The Effects of Combat Deployments on Veterans' Outcomes

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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 the causal contribution of combat deployments to declining veteran well-being. Deployments increase injuries, combat deaths, and disability compensation, but we find limited effects on suicide, deaths of despair, financial health, incarceration, or education. Our estimates suggest that deployment cannot

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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.

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

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

Many lay the blame for these worrisome trends on the long-run behavioral and health consequences of combat (e.g., Stiglitz and Bilmes 2008; Tanielian and Jaycox 2008; Bilmes 2021).[1] A majority of the public remains concerned that not enough is being done for veterans harmed by war (Frankovic 2021). However, attributing deteriorating veteran outcomes to the causal effects of combat exposure is difficult because of many other changes over this period. In response to recruiting shortfalls over the 2000s, for example, the US Army permitted soldiers to enlist with

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

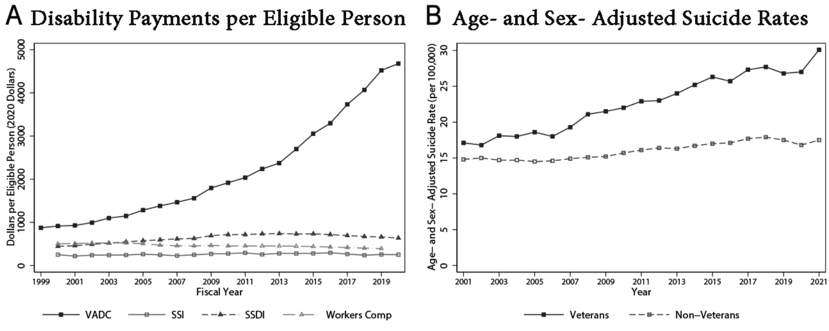

FIG. 1.—Trends in veterans' outcomes. *A*, Federal spending per person for several government programs (in 2020 USD). VADC = total VADC payments per living veteran (VA 2022); SSI = total federal Supplemental Security Income payments per fully insured worker (SSA, 2020, 2022b); SSDI = total SSDI payments per fully insured worker (SSA, 2022a, 2022b); Workers Comp = total Workers Compensation payments per member of the civilian labor force (Murphy et al., 2021; BLS, 2022). *B*, Age- and sex-adjusted suicide rates for adult veterans and nonveterans in the United States (VA 2023; for precise values, see https://www.mentalhealth.va.gov/suicide_prevention/data.asp).

lower Armed Forces Qualification Test (AFQT) scores and granted more waivers for prior felony convictions (Murphy 2019; DOD 2020). Policy changes also eased access to VADC (CBO 2014).

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

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

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

While deaths and injury sustained in conflict may be mechanically connected to deployment, noncombat deaths, including suicides and drug overdoses, are not. Our point estimates suggest that an average deployment has limited effects on these outcomes. The estimated effect on overall noncombat deaths within 8 years of enlistment is 0.05 pp. For deaths of despair, which primarily comprise suicide and drug- or alcohol-related deaths, the estimated effect is 0.002 pp. As a result, deaths that occur as a direct result of combat explain 91% of the overall 0.53-pp mortality effect. However, estimated effects on noncombat deaths are relatively imprecise. The 95% confidence intervals cannot rule out a 0.40-pp increase overall (32% of the mean) and a 0.27-pp increase in deaths of despair (34% of the mean).

To better understand the potential adverse effects of deployment, we conduct two additional analyses. First, we exploit the fact that some soldiers are assigned to brigades that experience more intense and regular violence while deployed to analyze whether more dangerous deployments lead to worse outcomes. Soldiers assigned to brigades with higher casualty rates have increased risk for combat death and injury and receive more VADC. However, we find that they are no more likely to die outside of combat. Importantly, large variation in casualty rates among units that deploy results in substantial power advantages relative to our analysis of the effects of the average deployment. For example, we can rule out that a 1 standard deviation increase in casualty rates increases noncombat mortality within 8 years by 0.09 pp (7% of the mean) and deaths of despair by 0.02 pp (3% of the mean) conditional on deploying for the same length of time.

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

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

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

Despite a large, multidisciplinary literature on the effects of military service, causal evidence on modern combat deployments remains scarce. Several papers explore the effects of compulsory and voluntary military service on earnings, education, disability, and mortality in the United States and elsewhere.[2] Related research has focused on specific aspects of service,

[2] See, e.g., Angrist (1990, 1998); Imbens and Klaauw (1995); Angrist, Chen, and Frandsen (2010, 2011); Card and Cardoso (2012); Bingley, Lundborg, and Lyk-Jensen (2020); Greenberg et al. (2022).

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

Our results also speak to a large literature on the varied legacies of exposure to violence among civilians and armed group participants. These studies explore how war affects a variety of outcomes, including social behavior, voting, preferences, future violent acts, education, and financial decisions, mostly in the context of developing countries (e.g., Blattman 2009; Blattman and Annan 2010; Chamarbagwala and Morán 2011; Leon 2012; Callen et al. 2014; Bauer et al. 2016; Brown and Velásquez 2017; Moya 2018; Brown et al. 2019; Brück, Di Maio, and S. H. Miaari 2019; Couttenier et al. 2019; Jakiela and Ozier 2019; Blumenstock et al. 2024). We present new evidence on this question from the perspective of professional soldiers from the United States fighting abroad. Even for the most violent deployments, we find limited effects outside of combat death/injury and VADC, suggesting that the context in which one is exposed to violence (e.g., who is exposed, in what capacity, and the support networks they can access) likely matters.

Finally, we contribute to a nascent literature on VADC, a program designed to insure US soldiers against injuries incurred or aggravated while in service.[5] With real annual expenditures increasing from $30 billion to $133 billion since 2002, VADC is now 94% as large as SSDI despite

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

[4] Other studies explore how combat deployments impact families and children (Angrist and Johnson 2000; Lyle 2006; Engel, Gallagher, and Lyle 2010). Bäckström and Hanes (2024) find no effect of Swedish peacekeeping deployments on earnings after accounting for selection.

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

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

I. Data and Summary Statistics

A. Administrative Military Personnel Records and Outcome Data

Our data combine army personnel records with administrative data on disability and mortality, allowing us to observe outcomes that extend beyond an individual's time in the military. We also link to additional data with national coverage of criminal, credit, and education outcomes. Our army data include soldier demographic characteristics, 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 paid only to soldiers who serve in a combat zone. Although our records do not reveal the precise location of deployments, combat deployments to locations other than Iraq and Afghanistan were incredibly rare for soldiers assigned to brigade combat teams (BCTs) during the period we study.

Our disability data combine payments for VADC with payments for SSDI and Supplemental Security Income (SSI). All payment amounts reflect annual payments adjusted for inflation to 2020 USD using the consumer price index for all urban consumers. Mortality data from the National

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

Death Index (NDI) contain the date and cause of death for deceased soldiers. We also link to incarceration data from Lexis-Nexis and army records; additional misconduct and criminal outcomes from army personnel and criminal records; credit data from Experian Credit Bureau; and postsecondary 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 (apps. A and B are available online) contains additional information on our data.

B. Sample Construction

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

C. 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 2001 and 2015. Column 2 reports averages after restricting to male soldiers who arrived at their first units between 2005 and 2015. Column 3 shows averages for our estimation sample. Relative to the average first-term male soldier who enlisted between 2005 and 2015, a soldier in our sample is less likely to be Black, has a lower average level of education, and has a slightly lower average AFQT score. These differences are largely driven by occupational differences that

[7] Women constitute roughly 9% of first-term soldiers in BCTs. We exclude them because women were precluded from serving in combat occupations during the period we study. Prior to 2012, women assigned to BCTs were only permitted to be assigned to brigade headquarters or support battalions. Women are also not permitted to deploy while pregnant, and our data do not permit us to observe all cases of pregnancy. Nevertheless, including women in our sample yields similar estimates (see sec. III.F).

[8] See table A.1 (tables A.1–A.30 and B.1 are available online). Appendix B.2 contains additional details on sample construction.

TABLE 1
Summary Statistics

	2005–15			Never Deployed	Ever Deployed
	Full Sample (1)	Male Soldiers (2)	Estimation Sample (3)	Estimation Sample (4)	Estimation Sample (5)
A. Demographics:					
Age	21.86	21.98	21.80	21.65	21.88
Married	15.39	15.38	14.62	14.23	14.82
Black	18.95	16.41	14.04	18.14	11.94
Hispanic	12.93	12.79	13.05	14.08	12.51
Other race	5.64	5.48	5.15	5.49	4.98
Female	15.18	.00	.00	.00	.00
Number of dependents	.33	.32	.31	.30	.32
High school dropout or GED	11.89	12.32	12.91	8.73	15.05
High school graduate	76.37	75.68	76.67	81.44	74.22
Some college+	11.64	11.94	10.37	9.80	10.66
AFQT score	58.86	59.47	58.01	56.36	58.86
B. Service experience:					
Combat occupation	36.62	42.57	64.30	62.18	65.39
Months deployed within 3 years	5.98	5.77	6.52	.00	9.87
Combat injury within 3 years	1.57	1.65	2.24	.00	3.39
Combat death within 3 years	.20	.20	.29	.04	.41
Observations	782,232	483,367	157,415	53,425	103,990

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

emerge from our restriction to soldiers whose initial assignment is to a BCT. Panel B, for example, shows that 64% of soldiers in the estimation sample work in combat occupations, compared to only 43% of male soldiers assigned to all units between 2005 and 2015.

