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 effects of combat deployments on soldiers' well-being. To eliminate non-random selection into deployment, we leverage quasi-random assignment of newly recruited soldiers to units on staggered deployment cycles. Deployments increase injuries, combat deaths, and disability compensation, but we find limited evidence that they affect suicide, deaths of despair, financial health, incarceration, or education. More dangerous deployments have similarly limited effects. 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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Almost 2 million American servicemembers deployed to Iraq or Afghanistan following September 11, 2001 (Bilmes, 2021). Simultaneously, the outcomes of U.S. veterans deteriorated. The age and sex adjusted suicide rate of veterans rose nearly twice as fast as non-veterans (Department of Veterans Affairs, 2021). As shown in Figure 1, real annual Veterans Affairs Disability Compensation (VADC) payments per living veteran rose from $900 to $4,700 over this period, reaching total annual expenditures of nearly $100B by 2021. As a result, VADC growth has outpaced widely documented growth in other programs, and is currently 10 times larger per eligible beneficiary than Social Security Disability Insurance (Autor and Duggan, 2006a).

Many lay the blame for the rise in VADC and the decline in veteran well-being on the long-run behavioral and health consequences of combat deployments (e.g. Stiglitz and Bilmes, 2008; Tanielian and Jaycox, 2008; Bilmes, 2021).[1] However, assessing the causal role of warfighting is challenging because many other factors have changed over this period. For example, in response to recruiting shortfalls, the Army permitted soldiers to enlist with lower Armed Forces Qualification Test (AFQT) scores and granted more waivers for prior felony convictions (DoD, 2020; Murphy, 2019). Changes in policy also made it easier for veterans to access 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' outcomes. We construct a unique dataset that combines numerous military and non-military administrative data sources. Our data allow us to investigate the effects of deployment on VADC and noncombat deaths, including deaths of despair and suicides, and other key measures of veteran well-being. Unlike prior literature, we do not rely on survey data based on retrospective questions about military experience. The span of our data permits us both to measure effects over long time horizons and to compare cohorts of soldiers who enlisted between 2001 and the present. This allows us to contrast the effects of deployment over this period with the impacts of changes in the characteristics of soldiers.

Despite our detailed data, identifying the causal effect of combat deployments remains difficult because soldiers are not deployed at random. For example, unit commanders

[1] 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; post-traumatic 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).

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 an interface populated with 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 of these units deploy in the near-term, while others do not. As a result, our strategy isolates variation in exposure to both any deployment and its duration that is orthogonal to soldiers' 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 9.4pp and annual VADC compensation by $2,602 per person eight years after enlistment. Some of this increase reflects the dangers of warfighting. Deployment causes a 4.4pp increase in the probability of being wounded in combat and a 2.6pp 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 0.53pp (30% of the mean) within 8 years. Yet, direct injuries alone do not fully 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, could also contribute to the large, observed VADC effects.

While deaths and injury from warfighting are mechanically connected to deployment, noncombat deaths, including suicide and drug overdose, are not. Our point estimates suggest an average deployment has modest effects on these outcomes. The estimated effect on overall noncombat deaths within eight years of enlistment is 0.05pp. For deaths of despair, which primarily comprise suicide and drug or alcohol-related deaths, the estimated effect is 0.002pp. As a result, deaths that occur as a direct result of combat explain 91% of the overall 0.53pp mortality effect. However, 95% confidence intervals cannot rule out a 0.40pp increase in noncombat deaths (32% of the mean) and a 0.27pp increase in deaths of despair (34% of the mean).

To better understand whether deployment has meaningful adverse effects beyond increasing average disability and mortality due to combat, 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 over our analysis of the impact of average deployment. For example, we can rule out that a standard deviation increase in peer casualty rates increases noncombat mortality within 8 years by 0.08pp (7% of the mean) and deaths of despair by 0.02pp (3% of the mean) conditional on deploying for the same length of time.

Second, we explore whether deployments have adverse effects on other measures of veteran 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 outcomes on misconduct, incarceration, credit, or educational attainment. These results are consistent with deployment having limited adverse affects beyond combat risk. These findings are also consistent with the possibility that VADC payments help alleviate adverse effects.

We conclude by revisiting the striking trends in veterans' outcomes that have been the focus of much public attention. We decompose between-cohort changes in mean outcomes into components attributable to deployment, observable selection (changes in soldiers' average characteristics), and all other factors. The results 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. Observable factors also explain little of the increase in VADC over time. This suggests that changes in overall VADC generosity and eligibility criteria may be responsible for the most recent surge.

The results also show that deployment does not explain changes 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 these outcomes. For example, changes in these characteristics alone 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.

Our paper relates to a multidisciplinary literature on the effects of military service and deployment in particular.[2] While aspects of service can improve labor market outcomes (e.g., Barr, 2019; Wilson and Kizer, 1997; Breznitz, 2005; Borgschulte and Martorell, 2018), service also exposes enlistees to significant risk, perhaps none more salient than combat. Several studies primarily published in medical journals have focused on links between deployment and health, including psychological and cognitive injuries.[3] Other work has linked deployment 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). Yet, despite its importance, causal evidence of the impact of modern combat deployments on soldiers' outcomes remains scarce, with most analyses relying on survey data and observational research designs. We extend this important work by using high quality administrative data on soldier outcomes and by leveraging quasi-random variation in the soldier-to-unit assignment mechanism for identification.

Lastly, while several papers have explored potential mechanisms behind the growth of the SSDI program in the 1990s and 2000s (see, for example Black et al., 2002; Autor and Duggan, 2003, 2006b; Duggan and Imberman, 2009; Von Wachter et al., 2011; Burkhauser and Daly, 2012; Mueller et al., 2016), few papers have studied the even larger recent rise in VADC.[4] Specifically, 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

[2] 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) study the effects of service both inside and outside of the U.S. on earnings and disability compensation.

[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] Angrist et al. (2010) 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) show that by 2006, the 2001 Agent Orange policy decision increased VADC enrollment by 5pp among Vietnam-era veterans who served in theater relative to Vietnam-era veterans who did not serve in theater, and Coile et al. (2021) find evidence that this growth continued beyond 2006.

and Zhang, 2022), much less is known on the effects of recent combat deployments on disability (Heaton et al., 2012), and the role of deployment relative to alternative channels in explaining VADC's unparalleled growth.

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 outcome 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 Army 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 time frame of our study (2005-2015).

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 that we adjust for inflation by converting to 2020 USD using the CPI-U. Mortality data from the National Death Index (NDI) reveal the date and cause of death for deceased soldiers. We also link to incarceration data from LexisNexis and Army records, 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, or initial entry, soldier. Our baseline estimation sample consists of all 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. Restricting the sample to soldiers assigned to BCTs excludes soldiers assigned to training units that rarely deploy or 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 occupation assigned to different BCTs at the same location within the same year, we necessarily exclude soldiers assigned to locations with a single BCT. Finally, we restrict our sample to male soldiers.[5] Our final sample consists of 157,415 first-term male soldiers assigned to a US-based BCT between 2005 and 2015. Appendix B.2 contains additional details on sample construction.

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) shows averages for our estimation sample. Column (3) restricts our estimation sample to the subset of soldiers who do not deploy within 8 years of arrival at their first duty-station. Column (4) restricts our estimation sample to soldiers who deployed within 8 years of arrival at their first duty-station. Relative to the average first term soldier, our sample is less likely to be Black, more likely to have dependent children, and has lower average levels of education. These differences are largely driven by occupational differences that emerge from our restriction to soldiers whose initial assignment is to a brigade combat team. This is evident in panel (b), where we observe that 64% of soldiers in the estimation sample work in combat occupations compared to only 37% of soldiers in the full sample.

Columns (3) and (4) reveal that soldiers who deploy are less likely to be Black, less likely to be Hispanic, and have lower average levels of education yet *higher* AFQT scores. What drives this selection? Some of this occurs because soldiers in combat occupations are more likely to deploy even conditional on assignment to a BCT. Many soldiers rotate to a

[5]Women, who constitute roughly 9 percent of first-term soldiers in BCTs, are not permitted to deploy while pregnant, and our data do not permit us to observe all cases of pregnancy.

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. A second reason we see selection into deployment is that when BCTs deploy, unit commanders have discretion in determining which soldiers to leave behind in garrison as a "rear-detachment" that facilitates administrative matters and serves as a link between the deployed unit and their stateside infrastructure. Soldiers who perform poorly in training or who misbehave are more likely to remain in the rear guard and hence to be negatively selected on unobservable characteristics. We also expect that soldiers with extenuating family circumstances may be more likely to be left behind. This selection into deployment motivates the instrumental variables (IV) strategy we describe next.

2 Identifying causal effects of deployment

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

After completing initial entry training, soldiers are assigned to their first unit primarily based on Army personnel requirements (Army Regulation 614-200). The starting point 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), located in Fort Knox, Kentucky, 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.[6]

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 service-member 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).[7] But given the hi-

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

[7]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).

erarchical, command-and-control nature of military service, low-ranking, first-term soldiers in fact have virtually no ability to influence which specific brigade they are assigned within a given duty-station. As a result, brigade assignments are as good as random conditional on MOS, duty-station, contract term-length, and arrival time.[8] 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 create units that were designed to be self-sustaining and interchangeable to facilitate unit replacements during combat operations (Johnson et al., 2012). The Army accomplished this partly by standardizing unit structure through a process known as "modularity" and partly by standardizing the stateside training regimen that units engaged in during the run-up to a deployment.

In addition to standardizing the training environment, ARFORGEN 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).[9] As a general rule, the majority of soldiers assigned to a BCT would deploy for 9-15 months, followed by anywhere from 1 to 5 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 con-

[8] 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.

[9] Table A.1 reports the number of different BCTs to which a soldier can be assigned to in each year.

trols 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 IV approach that exploits BCT-level variation in deployment exposure. At each point in time over our sample frame, any given BCT within 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 solider assigned to a BCT that has just returned from overseas.

To build intuition for the variation exploited by our research design, it is helpful to consider a stylized example. Suppose we observe Private Bruhn and Private Greenberg, who 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 Second Brigade Combat Team, which will deploy within the next calendar year; Private Greenberg is assigned to the First Brigade Combat Team, which will not deploy until 2008. The institutional details of the soldier-unit assignment procedure suggest that the choice by an HRC career manager to assign Private Bruhn to First Brigade and Private Greenberg to Second Brigade is as-good-as random. Thus we can compare the outcomes of Privates Bruhn and Greenberg to estimate the causal effect of assignment to First Brigade relative to Second 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.[10]

2.4 Empirical implementation

We implement the research design using the following 2SLS model:

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

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

[10]In a heterogeneous effects framework, we also need a monotonicity condition to identify a local average treatment effect (Imbens and Angrist, 1994): there are no soldiers who 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. In practice, first term soldiers should not be able to influence time spent deployed. This is consistent with the high compliance rates we document in Section 2.6.

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.[11]

Our treatment variable is D_i, which measures the number of months that soldier i spent deployed within three years of arrival at their initial duty-station.[12] 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).

2.5 Instrument validity

Table 2 presents evidence in support of the assumption that soldiers are as-good-as randomly assigned to brigades conditional on our set of fixed effects. Panel (a) reports results from 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 the pre-treatment soldier characteristics (the p-value of a joint F-test is 0.342). 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, illustrating the raw variation exploited by the research design.[13] We see no relationship between our instrument and predicted months deployed, despite

[11] As noted in Section 2.1, 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, the calendar year of arrival, in order to leverage as much variation as possible and increase our statistical power; however, we obtain similar (albeit noisier) results using quarter of arrival instead.

[12] In the above 2SLS specification, the β coefficient on D_i captures the effect of combat deployment. Under the LATE framework (Imbens and Angrist, 1994; Angrist et al., 1996) and its extension to ordered treatments (Angrist and Imbens, 1995), β captures 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."

[13] Both the x-axis and y-axis report residualized variables after partialling out duty-station by job by initial assignment period by term-length fixed effects and then adding back the sample means of each variable.

a strong relationship with actual months deployed.

Panel (b) of Table 2 presents naive OLS regressions of pre-treatment soldier characteristics on the months-deployed treatment variable and our preferred set of fixed effects. In contrast to the balance on our leave-out months deployed instrument, actual months-deployed is strongly correlated with individual soldier characteristics even conditional on our demanding set of fixed effects. Among soldiers with similar occupations who arrive at the same duty-station at similar points in time, soldiers with high school diplomas and that have higher AFQT scores deploy for longer lengths of time on average, while soldiers who are married, have more dependents, and who are older deploy for less time on average. This is consistent with Section 1.3 where we discuss how selection may emerge even within occupation and unit as a result of decisions by commanders regarding which soldiers they choose to bring to war.

For BCT assignment to serve as a valid instrument, it also must satisfy an exclusion restriction. 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 ARFORGEN system highly standardized the stateside training sequence soldiers received as they prepared for their next deployment (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 necessitated a well-established cycle that was 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., unit culture).[14] The cyclical nature of deployment cycles 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).

[14]This argument would motivate considering BCT assignments in a "many invalid" instrument framework (Kolesár et al., 2015), but we do not pursue this further here. The staggered nature of deployments also makes it technically possible to include BCT fixed effects in our main specification, although doing so substantially decreases precision. For that reason, we focus on results without them.

For these reasons, we believe that exclusion is a reasonable assumption in this context, allowing us interpret our 2SLS estimates as treatment effects of deployment. 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 properties of 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.[15]

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 0.9606 (0.0054), reported in the upper-left corner.[16]

Panel (a) of Figure 3 shows the dynamics of the first stage over time since entering the army. The color of the points changes at 48 months post-arrival, visually denoting where our sample goes from being balanced to unbalanced.[17] 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.

Initial BCT assignment is also strongly correlated with exposure to combat itself. Figure 3 reports 2SLS estimates of the effects of deployment on combat-related outcomes such as suffering a combat injury (aka, being "Wounded in Action" (WIA), which is defined as an injury resulting from adversarial action), suffering a serious combat injury (defined as an injury from adversarial action that is life-threatening or life-altering, or where death is possible within 72 hours) (Department of Veterans Affairs, 2008), and being killed in

[15]We also note that part of the deployment experience involves increasing personnel strength as it gears up for deployment (e.g. increasing the number of officers and share of officers who are male). We consider these changes to part of the part of the deployment "treatment," although their importance relative to the impacts of actual war-fighting abroad is likely small.

