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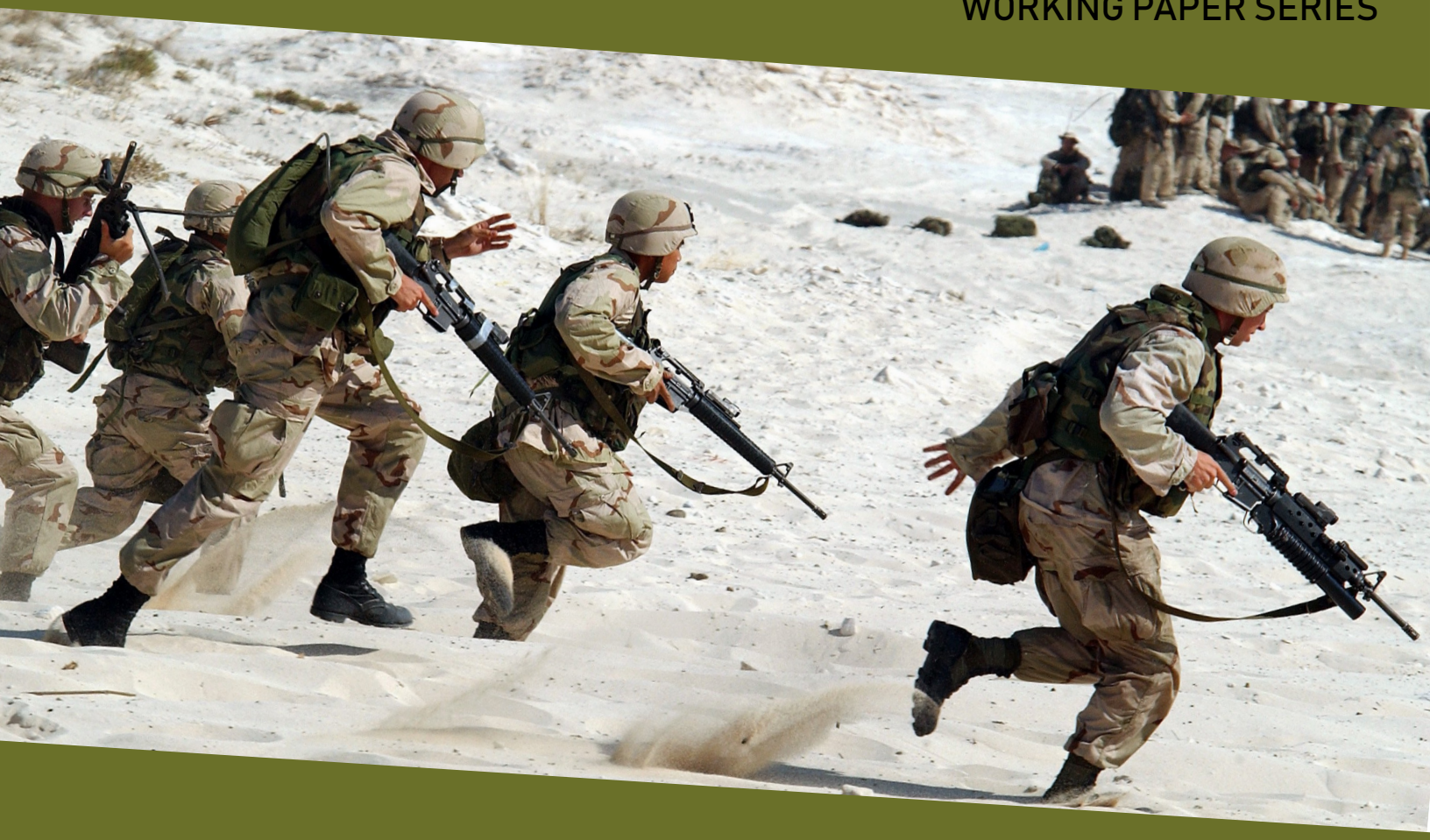

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JULY 21, 2020

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Post-9/11 War Deployments Increased Crime among Veterans

Abstract

Several high-profile news stories have linked post-September 11 (9/11) combat service to violent crime among veterans. Nevertheless, there is scant causal evidence for this claim. We exploit the administrative procedures by which U.S. Armed Forces senior commanders conditionally randomly assign active duty servicemen to overseas deployments to estimate the causal impact of modern warfare on crime. Using data from two national surveys and a unified framework, we find consistent evidence that post-9/11 combat service substantially increased the probability of crime commission among veterans. Combat increases the likelihood of property and violent crime, arrest, gang membership, trouble with police, and punishment under the Uniform Code of Military Justice. Back-of-the-envelope calculations suggest that post-9/11 combat exposure generated approximately $26.7 billion in additional crime costs. Finally, we document descriptive evidence that Traumatic Brain Injury and Post-Traumatic Stress Disorder (PTSD) may be important mechanisms to explain post-9/11 combat-induced increases in crime.

Keywords: post-9/11 combat service; military deployments; war; crime; arrests

JEL codes: H56; K14

"[P]eople in war had so inured themselves to corrupt and wicked manners had taken a delight and pleasure in robbing and stealing, that through manslaughter they had gathered boldness to mischief, that their laws were held in contempt, and nothing set by or regarded."

- Sir Thomas Moore, Utopia, 1516

"The [unit's] soldiers who survived all exhibited signs of posttraumatic stress disorder and other psychological conditions. Twelve of them have been arrested for murder or attempted murder."

- U.S. Supreme Court Justice Sonia Sotomayor,
Lockhart v. Alabama, 2015

1. Introduction

Approximately 181,500 veterans are incarcerated in the United States, representing 8 percent of the prison and jail populations (Bronson et al. 2015). Many are veterans of wars waged in the Global War on Terrorism (GWOT), launched in the wake of the September 11, 2001 attacks on the United States. Thirteen percent of currently incarcerated veterans served in the Afghanistan or Iraq conflicts and approximately 45,000 incarcerated veterans were exposed to combat during their war deployments. While the incarceration rate for veterans is nearly 10 percent lower than for civilians (855 per 100,000 veterans compared to 986 per 100,000 U.S. civilians), a far greater share of incarcerated veterans has been sentenced for violent offenses (64 percent versus 48 percent) (Bronson et al. 2015). Surveys of post-9/11 veterans suggest that approximately one-third have exhibited non-job-related physical aggression and a further 11 percent engaged in "severe or lethal violence" (Elbogen et al. 2012).

High-profile incidents of violence committed by post-9/11 veterans, including aggravated assaults (Simkins 2018) and mass shootings (Keneally 2018), have permeated the national news media. The link between modern warfare and violence has been tied to historically high rates of Post-Traumatic Stress Disorder (PTSD), Traumatic Brain Injury (TBI), and substance abuse among combat veterans deployed during GWOT (Tanielian and Jaycox 2008; Cesur et al. 2019).[1]

[1] See Elboge (2012), MacManus et al. (2013), and Cesur and Sabia (2016) for a discussion of the link between "invisible wounds of war" and subsequent violence.

Isolating the causal effect of modern war service on crime is a challenging task due to endogenous selection into the military. Prior studies of the effect of U.S. military service on crime have used draft lottery assignment to generate exogenous variation in wartime service (Rohlfs 2010; Lindo and Stroeker 2014). However, the abolition of the U.S. draft lottery in January 1973, followed by a transition to all-volunteer Armed Forces (AVFs), necessitates an alternative identification strategy. Moreover, from a policy perspective, in the absence of a return to the draft, a more relevant comparison group may be necessary to evaluate the crime costs of waging of modern warfare.

There are a number of reasons why the effect of modern warfare on crime may differ from prior draft-era conflicts. First, the marginal fighter has changed in ways that could exacerbate or dampen the effects of war service on crime (Sabia and Skimmyhorn 2019). AVFs are drawn from less socially connected and more disadvantaged populations than conscripted forces (Elder et al. 2010; Laich and Wilkerson 2017), traits that may generate larger adverse effects of war service. On the other hand, if volunteers are better matched to job duties than conscripted civilians, they may face lower private costs of fighting a war, generating smaller impacts on crime. Moreover, there is evidence that those who selected into combat occupations during the Afghanistan and Iraq wars were more likely to be drawn from white and higher-income neighborhoods (Carter et al. 2017), factors that could be protective, and therefore mute crime effects.

In addition, the *nature of modern warfare* itself has changed in ways that could alter the effect of war service on crime. First, modern military training techniques, which include more effective and depersonalized killing strategies such as unmanned drones, may more easily enable servicemembers to overcome their resistance to kill other human beings (Grossman 2009).[2]

[2] For example, while those trained to fight previous wars such as the Second World War did shoot at bullseye targets, in more recent times the training of military personnel shifted to use of more realistic targets such as a photo of a human being or silhouette, or even use of a video simulator (Grossman 2009). According to the British Broadcasting Company (BBC):

> "[Military training] methods are perfected, increasingly directed to reducing inhibitions. This is one of the reasons why simulation systems are in use in many countries today, and the soldiers themselves are the targets. The soldier, his weapon, the vehicles - as much as possible, is electronically networked. This creates a pseudo-realistic situation - the soldiers shoot straight at their comrades, who are acting as 'enemies.' The aim is desensitization with a view to future reality." (BBC 2015, https://www.bbc.com/news/magazine-30573936.

Hence, modern combat training may more easily condition post-9/11 veterans to engage in violent behavior both while in war theatre and their post-separation lives.

Second, post-9/11 military engagements in Iraq and Afghanistan often took the form of "asymmetric warfare" in which enemies of vastly inferior military strength employed remotely detonated improvised explosive devices (IEDs), roadside bombs, and mines (Congressional Research Service 2007; Wallace 2009; Thornton 2007; Wilson 2007; Buffaloe 2006) against U.S. and allied forces. While casualty rates in these modern conflicts were substantially lower than in draft-era conflicts (Sabia and Skimmyhorn 2019), the nature of injuries among those who have survived asymmetric warfare changed significantly.[3] In particular, TBI became the signature injury of GWOT (McKee and Robinson 2014; Hoge et al. 2008; Okie 2005), affecting over 17 percent of post-9/11 veterans (Lindquist et al. 2011). TBI has been linked to substantial declines in cognitive skills and social interactions (Lash 2015; National Research Council 2008; Zoroya 2007), factors that may exacerbate the risk of crime (Elbogen et al. 2012; Williams et al. 2018). Furthermore, nearly 20 percent of post-9/11 veterans suffered "invisible wounds of war" (Tanielian and Jaycox 2008; Hoge et al. 2004), which has been linked to increased hostility (Jakupcak et al. 2007) and violence (Orcutt et al. 2003; Taft et al. 2005; Cesur and Sabia 2016) among veterans (Sreenivasan et al. 2013).[4]

Third, in part due to the heavy psychological toll that modern warfare has taken on veterans, there is strong evidence that post-9/11 combat deployments are linked to substance use disorders, including heavy episodic alcohol use (Seal et al. 2011; Wilk et al. 2010; Teeters et al. 2017; Cesur et al. 2017) and opioid addiction (Cesur et al. 2019). Accordingly, combat-induced addiction may generate more property crime for income-generating purposes, as well as more violent crime through addiction-induced reductions in inhibitions or increases in discount rates.

Fourth, modern warfare has had a more negative effect on veterans' schooling and labor market outcomes than prior conflicts. There is evidence to indicate that draft exposure in prior conflicts increased veterans' educational attainment (Lemieux and Card 2001; Angrist 1993; Stanley 2003; Barr 2015), driven by the availability of generous schooling benefits under the GI Bill. In contrast, post-9/11 combat deployments and modern warfare had negative effects on

[3] Advances in military medicine, evacuation techniques, and body armor have resulted in a casualty rate for GWOT-era service that is 70 percent lower than in draft-era wars (Fischer 2015; Fazal 2014; Stiglitz and Bilmes 2008; Physicians for Social Responsibility 2006).
[4] See Glenn et al. (2002), Jakupcak et al. (2007), and Gartner and Kennedy (2018) for additional evidence.

education, particularly for those exposed to casualties among members of their units, and employment among separating veterans (Sabia and Skimmyhorn 2019). These declines in schooling and labor force attachment may generate larger increases in crime (Lin 2008; Fone et al. 2019; Lochner and Moretti 2004; Machin et al. 2011) than was seen from prior wars.[5]

Finally, modern veterans' benefits programs could differently affect combat veterans' propensity for crime. Large entitlement benefits programs provided by the Department of Veteran Affairs (VA) to post-9/11 veterans — including the Veterans Disability Compensation (VDC) program, Unemployment Compensation for Ex-Servicemembers (UCX), the Post-9/11 GI Bill, and Transition Assistance Programs (TAPs) — may reduce the propensity to engage in income-generating illegal activities by providing a more generous social safety net.[6] On the other hand, unintended consequences of many modern veterans' entitlements programs, such as work disincentives generated by the Veterans Disability Compensation program (VDC) (Angrist et al. 2010; Autor et al. 2010, 2016), could increase veteran idleness and reduce their social connectedness (Gade et al. 2013), potentially resulting in more crime. In summary, the impact of post-9/11 combat on crime is, a priori, ambiguous, and remains an empirical question.