Column 4 restricts our estimation sample to the subset of soldiers who did not deploy. Column 5 restricts to soldiers who deployed. These columns reveal that soldiers who deploy are less likely to be Black, less likely to be Hispanic, have lower levels of education, and have higher AFQT scores. What drives this selection? First, soldiers in combat occupations are more likely to deploy. This is true even conditional on initially being assigned to a BCT. Many soldiers rotate to a new unit after 2–3 years, and soldiers in noncombat jobs are more likely to rotate to a unit with a low propensity to deploy. Second, changes in deployment rates over time may be correlated with changes in the composition of soldiers. Finally, when BCTs deploy, unit commanders have discretion in determining which soldiers to leave behind in garrison as a "rear detachment" that facilitates administrative matters. Soldiers who perform poorly in training or misbehave are more likely to remain in this rear guard. We also expect that soldiers with extenuating personal or family circumstances may be more likely to be left behind. Differences in the observed and unobserved characteristics of soldiers who do and do not deploy motivates the instrumental variables (IV) strategy we describe next.

II. Identifying the Causal Effects of Deployment

A. *"Faces-to-Spaces": The Soldier-Unit Assignment Mechanism*

After completing initial training, soldiers are assigned to their first unit primarily on the basis of army personnel requirements (US Army 2019a). The starting point of this process is the army's demand for soldiers, as determined by personnel-structure documents that identify the number of soldiers required in each unit within each occupation and rank and by senior leader guidance on how to manage personnel shortages. The US Army's Human Resources Command (HRC) compares the demand for soldiers to the current supply, expected attrition, and expected training requirements to project entry-level soldier vacancies at the brigade × occupation level 7–18 months into the future.[9]

The "faces-to-spaces" system matches first-term soldiers with projected vacancies given a soldier's military occupational specialty (MOS) and occasionally soldier-specific factors (e.g., if a soldier is married to another

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

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).[10] But given the hierarchical, command-and-control nature of military service, low-ranking, first-term soldiers have virtually no ability to influence which specific brigade they are assigned within a given duty station. Variation is driven by idiosyncratic choices made by the HRC career manager, who must make hundreds of assignment decisions each month in addition to their other duties. As a result, brigade assignments are as good as random conditional on MOS, duty station, contract term length, and arrival time.[11] In support of this claim, we show evidence of balance on observable characteristics in section II.E.

B. The ARFORGEN Model

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

ARFORGEN also aimed to create a regular, cyclical deployment time line designed to ensure that 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 fig. A.1 for an example; figs. A.1–A.15 are available online). 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.

[10] Specific details on the assignment process come from Army Regulation 614–200 (US Army 2019a), knowledge acquired through conversations with HRC officials, and the first-hand work experience of a member of this research team (Greenberg, who was recently assigned to HRC for his military assignment).

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

C. Research Design

Our rich data make it straightforward to account for changes in observable characteristics across cohorts and selection into different military occupations. However, simple controls for soldier characteristics will not isolate the causal effects of combat deployments because soldiers are not randomly sent to war. As discussed in section I.C, 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. At each point in time, each 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 soldier assigned to a BCT that has just returned from overseas.

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

D. Empirical Implementation

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

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

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

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

[12] There is some ambiguity regarding the relevant time horizon that assignment officers consider when making soldier-unit matches. Our preferred model uses a relatively large window to leverage as much variation as possible. However, we obtain similar results using

Our treatment variable, D_i, measures the number of months that soldier i spent deployed within 3 years of arrival at his initial duty station. Three years is the modal enlistment term length and hence a natural period over which to capture a first-term soldier's deployment exposure. Measuring deployment over other time horizons (e.g., 8 years) yields similar results, as discussed below.

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

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

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

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

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

where

$$\omega_d = \frac{\Pr(D_i(1) \geq d > D_i(0))}{\sum_{l=1}^{\bar{D}} \Pr(D_i(1) \geq l > D_i(0))}.$$

With a multivalued instrument, the estimand averages these averages of causal effects along the support of the instrument.

We therefore interpret our estimates as capturing a combination of extensive-margin effects (i.e., no deployment vs. some) and intensive-margin effects (i.e., some deployment vs. more).[13] Figure A.2 reports estimates

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quarter of arrival instead. Moreover, including covariates, such as demographics, AFQT, any waiver for a prior felony conviction or other disqualifying conduct, high school graduate, family status, ASVAB composite line scores, and medical and drug testing results, does not impact our estimates, consistent with the balance results discussed below.

[13] Our specification also includes a saturated set of control variables in the form of the fixed effects, but no interactions between the instrument and controls. Such models capture positively weighted averages of causal effects for compliers when (roughly) the first-stage

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

We therefore view our 2SLS estimates using length of deployment as the endogenous variable as informative about the effects of "any deployment" relative to none. Throughout the analysis, we refer to estimated causal effects—scaled by a factor 10 to capture the mean deployment length—as the impact of an average deployment. However, we also explore models with multiple endogenous variables that allow for nonlinear effects of deployment. As we show below, these models find similar implied effects of receiving 10 versus 0 months of deployment to our rescaled primary 2SLS estimates.

E. *Instrument Validity*

Table 2 presents evidence to support the assumption that soldiers are as good as randomly assigned to brigades conditional on our set of fixed effects. Column 1 reports estimates of equation (2), where the outcome has been replaced with pretreatment soldier characteristics. The leave-out months-deployed instrument is not correlated with individual covariates and does not jointly predict them (the p-value of a joint F-test is .27). Figure 2 presents further evidence of covariate balance by nonparametrically regressing predicted months deployed, formed using a regression on all available exogenous covariates, on the instrument. Variation in the instrument itself is plotted in the histogram.[14] We see no relationship between our instrument and predicted months deployed, despite a strong relationship with actual months deployed.

Column 2 of table 2 presents naive ordinary least squares regressions of pretreatment soldier characteristics on the months-deployed treatment variable and our baseline set of fixed effects. In contrast to the balance on our leave-out months-deployed instrument, actual months deployed is strongly conditionally correlated with soldiers' characteristics. Among soldiers in

effect has the same sign for all covariate groups (Blandhol et al. 2022), a natural restriction in our setting.

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

TABLE 2
Covariate Balance

	Deployment Instrument (1)	OLS with FE (2)
Black	.55	−1.39***
	(.43)	(.20)
Hispanic	−.38	1.51***
	(.46)	(.20)
Other race	.02	.76***
	(.29)	(.13)
Married	.13	−1.40***
	(.47)	(.22)
Number of dependents	.01	−.04***
	(.01)	(.01)
HS graduate+	.48	3.35***
	(.42)	(.22)
Age	.08	−.06***
	(.05)	(.02)
AFQT	−.28	.83***
	(.22)	(.10)
Observations	157,415	157,415
p-Value on joint test	.27	.00

Note.—This table reports the results of tests for covariate balance. Each row in col. 1 reports the coefficient from a separate regression of the stated covariate on our instrument (peer months deployed within 3 years). Regressions include duty-station × job × arrival-year × term-length fixed effects, as in eq. (2). For ease of interpretation, coefficients and standard errors are scaled by 10, so that they can be interpreted as the effects of a typical-length deployment. We fail to reject the null hypothesis that all coefficients are jointly 0. For comparison, each row in col. 2 reports the coefficient from a separate regression of the stated covariate on the endogenous variable months deployed (again scaled by 10 and conditional on the same set of fixed effects). The results show strong evidence of selection. Robust standard errors are reported in parentheses. OLS = ordinary least squares. FE = fixed effects.
*** Significant at the 1% level.

the same occupation, duty-station, term-length, and arrival-year group, those with high school diplomas and higher AFQT scores deploy on average for longer, while soldiers who are married, have more dependents, or are older deploy less. These patterns of observable selection are consistent with nonrandom deployment even within occupation and unit, potentially as a result of commanders' ability to select which soldiers to bring to war.