[16]Figure A.2 shows that the instrument also shifts the extensive margin of any deployment within three years of arrival. Figure A.3 plots the reduced form relationship between our instrument and our main outcomes.

[17]For example, since we do not observe most outcomes beyond December 2019, our estimates at 5 years after arrival exclude any soldiers who arrived at their first operational assignment after December 2014.

combat.[18] Point estimates are scaled up by factor of 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 (4) 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. In general, the results reported in Figure 3 clearly show that the variation in deployment induced by the instrument is strongly related to exposure to violence and combat.

3 The long-run causal effect of deployment

This section presents three sets of results on the long-run effects of combat deployment. We begin with the impacts on VADC and explore to what degree the large effects we document are the direct result of injuries sustained in war as opposed to more general physical and mental trauma or deployment facilitating access to VADC. The second set of results pertains to the long-run impacts on deaths and non-combat deaths especially. Last, we also examine the long-run effects on additional outcomes such as misconduct, incarceration, credit, and educational attainment.

3.1 Disability Compensation

Figure 4 plots 2SLS estimates of the causal effect of deployment at an annual frequency relative to the year of arrival at first duty-station. As above, the point estimates are scaled up by factor of 10 to reflect the effect of an average length deployment. Panel (a) of Figure 4 plots results for an indicator that takes a value of one if the soldier received VADC at any point during the most recent calendar year. Panel (b) of Figure 4 plots results for total VADC disability payments (in 2020 USD) received by the soldier during the most recent calendar year. The estimates show that deployment causes meaningful long run-increases in both the likelihood of receiving any VADC and in the total dollars received.

[18]Combat deaths include soldiers identified as "Killed in Action" (KIA) in official casualty records from the Defense Casualty Analysis System (94% of combat deaths in our estimation sample), soldiers who die in Iraq, Afghanistan, or Kuwait as a result of unspecified vehicle accidents (ICD-10 code V899; 4% of combat deaths), noncommercial aircraft accidents (ICD-10 code V958; 2% of combat deaths), or explosions of blasting or other materials (ICD-10 code W40; $<$1% of combat deaths), 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% of combat deaths). 99% of KIA deaths from casualty records are also identified as war deaths in the NDI.

Panel (a) of Table 3 summarizes findings for these outcomes at 2-year intervals. Panel (a) displays results for both the intensive and extensive margin of VADC receipt plotted in Figure 4. In addition, panel (a) also provides extensive margin results separately for receipt of any disability (VADC, SSDI, or SSI) along with intensive margin results for total disability payments (the sum of VADC, SSDI, and SSI). As with Figure 4, we have scaled up the coefficients in Table 3 by a factor of 10 so that the point estimate in the table reflects the causal effect of a deployment of (approximately) average length. All binary outcomes are expressed in percentages. We do the same for all subsequent tables.

Eight-years after first arrival, deployment increases the likelihood of receiving any VADC by 9.4pp off a base of 37%. These extensive margin results translate into an increase of $2,602 dollars paid per year, which is 42% of the amount paid to the average soldier in our sample. Examining results on total disability payments from VADC, SSDI, and SSI, we see that the effect of deployment on receipt of disability from the three main federal programs predominantly operates through the VADC program.[19]

One caveat for interpreting the impact of deployment on disability receipt is that some of the treatment effects are mediated by the fact that deployment initially reduces the likelihood that soldiers leave the military, but then increases separation from the Army four years after soldiers arrive to their first duty-station and beyond. In Table A.5, we show that 8 years after arrival at first duty-station, a typical deployment increases separation from the Army by 2.6pp (relative to a mean of 83%). Since soldiers cannot receive VADC while on active duty, it is possible that the large causal increases in disability receipt we have documented will shrink a little over time as soldiers in the control group leave the Army and subsequently collect. This makes our point estimates inappropriate for naively extrapolating out to the long-run impact that the increase in VADC will have on the government budget. However, these estimates are still appropriate for quantifying the retrospective impact that deployment has had on VADC receipt up until our data end in 2019. Moreover, even under extreme assumptions, separation can only mechanically account for a small portion of our estimated VADC effects.[20]

[19] We probe the robustness of our results to various specifications. Specifications of Equation (1) with quarter of arrival fixed effects show similar impacts for our primary outcomes (Table A.3). Table A.4 explores non-linearity in the effects by estimating multiple endogenous variable models (e.g., months deployed and months deployed squared). Higher order interactions do not enter significantly for VADC receipt and VADC amounts, although standard errors increase substantially.

[20] For example, assume the worst possible case that all 2.6pp of soldiers separated as a result of deployment received 99th percentile VADC ($46,000) by year 8 and otherwise would not have received VADC. This implausible upper bound could still only explain 2.6pp of the 9.42pp effect on VADC receipt and $1,196

Sending soldiers into conflict results in injuries that require long term care and hence qualify soldiers to receive disability compensation. We explore this directly in panel (b) of Table 3, which presents results related to the causal effect of deployment 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 8 years by 0.48pp and increases the likelihood that a soldier experiences a combat injury by 4.4pp. The magnitude of these effects are meaningful. 0.48pp is 37% of the death rate among compliers who do not deploy (1.3pp).

Since combat injuries only capture injuries during combat deployments, they cannot capture injuries among 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. For reference, only 43% of soldiers in our sample who experienced combat injuries also received a medical profile at some point while still in the Army, 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 permanent and therefore severe enough to limit their ability to continue to serve in the Army.[21]

In the short term, deploying reduces the likelihood that a soldier receives any medical profile (-.76pp within the first two years of assignment). 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 we see that the average deployment causes an 1.71pp increase in the likelihood of having any medical profile, a 7% increase relative to mean profile rates (25.6%). The average deployment also increases the likelihood of having a significant medical profile by 2.62pp, a 17% increase relative to mean significant profile rates (15.0%). Overall, the results for combat injuries and medical

of the $2,602 effect.

[21] 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.

profiles suggest that deployment meaningfully impacts a soldier's probability of injury, but that injuries resulting from combat attacks on US forces are relatively rare (occurring for fewer than 1 in 20 deployed soldiers). Further, the results suggest that not all of these injuries result in significant profiles that force a soldier to leave the Army, and that many soldiers suffer injuries even in the absence of combat deployments.

To what extent can combat injuries alone explain our estimated disability effects? In Table A.6, we investigate the association between being injured in combat or receiving a significant army profile and future VADC receipt. Among those who deploy, a combat injury in the first term is associated with a 24.45pp increase in VADC receipt and an $8,663 increase in VADC payments. Applying this estimate to the 4.43pp increase in combat injuries caused by deployment by year 8, we would expect combat injuries to explain around a 1.08pp increase in VADC receipt, or $384. This figure changes little when using Significant Army Profiles instead of Combat Injuries. As such, injuries sustained in combat explain only a small portion of the overall effect of deployment on VADC.

Table A.7 further explores the drivers of VADC receipt by showing how deployment affects VADC receipt for the top 5 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 12.13pp increase or 83% of the mean). Taken together, these results suggest that deployment increases VADC 8 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 are possibly 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 to benefits among otherwise similar soldiers. 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, which require that a psychiatrist or psychologist can confirm that a stressor adequately supports a diagnosis of PTSD and that the veteran claim that the stressor is related to fear of hostile military or terrorist activity consistent with their service (38 CFR § 3.304(f)). Finally, it is possible that other channels, including, for example, peer effects, information dissemination, additional screening, or changes in expectations about the probability of making a successful claim, disproportionately encourage soldiers who have deployed to apply for VADC.

3.2 Noncombat deaths and "deaths-of-despair"

Table 4 displays our results for mortality outcomes derived from the National Death Index. The table reports results for all-cause mortality, deaths due to combat, all noncombat deaths, and then specific subcategories of noncombat deaths: "deaths of despair", the two primary subcategories of deaths of despair (suicide and drug or alcohol-related deaths),[22] and deaths resulting from motor-vehicle accidents, assault, and all other causes.

We find large and statistically significant effects of deployment on mortality. Within two years of arrival at first duty-station, the average deployment causes a 0.50pp increase in mortality, and it remains largely stable thereafter. Eight years after arrival at first duty-station, we see that deployment causes a 30% increase in all-cause mortality relative to the outcome of the average soldier (an effect size of 0.53pp relative to a mean of 1.75pp). However, 91% of this effect is concentrated in deaths resulting from combat (0.48pp at eight years after arrival).

In contrast to combat deaths, the impact of deployment on all noncombat deaths is substantially smaller in magnitude and not statistically significant. We also find no statistically significant evidence that deployment causes deaths of despair, its subcategories, or other causes. Eight years after arrival, point estimates suggest that deployment increases noncombat deaths from all causes by 0.05pp, roughly 4% of the mean in our sample (1.25pp), has no effect on deaths of despair, and, if anything, a slight negative effect on suicide (-0.02pp). However, we caution that mortality is a rare outcome among the relatively young individuals in our sample who are typically around the age of 22 when they arrive at their first duty-station. As such, our results on noncombat deaths are not estimated with the degree of precision necessary to rule out large adverse effects. For example, 95% confidence intervals only allow us to rule out that deployment increases

[22] 98% of "deaths of despair" are suicides or deaths resulting from drugs or alcohol (or both). The remaining 2% are firearm deaths resulting form undetermined intent.

noncombat deaths within 8 years by 0.40pp, which is 32% of the mean.[23]

A series of additional analyses help provide more context for our findings regarding noncombat deaths. First, in the remainder of this section we show that deployments have little effect on other adverse outcomes that are are more precisely estimated than noncombat deaths. Second, in Section 4 we are able to show with substantial precision that more violent deployments do not cause more noncombat deaths than average deployments. Thus, if the positive point estimates in Table 4 represent real causal effects (rather than noise emerging from sampling variation), then the results in Section 4 will suggest that the causal channel that generates the behavioral changes that lead to suicide and other deaths of despair works through a mechanism other than exposure to violence. Finally, in Section 5 we show that cohort trends in noncombat deaths are not well explained by deployment, but are at least partly explained by selection.

3.3 Misconduct, Credit Scores, and Education

Panel (a) of Table 5 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 the propensity for separation for misconduct, but this is almost certainly a short-term incapacitation effect. Soldiers may have less opportunity to misbehave while deployed and commanders will often defer Army separation proceedings until after the unit has returned stateside. This is consistent with the fact that our point estimates of the impact of deployment on separation for misconduct quickly revert to nearly zero by year 4 and remain that way 8-years after arrival at first duty-station. These estimates are precise: we can rule out effect sizes 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 8 years. Moreover, the results reported in Table A.8 suggest that while soldiers remain in the Army, deployment does not increase their propensity to be demoted or to become the subject of military investigations.[24]

Panel (b) of Table 5 suggests that deployment has a precise null effect on Vantage credit

[23] As with VADC effects, Panel (c) of Table A.4 shows that non-linear effects of deployment on these outcomes are difficult to detect.

[24] Results on demotions and military criminal investigations suffer from a censoring problem because we only observe these outcomes while soldiers are in the military and we previously showed that deployments cause some soldiers to leave the military (Table A.5).

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 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 5 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 5 suggests that deployment may have a small, positive effect on ever having enrolled in college by year 6, but this effect is indistinguishable from zero by year 8. Similarly, deployment appears to have no effect on earning an associate's degree or higher.[25] These estimates are precise enough to rule out that a ten month deployment decreases college enrollment by 0.5pp (1% of the sample mean) and decreases degree attainment by 0.26pp (3% of the mean).

Overall, the null and often precise effects of deployment on misconduct separations, incarceration, credit scores, and education outcomes are consistent with deployment having limited effects beyond direct combat risk and VADC receipt. Of course, if VADC ameliorates the adverse effects of deployment, then this could also potentially explain our collection of null results. There is limited evidence that VADC directly mediates mortality (Autor et al., 2016; Silver and Zhang, 2022; Trivedi et al., 2022), which is consistent with evidence that lottery wealth does not impact mortality (Cesarini et al., 2016). However, Silver and Zhang (2022) find that VADC payments improve self-reported health and decrease food insecurity and homelessness despite having no impact on blood pressure, HbA1c glucose levels, body mass index, major depressive disorder, and alcohol and substance use disorders. Moreover, other studies find that employment, public assistance, cash transfers, and public insurance reduce crime or recidivism (Yang, 2017; Rose, 2018; Palmer et al., 2019; Deshpande and Mueller-Smith, 2022). Increased income should also improve credit and the ability to finance post-secondary education. Irrespective of the extent to which VADC ameliorates outcomes, the results in this section suggest that the

[25] 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 suggest deployment may have a small, positive effect on college enrollment but no effect on earning an associate's or bachelors degree by 2020.

deployments observed in our data, which occurred in a context with generous VADC compensation, had little impact on criminal, credit, or education outcomes.

4 Do more dangerous deployments cause different effects?

To this point, we have confined our analysis to the effects of the average deployment. Yet this approach potentially masks heterogeneity in the degree of danger soldiers experience during their deployment. To explore this possibility, we 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:

$$W_i = \frac{1}{n_{bq} - 1} \sum_{\ell \in N_{bq(-i)}} CAS_\ell \tag{4}$$

where $CAS_\ell = 1$ if soldier ℓ suffers a combat death or combat injury within 3 years of arriving to 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:

$$Y_i = \delta_{k(i)} + \beta D_i + \gamma\big(D_i \times W_i\big) + \epsilon_i \tag{5}$$

$$D_i = \omega_{0,k(i)} + \pi_0 Z_i + \rho_0\big(Z_i \times W_i\big) + u_{0,i} \tag{6}$$

$$\big(D_i \times W_i\big) = \omega_{1,k(i)} + \pi_1 Z_i + \rho_1\big(Z_i \times W_i\big) + u_{1,i} \tag{7}$$

[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.

where Y_i, D_i, and $\delta_{k(i)}$ are defined as above and where and $\omega_{0,k(i)}$ and $\omega_{1,k(i)}$ correspond to and $\omega_{k(i)}$ from Equation (2).[27]

Peer casualties are an ideal measure for estimating heterogeneity in the severity of combat for several reasons. First, this approach avoids the potential for bias inherent in many alternative approaches (e.g. comparing soldiers in combat occupations to those in non-combat occupations). Second, the residual variation in $\left(Z_i \times W_i\right)$ from Equation (6) is 68% larger than the residual variation in the instrument, Z_i, which improves our precision for rare outcomes like mortality and incarceration. As an example of the substantial variation in casualty rates across units that deployed, note that among soldiers who arrived to their unit in 2009 and deployed within three years, 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, even if it is possible for individual soldiers to take actions that influence their likelihood of being killed or wounded, unit-level casualty rates are predominately a function of exogenous factors outside of a soldier's individual control, such as the location of the servicemember's deployment (which we cannot observe), the unit's specific mission, and the broader geopolitical environment. To lend support to this assertion, Table 6 reports results from a reduced form regression analogous to Equation (6), 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 as is a test that all coefficients are jointly zero.