This study provides new evidence on the impact of modern warfare on crime among veterans. We exploit the administrative procedures by which senior commanders assign active-duty servicemembers to overseas deployments to estimate the causal impact of post-9/11 combat deployments on crime. Because U.S. Armed Forces Human Resources Command treats servicemen of identical military rank and occupation as perfect substitutes for the purposes of unit deployment assignments, the natural experiment we propose is able to mimic conditional random assignment.

Using data from the military module of the 2007-2008 National Longitudinal Study of Adolescent to Adult Health (Add Health) and the 2008 Department of Defense Survey of Health and Related Behaviors Among Active Duty Personnel (HRBS), we find that post-9/11 combat deployments substantially increased the risk of crime. Increases in violent crime appear largest for those who were exposed to enemy firefight during combat deployments. Furthermore, we

[5] Additionally, as with previous wars, military deployments may decrease men's wages (Angrist 1990, 1998; Angrist and Krueger 1994), which could have similar effects on crime.

[6] Moreover, the quality of mental health services provided by the Veterans Health Administration is relatively high (Mantorell and Bergman 2013), and AVF veterans are more likely to seek help for mental health distress than their conscripted counterparts from prior wars (Stiglitz and Bilmes 2008), each of which may mute the effect of post-9/11 combat-induced psychological harm on crime.

document that combat exposure increases the probability that a veteran joins a gang, has trouble with military or civilian police, is arrested, and faces punishment under the Uniform Code of Military Justice (UCMJ). These effects are largest among enlisted servicemen under the age of 33. The magnitudes of our estimates suggest that post-9/11 combat exposure generated substantial crime costs to society: approximately $26.4 billion in additional costs for violent crime and $315 million in costs for property crime (in 2018 dollars). Auxiliary analyses reveal TBI and PTSD as likely mechanisms through which the identified relationship may be operating.

2. Background

Criminal activity following combat deployments are dealt with by military law, administered via the UCMJ or by civilian courts. We begin by describing these procedures.

2.1 Crime During Active Duty

Active-duty servicemembers are subject to the UCMJ for criminal offenses committed while on active duty or, in the case of reservists, when performing a duty that is directly related to their reservist status (United States Code, Title 10, Subtitle A, Part II, Chapter 47, Article 2). The UCMJ outlines expectations for servicemembers' standards of conduct, procedures that govern the adjudication of alleged conduct violations, and possible sanctions for those violations.[7]

Reports of servicemembers' alleged UCMJ violations are typically brought to their commanders' attention by a commissioned officer, warrant officer, petty officer, or noncommissioned officer. Once apprehended, a member of command authority may confine the member in military jail or impose restrictions on servicemembers movements (i.e., on post or base). Unlike civilian courts where a district attorney decides to press charges, under the UCMJ, senior commanders make this decision. Servicemembers have a right against self-incrimination and are entitled to be informed of the suspected offense(s) before being questioned by an officer (UCMJ Article 31). If the offense is one that warrants a general court-martial, the prosecutor

[7] Article I Section 8 of the U.S. Constitution, ratified in 1789, gave the U.S. Congress the authority to regulate conduct in the Armed Forces. The UCMJ was passed by the U.S. Congress on May 5, 1950, and signed into law by President Truman on the following day. This law established the modern U.S. military justice system, which covers all service branches of the Armed Forces and governing behavior of active-duty personnel, reservists, those in custody, and those serving with (and employed by) the Armed Forces (Article 2 UCMJ).

must be a military lawyer (judge advocate), and an appointed military lawyer may defend the accused servicemember.

Courts-martial may take three forms. In a *summary court-martial*, a single commissioned officer acting as judge and jury generally presides over cases reserved for lesser infractions such as simple assaults, failing to properly salute a superior officer, or abandoning watch. Punishments typically involve confinement, forfeiture of pay, and reduction of pay to lowest pay grade (United States Code, Title 10, Subtitle A, Part II, Chapter 47, Article 17). [8] A *special court-martial*, adjudicated by a military judge and four members of a jury, generally hears more serious cases such as those involving issuing false statements, weapons violations, drug possession, and disrespect toward a superior commissioned officer (Court Martial Reports 2019; Navy Trial Results 2019). Punishments are more severe than those handed down from summary courts-martial and also include bad conduct discharges. Finally, a *general court-martial* — which adjudicates the most serious cases dealing with larceny of military property, assault, rape, espionage, war crimes and murder (Court Martial Reports 2019; Navy Results of Trial 2019) — includes a military judge, the accused, prosecuting and defense attorneys, and a panel of at least five members (United States Code, Title 10, Subtitle A, Part II, Chapter 47). For general courts-martial, which handles the most serious charges, punishments can include the death penalty, imprisonment for life, and dishonorable discharge from the Armed Forces.[9] Between 2001 and 2010, there were 69,272 courts-martial across all branches of the Armed Services with the number of courts-martial remaining steady or slightly declining (U.S. Court of Appeals for the Armed Forces 2019).

Minor offenses such as non-alcohol-related traffic law violations, lateness to meetings, and disobeying minor orders are generally not handled via courts-marital, but rather with non-judicial punishments (NJPs) under Article 15 of the UCMJ. Article 15 NJPs are seen as corrections to misconduct without the stigma of a court-martial with a two-year statute of limitations.[10] Disciplinary punishments that can be imposed under Article 15 generally involve

[8] For instance, the following punishments are prohibited in such cases: death, dismissal, dishonorable or bad conduct discharge, confinement for more than one month, hard-labor without confinement for more than 45 days, restriction to specified limits for more than two months, and forfeiture of more than two-thirds of one month's pay (United States Code, Title 10, Subtitle A, Part II, Chapter 47, Article 17).
[9] For general courts-martial, any punishment not forbidden by this chapter, including the penalty of death when specifically authorized by this chapter (United States Code, Title 10, Subtitle A, Part II, Chapter 47, Article 17).
[10] Servicemembers have the right to demand a court-martial for these charges if they wish to have a formal trial.

forfeiture of pay, restriction to base or ship, a reprimand, and specific punishments dependent on rank.[11] From 2000 through 2010, there were between 65,000 and 80,000 Article 15 punishments imposed throughout all service branches (U.S. Court of Appeal for the Armed Forces 2019).

A limited class of offenses is only prosecutable and punishable by military law, such as insubordination, failure to obey an order, sedition, and mutiny (UCMJ Articles 91, 92, 94).[12] However, some offenses committed while a servicemember is on active duty such as driving under the influence (DUI), assault, robbery, and murder (Urbanic Law 2017; Military DUI: Court Martial and Civilian Charges 2016), can be tried by both military and (state) civilian courts.[13]

2.2 Post-Separation Crime

Following active duty service, trials of veterans' crimes are largely handled by state and federal courts (Ziezulewicz 2017). Several high-profile national media stories following criminal trials have linked post-9/11 combat service to violence. For example, a 2008 *New York Times* article highlighted 121 homicide cases committed by Iraq and Afghanistan veterans (Sontag and Alvarez 2008). Burchett et al. (2008) examined the facts behind these cases and documented that 90 percent of the individuals had served in Iraq, with approximately 40 percent exposed to combat. Also, stories appearing in major media outlets such as the *Chicago Tribune, Los Angeles Times*, and *New York Times* have linked post-9/11 service to mass shootings.[14]

Substantial numbers of PTSD-linked offenses coupled with a growth in post-9/11 veteran engagement with the criminal justice system have prompted the establishment of veterans' treatment courts (VTCs) in over 500 counties across the United States (Douds and Hummer

[11] Enlisted personnel can include such actions as a reduction in rank, forfeiture of pay (up to one-half of one month's pay per month for two months), restriction to base or the ship (up to 60 days), extra duties, correctional custody (up to 30 days), and a reprimand. For officers, permissible punishments can include forfeiture of pay (up to one-half of one month's pay per month for two months), restriction to base or the ship (up to 60 days), arrest in quarters (up to 30 days), and a reprimand. (United States Code, Title 10, Subtitle A, Part II, Chapter 47, Article 15).

[12] Adultery, fraternization, straggling, wearing unauthorized insignia, decoration, badge, ribbon, device, or lapel button, all fall under Article 134. These infractions can lead to punishments via Article 15 or courts-martial, depending on the implications of the offense.

[13] A servicemember subject to a military court trial cannot be tried for the same offense in federal court (*Grafton v. the United States* 1907).

[14] See, for example, the following news stories from the *New York Times*, the *Chicago Tribune*, and the *Los Angeles Times*: https://www.nytimes.com/2018/11/08/us/shooting-california-thousand-oaks.html https://www.chicagotribune.com/nation-world/ct-california-bar-shooting-20181108-story,amp.html https://www.latimes.com/world-nation/story/2019-09-01/us-mass-shootings-most-recent-list

2019). VTCs are a hybrid of mental health and drug courts. Veterans selected for admission to a VTC are given an opportunity to complete an individualized treatment program — which requires regular court appearances, participation in treatment sessions, and random drug testing — in exchange for avoiding incarceration (National Center for State Courts 2012; Tsai et al. 2016).[15,16]

2.3 Prior Literature on the Relationship between the Military Draft and Crime

Much of the economics literature examining the impact of military service on crime has used the draft lottery to generate exogenous variation in service. This approach identifies the effects of service (due to draft) on the criminal behavior of a civilian who is "randomly" drawn into military service. The results of these studies are mixed across countries, conflicts, and whether military service occurs during peacetime or wartime.

Rohlfs (2010) exploits heterogeneity in draft lottery risk across U.S. birth cohorts to estimate the impact of Vietnam War-era military service on crime. Using data from the National Vietnam Veterans Readjustment Study (NVVRS) and U.S. Defense Manpower Data Center (DMDC), he finds that exposure to the Vietnam draft was associated with a reduction in crime rates while servicemembers were on active duty, consistent with an incapacitation effect. However, following their return, exposure to the draft lottery was associated with an 8 to 16 incident per year increase in violent acts committed by African Americans. For whites, however, the estimated effect on violent crime was smaller and statistically indistinguishable from zero (1 to 1.5 incidents per year). Based on these findings, Rohlfs (2010) estimates that waging the Vietnam war generated approximately $65 billion in additional crime costs (in 2007 dollars).

[15] While there is state-level heterogeneity in eligibility for VTCs (Jaafari 2019; Timko et al. 2017), deferments to these courts are generally reserved for non-violent offenders who have suffered deployment-related psychological trauma (Brummet 2013). Still, surveys of VTC eligibility criteria suggest that 62 percent of VTCs will not entirely rule out violent offenders when determining veteran court eligibility (Flatley et al. 2017). Domestic violence appears to be the most common violent crime that will not necessarily disqualify a veteran from a VTC.

[16] Studies on the effectiveness of VTCs on recidivism have been hampered by the lack of exogenous variation in the assignment. The literature has mainly been descriptive and either compared VTC participants with VJO-engaged non-participants and controlled for observable differences between the two groups (Blue-Howells et al. 2013) or examined only VTC participants and relied on a before-after estimator (Tsai et al. 2016). Unsurprisingly, the findings from these studies are mixed, though there is some evidence that VTC participation is positively related to the probability of recidivism, which the authors interpret as a consequence of enhanced monitoring (Tsai et al. 2016).

Lindo and Stroecker (2014) also study the Vietnam conflict, but obtain data on the exact birth date of inmates to identify individual risk probabilities for the draft. Combining data from 1979, 1989, and 1991 Surveys of Inmates in State and Federal Correctional Facilities (SISFCF) with data from the Vital Statistics of the United States (VSUS), the authors create measures of incarceration probabilities for each day of birth for cohorts affected by Vietnam era draft lotteries. These data are then supplemented with 1983-1991 prison admissions data from the National Corrections Reporting Program (NCRP) and U.S. Census data from 2000 to estimate the effect of the Vietnam era military service on incarceration. Two-sample instrumental variable estimates show that military service increased the incarceration probability for a violent crime by 0.34 percentage points, but decreased the probability of confinement for a nonviolent crime by 0.30 percentage points. The authors explain this finding by suggesting that military service does not change an individual's propensity to commit a crime, but instead may cause them to commit more-severe crimes involving violence.

Siminski et al. (2014) study the impact of the Vietnam draft on crimes committed by Australian men using cross-cohort differences in exposure to the draft. Using administrative data on (i) birth dates for the universe of Australian male cohorts who were draft-eligible, and (ii) criminal court cases in the three largest Australian states (New South Wales, Victoria, and Queensland) between 1994-2010, the authors find no evidence that draft-induced military service increased the probability of violent or non-violent crime. The estimates are sufficiently precise that, with 95 percent confidence, the authors can rule out crime rate changes of larger than 11 percent in response to military service. Siminski et al. (2014) argue that differences in the effects of Vietnam war exposure for Australian as compared to U.S. combat veterans can be explained by Australia's Vietnam military training programs preparing recruits for a war of covert counterinsurgency rather than large-scale conventional battles.[17]

Other studies have examined the effect of military service across periods that include both peacetime and wartime service. Galiani et al. (2011) study the crime effects of conscription in Argentina between 1901 and 1995.[18] Using data from the Argentine Ministry of Justice, the

[17] Specifically, the authors argue that counterinsurgency training may be less realistic and, therefore, less desensitizing than conventional war training (Anderson and Rees 2015).