For BCT assignment to serve as a valid instrument, it must also satisfy an exclusion restriction.[15] In our setting, exclusion requires that assignment to different BCTs affects outcomes only through the quantity of time spent deployed. While it is possible that individual BCTs may also directly

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

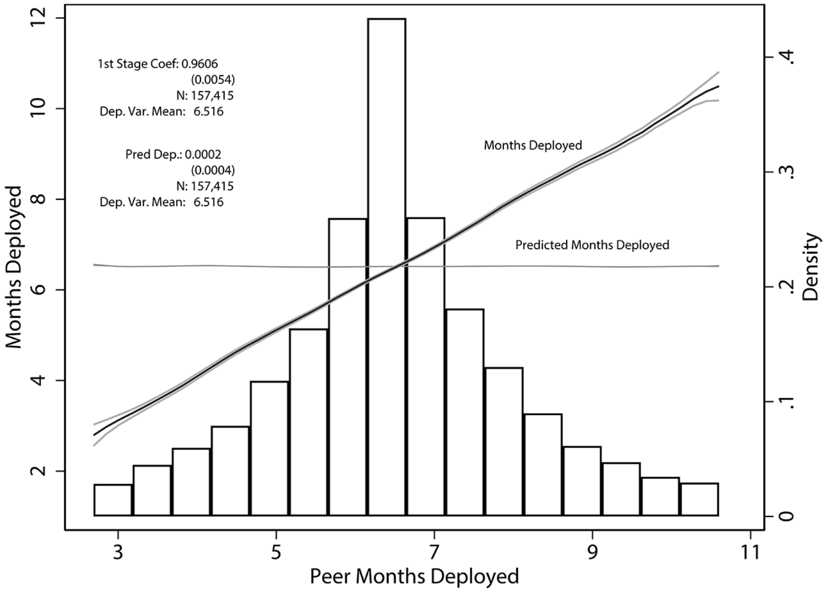

FIG. 2.—First-stage effects on deployment. The figure shows the variation in our instrument, our first stage, and covariate balance. We residualize our outcome (months deployed within 3 years of arrival at the BCT) and our instrument (peer months deployed based on BCT and quarter of arrival) on duty-station × job × initial-assignment-period × term-length fixed effects. The histogram of our residualized (and recentered at the sample mean) instrument is shown in the background. We drop the bottom and top 2.5 percentiles of the instrument for the figure, but not for the reported regression coefficients. The upward-sloping curve shows a local linear regression of residualized months deployed on our residualized instrument and associated 95% confidence intervals. The first-stage coefficient and standard error are reported in the top left corner. The horizontal line shows a local linear regression of predicted months deployed on our residualized instrument. Predicted months deployed is constructed from our baseline fixed effects and the following soldier-level covariates (all measured when soldiers enlist): age, white, Black, Hispanic, female, linear, quadratic, and cubic terms for AFQT (and an indicator for rare situations where soldiers are missing AFQT scores), any waiver for a prior felony conviction or other disqualifying conduct, married, number of dependents, high school grad, an indicator for requiring a medical waiver, indicators for failing alcohol, marijuana, or cocaine drug tests, and Armed Services Vocational Aptitude Battery (ASVAB) composite line scores (clerical, combat, electronics, field artillery, general maintenance, general technical, mechanical maintenance, operators and food, and surveillance and communications). The top left hand corner reports the coefficient on the corresponding regression.

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 produces rigorously standardized stateside training sequences for soldiers preparing for their next deployment, as discussed in section II.B. By comparing soldiers assigned to different brigades within the same duty station, our identification strategy ensures that soldiers have nearly identical state-side training environments regardless of their unit of assignment.

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

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

F. First Stage

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

Figure 3*A* shows the dynamics underlying the first-stage effect in figure 2 by plotting the relationship between the instrument and months deployed within varying horizons of arrival. The shape and shade of the points changes at 48 months after arrival, when our sample goes from

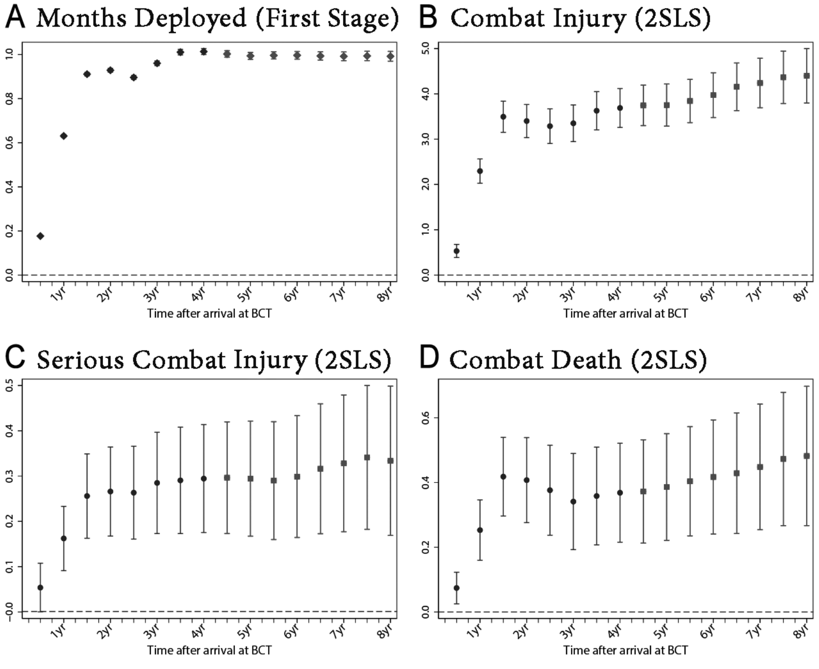

FIG. 3.—Dynamic effects on deployment, injury, and death. *A*, Reduced-form relationship between total months deployed within the time since arrival indicated on the *x*-axis and the instrument using the specification in equation (2). After 3 years, deployment increases roughly one-to-one with peer months deployed. *B–D*, 2SLS estimates from equation (1) of the effect of months deployed on combat injuries (i.e., "wounded in action," defined as an injury resulting from an attack against 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. These outcomes are also measured within the time since arrival indicated on the *x*-axis. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. The shape and shade of the points changes 4 years after arrival, when our sample goes from being balanced to unbalanced. Since most of our outcomes are only available through December 2019, we do not observe outcomes more than 4 years after arrival for soldiers who arrived to their first operational assignment in December 2015. Error bars represent 95% confidence intervals.

being balanced to unbalanced.[16] Initially, effects are small because 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 1, implying that initial exposure effects are highly persistent.

[16] For example, because 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.

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

III. Causal Effects of Deployment

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

A. Disability Compensation

Figures 4*A* and 4*B* plot estimates of the causal effect of deployment on whether the soldier receives any VADC and total VADC payments (in 2020 USD) in the calendar year preceding the time indicated on the *x*-axis. As above, the point estimates are scaled by a factor of 10 to reflect the effect of an average deployment. Effects are initially small as soldiers remain in service. They increase rapidly 3 or 4 years after arrival, however. By 8 years after arrival, deployment causes large increases in both the likelihood of receiving any VADC and total dollars received.

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

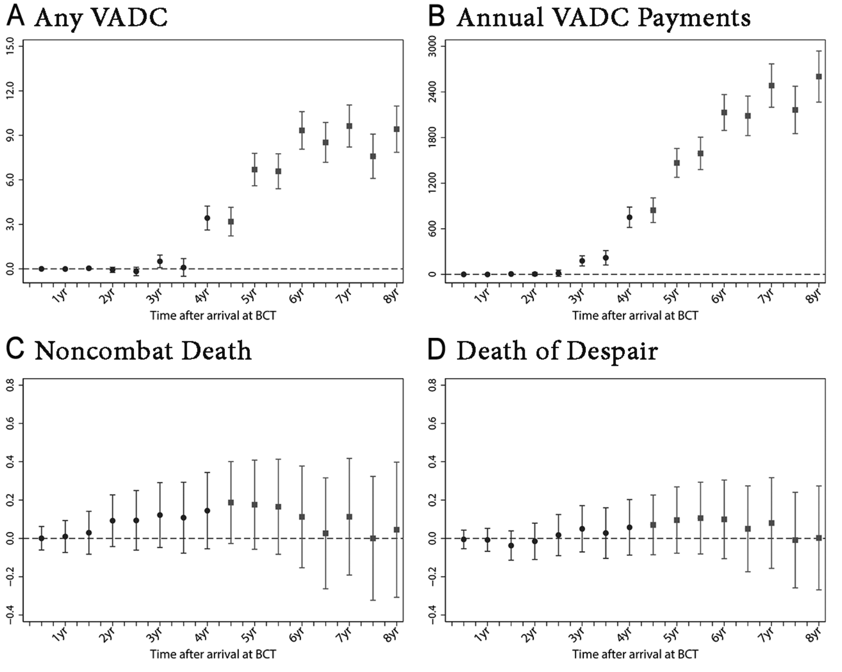

FIG. 4.—Dynamic effects on disability and mortality. This figure plots 2SLS estimates from equation (1) of the effects of deployment on various outcomes measured within the time since arrival indicated on the *x*-axis. *A*, Effects on receipt of any VADC in the most recent calendar year. *B*, Effects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