Table 7 reports 2SLS estimates of Equation (5), 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 determined 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 (3.5%). Thus, we interpret estimates of β as the average effect of a 10 month deployment with zero peer casualties and estimates of γ as the additional impact of a standard deviation increase in

[27] We obtain nearly identical results when we estimate Equations (5) and (6) by including W_i as a separate variable in our IV model without interacting W_i with months deployed (see Table A.11). When we estimate Equations (5) and (6) with both a main term for W_i and a $\left(D_i \times W_i\right)$ interaction term, we obtain qualitatively similar results, but with standard errors that are 3-4 times as large as the standard errors from our preferred specification.

peer casualties during the same deployment.

Panel (a) of Table 7 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 1.16pp increase in combat injuries (only 26% as large as the effect of an average deployment; compare to Table 3),[28] and a 0.84pp 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 0.27pp, combat injuries by 1.86pp, and severe medical profiles by 1.02pp (all 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 increases receipt of any VADC payments by 1.29pp. Although these estimates leave little doubt that VADC receipt and payments increase with exposure to violence, deployments with no peer casualties also have substantial effects on disability receipt, increasing annual VADC payments by $1876 and any VADC receipt by 7.15pp. Table A.12 further shows exposure to violence is strongly linked to serious combat injuries and VADC receipt with a documented amputation. These outcomes are 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 exposure to physical violence. It is also consistent with the possibility that deployment could increase eligibility or applications for 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 7 reveals no relationship between peer casualty rates and noncombat deaths, deaths of despair, or key subcategories of deaths of despair (i.e. suicide and drug or alcohol-related deaths). These estimates are statistically insignificant, but negative in magnitude and precise enough to

[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.

rule out meaningful effects. For example, we can rule out that a standard deviation increase in peer casualty rates increases noncombat deaths by 0.09pp conditional on the same length of deployment, 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.[29]

Panel (c) of Table 7 suggests that exposure to more violence during deployments has no 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 0.29pp (1% of the mean), increases incarceration by 0.23pp (9% of the mean), decreases Vantage credit scores by 0.8 points (0.1% of the mean), and decreases college enrollment by 0.4pp (0.6% of the mean).

Simpler comparisons of soldiers in combat occupations (e.g. infantry) to soldiers in noncombat occupations (e.g. supply specialists or human resource specialists) also paint a similar picture (Table A.13). Soldiers in combat occupations generally experience more dangerous deployments than soldiers in noncombat occupations, and indeed, a 10-month deployment increases combat deaths among soldiers in combat occupations by 0.65pp, which is nearly four times as large as the corresponding estimate for soldiers in noncombat jobs (0.17pp). Effects on combat injuries are also several times larger for soldiers in combat occupations (6.05pp vs. 1.50pp). The impact of deployment on disability receipt is larger for soldiers in combat occupations than it is for soldiers in noncombat occupations. However, differential effects on disability are not as dramatic as differential effects on trauma outcomes. Subsequent panels of Table A.13 reveal little heterogeneity by occupation in the effect of deployment on noncombat mortality or other outcomes. These patterns are consistent with our peer casualty findings, but we prioritize the latter as these address self-selection into occupations and account for heterogeneity in risk within occupations.[30]

[29]Table A.4 explores non-linearity in peer-casualty effects (see columns 6-8). The estimates suggest the marginal impacts on VADC outcomes are decreasing in the severity of violence, but continue to be large and positive. Effects on non-combat deaths continue to show no evidence of any effects. In addition, Figure A.4 plots the relationship between BCT$\times$quarter-of-arrival specific effects of a 10 month deployment on outcomes and effects on peer casualties.

[30]We also report heterogeneity along other margins: AFQT, moral waivers, and race, in Tables A.14 and A.15. Heterogeneity by race mirrors heterogeneity in combat and noncombat occupations, consistent with the fact that white soldiers disproportionately work in combat occupations.

It is possible that our peer casualty parameterization does not accurately capture which deployment experiences affect outcomes the most. As an alternative approach, Table A.16 reports unbiased estimates of the variance of BCT$\times$quarter-of-arrival assignments' direct effects on several outcomes. These effects measure the reduced-form differences in outcomes across BCTs driven by whatever experiences soldiers in each BCT have instead of forcing effects to operate through deployment and peer casualties as parameterized in the 2SLS models estimated above. 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. The standard deviation of BCT effects on the likelihood of receiving any VADC, for example, is 5.8 p.p. The estimated variance in BCT effects on non-combat deaths, however, is negative (indicating no variance) and its standard error implies confidence intervals that include zero. Sub-setting to BCT$\times$quarter-of-arrival combinations with at least 100 soldiers assigned increases the estimate slightly, but it remains small. The evidence thus suggests that BCT assignment does not affect non-combat deaths in important ways regardless of the type of experiences those soldiers have while in the service.[31]

Overall, the results from our exploration of heterogeneity along exposure to peer casualties 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 disability 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 next explore whether changes in the frequency and combat intensity of deployment over time can explain veteran trends in disability and mortality.

[31] In a constant-effects model where impacts on outcomes flow solely through the impacts of BCT assignments on deployment, the ratio of the standard deviations of effects on outcomes to the standard deviation of effects on deployment yields an asymptotically consistent estimate of causal effects deployment. These estimates are slightly larger, though not necessarily statistically different, than those in our main 2SLS strategy, with estimated effects of a 10-month deployment on any VADC of 14.6 p.p. among BCT$\times$quarters with > 100 soldiers, for example.

5 Explaining trends in veterans' outcomes

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 deterioration in veteran well-being? How does the effect of deployment compare to the potential impact of changes in the composition of servicemen and women?

We examine these questions by augmenting our interacted deployment and casualty specification with a rich set of observable characteristics measured at the time of assignment, denoted X_i, that includes age, race, sex, AFQT and ASVAB subtest scores, moral character waivers, marital status, and educational attainment:

$$Y_i = \delta_{k(i)} + \beta D_i + \gamma\big(D_i \times W_i\big) + X_i'\Gamma + \epsilon_i \tag{8}$$

$$D_i = \omega_{0,k(i)} + \pi_0 Z_i + \rho_0\big(Z_i \times W_i\big) + X_i'\Gamma_0 + u_{0,i} \tag{9}$$

$$\big(D_i \times W_i\big) = \omega_{1,k(i)} + \pi_1 Z_i + \rho_1\big(Z_i \times W_i\big) + X_i'\Gamma_1 + u_{1,i} \tag{10}$$

We estimate this model on all entry cohorts from 2001 to 2011, measuring outcomes eight years after arrival. Since our primary analysis uses cohorts beginning in 2005, 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 helps to estimate coefficients on Γ. The underlying point estimates can be found in Table A.17.

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 cohort for solider i, we then measure the change across cohorts, e.g., $E[Y_i|c_i = 2011] - E[Y_{it}|c_i = 2001]$, the change in average effects of deployment and peer casualties, e.g., $\beta\left(E[D_i|c_i = 2011] - E[D_i|c_i = 2001]\right) + \gamma\left(E[D_iW_i|c_i = 2011] - E[D_iW_i|c_i = 2001]\right)$, and the change in the effects of observables, e.g., $\left(E[X_i|c_i = 2011] - E[X_i|c_i = 2001]\right)'\Gamma$. 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 by their causal effects estimated using our 2SLS strategy, relate to changes in outcomes. Cohort trends in months deployed are plotted in Figure

A.5. The component of changes attributable to X_i captures how selection into service reflected in AFQT scores, moral waivers, and other observable factors explain 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 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. For example, the Veterans' Benefits Improvement Act of 2008 expanded the scope of the disability compensation system and improved the claims process, possibly directly increasing VADC claims. Related policy changes, such as July 2010 policy that eased the evidentiary standards for claiming PTSD (Department of Veterans Affairs, 2010; Broten, 2020) and the VOW to Hire Heroes Act of 2011 that mandated servicemembers leaving the military receive information on VADC benefits (Public Law 112-56, 2013), may have had similar effects.

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, including unobservable selection and policy.

Finally, the implicit constant effects assumption in Equation 8 may be misspecified. The effects of deployment may vary across cohorts, for example, as the nature of their deployments shifted over the course of the wars in ways not captured by the peer casualty interaction. As a result, the explanatory power of the causal effects of deployment for between-cohort changes in outcomes may be either under- or over-stated, something we discuss further below.

To validate and illustrate our approach, we begin with an outcome we expect to be mechanically well explained by changes in deployment and violence: combat injury. The results are presented in Panel (a) of Figure 5. The solid black line 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 blue dashed line shows the changes in combat injuries predicted by our causal effects of deployment and changes in average peer exposure. The gold dashed line shows the changes attributable to change in soldiers' observable characteristics. The red dashed line shows the sum of these two factors.

The results show that combat injuries increased sharply across cohorts, increasingly by nearly 270% from the 2001 to the 2005 cohort, then falling to sub-2001 levels for the 2011 cohort. The blue dotted line shows that the contribution of deployment closely tracks the evolution of 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 (see Table A.18 for point estimates). 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, especially conditional on the fixed effects for military occupations included in our baseline model. Figure A.6 shows that results change little if we use the baseline model without the peer causality interaction, although deployment effects do a slightly worse job of tracking changes in combat injuries, which is as expected. And panel (a) of Figure A.7 shows a similar pattern when using effects on combat deaths instead of injuries to validate the model.

Panel (b) of Figure 5 repeats the same exercise for annual VADC payments (in 2020 dollars), again measured eight years post-arrival. As with combat injuries, soldiers' observable characteristics explain a small share of changes in VADC. While deployment explains an important share of the increase in disability compensation into the mid- to late-2000s, trends diverge with the 2005 cohort. Notably, first term enlistment contracts for these cohorts would have expired over the course of 2008, 2009, and 2010, making them the first group of soldiers exposed to the changes in VADC eligibility criteria near the end of their initial enlistment.[32] Average VADC payments have continued to grow at roughly the same rate since the 2001 cohort, despite a decline in deployment and deployment lethality and hence any causal contribution of deployment itself. Figure A.7 shows that VADC for PTSD shows a similar pattern, though combat deployments explain a larger share of the increase through the 2005 cohort. Afterwards, however, this type of VADC payments also decouples from trends in exposure to combat and continues to grow.

[32] Policy changes to VADC eligibility and generosity generally apply to all claims made after a point in time, not to specific cohorts. It is plausible, however, that policy changes disproportionately affect cohorts who have yet to separate.

Given VADC policy changes can affect all cohorts, Figure A.8 gives a more comprehensive illustration of VADC trends by performing the decomposition in Figure 5 Panel (b) for each cohort and year after arrival, plotting the results in calendar time. The results show that soldiers' observables explain little of VADC changes at all horizons. Deployment effects explain most changes through 2010/11, after which trends begin to diverge.

Finally, Panel (c) of Figure 5 shows a very different pattern emerges for noncombat deaths. The contribution of changes in deployment is effectively flat over the course of the sample, despite a sharp ramp-up in the noncombat deaths that begins to decline only with the 2009 cohort. This pattern reflects the relatively small estimated causal effect of deployments on noncombat deaths. Results change little when looking at specific forms of noncombat death shown in Figure A.7, including suicides and deaths of despair.

Observable selection, however, is a better predictor of changes in noncombat deaths. The pattern in the gold dashed line tracks increases and declines in the outcome, and Table A.18 shows that about 32% of the between cohort variation in noncombat death rates would be predicted based on these characteristics alone. Because these are changes attributable to the observable characteristics available in our data, it seems likely that changes in unobserved characteristics would explain even more of the observed changes over time. Much of the worrying trends in veteran suicides, deaths of despair, and other sources of noncombat mortality would appear to be the result of shifts on who is serving rather than direct effects of the war itself.

Figure A.9 replicates these decomposition exercises using only the main sample of cohorts beginning in 2005, avoiding extrapolation to units and years outside of our sample. We reach very similar conclusions regarding both VADC and mortality trends and, when looking at noncombat deaths, find an even clearer role for observables.

Putting the results together, Figure 5 paints a mixed picture of the drivers of veterans' shifting outcomes since the Global War on Terror began. Exposure to deployment has clearly played a role in outcomes most closely connected to service overseas, including VADC. Yet, the most recent rapid changes in VADC are not the direct result of fighting overseas and instead likely explained by shifts in policy. The surge in veterans' noncombat deaths, on the other hand, appears to largely be an artifact of who was encouraged and allowed to serve over the past two decades. Although our observable characteristics do not explain all of the changes in this outcome, it seems plausible that selection on unobserved risk would explain a meaningful share of the residual gap.

6 Conclusion

Nearly 20 years of war in Iraq and Afghanistan has had a profound impact on the servicemen and women 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 deployment itself is the main driver of the concerning trends in veterans' outcomes. Although the lingering costs of war are responsible for some of the explosion in spending on veterans' disability benefits over the past decade, disability compensation has also risen for the most for recent cohorts who were least exposed to deployment. Deployments also do not appear to explain recent trends in deaths of despair. Instead, relaxed recruiting requirements over the course of the wars may have increased the share of veterans likely to experience these outcomes.