[18] Wars during this period included the Revolution of 1905, Revolución Libertoda in 1955, the 1963 Argentine Navy Revolt, the far-leftist insurgency of 1970-1979, Operativo Independencia in 1975-1977, the Falklands War (Malvinas War) in 1982, and the 1990 Gulf War.

authors find that conscription increased the likelihood of subsequent prosecutions and incarcerations among veterans of both peacetime and wartime military service. Across both types of service, IV estimates show that military service raises the probability of incarceration by 4 percent.

Albæk et al. (2017) use longitudinal administrative data from Denmark, which link draft records with the educational, labor market, and criminal records of the 1964 birth cohort, to estimate the effect of peacetime military service on crime. The authors find that conscripted peacetime service reduced property crime among youths by 18 percent in the year of military enlistment, and by 10 to 14 percent in each of the subsequent three years. They find no evidence that violent crime was affected by peacetime service in either the short- or long-run. Albæk et al. (2017) conclude that because there are no education or labor market effects of military service, the property crime reductions they detect can be explained by other channels, such as changing tastes toward crime.

Finally, Lyk-Jensen (2018) also study the military draft in Denmark using longitudinal administrative data. However, she examines a wider set of birth cohorts (1976 through 1983) than considered in Albæk et al. (2017) and finds no evidence that peacetime military service affected property or violent crime.

2.4 Post-9/11 War Deployments and Crime

Only one study of which we are aware has credibly estimated the impact of post-9/11 military deployments on crime. Anderson and Rees (2015) exploit the timing of overseas unit deployments among post-9/11 servicemembers stationed in Fort Carson, Colorado as a natural experiment to estimate the effect of local veteran presence on arrests.[19] They find that the return of a previously deployed unit had no impact on county-level arrests among 18-to-29 year-olds. However, they also document that the presence of a trained but *never-deployed* unit is associated with a 5.4 percent increase in the probability of a violent crime arrest in El Paso county, where Fort Carson is located. The authors attribute this result to the possibility that combat exposure may have increased soldiers' compassion, self-discipline, resilience, and spirituality, which could

[19] Note that these authors employ a different comparison group than the one generated from the draft lottery, relying on exogeneity of the timing of unit deployment assignments, following Lyle (2006). Unlike conscripted military service, this approach will capture the effects of deployments rather than the effect of military service per se.

deter violent crime.[20] However, because the authors use aggregate county-level crime data, they cannot detect whether crime averted (or caused) is committed by veterans. Furthermore, the time horizon of their work is limited to the immediate effects of combat deployments upon returning to the United States.

The current study extends the prior literature in general, and Anderson and Rees (2015) in particular, in several important ways. First, we are able to measure crime-related behaviors that do not necessarily result in civilian arrest, including (i) court-martials or Article 15 non-judicial punishments handled exclusively through the Uniform Code of Military Justice (UCMJ), (ii) criminal behavior committed outside of the unit's stationed location, including during overseas deployments, and (iii) criminal behavior that does not result in arrest. These crime measures have the potential to capture costly criminal behaviors that are undetectable in other data, such as those examined by Anderson and Rees (2015). Second, we have individual-level data on potentially important mechanisms through which post-9/11 combat deployments may affect criminal behavior, including TBI, PTSD, illicit substance use, and exposure to intense battlefield trauma. These data will allow us, for the first time, to descriptively examine the relative importance of several key channels. Third, our analyses rely on (i) a longitudinal dataset that allows us to control for the pre-enlistment propensity for criminal behavior, and (ii) a large military dataset designed to be representative of all active-duty U.S. servicemembers around the globe. This latter source will permit more generalizable estimates of the effect of post-9/11 combat service. Finally, our data will allow us to examine both short- and longer-run impacts of military deployments, which may be important if there are a considerable lag between separation from the military and engagement with the criminal justice system (Wolfe 2013).

3. Identification Strategy

The identification strategy we rely on employs a different comparison group than the one generated from studies of the draft lottery, and the results apply to a different population. Rather than identifying the causal effect of military service per se, we identify the impact of conditionally randomly assigning an active-duty serviceman to a combat deployment. The policy parameter we obtain is essential in assessing the social costs of waging war with AVFs and is

[20] Anderson and Rees (2015) conclude that soldiers deployed to war may reduce their taste for violence or even serve as role models in the community, instilling discipline, and discouraging aggressive behavior.

arguably more policy-relevant given that there is little prospect for the reinstitution of the military draft in the U.S. (Roper Center 2017).[21]

Our natural experiment relies on the administrative procedures by which senior commanders in the U.S. Armed Forces assign active duty deployed servicemembers to combat deployments. Individual servicemembers rarely receive deployment orders from Human Resources Command that are independent of orders given to other servicemembers.[22] Rather, units (i.e., battalions) receive deployment orders. In the assigning of individual servicemembers to units and in making unit deployment assignments and then issuing deployment duties, Armed Forces Human Resource Command treats active-duty servicemen of identical rank and military occupation as perfect substitutes. As a rule, deployment assignments are made without regard to other background characteristics of servicemen, including predisposition for crime, personality, family background, home circumstances, and marital status (Engel et al. 2010).[23]

Unit deployment decisions are based on (i) the operational needs of the Armed Forces determined by world events, and (ii) the readiness and availability of units, measured by the occupational skill set of unit servicemembers and the timeliness of equipment for shipment (Army Regulation 220-1). Such factors are exogenous to individual servicemembers' propensity for crime. There is no evidence that senior commanders issue unit-level deployment orders based on non-military characteristics of units (Carter and Skimmyhorn 2019; Sabia and Skimmyhorn 2019).

There are two main threats to this identification strategy. First, some individuals may be selected as "stay-back" personnel to remain on base for administrative duties. We argue that this is not a significant threat to identification because stay-back personnel represent just five percent of all active-duty servicemembers (Sabia and Skimmyhorn 2019). Studies using administrative data that have instrumented for individual deployment using unit-level deployment orders to address stay-back selection have found no evidence that the estimated effects of individual combat deployment on labor market outcomes (Sabia and Skimmyhorn 2019), program receipt (Sabia and Skimmyhorn 2019), or child well-being (Lyle 2006) are biased.

[21] See: https://ropercenter.cornell.edu/blog/suppose-they-gave-war-and-nobody-came-changing-opinions-draft

[22] The identification strategy we propose is similar to that used by a number of previous studies (e.g., Cesur et al. 2013; Cesur and Sabia 2016; and Lyle 2006).

[23] Thus, while servicemen can affect their likelihood of seeing combat through occupation choice and re-enlistment decisions (reflected in rank), servicemen of identical rank and occupation will face equal probabilities of combat deployments at any point in time.

Second, some active-duty servicemen may be deemed "non-deployable" for reasons related to physical and mental health (National Guard Association of the United States 2018). Estimates suggest that between five and 15 percent of active-duty servicemembers have been classified as non-deployable (Arnold et al. 2011; Lacdan 2018). Given that the health effects of post-9/11 combat may be an important channel through which combat service affects crime, we address this concern in two ways.[24] We restrict our sample to servicemen who have been deployed overseas and exploit conditionally random variation in the location of that overseas deployment assignment (e.g., to a combat or non-combat zone). And in a series of robustness checks, we condition on combat deployment length and explore the impact of exposure to enemy firefight within a combat zone. While this approach may understate the full effect of combat assignment, this "treatment effect" is unlikely to be contaminated by non-deployability.[25]

4. Data and Measures

Our analysis makes use of two data sources: the military module of the 2007-2008 National Longitudinal Study of Adolescent to Adult Health (Add Health), and the 2008 Department of Defense Survey of Health and Related Behaviors among Active Duty Personnel (HRBADP). While each dataset has its strengths and drawbacks, which are discussed below, the shortcomings of one survey are often rectified in the other.

The Add Health data set is a nationally representative school-based panel survey that, when weighted, is designed to be representative of all public and private U.S. middle and high school students during the 1994-95 school year. The first wave of data collection (Wave I) occurred during the 1994-95 academic year when respondents were largely ages 12 to 18. The first follow-up survey (Wave II) occurred one year later in 1996, the second follow-up survey (Wave III) in 2001-2002 when respondents were ages 18 to 26, and the third follow-up survey (Wave IV) in 2007-2008 when respondents were ages 24 to 34.[26]

[24] To the extent that the factors associated with being "non-deployable" are positively correlated with crime, our estimates would be a lower bound.

[25] An additional concern with the above-described identification strategy is the possible sample selection bias. This could occur if combat assignments affect the likelihood of re-enlistment (and hence future combat exposure), which could impact the sample on which we can measure our outcomes under study (Sabia and Skimmyhorn 2019). While all of our regressions control for rank and enlistment length, one of our datasets, the National Longitudinal Study of Adolescent Health, allows us to measure criminal outcomes both for those who remain on active duty and those who have separated back to civilian life.

[26] Wave V of the Add Health data was collected in 2016-17 and is due to be released in 2020.

Our Add Health-based analysis focuses on the military module of Wave IV. We identify 482 men who (i) reported active duty service in the U.S. Armed Forces (Army, Marines, Navy, or Air Force) after age 17, (ii) reported an overseas deployment, and (iii) provided non-missing information on criminal behavior. Of these 482 men, 286 had separated from the military by the time of the Wave IV survey, while 196 remained on active duty.

We generate our measures of combat assignments using self-reports of overseas deployments. The variable *Combat Deployment* is set equal to 1 if the active-duty deployed serviceman reports having been sent to a combat zone and set equal to 0 if overseas deployments were exclusive to non-combat zones. The definition of a combat zone is made by senior commanders. Over the sample period during which we measure deployments, combat deployments are almost exclusively in the post-9/11 period and reflect deployments to Iraq and Afghanistan as part of Operation Enduring Freedom and Operation Iraqi Freedom.[27] Because servicemen receive hostile fire or imminent danger pay when deployed to combat zones, there is strong reason to expect that survey responses will be accurate. In our analysis sample, 75.5 percent of active duty deployed servicemen received a combat zone deployment assignment while 24.5 percent were exclusively deployed overseas to non-combat zones.

In addition to deployment assignments, the Add Health includes information on whether an overseas deployed serviceman was exposed to combat. Specifically, respondents are asked:

> *"Thinking about all of your deployments, [have you] or members of [your] unit, received incoming fire from small arms, artillery, rockets, or mortars…or [has your] unit fired on the enemy?"*

If the active-duty serviceman reported engaging the enemy in a firefight, *Combat Exposure* is set equal to 1; it is set equal to 0 otherwise. We find that 36.7 percent of servicemen were exposed to combat during their overseas deployment assignments.

We measure criminal behavior on the extensive margin using self-reported information from the Add Health. While these measures may understate the true prevalence of crime due to its illicit nature, self-reported measures will also capture criminal behavior that remains

[27] During this period, combat zone deployments could also include assignments in Qatar, Kuwait, Saudi Arabia, Yemen, Sudan, Tunisia, Egypt, Syria, and Guantanamo Bay, Cuba.

undetected, did not result in arrest, and would not be captured in administrative data.[28] Moreover, as long as measurement error in crime is unrelated to deployment assignment, marginal effects in terms of percent changes in crime should be unbiased.

At Wave IV, respondents are asked whether, in the last year, they had "deliberately damaged property that didn't belong to you" or "stole something." *Property Crime* is set equal to 1 if respondents reported stealing or damaging property, and it is set equal to 0 otherwise. We find that 9.8 percent of deployed active duty servicemen report committing a property crime.[29]

Second, *Violent Crime* is set equal to 1 if, in the last 12 months, respondents had "used or threatened to use a weapon to get something from someone," had "pulled a knife or gun on someone," had "shot or stabbed someone," "took part in a physical fight where a group of your friends was against another group," or "hurt someone badly enough in a physical fight that he or she needed care from a doctor or nurse." Otherwise, *Violent Crime* is set equal to 0. We find that 24.2 percent of the sample reported engaging in violent criminal activity.[30]

Next, *White Collar Crime* is set equal to 1 if the respondent had, in the last year, "used someone else's credit card, bank card, or ATM card without his/her permission or knowledge," "deliberately write a bad check," or "buy, sell, or hold stolen property." Otherwise, *White Collar Crime* is set equal to 0. Four percent of overseas deployed servicemembers reported commission of white-collar crime.[31]

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[28] Survey administrators took several steps to maintain data security and to minimize the potential for interviewer influence. First, respondents were not provided with any printed questionnaires. Instead, all data were recorded on laptop computers. Second, the respondents listened to pre-recorded questions through earphones for sensitive topics such as criminal behavior and entered their answers directly on the laptops. The rates of crime reported in the Add Health have been shown to be consistent with those of other sources (see Anderson et al. 2015; Mocan and Tekin 2010; Currie and Tekin 2012).