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

By 8 years after arrival at first duty station, deployment increases the likelihood of receiving any VADC by 9.4 pp from a base of 37% and increases total VADC payments by $2,602 per year, which is 42% of the amount paid to the average soldier in our sample. Deployment increases

TABLE 3
EFFECTS ON DISABILITY AND COMBAT DEATH AND INJURY

	2 Years (1)	4 Years (2)	6 Years (3)	8 Years (4)	8 Years Mean (5)
A. Disability receipt:					
Any VADC	−.07	3.43***	9.33***	9.42***	37.37
	(.09)	(.41)	(.64)	(.80)	
Any SSDI or SSI	.52***	1.30***	1.99***	2.60***	3.39
	(.08)	(.16)	(.22)	(.30)	
Any disability	.42***	3.94***	9.56***	9.52***	37.81
	(.12)	(.43)	(.65)	(.80)	
VADC ($)	3.70	751.09***	2,129.52***	2,602.30***	6,129.44
	(10.77)	(68.11)	(120.32)	(171.73)	
SSDI/SSI ($)	32.03***	168.69***	325.41***	426.38***	569.37
	(6.48)	(22.97)	(36.65)	(52.40)	
Disability ($)	35.72***	919.78***	2,454.93***	3,028.68***	6,698.81
	(12.82)	(77.69)	(137.79)	(198.29)	
B. Trauma:					
Combat death	.41***	.37***	.42***	.48***	.50
	(.07)	(.08)	(.09)	(.11)	
Ever combat injury	3.42***	3.71***	4.00***	4.43***	4.17
	(.19)	(.22)	(.25)	(.31)	
Army profile	−.76**	2.12***	2.14***	1.71**	25.53
	(.36)	(.52)	(.61)	(.72)	
Significant army profile	−.53**	2.08***	2.48***	2.62***	15.04
	(.24)	(.40)	(.48)	(.59)	
Observations	157,415	157,415	129,176	101,387	101,387

NOTE.—This table reports 2SLS estimates of the effects of months deployed on disability and trauma. We scale coefficients and standard errors by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. Panel A reports the effects of months deployed on disability receipt and compensation outcomes 2, 4, 6, and 8 years after arrival. Disability = VADC plus SSI plus SSDI; Disability, VADC, and SSI/SSDI reflect the amount of annual payments (in 2020 USD). The first rows of panel B report the effect of months deployed on combat deaths, followed by combat injury. The third and fourth rows of panel B report the effect of months deployed on all and significant army health profiles. Column 5 reports the mean of each outcome 8 years after a soldier's arrival. Robust standard errors are reported in parentheses.
** Significant at the 5% level.
*** Significant at the 1% level.

SSDI and SSI payments by $426, the vast majority of which is from SSDI rather than SSI. Total annual disability payments from all three programs thus increase by $3,029, with VADC accounting for the bulk of this effect.

Part of the effect of deployment on VADC likely reflects the fact that sending soldiers into conflict results in injuries that require long-term care and hence qualify soldiers for compensation. We explore this directly in panel B of table 3, which presents effects on various indicators of trauma that occur during combat or military service more generally. Consistent with the results reported in figure 3, deployment increases the likelihood that a soldier suffers a combat death within 8 years by 0.48 pp and increases the likelihood that a soldier experiences a combat injury by 4.4 pp. The magnitude of these effects is meaningful. For example, a 0.48-pp increase is 38% of the mean noncombat death rate (1.25 pp).

Since combat injuries only capture injuries during combat deployments, they cannot affect soldiers who do not deploy. We therefore turn to army medical personnel records to better understand the impact of combat deployments on all injuries. Specifically, "any army profile" is an indicator that equals 1 if the soldier ever had a "medical profile," which is formal documentation of a temporary or permanent medical condition that limits the soldier's ability to perform assigned duties. Only 43% of soldiers in our sample who experienced combat injuries also received a medical profile, suggesting that many combat injuries do not substantially limit a soldier's physical performance. We also explore the impact of deployments on receipt of a "significant profile," an indicator that equals 1 if the soldier ever had a medical profile that the army deemed severe enough to limit their ability to continue to serve.[18]

In the short term, deployment reduces the likelihood that a soldier receives any medical profile (−0.76 pp within the first 2 years after arrival). This is likely due to the fact that soldiers with certain types of profiles are barred from deploying. Thus commanders will often require soldiers with minor medical ailments to receive medical attention in the run-up to deployment to ensure they do not receive a profile. However, by 8 years after arrival the average deployment causes a 1.71-pp increase in the likelihood of having any medical profile, a 7% increase relative to mean profile rates (25.5%). The average deployment also increases the likelihood of having a significant medical profile by 2.62 pp, a 17% increase relative to mean significant-profile rates (15.0%).

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

[18] See US Army (2019b) 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 2 years later.

Table A.5 further explores the drivers of VADC by showing how deployment affects VADC receipt for the five most common conditions for veterans of the Global War on Terror (VA 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 on receiving any VADC with a posttraumatic stress disorder diagnosis, with the latter effect being particularly large (a 12.82-pp increase, 77% of the mean).

Some of the effects of deployment on VADC may also reflect that soldiers cannot receive disability while on active duty and that deployment increases long-run separation from the army. Table A.6 shows that by 8 years after arrival deployment increases separation by 2.6 pp (relative to a mean of 83%). However, even under extreme assumptions, separation can account for only a relatively small portion of the estimated VADC effects. If, for example, the additional 2.6 pp of soldiers who separate as a result of deployment received the 99th percentile of VADC payments ($46,000) by year 8 and otherwise would not have received VADC at all, effects on separation would explain 2.6 pp of the 9.42-pp effect on VADC receipt and $1,196 of the $2,602 effect.

Taken together, these results suggest that deployment increases VADC 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 may be the consequences of physical overuse or psychological harm resulting from deployment. However, it is also possible that the experience of deployment increases VADC receipt by directly increasing access. VADC is available to soldiers for any illness or injury that can be connected to their military service. The relevant rule 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 (38 C.F.R. § 3.304 [2010], part d). This is particularly true for PTSD claims, where stressors linked to combat or a veteran's fear of hostile military or terrorist activity consistent with the veteran's service are sufficient (38 C.F.R. § 3.304 [2010], part f). Other channels, including information dissemination, additional screening, peer effects, or changes in expectations about the probability of a successful claim, may also disproportionately encourage soldiers who have deployed to apply for and receive VADC.

B. Noncombat Deaths and Deaths of Despair

Figure 5 displays results for mortality outcomes derived from the NDI at various time horizons. The figure reports results for all-cause mortality,

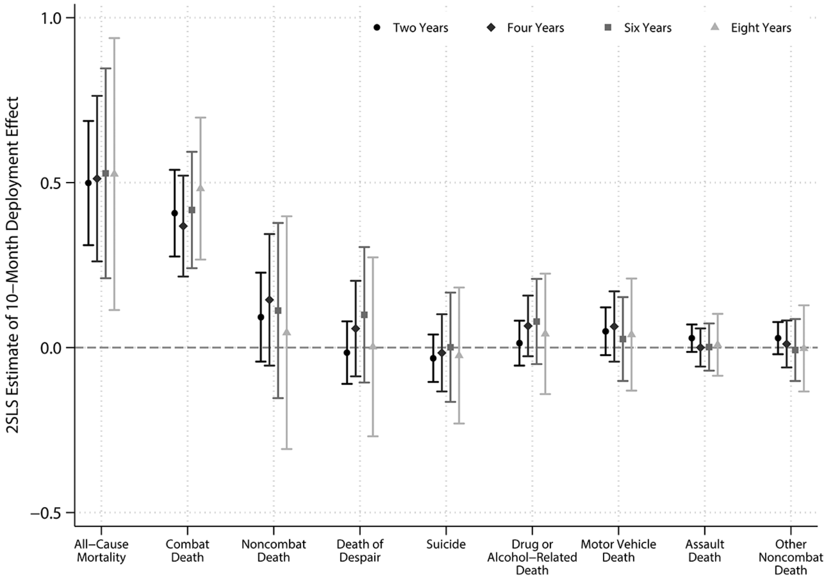

Fig. 5.—Effects of deployment on mortality. This figure reports 2SLS estimates from equation (1) of the effects of deployment on all-cause mortality and various subclassifications of mortality. We scale coefficients by 10 so that estimates can be interpreted as the effects of being deployed for 10 months. Each set of points reports the effects of deployment on mortality 2, 4, 6, and 8 years after arrival from causes shown on the *x*-axis. Error bars represent 95% confidence intervals.

deaths due to combat, all noncombat deaths, and specific subcategories of noncombat deaths including deaths of despair, suicide, and drug- or alcohol-related deaths,[19] and deaths resulting from motor-vehicle accidents, assault, and all other causes. We find large and statistically significant effects of deployment on overall mortality. Within 2 years of arrival at first duty station, the average deployment causes a 0.50-pp increase in deaths. This effect remains stable over longer horizons.[20]

More than 90% of this effect is due to deaths resulting from combat (0.48 pp at 8 years after arrival). By contrast, the impacts of deployment on overall noncombat death and its subcategories are substantially smaller

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

[20] Table A.7 reports the point estimates and standard errors associated with fig. 5 and the overall mean after 8 years from enlistment.

and not statistically significant. Eight years after arrival, point estimates imply that deployment increases noncombat deaths by 0.05 pp overall, or roughly 4% of the mean (1.25 pp), that it has no effect on deaths of despair, and that, if anything, it has a slight negative effect on suicides (−0.02 pp). However, because mortality is a rare outcome among the individuals in our sample, who are typically around age 22 when they arrive at their first duty station, these results are not estimated with precision sufficient to rule out meaningful adverse effects. For example, 95% confidence intervals only allow us to rule out that deployment increases noncombat deaths within 8 years by 0.40 pp, which is 32% of the mean.