Nevertheless, several important qualifications are warranted. First, we estimate the effects of deployment in a context that is broadly supportive of veterans. The limited adverse effects of deployment on noncombat deaths and other measures of well-being that we document could be the result of the offsetting, positive impacts of VADC payments and other forms of assistance. Whether the effects of deployment would be more deleterious in a less supportive setting is an important open question. Relatedly, given that combat deployments alone cannot explain changes in veteran outcomes, further study of the political economy of waging war and how it affects recruiting and veteran benefit policies is warranted.

Second, although we estimate effects up to 8 years out on a range of key outcomes, we have limited access to measures of veteran health. As a result, we are unable to rule out substantial longer term health consequences of deployment. Future research with additional data could use our research design to quantify the impact of deployment on long-term health. Doing so would help better assess the extent to which VADC adequately insures soldiers for all the risks they undertake while fighting.

Taken together, the results simultaneously demonstrate some of the enormous costs of fighting overseas wars while offering a note of caution against laying too much blame for veterans' outcomes on combat deployment itself. To better support veterans of both past and future wars, it is important to understand a broad set of determinants of veterans' outcomes, as well as the drivers of selection into service.

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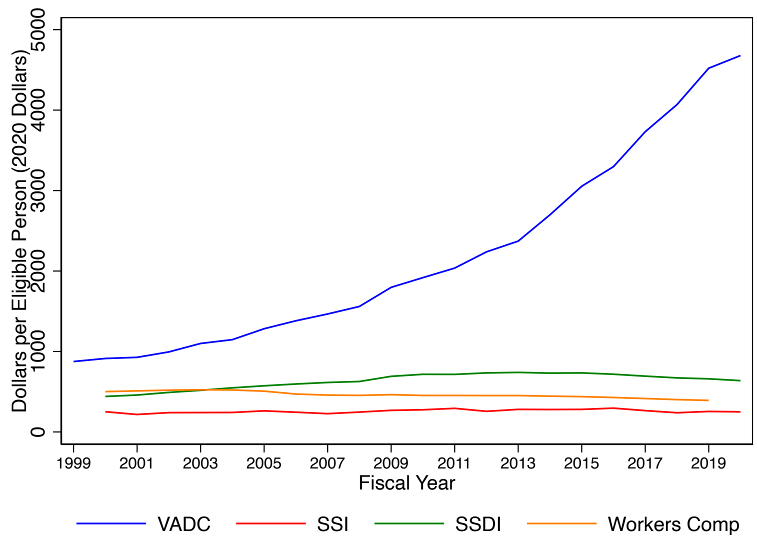
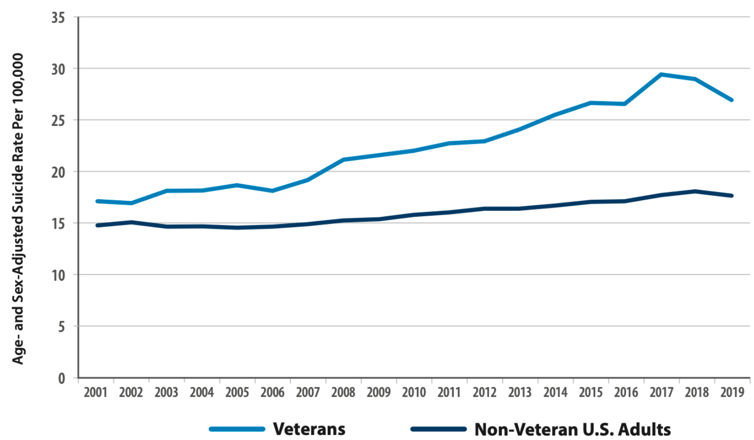
Figures

Figure 1: Trends in veterans' outcomes

(a) Disability Payments Per Eligible Person

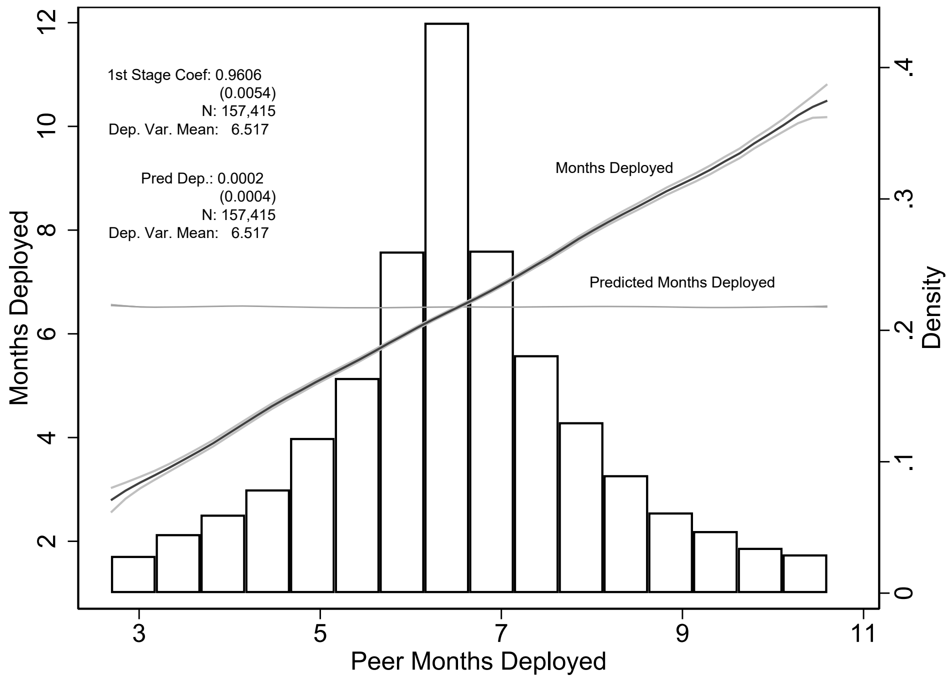


(b) Age- and Sex- Adjusted Suicide Rates



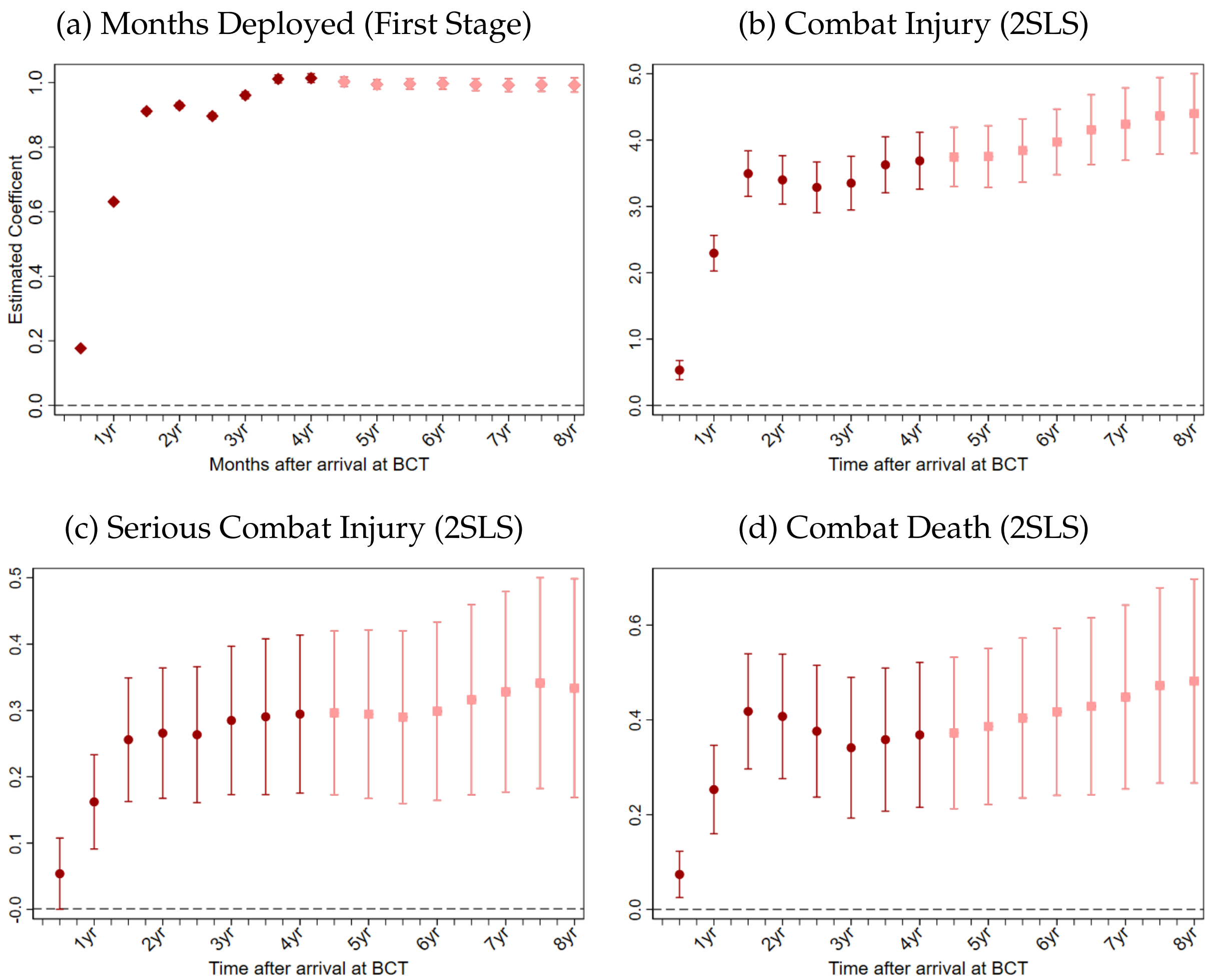
Notes: Panel (a) of this figure plots federal spending per person for several government programs (in 2020 USD). The dark blue line shows total Veterans Affairs Disability Compensation (VADC) payments per living veteran (US Department of Veterans Affairs, 2022). The dark red line shows total federal Supplemental Security Income payments per fully insured worker (Social Security Administration, 2020, 2022b). The green line shows total Social Security Disability Insurance payment per fully insured worker (Social Security Administration, 2022a,b). The orange line shows total Workers Compensation payments per member of the civilian labor force (Murphy et al., 2021; Bureau of Labor Statistics, 2022). Panel (b) is taken directly from a 2021 report written by the Department of Veteran Affairs (Department of Veterans Affairs, 2021).

Figure 2: First stage effects of deployment



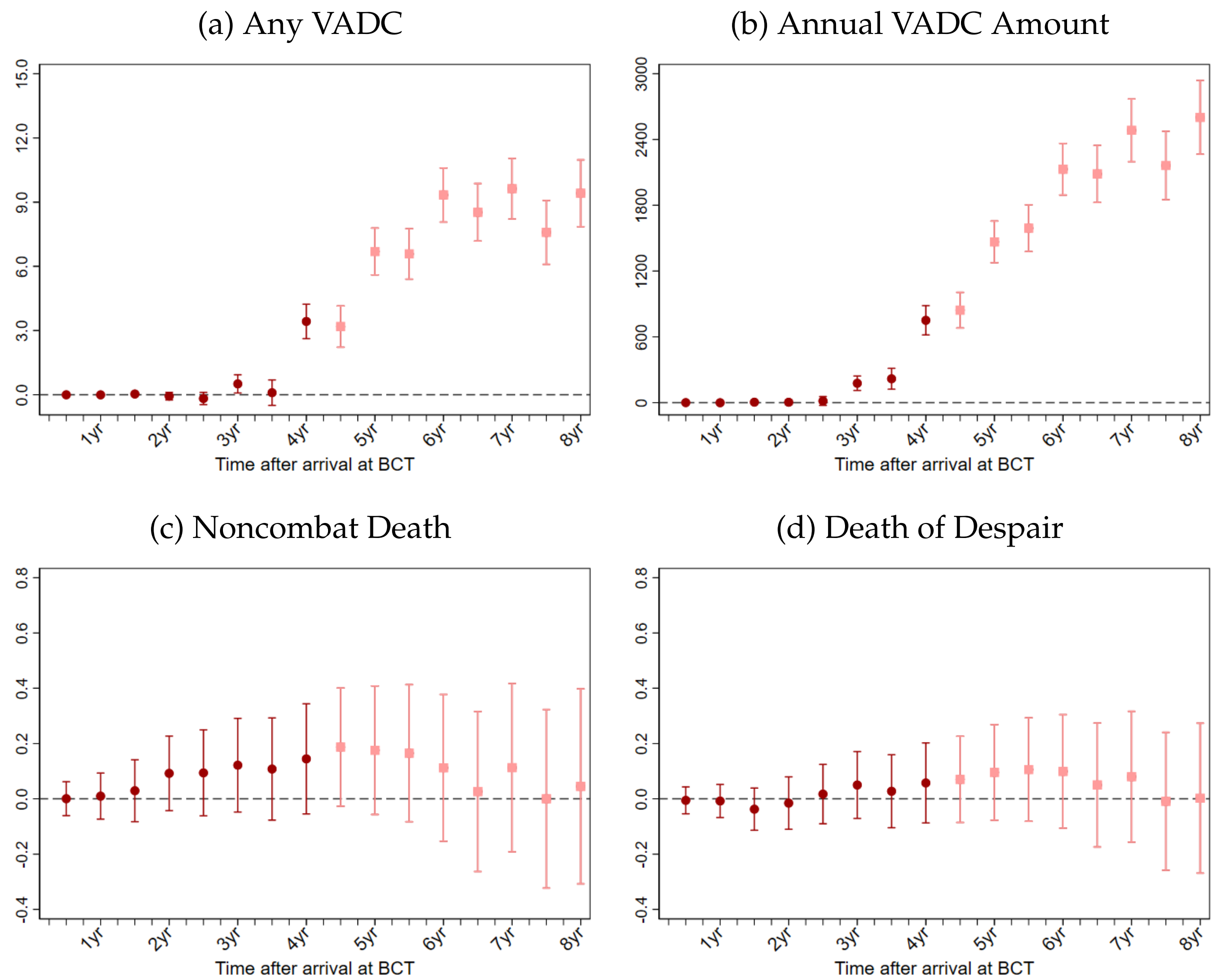
Notes: This figure shows variation in our instrument, our first stage, and covariate balance. We residualize our outcome (months deployed within three years of arrival at the Brigade Combat Team (BCT)) and our instrument (peer months deployed based on BCT and quarter of arrival) on duty-station by job 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 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 is reported in the top left hand corner. The horizontal line shows a local linear regression of predicted months deployed using all the covariates from Section 5 and our baseline fixed effects on our residualized instrument. The top left hand corner reports the coefficient on the corresponding balance regression.

