use disorders, although the average veteran in their sample is substantially older than ours.[24]

Several additional results in Sections 4 and 5 provide further evidence that the extra disability compensation is unlikely to explain the null impacts of deployment on mortality and other adverse outcomes. In Section 4, we continue to find null effects across differentially risky deployments, although even the least dangerous deployments still have large impacts on VADC ($1,876, or 72% of the overall effect). This suggests that if VADC is offsetting negative effects of deployment, these effects are not tightly tied to combat exposure. In Section 5, we estimate effects across time and

[23] This does not account for the combat zone tax exclusion which is an additional monetary benefit.

[24] Recent studies of lottery winners also find no effect on mortality (Cesarini et al., 2016). In contrast, other work has found that additional income through SSDI reduces mortality among lower-income beneficiaries (Gelber et al., forthcoming), and that job loss or poor labor market conditions can lead to worse health outcomes and increased mortality (Sullivan and Von Wachter, 2009; Maclean, 2013; Currie and Schwandt, 2014; Schwandt and Von Wachter, 2023). Ultimately, since those receiving VADC in our sample differ from these other contexts in terms of age, health, and employment status, and since VADC is generally not work-limiting (Autor et al., 2016), it is difficult to extrapolate from these other contexts.

find that impacts on VADC more than double from $1,383 to $2,974 between the 2005-2007 cohort and the 2011-2013 cohorts. Over this time, deployments also became substantially less dangerous. Yet the effects of deployment on other outcomes such as post-secondary enrollment, incarceration, and noncombat death do not change across cohorts.[25] These patterns are more consistent with both deployment and VADC having limited effects on noncombat mortality, education, and other outcomes than an alternative in which additional VADC received as a result of deployment offsets its adverse impacts.

Regardless of the extent to which the marginal VADC dollar for those who deploy ameliorates outcomes, it is certainly the case that deployments in our context occur in a setting where public support for veterans is high (Frankovic, 2021) and where all veterans, not just those who deployed to combat, have access to generous healthcare, education benefits, disability compensation, and other support services. Our results speak to the effects of modern combat deployments in this current environment and not necessarily to deployments in context with a weaker support network for veterans.

3.5 Heterogeneity

Table A.12 estimates effects when splitting the sample along several important dimensions of heterogeneity, including AFQT scores, moral waivers, and race. Effects are broadly similar, although there is some evidence of differences across race groups. These differences likely reflect the fact that white soldiers disproportionately work in combat occupations and thus are exposed to more violence while deployed, a point we return to in Section 4 below. Point estimates for noncombat deaths also vary across subgroups, but are too imprecise to detect any significant differences.

3.6 Robustness analyses

Several analyses support the robustness of our main results. First, we explore sensitivity to the time horizon for measuring deployment. Our primary specification measures months deployed over the three years after arrival at a soldier's first duty-station since three years is the modal term-length of first-term soldiers (63% of our sample). Additionally, soldiers with longer initial terms will often move to other units after this time frame. In Table A.13, we show that defining the endogenous variable D_i over longer time horizons (and changing the instrument accordingly) changes results very little. This finding is consistent with our initial time span adequately capturing the variation in total months ever deployed.

[25] One possible exception to this finding, the effect of deployment on credit scores slightly increases, from a statistically insignificant reduction of 2.1 points in the 2005-2007 cohort to a statistically significant increase of 5.2 points in the 2011-2013 cohort, consistent with VADC improving credit.

Second, we show that our main results are similar when adjusting the sample restrictions or specification. Table A.14 shows that our main results are very similar when we include women in our analysis. Table A.15 reports on estimates of Equation (1) when we replace year of arrival fixed effects with quarter of arrival fixed effects, again yielding estimates similar to those from our preferred specification.

Third, we explore whether our baseline specification masks important nonlinear relationships between deployment length and our primary outcomes. Table A.16 estimates multiple endogenous variable models with, for example, both months deployed and months deployed squared. The implied effects of 10 versus zero months of deployment is very similar to our primary 2SLS estimate. No coefficients are significantly different than zero for noncombat deaths. Table A.17 shows that changing the endogenous variable to an indicator for any deployment and the instrument to be the share of peers ever deployed, which might lead to more extensive margin shifts among compliers, yields similar effects. If anything, effects on noncombat deaths become more negative, changing from 0.05 to -0.13 overall and from -0.02 to -0.15 for suicides. Simply studying the reduced-form relationship between the instrument and outcomes nonparametrically also yields similar conclusions, as shown in Figure A.5. Consistent with our primary results, although combat death and VADC are increasing in the instrument, there is no evidence of any relationship with noncombat death.

Finally, recent research has used a variety of different estimators in settings where the instruments are indicators for quasi-random group assignment (e.g., judges or examiners). Table A.18 shows the results of balance tests using alternative estimators such as LIML or UJIVE (Kolesar, 2013) and provides further validation for our research design. Table A.19 shows that results for VADC and other key outcomes change little when using these alternative estimators, as well as the traditional over-identified 2SLS estimator.

4 Do more dangerous deployments have different effects?

To this point, we have confined our analysis to the effects of the average deployment. Yet this approach potentially masks important heterogeneity in the degree of danger soldiers experience while deployed. To explore this possibility, we next compare the causal effects of deployment among soldiers in the same occupation but whose BCTs experience different degrees of violence while in combat.

To measure exposure to violence for each BCT, we use the casualty rates of other soldiers assigned to the same brigade in the same quarter. We construct this variable, W_i, as the leave-out mean of fatal and non-fatal combat casualties for all soldiers other than i assigned to the same brigade in

the same quarter:

$$W_i = \frac{1}{n_{bq} - 1} \sum_{\ell \in N_{bq(-i)}} CAS_\ell \tag{5}$$

where $CAS_\ell = 1$ if soldier ℓ suffers a combat death or combat injury within 3 years of arriving at his brigade.[26] Following the construction of the instrument Z_i, $N_{bq(-i)}$ is the set of all soldiers other than i assigned to brigade b during quarter q and $n_{bq} = |N_{bq}|$ is the number of soldiers assigned to brigade b during quarter q. The average peer casualty rate in our sample is 2.5% with a standard deviation of 3.5%.

We then estimate the effect of peer casualties by adding an interaction between months deployed and the peer casualty measure to our original IV model:

$$Y_i = \delta_{k(i)} + \beta D_i + \gamma\big(D_i \times W_i\big) + \epsilon_i \tag{6}$$

$$D_i = \omega_{0,k(i)} + \pi_0 Z_i + \rho_0\big(Z_i \times W_i\big) + u_{0,i} \tag{7}$$

$$\big(D_i \times W_i\big) = \omega_{1,k(i)} + \pi_1 Z_i + \rho_1\big(Z_i \times W_i\big) + u_{1,i} \tag{8}$$

where Y_i, D_i, and $\delta_{k(i)}$ are defined as above and and $\omega_{0,k(i)}$ and $\omega_{1,k(i)}$ correspond to the $\omega_{k(i)}$ from Equation (2). The model excludes the main effect of W_i because soldiers who do not deploy cannot be affected by more violent deployments by construction. We show below that estimates change very little using alternative modeling choices, however.

Peer casualties are an ideal measure for estimating heterogeneity in the severity of combat violence for several reasons. First, this approach avoids the potential bias inherent in many alternative approaches (e.g. comparing soldiers in combat occupations to those in noncombat occupations). Second, the residual variation in interacted peer casualties ($Z_i \times W_i$) from Equation (7) is 68% larger than the residual variation in the peer deployments itself (Z_i), which improves our precision for rare outcomes like mortality and incarceration. Among soldiers who arrived at their unit in 2009 and deployed within three years, for example, 25% had no peers who were wounded or killed in action, the median peer-casualty rate was 1.8%, and the 90th percentile of peer-casualty rates was 10.9%. Third, unit-level casualty rates are predominately a function of exogenous factors such as the location of the deployment (which we cannot observe), the unit's mission, and the broader

[26] We sum non-fatal and fatal casualties because fatal casualty are rare (89% of casualties are non-fatal). Non-fatal and fatal peer casualties are strongly correlated. After partialling out duty-station by job by assignment year by term-length fixed effects, the correlation coefficient between the residualized peer non-fatal casualty rate and a residualized peer fatal casualty rate calculated in the same manner is 0.29, but the residual variation in the peer non-fatal casualty rate is 437% as large as the residual variation in the peer fatal casualty rate measure.

geopolitical environment.[27]

Table 5 reports 2SLS estimates of Equation (6), with column (1) reporting the coefficient estimate for the main deployment effect (β), column (2) reporting the coefficient estimate for the interaction term (γ), and column (3) reporting the outcome mean. To reduce the size of the table, we restrict to outcomes measured eight years after a soldier arrives at his initial brigade. We continue to scale estimates of β by 10 and we scale estimates of γ by 10σ, where σ is the sample standard deviation of peer casualty rates. Thus, estimates of β can be interpreted as the average effect of a 10 month deployment with zero peer casualties and estimates of γ as the impact of a standard deviation increase in peer casualties during the same deployment.

Panel (a) of Table 5 reveals that more violent deployments cause more trauma. The estimates reported in column (1) suggest that effects on trauma are relatively modest among soldiers who experience deployments with no peer casualties. A deployment with no peer casualties has no effect on combat deaths, a 1.16 pp increase in combat injuries (only 26% as large as the effect of an average deployment; compare to Table 3),[28] and a 0.84 pp statistically insignificant increase in sustaining a medical profile severe enough to preclude future military service. In sharp contrast, column (2) reveals that each standard deviation increase in peer casualty rates over a 10 month deployment further increases combat deaths by 0.27 pp (54% relative to the mean), combat injuries by 1.86 pp (45%), and severe medical profiles by 1.02 pp (7%), all statistically significant with t-stats > 6.

The increased risk associated with more dangerous deployments also manifests through statistically significant increases in disability. A standard deviation increase in peer casualty rates during a 10 month deployment increases annual VADC payments by \$414 and receipt of any payments by 1.29 pp. Although these estimates leave little doubt that VADC increases with exposure to violence, deployments with no peer casualties also have substantial effects, increasing annual payments by \$1876 and any receipt by 7.15 pp. Table A.21 reports effects on VADC by diagnoses and further shows that exposure to violence is strongly linked to serious combat injuries and VADC receipt with a documented amputation. These outcomes are extremely rare among deployments with 0 peer casualties. In contrast, deployments with 0 peer casualties greatly increase receipt of VADC for PTSD as well as other common conditions. This is consistent with the possibility that deployment can be physically and mentally strenuous even when it does not substantially increase

[27] To lend support to this assertion, Table A.20 reports results from a reduced form regression analogous to Equation (7), but where the left-hand side variable has been replaced with exogenous soldier characteristics. Neither the deployment instrument nor the interaction of the deployment instrument with the peer casualty measure are strongly correlated with soldier characteristics. For each term, a joint test of significance is consistent with balance.

[28] These are instances where the soldier is the only member of his peer group (first term soldiers who arrive at a BCT within the same quarter) who suffers a combat casualty.

exposure to physical violence, but is also consistent with the possibility that deployment could facilitate the process of applying for and being granted disability compensation.

Outside of trauma outcomes and disability receipt, we find little evidence that exposure to peer casualties causes other adverse outcomes. Panel (b) of Table 5 reveals no relationship between peer casualty rates and noncombat deaths, deaths of despair, or key sub-categories of deaths of despair (i.e., suicide and drug or alcohol-related deaths). These estimates are not statistically different from zero and are precise enough to rule out meaningful effects. For example, we can rule out that a standard deviation increase in peer casualty rates increases noncombat deaths by more than 0.09 pp, which is 7% of the sample mean. Relatedly, we can rule out that the same increase in peer casualty rates causes a 3% increase in deaths of despair, an 8% increase in suicide, and a 1% increase in drug or alcohol-related deaths.

Panel (c) of Table 5 shows that exposure to more violence during deployments has no significant effect on misconduct separations, incarceration, credit scores, post-secondary enrollment, or graduation. These estimates are precise enough to rule out that a standard deviation increase in peer casualties during a deployment increases separations for misconduct by 0.29 pp (1% of the mean), increases incarceration by 0.23 pp (9% of the mean), decreases Vantage credit scores by 0.8 points (0.1% of the mean), and decreases college enrollment by 0.4 pp (0.8% of the mean).[29]

Overall, these results reveal that soldiers exposed to more dangerous deployments are substantially more likely to die in combat and suffer physical injuries. Exposure to violence also increases disability receipt, although deployments also substantially increase compensation among soldiers who experience relatively safe deployments. Despite increases in physical trauma and disability receipt, we find little evidence that more dangerous deployments increase noncombat related deaths or our other adverse outcomes. Equipped with estimates of how deployment and exposure to peer casualties impact disability and mortality outcomes, we explore in Section 5 whether changes in the frequency and combat intensity of deployment over time can explain veteran trends in disability and mortality.

4.1 Additional robustness analyses

It is possible that our peer casualties proxy does not accurately capture which deployment experiences affect outcomes the most. As a simple alternative, we directly examine how BCT assignment influences outcomes, allowing us to capture the impact of whatever experiences soldiers in each BCT have while deployed. We do so by constructing BCT-specific causal effects of deployment

[29] Table A.22 shows that simple comparisons of soldiers in combat occupations (e.g. infantry) to soldiers in noncombat occupations (e.g. supply specialists or human resource specialists) paint a similar picture to our findings in Table 5.

on key outcomes. These estimates are formed by dividing the coefficient on the BCT-by-quarter dummy in the reduced form by the coefficient on the same dummy in the first stage, essentially constructing separate 2SLS estimates for each BCT-by-quarter instrument. Figure A.6 clearly shows that assignment to BCTs where a 10 month deployment lead to more violence also lead to more VADC receipt. However, the BCT-specific effects of deployment on violence are unrelated to BCT-specific effects on noncombat death.

Consistent with these findings, Table A.23 reports unbiased estimates of the variance of BCT assignment's reduced-form effects on these outcomes. A large variance indicates that soldiers assigned to different BCTs experience very different average outcomes, whereas a small variance indicates they do not. The results show large differences in deployment and VADC receipt across BCTs, but the estimate for noncombat deaths is negative with a confidence interval that includes zero. BCT assignment thus does not appear to affect noncombat deaths in important ways regardless of the type of experiences those soldiers have while in the service.

It is also possible that the linear interaction in Equation (6) provides a poor approximation to how combat violence and deployment jointly affect outcomes. Reassuringly, alternative specifications produce similar results. For example, we find similar marginal effects of peer casualties at ten months of deployment whether or not we include the main effect of W_i, as shown in Table A.24. Estimating a more flexible model that interacts D_i with bins of W_i also yields similar conclusions, as shown in Table A.25. Table A.26 reports effects in the subsample of soldiers with $W_i = 0$ to explore whether our specification adequately extrapolates to soldiers with zero exposure to peer casualties. Although these results are noisier, they are similar to the effects reported in column (1) of Table 5, and a joint hypothesis test for whether the estimates are equal does not reject (p-value $= 0.312$).

Finally, Figure A.7 helps further assuage functional form concerns by non-parametrically plotting outcomes against the rates of peer deployment and casualties. We divide Z_i and W_i into five equal-sized groups and estimate the reduced-form effects of each of the 25 combinations on key outcomes. Panels (a) and (b) show that increases in either variable lead to a greater likelihood of injury and VADC receipt. However, panels (c) and (d) clearly show no relationship between either Z_i or W_i and noncombat deaths. Moreover, none of the cells in panels (c) and (d) is statistically different from the omitted category.

5 The drivers of declining veteran well-being

We conclude by examining whether our estimated causal effects of deployment can help explain recent trends in veterans' outcomes. What role did combat deployments to Iraq and Afghanistan

play in the rapid rise in VADC and simultaneous shifts in veteran mortality? How does the effect of deployment compare to the potential impact of changes in the characteristics of servicemembers? And what do the answers to these questions imply for the current design of VADC as a program meant to insure veterans against the unique risks of military service?

We begin by augmenting our interacted deployment and casualty specification with a rich set of observable characteristics measured prior to the time of assignment, denoted X_i, that includes age, race, sex, AFQT and ASVAB subtest scores, moral character waivers, marital status, number of dependents, educational attainment, and results from medical and drug tests administered at Military Entrance Processing Stations (MEPS):

$$Y_i = \delta_{k(i)} + \beta D_i + \gamma\big(D_i \times W_i\big) + X_i'\Gamma + \epsilon_i \tag{9}$$

$$D_i = \omega_{0,k(i)} + \pi_0 Z_i + \rho_0\big(Z_i \times W_i\big) + X_i'\Gamma_0 + u_{0,i} \tag{10}$$

$$\big(D_i \times W_i\big) = \omega_{1,k(i)} + \pi_1 Z_i + \rho_1\big(Z_i \times W_i\big) + X_i'\Gamma_1 + u_{1,i} \tag{11}$$

We estimate this model using the full sample in Table 1 for all entry cohorts from 2001 to 2011, measuring outcomes eight years after arrival. Since our primary analysis uses a subset of individuals from post-2005 cohorts, for whom the instrument is well defined, we augment our baseline set of fixed effects $\delta_{k(i)}$ to include an additional interaction with an indicator variable for our analysis sample and set our instrument to zero outside of this sample. Including pre-2005 cohorts allows us to analyze a longer time period, including key years in the Global War on Terror.[30]

We use these estimates to decompose changes in outcomes over time into components explained by deployment, changes in soldiers' observable characteristics, and all other factors. We do so by collapsing the data to cohort-level means of Y_i, D_i, W_i, and X_i. Letting c_i be the annual enlistment cohort for soldier i, we decompose changes in outcomes between cohorts into the following three components:

$$\begin{aligned} &E[Y_i|c_i = c'] - E[Y_i|c_i = c] \\ &= \underbrace{\beta\left(E[D_i|c_i = c'] - E[D_i|c_i = c]\right) + \gamma\left(E[D_iW_i|c_i = c'] - E[D_iW_i|c_i = c]\right)}_{\text{Impact of changes in deployment and exposure to violence}} \\ &\quad + \underbrace{\left(E[X_i|c_i = c'] - E[X_i|c_i = c]\right)'\Gamma}_{\text{Effect of changes in soldiers' observable characteristics}} + \underbrace{E[\epsilon_i|c_i = c'] - E[\epsilon_i|c_i = c]}_{\text{Unexplained differences (e.g., policy changes)}} \end{aligned} \tag{12}$$

We measure these changes over key peak-to-trough intervals for each outcome. The component attributable to deployment and violence captures how time trends in exposure to combat, re-scaled

[30]Restricting to our primary analysis sample yields similar conclusions, as we show below.

by their causal effects estimated using our 2SLS strategy, relate to changes in outcomes.[31] The component attributable to X_i captures how selection into service reflected in observable factors explains changes in outcomes. Because Γ is estimated in a model that includes duty-station by job by enlistment period by term-length fixed effects, the effects of the these covariates are estimated by comparing soldiers serving in the same place, in the same jobs, and at the same time. Our decompositions then measure how much of between cohort changes in outcomes is reflected in between cohort changes in observables when scaled by their estimated effects.

Any residual, unexplained changes in outcomes may come from several sources. First, policy changes may directly affect outcomes, as we discuss in Section 5.2.1. Second, unobserved characteristics related to outcomes may shift across cohorts. Since no two cohorts enlist at the same time, it is difficult to separate unobserved selection from the impact of policy. We focus instead on what can be explained by deployment and observable characteristics, attributing the residual to all other factors.

As a validation test, we begin with trends in combat injuries, which we expect to be mechanically explained by changes in deployment and violence. The results are presented in Panel (a) of Figure 6.[32] The solid black line with square markers shows the change in actual outcomes for each cohort relative to the 2001 cohort. The outcome is measured as of eight years after enlistment. The dashed line with hollow triangle markers shows the changes in combat injuries predicted by our causal effects of deployment and changes in average peer exposure. The dashed line with hollow square markers shows the changes attributable to changes in soldiers' observable characteristics. The final line with solid triangle markets shows the changes attributable to all of these factors.

The results show that combat injuries increased by nearly 2.7 pp from the 2001 to the 2005 cohort, then fell to sub-2001 levels for the 2011 cohort. The series including changes predicted by deployment closely track the evolution of actual combat injuries, as one would expect given that combat deployments are the only way to become wounded in combat. The causal effects of deployment explain about 97% of the 2001-2005 increase and about 76% of the 2006-2011 decline. Predicted effects diverge slightly from observed effects beginning in 2007, suggesting some mis-specification emerging from the the inability of the peer casualty measure to fully capture the changing nature of combat over time. Soldiers' observed characteristics, by contrast, explain none of the changes in combat injuries, suggesting that who is wounded in war is largely random conditional on our baseline set of fixed effects.[33]

[31] Figure A.8 reports cohort trends in months deployed and combat death, showing an inverse-U shape pattern, with deployment and risk levels increasing, peaking, and then gradually reducing such that 2013 levels are lower than in 2001.

[32] Tables A.27 and A.28 contain the corresponding point estimates of the components in Equation 12.

[33] Figure A.9 shows that results change little if we use the baseline model without the peer causality interaction,

5.1 Explaining trends in noncombat deaths

Next, we turn to noncombat deaths, which saw a sharp ramp-up over the 2000s that began to decline only with the 2009 cohort. Panel (b) of Figure 6 shows that changes in deployment and exposure to violence across cohorts effectively explain none of the changes. This finding reflects the relatively small estimated causal effect of deployments on noncombat deaths. Results change little when looking at specific forms of noncombat death such as deaths of despair or suicides, as shown in Figure A.10. Deployment also remains unable to explain the patterns in noncombat death trends after we account for statistical uncertainty in our estimates, as shown in Figure A.11.

Observable selection, however, is a far better predictor of changes in noncombat deaths. The dashed line with hollow square markers tracks increases and declines in the outcome accounted for by changes in soldiers' characteristics such as moral conduct waivers and AFQT scores. As more soldiers with lower AFQT scores and more moral waivers enlisted in the mid-2000s, noncombat deaths increased. Selection into service improved following the 2008 entry cohort, and noncombat deaths then decreased. Table A.27 shows that about 32% of the between cohort variation in noncombat deaths can be explained by observable characteristics alone. These patterns are even starker if we focus only on our analysis sample, as shown in Figure A.12. Because these characteristics include only the observables available in our data, it seems likely that changes in unobserved characteristics would explain even more of the observed changes over time.

Taken together, these results suggest that several of the worrying trends in veteran well-being (e.g., deaths of despair and suicides) appear to be the result of shifts in who is serving rather than the direct effects of the war itself.

5.2 Explaining trends in disability payments

Panel (c) of Figure 6 repeats the same exercise for annual VADC payments eight years post-arrival (in 2020 dollars). While deployment explains an important share of the increase in disability compensation into the mid-2000s, trends diverge after the 2005 cohort. Average VADC payments continued to grow at roughly the same rate, despite a decline in the causal contribution of deployment itself. As a result, the 2011 entry cohorts received the most VADC despite having the lowest deployment rate among the cohorts we analyze. The dashed line with hollow square markers shows that this pattern is also not well explained by changes in soldiers' observable characteristics.[34] By

although deployment effects do a slightly worse job of tracking changes in combat injuries, which is as expected. And panel (a) of Figure A.10 shows a similar pattern when using effects on combat deaths instead of injuries to validate the model.

[34] Figure A.10 shows that VADC for PTSD shows a similar pattern. Figure A.13 shows that results change little when performing the decomposition separately for each year after arrival.

contrast, Panel (d) shows that trends in the other major disability insurance programs veterans accessed over this period, SSI and SSDI, are much better explained by deployments throughout the entire sample period.

To the extent that disability insurance programs serve as insurance for veterans' exposure to combat and compensate them for any resultant harms, some connection between deployment, VADC, and SSI/SSDI is natural. The decoupling between recent trends in VADC and deployment, however, suggests that VADC's targeting has changed over time, at least relative to other major programs. To better understand this point, we estimate the effects of deployment on VADC, SSI/SSDI, and combat deaths for sets of three-year rolling cohorts from 2005 to 2013.

The results are presented in Figure 7. Every point is an estimate from a different sub-set of soldiers in our primary specification, restricting to those who enlisted during the three years listed on the x-axis.[35] Each point denoted by a triangle shows average outcomes among *untreated* compliers—that is, soldiers who did not deploy during their first three-year term but would have been deployed if assigned to a different BCT. Each point denoted by a circle shows the expected outcome for a soldier with a 10 month deployment. The gap between the two series corresponds to the treatment effect of a 10 month deployment.

The results show that deployments have become less dangerous over time and simultaneously generated larger increases in VADC receipt. A 10-month deployment for soldiers who enlisted between 2005 and 2007 increased the risk of combat death by 0.72 pp. By the 2009-2011 cohort, the effect of deployment on combat death fell by more than four times to 0.17 pp. If VADC is compensating soldiers for the risks of deployment, these results suggest that the need for such compensation, if anything, has decreased over time.[36] However, the causal effect of a 10-month deployment on VADC compensation more than doubled during this period, from $1383 to $2974. Simultaneously, VADC payments have been increasing over time for soldiers who do *not* deploy, with untreated complier means increasing by 21% from 2005-2007 to 2011-2013. Since these soldiers do not deploy by construction, VADC payments are unlikely to be insuring them against harms from combat deployments.

SSI/SSDI, by contrast, shows a very different pattern. The effect of a 10 month deployment on total payments from these programs *decreased* from $490 to $145 over the same period, consistent with

[35] We focus on outcomes 6 years after arrival for cohorts from 2005-2013 in order to be able to show a longer time series, but Figure A.14 shows that we reach the same conclusions using outcomes 8 years after arrival for the 2005-2011 cohorts.

[36] Of course, we cannot rule out the possibility that VADC was under-insuring such risks initially or that deployments became riskier in a way unrelated to combat risk (e.g. effects on mental health were more severe in later deployments). Figure A.15, however, shows that the effects of deployment on incarceration, education, and credit outcomes have been stable over time.

the declining combat risk. Untreated means of SSI/SSDI receipt have also been declining since the 2006-2008 cohorts, consistent with decreasing severity of combat and increasingly positive selection into service. Thus, VADC has expanded in the opposite direction of what would have been expected based on declining combat risk. SSI/SSDI has not. To the extent that the need for insurance against other noncombat risks is stable or also declining over time, our estimates are consistent with VADC increasingly acting more as a transfer program disconnected from risk rather than as a vehicle for targeted insurance.