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

C. Misconduct, Credit Scores, and Education

Panel A of table 4 explores whether deployment causes soldiers to be separated from the army for misconduct or incarcerated at any point during or after military service. Within 2 years of arrival at first duty station, deployment reduces separation for misconduct, but this is almost certainly due to mechanical and administrative impacts of deployment. Soldiers may have less opportunity to misbehave while deployed, and commanders will often defer army separation proceedings until after the unit has returned stateside. Consistent with this idea, effects on "ever separated for misconduct" quickly revert to nearly 0 by year 4 and remain at that level 8 years after arrival at first duty station. These estimates are precise: we can rule out increases in misconduct separations larger than 1.4% of the mean. Although less precise, results for incarceration suggest that deployment increases incarcerations by a statistically insignificant 0.1 pp (4% of the mean) within 8 years. Consistent with null effects on antisocial behavior, the results reported in table A.8 show that while soldiers remain in the

TABLE 4
Effects on Misconduct, Credit Scores, and Education

	2 Years (1)	4 Years (2)	6 Years (3)	8 Years (4)	8 Years (Mean) (5)
	A. Misconduct and Incarceration (after arrival)				
Separated for misconduct/ barred	−3.92***	−.63	−.52	−1.02	25.05
	(.38)	(.53)	(.60)	(.70)	
Ever incarcerated	−.09	.05	.12	.10	2.41
	(.07)	(.14)	(.19)	(.25)	
	B. Education Outcomes (after arrival)				
Enroll	−1.48***	.66	1.59**	1.09	55.70
	(.40)	(.58)	(.73)	(.81)	
Associate's degree+	−.08	−.16	.01	.66	8.69
	(.07)	(.13)	(.27)	(.47)	
Observations	157,415	157,415	129,176	101,387	101,387
	C. Credit Scores (as of 2017/2020)				
	June 2017	Avg(Y_{2017})	Dec. 2020	Avg(Y_{2020})	
Vantage score	.52	622.10	1.91	655.20	
	(1.32)		(1.33)		
Observations	142,010		144,708		

Note.—This table reports 2SLS estimates of the effects of months deployed on army separations resulting from misconduct, incarceration during or after military service (as captured through military and national Lexis-Nexis records), Vantage credit scores from Experian Credit Bureau, and postsecondary education outcomes from National Student Clearinghouse. Panels *A* and *B* have the same number of observations, except for incarceration, where we drop <1% of the sample that was not sent to Lexis-Nexis. The sample size for incarceration is 156,246 at 2 years and 4 years, 128,120 at 6 years, and 100,381 at 8 years. For Vantage scores in 2017, we drop 1% of our sample that was not sent to Experian. In addition, we drop individuals who have no credit scores (2SLS regressions on an indicator for having a credit score are insignificantly different from 0). Robust standard errors are reported in parentheses.

** Significant at the 5% level.

*** Significant at the 1% level.

army, deployment does not increase demotions or the probability of becoming the subject of military investigations.[21]

Panel B of table 4 shows that deployment may have a small, positive effect on ever having enrolled in college by year 6, but this effect is indistinguishable from 0 by year 8. Similarly, deployment appears to have no effect on earning an associate's degree or higher.[22] These estimates are

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

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

precise enough to rule out that a 10-month deployment decreases college enrollment by 0.5 pp (1% of the sample mean) and decreases degree attainment by 0.26 pp (3% of the mean).

Finally, Panel C of table 4 shows that deployment has a precise null effect on Vantage credit scores from the Experian credit bureau. We consider the Vantage credit score to be an omnibus measure of financial health but report on additional credit outcomes and on national foreclosure outcomes from Lexis-Nexis in table A.9. Because we only have access to credit bureau data at two points in time (June 2017 and December 2020), panel C of table 4 only reports credit score results as of these two dates pooled across all enlistment cohorts. The point estimate from 2020 indicates that deployment increases Vantage scores by 1.91 points on average, which is small relative to the mean score of 655 and the standard deviation of 92 within our sample. Taking the lower bound of the 95% confidence interval, we can rule out that deployment decreases credit scores by more than 0.1% of the mean (less than 1% of a standard deviation).

D. *Discussion*

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

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

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

Deployment also increases both earnings during service (through extra combat-related pay and tax deductions) and, importantly, VADC compensation. While table A.11 shows that a 10-month deployment increases cumulative army pay over the first 4 years of service by around $5,000, or 3% of the mean,[23] the same deployment increases VADC by $2,600 annually. This extra compensation may offset any negative effects on the outcomes we measure. We view this explanation as less likely, however. Although the few studies that examine the link between VADC and health find positive impacts, there is limited evidence of improvements in mortality (Autor et al. 2016; Trivedi et al. 2022). Moreover, Silver and Zhang (2022) find that VADC payments improve self-reported health and decrease food insecurity and homelessness but have no impact on mortality, blood pressure, HbA1c glucose levels, body mass index, major depressive disorder, and alcohol- and substance-use disorders, although the average veteran in their sample is substantially older than that in ours.[24]

Several additional results in sections IV and V provide further evidence that the extra disability compensation is unlikely to explain the null impacts of deployment on mortality and other adverse outcomes. In section IV, we continue to find null effects across differentially risky deployments, although even the least dangerous deployments still have

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

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

large impacts on VADC ($1,876 or 72% of the overall effect). This suggests that if VADC is offsetting negative effects of deployment, such effects are not tied tightly to combat exposure. In section V, we estimate effects across time and find that impacts on VADC more than double from $1,383 to $2,974 between the 2005–07 cohorts and the 2011–13 cohorts. Over this time, deployments also became substantially less dangerous. Yet the effects of deployment on other outcomes such as postsecondary enrollment, incarceration, and noncombat death do not change across cohorts.[25] These patterns are more consistent with both deployment and VADC having limited effects on noncombat mortality, education, and other outcomes than an alternative in which additional VADC received as a result of deployment offsets the adverse impacts of deployment.

Regardless of the extent to which the marginal VADC dollar ameliorates outcomes for those who deploy, it is certainly the case that deployments in our context occur in a setting where public support for veterans is high (Frankovic 2021) and where all veterans, not just those who deployed to combat, have access to generous healthcare, education benefits, disability compensation, and other support services. Our results speak to the effects of modern combat deployments on veterans' outcomes in the current environment but not necessarily those in contexts without the same support.

E. Heterogeneity

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

F. Robustness Analyses

Several analyses support the robustness of our main results. First, we explore sensitivity to the time horizon for measuring deployment. Our primary specification measures months deployed over the 3 years after arrival at a soldier's first duty station because 3 years is the modal term length of

[25] One possible exception to this finding: the effect of deployment on credit scores slightly increases, from a statistically insignificant reduction of 2.1 points in the 2005–07 cohort to a statistically significant increase of 5.2 points in the 2011–13 cohort, consistent with VADC receipt improving credit.

first-term soldiers (63% of our sample). Additionally, soldiers with longer initial terms will often move to other units after this time frame. In table A.13, we show that defining the endogenous variable D_i over longer time horizons (and changing the instrument accordingly) changes the results very little. This finding is consistent with our initial time span adequately capturing the variation in total months ever deployed.

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

Third, we explore whether our baseline specification masks important nonlinear relationships between deployment length and our primary outcomes. Table A.16 estimates multiple endogenous variable models with, for example, both months deployed and the square of months deployed. The implied effects of 10 versus 0 months of deployment are very similar to our primary 2SLS estimate. No coefficients are significantly different from 0 for noncombat deaths. Table A.17 shows that changing the endogenous variable to an indicator for any deployment and the instrument to the share of peers ever deployed, which might lead to more extensive margin shifts among compliers, yields similar effects. If anything, effects on noncombat deaths become more negative, changing from 0.05 to −0.13 overall and from −0.02 to −0.15 for suicides. Simply studying the reduced-form relationship between the instrument and outcomes nonparametrically also yields similar conclusions, as shown in figure A.5. Consistent with our primary results, although combat death and VADC are increasing in the instrument, there is no evidence of any relationship with noncombat death.