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



Notes: Panel (a) of this figure plots the reduced form relationship between months deployed and our instrument conditional on duty-station by job by initial assignment period by term length fixed effects (Equation 2). In panels (b), (c), and (d), we report the 2SLS effect (Equation 1) of months deployed on combat injuries (i.e. "wounded in action", defined as an injury resulting from an attack against US 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. 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. In all panels, the first red dot reports the coefficient and 95% confidence intervals for the effect of months deployed on the outcome within 6 months after a soldier's arrival (e.g. any combat injuries within 6 months of arrival), the second red dot does so within one year of arrival and so forth. The color of the points changes at 4 years post-arrival, visually denoting where our sample goes from being balanced to unbalanced (e.g., since most of our outcomes are only valid 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).

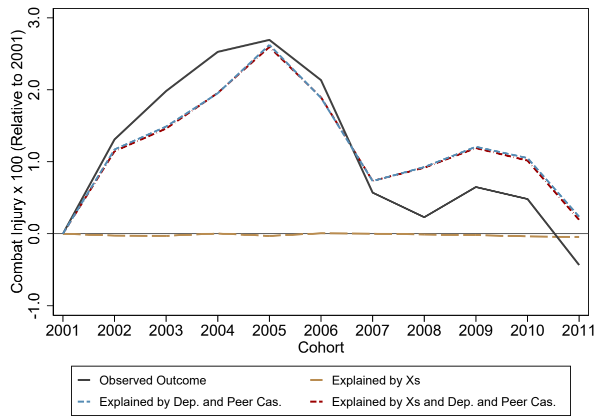
Figure 4: Dynamic effects on disability and mortality



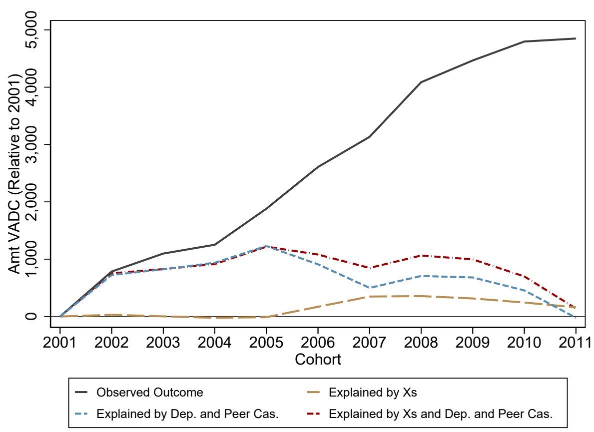
Notes: This figure plots the 2SLS effect (Equation 1) of months deployed on various outcomes of interest. In panel (a), we report the effect of months deployed on receipt of any VADC in the given half-year of arrival. In panel (b), we report the effect on annual VADC payments (in 2020 dollars) in the given half-year of arrival. In panel (c), we report the effect of months deployed on any noncombat death by the given half-year of arrival. This is defined as any fatality as reported in the NDI data excluding combat deaths as defined previously. In panel (d), we report the effect on any death of despair by the given half-year of arrival. Deaths of despair include all suicides (NDI recorded motivation as intentional self-harm or undetermined intent) plus any death caused by a firearm, drug, alcohol, or poison. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. 95% confidence intervals are shown.

Figure 5: Decomposition of veteran outcome trends

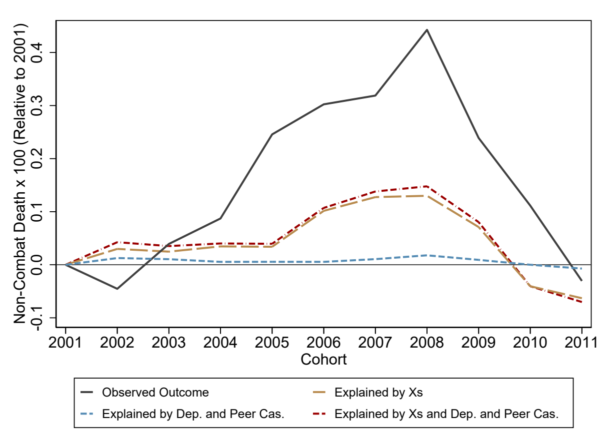
(a) Combat Injury



(b) Annual VADC Amount



(c) Noncombat Death



Notes: This Figure plots cohort trends (among new arrivals) in the stated outcome as a solid black line (normalized to 2001 levels). As described in Section 5, we use estimates of Equation 8 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.). We collapse predicted outcomes to the cohort level and normalize to 2001 levels. The blue dashed lines show how predicted outcomes evolve across cohorts based solely on months deployed and peer casualties. The gold dashed lines show how predicted outcomes based on Xs evolves across cohorts. The red dashed lines show how predicted outcomes based on both Xs and deployment evolve across cohorts. Panel (a) decomposes cohort trends in Combat Injuries 8 years after arrival. Panel (b) does so for Annual VADC Amount 8 years after arrival, and panel (c) does so for any Noncombat Death within 8 years after arrival.

Tables

Table 1: Summary statistics

	Full Sample (1)	Estimation Sample (2)	Never Deployed (3)	Ever Deployed (4)
	Panel (a): Demographics			
Age	21.86	21.80	21.65	21.88
Married	15.39	14.62	14.23	14.82
Black	18.95	14.04	18.14	11.94
Hispanic	12.93	13.05	14.08	12.51
Other race	5.64	5.15	5.49	4.98
Female	15.18	0.00	0.00	0.00
Number of Dependent children	0.48	1.02	1.03	1.01
HS dropout or GED	11.89	12.91	8.73	15.05
HS graduate	76.37	76.67	81.44	74.22
Some college+	11.64	10.37	9.80	10.66
AFQT score	58.86	58.01	56.36	58.86
	Panel (b): Service Experience			
Combat occupation	36.62	64.30	62.18	65.39
Mths deployed w/in 3 yrs	5.98	6.52	0.00	9.87
Combat Injury w/in 3 yrs	1.57	2.24	0.00	3.39
Combat death w/in 3 yrs	0.20	0.29	0.04	0.41
Observations	782,232	157,415	53,425	103,990

Notes: This table reports summary statistics for our two key samples. 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 our primary estimation sample (male soldiers who arrived at a Brigade Combat Team between 2005-2015 along with other more minor sample restrictions as described in Section 1.2 and Appendix B.2). In Column (3) we take our estimation sample from Column (2) and restrict to soldiers who did not deploy within 8 years (or, for the latest cohorts, the last year in our data), while in Column (4) we examine soldiers who did deploy within 8 years. 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)
Dep. Chlidren	0.17	-0.67***
	(0.27)	(0.15)
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	0.34	0.00

Notes: This table reports covariate balance regressions. 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 initial assignment period (year of arrival) by term length fixed effects (as in Equation 2). For ease of interpretation, coefficient and standard errors are scaled by 10, so that they can be interpreted as the effects of being 10 months deployed (a typical deployment length). We fail to reject the null hypothesis that all coefficients are jointly zero. In contrast, in column (2) we report the coefficient from a separate regression of the stated covariate on actual months deployed (again scaled by 10 and conditional on the same set of fixed effects). Heteroskedasticity-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 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 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 the 2SLS effect (Equation 1) 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) and VADC Disability amounts are measured in 2020 dollars. 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. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 4: 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 the 2SLS effect (Equation 1) 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 deaths caused by a firearm, drugs, alcohol, or poison (excluding homicides). 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 5: 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	156,247	156,247	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 the 2SLS effect (Equation 1) 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). Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table 6: 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)
Dep. Chlidren	0.02	0.10
	(0.28)	(0.10)
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	0.44	
P-value on (Dep Inst x Peer Cas) = 0	0.82	
P-value on (Dep Inst) = (Dep Inst x Peer Cas) = 0	0.65	

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 (3.5 percent). 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 7: Effects of violent deployments

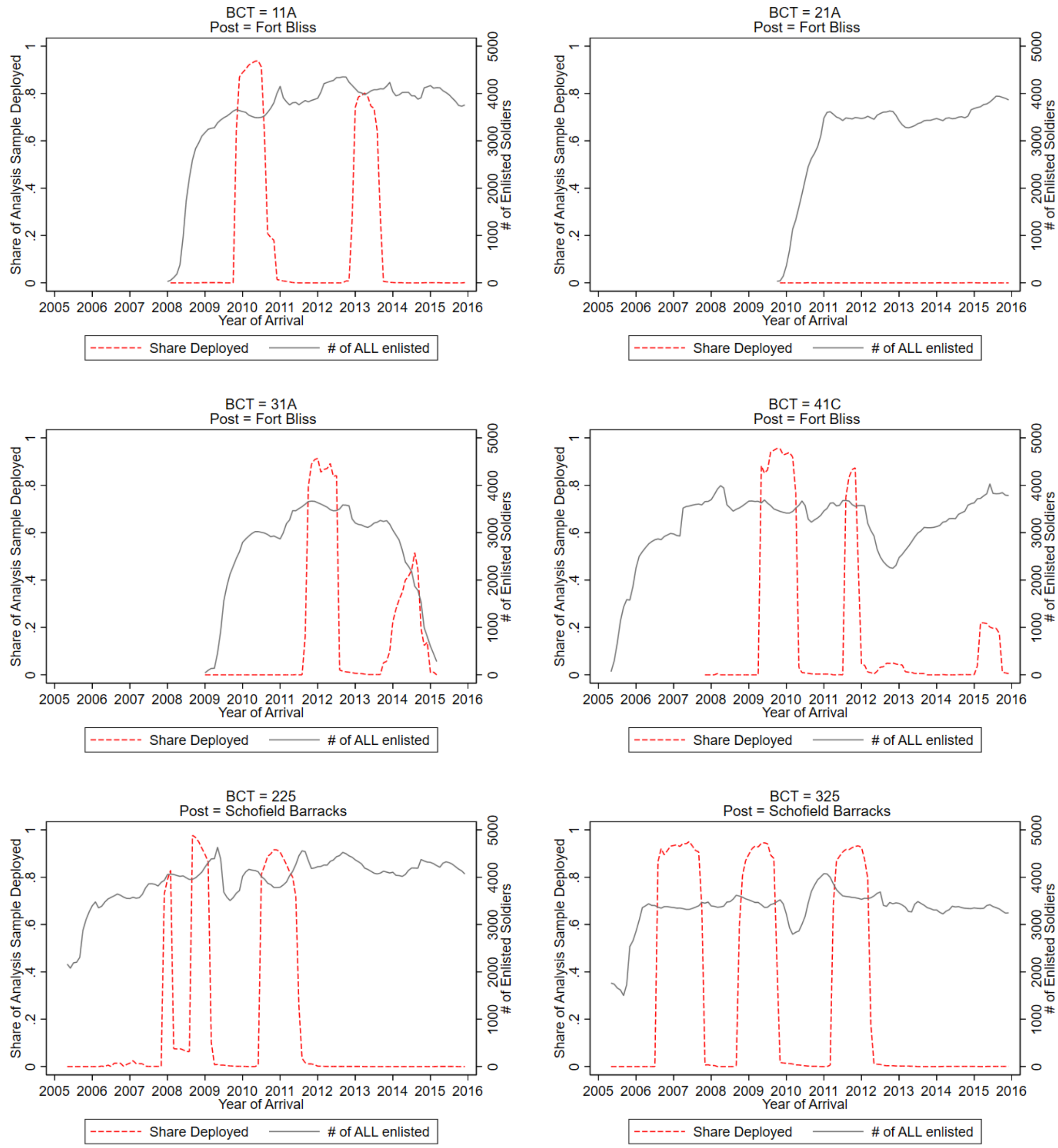
	(1) 10 Months Deployed	(2) (10 Mths Dep) $\times$ (1σ Peer Cas)	(3) Mean
	Panel (a): Trauma and Disability		
Combat Death	0.01	0.27***	0.50
	(0.12)	(0.04)	
Ever Combat Injury	1.16***	1.86***	4.17
	(0.33)	(0.11)	
Significant Army Profile	0.84	1.02***	15.04
	(0.64)	(0.16)	
Annual Amt VADC	1876***	414***	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 5 (with corresponding first stage equations 6 and 7) 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 (3.5 percent). In addition, the sample sizes for incarceration and credit are smaller: 100,381 for Ever Incarcerated and 93,252 for Vantage Credit Score 2020. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Online Appendix

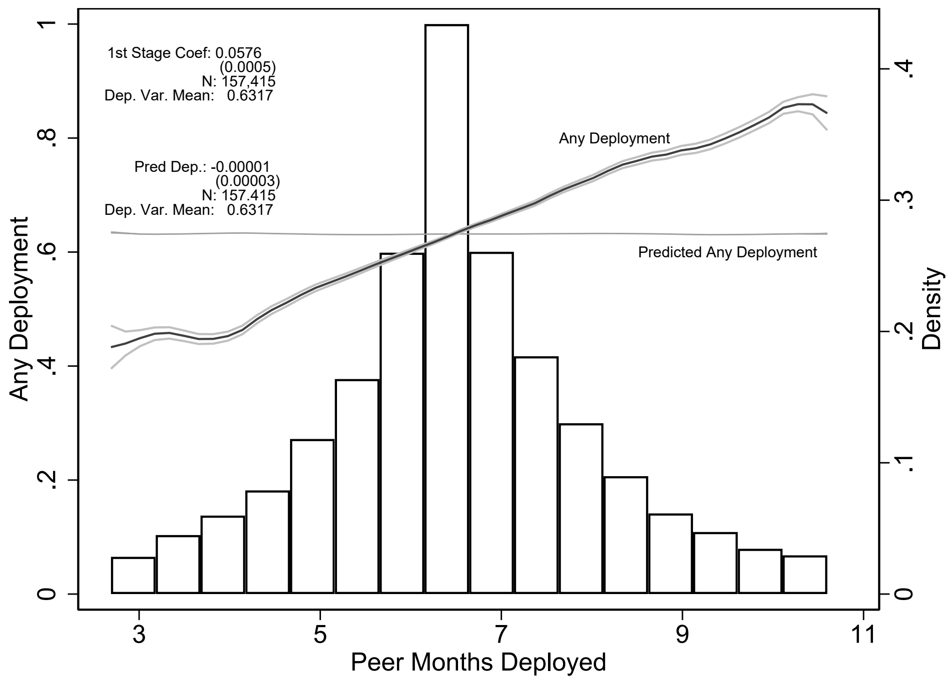
A Additional Results

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).