5.2.1 Policy changes and VADC

What explains the recent growth in the VADC program if not changes in deployment, combat risk, or the composition of servicemembers? A broad suite of regulatory and policy changes aimed at expanding access to VADC, along with possibly changing applicant norms, likely help account for the program's growth. Tables A.29–A.30 outline regulatory and policy changes we identified between 2000 and 2015. Consistent with a political environment supportive of veterans, these changes almost always eased access, expanded compensation, or lowered evidentiary standards.

Several key policy changes appear particularly relevant.[37] In response to the Veterans' Benefit Improvement Act of 2008, the VA made several large pushes to simplify the process of applying for benefits, providing consistent information and application support to all soldiers and greatly expanding their ability to apply for VADC before leaving the service.[38] Concurrently, there has been a rise in no-cost support for veterans filing claims. Regulatory changes in the 2000s also assigned the VA a duty to assist claimants and help substantiate their claims, and shifted the burden

[37] We are not the first to suggest that these policies have bite. A 2014 CBO report captured the importance of the changing policy and regulatory environment, stating "part of the explanation for increases in the number of recipients and the amount of average payment per recipient can be found in the Veterans' Claims Assistance Act of 2000 and the Veterans' Benefits Improvement Act of 2008, which required VA to help veterans apply for disability benefits and help with substantiating claims. VA also has increased its outreach concerning post-traumatic stress disorder and eased diagnostic requirements for that condition. ... some policy changes have been directed at veterans who served in Iraq and Afghanistan ... for example, VA greatly expanded its outreach efforts to current service members and established predischarge programs to accept applications before separation" (Congressional Budget Office, 2014). Prior work has also shown that changes in eligibility criteria had large effects on Vietnam-era veterans' VADC receipt rates (Angrist et al., 2010; Duggan et al., 2010; Autor et al., 2016; Coile et al., 2019).

[38] Broten (2021) describes how the Benefits Delivery at Discharge (BDD) program and the Quick Start program (which started in July 2008) allowing servicemembers to apply for benefits while in service were expanded following the Veterans' Benefits Improvement Act of 2008. By FY 2009 (United States Congress, May 6, 2010) around 65% of separating servicemembers who filed claims within one year of discharge did so through these programs. Proposed in 2007 and fully implemented by 2011, the Integrated Disability Evaluation System (IDES) streamlined VA and DOD systems for evaluating disabilities of active duty soldiers with health conditions that limited their ability to carry out military duties (Broten, 2021). Additionally, the 2011 VOW to Hire Heroes Act mandated participation of servicemembers in the Transition Assistance Program in which disability compensation information and filing support is offered (Public Law 112-56, 2013). These changes were accompanied by a move to an online application system and generally greater encouragement and outreach.

of proving that disabilities did not exist prior to military service away from the claimant onto the VA (Public Law 106-475, 70 FR 23027).

Importantly, various new regulations also eased evidentiary standards and expanded the list of presumptive conditions linked to various exposures. While some of these, like a 2011 change relaxing verification of Military Sexual Trauma (MST) stressors apply equally to deployers and non-deployers alike, others were particularly applicable to those who deployed. For example, a July 2010 policy eased evidentiary standards for claiming PTSD, eliminating the requirement for corroborating that the claimed in-service stressor occurred if the stressor is linked to being in a combat zone or is consistent with the 'places, types, and circumstances of the Veterans' service' (Department of Veterans Affairs, 2010). Contreary et al. (2017) find "that the reduction in the burden of proof on veterans who served in combat zones seeking DC for PTSD increased DC receipt among these veterans."

Overall, better information and support for applicants, changing application norms, and explicit law, regulation, and policy changes implemented at the height of the war, are plausibly important drivers of the recent rise in VADC.

6 Conclusion

Nearly 20 years of war in Iraq and Afghanistan has had a profound impact on the soldiers who fought there. Our results show that combat deployments presented both immediate risks, in the form of death and injury, and long-term costs in the form of large increases in disability payments. Nevertheless, we find limited evidence that combat deployments affect other important dimensions of veteran well-being including deaths outside of combat, misconduct, credit, and education. Moreover, deployments do not appear to be the main driver of the concerning trends in veterans' outcomes, which instead appear tied to relaxed recruiting requirements over the course of the wars and changes in policy.

These effects of deployment are estimated in a context that is broadly supportive of veterans and features extensive support networks, including VA health care and disability compensation. Whether the effects of deployment would be more deleterious in a less supportive setting is an important open question. Additionally, although we estimate effects up to 8 years out on a range of key outcomes, we have limited access to measures of veteran health. We cannot rule out adverse impacts on health and economic outcomes we do not measure, or the possibility of longer-term consequences of deployment. Future research with additional data could use our research design to quantify the impact of deployment on long-term health and other outcomes.

Taken together, our results have several implications for policy. First, who the Army permits to enlist has important consequences on downstream veterans' outcomes, oftentimes more so than the effects of combat deployments. As the Army undergoes one of its worst recruiting crises in years, it may be forced to once again recruit from more at-risk populations and should anticipate worsening average veteran outcomes, despite the end of operations in Iraq and Afghanistan. Second, our results suggest that the recent growth in VADC at least partly reflects the (possibly unintended) lingering consequences of policy decisions. Absent meaningful policy intervention, VADC receipt will likely remain high. Future research should continue to investigate the causes and consequences of ballooning VADC payments and assess whether the program could benefit from fundamental reforms. The fact that policy responses drive some of the costs of war also suggests that further study of the political economy of waging war is warranted.

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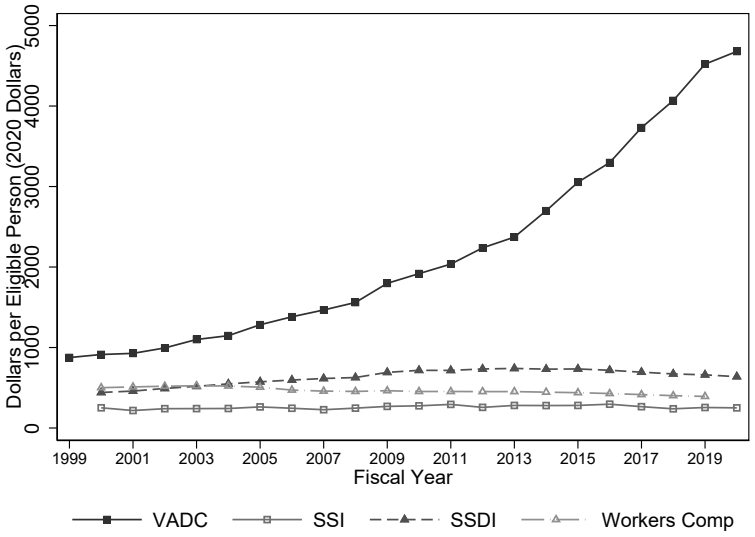
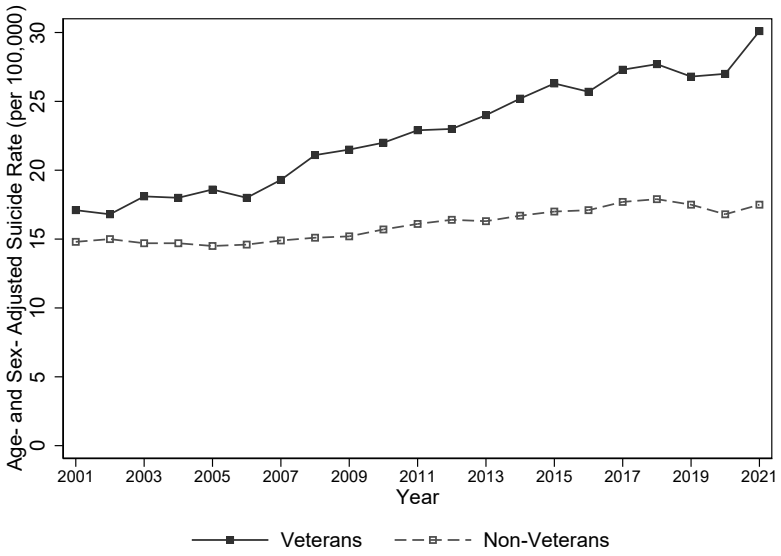
Figures

Figure 1: Trends in veterans' outcomes

(a) Disability Payments Per Eligible Person

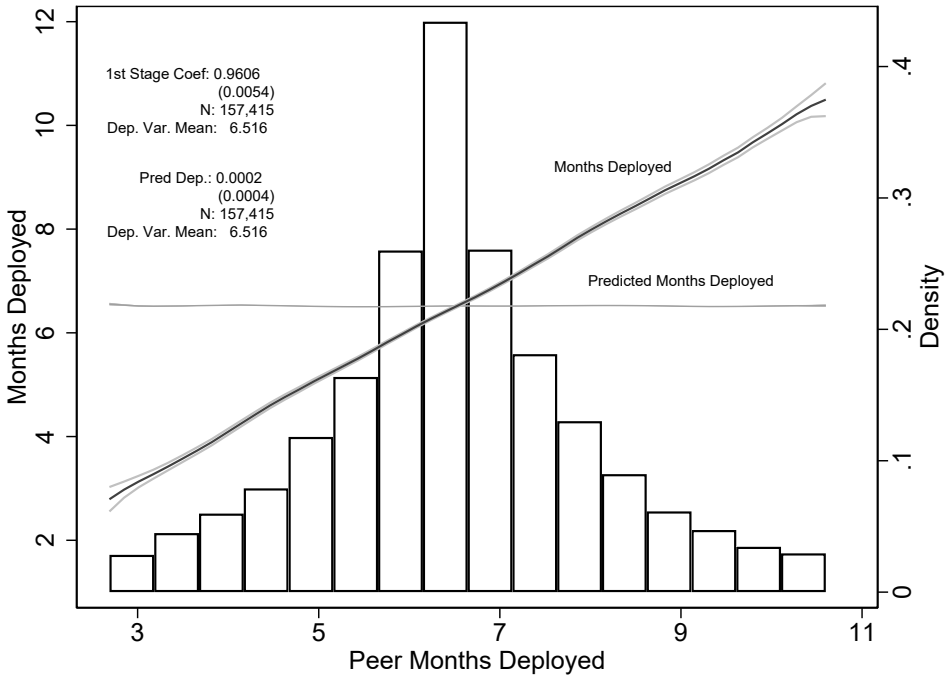


(b) Age- and Sex- Adjusted Suicide Rates



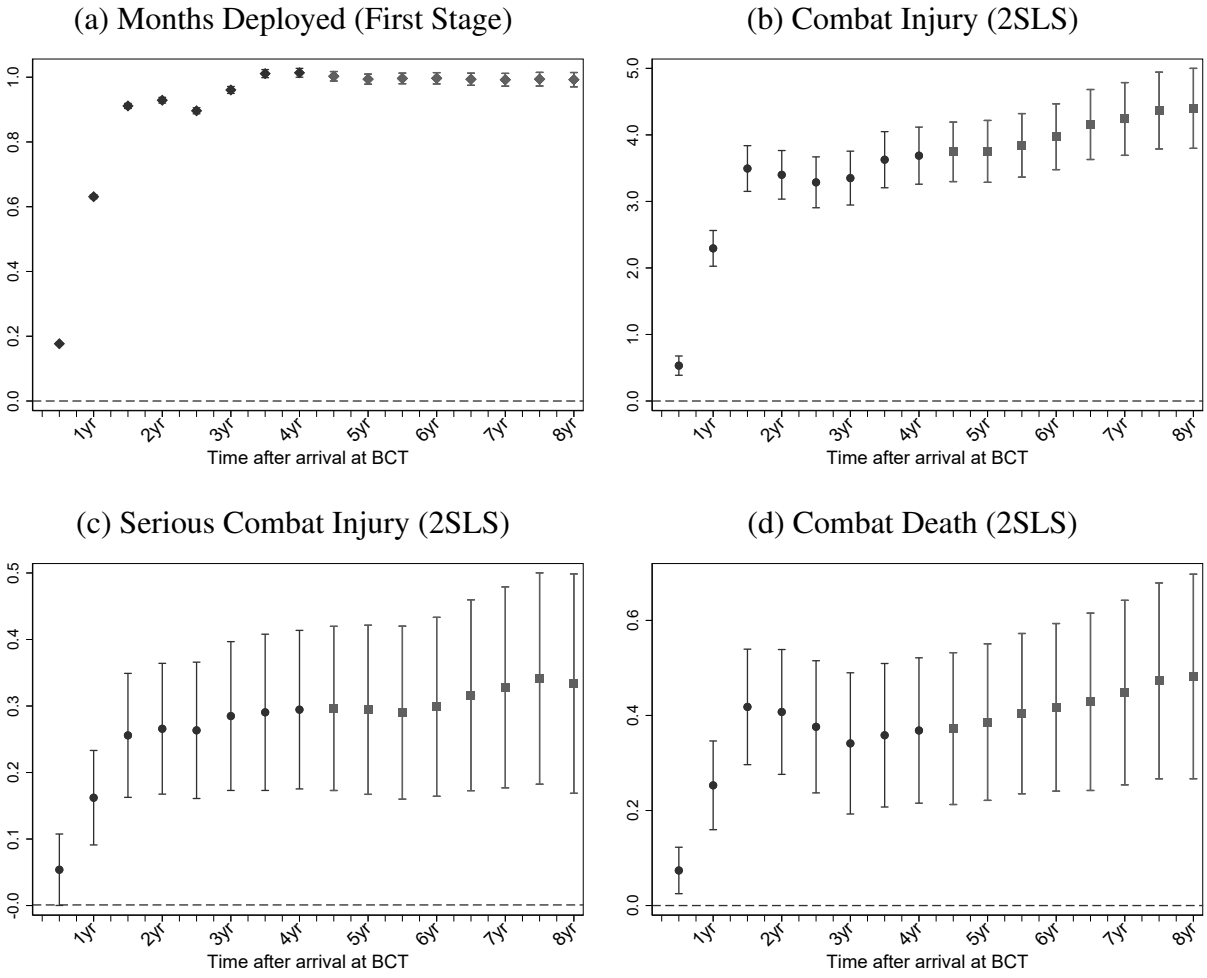
Notes: Panel (a) of this figure plots federal spending per person for several government programs (in 2020 USD). The solid line shows total Veterans Affairs Disability Compensation (VADC) payments per living veteran (US Department of Veterans Affairs, 2022). The remaining lines shows total federal Supplemental Security Income payments per fully insured worker (Social Security Administration, 2020, 2022b), total Social Security Disability Insurance payment per fully insured worker (Social Security Administration, 2022a,b), and total Workers Compensation payments per member of the civilian labor force (Murphy et al., 2021; Bureau of Labor Statistics, 2022), as indicated by the legend. Panel (b) plots age- and sex-adjusted suicide rates for adult veterans and non-veterans in the U.S. as reported in Department of Veterans Affairs (2023). The precise values for the series in panel (b) are from the Data Appendix of Department of Veterans Affairs (2023), available at https://www.mentalhealth.va.gov/suicide_prevention/data.asp (accessed 17 December 2023).

Figure 2: First stage effects on deployment



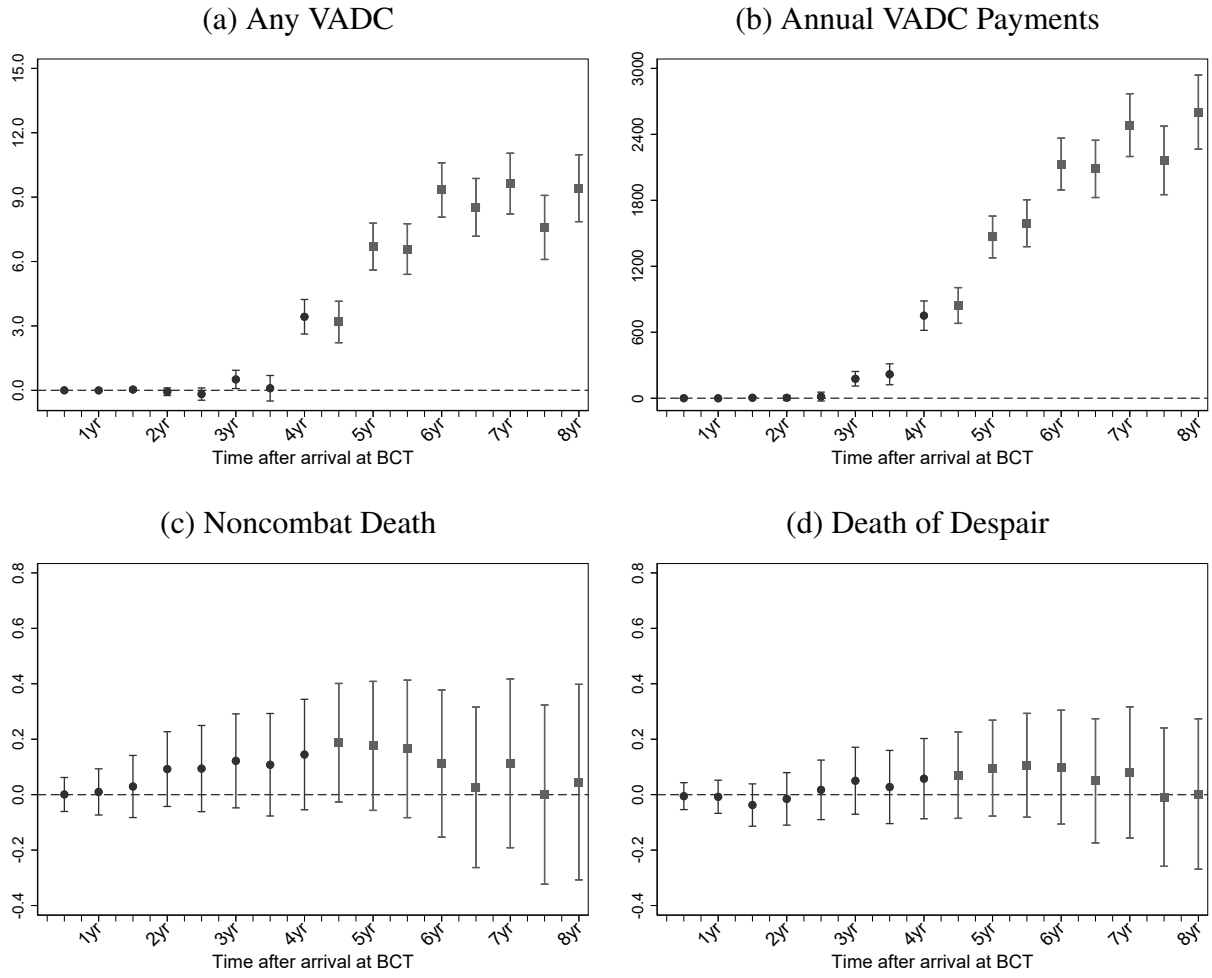
Notes: This figure shows the variation in our instrument, our first stage, and covariate balance. We residualize our outcome (months deployed within three years of arrival at the Brigade Combat Team (BCT)) and our instrument (peer months deployed based on BCT and quarter of arrival) on duty-station by job by initial assignment period by term length fixed effects. The histogram of our residualized (and re-centered at the sample mean) instrument is shown in the background. We drop the bottom and top 2.5 percentiles of the instrument for the figure, but not for the reported regression coefficients. The upward sloping curve shows a local linear regression of residualized months deployed on our residualized instrument and associated 95% confidence intervals. The first stage coefficient and standard error are reported in the top left hand corner. The horizontal line shows a local linear regression of predicted months deployed on our residualized instrument. Predicted months deployed is constructed using our baseline fixed effects and the following soldier-level covariates (all measured when soldiers enlist): age, white, Black, Hispanic, female, linear, quadratic, and cubic terms for AFQT (and an indicator for rare situations where soldiers are missing AFQT scores), any moral waiver, married, number of dependents, high school grad, an indicator for requiring a medical waiver, indicators for failing alcohol, marijuana, or cocaine drug tests, and ASVAB composite line scores (clerical, combat, electronics, field artillery, general maintenance, general technical, mechanical maintenance, operators and food, and surveillance and communications). The top left hand corner reports the coefficient on the corresponding regression.

Figure 3: Dynamic effects on deployment, injury, and death



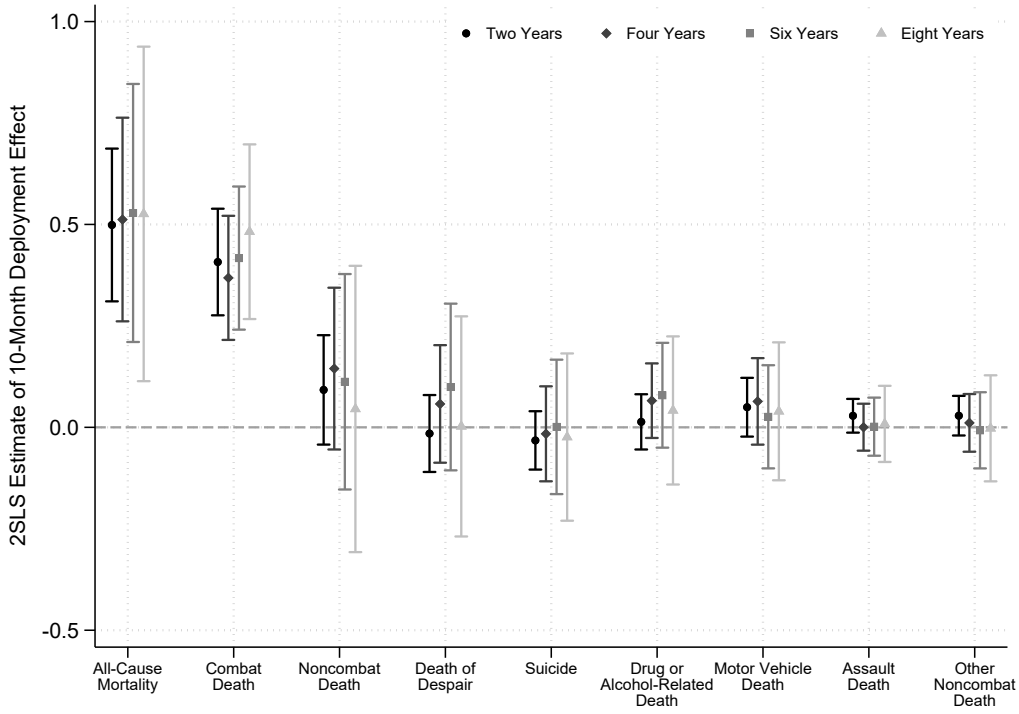
Notes: Panel (a) of this figure plots the reduced-form relationship between total months deployed within the time since arrival indicated on the x-axis and the instrument using the specification in Equation 2. After 3 years, deployment increases roughly one-to-one with peer months deployed. In panels (b), (c), and (d), we report the 2SLS estimates from Equation 1 of the effect of months deployed on combat injuries (i.e. "wounded in action", defined as an injury resulting from an attack against U.S. forces), serious or very serious combat injuries (injuries from adversarial action that are life-threatening or life-altering, or where death is possible within 72 hours), and combat deaths, respectively. These outcomes are also measured within the time since arrival indicated on the x-axis. We scale coefficients and standard errors in panels (b)-(d) by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. The shape and shade of the points changes 4 years post-arrival, when our sample goes from being balanced to unbalanced. Since most of our outcomes are only available through December 2019, we do not observe outcomes more than 4 years after arrival for soldiers who arrived to their first operational assignment in December 2015. Error bars represent 95% confidence intervals.

Figure 4: Dynamic effects on disability and mortality



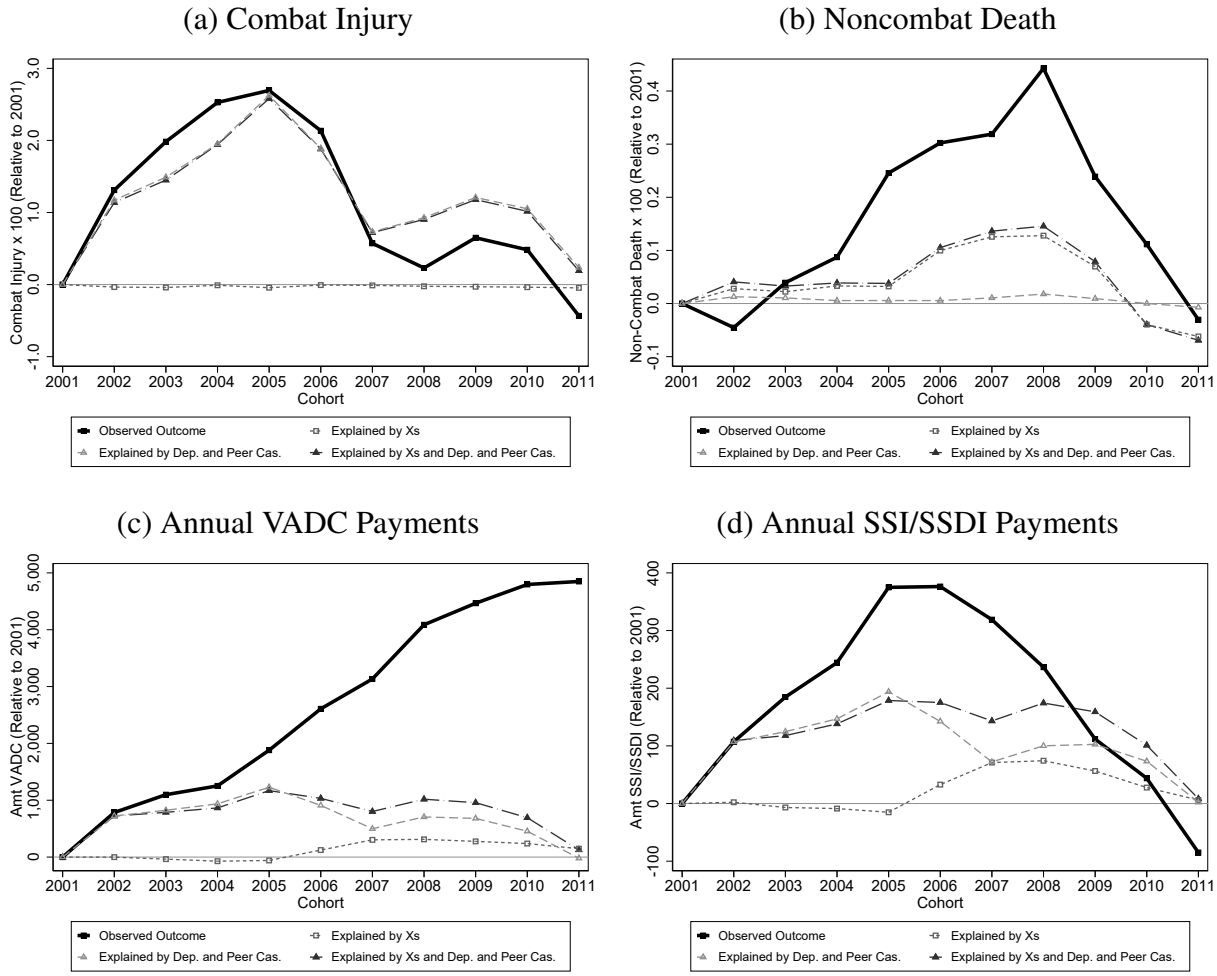
Notes: This figure plots 2SLS estimates from Equation 1 of the effects of deployment on various outcomes measured within the time since arrival indicated on the x-axis. In panel (a), we report effects on receipt of any VADC in the most recent calendar year. In panel (b), we report effects on annual VADC payments (in 2020 dollars). In panel (c), we report the effect of months deployed on any noncombat death, defined as any fatality as reported in the NDI data excluding combat deaths. In panel (d), we report the effect on any death of despair, which includes all suicides (NDI recorded motivation as intentional self-harm or undetermined intent) plus any death caused by drugs, alcohol, or poison, or firearm deaths resulting from undetermined intent. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. The shape and shade of the points changes 4 years post-arrival, when our sample goes from being balanced to unbalanced. Error bars represent 95% confidence intervals.