[29] Respondents to the Add Health were asked:

"In the past 12 months, how often did you deliberately damage property that didn't belong to you?"
"In the past 12 months, how often did you steal something (worth more/less than $50)?"

[30] Respondents to the Add Health were asked:

"In the past 12 months, how often did you use/threaten to use a weapon to get something from someone?"
"Which of the following things happened in the past 12 months: You pulled a knife or gun on someone? (Yes/No); You shot or stabbed someone? (Yes/No)"
"In the past 12 months, how often did you take part in a physical fight where a group of your friends was against another group?"
"In the past 12 months, how often did you hurt someone badly enough in a physical fight that he or she needed care from a doctor or nurse?"

[31] Respondents to the Add Health were asked:

"In the past 12 months, how often did you use someone else's credit card, bank card, or automatic teller card without their permission or knowledge?"
"In the past 12 months, how often did you deliberately write a bad check?"
"In the past 12 months, how often did you buy, sell, or hold stolen property?"

We measure illicit drug sale in an analogous way, with an indicator, *Drug Sale,* set equal to 1 if the respondent "sold marijuana or other drugs" in the last year. We observe that 3.5 percent of the deployed sample reported selling drugs in the last year.

Finally, using self-reports of arrest histories, we generate an indicator for whether a respondent was arrested in the last year (*Arrested*). We find that 3.8 percent of deployed servicemen reported a criminal arrest in the last year.

In Table 1, we present the means of our five crime outcomes by deployment assignment and combat exposure. As shown in the table, the prevalence of crime is higher for those assigned to combat zones relative to non-combat zones (column 2 vs. column 3) and higher still for those assigned to combat zones where enemy firefight materializes (column 4).[32] This pattern appears to hold across property, violent, white-collar, and drug crimes. The only exception to this pattern is the rate of arrest, which seems to be lower among those assigned to combat zones relative to non-combat zones.

An important advantage of the Add Health is that the survey includes information on all key military observables available to Human Resources Command when the conditional random assignment of servicemembers to overseas deployments are made. These include the branch of service (Amy, Marines, Navy, and Air Force), highest rank attained[33], duration of service, occupation, and whether still on active duty at the time of the Wave IV survey. The availability of these variables ensures that the observed variation in overseas deployment assignment will be orthogonal to criminal behavior.

The Add Health is also rich in pre-deployment measures of personal and family background characteristics including age, race/ethnicity, height, weight, number of siblings, parental educational attainment, parental marital status, Peabody Picture Vocabulary Test (PPVT) score, and religiosity (each measured at Wave I). Importantly, we can also measure criminal behaviors prior to enlistment into the U.S. Armed Forces. Given the procedures through

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[32] Appendix Table 1 shows the descriptive statistics for control variables in the Add Health.

[33] For enlisted servicemembers, ranks in the sample vary from Army Private to Chief Warrant Officer in the Army, Seaman Recruit to Chief Petty Officer in the Navy, Airman to Master Sergeant in the Air Force, and Marine Lance Corporal to Staff Sergeant in the Marines. For officers, ranks vary from Captain to Chief Warrant Officer in the Army, Lieutenant Junior Grade to Lieutenant Commander in the Navy, Lieutenant to Chief Warrant Officer in the Air Force, and Second Lieutenant to Captain in the Marines.

which military deployment assignments are made, deployment assignments should be unrelated to these observables, conditional on within-branch rank, occupation, and timing of service.

One of the limitations of the Add Health is that the size for the military sample is relatively small. This means that the research design has relatively low statistical power, a problem that is exacerbated when attempting to examine heterogeneous effects of combat across service branches and servicemember characteristics. Moreover, estimated combat effects using the Add Health data may not generalize to all active duty overseas deployed servicemen.

To supplement our Add Health analysis, we turn to the 2008 HRBADP Survey. The HRBADP Survey, a representative survey of the active-duty members of the armed forces, includes 28,546 men and women between ages 18 to 50. Although a large majority of interviews were conducted at facilities under the jurisdiction of the U.S. Military, such as military bases, camps, posts, stations, yards, and centers, a small fraction of surveys were completed via mail for those who could not be present in on-site surveys. Participating individuals completed these self-administered surveys via the use of pencil and paper. As the participants did not answer the questions using a computer-assisted self-interview (CASI) survey instrument, estimates on sensitive information may suffer from underreporting bias.[34] Nevertheless, if combat assignment is orthogonal to the likelihood of criminal involvement, such measurement error should not affect our results. When weighted, the HRBADP survey sample is designed to be representative of all U.S. active-duty personnel serving around the globe.

Our main analysis sample is comprised of 11,542 active duty servicemen who (i) reported an overseas deployment assignment, (ii) provided non-missing information on whether they were exposed to combat, and (iii) provided non-missing information on crime-related behavior. In contrast to the Add Health, the HRBADP survey measures criminal behavior that has exclusively occurred while the servicemember was on active duty. Hence, we measure the short-run effects of combat assignment on criminal behavior that could be (or is) punished according to military rather than civilian law.

Our primary measure of combat in the HRBADP survey is *Combat Exposure*, which captures a similar measure as that reported in the Add Health. Respondents are asked:

[34] The data collection effort was conducted by the Research Triangle Institute. Bray et al. (2009) provide detailed information on the HRBADP data collection procedures.

"Thinking about all of your deployments, [have you] or members of [your] unit, received incoming fire from small arms, artillery, rockets, or mortars…or [has your] unit fired on the enemy?"

Combat Exposure is set equal to 1 for servicemen who reported engaging the enemy in a firefight, and it is set equal to 0 otherwise. We find that 51.5 percent of the HRBADP sample reported exposure to combat, much of it in Afghanistan and Iraq, including the so-called surge in 2007-2008. One limitation of the HRBADP deployment data is that we cannot disentangle overseas deployments to non-combat zones and deployments to combat zones without enemy engagement beyond the year before the administration of the survey. Thus, if there are crime effects of combat from combat zone deployments without combat exposure – perhaps because of the psychological consequences of fearing war could materialize – estimates from the HRBADP Survey will understate the full crime effect of combat assignment.

We measure five crime-related outcomes in the HRBADP survey. First, respondents are instructed to indicate whether (and how many times) in the last year, the following events happened to them:

"I received a UCMJ punishment (e.g. Court Marital, Article 15, Captain's Mast, Office Hours, Letter of Reprimand, Other)."
"I had trouble with the police (civilian or military)."
"I was arrested."
"I got into a fight where I hit someone other than a member of my family."

From these items, four dichotomous variables are generated. *UCMJ Punishment, Trouble with Police, Arrested*, and *Physical Fight* are each set equal to 1 if the respondent indicated that the event had happened to him at least once in the last year. These variables are set to 0 otherwise. In our analysis sample, 6.9 percent of individuals reported receiving a UCMJ punishment, 6.7 percent reported trouble with police, 0.5 percent reported to being arrested, and 6.7 percent reported hitting others in physical fights. Finally, respondents are asked whether they were currently gang members (*Gang Member*). We find that 1.3 percent of our analysis sample reported being in a gang.

Table 2 presents means of the key variables from the HRBADP Survey for the pooled sample (column 1), those deployed to combat zones with enemy firefight (column 2), and those

deployed to non-combat zones or combat zones without enemy firefight (column 3).[35] Consistent with findings from the Add Health in Table 1, results from the DOD survey show higher crime prevalence and gang membership rates for those exposed to combat during combat deployments as compared to those deployed overseas who do not face enemy engagement. For example, UCMJ punishments were 36.2 percent more likely for active duty servicemen exposed to combat relative to those who were deployed but not exposed to combat.

An important advantage of the HRBADP Survey is the large sample that, when weighted, is designed to be representative of all active duty deployed servicemembers. With a sample size over 25 times greater than that of the Add Health military module, the HRBADP Survey data allow for higher statistical power as well as an exploration of heterogeneous combat effects by the branch of service and servicemember background. Moreover, the estimates have a greater degree of external validity. However, limitations of these data include (i) an inability to examine longer-run post-separation criminal behavior, (ii) lack of data on military occupation, which poses a (surmountable) challenge to identification, and (iii) an inability to disentangle overseas deployments to non-combat zones from deployments to combat zones without enemy firefight beyond the year prior to the 2007-2008 data collection effort. Many of these limitations are mitigated by the use of the complementary Add Health data.

5. Empirical Approach

We begin with a set of descriptive tests of the exogeneity of combat deployments. We use ordinary least squares regression (OLS) to examine whether personal and family background characteristics are related to the probability of a combat deployment, after controlling for military characteristics of servicemen available to Human Resources Command:

$$\textit{Combat Deployment}_i = \delta_0 + \mathbf{M}_i\,\delta_1 + \mathbf{X}_i\,\delta_2 + \delta_3 \boldsymbol{PreCrime}_i + \nu_i \quad (1)$$

$$\textit{Combat Exposure}_i = \alpha_0 + \mathbf{M}_i\,\alpha_1 + \mathbf{X}_i\,\alpha_2 + \alpha_3 \boldsymbol{PreCrime}_i + \mu_i \quad (2)$$

where $\mathbf{M}_i$ is a vector of military observables available to Human Resources Command including indicators for the branch of service, military rank, occupation, year of enlistment, whether on active duty at the time of the Wave IV survey, and the timing of the interview; $\mathbf{X}_i$ is a vector of

[35] Appendix Table 2 displays the summary statistics for control variables in the HRBADP.

personal and family background controls for individual *i* measured prior to enlistment, including age, gender, height, number of siblings, parental educational attainment, parental marital status, Peabody Picture Vocabulary Test (PPVT) score, race/ethnicity, religion, height, and weight; and $\boldsymbol{PreCrime}_i$ is a vector of servicemembers' criminal behaviors measured prior to enlistment into the U.S. Armed Forces.[36] If combat assignment is conditionally random, then $\delta_2 = \delta_3 = \alpha_2 = \alpha_3 = 0$.

Next, we estimate the impact of combat assignment on crime in the Add Health:

$$Crime_i = \beta_0 + \beta_1 Combat\ Deployment_i + \mathbf{M}_i \beta_2 + \mathbf{X}_i \beta_3 + \beta_4 \boldsymbol{PreCrime}_i + \varepsilon_i \quad (3)$$

If combat assignment is exogenous to crime, then β_1 can be interpreted as the causal impact of assignment to a combat zone (relative to assignment to a non-combat zone overseas deployment) on the probability of subsequent crime commission.

We then add an indicator for whether the respondent was exposed to combat during his combat deployment assignment:

$$Crime_i = \gamma_0 + \gamma_1 Combat\ Deployment\ without\ Exposure_i + \gamma_2 Combat\ Exposure_i + \mathbf{M}_i \gamma_3 + \mathbf{X}_i \gamma_4 + \gamma_5 \boldsymbol{PreCrime}_i + \sigma_i \quad (4)$$

In the above specification, γ_1 can be interpreted as the crime effect of deployment to a combat zone without enemy firefight, and γ_2 can be interpreted as the crime effect of deployment to a combat zone with enemy engagement.

Turning to the HRBADP Survey, after conducting a similar set of descriptive tests for the exogeneity of deployment assignment, we estimate:

$$Crime_i = \lambda_0 + \lambda_1 Combat\ Exposure_i + \mathbf{H}_i \lambda_2 + \mathbf{Z}_i \lambda_3 + \omega_i \quad (5)$$

where $\mathbf{H}_i$ is a vector of military observables available in the HRBADP Survey, including the branch of service, branch-specific rank-by-major command, and $\mathbf{Z}_i$ is a vector of individual-level observables including race/ethnicity, age, and educational attainment. Our key coefficient of interest, λ_1, should produce unbiased estimates of the effect of combat exposure on criminal

[36] Since the sampling design of Add Health is school-based, the standard errors are corrected for clustering at the school level.

activity to the extent that this specification captures the conditional random assignment of servicemembers to combat zones.[37]

6. Results

6.1 Exogeneity of Combat Deployments

We begin by presenting results from balancing tests performed on the Add Health sample in Table 3A. These tests are designed to capture whether our assumption of conditional random assignment of deployment is valid. All models include the vector of military observables (the branch of service, rank, occupation, timing, and length of enlisted service), and then adds sequential sets of background characteristics from the vectors $\mathbf{X}_i$ and $\boldsymbol{PreCrime}_i$. Note that the coefficients and standard errors, shown in Table 3, are generated from regressions that include controls for $\mathbf{M}_i$ and the addition of pre-enlistment non-military characteristics *one at a time* (and then finally all together). Thus, they are tests of conditional mean differences in personal and family background characteristics, conditional on all of the military observables (branch of service, military rank, occupation, and length/timing of enlistment) that Human Resources Command uses to make deployment assignments.

Our results in column (1) of Table 3A show that background characteristics in $\mathbf{X}_i$ have no individual predictive power in explaining deployment to a combat zone. Moreover, criminal behavior measured prior to enlistment (at Wave I of the Add Health survey) does not predict being assigned to a combat zone (joint F-stat = 0.78 and p-value = 0.54). Finally, when all variables in the vector $\mathbf{X}_i$ and $\boldsymbol{PreCrime}_i$ are simultaneously included as controls, a test of the joint significance of all of these observables generates an F-statistic of 1.01 and a p-value of 0.46. This pattern of findings is consistent with the administrative procedures through which active-duty servicemen are assigned to their deployment duties.