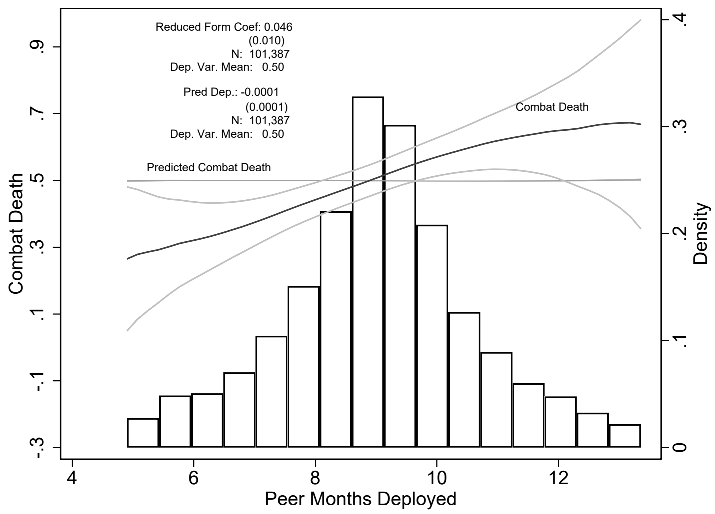
Figure A.2: Any deployment on average peer months deployed



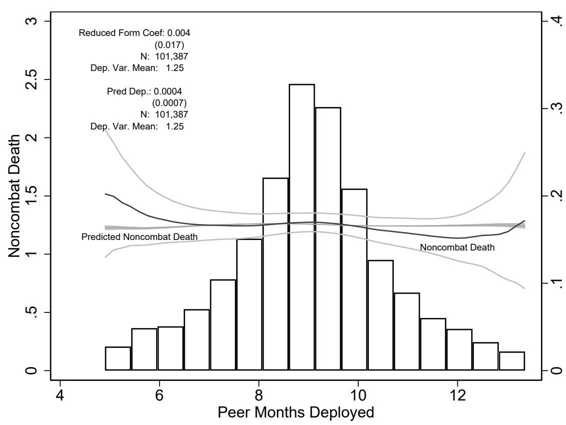
Notes: This figure repeats the exercise in Figure 2, replacing the outcome with any deployment within the first three years after arrival. It shows that our instrument also generates variation in the extensive margin of deployment. See notes to Figure 2 for additional details.

Figure A.3: Reduced form effects of peer months deployed

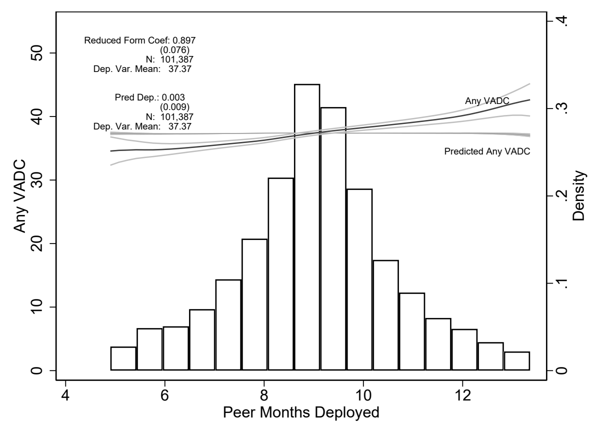
(a) Combat Death



(b) Noncombat Death



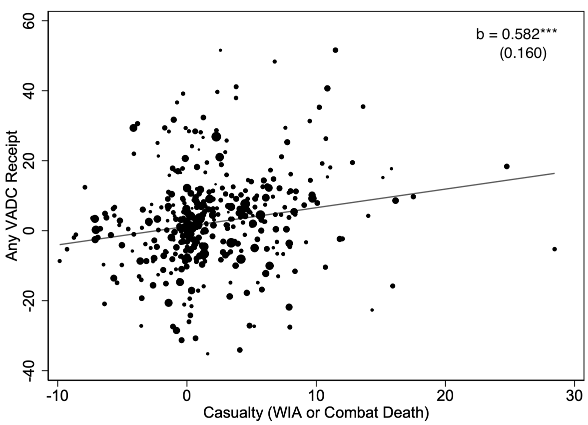
(c) Any VADC



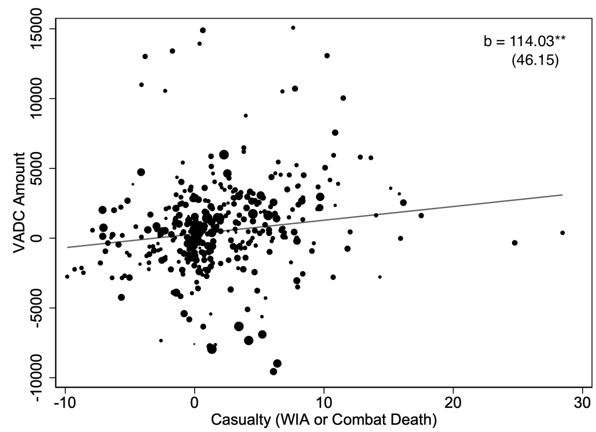
Notes: The figure repeats the exercise in Figure 2, replacing the outcome with Combat Death within 8 years of arrival in panel (a), Noncombat Death within 8 years of arrival in panel (b), and Any VADC Receipt 8 years after arrival in panel (c) for the 2005-2011 cohorts. Coefficients are not scaled by 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.4: BCT-specific effects of deployment on casualties, VADC, and non-combat deaths

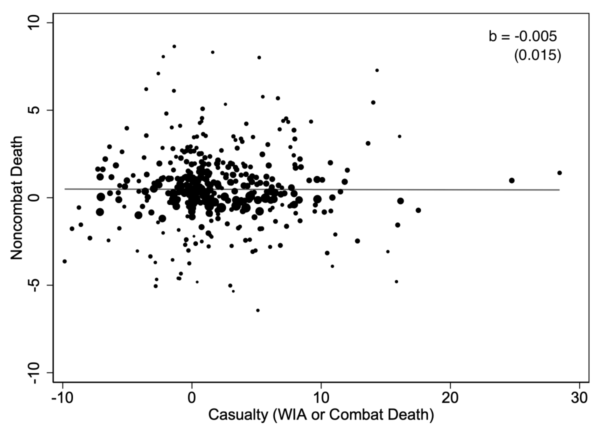
(a) Any VADC



(b) VADC Amount

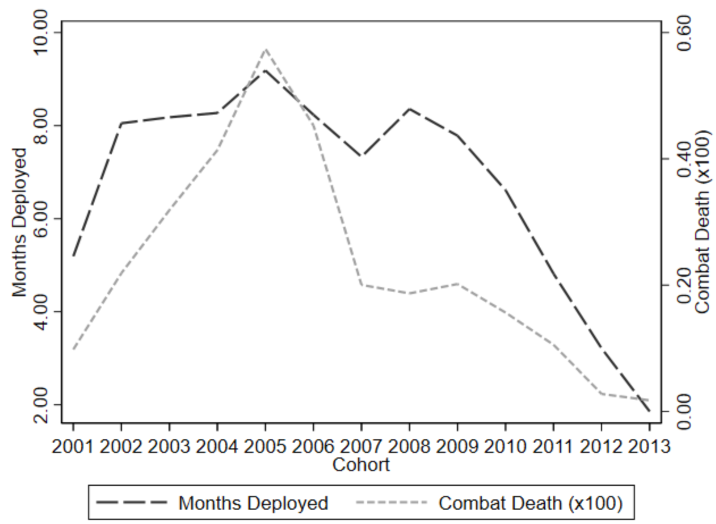


(c) Noncombat Death



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 noncombat 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 non-combat 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.

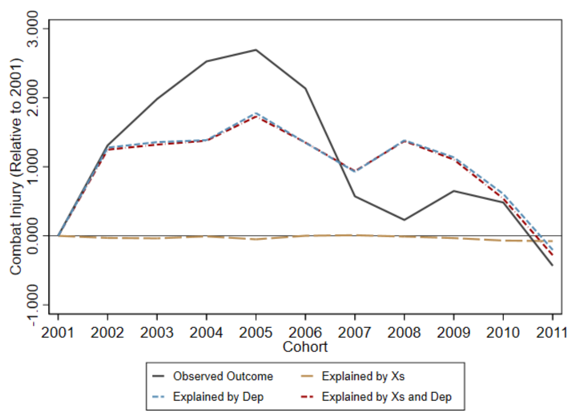
Figure A.5: Deployment and combat death trends by cohort



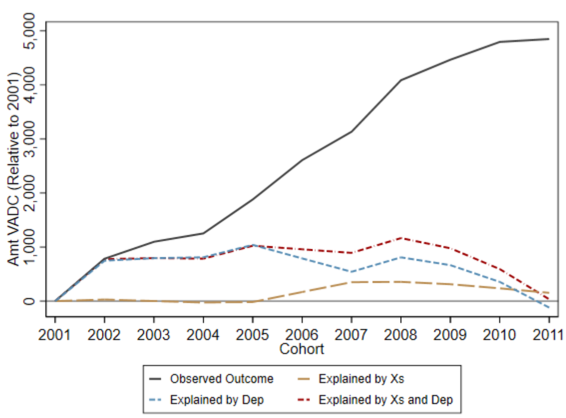
Notes: This figure plots average months deployed (within 3 years of arrival) on the left axis (darker, long-dashed line) and average combat death rates (within 3 years of arrival) on the right axis (lighter, short-dashed line) by year-of-arrival cohort.