Figure 5: Effects of deployment on mortality



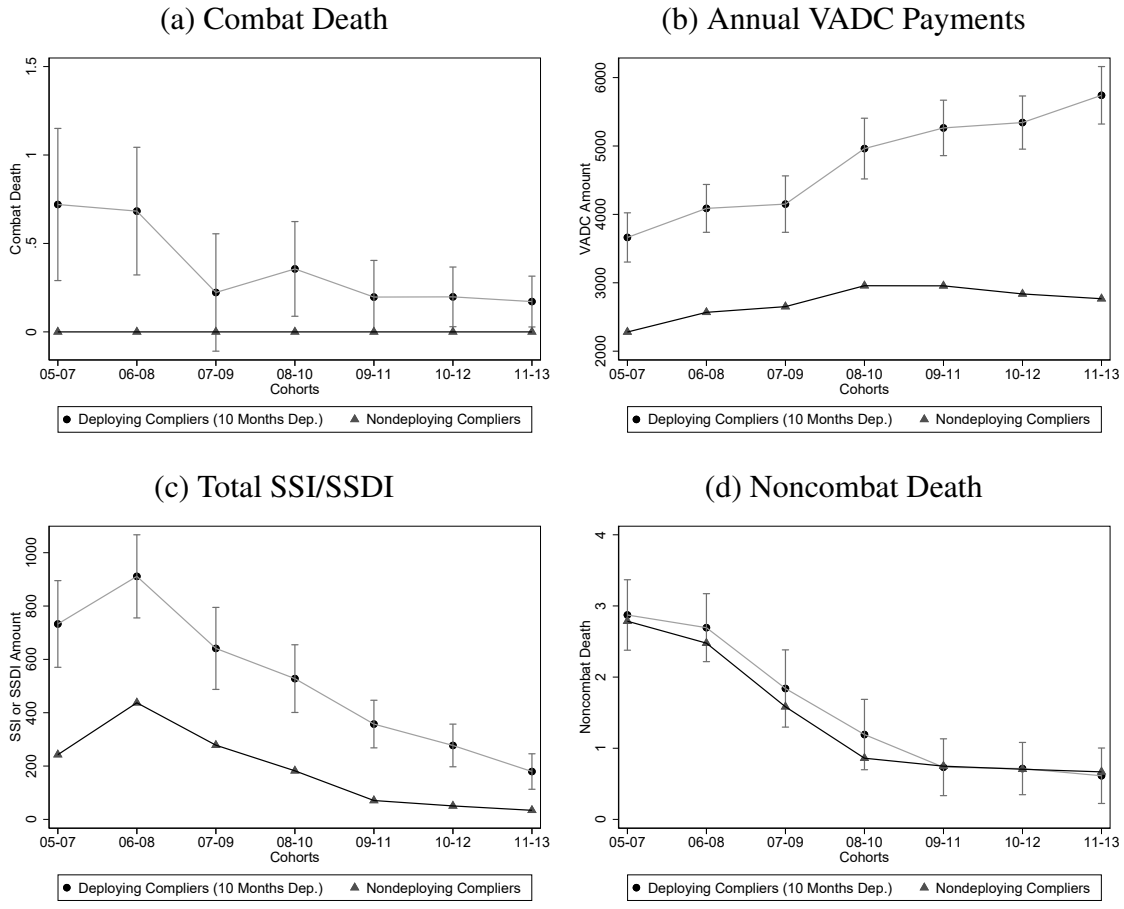
Notes: This figure reports 2SLS estimates from Equation 1 of the effects of deployment on all cause mortality and various sub-classifications of mortality. We scale coefficients by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. The first set of points reports the effects of deployment on all cause mortality two, four, six, and eight years after arrival. The next set of points reports the effects on deaths due to combat. The third set reports the effects on noncombat deaths. The fourth set reports the effects on deaths of despair, which include all suicides (NDI recorded motivation as intentional self-harm or undetermined intent) plus any death caused by drugs, alcohol, or poison, or firearm deaths resulting from undetermined intent. The fifth set of points reports effects on suicides and the sixth column reports effects on any death determined in the NDI to be caused by drugs or alcohol. The seventh set reports effects on deaths resulting from motor vehicle accidents, the eighth deaths resulting from assaults, and the last noncombat deaths resulting from any other cause not already considered. Error bars represent 95% confidence intervals.

Figure 6: Decomposition of veteran outcome trends



Notes: This figure shows a decomposition of changes in the average outcomes of cohorts of soldiers who enlisted between 2001 and 2011. Actual cohort level outcomes, normalized to 2001 levels, are plotted in the solid black lines with square markers. As described in Section 5, we use estimates of Equation 9 to generate predicted outcomes based on covariates (Xs), a causal effect of months deployed (Dep.), and a causal effect of the interaction between months deployed and peer casualties (Peer Cas.). The dashed lines with hollow square markers show how changes in covariates alone predict changes in outcomes across cohorts. The dashed lines with hollow triangle markers show how changes in deployment and peer casualties predict changes in outcomes. The dashed lines with solid triangle markers show changes predicted by all factors. Panel (a) decomposes trends in combat injuries within 8 years of arrival. Panel (b) does the same for any noncombat death within 8 years of arrival. Panel (c) does the same for annual VADC payments 8 years after arrival. Panel (d) does so for annual SSI/SSDI payments 8 years after arrival. Covariates are the same as those used to construct predicted months deployed in Figure 2: age at enlistment, white, Black, Hispanic, female, linear, quadratic, and cubic terms for AFQT (and an indicator for rare situations where soldiers are missing AFQT scores), any moral waiver, married, number of dependents, high school grad, an indicator for requiring a medical waiver at enlistment, indicators for failing alcohol, marijuana, or cocaine drug tests, and ASVAB composite line scores (clerical, combat, electronics, field artillery, general maintenance, general technical, mechanical maintenance, operators and food, and surveillance and communications).

Figure 7: Treatment effects across 3-year rolling cohorts



Notes: This figure plots average outcomes among untreated compliers—i.e., soldiers who deploy for zero months—for a range of outcomes. These estimates are plotted using the circular markers. For comparison, the triangles plot estimates of average outcomes among soldiers who deploy for 10 months, computed as the untreated mean plus 10 times the estimated treatment effect of months deployed for the relevant outcome. Treatment effects are thus the gap between the two lines. Each estimate restricts the sample to the cohorts listed on the x-axis. Panel (a) plots combat death, with untreated means set to 0 since combat deaths only occur on deployment. Panel (b) plots annual VADC payments, panel (c) plots SSI/SSDI payments, and panel (d) plots noncombat death. All outcomes are measured as of 6 years after arrival at first duty station. 95% confidence intervals on the estimated treatment effect are depicted by the vertical bars.

Tables

Table 1: Summary Statistics

	Full Sample 2001-2015	Male Soldiers 2005-2015	Estimation Sample 2005-2015	Estimation Sample Never Deployed	Estimation Sample Ever Deployed
	(1)	(2)	(3)	(4)	(5)
			Panel (a): Demographics		
Age	21.86	21.98	21.80	21.65	21.88
Married	15.39	15.38	14.62	14.23	14.82
Black	18.95	16.41	14.04	18.14	11.94
Hispanic	12.93	12.79	13.05	14.08	12.51
Other Race	5.64	5.48	5.15	5.49	4.98
Female	15.18	0.00	0.00	0.00	0.00
Number of Dependents	0.33	0.32	0.31	0.30	0.32
HS Dropout or GED	11.89	12.32	12.91	8.73	15.05
HS Graduate	76.37	75.68	76.67	81.44	74.22
Some College+	11.64	11.94	10.37	9.80	10.66
AFQT Score	58.86	59.47	58.01	56.36	58.86
			Panel (b): Service Experience		
Combat Occupation	36.62	42.57	64.30	62.18	65.39
Mths Deployed w/in 3 yrs	5.98	5.77	6.52	0.00	9.87
Combat Injury w/in 3 yrs	1.57	1.65	2.24	0.00	3.39
Combat Death w/in 3 yrs	0.20	0.20	0.29	0.04	0.41
Observations	782,232	483,367	157,415	53,425	103,990

Notes: This table reports summary statistics. In column (1), we report averages for all first-term soldiers under standard enlistment contracts who arrived at their units between 2001-2015. In column (2) we restrict to male soldiers who arrived to their first units between 2005-2015. In column (3) we restrict to our primary estimation sample, male soldiers who arrived at a Brigade Combat Team between 2005-2015 and satisfy other minor sample restrictions described in Section 1.2 and Appendix B.2. Columns (4) and (5) split the sample from column (3) into soldiers who did and did not deploy within 8 years (or, for the latest cohorts, the last year in our data). All demographic variables and occupations are measured prior to arrival at one's unit. Months deployed, combat injuries, and combat deaths are calculated over the 3 years after arrival.

Table 2: Covariate balance

	Deployment Instrument	OLS with FE
	(1)	(2)
Black	0.55	-1.39***
	(0.43)	(0.20)
Hispanic	-0.38	1.51***
	(0.46)	(0.20)
Other Race	0.02	0.76***
	(0.29)	(0.13)
Married	0.13	-1.40***
	(0.47)	(0.22)
Num. Dependents	0.01	-0.04***
	(0.01)	(0.01)
HS Graduate +	0.48	3.35***
	(0.42)	(0.22)
Age	0.08	-0.06***
	(0.05)	(0.02)
AFQT	-0.28	0.83***
	(0.22)	(0.10)
Observations	157,415	157,415
P-value on Joint Test	0.27	0.00

Notes: This table reports the results of tests for covariate balance. Each row in column (1) of this Table reports the coefficient from a separate regression of the stated covariate on our instrument (peer months deployed within 3 years). Regressions include duty-station by job by arrival year by term length fixed effects, as in Equation 2. For ease of interpretation, coefficients and standard errors are scaled by 10, so that they can be interpreted as the effects of a typical-length deployment. We fail to reject the null hypothesis that all coefficients are jointly zero. For comparison, column (2) reports coefficients from a separate regression of the stated covariate on the endogenous variable, months deployed (again scaled by 10 and conditional on the same set of fixed effects). The results show strong evidence of selection. Robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 3: Effects on disability and combat death and injury

	(1) 2 yrs	(2) 4 yrs	(3) 6 yrs	(4) 8 yrs	(5) 8 yrs mean
			Panel (a): Disability Receipt		
Any VADC Receipt	-0.07	3.43***	9.33***	9.42***	37.37
	(0.09)	(0.41)	(0.64)	(0.80)	
Any SSDI or SSI	0.52***	1.30***	1.99***	2.60***	3.39
	(0.08)	(0.16)	(0.22)	(0.30)	
Any Disability	0.42***	3.94***	9.56***	9.52***	37.81
	(0.12)	(0.43)	(0.65)	(0.80)	
Annual Amt VADC	3.70	751.09***	2129.52***	2602.30***	6129.44
	(10.77)	(68.11)	(120.32)	(171.73)	
Annual Amt SSDI or SSI	32.03***	168.69***	325.41***	426.38***	569.37
	(6.48)	(22.97)	(36.65)	(52.40)	
Annual Amt Disability	35.72***	919.78***	2454.93***	3028.68***	6698.81
	(12.82)	(77.69)	(137.79)	(198.29)	
			Panel (b): Trauma		
Combat Death	0.41***	0.37***	0.42***	0.48***	0.50
	(0.07)	(0.08)	(0.09)	(0.11)	
Ever Combat Injury	3.42***	3.71***	4.00***	4.43***	4.17
	(0.19)	(0.22)	(0.25)	(0.31)	
Army Profile	-0.76**	2.12***	2.14***	1.71**	25.53
	(0.36)	(0.52)	(0.61)	(0.72)	
Significant Army Profile	-0.53**	2.08***	2.48***	2.62***	15.04
	(0.24)	(0.40)	(0.48)	(0.59)	
Observations	157,415	157,415	129,176	101,387	101,387

Notes: This table reports 2SLS estimates of the effects of months deployed on disability and trauma. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. Panel (a) reports the effects of months deployed on disability compensation and receipt outcomes 2, 4, 6, and 8 years after arrival. Total Disability (VADC plus SSI plus SSDI), VADC, and SSI/SSDI amounts are measured in 2020 dollars and reflect annual payments. The first rows of panel (b) report the effect of months deployed on combat deaths, followed by combat injury. The third and fourth rows of panel (b) report the effect of months deployed on all and significant Army health profiles. Column (5) reports the mean of each outcome 8 years after a soldier's arrival. Robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 4: Effects on misconduct, credit scores, and education

	(1) 2 yrs	(2) 4 yrs	(3) 6 yrs	(4) 8 yrs	(5) 8 yrs mean
			Panel (a): Misconduct and Incarceration		
Separated for Misconduct/Barred	-3.92***	-0.63	-0.52	-1.02	25.05
	(0.38)	(0.53)	(0.60)	(0.70)	
Observations	157,415	157,415	129,176	101,387	101,387
Ever Incarcerated	-0.09	0.05	0.12	0.10	2.41
	(0.07)	(0.14)	(0.19)	(0.25)	
Observations	156,246	156,246	128,120	100,381	100,381
			Panel (b): Credit Scores (as of 2017/2020)		
	Jun 2017	Avg(Y_{2017})	Dec 2020	Avg(Y_{2020})	
Vantage Score	0.52	622.10	1.91	655.20	
	(1.32)		(1.33)		
Observations	142,010		144,708		
			Panel (c): Education Outcomes		
	2 yrs	4 yrs	6 yrs	8 yrs	8 yrs mean
Enroll Post-arrival	-1.48***	0.66	1.59**	1.09	55.70
	(0.40)	(0.58)	(0.73)	(0.81)	
Assc Deg+ Post-arrival	-0.08	-0.16	0.01	0.66	8.69
	(0.07)	(0.13)	(0.27)	(0.47)	
Observations	157,415	157,415	129,176	101,387	101,387

Notes: This table reports 2SLS estimates of the effects of months deployed on Army separations resulting from misconduct, incarceration during or after military service as captured through military and national LexisNexis records, Vantage credit scores from Experian credit bureau, and post-secondary education outcomes from National Student Clearinghouse. For incarceration we drop $< 1\%$ of the sample that was not sent to LexisNexis. For Vantage scores in 2017 we drop 1% of our sample that was not sent to Experian. In addition, we drop individuals who have no credit scores (2SLS regressions on an indicator for having a credit score are insignificantly different from 0). Robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 5: Effects of violent deployments

	(1) 10 Months Deployed	(2) (10 Mths Dep) × (1σ Peer Cas)	(3) Mean
	Panel (a): Trauma and Disability		
Combat Death	0.01	0.27***	0.50
	(0.12)	(0.04)	
Ever Combat Injury	1.16***	1.86***	4.17
	(0.33)	(0.11)	
Significant Army Profile	0.84	1.02***	15.04
	(0.64)	(0.16)	
Annual Amt VADC	1876***	414***	6129
	(190)	(48)	
Any VADC Receipt	7.16***	1.29***	37.37
	(0.88)	(0.21)	
	Panel (b): Noncombat Mortality Outcomes		
Noncombat Death	0.07	-0.01	1.25
	(0.20)	(0.05)	
Death of Despair	0.10	-0.06	0.79
	(0.16)	(0.04)	
Suicide	0.02	-0.02	0.44
	(0.12)	(0.03)	
Drug- or Alcohol-Rel. Death	0.14	-0.06*	0.38
	(0.11)	(0.03)	
Motor Vehicle Death	0.04	-0.003	0.27
	(0.09)	(0.022)	
	Panel (c): Misconduct, Credit, and Education		
Separated for Misconduct	-0.90	-0.07	25.05
	(0.77)	(0.18)	
Ever Incarcerated	-0.07	0.10	2.41
	(0.28)	(0.07)	
Credit Score in 2020 (Vantage)	1.34	-0.01	655.78
	(1.74)	(0.41)	
College Enrollment	1.13	-0.03	55.70
	(0.89)	(0.21)	
Associate's Deg+	0.41	0.14	8.69
	(0.52)	(0.12)	
Observations	101,387		

Notes: This table reports 2SLS estimates of Equation 6 (with corresponding first stage Equations 7 and 8) on our primary outcomes as of 8 years after a soldier arrives at his initial operational assignment, except credit outcomes which are as of 2020. As described in Section 4, we augment our baseline model to include an interaction between months deployed and peer casualty rates, which proxy for more dangerous deployments. Peer casualties are the share of peer soldiers (those who arrive in the same BCT within the same quarter) who suffer non-fatal combat injuries or fatal combat deaths within three years. Column (1) reports $\hat{\beta}$ while column (2) reports $\hat{\gamma}$. Each row represents a separate regression on a separate outcome. Coefficients and standard errors in column (1) are scaled by 10. Coefficients and standard errors in column (2) are scaled by 10σ, where σ is the sample standard deviation of peer casualties. In addition, the sample sizes for incarceration and credit are smaller: 100,381 for Ever Incarcerated and 93,252 for Vantage Credit Score 2020. Robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

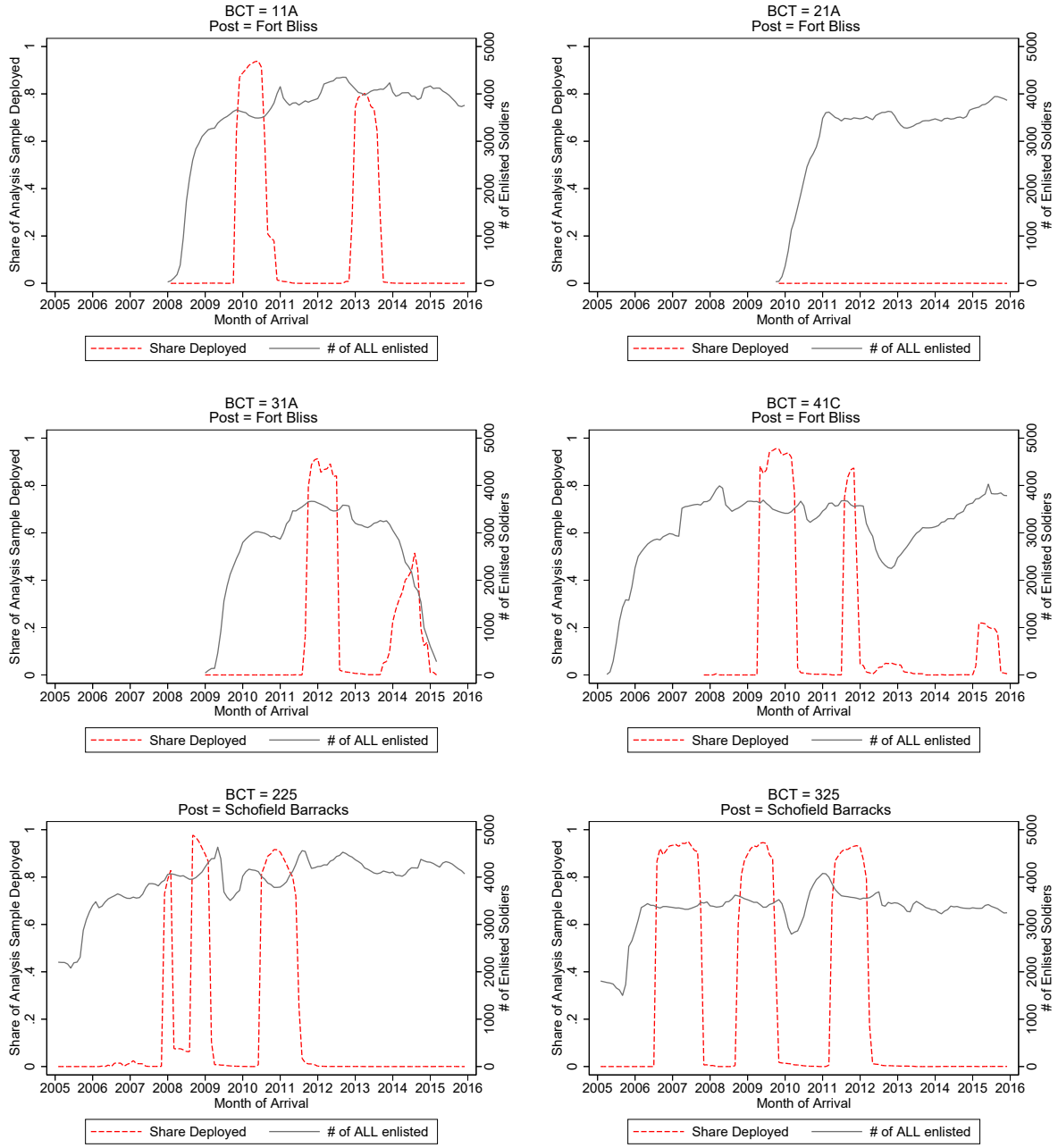
Online Appendix for "The Effects of Combat Deployments on Veterans' Outcomes"

By Jesse Bruhn, Kyle Greenberg, Matthew Gudgeon, Evan K. Rose, and Yotam Shem-Tov

Online Appendix Table of Contents

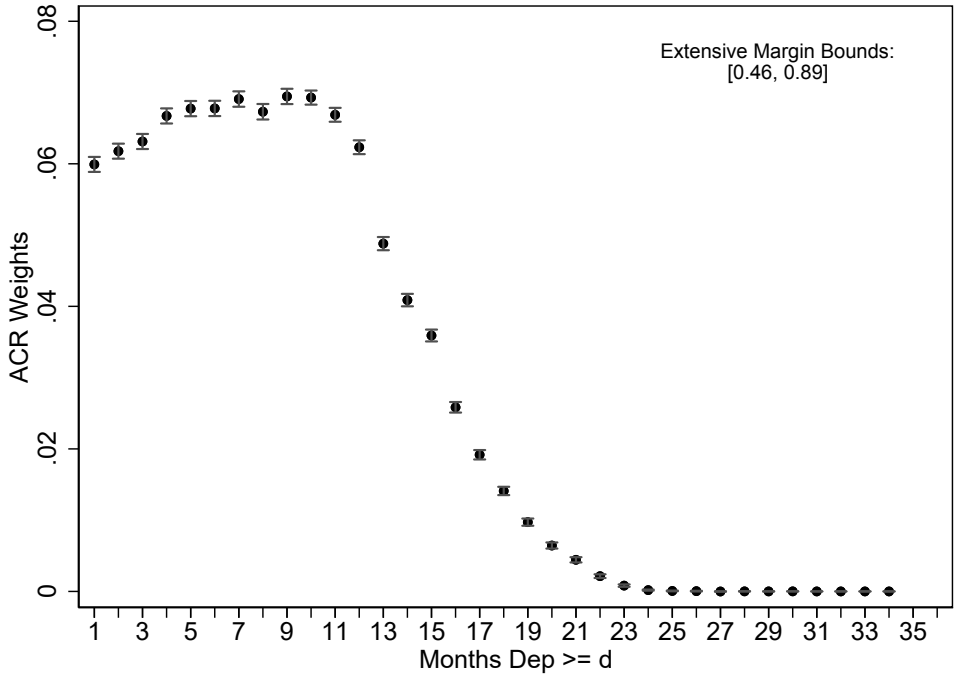
A Additional Results

Figure A.1: Share of BCT deployed by month (select BCTs)



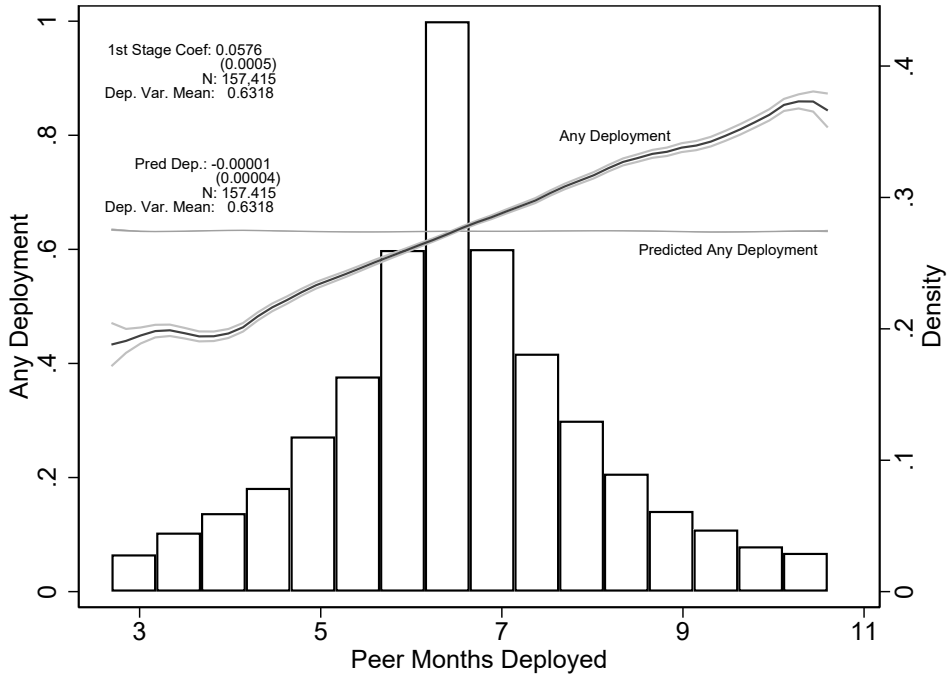
Notes: The figures above report the share of all enlisted personnel within each brigade combat team who are deployed to a combat zone by month (left axis) and the total number of enlisted personnel assigned to the BCT by month (right axis). The figures report on the six BCTs that were headquartered in Fort Bliss, Texas or Schofield Barracks, Hawaii during the period of our study (2005-2015).