We then repeat the above analysis by exploring whether those assigned to combat zones where enemy firefight materializes are systematically different from those deployed to combat

[37] Prior work by Cesur et al. (2019) and Cesur and Sabia (2016) finds that Major Command-by-rank-by-education controls adequately proxy for occupation. We take two approaches to push this assumption further. First, in Appendix Table 3, we show that when we restrict the Add Health observables to those available in the HRBADP Survey and re-estimate equations (3) and (4), estimates of β_1, γ_1, and γ_2 are largely unchanged. Second, we use a nearest neighbor matching approach to match combat deployed, and non-combat deployed servicemen using each of the right-hand-side variables in equation (5). As discussed below, the nearest neighbor matching estimates of the effect of combat deployments on crime are quantitatively similar to those produced by equation (5).

zones without enemy firefight (column 2) or to non-combat zones (column 3). The pattern of results is remarkably similar. The only exception is for Hispanics, who are less likely to be assigned to combat zones with enemy firefight. In supplemental analyses, we restrict our sample to non-Hispanic whites and find a pattern of results similar to those reported on the full sample.

In Table 3B, we show descriptive evidence for conditional random assignment in the HRBADP Survey. Because we lack data on occupation, we are careful to match "treatment" (assigned to combat zones where enemy firefight occurred) and "control" (assigned to non-combat zones or combat zones without enemy firefight) servicemen on available military characteristics. Consistent with our Add Health-based results, the results in Table 3B show little evidence that sociodemographic controls in the HRBADP Survey predict combat exposure. Together, findings in Tables 3A and 3B are consistent with the hypothesis that deployment orders are issued exogenously to future crime commission.

6.2 Add Health Results

Table 4 presents estimates of the effect of combat zone deployment on crime using the Add Health data.[38] Controlling for the vector $\mathbf{M}_i$ alone (Panel I), we find that assignment to a combat zone is associated with a 0.092 increase in the probability of committing a property crime (column 1), and a 0.037 increase in the probability of committing a white-collar crime (column 3). While the estimated effects on violent crime (column 2) and drug crime (column 4) are also positive and economically meaningful, 0.064 and 0.023 respectively, these estimates are not statistically distinguishable from zero at conventional levels. Interestingly, the estimated effect of combat zone deployment on the probability of arrest is zero (column 5). This could suggest that criminal activities in which servicemembers are engaged are infrequently detected or punished. If that is the case, focusing on arrest probabilities alone may understate the crime-related social costs of war. However, our measure of arrests captures only those taken into custody in the last year and released before the administration of the Add Health survey. Thus, this measure likely only captures minor crimes that are committed recently.

In Panel II of Table 4, we explore heterogeneity in the crime effects of combat service by whether the combat zone deployment involved exposure to enemy firefight. Our findings suggest

[38] Standard errors are corrected for clustering on the school, which is the unit of sampling in the Add Health. However, if we cluster standard errors at the occupation-by-rank level, the results are qualitatively similar.

that the effects of combat deployments on property crime (column 1) and white-collar crime (column 3) do not differ by whether combat exposure materializes. However, our point estimates suggest that exposure to enemy firefight has a larger positive effect (0.118 vs 0.018) on the probability that a serviceman subsequently commits a violent crime (column 2). A similar pattern of result appears for drug crimes (column 4), though this effect is imprecisely estimated.

In the final two panels of Table 4, we add controls for individual and family background characteristics (Panel III) and pre-enlistment crime (Panel IV) respectively. We highlight two important findings in these panels. First, the magnitudes of the estimated effects of combat assignment on crime are quite similar to those shown in Panel II, consistent with the hypothesis that combat deployments are assigned exogenously to a wide set of pre-enlistment non-military observables, further adding to our confidence in our identification strategy. Second, the findings in Panel IV now show that combat exposure is associated with *significantly* larger increases in the probabilities of both violent and drug crime, in part because the estimates are more precisely estimated. In summary, the findings in Table 4 suggest that physical or psychological shocks from firefight exposure may be more important mechanisms to explain increases in violent offenses as compared to property crime.

In Table 5, we explore whether the effects of combat zone deployment on crime differ for those who have separated from the military as compared to those who remain on active duty. While the sample sizes are relatively small, and therefore, the estimates are imprecise, we find the strongest evidence for property crime effects among those whose active duty service has concluded rather than for those who remain on active duty. To the extent that those who remain on active duty are more stable financially and have easier access to credit, healthcare, and housing, such individuals may have less of an incentive to engage in economically motivated illegal activities. In contrast, combat-induced increases in violent crime are present both for those who have separated from the military and those for whom service is ongoing at the time of the Wave IV survey.

6.3 HRBADP Survey Main Findings

Table 6 presents our main findings from the HRBADP Survey.[39] In Panel I, we control for military observables available in the HRBADP survey. We find that assignment to a combat zone with enemy firefight induces a 0.025 increase in the probability of receiving a punishment under the UCMJ (column 1), a 0.022 increase in the probability of trouble with military or civilian police (column 2), a 0.005 increase in the probability of arrest (column 3), a 0.035 increase in violent physical fighting with strangers (column 4), and a 0.017 increase in the probability of gang membership (column 5). These results provide further evidence that post-9/11 combat deployments increased criminal activity among recent combat veterans and suggest that many of these crime costs materialize prior to separation from the U.S. Armed Forces.

While the results presented in the top panel are qualitatively similar to those shown in Table 4, they are quantitatively smaller. This is consistent with the above argument that the effect of deployment on crime is likely to be understated in the analysis with the HRBADP data as compared to the Add Health data to the extent that those assigned to combat zones without enemy firefight also experience increases in the propensity for crime.

In Panel II of Table 6, we add controls for non-military characteristics to the regression. Reassuringly, the estimated crime effects are nearly identical, consistent with the conditional random assignment of overseas deployments. Along the same lines, in Panel III, we use a nearest neighbor matching procedure to ensure that those deployed to combat zones with enemy firefight and those deployed without seeing enemy firefight were well matched on observables available to Human Resources Command (branch of service, rank, occupation, Major Command, and education).[40] The results from the matching estimator are quantitatively similar to those obtained in Panels I and II.

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[39] Estimated effects of combat service on crime obtained from the HRBADP survey data may differ from those obtained from the Add Health data for several reasons. First, the sample is more representative of all active-duty servicemen than the non-representative Add Health sample. Second, combat zone deployments without enemy firefight cannot be disentangled from non-combat zone deployments in the HRBADP data, a limitation that may understate the crime effects of combat deployments. Finally, estimated crime effects we uncover in the HRBADP data are driven by criminal activity prior to separation from the U.S. Armed Forces. For these reasons, we view the results obtained from the analysis with HRBADP data as a complement to the analysis with Add Health data rather than a substitute. Standard errors in all of our HRBADP Survey results are corrected for clustering on the stratum, which is the unit of sampling. If we instead cluster standard errors at the Major Command-by-rank level, the results are nearly identical.

[40] We employ a nearest neighbor matching procedure without replacement using a caliper value of 0.00001. Employing alternative caliper values, such as 0.001, 0.0001, and 0.0005, produce results very similar to those presented here. We also estimated out models using alternative matching procedures, including nearest neighbor

An examination of heterogeneity in the crime effects of combat exposure by the branch of service suggests larger crime effects for those serving in the Army (Panel IV), Marines (Panel V), and Navy (Panel VI) relative to the Air Force (Panel VII), with the exception of arrests. [41] These results are consistent with much of the prior military literature, which finds that the adverse health (Cesur et al. 2013), human capital (Sabia and Skimmyhorn 2019; Cesur et al. 2016), and family violence (Cesur and Sabia 2016) effects of post-9/11 deployments are smaller for airmen relative to those serving in other branches.

In Table 7, we find that the crime effects of post-9/11 combat exposure are more substantial for servicemen younger than age 33 as compared to those ages 33 and older (Panel I). Interestingly, we also find evidence that post-9/11 combat assignments increase criminal behavior among women (Panel II) and do not appear to largely differ by race or ethnicity, except white-collar crime (Panel III). Finally, each of the crime effects we uncover is uniformly larger among enlisted servicemen as compared to officers (Panel IV), consistent with evidence that the largest adverse health effects of combat service are found for enlisted servicemen (Cesur et al. 2019). This pattern is also consistent with the notion that the opportunity cost of engaging in crime is likely to be higher among officers.

6.4 Length of Deployments and Intensity of Combat Exposure

While combat exposure is more likely with longer and more frequent deployments, it may be important to disentangle these effects to judge how important psychological trauma and physical injury of enemy engagement are in explaining the crime effects we observe. We do this in Table 8. Across crime outcomes, we find evidence that the length and frequency of combat deployments are positively related to the probability of crime (columns 2 and 3).[42] However, including combat exposure, the number of post-9/11 combat deployments, and average length of combat deployment all in the same model (column 4) results in only one variable, combat exposure, being economically consequential and statistically distinguishable from zero in most

matching with replacement, k-nearest neighbor, and exact matching on the branch of service. Our results are robust to employing these matching methods.

[41]However, given that the mean for arrests is less than one half of one percent, we test the robustness of this finding to the use of a probit model rather than OLS. These findings in Appendix Table 4 show little evidence that combat exposure affected the probability of arrest for those in the air force. The remainder of findings are comparable to our OLS results.

[42] There is also strong evidence that length and number of deployments (as well as the location of deployment) is also conditionally randomly assigned (Anderson and Rees 2015; Lyle 2006).

cases. This suggests that combat exposure is likely the primary trigger for the rise in criminal propensities among active-duty servicemen.

To examine whether the intensity of combat exacerbates the effects of combat exposure, Panel I of Table 9 presents estimates of the effect of a combat intensity index on crime. This index, provided by the HRBADP survey, is constructed based on 17 questions pertaining to combat experiences, including whether the veteran has engaged in hand-to-hand combat, fired on the enemy, knew someone who died in combat, and interacted with prisoners of war.[43] The index ranges from 0 to 68 with higher levels indicating more intense combat. We use the constructed combat intensity index by the HRBADP, which defined *High Intensity Combat* as having 10 or more positive indicators of combat and *Lower Intensity Combat* as 1 to 9 (Bray et al. 2009).

The estimates, displayed in Panel I of Table 9, show that high intensity combat exposure has a far larger effect on the probability of crime commission than lower intensity combat exposure. For example, high intensity exposure is associated with a 0.038 increase in the probability of UCMJ punishment (column 1), a 0.008 increase in the probability of arrest (column 3), and a 0.029 increase in the probability of being a gang member (column 5).

In Panel II of Table 9, we explore the impact of specific forms of battlefield trauma on crime. Specifically, we examine whether the veteran (i) suffered an injury during his deployment to a combat zone; (ii) witnessed the injury or death of an allied soldier; and (iii) witnessed the

[43] The combat exposure index is constructed by the HRBADP survey based on answers to the following 17 questions, with possible frequencies: 0; 1 to 3; 4 to 12; 13 to 50; and 51 or more.

Thinking about all of your deployments, how many times have you had each of the following experiences?
I was sent outside the wire on combat patrols, convoys, or sorties.
I, or members of my unit, received incoming fire from small arms, artillery, rockets, or mortars.
I, or members of my unit, encountered mines, booby traps, or IEDs (improvised explosive devices).
I worked with landmines or other unexploded ordnances.
My unit fired on the enemy.
I personally fired my weapon at the enemy.
I engaged in hand-to-hand combat.
I was responsible for the death or serious injury of an enemy.
I witnessed members of my unit or an ally unit being seriously wounded or killed.
My unit suffered causalities.
I saw dead bodies or human remains.
I handled, uncovered, or removed dead bodies or human remains.
Someone I knew well was killed in combat.
I took care of injured or dying people.
I interacted with enemy prisoners of war.
I witnessed or engaged in acts of cruelty, excessive force, or acts violating rules of engagement.
I was wounded in combat.

death of an enemy soldier. We find that the relationship between combat deployment and subsequent crime is the strongest among those who suffered an injury. This finding lends further support to the notion that physiological or physical trauma suffered during combat missions may be key to subsequent criminal behavior.[44]

6.5 Mechanisms

The above results, across two datasets, show that post-9/11 combat deployments substantially increased the probability of veteran crime commission. We now descriptively explore observable mechanisms through which combat exposure might affect crime. Specifically, we examine the roles of TBI, PTSD, binge drinking, and substance abuse as potential channels.[45] First, consistent with the prior literature, we document in Appendix Table 5 that combat assignment to zones with enemy firefight substantially increases the risk of exhibiting the symptoms of TBI, PTSD, and substance abuse.

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[44]However, we note that 70 percent of those who witnessed an enemy death also witnessed an ally hurt, and 50 percent of those who observed the death or injury of an ally also observed an enemy death.