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

IV. Do More Dangerous Deployments Have Different Effects?

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

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

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

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

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

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

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

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

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

Peer casualties are an ideal measure for estimating heterogeneity in the severity of combat violence for several reasons. First, this approach avoids the potential bias inherent in many alternative approaches (e.g., comparing soldiers in combat occupations to those in noncombat occupations). Second, the residual variation in interacted peer casualties $(Z_i \times W_i)$

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

from equation (7) is 68% larger than the residual variation in the peer deployments itself (Z_i), which improves our precision for rare outcomes like mortality and incarceration. Among soldiers who arrived at their unit in 2009 and deployed within 3 years, for example, 25% had no peers who were wounded or killed in action, the median peer-casualty rate was 1.8%, and the 90th percentile of peer-casualty rates was 10.9%. Third, unit-level casualty rates are predominately a function of exogenous factors such as the location of the deployment (which we cannot observe), the unit's mission, and the broader geopolitical environment.[27]

Table 5 reports 2SLS estimates of equation (6), with column 1 reporting the coefficient estimate for the main deployment effect (β), column 2 reporting the coefficient estimate for the interaction term (γ), and column 3 reporting the outcome mean. To reduce the size of the table, we restrict to outcomes measured 8 years after a soldier arrives at his initial brigade. We continue to scale estimates of β by 10, and we scale estimates of γ by 10σ, where σ is the sample standard deviation of peer-casualty rates. Thus estimates of β can be interpreted as the average effect of a 10-month deployment with zero peer casualties and estimates of γ as the impact of a 1 standard deviation increase in peer casualties during the same deployment.

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

The increased risk associated with more dangerous deployments also manifests itself through statistically significant increases in disability. A 1 standard deviation increase in peer-casualty rates during a 10-month deployment increases annual VADC payments by \$414 and receipt of any

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

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

TABLE 5
EFFECTS OF VIOLENT DEPLOYMENTS

	10 Months Deployed (1)	10 Months Deployed × 1σ Peer Casualty (2)	Mean (3)
	A. Trauma and Disability		
Combat death	.01	.27***	.50
	(.12)	(.04)	
Ever combat injury	1.16***	1.86***	4.17
	(.33)	(.11)	
Significant army profile	.84	1.02***	15.04
	(.64)	(.16)	
Annual VADC amount ($)	1,876***	414***	6,129
	(190)	(48)	
VADC receipt	7.16***	1.29***	37.37
	(.88)	(.21)	
	B. Noncombat Mortality Outcomes		
Noncombat death	.07	−.01	1.25
	(.20)	(.05)	
Death of despair	.10	−.06	.79
	(.16)	(.04)	
Suicide	.02	−.02	.44
	(.12)	(.03)	
Drug- or alcohol-related death	.14	−.06*	.38
	(.11)	(.03)	
Motor vehicle death	.04	−.003	.27
	(.09)	(.022)	
	C. Misconduct, Credit, and Education		
Separated for misconduct	−.90	−.07	25.05
	(.77)	(.18)	
Ever incarcerated	−.07	.10	2.41
	(.28)	(.07)	
Credit score (2020)	1.34	−.01	655.78
	(1.74)	(.41)	
College enrollment	1.13	−.03	55.70
	(.89)	(.21)	
Associate's degree+	.41	.14	8.69
	(.52)	(.12)	
Observations	101,387		101,387

NOTE.—This table reports 2SLS estimates of eq. (6), with corresponding first-stage eqq. (7) and (8) on our primary outcomes, as of 8 years after a soldier arrives at his initial operational assignment, excepting credit outcomes, which are as of 2020. As described in sec. IV, 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 nonfatal combat injuries or fatal combat deaths within 3 years. Column 1 reports $\hat{\beta}$, and col. 2 reports $\hat{\gamma}$. Each row represents a separate regression on a separate outcome. Coefficients and standard errors in col. 1 are scaled by 10. Coefficients and standard errors in col. 2 are scaled by 10σ, where σ is the sample standard deviation of peer casualties. Sample sizes for incarceration and credit are smaller: 100,381 for ever incarcerated and 93,252 for credit score (2020). Robust standard errors are reported in parentheses.

* Significant at the 10% level.

*** Significant at the 1% level.

payments by 1.29 pp. Although these estimates leave little doubt that VADC increases with exposure to violence, deployments with no peer casualties also have substantial effects, increasing annual payments by $1,876 and any receipt by 7.15 pp. Table A.21 reports effects on VADC by diagnoses and further shows that exposure to violence is strongly linked to serious combat injuries and VADC receipt to a documented amputation. These outcomes are extremely rare among deployments with no 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, but it is also consistent with the possibility that deployment could facilitate the process of applying for and being granted disability compensation.

Outside of trauma outcomes and disability receipt, we find little evidence that exposure to peer casualties causes other adverse outcomes. Panel B of table 5 reveals no relationship between peer-casualty rates and noncombat deaths, deaths of despair, or key subcategories of deaths of despair (i.e., suicide and drug- or alcohol-related deaths). These estimates are not statistically different from 0 and are precise enough to rule out meaningful effects. For example, we can rule out that a 1 standard deviation increase in peer-casualty rates over a 10-month deployment increases noncombat deaths by more than 0.09 pp, which is 7% of the sample mean. Relatedly, we can rule out that the same increase in peer-casualty rates causes a 3% increase in deaths of despair, an 8% increase in suicide, and a 1% increase in drug- or alcohol-related deaths.

Panel C of table 5 shows that exposure to more violence during deployments has no significant effect on misconduct separations, incarceration, credit scores, postsecondary enrollment, or graduation. These estimates are precise enough to rule out that a standard deviation increase in peer casualties during a 10-month deployment increases separations for misconduct by 0.29 pp (1% of the mean), increases incarceration by 0.23 pp (9% of the mean), decreases Vantage credit scores by 0.8 points (0.1% of the mean), and decreases college enrollment by 0.4 pp (0.8% of the mean).[29]

Overall, these results reveal that soldiers exposed to more dangerous deployments are substantially more likely to die in combat and suffer physical injuries. Exposure to violence also increases disability receipt, although deployments also substantially increase compensation among

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

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 affect disability and mortality outcomes, we explore in section V whether changes in the frequency and combat intensity of deployment over time can explain veterans' trends in disability and mortality.

Additional robustness analyses.—It is possible that our peer-casualties proxy does not accurately capture which deployment experiences affect outcomes the most. As a simple alternative, we directly examine how BCT assignment influences outcomes, allowing us to capture the impact of whatever experiences soldiers in each BCT have while deployed. We do so by constructing BCT-specific causal effects of deployment on key outcomes. These estimates are formed by dividing the coefficient on the BCT × quarter dummy in the reduced form by the coefficient on the same dummy in the first stage, essentially constructing separate 2SLS estimates for each BCT × quarter instrument. Figure A.6 clearly shows that assignment to BCTs where a 10-month deployment leads to more violence also leads to more VADC receipt. However, the BCT-specific effects of deployment on violence are unrelated to BCT-specific effects on noncombat death.

Consistent with these findings, table A.23 reports unbiased estimates of the variance of BCT assignment's reduced-form effects on these outcomes. A large variance indicates that soldiers assigned to different BCTs experience very different average outcomes, whereas a small variance indicates that they do not. The results show large differences in deployment and VADC receipt across BCTs, but the estimate for noncombat deaths is negative with a confidence interval that includes 0. BCT assignment thus does not appear to affect noncombat deaths in important ways regardless of the experiences those soldiers have while in the service.

It is also possible that the linear interaction in equation (6) provides a poor approximation of how combat violence and deployment jointly affect outcomes. Reassuringly, alternative specifications produce similar results. For example, we find similar marginal effects of peer casualties at 10 months of deployment whether or not we include the main effect of W_i, as shown in table A.24. Estimating a more flexible model that interacts D_i with bins of W_i also yields similar conclusions, as shown in table A.25. Table A.26 reports effects in the subsample of soldiers with $W_i = 0$ to explore whether our specification adequately extrapolates to soldiers with zero exposure to peer casualties. Although these results are noisier, they are similar to the effects reported in column 1 of table 5, and a joint-hypothesis test for whether the estimates are equal does not reject (p-value = .312).

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

V. The Drivers of Declining Veteran Well-Being

We conclude by examining whether our estimated causal effects of deployment can help explain recent trends in veterans' outcomes. What role did combat deployments to Iraq and Afghanistan play in the rapid rise in VADC and simultaneous shifts in veteran mortality? How does the effect of deployment compare to the potential impact of changes in the characteristics of service members? And what do the answers to these questions imply for the current design of VADC as a program meant to insure veterans against the unique risks of military service?