Figure A.2: ACR weights and bounds on extensive margin complier share



Notes: This figure reports the ω_d weights in the "Average Causal Response" (ACR) identified using 2SLS (see Equation (4)). Each point is an estimate of the change in deployment length induced by the instrument ($\Pr(D_i(1) \geq d > D_i(0))$), which corresponds to the normalized weights in the ACR. These changes reflect the likelihood that a soldier will be deployed for less than d months if assigned to a BCT with a low value of Z_i and that would be deployed for d months or more if assigned to a BCT with a high value of Z_i. The probabilities $\Pr(D_i(1) \geq d > D_i(0))$ can be estimated using our reduced-form specification and changing the outcome to an indicator for deploying for d or more months (i.e., $1(D_i \geq d)$). The text in the upper-right corner reports bounds on the share of compliers shifted from no deployment to some (i.e., on the extensive margin) estimated using the procedure described in Appendix E of Garin et al. (2023).

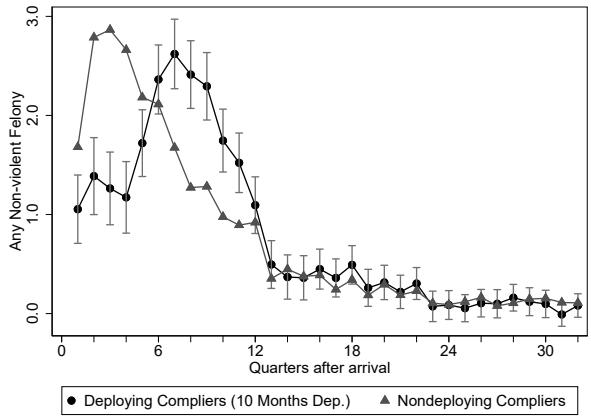
Figure A.3: Effects of peer months deployed on any deployment



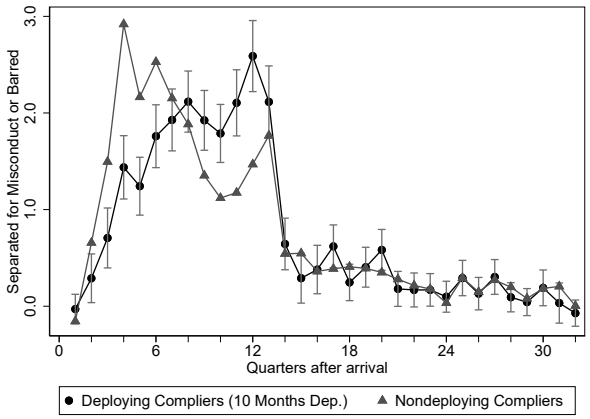
Notes: This figure repeats the exercise in Figure 2, replacing the outcome with any deployment within the first three years after arrival. It shows that our instrument also generates variation in the extensive margin of deployment. See notes to Figure 2 for additional details.

Figure A.4: Dynamic effects of deployment on misconduct and crime

(a) Any Non-Violent Felony *in* given quarter



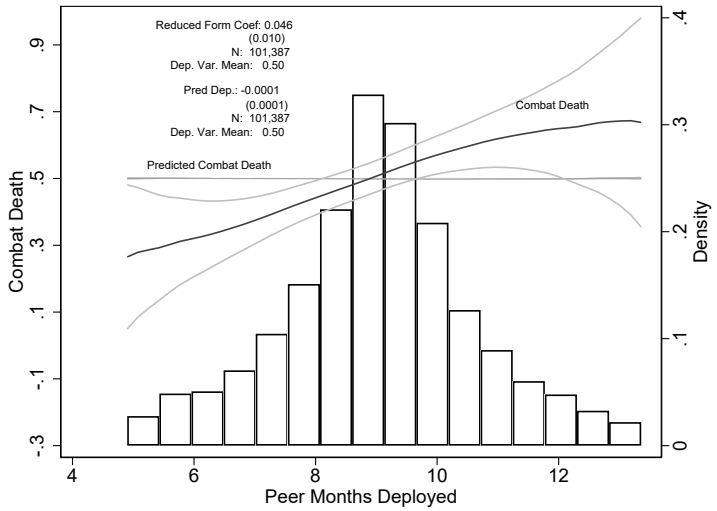
(b) Separated for Misconduct *in* given quarter



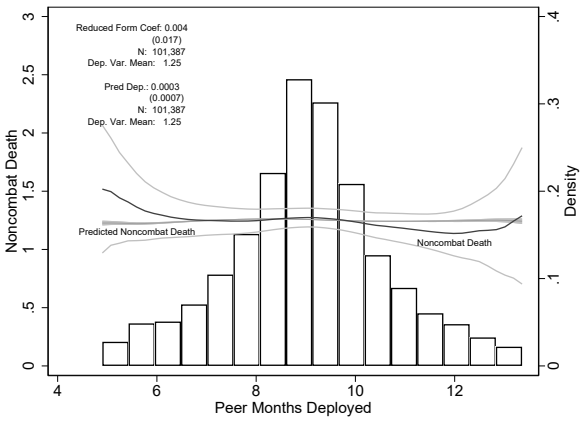
Notes: This figure plots expected means among nondeployers (untreated compliers) using triangles and the predicted outcome for deployers (constructed as the untreated complier mean plus the estimated treatment effect of a 10 month deployment) using circles for the outcome in the stated quarter after arrival. Panel (a) plots non-violent felony cases from military records in percentage points, and panel (b) plots separations for misconduct or separations while barred from re-enlistment for misconduct in percentage points. 95% confidence intervals on the estimated treatment effect are depicted by the vertical bars.

Figure A.5: Reduced form effects of peer months deployed

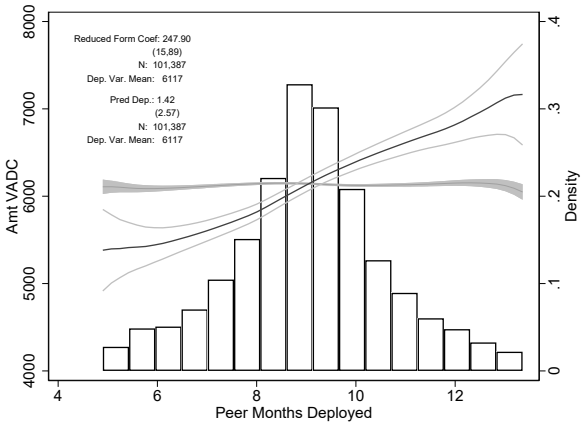
(a) Combat Death



(b) Noncombat Death

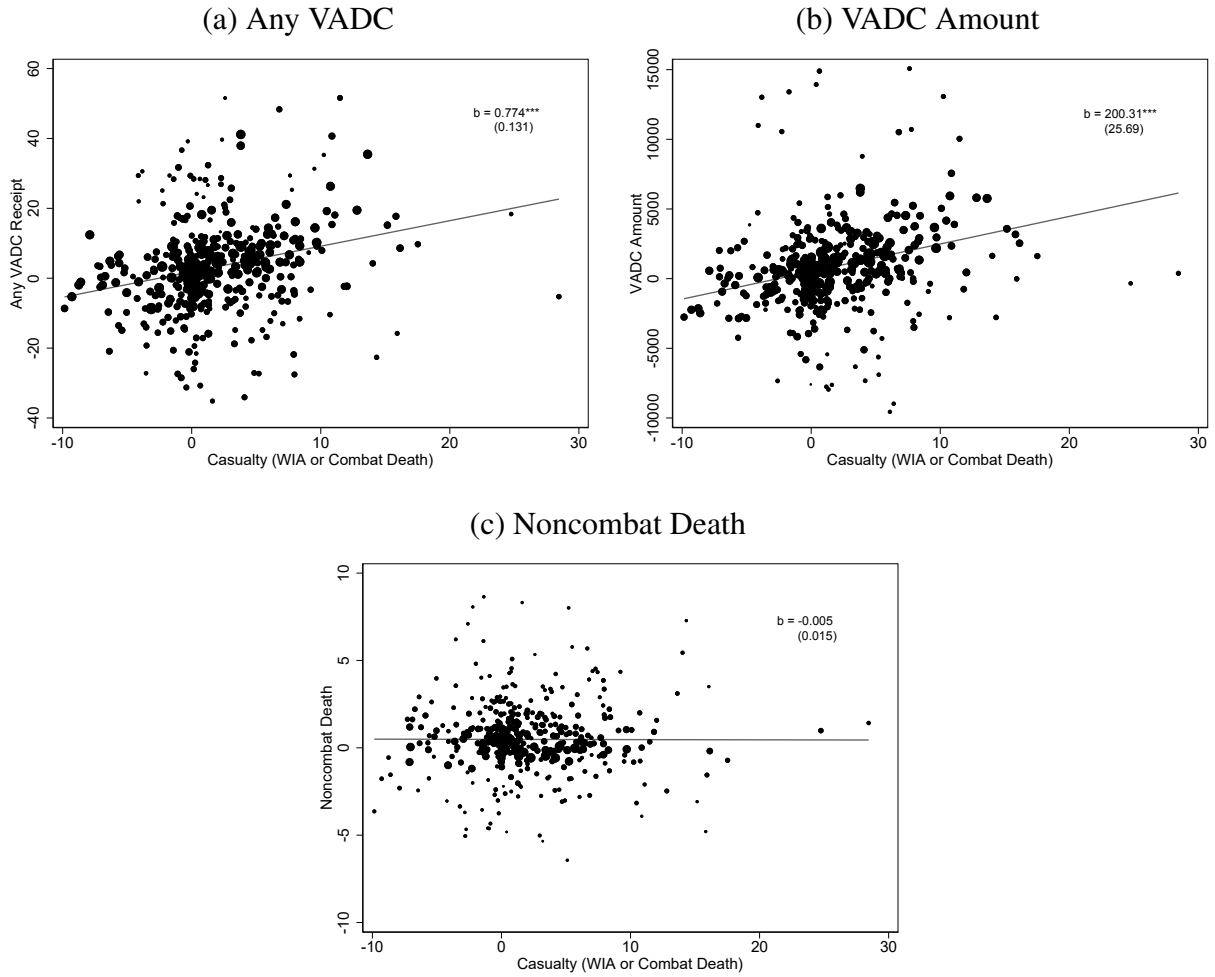


(c) VADC Amount



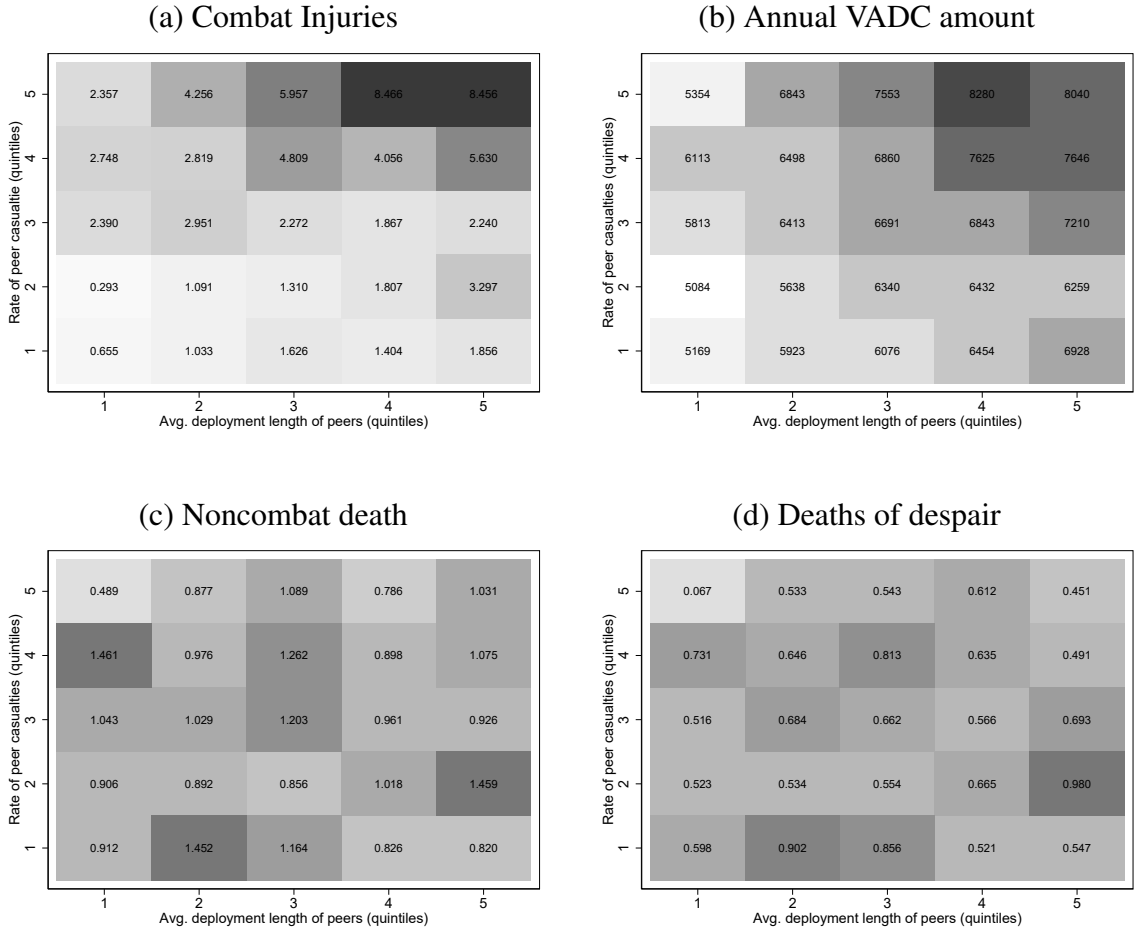
Notes: The figure repeats the exercise in Figure 2, replacing the outcome with combat death within 8 years of arrival in panel (a), noncombat death within 8 years of arrival in panel (b), and VADC amount 8 years after arrival in panel (c) for the 2005-2011 cohorts. Coefficients are not scaled by 10, and hence reflect the average effect of being assigned to a unit with one month higher peer deployment rates. See notes to Figure 2 for additional details.

Figure A.6: BCT-specific effects of deployment on casualties, VADC, and noncombat deaths



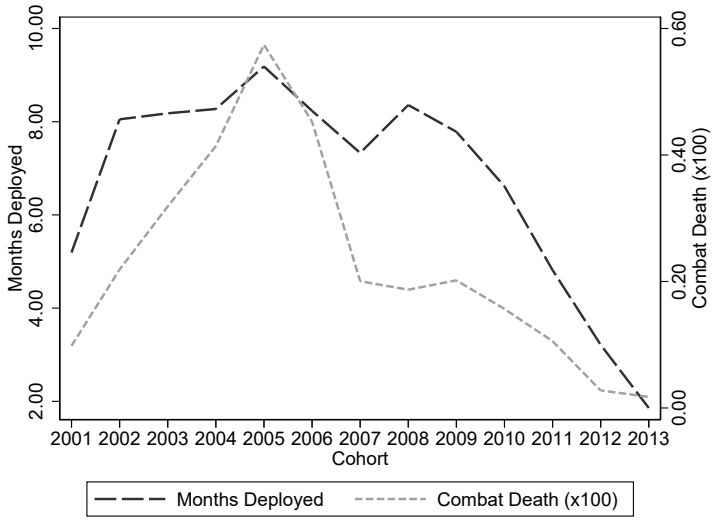
Notes: These figures plot the relationship between BCT×quarter-specific effects of a 10-month deployment on casualty rates and effects on other outcomes: any VADC in panel a), VADC amount in panel b), and non-combat death in panel c). All outcomes are measured as of 8 years after arrival. BCT×quarter refers to the combination of brigade assigned and quarter of enlistment. We construct these estimates by interacting months deployed with indicators for assigned BCT×quarter in our main specification, omitting the largest BCT×quarter (3rd Cavalry Regiment, Fort Hood in 2006Q3) as the reference group. The instrument is a set of dummies for BCT×quarter assignment, so that the full 2SLS system is just-identified. Each dot corresponds to the coefficient on the BCT×quarter interaction for two outcomes, with effects on casualties on the x-axis and other outcomes on the y-axis. The positive slope in panels a) and b) shows that for soldiers assigned to BCT×quarters where deployment led to higher casualty rates, deployment also caused increases in VADC. Panel c) shows that the same is not true of noncombat deaths. We show effects for BCT×quarter-of-arrival combinations with at least 100 observations, though results change little if we include all estimated effects. For display purposes, we also trim the bottom and top percentile of estimated y-axis outcome coefficients, but these are included in the regression results reported in the top right hand corner. Points are weighted by the inverse of the standard error on the estimated y-axis outcome coefficient.

Figure A.7: Joint reduced form effects of peer deployment and peer casualties



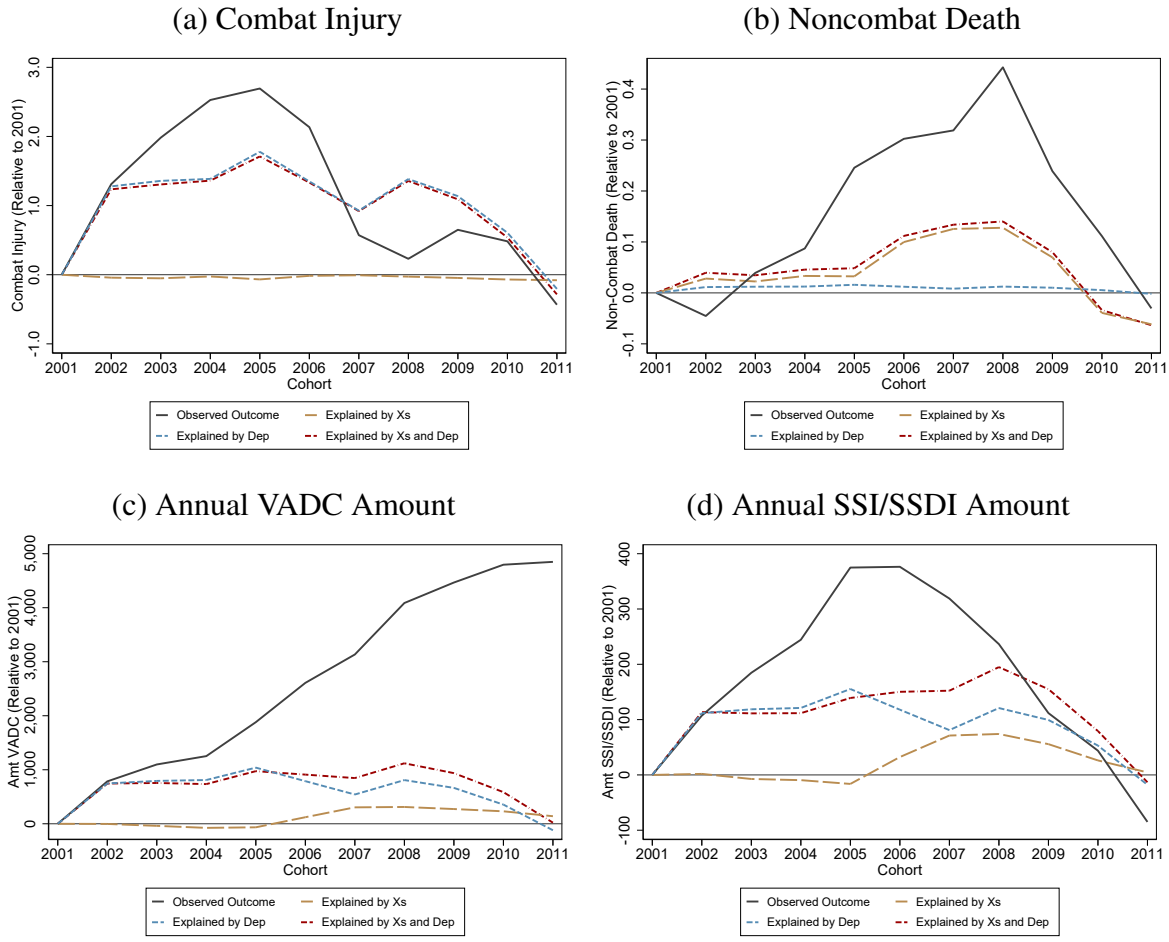
Notes: This figure non-parametrically examines the impact of exposure to different combinations of deployment and violence on combat injuries (panel (a)), annual VADC amount (panel (b)), noncombat death (panel (c)), and deaths of despair (panel (d)). We divide the average deployment among peers (our instrument for deployment) and peer casualties into quintiles, yielding 25 combinations of observations. We then estimate the reduced form effect of each cell relative to the lowest level of both peer deployment and peer casualties (i.e., cell $1-1$). We display the results as heat plots, where the number within each cell is the sum of the reduced form effect for that cell and the mean of the omitted category (i.e. cell $1-1$). Specifically, panel (a) shows that increases in either instrument increase the likelihood of injury. Similarly, panel (b) shows that higher levels of either instrument increase VADC amounts. However, panels (c) and (d) clearly shows no relationship between either instrument and noncombat death or death of despair more specifically. Moreover, none of the cells in panels (c) and (d) is statistically different than the omitted category of the lowest level of both instruments (cell $1-1$).

Figure A.8: Deployment and combat death trends by cohort



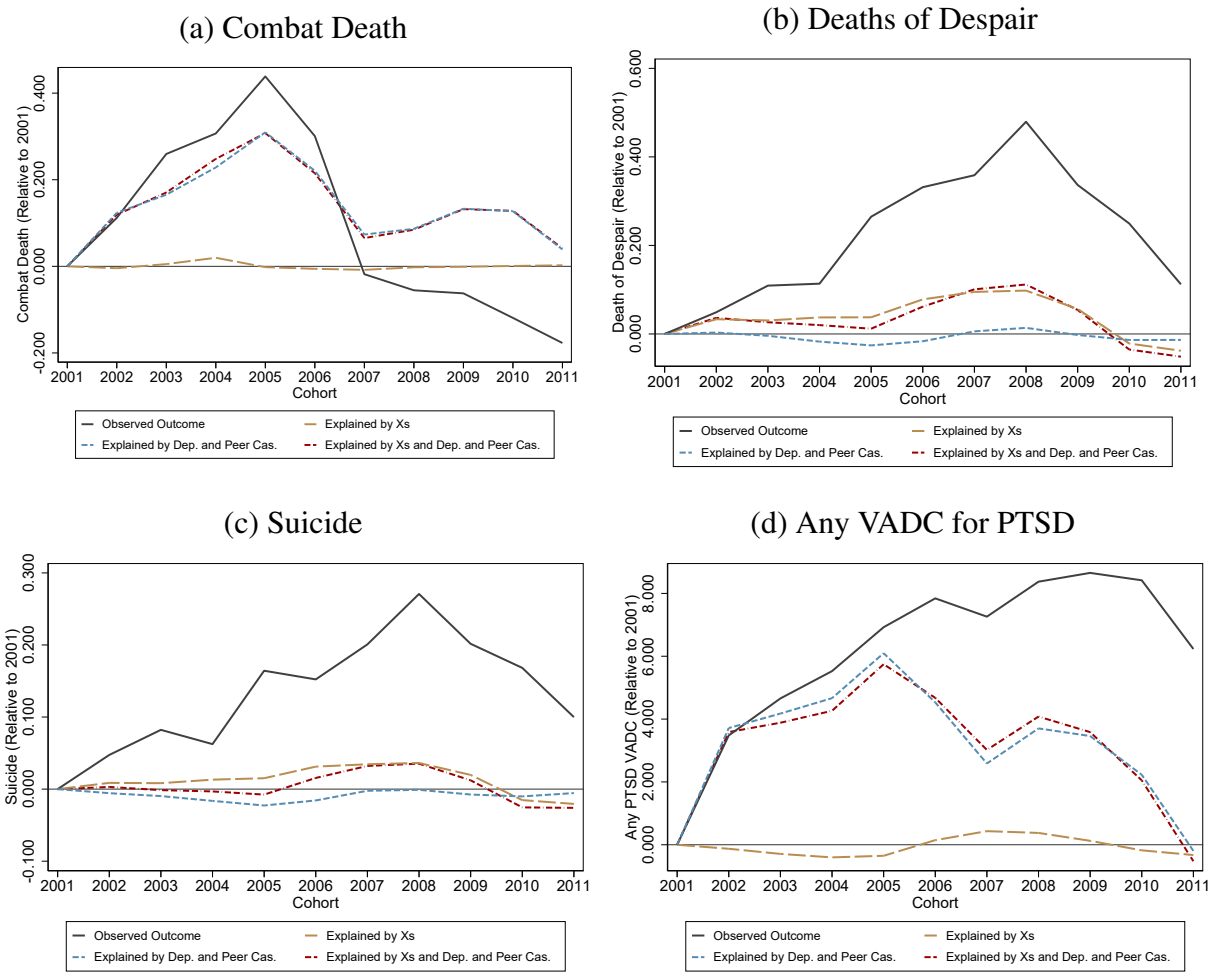
Notes: This figure plots average months deployed (within 3 years of arrival) on the left axis (darker, long-dashed line) and average combat death rates (within 3 years of arrival) on the right axis (lighter, short-dashed line) by year-of-arrival cohort.

Figure A.9: Cohort decomposition without peer casualty interaction



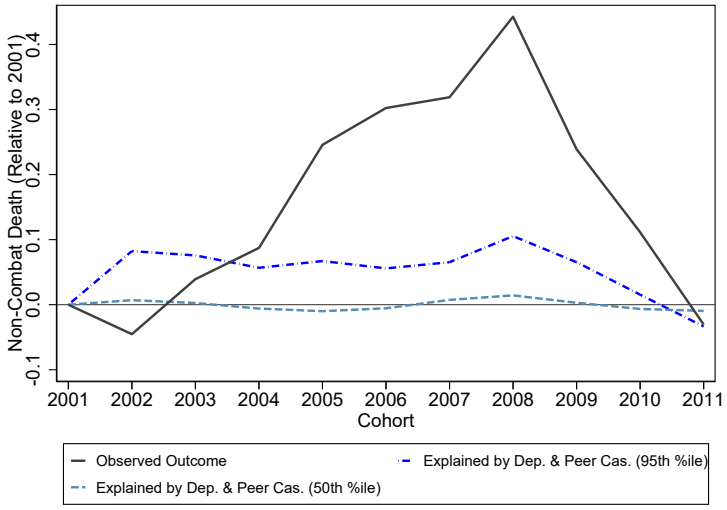
Notes: Like Figure 6, this figure plots cohort trends in the stated outcome and decomposes them into parts we can explain using the specification discussed in Section 5. Here, the decomposition is based off of estimates of Equation 9 that omit the peer casualty interaction.