[45] We define *TBI* as an indicator using HRBADP Survey criteria in which an individual is identified as exposed to TBI based on responses to a list of items about events experienced during deployments, including*"[experiencing] blast or explosion (IED, RPG, land mine, grenade, etc.)""vehicular accident/crash (any vehicle, including aircraft),""fragment wound above the shoulders,""bullet wound above the shoulder, "fall," lost consciousness or got 'knocked out,'" "felt dazed, confused, or 'saw stars',"didn't remember the event," "had a concussion or symptoms of a concussion (such as headache, dizziness, irritability, etc.)*," or "*had a head injury*."

We define *PTSD* as an indicator using HRBADP Survey criteria in which an individual is identified as exhibiting the symptoms of PTSD based on responses to a list of items about things bothering the respondent in the last month, including *"Repeated, disturbing memories, thoughts or images of a stressful experience," "Repeated, disturbing dreams of a stressful experience," "Suddenly acting or feeling as if a stressful experience were happening again (as if you were reliving it)," "Feeling very upset when something reminded you of a stressful experience," "Having physical reactions (e.g., heart pounding, trouble breathing, sweating) when something reminded you of a stressful experience," "Avoiding thinking about or talking about a stressful experience or avoiding having feelings related to it," "Avoiding activities or situations because they reminded you of a stressful experience," "Trouble remembering important parts of a stressful experience, "Loss of interest in activities you used to enjoy," "Feeling distant or cut off from other people," "Feeling emotionally numb or being unable to have loving feelings for those close to you," "Feeling as if your future somehow will be cut short," "Trouble falling or staying asleep," "Feeling irritable or having angry outbursts," "Having difficulty concentrating," "Being "superalert" or watchful or on guard," or "Feeling jumpy or easily startled."*

Binge Drinking is a dummy variable set equal to 1 if the respondent reports consuming at least 5 drinks in a single occasion in the past 30 days.

Substance Abuse is a dummy variable set equal to 1 if the respondent reported consuming marijuana, cocaine, PCP, MDMA, hallucinogens, methamphetamine, heroin, GHB, inhalants, or painkillers (for non-medical reasons) in the last 12 months.

In Table 10, we descriptively explore their mediating role of various factors. In Panel I, we reproduce our baseline estimates of the effect of combat exposure on crime. Then, we add a control for TBI (Panel II), PTSD (Panel III), binge drinking (Panel IV), substance abuse (Panel V), and finally all of these endogenous mediators (Panel VI). The findings show that the estimated crime effects fall by the largest magnitudes (20 to 38 percent) with the addition of TBI in the model (Panel II versus Panel I). PTSD appears to be the second most important mediator, with estimated crime effects declining by 19 to 26 percent. Binge drinking and substance abuse appear to be relatively less important pathways to crime, suggesting that addiction may play a smaller role. Together, the inclusion of controls for all of our observed mediators (Panel VI) reduces the magnitudes of our estimated crime effects of combat by 41 to 54 percent.

7. Conclusions

U.S-led wars in Iraq and Afghanistan following the September 11, 2011 attacks on the American homeland constitute the longest wars in the nation's history. The Global War on Terrorism (GWOT) constitutes the second most costly war in American history, topped only by World War II (Congressional Research Service 2010). Estimates from the Watson Institute (2019) place the budgetary costs of this conflict at over $5.6 trillion, which includes increased public expenditures on VDC benefits (Sabia and Skimmyhorn 2019; Stiglitz and Bilmes 2008) and Veterans Health Administration-provided health care benefits (Congressional Budget Office 2010; Stiglitz and Bilmes 2008). However, the cost of fighting the GWOT on public budgets may vastly understate the social costs of this conflict if waging Operation Enduring Freedom, Operation New Dawn, and Operation Iraqi Freedom have increased the likelihood of criminal behavior by veterans following war deployments.

This paper provides new evidence on the effect of post-9/11 combat deployments on subsequent criminal behavior among veterans. Using data from two national surveys and exploiting exogenous assignment to combat zones generated by U.S. Armed Forces administrative procedures, our analysis shows that deployment to combat zones in the post-9/11 era substantially increased criminal tendencies among servicemembers. In addition to significant increases in property and violent crime, our results indicate that combat exposure resulted in a higher likelihood of trouble with military and civilian police, arrest, and punishment under UCMJ. Our auxiliary analysis suggests that the rise in TBI and PTSD are the likely explanations

behind the rise in crime among servicemembers. Thus, the changing nature of modern warfare and war-related injuries, along with the waging of war with AVFs, may help to explain why the crime effects of post-9/11 warfare are larger than in prior conflicts.

Back-of-the-envelope calculations suggest that combat exposure generated substantial economic costs to society in the form of additional crime. Using the marginal effects we obtain from Table 4, and per-crime costs of property and violent crime reported by Chalfin (2015), we estimate post-9/11 combat exposure-induced crime costs of approximately $26.4 billion for violent crime and $315 million for property crime (in 2018 dollars).[46] Moreover, we note that these estimates may be lower bound given that assignment to a combat zone without enemy firefight may also generate increases in property crime.

Our findings have a number of important public policy implications. The discovery of high social costs of crime from post-9/11 combat deployments significantly influences the cost-effectiveness of programs designed to ease veterans' transition from military to civilian life, such as the Department of Labor Veterans' Employment and Training Service (VETS) and the Morale, Welfare, and Recreation (MWR) programs. Moreover, our findings have important implications for the optimal provision of services designed to comprehensively address PTSD, TBI, and other combat-related trauma among active-duty personnel. Finally, a recent poll shows that the American public is increasingly opposed to new U.S. military interventions (Carden 2018). Revealing previously undocumented social costs of post-9/11 warfare may strengthen this sentiment and influence future decisions by the U.S. government to engage in military action.

[46] The median cost per incident for each index crime among estimates in the extant literature reported by Chalfin (2015) is $5,600,000 for homicide, $157,500 for rape, $40,950 for robbery, $89,250 for assault, $5,431 for burglary, $2,086 for larceny, and $9,341 for motor vehicle theft in 2012 dollars. Using the median cost per incident and the total number of each index crimes committed in the United States in 2018 reported by the Federal Bureau of Investigation, we calculated the weighted average cost of each violent crime (i.e., homicide, rape, robbery, and assault) to be $176,619 and each property crime (i.e., burglary, larceny, and motor vehicle theft) to be $3,827, both in 2018 dollars. A 2018 RAND study indicates that, of the 2.77 million service members deployed to GWOT in Iraq and Afghanistan, 2.1 million were active duty members (Wenger et al. 2018). According to the HRBADP, which is designed to be representative of active duty forces, 51.5 percent of service members had been exposed to combat. Combining these figures with our estimates of the effect of combat exposure on violent crime ranging from 0.118 to 0.138 (Panels II through IV of Table 4) and 0.076 to 0.093 for property crime, we obtain cost estimates of $22.5 to $26.4 billion for violent crime and $315 to $385 million for property crime.

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Table 1: Combat and Crime Measures, Add Health

	(1) Full Sample	(2) Combat Zone	(3) Non-Combat Zone	(4) Combat Exposure	(5) Combat Service without Exposure
Combat Measures					
Combat Zone Deployment	0.755	--	--	--	--
	(0.430)	--	--	--	--
Combat Exposure	0.367	0.486	--	--	--
	(0.483)	(0.501)	--	--	--
Combat Service without Exposure	0.388	0.514	--	--	--
	(0.488)	(0.501)	--	--	--
Dependent Variables					
Property Crime	0.098	0.113	0.051	0.114	0.112
	(0.298)	(0.317)	(0.221)	(0.319)	(0.317)
Violent Crime	0.242	0.257	0.195	0.297	0.219
	(0.429)	(0.438)	(0.398)	(0.458)	(0.415)
White Collar Crime	0.040	0.041	0.034	0.034	0.048
	(0.195)	(0.200)	(0.182)	(0.183)	(0.215)
Drug Crime	0.035	0.036	0.034	0.046	0.027
	(0.185)	(0.186)	(0.182)	(0.210)	(0.162)
Arrested	0.038	0.030	0.059	0.029	0.032
	(0.190)	(0.172)	(0.237)	(0.167)	(0.177)
Observations	482	364	118	177	187

Standard deviations in parentheses.

Table 2: Combat and Crime Measures, HRBADP Survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Combat Exposure	Non-Combat Exposure	Army	Marines	Navy	Air Force
Combat Measure							
Combat Exposure	0.515	--	--	0.805	0.706	0.220	0.443
	(0.500)			(0.396)	(0.456)	(0.414)	(0.497)
High Intensity Combat	0.317	0.609	0.008	0.631	0.464	0.114	0.161
	(0.466)	(0.488)	(0.087)	(0.483)	(0.499)	(0.318)	(0.368)
Number of Post-9/11	1.884	2.199	1.550	1.751	1.692	1.945	2.084
Deployments	(1.674)	(1.562)	(1.725)	(1.452)	(1.302)	(1.748)	(1.979)
Average Deployment	2.808	3.561	2.007	3.623	3.111	2.475	2.250
Length	(3.728)	(4.063)	(3.144)	(4.587)	(3.766)	(3.407)	(3.037)
Dependent Variables							
UCMJ Punishment	0.069	0.079	0.058	0.082	0.074	0.054	0.070
	(0.253)	(0.270)	(0.234)	(0.275)	(0.262)	(0.227)	(0.255)
Trouble Police	0.067	0.079	0.055	0.086	0.081	0.058	0.050
	(0.251)	(0.270)	(0.228)	(0.281)	(0.273)	(0.235)	(0.218)
Arrested	0.005	0.006	0.003	0.006	0.004	0.005	0.004
	(0.068)	(0.079)	(0.053)	(0.074)	(0.063)	(0.069)	(0.065)
Fight	0.067	0.085	0.048	0.096	0.089	0.060	0.032
	(0.250)	(0.279)	(0.213)	(0.295)	(0.285)	(0.238)	(0.176)
Gang Member	0.013	0.019	0.005	0.020	0.015	0.011	0.005
	(0.111)	(0.138)	(0.072)	(0.141)	(0.123)	(0.106)	(0.070)
Observations	11542	5948	5594	2563	2507	3374	3098

Standard deviations in parentheses

Table 3A: Evidence on the Exogeneity of Deployment Assignment (Add Health)

	Combat Deployment vs Non-Combat Deployment (1)	*Combat Exposure vs No Combat Exposure* (2)	*Combat Exposure vs Non-Combat Deployment* (3)
Pre-Deployment Property Crime	-0.056	0.024	-0.033
	(0.049)	(0.052)	(0.072)
Pre-Deployment Violent Crime	0.007	0.065	0.050
	(0.042)	(0.049)	(0.058)
Pre-Deployment Drug Crime	-0.073	-0.059	-0.088
	(0.059)	(0.077)	(0.088)
Pre-deployment Arrest	0.023	0.122	0.078
	(0.101)	(0.127)	(0.109)
Joint F-test (p-value) for Pre-Deployment Crime	*0.78 (0.54)*	*0.64 (0.63)*	*0.63 (0.64)*
Wave 1 Height	-0.002	0.002	0.001
	(0.005)	(0.006)	(0.008)
Wave 1 Weight	-0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)
Wave 1 Protestant	-0.013	0.011	-0.008
	(0.066)	(0.090)	(0.099)
Wave 1 Catholic	0.056	0.001	0.008
	(0.075)	(0.095)	(0.111)
Wave 1 Other Religion	0.030	0.005	0.081
	(0.133)	(0.159)	(0.237)
F-test (p-value) for Religion Indicators	*0.65 (0.58)*	*0.02 (0.99)*	*0.08 (0.97)*
Age in Years	0.232	0.181	0.429
	(0.411)	(0.409)	(0.598)
Age in Years Squared	-0.004	-0.003	-0.008
	(0.007)	(0.007)	(0.011)
F-test (p-value) for Age	*0.29(0.75)*	*0.30 (0.74)*	*0.56 (0.57)*
Race: Black	0.005	-0.060	-0.047
	(0.052)	(0.058)	(0.073)
Race: Other	0.140**	-0.035	0.111
	(0.062)	(0.070)	(0.118)
F-test (p-value) for Race	*2.63 (0.07)*	*0.54(0.58)*	*0.78(0.46)*