We begin by augmenting our interacted deployment and casualty specification with a rich set of observable characteristics measured prior to the time of assignment, denoted X_i, that includes age, race, sex, AFQT and ASVAB subtest scores, any waiver for a prior felony conviction or other disqualifying conduct, marital status, number of dependents, educational attainment, and results from medical and drug tests administered at military entrance processing stations (MEPSs):

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

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

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

We estimate this model using the full sample in table 1 for all entry cohorts from 2001 to 2011, measuring outcomes 8 years after arrival. Since our primary analysis uses a subset of individuals from post-2005 cohorts for whom the instrument is well defined, we augment our baseline set of fixed effects $\delta_{k(i)}$ to include an additional interaction with an indicator variable for our analysis sample and set our instrument to 0 outside of this sample. Including pre-2005 cohorts allows us to analyze a longer time period, including key years in the Global War on Terror.[30]

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

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

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

We measure these changes over key peak-to-trough intervals for each outcome. The component attributable to deployment and violence captures how time trends in exposure to combat, rescaled by their causal effects estimated using our 2SLS strategy, relate to changes in outcomes.[31] The component attributable to X_i captures how selection into service, reflected in observable factors, explains changes in outcomes. Because Γ is estimated in a model that includes duty-station $\times$ job $\times$ enlistment-period $\times$ term-length fixed effects, the effects of the these covariates are estimated by comparing soldiers serving in the same place, in the same jobs, and at the same time. Our decompositions then measure how much of between-cohorts changes in outcomes is reflected in between-cohorts changes in observables when scaled by their estimated effects.

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

As a validation test, we begin with trends in combat injuries, which we expect to be mechanically explained by changes in deployment and violence. The results are presented in figure 6*A*.[32] The solid black line with

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

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

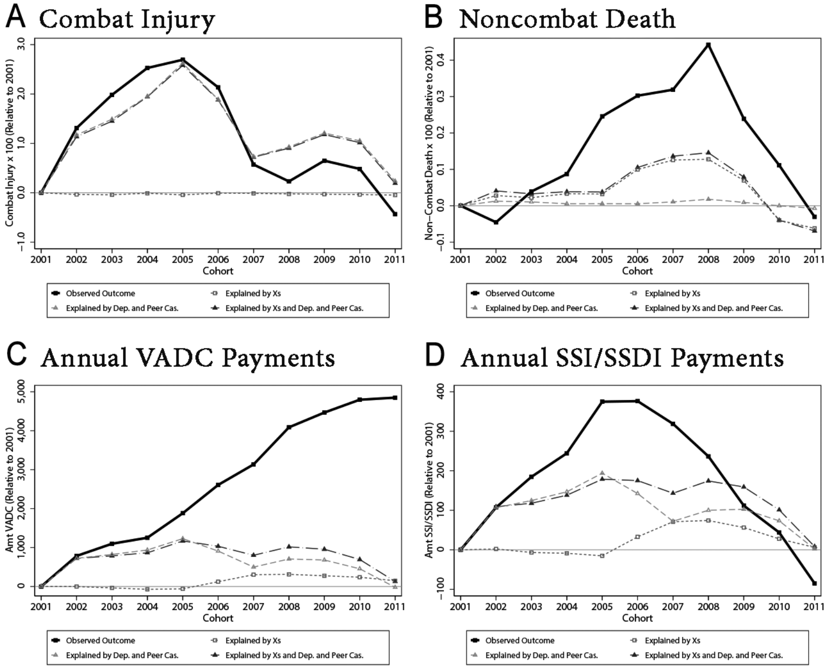

FIG. 6.—This figure shows a decomposition of changes in the average outcomes of cohorts of soldiers who enlisted between 2001 and 2011. Actual cohort-level outcomes, normalized to 2001 levels, are plotted in the solid black lines with square markers. As described in sec. V, we use estimates of equation (9) to generate predicted outcomes based on covariates (*X*s), a causal effect of months deployed (Dep.), and a causal effect of the interaction between months deployed and peer casualties (Peer Cas.). The dashed lines with hollow square markers show how changes in covariates alone predict changes in outcomes across cohorts. The dashed lines with hollow triangle markers show how changes in deployment and peer casualties predict changes in outcomes. The dashed lines with solid triangle markers show changes predicted by all factors. *A*, Decomposition of trends in combat injuries within 8 years of arrival. *B*, Decomposition of trends in noncombat deaths within 8 years of arrival. *C*, Decomposition of trends in annual VADC payments 8 years after arrival. *D*, Decomposition of trends in annual SSI/SSDI payments 8 years after arrival. Covariates are the same as those used to construct predicted months deployed in fig. 2: age at enlistment, white, Black, Hispanic, female, linear, quadratic, and cubic terms for AFQT (and an indicator for rare situations where soldiers are missing AFQT scores), any waiver for a prior felony conviction or other disqualifying conduct, married, number of dependents, high school grad, an indicator for requiring a medical waiver at enlistment, indicators for failing alcohol, marijuana, or cocaine drug tests, and ASVAB composite line scores (clerical, combat, electronics, field artillery, general maintenance, general technical, mechanical maintenance, operators and food, and surveillance and communications).

squares shows the change in actual outcomes for each cohort relative to the 2001 cohort. The outcome is measured as of 8 years after enlistment. The dashed line with open triangles shows the changes in combat injuries predicted by our causal effects of deployment and changes in average peer exposure. The dashed line with hollow square markers shows the changes attributable to changes in soldiers' observable characteristics. The final line with solid triangles shows the changes attributable to all of these factors.

The results show that combat injuries increased by nearly 2.7 pp from the 2001 to the 2005 cohort, then fell to sub-2001 levels for the 2011 cohort. The series including changes predicted by deployment closely track the evolution of actual combat injuries, as one would expect given that combat deployments are the only way to become wounded in combat. The causal effects of deployment explain about 97% of the 2001–05 increase and about 76% of the 2006–11 decline. Predicted effects diverge slightly from observed effects beginning in 2007, suggesting some misspecification emerging from the inability of the peer-casualty measure to fully capture the changing nature of combat over time. Soldiers' observed characteristics, by contrast, explain none of the changes in combat injuries, suggesting that who is wounded in war is largely random conditional on our baseline set of fixed effects.[33]

A. *Explaining Trends in Noncombat Deaths*

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

Observable selection, however, is a far better predictor of changes in noncombat deaths. The dashed line with hollow square markers tracks increases and declines in the outcome accounted for by changes in soldiers' characteristics such as any waiver for a prior felony conviction or other disqualifying conduct and AFQT scores. As soldiers with lower AFQT scores and more any waiver for a prior felony conviction or other disqualifying conduct enlisted in the mid-2000s, noncombat deaths increased. Selection into service improved following the 2008 entry cohort, and noncombat deaths then decreased. Table A.27 shows that about 32% of the between-cohorts variation in noncombat deaths can be explained by observable characteristics alone. These patterns are even starker if we

[33] Figure A.9 shows that results change little if we use the baseline model without the peer-casualty interaction, although deployment effects do a slightly worse job of tracking changes in combat injuries, which is as expected. And figure A.10*a* shows a similar pattern when we use effects on combat deaths instead of injuries to validate the model.

focus only on our analysis sample, as shown in figure A.12. Because these characteristics include only the observables available in our data, it seems likely that changes in unobserved characteristics would explain even more of the observed changes over time.

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

B. Explaining Trends in Disability Payments

Figure 6*C* repeats the same exercise for annual VADC payments 8 years after arrival (in 2020 USD). While deployment explains an important share of the increase in disability compensation into the mid-2000s, trends diverge after the 2005 cohort. Average VADC payments continued to grow at roughly the same rate, despite a decline in the causal contribution of deployment itself. As a result, the 2011 entry cohorts received the most VADC despite having the lowest deployment rate among the cohorts we analyze. The dashed line with hollow square markers shows that this pattern is also poorly explained by changes in soldiers' observable characteristics.[34] By contrast, figure 6*D* shows that trends in the other major disability insurance programs veterans accessed over this period, SSI and SSDI, are much better explained by deployments throughout the entire sample period.

To the extent that disability insurance programs serve as insurance for veterans' exposure to combat and compensate them for any resultant harms, some connection between deployment, VADC, and SSI/SSDI is natural. The decoupling between recent trends in VADC and deployment, however, suggests that VADC's targeting has changed over time, at least relative to the other major programs. To better understand this point, we estimate the effects of deployment on VADC, SSI/SSDI, and combat deaths for sets of 3-year rolling cohorts from 2005 to 2013.