Figure A.10: Cohort decompositions for additional outcomes



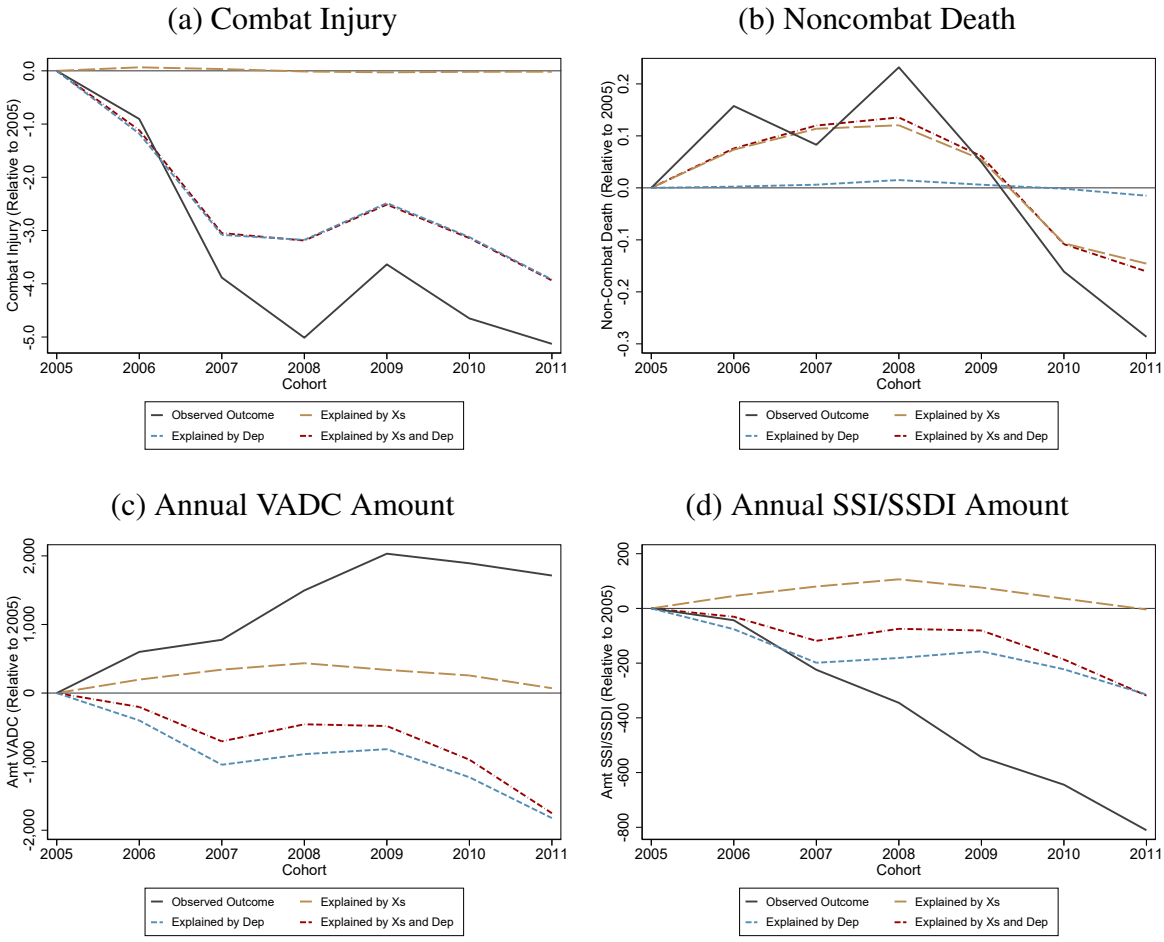
Notes: Like Figure 6, this figure plots cohort trends in the stated outcome and decomposes them into parts we can explain using the specification discussed in Section 5. Here, we perform this exercise for four additional outcomes. Panel (a) decomposes cohort trends in combat deaths 8 years after arrival. Panel (b) does so for deaths of despair within 8 years after arrival, panel (c) does so for suicides within 8 years after arrival, and panel (d) does so for any VADC with PTSD receipt.

Figure A.11: Accounting for statistical uncertainty in the effect of deployment on noncombat death



Notes: This figure examines how the share of noncombat deaths across cohorts explained by deployment and peer casualties changes when we account for statistical uncertainty in the effect of deployment on noncombat death. Specifically, we bootstrap the entire procedure in Figure 6 1000 times and calculate the year over year variance explained by deployment and peer casualties following each run (as in column (3) of Table A.27). We then re-construct Figure 6 using the model that yielded the 95th percentile of the year over year variance explained by deployment and peer casualties (shown in darker blue). The median from this procedure (which mimics our baseline) is shown in light blue for comparison.

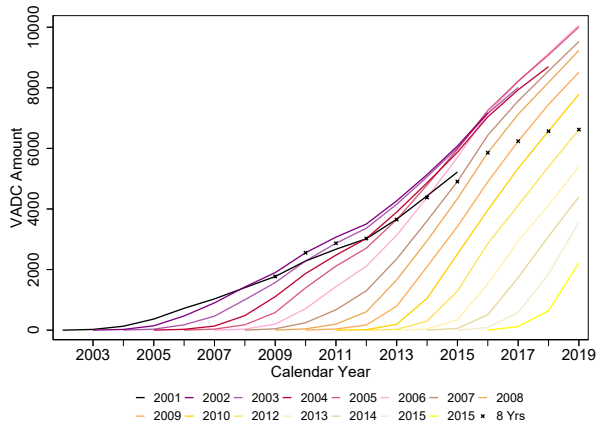
Figure A.12: Cohort decomposition using analysis sample only



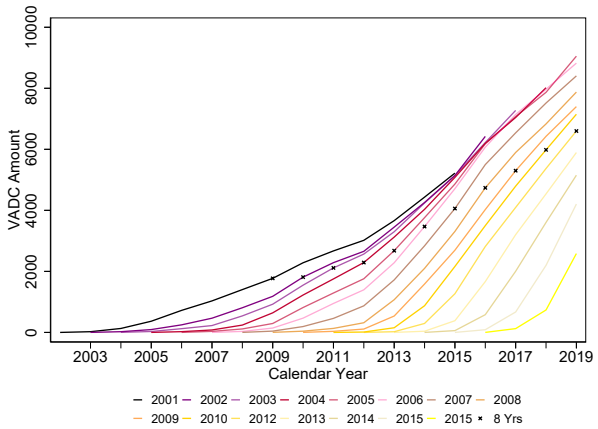
Notes: In this figure, we perform the same exercise as we did in Figure 6 but restrict to our primary analysis sample (Table 1 column (3)). Outcome means are plotted relative to the 2005 cohort mean.

Figure A.13: VADC trends over time by enlistment cohort

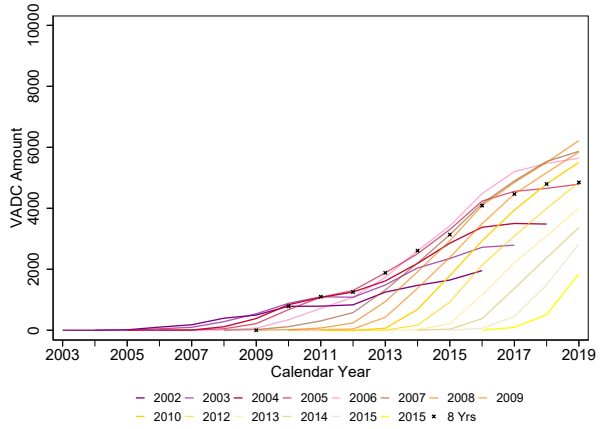
(a) Trends in VADC by Cohort



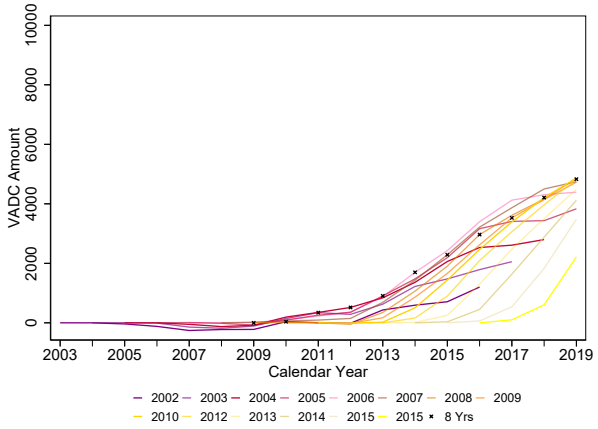
(b) Subtracting portion explained by Δ X and Δ Dep. (rel to 2001 cohort)



(c) Differences in VADC Rel. to 2001 Cohort Trajectory

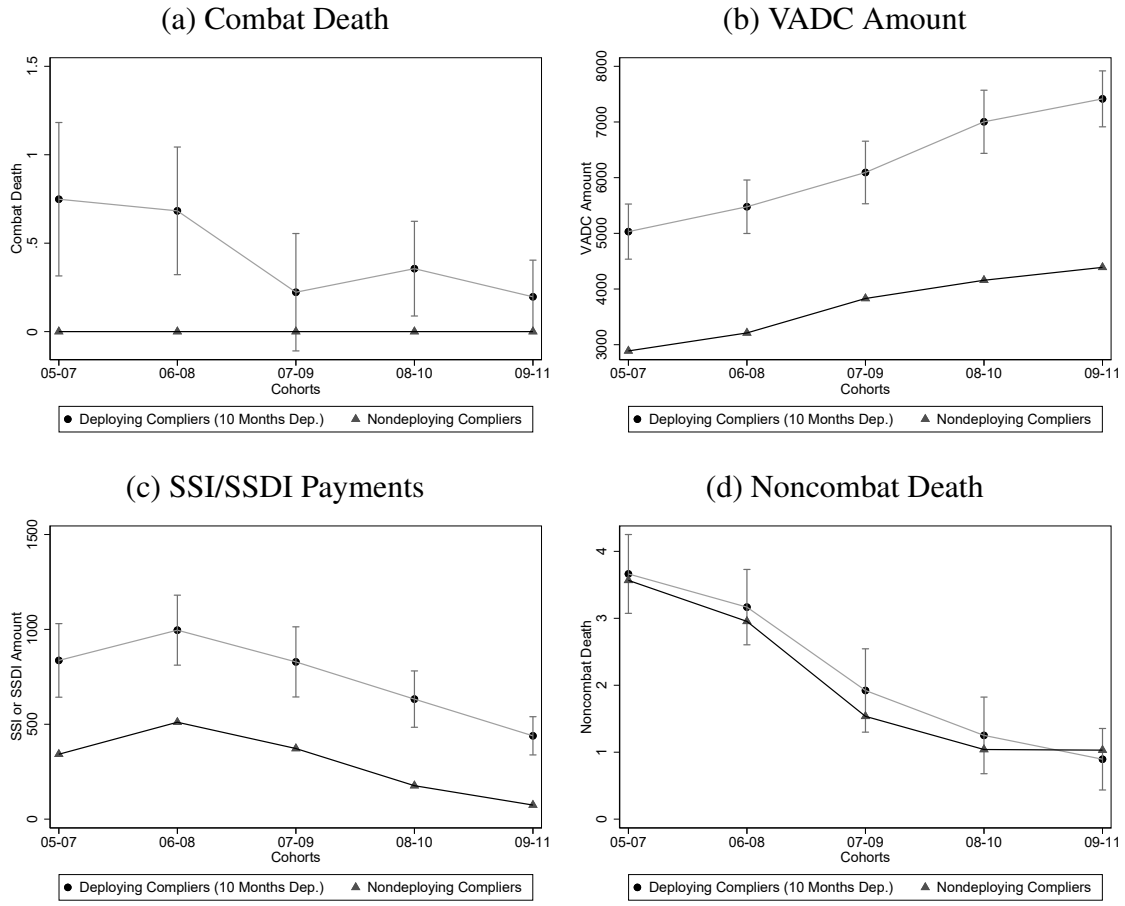


(d) Differences after subtr. portion explained by Δ X and Δ Dep



Notes: Panel (a) plots average VADC over calendar years for each cohort that arrived at their first operational unit between 2001 and 2015. Each series reflects a different cohort and begins 1 year after the cohort first arrives at their unit and extends to 14 years after assignment. Panel b) plots average VADC by cohort after subtracting off what can be explained by changes in months deployed and Xs (relative to the 2001 cohort). Panel c) plots, for each cohort, the difference in VADC relative to the 2001 cohort in the same year after assignment. Panel d) plots the same difference in VADC relative to the 2001 cohort as panel (c) after accounting for differences explained by changes in Xs or deployment. The "x-marks" on the plots annotate average VADC amounts 8 years after assignment.

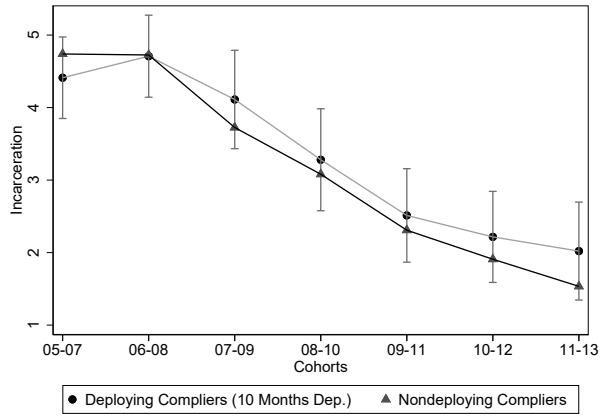
Figure A.14: Treatment effects across 3-year rolling cohorts (8 years out)



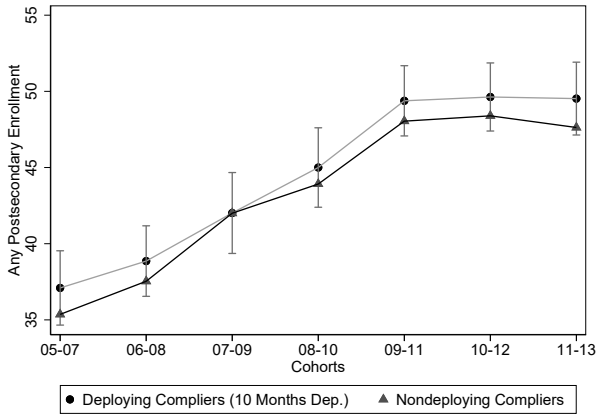
Notes: This figure plots expected outcome means among nondeployers (untreated compliers) using triangles and the predicted outcome for deployers (constructed as the untreated complier mean plus the estimated treatment effect of a 10 month deployment) using circles among various rolling cohorts as stated on the x-axis. Untreated complier means are set to 0 for combat death since these only occur on deployment. Treatment effects of being 10 months deployed can be inferred as the gap between the two lines. Panel (a) plots combat death, (b) VADC amount, (c) SSI/SSDI amount, and (d) noncombat death. All outcomes are measured as of 8 years out. 95% confidence intervals on the estimated treatment effect are depicted by the vertical bars.

Figure A.15: Treatment effects across 3-year rolling cohorts (other outcomes)

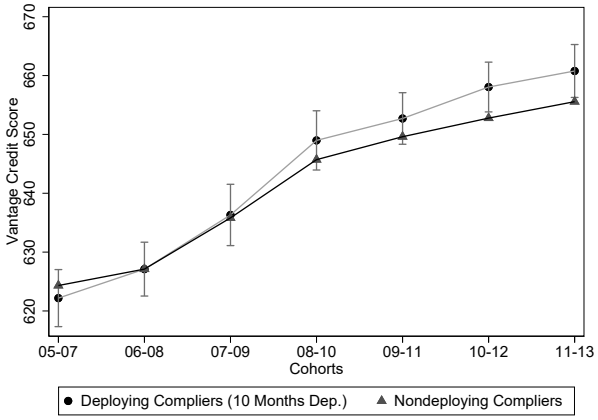
(a) Incarceration



(b) Post-secondary Enrollment



(c) Credit Score



Notes: This figure plots expected outcome means among nondeployers (untreated compliers) using triangles and the predicted outcome for deployers (constructed as the untreated complier mean plus the estimated treatment effect of a 10 month deployment) using circles among various rolling cohorts as stated on the x-axis. Treatment effects of being 10 months deployed can be inferred as the gap between the two lines. Panel (a) plots any incarceration within 6 years, (b) any post-secondary enrollment within 6 years, and (c) credit score in 2020. 95% confidence intervals on the estimated treatment effect are depicted by the vertical bars.

Table A.1: Number of BCTs and duty-stations (Posts) by year

	Num of BCTs (Posts)
2005	20 (7)
2006	26 (8)
2007	29 (9)
2008	31 (10)
2009	33 (10)
2010	35 (10)
2011	31 (10)
2012	26 (10)
2013	26 (10)
2014	26 (10)
2015	26 (10)

Notes: This table reports the number of Army brigade combat teams (BCTs) and duty-stations (or Army "posts") soldiers in our sample were assigned to in each year indicated in the left column.

Table A.2: Reduced form effects

	(1) 2 yrs	(2) 4 yrs	(3) 6 yrs	(4) 8 yrs	(5) 8 yrs mean
			Panel (a): VADC and Trauma of War		
Combat Death	0.39***	0.35***	0.40***	0.46***	0.50
	(0.06)	(0.07)	(0.09)	(0.10)	
Ever Combat Injury	3.29***	3.57***	3.83***	4.22***	4.17
	(0.18)	(0.21)	(0.24)	(0.29)	
Significant Army Profile	-0.50**	2.00***	2.38***	2.50***	15.04
	(0.23)	(0.38)	(0.46)	(0.56)	
Annual Amt VADC	3.55	721.48***	2040.20***	2478.99***	6,129.44
	(10.34)	(64.85)	(114.19)	(163.03)	
Any VADC Receipt	-0.06	3.29***	8.94***	8.97***	37.37
	(0.09)	(0.39)	(0.61)	(0.76)	
			Panel (b): Non-combat Mortality Outcomes		
Noncombat Death	0.09	0.14	0.11	0.04	1.25
	(0.07)	(0.10)	(0.13)	(0.17)	
Death of Despair	-0.01	0.06	0.10	0.00	0.79
	(0.05)	(0.07)	(0.10)	(0.13)	
Suicide	-0.03	-0.02	0.00	-0.02	0.44
	(0.04)	(0.06)	(0.08)	(0.10)	
Drug- or Alcohol-Rel. Death	0.01	0.06	0.08	0.04	0.38
	(0.03)	(0.05)	(0.06)	(0.09)	
Motor Vehicle Death	0.05	0.06	0.02	0.04	0.27
	(0.04)	(0.05)	(0.06)	(0.08)	
			Panel (c): Misconduct, Credit, and Education		
Separated for Misconduct/Barred	-3.76***	-0.61	-0.49	-0.97	25.05
	(0.37)	(0.51)	(0.58)	(0.67)	
Ever Incarcerated	-0.09	0.05	0.11	0.09	2.41
	(0.06)	(0.13)	(0.18)	(0.24)	
Credit Score in 2020 (Vantage)	1.85	1.85	2.03	1.27	655.78
	(1.29)	(1.29)	(1.36)	(1.51)	
Enrolled by 2020 (Post-Arrival)	1.68***	1.68***	1.70**	1.35*	65.64
	(0.64)	(0.64)	(0.67)	(0.74)	
Assc Deg+ by 2020 (Post-Arrival)	0.01	0.01	0.06	0.01	19.51
	(0.50)	(0.50)	(0.55)	(0.63)	
Observations	157,415	157,415	129,176	101,387	101,387

Notes: This table reports estimates of the reduced form effects of peer months deployed on our key outcomes of interest (analogous to Equation 2). In addition, the sample sizes are smaller for ever incarcerated (100,381 at 8 years) and for Vantage credit score (93,252 at 8 years). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.3: Frandsen, Lefgren, and Leslie (2023) tests of monotonicity and exclusion

	(1) Any VADC	(2) Combat Death	(3) Noncombat Death
$N \geq 250$			
Test stat	3.1135	2.6349	2.0268
P-value	0.1936	0.74	0.9991
$N \geq 100$			
Test stat	3.6196	2.8652	2.4778
P-value	0.1261	0.9165	0.9998
$N \geq 50$			
Test stat	3.6196	2.8652	-
P-value	0.185	0.9762	-

Notes: This table reports results of the tests proposed in Frandsen et al. (2023). Each pair of rows reports the test statistic and p-value subsetting the sample to BCT-quarter cells with at least N observations. We restrict cell size because asymptotic approximations required for the test are unlikely to perform will in cells with a small numbers of observations. Column 1 uses an indicator for any VADC receipt within 6 years as the outcome. Column 2 uses an indicator for combat death. Column 3 uses an indicator for noncombat death. Tests for cells with at least 50 observations for noncombat death cannot be computed due to the rarity of the outcome.

Table A.4: Association between deployment injuries and VADC receipt

	(1)	(2)	(3)	(4)	(5)
			Panel (a): Any VADC Reciept		
10 Months Deployed	-0.35	-1.00*	1.07*	0.61	3.73***
	(0.58)	(0.57)	(0.55)	(0.55)	(0.62)
Combat Injury by 3 Years		24.45***		21.53***	24.95***
		(0.88)		(0.98)	(1.01)
Significant Army Profile by 3			42.52***	42.20***	38.32***
Years			(0.68)	(0.75)	(0.73)
Combat Injury by 3 Years X				-13.02***	-18.39***
Significant Army Profile by 3 Years				(1.77)	(1.65)
			Panel (b): Annual Amt VADC		
10 Months Deployed	60.77	-171.66	419.75***	140.40	656.32***
	(144.41)	(137.94)	(137.26)	(132.43)	(154.74)
Combat Injury by 3 Years		8662.65***		6664.35***	7881.95***
		(291.28)		(289.14)	(324.50)
Significant Army Profile by 3			10783.13***	9320.88***	8854.73***
Years			(248.82)	(244.28)	(253.32)
Combat Injury by 3 Years X				5220.51***	4007.58***
Significant Army Profile by 3 Years				(868.40)	(882.39)
Conditional on Deployment	X	X	X	X	X
Conditional on Separation					X
Observations	83,370	83,370	83,370	83,370	66,943

Notes: This table reports associations between being wounded in action or having a significant Army profile within 3 years of arrival on VADC receipt (panel (a)) and VADC amount (panel (b)) as of 8 years after enlistment. All specifications restrict to soldiers who have deployed. Note that because all specifications include months deployed as a regressor, the coefficients on additional terms are properly interpreted as holding constant length of time deployed. The specification in column (5) further restricts to soldiers who have separated within 8 years. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.5: Disability diagnoses and additional outcomes

	(1) 2 yrs	(2) 4 yrs	(3) 6 yrs	(4) 8 yrs	(5) 8 yrs mean
			Panel (a): Top 5 Conditions		
Tinnitus	-0.01	2.07***	6.11***	6.39***	20.63
	(0.06)	(0.31)	(0.50)	(0.65)	
Limitation of flexion, knee	-0.05	0.54**	1.96***	1.90***	10.77
	(0.04)	(0.24)	(0.38)	(0.51)	
PTSD	0.02	3.67***	10.87***	12.82***	16.71
	(0.03)	(0.21)	(0.41)	(0.59)	
Lumbosacral or cervical strain	0.03	1.04***	3.37***	2.65***	14.07
	(0.06)	(0.28)	(0.44)	(0.56)	
Limitation of motion of the ankle	0.08**	0.41**	1.12***	1.05***	5.78
	(0.03)	(0.18)	(0.29)	(0.38)	
			Panel (b): Additional Outcomes		
Serious or Very Serious Combat Injury	0.29***	0.32***	0.32***	0.36***	0.43
	(0.05)	(0.07)	(0.07)	(0.09)	
Very Serious Combat Injury	0.02	0.05*	0.05*	0.07*	0.10
	(0.02)	(0.03)	(0.03)	(0.04)	
Any VADC w/ Amputation			0.22***	0.20***	0.24
			(0.05)	(0.07)	
Any VADC IU or CDR 100	0.01	0.54***	1.79***	2.77***	4.12
	(0.01)	(0.10)	(0.20)	(0.32)	
Any SSI or SSDI	0.52***	1.30***	1.99***	2.60***	3.39
	(0.08)	(0.16)	(0.22)	(0.30)	
Amount SSI or SSDI	32.03***	168.69***	325.41***	426.38***	569.37
	(6.48)	(22.97)	(36.65)	(52.40)	
Observations	157,415	157,415	129,176	101,387	101,387

Notes: Panel (a) of this table reports the 2SLS effect (Equation 1) of months deployed on receiving VADC with any of the 5 most common disabilities among GWOT recipients based on https://www.benefits.va.gov/REPORTS/abr/docs/2021_compensation.pdf. Conditions are not mutually exclusive – soldiers can receive VADC for multiple disabilities. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. Panel (b) examines the effect of deployment on rarer but severe outcomes: serious combat injuries (defined as an injury from adversarial action that a medical authority deems to be life-threatening or life-altering, or where death is possible but not likely within 72 hours), very serious combat injuries (a serious combat injury where a medical authority declares it more likely than not that death will occur within 72 hours), receiving VADC with a documented amputation, receiving VADC with an individual unemployability designation or with a disability rating of 100%, and any and amount SSI or SSDI (which are both work limiting). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.6: Separations

	(1)	(2)	(3)	(4)	(5)
	2 yrs	4 yrs	6 yrs	8 yrs	8 yrs mean
Ever Separated	-4.96***	2.60***	3.89***	2.57***	82.87
	(0.46)	(0.69)	(0.67)	(0.64)	
Voluntarily Separated	0.01	1.74***	2.36***	0.85	33.59
	(0.04)	(0.58)	(0.69)	(0.78)	
Separated for Misconduct/Barred	-3.92***	-0.63	-0.52	-1.02	25.05
	(0.38)	(0.53)	(0.60)	(0.70)	
Separated for Disability	-0.40**	1.69***	2.57***	2.87***	13.03
	(0.17)	(0.35)	(0.45)	(0.56)	
Observations	157,415	157,415	129,176	101,387	101,387