	Combat Deployment vs Non-Combat Deployment (1)	*Combat Exposure vs No Combat Exposure* (2)	*Combat Exposure vs Non-Combat Deployment* (3)
Ethnicity: Hispanic	-0.014	-0.161***	-0.084
	(0.048)	(0.053)	(0.081)
Some College	0.027	-0.002	0.024
	(0.054)	(0.056)	(0.075)
College	0.109	-0.001	0.074
	(0.082)	(0.086)	(0.121)
F-test (p-value) for Education	*1.02 (0.37)*	*0.01 (0.99)*	*0.19 (0.83)*
Wave 1 PPVTS	-0.001	0.000	-0.001
	(0.001)	(0.002)	(0.002)
$19K=<Parental Income <$28K	-0.011	0.005	-0.036
	(0.086)	(0.095)	(0.134)
$28K=<Parental Income <$36K	0.054	0.066	0.045
	(0.075)	(0.097)	(0.119)
$36K=<Parental Income <$45K	0.057	0.015	0.045
	(0.083)	(0.081)	(0.123)
$45K=<Parental Income <$56K	0.090	0.056	0.104
	(0.073)	(0.080)	(0.102)
$56K=<Parental Income <$83K	0.165**	0.098	0.190
	(0.083)	(0.105)	(0.124)
$83K=<Parental Income	0.110	0.146	0.234
	(0.099)	(0.133)	(0.156)
F-test (p-value) for Parental Income	*1.11 (0.36)*	*0.43 (0.86)*	*1.67 (0.33)*
Parents: Married	-0.093	0.045	-0.107
	(0.076)	(0.136)	(0.149)
Parents: Divorced, Separated or Widowed	-0.129	0.056	-0.140
	(0.092)	(0.152)	(0.170)
F-test (p-value) for Parental Marital Status	*0.99 (0.37)*	*0.07 (0.93)*	*0.34 (0.73)*
Mothers Education: High School	0.029	0.014	0.020
	(0.097)	(0.074)	(0.116)
Mothers Education: Above High School	0.023	0.060	0.075
	(0.091)	(0.074)	(0.122)
F-test (p-value) for Parental Education	*0.05 (0.96)*	*0.55 (0.57)*	*0.46 (0.63)*
One sibling	0.048	0.085	0.077
	(0.095)	(0.137)	(0.141)
Two siblings	0.029	0.094	0.037

	Combat Deployment vs Non-Combat Deployment (1)	*Combat Exposure vs No Combat Exposure* (2)	*Combat Exposure vs Non-Combat Deployment* (3)
	(0.099)	(0.144)	(0.161)
Three siblings	0.047	0.094	0.044
	(0.107)	(0.142)	(0.164)
Four siblings	0.020	0.089	0.004
	(0.103)	(0.143)	(0.155)
Five or more siblings	0.010	0.076	-0.013
	(0.108)	(0.133)	(0.173)
F-test (p-value) for Number of Siblings	*0.189 (0.98)*	*0.109(0.99)*	*0.246 (0.87)*
Jailed Father	-0.063	-0.026	-0.080
	(0.048)	(0.051)	(0.055)
Joint F-test (p-value) for all covariates	*1.01 (0.46)*	*0.77 (0.94)*	*1.17(0.28)*
Observations	482	482	295

Notes: Standard errors clustered on the school are in parentheses, *** p<0.01, ** p<0.05, * p<0.1 Each model includes controls for military-specific variables, including binary indicators for current active-duty military service status, total service length, military rank, branch of service, timing of service, occupation, and the timing of the interview. Coefficient(s) and standard error(s) are separately estimated for each variable (group). Each specification also includes dummies for missing information. The sample is comprised of male servicemembers only.

Table 3B: Evidence on the Exogeneity of Deployment Assignment (HRBADP Survey)

	Combat Exposure vs No Combat Exposure
Some College	0.024
	(0.018)
College Education	0.032
	(0.038)
Joint F-test (p-value) for Education	*0.94 (0.40)*
Age	0.002
	(0.008)
Age Squared	-0.000
	(0.000)
Joint F-test (p-value) for Age	*0.04 (0.96)*
Black	-0.008
	(0.030)
Race Other	0.001
	(0.033)
Joint F-test (p-value) for Race	*0.04(0.95)*
Hispanic	0.013
	(0.033)
Married	-0.008
	(0.023)
Divorced	-0.030
	(0.055)
Joint F-test (p-value) for Education	*0.15 (0.86)*
Joint F-test (p-value) for all covariates	*0.71(0.69)*
Observations	3,100

Robust standard errors corrected for clustering on the stratum are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. The sample consists of a sample in which each active duty deployed serviceman is matched on military characteristics available in the HRB survey: military rank, branch of service, and branch specific major command. The final sample is that which has common support on these characteristics.

Table 4: The Effect of Combat Deployments on Crime, by Whether Combat Exposure Occurred (Add Health)

	(1)	(2)	(3)	(4)	(5)
	Property Crime	Violent Crime	White Collar Crime	Drug Crime	Arrested
Panel I: Military Controls					
Combat Deployment	0.092***	0.064	0.037**	0.023	-0.001
	(0.034)	(0.053)	(0.017)	(0.021)	(0.018)
Panel II: Military Controls					
Combat Exposure	0.093**	0.118*	0.031*	0.037	0.001
	(0.043)	(0.067)	(0.016)	(0.025)	(0.019)
Combat Deployment without Exposure	0.091**	0.018	0.042*	0.010	-0.003
	(0.035)	(0.057)	(0.022)	(0.022)	(0.023)
Panel III: Panel II + Background Controls					
Combat Exposure	0.076*	0.132*	0.031	0.048*	0.009
	(0.043)	(0.068)	(0.020)	(0.025)	(0.021)
Combat Deployment without Exposure	0.096**	0.006	0.031	0.023	-0.003
	(0.038)	(0.056)	(0.023)	(0.028)	(0.027)
Panel IV: Panel III + Pre-Enlistment Crime					
Combat Exposure	0.076*	0.138**	0.029	0.049*	0.008
	(0.043)	(0.069)	(0.020)	(0.025)	(0.022)
Combat Deployment without Exposure	0.114***	0.020	0.037	0.027	0.002
	(0.037)	(0.060)	(0.025)	(0.029)	(0.027)
Observations	480	480	480	480	480

Robust standard errors corrected for clustering on the school are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. Military Controls include binary indicators for current active-duty military service status, total service length, military rank, branch of service, service exclusively after September 11, occupation, and the timing of the interview. Individual and family controls include height, weight, religion indicators, age, age squared, race/ethnicity indicators, Wave 1 Picture Vocabulary Test Score, parental income dummies, parental marital status indicators, maternal education, and number of siblings indicators. Models also include missing dummy categories for each of the control variables with missing information. The sample is comprised of men only.

Table 5: Heterogeneity in Crime Effects of Combat Deployments, by Current and Prior Active Duty Service (Add Health)

	(1)	(2)	(3)	(4)	(5)
	Property Crime	Violent Crime	White Collar Crime	Drug Crime	Arrested
Panel I: Current Active Duty Sample					
Combat Exposure	-0.113	0.205	0.017	0.011	-0.006
	(0.087)	(0.136)	(0.031)	(0.022)	(0.015)
Combat Deployment without Exposure	-0.063	0.059	0.030	0.024	-0.013
	(0.078)	(0.141)	(0.025)	(0.023)	(0.014)
Observations	194	194	194	194	196
Panel II: Past Active Duty Sample					
Combat Exposure	0.112*	0.100	0.002	0.067	0.021
	(0.060)	(0.086)	(0.031)	(0.041)	(0.036)
Combat Deployment without Exposure	0.190***	0.031	0.022	0.004	-0.008
	(0.052)	(0.089)	(0.042)	(0.047)	(0.053)
Observations	286	286	286	286	284

Robust standard errors corrected for clustering on the school are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. Military Controls include binary indicators for current active-duty military service status, total service length, military rank, branch of service, service exclusively after September 11, occupation, and the timing of the interview. Individual and family controls include height, weight, religion indicators, age, age squared, race/ethnicity indicators, Wave 1 Picture Vocabulary Test Score, parental income dummies, parental marital status indicators, maternal education, and number of siblings indicators. Models also include missing dummy categories for each of the control variables with missing information. The sample is comprised of men only.

Table 6: Estimated Effect of Combat Exposure on Crime (HRBADP Survey)

	(1)	(2)	(3)	(4)	(5)
	UCMJ punishment	Trouble Police	Arrested	Fight	Gang Member
Panel I: Military Controls Only					
Pooled Sample	0.025***	0.022***	0.005***	0.035***	0.017***
	(0.006)	(0.006)	(0.002)	(0.007)	(0.004)
Observations	11,542	11,511	11,542	11,512	11,386
Panel II: Military + Background Controls					
Pooled Sample	0.026***	0.023***	0.005***	0.037***	0.017***
	(0.005)	(0.006)	(0.002)	(0.006)	(0.004)
Observations	11,542	11,511	11,542	11,512	11,386
Panel III: Nearest Neighbor Matching Estimates					
Pooled Sample	0.020***	0.024***	0.0034***	0.036***	0.014***
	(0.005)	(0.005)	(0.0013)	(0.005)	(0.002)
Observations	[3,100]	[3,100]	[3,100]	[3,100]	[3,100]
Panel IV: Army					
Army Sample	0.043**	0.045***	0.003	0.051***	0.027**
	(0.013)	(0.010)	(0.003)	(0.010)	(0.007)
Observations	2,563	2,550	2,563	2,556	2,525
Panel V: Marines					
Marines	0.036**	0.022	0.002	0.035*	0.019
	(0.012)	(0.014)	(0.005)	(0.014)	(0.010)
Observations	2,507	2,501	2,507	2,498	2,465
Panel VI: Navy					
Navy	0.023**	0.033***	0.006	0.066***	0.022
	(0.007)	(0.007)	(0.003)	(0.013)	(0.013)
Observations	3,374	3,371	3,374	3,367	3,328
Panel VII: Air Force					
Air Force	0.015	0.004	0.008***	0.015***	0.008**
	(0.011)	(0.010)	(0.002)	(0.003)	(0.003)
Observations	3,098	3,089	3,098	3,091	3,068

In Panels I, II, IV, V, VI, and VII Robust standard errors corrected for clustering on the stratum are in parentheses. In Panel III, bootstrapped standard errors with 500 replications are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. Each model controls for military rank, branch of service, branch specific major command indicators, education dummies, age, age squared, and race/ethnicity dummies. The sample is comprised of men only.

Table 7: Heterogeneity in Crime Effects of Combat Exposure, by Servicemen Characteristics (HRBADP)

	(1)	(2)	(3)	(4)	(5)
	UCMJ punishment	Trouble Police	Arrested	Fight	Gang Member
Panel I: Age					
Ages 18-to-24	0.034*	0.038*	0.009*	0.058***	0.031**
	(0.017)	(0.021)	(0.005)	(0.018)	(0.012)
	2,545	2,535	2,545	2,535	2,489
Ages 25-to-32	0.036***	0.019*	0.008**	0.041***	0.020***
	(0.007)	(0.010)	(0.003)	(0.011)	(0.005)
	3,872	3,859	3,872	3,860	3,826
Ages 33 to 50	0.007	0.007	0.001	0.014*	0.003
	(0.005)	(0.006)	(0.001)	(0.007)	(0.002)
	5,057	5,050	5,057	5,050	5,010
Panel II: Gender					
Women	0.026**	0.021***	0.000	0.027**	0.008*
	(0.010)	(0.007)	(0.004)	(0.010)	(0.004)
	3,169	3,161	3,169	3,159	3,151
Panel III: E vs O Rank					
Enlisted	0.027***	0.025***	0.006***	0.039***	0.018***
	(0.007)	(0.008)	(0.002)	(0.009)	(0.005)
	8,792	8,764	8,792	8,764	8,667
Officer	0.008	-0.010	0.002	0.005	0.001
	(0.006)	(0.007)	(0.002)	(0.005)	(0.002)
	2,682	2,680	2,682	2,681	2,658
Panel IV: Race/Ethnicity					
Non-Hispanic White	0.025***	0.018**	0.004*	0.023***	0.008*
	(0.006)	(0.007)	(0.002)	(0.006)	(0.004)
	7,361	7,346	7,361	7,344	7,275
Non-White	0.018	0.012*	0.006***	0.024***	0.010**
	(0.012)	(0.006)	(0.002)	(0.006)	(0.004)
	3,075	8,378	8,399	8,378	8,300

Robust standard errors corrected for clustering on the stratum are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. Each model controls for military rank, branch of service, branch specific major command indicators, education dummies, age, age squared, race/ethnicity dummies, and marital status indicators. The sample is comprised of men only.

Table 8: Disentangling Combat Exposure Effects from Number and Length of Combat Deployments (HRBADP)

	(1)	(2)	(3)	(4)
Panel I: UCMJ punishment				
Combat Exposure	0.026***			0.022***
	(0.006)			(0.006)
Number of Post-9/11 Deployments		0.003*		0.002
		(0.002)		(0.002)
Months Deployed Last Year			0.002**	0.001*
			(0.001)	(0.001)
Observations	11,497	11,430	11,400	11,369
Panel II: Trouble Police				
Combat Exposure	0.023***			0.019***
	(0.006)			(0.006)
Number of Post-9/11 Deployments		0.005***		0.003*
		(0.002)		(0.002)
Months Deployed Last Year			0.002*	0.001
			(0.001)	(0.001)
Observations	11,466	11,400	11,371	11,340
Panel III: Arrest				
Combat Exposure	0.005***			0.005***
	(0.002)			(0.001)
Number of Post-9/11 Deployments		0.000		-0.000
		(0.000)		(0.000)
Months Deployed Last Year			0.000*	0.000
			(0.000)	(0.000)
Observations	11,497	11,430	11,400	11,369
Panel IV: Fight				
Combat Exposure	0.036***			0.030***
	(0.006)			(0.007)
Number of Post-9/11 Deployments		0.006***		0.003*
		(0.002)		(0.002)
Months Deployed Last Year			0.002***	0.001*
			(0.001)	(0.001)
Observations	11,467	11,401	11,372	11,341
Panel V: Gang Member				
Combat Exposure	0.016***			0.012***
	(0.004)			(0.003)
Number of Post-9/11 Deployments		0.004***		0.002**
		(0.001)		(0.001)
Months Deployed Last Year			0.001**	0.001
			(0.000)	(0.000)
Observations	11,341	11,281	11,252	11,222

Notes: Standard errors clustered on the stratum are in parentheses. Statistically significant at *10%, **5%, ***1%. Regressions control for military rank, branch of service, branch-specific major command indicators, education indictors, age, age squared, race/ethnicity dummies, and marital status indicators. The sample includes men only.