The results are presented in figure 7. Every point represents an estimate from a different subset of soldiers in our primary specification, restricting to those who enlisted during the 3 years listed on the *x*-axis.[35] Each triangle shows average outcomes among *untreated* compliers— that is, soldiers who did not deploy during their first 3-year term but would have been deployed if assigned to a different BCT. Each circle shows the expected outcome for a soldier with a 10-month deployment. The gap between the two series corresponds to the treatment effect of a 10-month deployment.

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

[35] We focus on outcomes 6 years after arrival for cohorts from 2005–13 in order to be able to show a longer time series, but figure A.14 shows that we reach the same conclusions using outcomes 8 years after arrival for the 2005–11 cohorts.

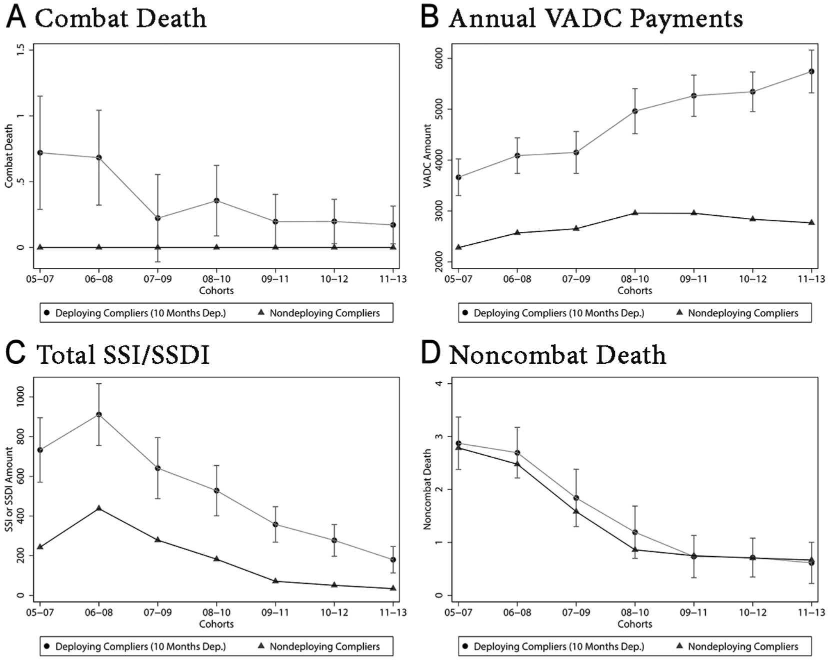

FIG. 7.—Treatment effects across 3-year rolling cohorts. The triangles show average outcomes among untreated compliers (i.e., soldiers who deploy for zero months) for a range of outcomes. For comparison, the circles plot estimates of average outcomes among soldiers who deploy for 10 months, computed as the untreated complier mean plus 10 times the estimated treatment effect of months deployed for the relevant outcome. Treatment effects are thus the gap between the two lines. Each estimate restricts the sample to the cohorts listed on the *x*-axis. *A*, Combat death, with untreated means set to 0 since combat deaths only occur on deployment. *B–D*, Annual VADC payments (*B*), SSI/SSDI payments (*C*), and noncombat death (*D*). All outcomes are measured as of 6 years after arrival at first duty station. Vertical bars show 95% confidence intervals on the estimated treatment effect.

The results show that deployments have become less dangerous over time but have simultaneously generated larger increases in VADC receipt. A 10-month deployment for soldiers who enlisted between 2005 and 2007 increased the risk of combat death by 0.72 pp. By the 2009–11 cohort, the effect of deployment on combat death fell by a factor of four to 0.17 pp. If VADC is compensating soldiers for the risks of deployment, these results suggest that the need for such compensation has, if anything, decreased over time.[36] However, the causal effect of a 10-month

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

deployment on VADC compensation more than doubled during this period, from $1,383 to $2,974. Simultaneously, VADC payments have been increasing over time for soldiers who do not deploy, with untreated-complier means increasing by 21% from 2005–07 to 2011–13. Since these soldiers do not deploy by construction, VADC payments are unlikely to be insuring them against harms from combat deployments.

SSI and SSDI, by contrast, show a very different pattern. The effect of a 10-month deployment on total payments from these programs decreased from $490 to $145 over the same period, consistent with the declining combat risk. Untreated means of SSI/SSDI receipt have also been declining since the 2006–08 cohorts, consistent with decreasing severity of combat and increasingly positive selection into service. Thus, VADC has expanded in the opposite direction of what would have been expected based on declining combat risk. SSI/SSDI has not. To the extent that the need for insurance against other noncombat risks is either stable or also declining over time, our estimates are consistent with VADC increasingly acting more as a transfer program disconnected from risk rather than as a vehicle for targeted insurance.

Policy changes and VADC.—What explains the recent growth in the VADC program if not changes in deployment, combat risk, or the composition of service members? A broad suite or regulatory, policy, and organizational practice changes likely help account for the program's growth, along with possible shifts in applicant norms. Tables A.29 and A.30 outline regulatory, policy, codification, and practice changes we identified between 2000 and 2015. Consistent with a political environment supportive of veterans, these changes almost always made it easier to apply, eased access, expanded compensation, or lowered evidentiary standards.

Several key policy changes appear particularly relevant.[37] In response to the Veterans' Benefit Improvement Act of 2008, the VA and DOD made several large pushes to simplify the process of applying for benefits, providing consistent information and application support to all soldiers and greatly expanding their ability to apply for VADC before

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

leaving the service.[38] Concurrently, there has been a rise in no-cost support for veterans filing claims. Regulatory changes in the 2000s also assigned to the VA the duty to assist claimants and help substantiate their claims (Veterans Claims Assistance Act of 2000, Pub. L. 106–475, 114 Stat. 2096). The VA also revised its interpretation of regulations regarding disabilities that existed to prior to service. In order to rebut claims filed after 2005, the VA now had to prove not only that the condition had existed prior to service, but also that it was not aggravated by service (Presumption of Sound Condition: Aggravation of a Disability by Active Service, 70 Fed. Reg. 23,027 [May 4, 2005]).

Importantly, various new regulations also eased evidentiary standards and expanded the list of presumptive conditions linked to various exposures. While some of these, like a 2011 change relaxing verification of military sexual trauma (MST) stressors, apply equally to deployers and nondeployers, others were particularly applicable to those who deployed. For example, a July 2010 policy eased evidentiary standards for claiming PTSD, eliminating the requirement to corroborate that the claimed in-service stressor occurred if that stressor is linked to being in a combat zone or is consistent with the "places, types, and circumstances of the Veterans' service" (Stressor Determinations for Posttraumatic Stress Disorder, 75 Fed. Reg. 39,843 [July 13, 2010]). Contreary, Tennant, and Ben-Shalom (2017) find that "the reduction in the burden of proof on veterans who served in combat zones seeking DC [i.e., VADC] for PTSD increased DC receipt among these veterans."

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

VI. Conclusion

Nearly 20 years of war in Iraq and Afghanistan has had a profound impact on the soldiers who fought there. Our results show that combat

[38] Broten (2021) describes how the Benefits Delivery at Discharge program and the Quick Start program (which started in July 2008) allowing service members to apply for benefits while in service were expanded following the Veterans' Benefits Improvement Act of 2008. By fiscal year (FY) 2009 around 65% of separating service members who filed claims within 1 year of discharge did so through these programs ("Quality vs. Quantity" 2010). Proposed in 2007 and fully implemented by 2011, the Integrated Disability Evaluation System streamlined VA and DOD systems for evaluating disabilities of active duty soldiers with health conditions that limited their ability to carry out military duties (Broten 2021). Additionally, the Veterans Opportunity to Work (VOW) to Hire Heroes Act of 2011 mandated participation of service members in the Transition Assistance Program, in which disability compensation information and filing support is offered (Public Law 112–56 [2011]). These changes were accompanied by a move to an online application system and generally greater encouragement and outreach.

deployments brought both immediate risks to soldiers, in the form of death and injury, and long-term costs to society, in the form of large increases in disability payments. Nevertheless, we find limited evidence that combat deployments affect other important dimensions of veteran well-being, including deaths outside of combat, misconduct, credit, and education. Moreover, deployments do not appear to be the main driver of the concerning trends in veterans' outcomes, which instead appear tied to relaxed recruiting requirements over the course of the wars and changes in policy.

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

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

Data Availability and Replication Files

The empirical results in this paper are derived from proprietary administrative data from the US Army, the Department of Veterans Affairs (VA), the Social Security Administration, the joint VA and Department of Defense Mortality Data Repository, Experian Credit Bureau, Lexis-Nexis, and the National Student Clearinghouse (NSC). Instructions on how to request access to the data, and the code required to replicate all figures and tables in the main text and the appendices, are provided in Bruhn et al. (2023) in the Harvard Dataverse, https://doi.org/10.7910/DVN/WZMGVY.

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