Notes: This table reports the 2SLS effect (Equation 1) of months deployed on separation from the Army and various sub-classifications of separation. We classify a soldier as "Ever Separated" if they stopped serving in the Active Duty Army prior to the year indicated in the column heading. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. The first row reports the effects of deployment on separation from the Army 2, 4, 6, and 8 years after arrival. The next reports the effects on voluntary separation, which occurs when a soldier completes his initial enlistment contract but decides not to re-enlist. The third row reports the effects on either being involuntarily separated from the Army for misconduct or being barred from reenlisting. The fourth row reports the effects on being separated from the Army due to a medical disability. Column 5 reports the mean of each outcome 8 years after a soldier's arrival. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.7: Effects on mortality

	(1) 2 yrs	(2) 4 yrs	(3) 6 yrs	(4) 8 yrs	(5) 8 yrs mean
Death (All Causes)	0.50***	0.51***	0.53***	0.53**	1.75
	(0.10)	(0.13)	(0.16)	(0.21)	
Combat Death	0.41***	0.37***	0.42***	0.48***	0.50
	(0.07)	(0.08)	(0.09)	(0.11)	
Noncombat Death	0.09	0.14	0.11	0.05	1.25
	(0.07)	(0.10)	(0.14)	(0.18)	
Death of Despair	-0.02	0.06	0.10	0.002	0.79
	(0.05)	(0.07)	(0.10)	(0.138)	
Suicide	-0.03	-0.02	0.001	-0.02	0.44
	(0.04)	(0.06)	(0.085)	(0.11)	
Drug- or Alcohol-Rel. Death	0.01	0.07	0.08	0.04	0.38
	(0.03)	(0.05)	(0.07)	(0.09)	
Motor Vehicle Death	0.05	0.06	0.03	0.04	0.27
	(0.04)	(0.05)	(0.06)	(0.09)	
Assault Death	0.03	0.0003	0.002	0.01	0.08
	(0.02)	(0.0295)	(0.037)	(0.05)	
Other Noncombat Death	0.03	0.01	-0.01	-0.002	0.15
	(0.02)	(0.04)	(0.048)	(0.07)	
Observations	157,415	157,415	129,176	101,387	101,387

Notes: This table reports 2SLS estimates of the effects of months deployed on all cause mortality and various sub-classifications of mortality. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. The first row reports the effects of deployment on all cause mortality 2, 4, 6, and 8 years after arrival. The next row reports the effects on deaths due to combat. The third row reports the effects on noncombat deaths. The fourth row reports the effects on deaths of despair, which include all suicides (NDI recorded motivation as intentional self-harm or undetermined intent) plus any death caused by drugs, alcohol, or poison, or firearm deaths resulting from undetermined intent. The fifth row reports effects on suicides and the sixth row reports effects on any death determined in the NDI to be caused by drugs or alcohol. The sixth row reports effects on deaths resulting from motor vehicle accidents, the seventh deaths resulting from assaults, and the last noncombat deaths resulting from any other cause not already considered. Column 5 reports the mean of each outcome 8 years after a soldier's arrival. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.8: Misconduct and criminal behavior outcomes

	(1) 2 yrs	(2) 4 yrs	(3) 6 yrs	(4) 8 yrs	(5) 8 yrs mean
	Panel (a): Administrative Sanctions (In Service)				
Ever Demoted	-1.99***	0.14	0.20	-0.04	22.20
	(0.44)	(0.52)	(0.58)	(0.68)	
Separated for Misconduct/Barred	-3.92***	-0.63	-0.52	-1.02	25.05
	(0.38)	(0.53)	(0.60)	(0.70)	
Observations	157,415	157,415	129,176	101,387	101,387
	Panel (b): Criminal Investigations (In Service)				
Ever Violent Felony	-0.31**	-0.14	-0.13	-0.07	2.24
	(0.13)	(0.17)	(0.20)	(0.23)	
Ever Non-Violent Felony	-2.18***	-0.39	-0.39	-0.56	21.10
	(0.43)	(0.51)	(0.57)	(0.66)	
Ever Misdemeanor (Non-traffic)	-2.10***	-1.27***	-1.33***	-1.37**	13.43
	(0.33)	(0.41)	(0.46)	(0.54)	
Ever Other Crime	-0.38*	0.10	0.09	-0.03	5.33
	(0.23)	(0.26)	(0.29)	(0.35)	
Observations	157,415	157,415	129,176	101,387	101,387
	Panel (c): Incarceration (In and Out of Service)				
Ever Incarcerated	-0.09	0.05	0.12	0.10	2.41
	(0.07)	(0.14)	(0.19)	(0.25)	
Observations	156,246	156,246	128,120	100,381	100,381

Notes: This table reports the 2SLS effect (Equation 1) of months deployed on misconduct, criminal behavior, and incarceration. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. Panel (a) examines the effects on demotions and separations for misconduct or while barred from re-enlistment for misconduct. Panel (b) examines criminal cases while serving using military criminal records from the Army Law Enforcement Reporting and Tracking System (ALERTS) system, which include final cases from Military Police (MP) or Criminal Investigation Division (CID) records for the stated crimes. Crime types are grouped into non-violent felonies, violent felonies, misdemeanors, and other (e.g. AWOL). Panel (c) examines effects on incarceration by the stated period (either in military incarceration records or national LexisNexis records). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.9: Financial health outcomes

	(1) Jun 2017	(2) Avg(Y_{2017})	(3) Dec 2020	(4) Avg(Y_{2020})
		Panel (a): Credit Scores		
Vantage Score	0.52	622.1	1.91	655.2
	(1.32)		(1.33)	
FICO Score			0.79	652.1
			(1.49)	
		Panel (b): Debt Composition		
Total debt	3529***	44,407	4793**	83,783
	(1194)		(1862)	
Mortage debt	2759***	27,044	3985**	61,123
	(1059)		(1699)	
Auto debt	489**	11,060	739***	13,064
	(221)		(264)	
Student debt	-139	1,436	-375**	2,478
	(106)		(172)	
		Panel (c): Bad Debt		
Derogatory debt	52.48	1,105.6	-95.06	663.1
	(90.72)		(75.28)	
Debt in colleciton	33.04	983.9	-69.03	1,127.0
	(56.33)		(49.81)	
Any bankruptcy	0.13	1.4	0.35*	1.8
	(0.17)		(0.19)	
Any Foreclosure Action			0.12	12.2
			(0.45)	
Observations	155,898		157,415	

Notes: Panels (a)-(c) report the 2SLS effect (Equation 1) of months deployed on Experian credit outcomes in June 2017 or December 2020. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. In 2017, we drop 1% of our sample that was not sent to Experian. In addition, the sample size for the Vantage and FICO credit score outcomes is smaller, 142,010 and 144,708 for Vantage score in 2017 and 2020 respectively and 135,797 for FICO score 2020, as some individuals have no credit scores (2SLS regressions on an indicator for having any credit score are insignificantly different from 0). The "Any Foreclosure Action" outcome is from LexisNexis records, as of December 2019, and has 156,247 observations. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.10: Education outcomes

Panel (a): Dynamic Outcomes

	2 yrs	4 yrs	6 yrs	8 yrs	8 yrs mean
Enroll Post-arrival	-1.48***	0.66	1.59**	1.09	55.70
	(0.40)	(0.58)	(0.73)	(0.81)	
Assc Deg+ Post-arrival	-0.08	-0.16	0.01	0.66	8.69
	(0.07)	(0.13)	(0.27)	(0.47)	
Bach Deg+ Post-arrival	-0.02	-0.08	-0.12	-0.05	3.99
	(0.06)	(0.09)	(0.16)	(0.33)	
Observations	157,415	157,415	129,176	101,387	101,387

Panel (b): Outcomes by 2017/2020

	Jun 2017	Avg(Y_{2017})	Dec 2020	Avg(Y_{2020})
Enrolled (Post-Arrival)	1.59**	49.98	1.75***	60.01
	(0.66)		(0.67)	
Associates Deg+ (Post-Arrival)	0.21	8.99	0.01	15.62
	(0.42)		(0.52)	
Bachelors Deg+ (Post-Arrival)	0.13	4.66	-0.15	9.18
	(0.31)		(0.42)	
Any post 9/11 GI bill use	2.36***	37.66	3.12***	48.27
	(0.67)		(0.70)	
Observations	157,415		157,415	

Notes: Panel (a) reports the 2SLS effect (Equation 1) of months deployed on education enrollment and attainment outcomes that occur after soldiers arrive at their brigade combat team. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. The first row reports the effects of deployment on any college attendance from the NSC. The second row reports the effects on any degree attainment, while the third reports the effects on any bachelors degree or higher attained post-arrival. Panel (b), instead of looking at outcomes within 2, 4, 6, and 8 years after arrival, looks at any education by 2017 or 2020. In panel (b), we also examine the effect of months deployed on use of the Post-9/11 GI Bill. Post-9/11 GI Bill usage is defined as of September 2017/2020 since we only observe annual snapshots in September each year. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.11: Effects of deployment on in-service outcomes

	(1)	(2)	(3)
	2 yr	3 yrs	4 yrs
Promotion to Sergeant	0.05	2.22***	1.98***
	(0.234)	(0.493)	(0.595)
Mean	2.744	14.765	24.855
Cumulative Army Pay	2880***	3913***	5143***
	(302)	(470)	(685)
Mean	71,561	108,973	139,187
Changed Army Occupation	-0.49***	0.08	-0.67***
	(0.186)	(0.210)	(0.258)
Mean	4.606	5.498	6.960
Ever Separated	-4.96***	-1.87***	2.60***
	(0.465)	(0.646)	(0.685)
Mean	13.475	40.996	56.979
Observations	157,415	157,415	157,415

Notes: This table reports the 2SLS effect (Equation 1) of months deployed on promotion to Sergeant (E5), cumulative military pay, and an indicator for changing army occupations. All outcomes are as of the years since arrival to first duty-station identified in the column headings. Cumulative military pay is through 2, 3, or 4 years, is zero in months when soldiers are no longer in the army, and includes base pay, Basic Allowance for Housing (BAH), Basic Allowance for Subsistence (BAS), hostile fire pay, hardship duty pay, and family separation pay. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.12: Heterogeneity by AFQT, waivers, and race

	(1) Full Sample	(2) Low AFQT	(3) High AFQT	(4) Any Moral Waiver	(5) No Moral Waiver	(6) Black	(7) Hispanic	(8) White
Combat Death	0.48***	0.47***	0.51***	0.60	0.48***	0.05	0.76*	0.50***
	(0.11)	(0.18)	(0.15)	(0.37)	(0.12)	(0.21)	(0.43)	(0.14)
Combat Death Mean	0.50	0.49	0.51	0.51	0.50	0.26	0.61	0.53
Ever Combat Injury	4.43***	4.62***	4.38***	4.01***	4.61***	1.92**	3.88***	5.09***
	(0.31)	(0.53)	(0.39)	(1.01)	(0.33)	(0.76)	(0.98)	(0.39)
Ever Combat Injury Mean	4.17	4.25	4.17	4.43	4.17	2.30	4.38	4.55
Significant Army Profile	2.62***	2.47**	2.78***	5.30***	2.32***	1.53	4.66**	2.93***
	(0.59)	(1.00)	(0.75)	(1.87)	(0.62)	(1.81)	(1.81)	(0.72)
Significant Army Profile Mean	15.04	15.23	14.95	16.79	14.78	13.30	12.63	15.89
Annual Amt VADC	2602.30***	3337.68***	2242.16***	2743.50***	2586.83***	3213.15***	2649.04***	2578.21***
	(171.73)	(308.46)	(212.51)	(558.69)	(182.24)	(617.13)	(584.36)	(202.37)
Annual Amt VADC Mean	6,129.44	6,515.79	5,910.34	6,953.52	6,012.65	6,497.45	6,331.53	6,096.78
Any VADC Receipt	9.42***	11.80***	8.07***	9.34***	9.35***	8.91***	11.95***	9.52***
	(0.80)	(1.37)	(1.01)	(2.42)	(0.85)	(2.60)	(2.56)	(0.96)
Any VADC Receipt Mean	37.37	38.08	36.99	39.98	37.02	35.59	36.63	38.11
Noncombat Death	0.05	-0.34	0.25	0.70	-0.08	-0.94*	0.14	0.12
	(0.18)	(0.30)	(0.24)	(0.71)	(0.18)	(0.56)	(0.47)	(0.22)
Noncombat Death Mean	1.25	1.15	1.31	2.11	1.12	1.06	0.86	1.36
Death of Despair	0.00	-0.27	0.16	0.39	-0.07	-0.35	-0.15	0.09
	(0.14)	(0.23)	(0.18)	(0.58)	(0.14)	(0.33)	(0.37)	(0.18)
Death of Despair Mean	0.79	0.71	0.83	1.41	0.69	0.45	0.53	0.89
Observations	101,387	62,968	37,209	12,584	87,653	10,951	10,465	72,111

Notes: This table presents results for the specification in Table 3 for different sample splits: above and below 50 AQFT, any vs. no moral waiver, and for Black, Hispanic, and white enlistees. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.13: Measuring Deployment over different time horizons

	3 year	4 year	5 year	6 year	7 year	8 year
			VADC & Trauma of War			
Combat Death	0.48***	0.53***	0.55***	0.56***	0.57***	0.57***
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Combat Injury	4.43***	4.40***	4.67***	4.57***	4.56***	4.56***
	(0.31)	(0.29)	(0.30)	(0.31)	(0.31)	(0.31)
Significant Army Profile	2.62***	2.68***	2.89***	2.88***	2.91***	2.89***
	(0.59)	(0.52)	(0.55)	(0.55)	(0.55)	(0.55)
Annual Amt VADC	2602***	2311***	2450***	2435***	2409***	2420***
	(172)	(153)	(162)	(163)	(165)	(166)
Any VADC Receipt	9.42***	8.55***	8.89***	8.94***	8.85***	8.91***
	(0.80)	(0.70)	(0.75)	(0.75)	(0.76)	(0.76)
			Noncombat Mortality			
Noncombat Death	0.05	-0.03	-0.02	-0.03	-0.03	-0.03
	(0.18)	(0.16)	(0.17)	(0.17)	(0.17)	(0.17)
Death of Despair	0.00	-0.02	-0.02	-0.02	-0.02	-0.02
	(0.14)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)
Suicide	-0.02	-0.06	-0.07	-0.07	-0.07	-0.07
	(0.11)	(0.09)	(0.10)	(0.10)	(0.10)	(0.10)
Drug- or Alcohol-Rel. Death	0.04	0.04	0.05	0.06	0.06	0.06
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Motor Vehicle Death	0.04	0.02	0.02	0.01	0.01	0.01
	(0.09)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
			Misconduct & Education			
Separated for Misconduct	-1.02	-0.01	-0.09	-0.22	-0.36	-0.41
	(0.70)	(0.62)	(0.66)	(0.65)	(0.66)	(0.66)
Ever Incarcerated	0.10	0.11	0.13	0.09	0.07	0.06
	(0.25)	(0.22)	(0.23)	(0.23)	(0.23)	(0.23)
Credit Score in 2020 (Vantage)	1.32	2.08	1.83	2.09	2.23	2.33
	(1.58)	(1.38)	(1.47)	(1.46)	(1.47)	(1.47)
College Enrollment	1.09	0.97	1.08	1.17	1.14	1.20
	(0.81)	(0.71)	(0.76)	(0.76)	(0.76)	(0.76)
Associate's Deg+	0.66	0.59	0.61	0.59	0.62	0.64
	(0.47)	(0.40)	(0.43)	(0.43)	(0.43)	(0.43)
Observations	101,387	101,387	101,387	101,387	101,387	101,387

Notes: This table changes both the endogenous variable and the instrument to be months deployed (or peer months deployed) within 3 years, 4 years, ..., up to 8 years after arrival. All outcomes remain measured as of 8 years after arrival. Observations for incarceration are 100,381 and for credit 93,252. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.14: Effects on disability, mortality, and education (including women and men)

	(1) 2 yrs	(2) 4 yrs	(3) 6 yrs	(4) 8 yrs	(5) 8 yrs mean
			Panel (a): VADC and Trauma of War		
Combat Death	0.37***	0.35***	0.39***	0.45***	0.45
	(0.06)	(0.07)	(0.08)	(0.10)	
Ever Combat Injury	3.29***	3.62***	3.88***	4.28***	3.81
	(0.18)	(0.21)	(0.24)	(0.29)	
Significant Army Profile	-0.58**	2.01***	2.36***	2.26***	15.40
	(0.24)	(0.39)	(0.48)	(0.57)	
Annual Amt VADC	0.07	723.07***	2069.91***	2493.12***	6,195.35
	(10.65)	(67.34)	(117.99)	(167.06)	
Any VADC Receipt	-0.07	3.24***	9.14***	9.11***	37.73
	(0.09)	(0.41)	(0.63)	(0.77)	
			Panel (b): Noncombat Mortality Outcomes		
Noncombat Death	0.08	0.11	0.07	0.01	1.19
	(0.07)	(0.10)	(0.13)	(0.17)	
Death of Despair	-0.02	0.05	0.07	0.01	0.74
	(0.05)	(0.07)	(0.10)	(0.13)	
Suicide	-0.03	-0.02	-0.02	-0.04	0.41
	(0.03)	(0.06)	(0.08)	(0.10)	
Drug- or Alcohol-Rel. Death	0.01	0.06	0.07	0.06	0.36
	(0.03)	(0.04)	(0.06)	(0.09)	
Motor Vehicle Death	0.04	0.04	0.01	0.02	0.26
	(0.03)	(0.05)	(0.06)	(0.08)	
			Panel (c): Misconduct, Credit, and Education		
Separated for Misconduct/Barred	-3.71***	-0.52	-0.36	-0.80	24.04
	(0.36)	(0.51)	(0.58)	(0.67)	
Ever Incarcerated	-0.11*	0.01	0.05	0.04	2.27
	(0.06)	(0.13)	(0.18)	(0.24)	
Credit Score in 2020 (Vantage)	1.83	1.83	2.08	1.32	655.12
	(1.30)	(1.30)	(1.38)	(1.53)	
Enrolled by 2020 (Post-Arrival)	1.67***	1.67***	1.67**	1.32*	67.26
	(0.64)	(0.64)	(0.67)	(0.74)	
Assc Deg+ by 2020 (Post-Arrival)	-0.13	-0.13	-0.13	-0.24	21.04
	(0.52)	(0.52)	(0.57)	(0.65)	
Observations	172,479	172,479	141,599	111,413	111,413

Notes: This table reports the 2SLS effect (Equation 1) of months deployed on our primary outcomes in a sample that includes women and men assigned to BCTs between 2005 and 2015. See the notes for Table 3 for additional details. The sample sizes are smaller for incarceration (110,304 at 8 years) and for Vantage credit score 2020 (100,483 at 8 years). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.15: Effects of deployment using quarter of arrival FE

	(1) 2 yrs	(2) 4 yrs	(3) 6 yrs	(4) 8 yrs	(5) 8 yrs mean
			Panel (a): VADC and Trauma of War		
Combat Death	0.44***	0.40***	0.49***	0.56***	0.51
	(0.09)	(0.10)	(0.12)	(0.16)	
Ever Combat Injury	3.58***	3.99***	4.40***	5.11***	4.25
	(0.23)	(0.28)	(0.33)	(0.42)	
Significant Army Profile	-0.57*	1.77***	2.66***	2.78***	14.95
	(0.31)	(0.51)	(0.63)	(0.79)	
Annual Amt VADC	0.96	691.20***	2212.45***	2753.69***	6,127.57
	(14.95)	(89.95)	(158.80)	(230.75)	
Any VADC Receipt	-0.15	3.31***	9.84***	9.22***	37.42
	(0.12)	(0.54)	(0.85)	(1.08)	
			Panel (b): Noncombat Mortality Outcomes		
Noncombat Death	0.11	0.16	0.15	0.01	1.25
	(0.10)	(0.13)	(0.18)	(0.25)	
Death of Despair	0.05	0.14	0.22	0.19	0.79
	(0.07)	(0.10)	(0.14)	(0.20)	
Suicide	-0.01	0.02	0.07	0.03	0.44
	(0.06)	(0.08)	(0.12)	(0.16)	
Drug- or Alcohol-Rel. Death	0.06	0.11*	0.11	0.15	0.37
	(0.05)	(0.06)	(0.09)	(0.13)	
Motor Vehicle Death	-0.00	-0.04	-0.10	-0.12	0.27
	(0.05)	(0.07)	(0.08)	(0.12)	
			Panel (c): Misconduct and Education		
Separated for Misconduct/Barred	-3.37***	-0.44	0.07	-0.28	25.07
	(0.49)	(0.69)	(0.79)	(0.95)	
Ever Incarcerated	-0.08	0.06	0.36	0.25	2.41
	(0.09)	(0.18)	(0.26)	(0.35)	
Enroll Post-arrival	-1.36***	0.83	2.23**	0.79	55.49
	(0.51)	(0.74)	(0.96)	(1.10)	
Assc Deg+ Post-arrival	-0.03	-0.21	-0.27	0.44	8.64
	(0.10)	(0.17)	(0.36)	(0.64)	
Observations	157,415	157,415	129,176	101,387	101,387

Notes: This table reports the 2SLS effect (Equation 1) of months deployed on our primary outcomes in different years after arrival. Unlike our main specification, the results in this table are from a regression that replaces year of arrival by term, by occupation, by duty-station with quarter of arrival by term, by occupation, by duty-station fixed effects. In addition, the sample sizes are smaller for incarceration (100,381 at 8 years) and for Vantage credit score in 2020 (93,252 at 8years). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.16: Nonlinear effects

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel (a): Any VADC Receipt			
Months Deployed	0.942***	1.205***	0.567	0.804***	0.588	-5.929*
	(0.080)	(0.271)	(0.671)	(0.112)	(0.617)	(3.368)
Months Dep. Sq.		-0.014	0.072		0.009	0.618**
		(0.014)	(0.083)		(0.026)	(0.300)
Months Dep. Cu.			-0.003			-0.017**
			(0.003)			(0.008)
Deployed				3.391*	4.506	25.095**
				(1.991)	(3.752)	(11.578)
Effect of 0 to 10 Month Dep	9.417***	10.606***	9.909***	11.435***	11.326***	10.567***
	(0.796)	(1.408)	(1.578)	(1.442)	(1.462)	(1.510)
			Panel (b): VADC Amount			
Months Deployed	260.230***	341.235***	271.285*	220.170***	171.340	-1043.736
	(17.173)	(57.099)	(141.405)	(24.040)	(132.458)	(734.571)
Months Dep. Sq.		-4.440	5.011		2.117	115.581*
		(3.052)	(17.880)		(5.722)	(65.686)
Months Dep. Cu.			-0.322			-3.176*
			(0.603)			(1.781)
Deployed				989.393**	1241.337	5079.978**
				(417.094)	(800.357)	(2528.460)
Effect of 0 to 10 Month Dep	2602***	2968***	2892***	3191***	3166***	3025***
	(172)	(294)	(324)	(302)	(305)	(311)
			Panel (c): Noncombat Deaths			
Months Deployed	0.005	-0.052	0.138	0.020	-0.046	1.022
	(0.018)	(0.061)	(0.151)	(0.027)	(0.150)	(0.759)
Months Dep. Sq.		0.003	-0.023		0.003	-0.097
		(0.004)	(0.020)		(0.007)	(0.068)
Months Dep. Cu.			0.0009			0.003
			(0.0007)			(0.002)
Deployed				-0.384	-0.042	-3.417
				(0.440)	(0.886)	(2.607)
Effect of 0 to 10 Month Dep.	0.045	-0.210	-0.003	-0.183	-0.217	-0.093
	(0.180)	(0.295)	(0.315)	(0.296)	(0.298)	(0.302)
Observations	101,387	101,387	101,387	101,387	101,387	101,387

Notes: This table presents estimates of deployment on VADC receipt and noncombat deaths allowing for nonlinear effects of months deployed and peer casualties. In each case, equations are just identified and use the peer analogue (e.g. peer months deployed squared) as the corresponding instrument. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.17: The effects of any deployment

	(1) Any Dep.	(2) Baseline (10 Months Dep.)
	Panel (a): Trauma and Disability	
Combat Death	0.36***	0.48***
	(0.13)	(0.11)
Combat Injury	4.01***	4.43***
	(0.39)	(0.31)
Sig. Army Profile	2.81***	2.62***
	(1.01)	(0.59)
Amt VADC	3739***	2602***
	(299)	(172)
Any VADC	13.44***	9.42***
	(1.42)	(0.80)
	Panel (b): Noncombat Mortality	
Noncombat Death	-0.13	0.05
	(0.29)	(0.18)
Death of Despair	0.01	0.00
	(0.21)	(0.14)
Suicide	-0.15	-0.02
	(0.18)	(0.11)
Drug or Alcohol-Rel Death	0.10	0.04
	(0.12)	(0.09)
Motor Vehicle Death	-0.02	0.04
	(0.15)	(0.09)
	Panel (c): Misconduct, Credit, and Education	
Separated for Misconduct	0.17	-1.02
	(1.24)	(0.70)
Ever Incarcerated	0.33	0.10
	(0.46)	(0.25)
Credit Score	3.65	1.32
	(2.81)	(1.58)
College Enrollment	1.66	1.09
	(1.44)	(0.81)
Associates Deg+	0.93	0.66
	(0.91)	(0.47)
Observations	101,387	101,387

Notes: This table reports estimates that change the endogenous variable to be "any deployment" within 3 years of arrival and the instrument to be average peer rates of "any deployment" within 3 years of arrival. Our baseline specification where the endogenous variable is months deployed and the instrument is peers months deployed is reported in column (2) for ease of comparison. The sample size for incarceration and credit outcomes are smaller: 100,380 for incarceration and 93,252 for Vantage credit score in 2020. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.18: Alternative estimators: Covariate Balance

	(1) Baseline	(2) UJIVE	(3) LIML	(4) 2SLS
		(using BCT×quarter dummies)		
Black	0.57	0.64	0.58	0.50
	(0.45)	(0.45)	(0.44)	(0.44)
Hispanic	-0.40	-0.50	-0.47	-0.39
	(0.47)	(0.47)	(0.46)	(0.46)
Other Race	0.02	-0.01	0.00	0.03
	(0.31)	(0.31)	(0.30)	(0.30)
Married	0.13	0.13	0.20	0.11
	(0.49)	(0.50)	(0.48)	(0.48)
Num. Dependents	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
HS Graduate +	0.50	0.52	0.42	0.66
	(0.44)	(0.44)	(0.42)	(0.42)
Age	0.09*	0.08	0.10**	0.08
	(0.05)	(0.05)	(0.05)	(0.05)
AFQT	-0.29	-0.29	-0.27	-0.22
	(0.23)	(0.23)	(0.22)	(0.22)
Obs.	157,415	157,415	157,415	157,415