Figure A.6: Cohort decomposition without peer casualty interaction

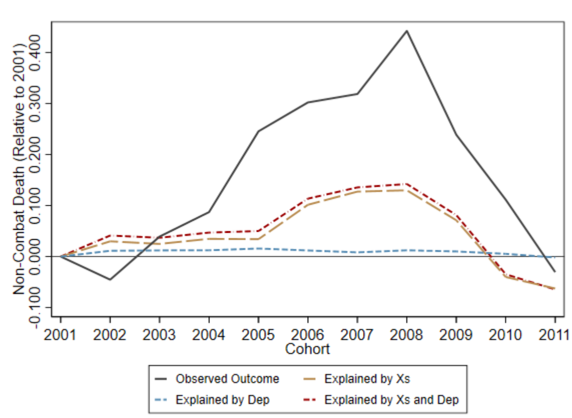
(a) Combat Injury



(b) Annual VADC Amount

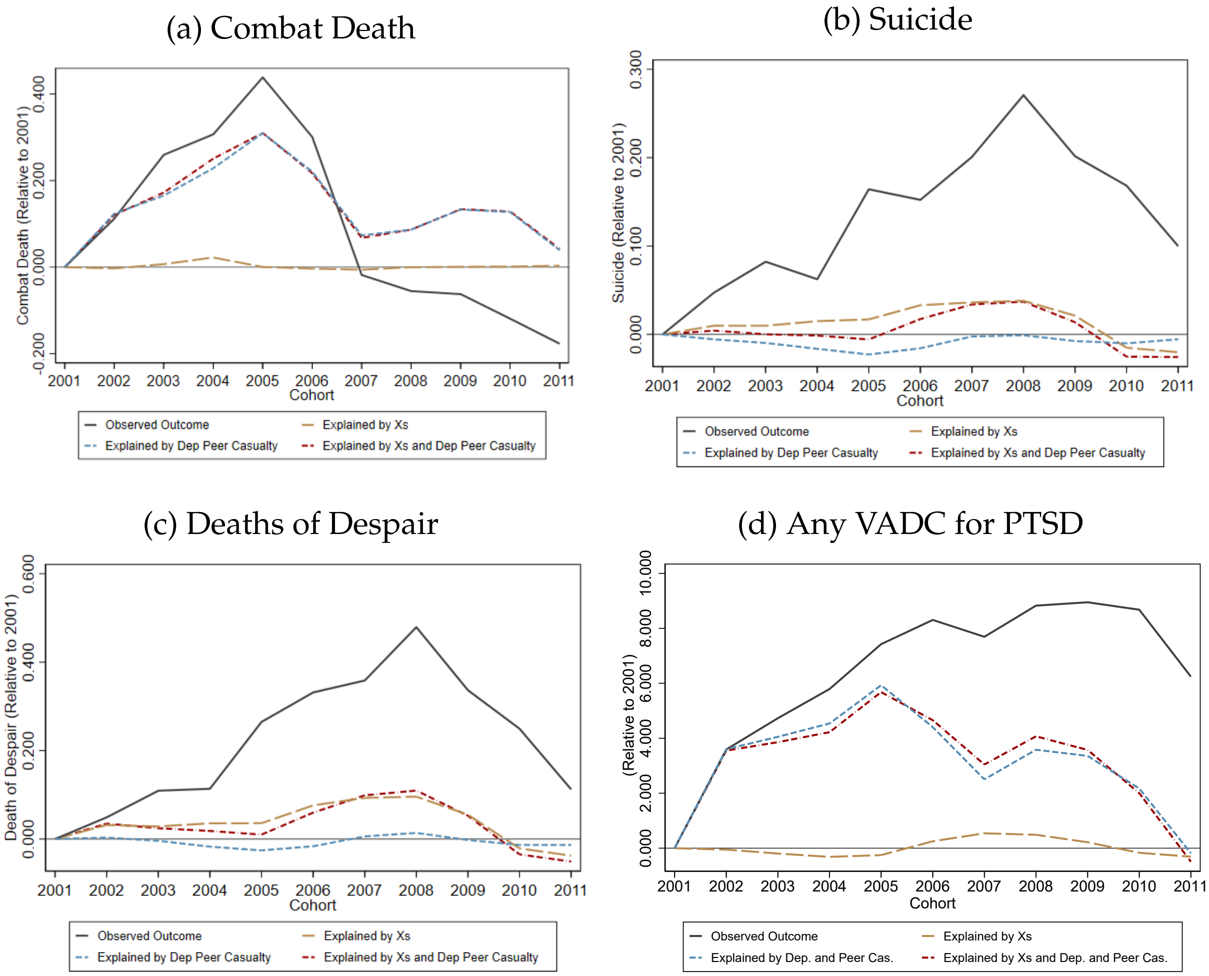


(c) Noncombat Death



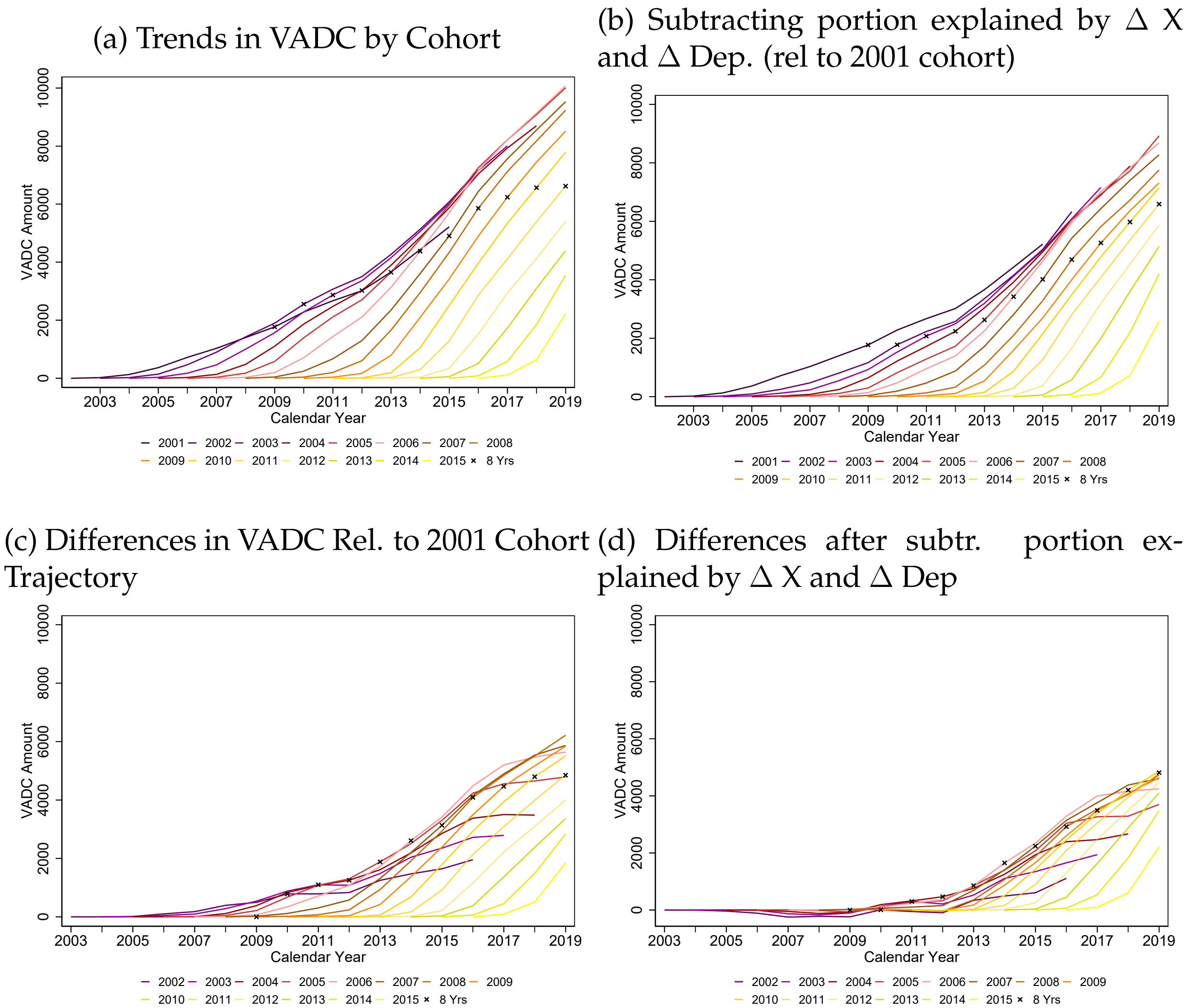
Notes: Like Figure 5, this Figure plots cohort trends (among new arrivals) in the stated outcome and decomposes them into parts we can explain using the specification discussed in Section 5. Here, the decomposition is based off of estimates of Equation 8 that omit the peer casualty interaction.

Figure A.7: Cohort decompositions for additional outcomes



Notes: Like Figure 5, this Figure plots cohort trends (among new arrivals) 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 Suicides within 8 years after arrival, panel (c) does so for Deaths of Despair within 8 years after arrival, and panel (d) does so for Any VADC with PTSD receipt.

Figure A.8: 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)

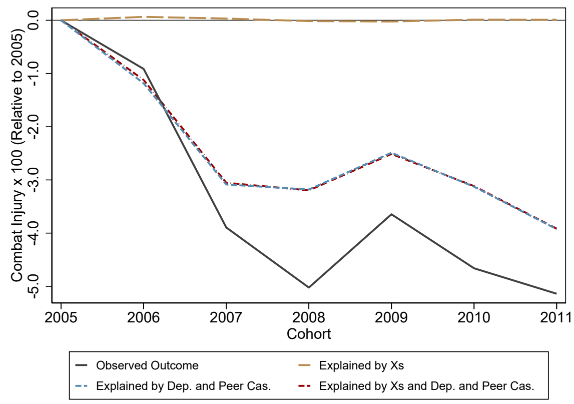
(c) Differences in VADC Rel. to 2001 Cohort Trajectory

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

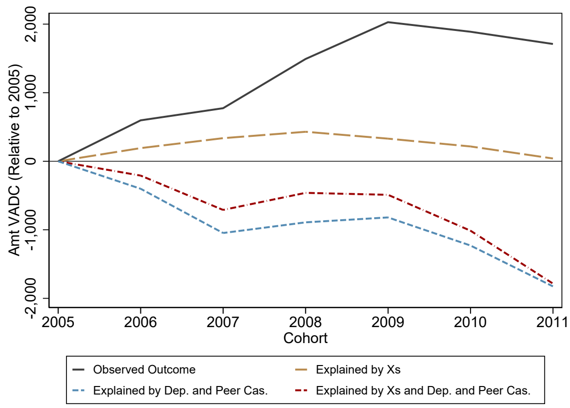
Notes: Panel a) plots average VADC over calendar years for each cohort that arrived at their first operational until between 2001 and 2015. Each series reflects a different cohort and begins 1 year after the cohort arrives at their unit and extends to 14 years after arrival. Panel b) plots average VADC 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 application. 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 a cohort arrived to its Brigade Combat Team.

Figure A.9: Cohort decomposition using analysis sample only

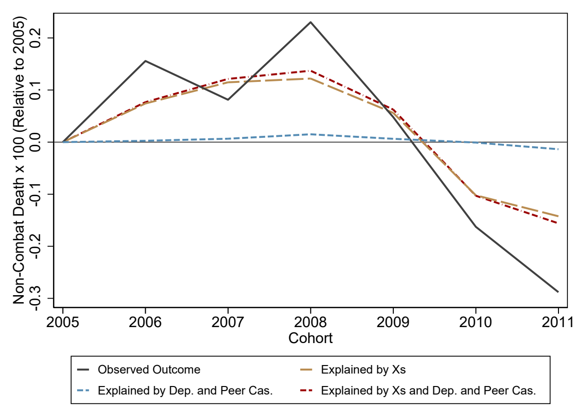
(a) Combat Injury



(b) Annual VADC Amount



(c) Noncombat Death



Notes: In this Figure, we perform the same exercise as we did in Figure 5 but restrict to our primary analysis sample (Brigade Combat Teams, post-2005).

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") in our sample by cohort year. A cohort year is the year that a soldier first enlists in the Army. Most soldiers arrive at their first BCT within a few months of enlisting.

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***	6129.44
	(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 8yrs) and for Vantage Credit Score (93,252 at 8yrs). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.3: 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***	6127.57
	(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): Non-combat 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 Ever Incarcerated (100,381 at 8 yrs) and for Vantage 2020 (93,252 at 8yrs). Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.4: Non-linear effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel (a): Any VADC Receipt				
10 Months Dep.	9.42***	12.05***	5.67		8.04***	7.16***	6.24***	6.09***
	(0.80)	(2.71)	(6.71)		(1.12)	(0.88)	(0.90)	(0.91)
10 Months Dep.^{2}		-0.14	0.72					
		(0.14)	(0.83)					
10 Months Dep.^{3}			-0.03					
			(0.03)					
Any Dep.				13.44***	3.39*			
				(1.42)	(1.99)			
10 Mths Dep. X 1 sd Peer Cas.						1.29***	3.28***	4.09***
						(0.21)	(0.49)	(0.79)
10 Mths Dep. X 1 sd Peer Cas.^{2}							-0.01***	-0.02***
							(0.00)	(0.01)
10 Mths Dep. X 1 sd Peer Cas.^{3}								0.00
								(0.00)
				Panel (b): VADC Amount				
10 Months Dep.	2602.30***	3412.34***	2712.85*		2201.70***	1876.16***	1700.71***	1639.90***
	(171.73)	(570.99)	(1414.05)		(240.40)	(190.42)	(196.31)	(198.19)
10 Months Dep.^{2}		-44.40	50.11					
		(30.52)	(178.80)					
10 Months Dep.^{3}			-3.22					
			(6.03)					
Any Dep.				3738.96***	989.39**			
				(298.60)	(417.09)			
10 Mths Dep. X 1 sd Peer Cas.						414.18***	795.64***	1121.15***
						(47.93)	(111.14)	(180.37)
10 Mths Dep. X 1 sd Peer Cas.^{2}							-1.99***	-5.63***
							(0.53)	(1.73)
10 Mths Dep. X Peer Cas.^{3}								0.01**
								(0.00)
				Panel (c): Noncombat Deaths				
10 Months Dep.	0.05	-0.52	1.38		0.20	0.07	0.06	0.03
	(0.18)	(0.61)	(1.51)		(0.27)	(0.20)	(0.21)	(0.21)
10 Months Dep.^{2}		0.03	-0.23					
		(0.03)	(0.20)					
10 Months Dep.^{3}			0.01					
			(0.01)					
Any Dep.				-0.13	-0.38			
				(0.29)	(0.44)			
10 Mths Dep. X 1 sd Peer Cas.						-0.01	0.00	0.19
						(0.05)	(0.12)	(0.19)
10 Mths Dep. X 1 sd Peer Cas.^{2}							-0.00	-0.00
							(0.00)	(0.00)
10 Mths Dep. X 1 sd Peer Cas.^{3}								0.00
								(0.00)
Observations	101,387	101,387	101,387	101,387	101,387	101,387	101,387	101,387

Notes: This table presents estimates of Deployment and Peer Casualties 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.5: 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.6: Association of deployment injuries with 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 Years			42.52***	42.20***	38.32***
			(0.68)	(0.75)	(0.73)
Combat Injury by 3 Years X Significant Army Profile by 3 Years				-13.02***	-18.39***
				(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 Years			10783.13***	9320.88***	8854.73***
			(248.82)	(244.28)	(253.32)
Combat Injury by 3 Years X Significant Army Profile by 3 Years				5220.51***	4007.58***
				(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.7: 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)	
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 receiving SSI or SSDI which are both work limiting. 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	156,247	156,247	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	0.520	622.102	1.910	655.201
	(1.322)		(1.329)	
FICO Score			0.792	652.1
			(1.488)	
		Panel (b): Debt Composition		
Total debt	3529.2***	44407.3	4793.0**	83783.1
	(1194.2)		(1862.0)	
Mortage debt	2759.3***	27044.3	3985.0**	61122.7
	(1059.1)		(1699.1)	
Auto debt	488.9**	11059.7	738.5***	13064.5
	(220.9)		(264.3)	
Student debt	-138.7	1435.6	-375.4**	2478.2
	(105.8)		(171.7)	
		Panel (c): Bad Debt		
Derogatory debt	52.48	1105.6	-95.06	663.1
	(90.72)		(75.28)	
Debt in colleciton	33.04	983.9	-69.03	1127.0
	(56.33)		(49.81)	
Any bankruptcy	0.134	1.37	0.346*	1.81
	(0.170)		(0.191)	
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
	(0.662)		(0.667)	
Associates Deg+ (Post-Arrival)	0.21	8.99	0.01	15.62
	(0.418)		(0.521)	
Bachelors Deg+ (Post-Arrival)	0.13	4.66	-0.15	9.18
	(0.312)		(0.421)	
Any post 9/11 GI bill use	2.36***	37.66	3.12***	48.27
	(0.70)		(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 controlling for peer casualties

	(1) 10 Months Deployed	(2) (10 Mths Dep) $\times$ (1σ Peer Cas)	(3) Mean
	Panel (a): VADC and Trauma of War		
Combat Death	0.17	0.28***	0.50
	(0.12)	(0.04)	
Ever Combat Injury	2.15***	2.09***	4.17
	(0.32)	(0.12)	
Significant Army Profile	1.35**	1.17***	15.04
	(0.61)	(0.18)	
Annual Amt VADC	2055.76***	500.31***	6129.44
	(181.09)	(52.89)	
Any VADC Receipt	7.67***	1.60***	37.37
	(0.84)	(0.23)	
	Panel (b): Non-combat Mortality Outcomes		
Noncombat Death	0.05	-0.01	1.25
	(0.19)	(0.06)	
Death of Despair	0.06	-0.06	0.79
	(0.15)	(0.05)	
Suicide	0.01	-0.03	0.44
	(0.11)	(0.03)	
Drug- or Alcohol-Rel. Death	0.10	-0.05	0.38
	(0.10)	(0.03)	
Motor Vehicle Death	0.04	0.00	0.27
	(0.09)	(0.02)	
	Panel (c): Misconduct, Credit, and Education		
Separated for Misconduct	-1.02	-0.00	25.05
	(0.74)	(0.20)	
Ever Incarcerated	-0.06	0.14*	2.41
	(0.26)	(0.07)	
Credit Score in 2020 (Vantage)	1.29	0.03	655.78
	(1.66)	(0.46)	
Enrolled by 2020 (Post-Arrival)	1.47*	-0.05	65.64
	(0.81)	(0.22)	
Assc Deg+ by 2020 (Post-Arrival)	-0.38	0.35*	19.51
	(0.69)	(0.19)	
Observations	101,387		