Notes: This table presents balance tests using alternative estimators. The first column is similar to Table 2, but reports the effect of own months deployed on the outcome using peer months deployed as the instrument (instead of simply reporting the reduced form effect of the instrument as in Table 2). This is done for comparability with subsequent columns. The second column uses a UJIVE estimator, the third column uses LIML, and the fourth column uses 2SLS. In each of columns 2-4, we use BCT-by-quarter of enlistment dummies as instruments. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.19: Alternative estimators: Effects on Health, Education, & Crime

	8 Years Out			
	Baseline	UJIVE	LIML	2SLS
		(using BCT×quarter dummies)		
		VADC & Trauma of War		
Combat Death	0.48***	0.50***	0.51***	0.44***
	(0.11)	(0.11)	(0.11)	(0.11)
Ever Combat Injury	4.43***	4.30***	4.38***	4.35***
	(0.31)	(0.30)	(0.30)	(0.30)
Significant Army Profile	2.62***	2.47***	2.52***	2.31***
	(0.59)	(0.59)	(0.57)	(0.57)
Annual Amt VADC	2602***	2537***	2565***	2512***
	(172)	(172)	(166)	(166)
Any VADC Receipt	9.42***	9.36***	9.36***	9.23***
	(0.80)	(0.80)	(0.77)	(0.77)
		Noncombat Mortality		
Noncombat Death	0.05	0.002	0.003	-0.04
	(0.18)	(0.179)	(0.173)	(0.17)
Death of Despair	0.002	-0.04	-0.03	-0.05
	(0.138)	(0.14)	(0.13)	(0.13)
Suicide	-0.02	-0.03	-0.03	-0.05
	(0.11)	(0.10)	(0.10)	(0.10)
Drug- or Alcohol-Rel. Death	0.04	0.01	0.03	0.01
	(0.09)	(0.09)	(0.09)	(0.09)
Motor Vehicle Death	0.04	0.05	0.03	0.02
	(0.09)	(0.09)	(0.08)	(0.08)
		Misconduct & Education		
Separated for Misconduct	-1.02	-1.19*	-0.82	-2.05***
	(0.70)	(0.71)	(0.69)	(0.68)
Ever Incarcerated	0.10	0.16	0.11	0.01
	(0.25)	(0.25)	(0.24)	(0.24)
Credit Score in 2020 (Vantage)	1.32	0.96	1.22	2.50*
	(1.58)	(1.56)	(1.51)	(1.51)
Enroll in College	1.09	1.17	1.11	1.42*
	(0.81)	(0.82)	(0.79)	(0.79)
Assc Deg+	0.66	0.75	0.70	0.73
	(0.47)	(0.47)	(0.46)	(0.46)
Obs.	101,387	101,387	101,387	101,387

Notes: This table presents estimated effects on outcomes using alternative estimators. The first column repeats our baseline specification and results. The second column uses a UJIVE estimator, the third column uses LIML, and the fourth column uses 2SLS. In each of columns 2-4, we use BCT-by-quarter of enlistment dummies as instruments. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.20: Covariate balance for peer casualties

	(1) Dep. Instrument $\times 10$	(2) (Dep. Inst. $\times 10$) $\times$ (1σ Peer Cas)
Black	0.47	0.05
	(0.47)	(0.11)
Hispanic	-0.64	0.17
	(0.49)	(0.12)
Other Race	0.13	-0.08
	(0.32)	(0.08)
Married	-0.01	0.10
	(0.51)	(0.13)
Num. Dependents	0.01	0.002
	(0.01)	(0.003)
HS Graduate +	0.55	-0.05
	(0.45)	(0.15)
Age	0.07	0.01
	(0.05)	(0.01)
AFQT	-0.28	-0.003
	(0.24)	(0.064)
Observations	157,415	
P-value on (Dep Inst) = 0	0.362	
P-value on (Dep Inst x Peer Cas) = 0	0.847	
P-value on (Dep Inst) = (Dep Inst x Peer Cas) = 0	0.590	

Notes: This table reports covariate balance regressions relating to the peer casualty specification discussed in Section 4. Each row reports the coefficients from a separate regression of the stated covariate on peer months deployed within 3 years (our "Dep. Instrument") and the interaction of peer months deployed with peer casualty rates. Peer casualties are the share of peer soldiers (those who arrive in the same BCT within the same quarter) who suffer non-fatal casualties (i.e. combat injuries, or being "Wounded In Action") or fatal casualties (i.e. combat deaths) within three years. All regressions include duty-station by job by initial assignment year by term-length fixed effects. For ease of interpretation, coefficients and standard errors in column (1) are scaled by 10. Coefficients and standard errors in column (2) are scaled by 10σ, where σ is the sample standard deviation of peer casualties. We fail to reject the null hypothesis that all coefficients for both the instrument and the interaction of the instrument and peer casualties are jointly zero. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.21: Effects of violent deployments on disability diagnoses

	(1) 10 Months Deployed	(2) (10 Mths Dep) × (1σ Peer Cas)	(3) Mean
	Panel (a): Top 5 Conditions		
Tinnitus	3.93***	1.40***	20.63
	(0.71)	(0.18)	
Limitation of flexion, knee	1.35**	0.31**	10.77
	(0.56)	(0.13)	
PTSD	9.87***	1.69***	16.71
	(0.65)	(0.18)	
Lumbosacral or cervical strain	1.54**	0.63***	14.07
	(0.62)	(0.15)	
Limitation of motion of the ankle	0.84**	0.12	5.78
	(0.42)	(0.10)	
	Panel (b): Additional Outcomes		
Serious or Very Serious Combat Injury	0.03	0.19***	0.43
	(0.10)	(0.03)	
Very Serious Combat Injury	0.01	0.04**	0.10
	(0.05)	(0.01)	
Any VADC w/ Amputation	0.01	0.11***	0.24
	(0.08)	(0.03)	
Any VADC IU or CDR 100	1.99***	0.45***	4.12
	(0.36)	(0.09)	
Any SSDI or SSI	1.69***	0.52***	3.39
	(0.33)	(0.09)	
Annual Amt SSI/SSDI	262.56***	93.44***	569.37
	(56.93)	(16.83)	
Observations	101,387		

Notes: This table reports 2SLS estimates of Equation 6 (with corresponding first stage equations 7 and 8) on the outcomes in Table A.5. As described in Section 4, we augment our baseline model to include an interaction between months deployed and peer casualty rates, which proxy for more dangerous deployments. Peer casualties are the share of peer soldiers (those who arrive in the same BCT within the same quarter) who suffer non-fatal combat injuries or fatal combat deaths within three years. Column (1) reports $\hat{\beta}$ while Column (2) reports $\hat{\gamma}$. Each row represents a separate regression on a separate outcome. Coefficients and standard errors in column (1) are scaled by 10. Coefficients and standard errors in column (2) are scaled by 10σ, where σ is the sample standard deviation of peer casualties. Estimates in column (1) can therefore be interpreted as the effect of a ten month deployment with zero peer casualties while estimates in column (2) can be interpreted as the additional effect of a standard deviation increase in peer casualties during a ten month deployment. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.22: Effects of deployment by occupation type as of 8 years

	(1)	(2)	(3)	(4)	(5)
	Combat Occupations		Noncombat Occupations		
	Mean	10 Months Deployed	Mean	10 Months Deployed	P-value of difference
		Panel (a): Trauma of War and Disability			
Combat Death	0.68	0.65***	0.18	0.17	0.0214
		(0.15)		(0.15)	
Ever Combat Injury	5.62	6.05***	1.64	1.50***	0.0000
		(0.44)		(0.31)	
Significant Army Profile	15.46	3.27***	14.32	1.46	0.1336
		(0.73)		(0.97)	
Annual Amt VADC	6,202.92	3033.76***	6,001.09	1821.98***	0.0007
		(210.51)		(295.91)	
Any VADC Receipt	38.24	11.16***	35.85	6.26***	0.0029
		(0.99)		(1.34)	
		Panel (b): Noncombat Mortality Outcomes			
Noncombat Death	1.41	0.02	0.97	0.09	0.8380
		(0.23)		(0.30)	
Death of Despair	0.92	-0.05	0.56	0.10	0.5607
		(0.18)		(0.20)	
Suicide	0.51	-0.04	0.31	0.00	0.8561
		(0.14)		(0.16)	
Drug- or Alcohol-Rel. Death	0.44	-0.03	0.27	0.17	0.2914
		(0.12)		(0.14)	
Motor Vehicle Death	0.29	0.04	0.23	0.03	0.9346
		(0.10)		(0.16)	
		Panel (c): Misconduct, Credit, and Education			
Separated for Misconduct/Barred	24.93	-1.58*	25.27	-0.02	0.2858
		(0.87)		(1.19)	
Ever Incarcerated	2.45	0.18	2.36	-0.06	0.6445
		(0.30)		(0.44)	
Credit Score in 2020 (Vantage)	657.49	0.40	652.78	3.00	0.4264
		(1.95)		(2.67)	
College Enrollment	53.44	1.87*	59.64	-0.32	0.1898
		(1.02)		(1.34)	
Assc Deg+	7.80	0.90	10.25	0.23	0.5112
		(0.56)		(0.86)	
Observations		64,473		36,914	

Notes: This table reports estimates of Equation 1 on our primary outcomes for samples split by combat and noncombat occupation. All outcomes are as of 8 years after a soldier arrives at his initial operational assignment except credit, which is as of 2020. Each row represents a separate regression on a separate outcome. Columns (1) and (3) report the mean outcome for combat and noncombat occupations. Columns (2) and (4) report the estimates from β on the regressions restricted to either combat or noncombat occupations. Column (5) reports the p-value on combat occupation interacted with 10 months deployed from a fully interacted model. All coefficients and standard errors are scaled by 10 so that estimates of β can be interpreted as the effect of a ten-month deployment. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.23: Estimated variance of direct BCT effects on outcomes

	All BCT ×quarter		BCT×quarter with $n > 100$	
	Variance	S.D.	Variance	S.D.
	(1)	(2)	(3)	(4)
Months deployed	12.39	3.52	12.68	3.56
	(0.22)	(0.03)	(0.23)	(0.03)
Any VADC	33.91	5.82	26.96	5.19
	(7.23)	(0.62)	(7.15)	(0.69)
VADC Amt.	3,111,615	1,764	2,778,300	1,667
	(636,957)	(181)	(533,535)	(160)
Noncombat death	-0.02	-	0.21	0.46
	(0.35)		(0.30)	(0.33)

Notes: This table presents estimates of the variance and standard deviation of direct effects of BCT assignments on the stated outcome. Each estimate is constructed by regressing outcomes on indicators for assignment to each BCT by quarter of arrival controlling for duty-station by job by year of arrival by term-length fixed effects, which are the same controls used in the baseline analysis. Letting $\hat{\beta}_b$ denote the coefficient on BCT-by-qtr indicator and s_b its associated standard error, the variance estimated as $\frac{1}{B}\sum_{b=1}^{B}(\beta_b - \bar{\hat{\beta}})^2 - s_b^2$, where B is the total number of indicators and $\bar{\hat{\beta}}$ is their average. The standard errors shown in parentheses for each estimate are computed as $\frac{1}{B^2}\sum_{b=1}^{B} 2s_b^4 + 4s_b^2\left((\beta_b - \bar{\hat{\beta}})^2 - s_b^2\right)$. Standard errors for the standard deviation estimates are computed by delta method. Outcomes are measured 8 years after arrival, with the exception of months deployed which is measured within 3 years of arrival for consistency with our main specifications.

Table A.24: Effects of deployment and peer casualties using alternative specifications

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a) - VADC and Trauma of War									
		$Y_i = \delta_{k(i)} + \beta D_i + \gamma(D_i \times W_i) + \epsilon_i$		$Y_i = \delta_{k(i)} + \beta D_i + \lambda W_i + \epsilon_i$		$Y_i = \delta_{k(i)} + \beta D_i + \lambda W_i + \gamma(D_i \times W_i) + \epsilon_i$			
	Mean	10 Months Deployed	**(10 Mths Dep) x (1σ Peer Cas)**	10 Months Deployed	**1σ Peer Casualties**	10 Months Deployed	1σ Peer Casualties	(10 Mths Dep) x (1σ Peer Cas)	$\frac{\partial Y_i}{\partial W_i}\|(\mathbf{D_i = 10})$
Combat Death	0.50	0.01	0.27***	0.17	0.28***	-0.01	-0.04	0.30**	0.27***
		(0.12)	(0.04)	(0.12)	(0.04)	(0.12)	(0.14)	(0.14)	(0.04)
Ever Combat Injury	4.17	1.16***	1.86***	2.15***	2.09***	1.87***	1.60***	0.47	2.06***
		(0.33)	(0.11)	(0.32)	(0.12)	(0.34)	(0.40)	(0.38)	(0.12)
Significant Army Profile	15.04	0.84	1.02***	1.35**	1.17***	1.38**	1.24*	-0.06	1.17***
		(0.64)	(0.16)	(0.61)	(0.18)	(0.69)	(0.64)	(0.59)	(0.18)
Annual Amt VADC	6,129	1876***	414***	2056***	500***	2245***	835***	-316*	519***
		(190)	(48)	(181)	(53)	(204)	(189)	(170)	(54)
Any VADC Receipt	37.37	7.16***	1.29***	7.67***	1.60***	8.56***	3.17***	-1.49*	1.69***
		(0.88)	(0.21)	(0.84)	(0.23)	(0.95)	(0.86)	(0.78)	(0.24)
Panel (b) - Noncombat Mortality Outcomes									
		$Y_i = \delta_{k(i)} + \beta D_i + \gamma(D_i \times W_i) + \epsilon_i$		$Y_i = \delta_{k(i)} + \beta D_i + \lambda W_i + \epsilon_i$		$Y_i = \delta_{k(i)} + \beta D_i + \lambda W_i + \gamma(D_i \times W_i) + \epsilon_i$			
	Mean	10 Months Deployed	**(10 Mths Dep) x (1σ Peer Cas)**	10 Months Deployed	**1σ Peer Casualties**	10 Months Deployed	1σ Peer Casualties	(10 Mths Dep) x (1σ Peer Cas)	$\frac{\partial Y_i}{\partial W_i}\|(\mathbf{D_i = 10})$
Noncombat Death	1.25	0.07	-0.01	0.05	-0.01	0.13	0.13	-0.12	0.00
		(0.20)	(0.05)	(0.19)	(0.06)	(0.22)	(0.20)	(0.19)	(0.06)
Death of Despair	0.79	0.10	-0.06	0.06	-0.06	0.14	0.08	-0.13	-0.05
		(0.16)	(0.04)	(0.15)	(0.05)	(0.17)	(0.16)	(0.15)	(0.05)
Suicide	0.44	0.02	-0.02	0.01	-0.03	-0.02	-0.10	0.06	-0.04
		(0.12)	(0.03)	(0.11)	(0.03)	(0.13)	(0.11)	(0.10)	(0.03)
Drug- or Alcohol-Rel. Death	0.38	0.14	-0.06*	0.10	-0.05	0.19*	0.11	-0.15	-0.04
		(0.11)	(0.03)	(0.10)	(0.03)	(0.11)	(0.12)	(0.11)	(0.03)
Motor Vehicle Death	0.27	0.04	-0.00	0.04	0.00	0.07	0.05	-0.04	0.00
		(0.09)	(0.02)	(0.09)	(0.02)	(0.10)	(0.10)	(0.09)	(0.02)
Panel (c) - Misconduct, Credit, and Education									
		$Y_i = \delta_{k(i)} + \beta D_i + \gamma(D_i \times W_i) + \epsilon_i$		$Y_i = \delta_{k(i)} + \beta D_i + \lambda W_i + \epsilon_i$		$Y_i = \delta_{k(i)} + \beta D_i + \lambda W_i + \gamma(D_i \times W_i) + \epsilon_i$			
	Mean	10 Months Deployed	**(10 Mths Dep) x (1σ Peer Cas)**	10 Months Deployed	**1σ Peer Casualties**	10 Months Deployed	1σ Peer Casualties	(10 Mths Dep) x (1σ Peer Cas)	$\frac{\partial Y_i}{\partial W_i}\|(\mathbf{D_i = 10})$
Separated for Misconduct	25.05	-0.90	-0.07	-1.02	-0.00	-0.49	0.93	-0.88	0.05
		(0.77)	(0.18)	(0.74)	(0.20)	(0.83)	(0.74)	(0.66)	(0.21)
Ever Incarcerated	2.41	-0.07	0.10	-0.06	0.14*	0.14	0.48*	-0.32	0.16**
		(0.28)	(0.07)	(0.26)	(0.07)	(0.30)	(0.28)	(0.25)	(0.07)
Enroll in College	55.70	1.13	-0.03	1.18	-0.08	0.82	-0.72	0.60	-0.12
		(0.89)	(0.21)	(0.85)	(0.23)	(0.97)	(0.87)	(0.79)	(0.24)
Assc Deg+	8.69	0.41	0.14	0.50	0.15	0.38	-0.06	0.20	0.14
		(0.52)	(0.12)	(0.49)	(0.13)	(0.58)	(0.51)	(0.46)	(0.14)
Credit Score in 2020 (Vantage)	655.78	1.34	-0.01	1.29	0.03	1.56	0.51	-0.45	0.06
		(1.74)	(0.41)	(1.66)	(0.46)	(1.88)	(1.69)	(1.51)	(0.47)
Observations		101,387		101,387		101,387			

Notes: Columns (1) and (2) of this table report 2SLS estimates of Equation 6 (with corresponding first stage Equations 7 and 8) on our primary outcomes as of 8 years after a soldier arrives at his initial operational assignment, except credit outcomes which are as of 2020. These are our baseline estimates reported in Table 5. Columns (3) and (4) report corresponding estimates that replace D_i and $(D_i \times W_i)$ with separate terms for D_i and W_i, as indicated by the equation below the panel headings. Columns (5)-(7) report estimates with D_i, W_i, and $(D_i \times W_i)$ (also indicated by the equation below the panel headings). Column (8) reports the marginal effect of W_i when $D_i = 10$ from the model with D_i, W_i, and $(D_i \times W_i)$ to have an estimate that is comparable to that of the other models. The samples sizes for credit and incarceration include 93,252 and 100,381 individuals, respectively. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.25: The effects of more violent deployments using a less parametric specification

	Panel (a) - VADC and Trauma of War				
	(1)	(2)	(3)	(4)	(5)
	$D_i \times 1\{W_i = 0\}$	$D_i \times 1\{0 < W_i \leq 4\}$	$D_i \times 1\{4 < W_i \leq 8\}$	$D_i \times 1\{W_i > 8\}$	Mean
Combat Death	-0.04	0.16	0.41***	0.87***	0.50
	(0.12)	(0.12)	(0.12)	(0.15)	
Ever Combat Injury	0.59*	2.07***	4.42***	6.72***	4.17
	(0.33)	(0.32)	(0.35)	(0.38)	
Significant Army Profile	1.08	1.45**	2.43***	3.87***	15.04
	(0.71)	(0.63)	(0.62)	(0.65)	
Annual Amt VADC	1833.86***	2089.29***	2677.34***	3003.32***	6,129.44
	(213.05)	(187.45)	(182.51)	(187.79)	
Any VADC Receipt	6.59***	7.66***	9.78***	10.73***	37.37
	(0.97)	(0.87)	(0.84)	(0.87)	
	Panel (b) - Noncombat Mortaility Outcomes				
	(1)	(2)	(3)	(4)	(5)
	$D_i \times 1\{W_i = 0\}$	$D_i \times 1\{0 < W_i \leq 4\}$	$D_i \times 1\{4 < W_i \leq 8\}$	$D_i \times 1\{W_i > 8\}$	Mean
Noncombat Death	0.08	0.05	0.04	0.04	1.25
	(0.22)	(0.19)	(0.20)	(0.20)	
Death of Despair	0.12	0.07	0.01	-0.07	0.79
	(0.18)	(0.15)	(0.15)	(0.15)	
Suicide	0.07	0.03	-0.07	-0.03	0.44
	(0.14)	(0.12)	(0.11)	(0.11)	
Drug- or Alcohol-Rel. Death	0.10	0.09	0.09	-0.05	0.38
	(0.12)	(0.10)	(0.10)	(0.10)	
Motor Vehicle Death	-0.02	0.03	0.04	0.06	0.27
	(0.10)	(0.09)	(0.09)	(0.10)	
	Panel (c) - Misconduct, Credit, and Education				
	(1)	(2)	(3)	(4)	(5)
	$D_i \times 1\{W_i = 0\}$	$D_i \times 1\{0 < W_i \leq 4\}$	$D_i \times 1\{4 < W_i \leq 8\}$	$D_i \times 1\{W_i > 8\}$	Mean
Separated for Misconduct	-1.31	-0.81	-1.01	-1.09	25.05
	(0.87)	(0.77)	(0.74)	(0.76)	
Enroll in College	1.65*	0.89	1.34	0.80	55.70
	(0.99)	(0.88)	(0.86)	(0.89)	
Assc Deg+	-0.11	0.62	0.83*	0.69	8.69
	(0.58)	(0.52)	(0.50)	(0.51)	
Ever Incarcerated	-0.12	0.00	0.17	0.13	2.41
	(0.31)	(0.27)	(0.27)	(0.27)	
Credit Score in 2020 (Vantage)	1.11	0.66	2.62	0.39	655.78
	(1.92)	(1.72)	(1.67)	(1.71)	

Notes: This table reports 2SLS estimates of $Y_i = \delta_{k(i)} + \gamma_1\big(D_i \times 1(W_i = 0)\big) + \gamma_2\big(D_i \times 1(0 < W_i \leq 4)\big) + \gamma_3\big(D_i \times 1(4 < W_i \leq 8)\big) + \gamma_4\big(D_i \times 1(W_i > 8)\big) + \epsilon_i$. Each row reports estimates of this regression equation on the outcome identified in the left-most column. Subsequent columns report the estimate corresponding to the term identified in the column heading. Column (5) reports the outcome mean, which is measured 8 years after soldiers arrive at their first operational unit. All regressions are restricted to the sample of soldiers assigned to their first unit between 2005 and 2011 (soldiers who have outcomes we can measure 8 years after arrival). The number of observations is 101,387, except for ever incarcerated (100,381 observations) and credit (93,252) observations). Standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.26: Effects of deployments with 0 peer casualties

	(1)	(2)	(3)	(4)
	Baseline Sample/Spec		$W_i = 0$ Sample	
	10 Months Deployed	(10 Mths Dep) × (1σ Peer Cas)	10 Months Deployed No Peer Cas.	P-Value of H_0: Col (1) = Col (3)
		Panel (a): Trauma and Disability		
Combat Death	0.01	0.27***	-0.13	0.461
	(0.12)	(0.04)	(0.15)	
Ever Combat Injury	1.16***	1.86***	0.98**	0.740
	(0.33)	(0.11)	(0.47)	
Significant Army Profile	0.84	1.02***	0.64	0.908
	(0.64)	(0.16)	(1.59)	
Annual Amt VADC	1876.16***	414.18***	1140.47**	0.146
	(190.42)	(47.93)	(482.20)	
Any VADC Receipt	7.16***	1.29***	2.83	0.071
	(0.88)	(0.21)	(2.29)	
		Panel (b): Noncombat Mortality Outcomes		
Noncombat Death	0.07	-0.01	-0.43	0.342
	(0.20)	(0.05)	(0.51)	
Death of Despair	0.10	-0.06	-0.50	0.166
	(0.16)	(0.04)	(0.42)	
Suicide	0.02	-0.02	-0.26	0.455
	(0.12)	(0.03)	(0.36)	
Drug- or Alcohol-Rel. Death	0.14	-0.06*	-0.36	0.084
	(0.11)	(0.03)	(0.28)	
Motor Vehicle Death	0.04	-0.00	0.12	0.732
	(0.09)	(0.02)	(0.21)	
		Panel (c): Misconduct, Credit, and Education		
Separated for Misconduct	-0.90	-0.07	3.26	0.050
	(0.77)	(0.18)	(2.03)	
Ever Incarcerated	-0.06	0.10	-0.07	0.993
	(0.28)	(0.07)	(0.74)	
Credit Score in 2020 (Vantage)	1.26	-0.01	5.92	0.291
	(1.62)	(0.38)	(4.22)	
Enroll in College	1.13	-0.03	0.91	0.926
	(0.89)	(0.21)	(2.35)	
Assc Deg+	0.41	0.14	-1.16	0.312
	(0.52)	(0.12)	(1.50)	
P-val, joint test that all est. in col (1) and (3) are equal:				0.312
Observations	101,387		15,687	

Notes: Columns (1) and (2) report 2SLS estimates of Equation 6 (with first stage Equations 7 and 8) on our primary outcomes as of 8 years after a soldier arrives at his initial operational assignment, except credit outcomes which are as of 2020. As described in Section 4, we augment our baseline model to include an interaction between months deployed and peer casualty rates, which proxy for more dangerous deployments. Peer casualties are the share of peer soldiers (those who arrive in the same BCT within the same quarter) who suffer non-fatal combat injuries or fatal combat deaths within three years. Column (1) reports $\hat{\beta}$ while Column (2) reports $\hat{\gamma}$. Each row represents a separate regression on a separate outcome. Coefficients and standard errors in column (1) are scaled by 10. Coefficients and standard errors in column (2) are scaled by 10σ, where σ is the sample standard deviation of peer casualties. In addition, the sample sizes for incarceration and credit are smaller: 100,381 for Ever Incarcerated and 93,252 for Vantage Credit Score 2020. Column (3) reports $\hat{\beta}$ estimates from Equation 1 when restricting the sample to soldiers assigned to BCTs with a peer casualty rate of *zero*. Column (4) reports the p-value from a test of whether the estimate in columns (1) and (3) are equal. The p-value at the bottom row of the table reports the p-value from a joint test of whether all the estimates in column (1) and (3) are equal. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.27: Cohort decomposition

	(1) Δ 01-peak	(2) Δ peak-11	(3) YoY Var. Expl.
	Panel (a): Combat Injury		
Dep. Variable	2.694	-3.127	
Xs	-0.043	-0.003	0.000
Frac. Explained by Xs	-0.02	0.001	
Dep. and Cas.	2.624	-2.387	0.602
Frac. Explained by Dep. and Cas.	0.97	0.76	
	Panel (b): Noncombat Death		
Dep. Variable	0.443	-0.473	
Xs	0.128	-0.190	0.315
Frac. Explained by Xs	0.29	0.40	
Dep. and Cas.	0.018	-0.025	0.021
Frac. Explained by Dep. and Cas.	0.04	0.05	
	Panel (c): Annual VADC Amt		
Dep. Variable	2608.58	2240.28	
Xs	123.55	24.23	0.085
Frac. Explained by Xs	0.05	0.01	
Dep. and Cas.	909.70	-930.00	-0.140
Frac. Explained by Dep. and Cas.	0.35	-0.42	
	Panel (d): Annual SSI/SSDI Amt		
Dep. Variable	376.329	-461.298	
Xs	32.777	-26.683	0.040
Frac. Explained by Xs	0.09	0.06	
Dep. and Cas.	142.436	-140.181	0.269
Frac. Explained by Dep. and Cas.	0.38	0.30	

Notes: This Table accompanies Figure 6 and reports measures of how well Xs and Deployment (Dep.) plus Deployment$\times$ peer casualties (Cas.) can explain differences across cohorts. In panel (a), "peak" corresponds to 2005 (the cohort with the highest combat injury rate), in panel (b) to 2008 (the cohort with the highest noncombat death rate), in panel (c) to 2006 (the middle cohort as there is no peak), and in panel (d) to 2006. In Column (1), "Dep. Variable" reports the change in Combat Death between the 2001 and the peak cohort, while "Xs" reports the predicted change in Combat Death between the 2001 and the peak cohort using only the Xs as predictors. The Fraction Explained by Xs is simply the ratio of the predicted change explained by the Xs over the actual change. "Dep. and Cas." reports the predicted change in Combat Death between the 2001 and the peak cohort using only months deployed and months deployed $\times$ peer casualties as predictors. Column (2) repeats this exercise for the change from peak to 2011. Column (3) reports a measure of how well the year-on-year cohort outcome levels (Y) (rel. to 2001) are explained by predicted changes based on Xs or Deployment ($\hat{Y}$), calculated as $\frac{cov(Y,\hat{Y})}{var(Y)}$.

Table A.28: Cohort decomposition for additional outcomes

	(1) Δ 01-peak	(2) Δ peak-11	(3) YoY Var. Expl.
		Panel (a): Combat Death	
Dep. Variable	0.439	-0.616	
Xs	-0.002	0.004	0.008
Frac. Explained by Xs	-0.004	-0.007	
Dep. and Cas.	0.310	-0.271	0.349
Frac. Explained by Dep. and Cas.	0.71	0.44	
		Panel (b): Deaths of Despair	
Dep. Variable	0.479	-0.367	
Xs	0.098	-0.136	0.238
Frac. Explained by Xs	0.20	0.37	
Dep. and Cas.	0.014	-0.027	0.030
Frac. Explained by Dep. and Cas.	0.03	0.07	
		Panel (c): Suicide	
Dep. Variable	0.271	-0.171	
Xs	0.036	-0.057	0.158
Frac. Explained by Xs	0.13	0.33	
Dep. and Cas.	-0.001	-0.004	0.028
Frac. Explained by Dep. and Cas.	-0.004	0.03	
		Panel (d): Any VADC for PTSD	
Dep. Variable	7.843	-1.611	
Xs	0.146	-0.472	0.072
Frac. Explained by Xs	0.02	0.29	
Dep. and Cas.	4.530	-4.727	-0.087
Frac. Explained by Dep. and Cas.	0.58	2.93	

Notes: As in Table A.27, this table reports measures of how well Xs and Deployment (Dep.) plus Deployment× peer casualties (Cas.) can explain differences across cohorts. "Peak" corresponds to 2005 for Combat Death, 2008 for Deaths of Despair and Suicide, and 2006 (midpoint) for any VADC for PTSD. See notes to Table A.27 for additional details.

Table A.29: Law, VA Regulation, and Policy/Practice Changes 2000-2015

Year	Policy Change	Type	Description	Citation
2000	Veteran Claims Assitance Act	Law	There is a 'duty to assist claimants' in obtaining evidence to substantiate their claims	Public Law 106-475
2001	Veterans Education and Benefits Expansion	Law	Repeals the 30-year manifestation period limitation, following service in Vietnam , required for veterans' disability benefits coverage for certain respiratory cancers. extends the period for the presumption of a service connection between certain herbicide-related disabilities and service in Vietnam to 20 years (previously ten); Extends the presumptive period for disabilities associated with Persian Gulf War service until December 31, 2011, or such later date as prescribed by the Secretary. Includes within such disabilities any poorly defined chronic multi-symptom illness of unknown etiology characterized by two or more of the currently listed symptoms. Includes additional signs or symptoms that may be considered a manifestation of an undiagnosed illness.	Public Law 107-103
2001	Type 2 Diabetes added as presumptive condition	VA Rule	Established a presumptive service connection for Type 2 Diabetes for those who served in Vietnam	66 FR 23166
2002	Veterans Benefits Improvement Act	Law	Considers the hearing loss and/or tinnitus of a veteran whose active military occupational specialty likely exposed such veteran to a high level of acoustic trauma to have been incurred in or aggravated by military service (and therefore to be compensable under veterans' disability compensation), notwithstanding that there is no record of evidence of such hearing loss or tinnitus during such service. Requires the payment of veterans' disability compensation to a woman veteran for the anatomical loss of half or more of the tissue of (previously, all of the tissue of) one or more breasts.	Public Law 107-330
2002	PTSD based on personal assault	VA Rule	Evidence other than the veteran's service records may corroborate the occurrence of the stressor for PTSD based on personal assault	67 FR 10330
2003	National Defense Authorization Act FY 2004	Law	Granted concurrent receipt of retirement pay and VADC for those rated with 50% or greater disability	Public Law 108–136
2004	Veterans Benefits Improvement Act	Law	Codifies increases in the rates of veterans' disability compensation; presumes various cancers to be service-connected in the case of veterans exposed to ionizing radiation; treats a disability incurred through Department treatment or vocational rehabilitation as a service-connected disability for purposes of eligibility for veterans' benefits.	Public Law 108-454
2005	Presumption of Sound Condition	VA Rule	The intended effect of this amendment is to require that VA, not the claimant, prove that the disability preexisted entrance into military service and that the disability was not aggravated by such service before the presumption of soundness on entrance onto active duty is overcome.	70 FR 23027
2006	Military Quality of Life and Veterans Affairs Appropriations Act	Law	Prohibits the use of funds: (1) to retroactively or prospectively revoke or reduce an award of veteran's disability compensation for post-traumatic stress disorder (PTSD) with respect to 2,100 compensation cases identified and reviewed by the Department's Inspector General; requires the Department to conduct an information campaign, in states with an average annual disability compensation payment of less than $7,300, to inform all veterans receiving disability compensation of the history of below average payments to veterans in such states, including instructions for submitting new claims and requesting review of past disability claims and ratings.	Public Law 109-105
2006	Veterans Housing Opportunity and Benefits Improvement Act	Law	Adds atherosclerotic heart disease or hypertensive vascular disease, stroke, and their complications to the list of diseases or conditions presumed to be associated with prisoner-of-war status, and therefore compensable under veterans' disability compensation.	Public Law 109-139
2006	Aggravation of Non-service connected disability	VA Rule	Any increase in severity of a nonservice-connected disease or injury that is proximately due to or the result of a service-connected disease or injury, and not due to the natural progress of the nonservice-connected disease, will be service connected.	71 FR 52744
2006	Phase-In of Full Concurrent Receipt of Military Retired Pay and Veterans Disability Compensation	VA Rule	This final rule implements section 641 of the National Defense Authorization Act for Fiscal Year 2004 (Pub. L. 108-136)	71 FR 67059
2007	Extension of the Presumptive Period for Compensation for Gulf War Veterans	VA Rule	Extends presumptive period for disabilities resulting from undiagnosed illnesses suffered by veterans who served in the Persian Gulf War	72 FR 68507
2007	Veteran Vision Equity Act	Law	Modifies the standard for awarding disability compensation to veterans for loss of vision	Public Law 110-157

Notes: This table reports various congressional laws directly related to VADC, VA rules, and policy changes. Congressional laws were found by searching congress.gov for laws containing 'disability" and 'veteran". We omit appropriations and authorizations for pay schedule adjustments. VA rules are taken from here https://www.va.gov/ORPM/VA_Regulations_Published_From_Fiscal_Year_FY_2004.asp, we focus only on final rules and omit rules relating to standard updates to diagnostic schedules. Policy changes are those we are aware of from our literature review.

Table A.30: Law, VA Regulation, and Policy/Practice Changes 2000-2015 (Continued)

Year	Policy Change	Type	Description	Citation
2008	Veterans Benefits Improvement Act	Law	Directs Secretary to pilot programs to speed up claims processing and to provide information and support submitting claims requiring reports to Congress; extends authority of non-VA contract physicians to carry out reviews; directs the Secretary to report to Congress on progress in addressing variances in compensation payments for veterans; adds osteoporosis to the list of disabilities presumed to be service-connected for POWs; establishes committee to advise the Secretary with respect to the maintenance and periodic readjustment of the VA schedule for rating veteran disabilities.	Public Law 110-389
2008-2011	Policy Responses to 2008 Act	Policy	2 pilot programs to improve claims process and information dissemination (McMahon et al., 2009). Quick Start program helping soldiers submit claims prior to leaving started in July 2008. Centralization and paperless processing of BDD claims (04-09) and Quick-start claims (09). Since 2009, all service members have been eligible to submit claims for VA disability compensation before leaving military service (Congressional Budget Office, 2014). Opening of online applications. Multiple outreach initiatives about these programs, including integration in TAP and DTAP briefings.	United States Congress (May 6, 2010); Broten (2021)
2008	National Defense Authorization Act FY 2009	Law	Requires, with respect to eligibility for retirement due to disability, a presumption that a disability was incurred on active duty unless there is clear and unmistakable evidence that the disability existed before entering into active duty and was not aggravated by active service. (Sec. 727)	Public Law 110-181
2009	Termination of Phase-In Period for Full Concurrent Receipt of Military Retired Pay and Veterans Disability Compensation	VA Rule	Veterans who are entitled to receive veterans disability compensation based on a VA determination of individual unemployability are no longer subject to a phase-in period	74 FR 14491
2009	Posttraumatic Stress Disorder	VA Rule	Eliminates the requirement of evidence corroborating occurrence of the claimed in-service stressor in claims in which PTSD is diagnosed in service	73 FR 64208
2009	Presumption of Service Connection for Osteoporosis for Former Prisoners of War	VA Rule	Establish a presumption of service connection for osteoporosis for former Prisoners of War (POWs). This amendment reflects statutory provisions of the Veterans Benefits Improvement Act of 2008.	74 FR 44288
2010	Veterans' Benefits Act	Law	Authorizes higher levels of veterans' disability compensation for veterans who experience difficulty using prostheses and those in need of regular aid from residuals of traumatic brain injury; amends the Veterans Benefits Act of 2003 to extend through 2012 VA authority for the performance of medical disability evaluations by contract physicians.	Public Law 111–275
2010	Stressor Determinations for Posttraumatic Stress Disorder	VA Rule	Liberalizes the evidentiary standard for establishing the required in-service stressor for PTSD. Eliminates the requirement for corroborating that the claimed in-service stressor occurred if related to fear of hostile military activity and stressor is consistent with the places, types, and circumstances of the veteran's service.	75 FR 39843
2010	Diseases Associated With Exposure to Certain Herbicide Agents	VA Rule	Expands the list of presumed conditions related to Agent Orange and other herbicide exposures	75 FR 53202
2011	VOW to Hire Heroes Act	Law	Mandatory participation of members of the Armed Forces in the Transition Assistance Program (in which disability compensation benefit info and support filing is offered)	Public Law 112-56
2011	Integrated Disability Evaluation System	Policy	Proposed in 2007 and fully implemented in 2011, the Integrated Disability Evaluation System (IDES) streamlined VA and DOD systems for evaluating disabilities of active duty soldiers with limiting health conditions.	
2011	Guidance Letter re: MST claims	Policy	Accept markers in lieu of stressors for PTSD claims linked to MST	United States Government Accountability Office (2014)
2013	Secondary Service Connection for Diagnosable Illnesses Associated With Traumatic Brain Injury	VA Rule	Rule regarding the association between traumatic brain injury (TBI) and five diagnosable illnesses. This amendment establishes that if a veteran who has a service-connected TBI also has one of these diagnosable illnesses, then that illness will be considered service connected as secondary to the TBI.	78 FR 76196
2015	Schedule for Rating Disabilities-Mental Disorders	VA Rule	Update Schedule for Rating Disabilities (VASRD) given changes to the Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition (DSM-5)	80 FR 14308

Notes: This table reports various congressional laws directly related to VADC, VA rules, and policy changes. Congressional laws were found by searching congress.gov for laws containing 'disability" and 'veteran". We omit appropriations and authorizations for pay schedule adjustments. VA rules are taken from here https://www.va.gov/ORPM/VA_Regulations_Published_From_Fiscal_Year_FY_2004.asp, we focus only on final rules and omit rules relating to standard updates to diagnostic schedules. Policy changes are those we are aware of from our literature review.

B Additional Details on Data and Sample Construction

B.1 Description of Data

Army Personnel Records. Our baseline administrative data on soldiers comes from the Total Army Personnel Database (TAPDB). These data contain demographic characteristics, education levels, Armed Forces Qualification Test (AFQT) scores, military occupation, length of enlistment contract (e.g., 3, 4, 5, or 6 years), and home of record information determined at the time of enlistment. The Army data also include a monthly panel of Army assignment data (i.e., assignment location, brigade of assignment) and medical injury data from the month of their initial enlistment through their last month in the Army or December 2019. We link this data to Army administrative pay records that permit us to identify when soldiers are deployed to a combat zone. Soldiers who serve in a country designated by the Department of Defense as a combat zone receive Imminent Danger Pay (IDP), which is often referred to as Hostile Fire Pay. Our pay records reveal the amount of IDP a soldier receives each month. For most of the years in our study, the DoD did not pro-rate IDP and paid soldiers $225 if the soldier served in a combat zone at any point within the calendar month. However, the DoD began pro-rating IDP in 2012. As a result, we classify a soldier as having been deployed in a specific month if he receives any IDP for that month. We also link to official casualty records from the Defense Casualty Analysis System (DCAS), which indicate if and when a soldier was wounded or killed in a combat zone as a result of hostile action.

Disability Records. Our data on disability receipt combine Veterans Affairs Disability Compensation (VADC) with the two main federally funded disability programs administered by the Social Security Administration (SSA)–Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI). These data allow us to observe disability records for each individual in our sample in the month of September for each year from 2001 through 2019. Records from VADC include a soldier's monthly benefit payment (which we multiply by 12 to reflect an annual payment) and the specific conditions for which the soldier qualifies for VADC. We adjust for inflation by converting all values to 2020 USD using the CPI-U.

National Death Index (NDI) Mortality Data. We linked the soldiers in our sample to mortality data from the Center for Disease Control and Prevention's National Death Index (NDI) obtained through the Mortality Data Repository operated jointly by the VA and the U.S. Department of Defense. The National Death Index is widely considered the gold standard for mortality data due to its comprehensiveness, accuracy, and ability to identify specific causes of death through ICD-10 codes (Cowper et al., 2002). Data from the NDI are valid through 2019 and indicate the month of death. We record individuals in our sample as deceased if they died in a current or prior month.

Army Misconduct Records. The Uniform Code of Military Justice allows commanders to handle low level misbehavior via a formal process known as an Article 15, which gives the commander the authority to formally punish misbehaving soldiers by demoting them and reducing their pay. When soldiers exhibit misconduct for reasons related to misbehavior (e.g. drinking while on duty),

Commanders have the authority to refer them for separation from the Army or to bar them from reenlisting. We combine formal misconduct separations and separations with a bar to re-enlistment into one outcome for simplicity and power; however, we find similar results when considering these outcomes in isolation. We define demotion as any reduction in rank from one month relative to the previous month.

Incarceration and Army Criminal Records. We acquired our national data on incarceration from LexisNexis through two data purchases in 2019 and 2020. Our data purchases cover all newly enlisted soldiers between 2002 and 2015.[39] The data covers all criminal histories on file at LexisNexis that would consequently show up in a background check when using their services.

The exact procedures LexisNexis follows to assemble its sample is a trade secret. In practice, LexisNexis' actual coverage can vary by state or even county and time. Incarceration records, which typically come from state department of corrections, exist for almost all states (48 plus Washington D.C.) with coverage generally beginning prior to our analysis sample. Additionally, we obtained Army incarceration records covering all incarceration spells in military prison. Our incarceration outcome combines any incarceration in LexisNexis or in military records.

We supplement LexisNexis and military incarceration outcomes with Army military justice records from the Army Law Enforcement Reporting and Tracking System (ALERTS) database. We observe all final Military Police (MP) and U.S. Army Criminal Investigative Division (CID) cases available in the ALERTS database from FY2005 through FY2021. Some CID criminal investigations are escalated from MP investigations when CID has jurisdictions, other CID investigations are triggered by reports from another agency (e.g. positive drug tests). Cases are final when investigations are marked as complete. The data include offense dates, descriptions, and codes. Offenses are grouped into four meta-categories: Violent Felonies (e.g. sex crimes, assault), Non-Violent Felonies (e.g. drug crimes), Misdemeanors (e.g. drunk and disorderly), and Other (predominantly AWOL or Desertion).

LexisNexis Foreclosure Data and Experian Credit Data on Financial Health and Well-Being. We acquired our national data on foreclosure actions from LexisNexis through the same two data purchases mentioned above. The LexisNexis foreclosure data contains variables related to all phases of the foreclosure process: defaults, notice of default, legal actions (e.g. notice of lis pendens and final judgment of foreclosure), auction, and final sale. However, these variables are inconsistently recorded across different states. For that reason, we take a conservative approach, and construct our variable as an indicator that takes a value of 1 if the soldier has any foreclosure action occurring after they arrive at their first unit.

We acquired servicemember credit data through two separate purchases from the Experian credit bureau. The first data purchase included a snapshot of soldier credit scores, debt holdings, and bankruptcy information from June 2017. The second data purchase included similar information from December 2020.

Our primary credit outcome is the Vantage credit score, which is based on a model developed by

[39] In practice, we purchased data for 99.3% of the soldiers in our analysis sample due to minor discrepancies in the entry date variable used for sample selection.

the three major credit bureaus and is meant to predict how likely a consumer is to repay borrowed money. For that reason, we take it as an omnibus measure of financial health. More practically, the vantage score ranges from 300 to 850 and is most heavily influenced by payment history, credit age / mix, credit utilization, and total balances with a smaller role for recent credit applications and available credit. We also observe FICO credit scores, but only in the 2020 credit data. Our credit data include information on total debt, the composition of debt (allowing us to distinguish between mortgage debt, auto debt, debt from student loans, and other debt), debt flagged as derogatory and in collection, and filings for bankruptcy.

National Student Clearinghouse (NSC) Education Data. Outcomes for post-secondary attendance and degree completion come from the National Student Clearinghouse (NSC). NSC records cover the years 2001 through 2020. In these records we can observe post-secondary enrollment and degree completion while soldiers are in the Army and after soldiers leave the Army. We complement NSC education records with Department of Veterans Affairs administrative records that indicate if a soldier ever used the Post-9/11 GI Bill. Post-9/11 GI Bill benefit levels increase with active duty service time, ranging from 40% of education benefits for soldiers who served for 90 days to 100% of benefits for soldiers who served for 36 months or longer (Barr, 2015; Barr et al., 2021). Also, soldiers who suffer combat injuries are eligible for 100% of Post-9/11 GI Bill benefits regardless of their length of service. Two potential explanations for the positive effect of deployment on Post-9/11 GI Bill usage that we observe in Table A.10, and the corresponding negative effect of deployment on student debt in Table A.9, are that deployment increases the likelihood that soldiers serve for at least 2 years (see Table A.6) and deployment increases combat injuries (see Table 3).

B.2 Details on Sample Construction

Our full sample consists of 782,232 first-term soldiers under standard enlistment contracts who arrived at their first unit between 2001 and 2015. This naturally excludes soldiers who did not complete basic training and follow-on schools. This also excludes a handful of other soldiers who enlist with initial occupations that are missing, incorrectly coded, or that the Army eliminated (therefore requiring the soldier to change occupations); who enlist with initial contracts of fewer than three years or longer than six years; or who first appear in our data at the rank of Sergeant or higher. This corresponds to column (1) of Table 1 and Table B.1 below.

Subsequent columns of Table B.1 report summary statistics for successive subsamples as we make additional restrictions to the sample. In column (2) of Table B.1, we restrict to first-term soldiers who enlisted between 2005 and 2015 (N = 565,7561). Restricting to first-term soldiers who were initially assigned to a brigade combat team (i.e. excluding soldiers assigned to training brigades or support brigades) yields a sample of 268,801 (column 3). We then exclude 10,012 soldiers with special assignment considerations or physical limitations that either prevent them from deploying or that could influence the brigade combat team the Army's Human Resources Command assigns them to (column 4). We also exclude 34,197 more soldiers assigned to brigade combat teams in Germany, Italy, and Korea (column 5). Soldiers who serve overseas have different assignment considerations and rules regarding how long they must remain in their assignment, and nearly all are assigned to brigade combat teams that are the only BCT at the specific Army duty-station

where they serve and thus would be dropped from our sample regardless. We then exclude 4,624 more soldiers who are assigned to BCTs before the BCT physically moves Army duty-stations, since moving duty-stations is a treatment that is separate from deployment and because some of these soldiers had the option to transfer to another unit at their original duty-station (column 6). For related reasons, we exclude 15,141 soldiers assigned to a unit that is deactivated within 36 months of the soldier arriving because some soldiers in these units had some ability to influence their follow-on assignment (column 7).

We next drop 27,325 soldiers assigned to Army duty-stations during a quarter where there is only one BCT at the Army duty-station during that quarter as our identification strategy compares soldiers within the same occupation who are assigned to different BCTs at the same duty-station in the same time-period. We exclude another 487 soldiers assigned to BCTs during a year where there are fewer than 100 soldiers among the remaining sample who are assigned to the BCT in the same year. BCTs typically have 3000 - 4000 enlisted soldiers assigned to them, and these small BCTs were most likely units that had factors that restricted the type of soldier HRC could assign to the unit (e.g., the unit was in the process of standing up or closing down). We also exclude 513 soldiers identified as being in military occupational specialty 09B ("Trainee Unassigned") (column 10). Finally, we exclude 15,374 women, who were precluded from serving in most combat occupations, were only permitted to be assigned to brigade headquarters and support battalions prior to 2012, and who are not permitted to deploy during pregnancy, which we cannot observe (column 11). Our remaining sample consists of 160,528 soldiers, but 3,113 are excluded from our analysis because they are singletons within a duty-station by job by initial assignment year by term-length fixed effect, resulting in an effective final sample of 157,415 soldiers (column 12).

Table B.1: Soldier Characteristics Across Sample Restrictions

	Full Sample											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
						Panel (a): Demographics						
Age	21.86	22.01	21.77	21.86	21.87	21.87	21.87	21.88	21.88	21.88	21.82	21.80
Married	15.39	15.97	14.26	14.56	15.21	15.23	15.05	15.38	15.38	15.34	14.75	14.62
Black	18.95	18.78	15.51	15.61	15.58	15.65	15.54	15.73	15.74	15.77	14.12	14.04
Hispanic	12.93	13.04	13.21	13.20	13.25	13.26	13.19	13.34	13.34	13.19	13.03	13.05
Other Race	5.64	5.75	5.42	5.41	5.36	5.36	5.34	5.37	5.37	5.34	5.16	5.15
Female	15.18	14.56	8.65	8.57	8.63	8.58	8.64	8.74	8.74	8.74	0.00	0.00
Number of Dependents	0.33	0.34	0.31	0.31	0.32	0.32	0.32	0.33	0.33	0.33	0.31	0.31
HS Dropout or GED	11.89	11.50	12.77	12.09	11.97	11.81	12.31	12.29	12.30	12.32	12.85	12.91
HS Graduate	76.37	75.79	76.37	76.81	76.89	77.02	76.56	76.60	76.60	76.60	76.64	76.67
Some College+	11.64	12.65	10.81	11.05	11.09	11.13	11.07	11.05	11.05	11.02	10.46	10.37
AFQT Score	58.86	58.74	57.87	57.90	57.89	57.90	57.88	57.72	57.71	57.78	58.15	58.01
						Panel (b): Service Experience						
Combat Occupation	36.62	36.72	57.91	57.86	57.86	57.84	57.85	57.65	57.68	57.85	63.37	64.30
Mths Deployed w/in 3 yrs	5.98	5.44	6.40	6.36	6.48	6.44	6.55	6.38	6.37	6.37	6.53	6.52
Combat Injury w/in 3 yrs	1.57	1.44	2.27	2.25	2.27	2.23	2.29	2.04	2.04	2.04	2.21	2.24
Combat Death w/in 3 yrs	0.20	0.17	0.27	0.27	0.27	0.26	0.27	0.26	0.26	0.26	0.28	0.29
Observations	782,232	565,761	268,801	258,789	223,992	219,368	204,227	176,902	176,415	175,902	160,528	157,415

This table reports summary statistics for successive subsamples as we make the restrictions described in Appendix Section B.2. Column (1) is identical to column (1) of Table 1. Column (12) is identical to column (5) of Table 1. Column (2) reports summary statistics when we restrict to soldiers who enlisted between 2005 and 2015. Column (3) further restricts to soldiers initially assigned to brigade combat teams (BCTs). Column (4) excludes soldiers with special assignment considerations or physical limitations. Column (5) excludes soldiers assigned to BCTs in Germany, Italy, and Korea. Column (6) excludes soldiers assigned to BCTs that change Army duty-stations and column (7) drops soldiers assigned to BCTs that deactivate within 3 years of a soldier's arrival. Column (8) excludes soldiers assigned to Army duty-stations where there is only one BCT. Column (9) excludes soldiers assigned to BCTs that receive fewer than 100 soldiers in the same year (among the remaining sample). Column (10) excludes soldiers with the 09B military occupational speciality ("Trainee Unassigned"). Column (11) excludes women. Column (12) excludes singletons within a duty-station by job by initial assignment year by term-length fixed effect. See Appendix Section B.2 for additional details. All demographic variables and occupations are measured prior to arrival at one's unit. Months deployed, combat injuries, and combat deaths are calculated over the 3 years after arrival.