Notes: This table reports estimates of Equation 1 on our primary outcomes 8 years after a soldier arrives at his initial operational assignment, but where we include peer casualty rates as a separate right-hand-side control. This is an alternative to the peer casualty specification discussed in Section 4 that interacts peer casualties with months deployed. Column (1) reports the effect of months deployed while Column (2) reports the effect of peer casualties. 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 (3.5 percent). In addition, the sample sizes are smaller for Ever Incarcerated (100,381) and for Vantage 2020 (93,252). Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Table A.12: Effects of violent deployments on disability diagnoses

	(1) 10 Months Deployed	(2) (10 Mths Dep) $\times$ (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)	
Observations	101,387		

Notes: This table reports 2SLS estimates of Equation 5 (with corresponding first stage equations 6 and 7) on the outcomes in Table A.7. 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 (3.5 percent). 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.13: 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	6202.92	3033.76***	6001.09	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.14: 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	6129.44	6515.79	5910.34	6953.52	6012.65	6497.45	6331.53	6096.78
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.15: Heterogeneity by AFQT, waivers, and race: peer casualty specification

	(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 Base	0.01	0.08	-0.04	0.50	-0.05	-0.34	0.62	-0.04
	(0.12)	(0.19)	(0.16)	(0.42)	(0.13)	(0.27)	(0.48)	(0.15)
Combat Death Peer Cas.	0.27***	0.22***	0.32***	0.06	0.31***	0.24**	0.08	0.30***
	(0.04)	(0.07)	(0.06)	(0.09)	(0.05)	(0.12)	(0.13)	(0.05)
Combat Death Mean	0.50	0.49	0.51	0.51	0.50	0.26	0.61	0.53
Ever Combat Injury Base	1.16***	0.67	1.41***	0.47	1.32***	0.64	0.83	1.37***
	(0.33)	(0.56)	(0.42)	(1.13)	(0.35)	(0.79)	(1.03)	(0.42)
Ever Combat Injury Peer Cas.	1.86***	2.21***	1.72***	1.94***	1.89***	0.78***	1.75***	2.09***
	(0.11)	(0.19)	(0.14)	(0.34)	(0.12)	(0.30)	(0.37)	(0.14)
Ever Combat Injury Mean	4.17	4.25	4.17	4.43	4.17	2.30	4.38	4.55
Significant Army Profile Base	0.84	0.46	1.18	4.06*	0.50	0.64	4.68**	0.73
	(0.64)	(1.09)	(0.82)	(2.09)	(0.68)	(2.00)	(1.98)	(0.78)
Significant Army Profile Peer Cas.	1.02***	1.13***	0.92***	0.68	1.04***	0.55	-0.01	1.23***
	(0.16)	(0.29)	(0.20)	(0.51)	(0.17)	(0.52)	(0.51)	(0.20)
Significant Army Profile Mean	15.04	15.23	14.95	16.79	14.78	13.30	12.63	15.89
Annual Amt VADC Base	1876.16***	2584.95***	1513.25***	2075.25***	1858.86***	2935.30***	2140.50***	1710.97***
	(190.42)	(338.52)	(236.42)	(632.38)	(201.38)	(686.47)	(633.91)	(224.62)
Annual Amt VADC Peer Cas.	414.18***	421.99***	421.10***	365.49**	418.22***	170.53	292.13*	486.64***
	(47.93)	(85.62)	(59.05)	(158.05)	(50.71)	(174.83)	(168.97)	(56.14)
Annual Amt VADC Mean	6129.44	6515.79	5910.34	6953.52	6012.65	6497.45	6331.53	6096.78
Any VADC Receipt Base	7.16***	9.82***	5.69***	8.11***	6.93***	8.40***	9.85***	6.72***
	(0.88)	(1.51)	(1.11)	(2.72)	(0.94)	(2.89)	(2.82)	(1.06)
Any VADC Receipt Peer Cas.	1.29***	1.11***	1.37***	0.67	1.39***	0.31	1.20*	1.57***
	(0.21)	(0.36)	(0.27)	(0.65)	(0.23)	(0.72)	(0.70)	(0.25)
Any VADC Receipt Mean	37.37	38.08	36.99	39.98	37.02	35.59	36.63	38.11
Noncombat Death Base	0.07	-0.22	0.20	0.84	-0.04	-0.88	0.11	0.10
	(0.20)	(0.34)	(0.26)	(0.82)	(0.20)	(0.60)	(0.54)	(0.25)
Noncombat Death Peer Cas.	-0.01	-0.07	0.03	-0.08	-0.02	-0.03	0.02	0.02
	(0.05)	(0.09)	(0.06)	(0.23)	(0.05)	(0.16)	(0.15)	(0.06)
Noncombat Death Mean	1.25	1.15	1.31	2.11	1.12	1.06	0.86	1.36
Death of Despair Base	0.10	-0.08	0.21	0.74	0.02	-0.32	-0.09	0.18
	(0.16)	(0.27)	(0.20)	(0.69)	(0.15)	(0.37)	(0.41)	(0.20)
Death of Despair Peer Cas.	-0.06	-0.11	-0.02	-0.19	-0.05	-0.02	-0.04	-0.05
	(0.04)	(0.08)	(0.05)	(0.20)	(0.04)	(0.12)	(0.12)	(0.05)
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 7 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.16: Estimated variance of direct BCT effects on outcomes

	All BCT $\times$quarter		BCT$\times$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	33.9	5.82	26.96	5.19
	(7.23)	(0.62)	(7.15)	(0.69)
VADC Amt.	3,111,614	1,764	2,778,300	1,669
	(636,957)	(181)	(533,535)	(160)
Non-combat death	-0.024	-	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 $\frac{1}{B}\sum_{b=1}^{B}(\beta_b - \bar{\bar{\beta}})^2 - s_b^2$, where B is the total number of indicators and $\bar{\bar{\beta}}$ is their average. The standard errors shown in parentheses for each estimate are computed as $\frac{1}{B^2}\sum_{b=1}^{B} 2s_b^4 + 4s_b^2\left((\beta_b - \bar{\bar{\beta}})^2 - s_b^2\right)$. 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.17: Regressions underlying cohort decomposition figures

	(1) Combat Injury	(2) Amt VADC	(3) Noncombat Death
10 Months Deployed	1.14***	1851.55***	0.08
	(0.33)	(188.55)	(0.20)
(10 Months Deployed) x (1σ Peer Cas)	1.66***	371.60***	-0.02
	(0.10)	(41.77)	(0.04)
Age at Entry	-0.03***	278.75***	0.00
	(0.01)	(5.17)	(0.00)
White	-0.02	858.79***	0.17***
	(0.11)	(55.58)	(0.06)
Black	-0.27**	822.94***	0.07
	(0.11)	(60.91)	(0.06)
Hispanic	0.11	567.64***	-0.16**
	(0.12)	(63.21)	(0.06)
Female	-0.32***	1518.85***	-0.45***
	(0.08)	(55.07)	(0.05)
Missing AFQT	-2.01*	-2250.21***	0.85
	(1.11)	(652.55)	(0.81)
Any Moral Waiver	-0.26***	321.86***	0.62***
	(0.09)	(49.85)	(0.06)
AFQT	-0.06	-91.27***	0.00
	(0.04)	(21.45)	(0.02)
AFQT^2	0.00	1.50***	0.00
	(0.00)	(0.35)	(0.00)
AFQT^3	-0.00	-0.01***	-0.00
	(0.00)	(0.00)	(0.00)
Married	0.29***	631.42***	-0.16***
	(0.07)	(46.83)	(0.04)
Any Children	0.13	391.98***	0.02
	(0.10)	(67.36)	(0.07)
Any Children	0.05	174.99***	0.41***
	(0.11)	(61.57)	(0.07)
High School Grad	0.05	-70.77	0.02
	(0.08)	(49.39)	(0.05)
MEPS Medical Flag	-0.14*	311.58***	-0.07
	(0.07)	(42.24)	(0.04)
MEPS Alchol Flag	-1.07	421.76	1.24
	(1.92)	(1114.02)	(1.94)
MEPS Marijuana Flag	-0.01	-138.49	-0.19
	(0.21)	(110.65)	(0.16)
MEPS Cocaine Flag	0.02	164.65	0.75**
	(0.37)	(213.36)	(0.30)
MEPS Alcohol Flag Missing	0.07	181.82	-0.19
	(0.25)	(129.43)	(0.16)
MEPS Marijuana Flag Missing	-0.12	-69.00	0.21
	(0.24)	(124.30)	(0.15)
ASVAB CL Score	-0.01	-0.97	-0.01
	(0.01)	(3.74)	(0.00)
ASVAB CO Score	-0.01	-11.06***	-0.01
	(0.01)	(3.38)	(0.00)
ASVAB EL Score	0.01	-0.97	0.00
	(0.01)	(3.45)	(0.01)
ASVAB FA Score	-0.01	-3.58	-0.00
	(0.01)	(2.74)	(0.00)
ASVAB GM Score	-0.00	1.80	0.00
	(0.01)	(2.82)	(0.00)
ASVAB GT Score	0.00	27.05***	0.01*
	(0.01)	(2.77)	(0.00)
ASVAB MM Score	0.01	28.01***	0.00
	(0.01)	(2.95)	(0.00)
ASVAB OF Score	0.01	-2.04	-0.00
	(0.01)	(3.52)	(0.00)
ASVAB SC Score	-0.00	-28.21***	0.00
	(0.01)	(4.53)	(0.01)
Observations	559,026	559,026	559,026

Notes: This table displays the regression coefficients from the regression underlying Figure 5 (Equation 8 as described in Section 5). Instruments are set to 0 for those outside of the analysis sample. Each regression includes our baseline fixed effects, which are further interacted with an indicator for our analysis sample. Missing ASVAB scores or MEPS flags (a rare event) are set to 0 and a full set of dummies for missing scores/flags are included in the regression (but not shown). Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Table A.18: 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	-0.029	-0.015	0.004
Frac. Explained by Xs	-0.011	0.005	
Dep. and Cas.	2.624	-2.387	0.602
Frac. Explained by Dep. and Cas.	0.974	0.763	
	Panel (b): Annual VADC Amt		
Dep. Variable	2608.58	2240.28	
Xs	170.33	-9.71	0.08
Frac. Explained by Xs	0.07	-0.00	
Dep. and Cas.	910.43	-930.88	-0.14
Frac. Explained by Dep. and Cas.	0.35	-0.42	
	Panel (c): Noncombat Death		
Dep. Variable	0.443	-0.473	
Xs	0.130	-0.193	0.318
Frac. Explained by Xs	0.294	0.408	
Dep. and Cas.	0.018	-0.025	0.021
Frac. Explained by Dep. and Cas.	0.040	0.053	

Notes: This Table accompanies Figure 5 and reports measures of how well Xs and Deployment (Dep.) plus Deployment× 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 2006 (the middle cohort as there is no peak), and in panel (c) to 2008 (the cohort with the highest non-combat death rate). 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 × 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.19: 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	0.000	0.003	0.010
Frac. Explained by Xs	0.001	-0.005	
Dep. and Cas.	0.310	-0.271	0.349
Frac. Explained by Dep. and Cas.	0.706	0.440	
	Panel (b): Suicide		
Dep. Variable	0.271	-0.171	
Xs	0.038	-0.058	0.163
Frac. Explained by Xs	0.140	0.340	
Dep. and Cas.	-0.001	-0.005	0.028
Frac. Explained by Dep. and Cas.	-0.003	0.027	
	Panel (c): Deaths of Despair		
Dep. Variable	0.479	-0.367	
Xs	0.096	-0.133	0.235
Frac. Explained by Xs	0.200	0.364	
Dep. and Cas.	0.014	-0.027	0.030
Frac. Explained by Dep. and Cas.	0.029	0.074	
	Panel (d): Any VADC for PTSD		
Dep. Variable	8.306	-2.063	
Xs	0.252	-0.561	0.086
Frac. Explained by Xs	0.030	0.272	
Dep. and Cas.	4.405	-4.587	-0.028
Frac. Explained by Dep. and Cas.	0.530	2.223	

Notes: As in Table A.18, 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 Suicide and Deaths of Despair, and 2006 (midpoint) for any VADC for PTSD. See notes to Table A.18 for additional details.

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.[33] 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

[33] 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.

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 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). 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.5) and deployment increases combat injuries (see Table 3).

B.2 Details on Sample Construction

Our baseline sample consists of 272,613 enlisted soldiers with no prior military service who were assigned to brigade combat teams between 2005 and 2015 immediately following completion of their basic entry training. We exclude 170 soldiers with initial enlistment terms (i.e., the number of years a soldier is contractually obligated to serve at the start of their military service) we cannot observe and another 2,714 soldiers who enlist with an initial enlistment term of fewer than three years since these are special contracts the Army rarely permits and because our instrument, which is based on the average number of months deployed among peers' first three years at their operational unit, will not accurately reflect deployment probabilities for this group. We exclude 4 soldiers who enlist with initial terms of 7 or 8 years (which are most likely data entry years) and 11 soldiers who enlisted with the rank of Sergeant or higher since these are most likely soldiers with prior military service.

We further exclude 10,060 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. We also exclude soldiers assigned to brigade combat teams in Germany, Italy, and Korea (34,924 more observations). 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 4,646 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. For related reasons, we exclude 15,186 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.

We next drop 27,403 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 514 soldiers assigned to BCTs during a year where there are fewer than 100 soldiers 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 1,078 soldiers in Army occupations that are missing, incorrectly coded, or that the Army eliminated (therefore requiring the soldier to change occupations) during the time period of our study. Finally, we exclude 15,375 women because we cannot observe if a soldier is pregnant and HRC career managers could assign pregnant soldiers to BCTs that do not have a pending deployment. Our remaining sample consists of 160,528 soldiers, but 3,071 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.