Table 9: Estimates of Effect of Combat Exposure, by Intensity of Exposure (HRBADP)

	(1)	(2)	(3)	(4)	(5)
	UCMJ punishment	Trouble Police	Arrest	Fight	Gang Member
Panel I: Intensity of Combat Exposure					
Moderate Intensity Combat Exposure	0.009	0.006	0.001	0.019***	0.005**
	(0.006)	(0.006)	(0.001)	(0.005)	(0.002)
High Intensity Combat Exposure	0.038***	0.035***	0.008***	0.068***	0.029***
	(0.007)	(0.007)	(0.002)	(0.007)	(0.006)
Observations	11,542	11,511	11,542	11,512	11,386
Panel II: Battlefield Trauma					
Injured in Combat	0.067***	0.069***	0.010*	0.091***	0.092***
	(0.019)	(0.024)	(0.005)	(0.022)	(0.018)
Witnessed Ally Hurt	0.021**	0.012*	0.003	0.022***	0.004
	(0.009)	(0.007)	(0.002)	(0.008)	(0.003)
Witnessed Enemy Death	0.019	0.023**	0.001	0.060***	0.020***
	(0.011)	(0.009)	(0.002)	(0.009)	(0.007)
Observations	11,255	11,227	11,255	11,228	11,119

Robust standard errors corrected for clustering on the stratum are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. Each model controls for military rank, branch of service, branch specific major command indicators, education dummies, age, age squared, and race/ethnicity dummies. The sample is comprised of men only.

Table 10: Descriptive Analysis of Possible Mechanisms (HRBADP)

	(1)	(2)	(3)	(4)	(5)
	UCMJ punishment	Trouble Police	Arrest	Fight	Gang Member
Panel I					
Combat Exposure	0.026***	0.023***	0.005***	0.037***	0.017***
	(0.005)	(0.006)	(0.002)	(0.006)	(0.004)
Panel II					
Combat Exposure	0.017***	0.017**	0.004**	0.023***	0.013***
	(0.006)	(0.007)	(0.002)	(0.007)	(0.004)
TBI	0.027***	0.018*	0.004**	0.039***	0.014***
	(0.006)	(0.009)	(0.002)	(0.007)	(0.004)
Panel III					
Combat Exposure	0.021***	0.017***	0.004**	0.030***	0.014***
	(0.005)	(0.006)	(0.002)	(0.006)	(0.004)
PTSD	0.063***	0.084***	0.007**	0.104***	0.050***
	(0.010)	(0.010)	(0.003)	(0.010)	(0.009)
Panel IV					
Combat Exposure	0.025***	0.022***	0.005***	0.035***	0.017***
	(0.005)	(0.006)	(0.002)	(0.006)	(0.004)
Binge Drinking	0.015***	0.037***	0.002	0.053***	0.010***
	(0.004)	(0.004)	(0.001)	(0.007)	(0.003)
Panel V					
Combat Exposure	0.023***	0.020***	0.004***	0.032***	0.015***
	(0.005)	(0.006)	(0.002)	(0.006)	(0.004)
Drug Use	0.074***	0.095***	0.010**	0.125***	0.057***
	(0.013)	(0.010)	(0.004)	(0.012)	(0.009)
Panel VI					
Combat Exposure	0.013**	0.012*	0.003	0.017**	0.010***
	(0.006)	(0.007)	(0.002)	(0.006)	(0.003)
TBI	0.021***	0.009	0.003*	0.027***	0.009**
	(0.006)	(0.009)	(0.002)	(0.006)	(0.004)
PTSD	0.051***	0.068***	0.005*	0.080***	0.041***
	(0.009)	(0.009)	(0.002)	(0.009)	(0.009)
Binge Drinking	0.009**	0.029***	0.001	0.043***	0.005**
	(0.004)	(0.003)	(0.001)	(0.006)	(0.002)
Drug Use	0.064***	0.080***	0.008**	0.105***	0.050***
	(0.013)	(0.009)	(0.004)	(0.011)	(0.008)
Observations	11,542	11,511	11,542	11,512	11,386

Robust standard errors corrected for clustering on the stratum are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. Each model controls for military rank, branch of service, branch specific major command indicators, education dummies, age, age squared, and race/ethnicity dummies. The sample is comprised of men only.

Appendix Table 1: Descriptive Statistics for Control Variables by Combat Assignment (Add Health)

	Full Sample	Combat Zone	Non-Combat Zone	Combat Exposure	Combat Service no Exposure
Pre-Deployment Property Crime	0.40	0.38	0.45	0.44	0.32
	(0.49)	(0.49)	(0.50)	(0.50)	(0.47)
Pre-Deployment Violent Crime	0.42	0.41	0.43	0.49	0.34
	(0.49)	(0.49)	(0.50)	(0.50)	(0.47)
Pre-Deployment Drug Crime	0.10	0.09	0.16	0.11	0.06
	(0.31)	(0.28)	(0.37)	(0.32)	(0.24)
Pre-Deployment Arrest	0.04	0.04	0.03	0.06	0.02
	(0.19)	(0.19)	(0.18)	(0.23)	(0.13)
Height	68.60	68.39	69.21	68.58	68.21
	(3.86)	(3.84)	(3.89)	(4.05)	(3.62)
Weight	146.36	144.63	151.73	145.80	143.52
	(29.97)	(29.97)	(29.48)	(29.62)	(30.34)
Wave 1 Protestant	0.62	0.60	0.68	0.63	0.57
	(0.49)	(0.49)	(0.47)	(0.49)	(0.50)
Wave 1 Catholic	0.24	0.26	0.17	0.23	0.29
	(0.43)	(0.44)	(0.38)	(0.42)	(0.45)
Wave 1 Other Religion	0.04	0.04	0.03	0.03	0.04
	(0.19)	(0.19)	(0.18)	(0.18)	(0.21)
Age in Years	28.68	28.57	29.03	28.49	28.64
	(1.70)	(1.73)	(1.57)	(1.76)	(1.71)
Race: Black	0.22	0.22	0.23	0.19	0.25
	(0.41)	(0.41)	(0.42)	(0.39)	(0.43)
Race: Other	0.08	0.09	0.04	0.06	0.12
	(0.27)	(0.29)	(0.20)	(0.24)	(0.32)
Ethnicity: Hispanic	0.16	0.17	0.15	0.12	0.22
	(0.37)	(0.37)	(0.36)	(0.32)	(0.41)
Some College	0.66	0.67	0.64	0.66	0.68
	(0.47)	(0.47)	(0.48)	(0.47)	(0.47)
College	0.16	0.17	0.15	0.16	0.18
	(0.37)	(0.37)	(0.36)	(0.37)	(0.38)
Wave 1 PPVTS	103.66	103.67	103.64	103.86	103.48
	(13.39)	(13.70)	(12.49)	(14.87)	(12.45)
$19K=<Parental Income <$28K	0.17	0.15	0.23	0.15	0.15
	(0.37)	(0.35)	(0.42)	(0.36)	(0.36)
$28K=<Parental Income <$36K	0.13	0.14	0.10	0.17	0.11
	(0.34)	(0.35)	(0.31)	(0.38)	(0.32)
$36K=<Parental Income <$45K	0.15	0.15	0.15	0.14	0.17
	(0.36)	(0.36)	(0.36)	(0.35)	(0.38)
$45K=<Parental Income <$56K	0.19	0.19	0.17	0.19	0.19
	(0.39)	(0.39)	(0.38)	(0.39)	(0.39)

	Full Sample	Combat Zone	Non-Combat Zone	Combat Exposure	Combat Service no Exposure
$56K=<Parental Income <$83K	0.11	0.12	0.08	0.11	0.13
	(0.32)	(0.33)	(0.28)	(0.31)	(0.34)
$83K=>Parental Income	0.06	0.07	0.05	0.07	0.06
	(0.24)	(0.25)	(0.21)	(0.26)	(0.24)
Parents: Married	0.75	0.74	0.77	0.73	0.76
	(0.43)	(0.44)	(0.42)	(0.45)	(0.43)
Parents: Divorced, Separated or Widowed	0.21	0.21	0.22	0.23	0.20
	(0.41)	(0.41)	(0.42)	(0.42)	(0.40)
One sibling	0.21	0.23	0.18	0.24	0.22
	(0.41)	(0.42)	(0.38)	(0.43)	(0.41)
Two siblings	0.24	0.23	0.25	0.24	0.23
	(0.43)	(0.42)	(0.44)	(0.43)	(0.42)
Three siblings	0.22	0.23	0.19	0.23	0.23
	(0.41)	(0.42)	(0.40)	(0.42)	(0.42)
Four siblings	0.10	0.09	0.14	0.09	0.10
	(0.31)	(0.29)	(0.34)	(0.29)	(0.30)
Five or more siblings	0.20	0.20	0.19	0.19	0.20
	(0.40)	(0.40)	(0.40)	(0.39)	(0.40)
Jailed Father	0.19	0.17	0.25	0.19	0.16
	(0.39)	(0.38)	(0.44)	(0.39)	(0.37)
Army	0.41	0.44	0.32	0.54	0.35
	(0.49)	(0.50)	(0.47)	(0.50)	(0.48)
Marines	0.20	0.18	0.26	0.27	0.10
	(0.40)	(0.39)	(0.44)	(0.44)	(0.30)
Navy	0.25	0.23	0.29	0.11	0.35
	(0.43)	(0.42)	(0.45)	(0.31)	(0.48)
Air Force	0.16	0.17	0.12	0.12	0.22
	(0.37)	(0.38)	(0.32)	(0.32)	(0.42)
Rank E4-E6	0.85	0.86	0.81	0.88	0.85
	(0.36)	(0.34)	(0.39)	(0.33)	(0.36)
Rank E7-E8	0.02	0.02	0.01	0.02	0.02
	(0.14)	(0.15)	(0.09)	(0.15)	(0.15)
Rank W1-W2	0.01	0.01	0.00	0.01	0.01
	(0.08)	(0.09)	0.00	(0.11)	(0.07)
Rank O1-O3	0.06	0.07	0.04	0.07	0.07
	(0.25)	(0.26)	(0.20)	(0.25)	(0.26)
Observations	482	364	118	177	187

Standard deviations in parentheses

Appendix Table 2: Descriptive Statistics for Control Variables by Combat Exposure and Branch of Service (HRBADP Survey)

	All	Combat Exposure	Non-Combat Exposure	Army	Marines	Navy	Air Force
Some College	0.47	0.47	0.48	0.48	0.35	0.48	0.56
	(0.50)	(0.50)	(0.50)	(0.50)	(0.48)	(0.50)	(0.50)
College Education	0.27	0.28	0.25	0.27	0.30	0.21	0.31
	(0.44)	(0.45)	(0.44)	(0.44)	(0.46)	(0.41)	(0.46)
Age	31.61	31.79	31.41	31.36	30.78	31.51	32.59
	(7.63)	(7.54)	(7.71)	(7.56)	(7.86)	(7.60)	(7.41)
Black	0.15	0.14	0.17	0.19	0.10	0.19	0.12
	(0.36)	(0.35)	(0.37)	(0.39)	(0.30)	(0.39)	(0.33)
Race Other	0.12	0.10	0.13	0.10	0.09	0.17	0.09
	(0.32)	(0.30)	(0.34)	(0.30)	(0.28)	(0.37)	(0.29)
Hispanic	0.13	0.13	0.12	0.14	0.15	0.13	0.09
	(0.33)	(0.34)	(0.33)	(0.35)	(0.35)	(0.33)	(0.29)
Army	0.22	0.35	0.09	--	--	--	--
	(0.42)	(0.48)	(0.29)	--	--	--	--
Marine	0.22	0.30	0.13	--	--	--	--
	(0.41)	(0.46)	(0.34)	--	--	--	--
Navy	0.29	0.12	0.47	--	--	--	--
	(0.45)	(0.33)	(0.50)	--	--	--	--
Air Force	0.27	0.23	0.31	--	--	--	--
	(0.44)	(0.42)	(0.46)	--	--	--	--
Rank E4-E6	0.53	0.50	0.55	0.55	0.39	0.59	0.54
	(0.50)	(0.50)	(0.50)	(0.50)	(0.49)	(0.49)	(0.50)
Rank E7-E9	0.17	0.17	0.17	0.14	0.15	0.18	0.19
	(0.37)	(0.37)	(0.37)	(0.35)	(0.36)	(0.38)	(0.39)
Rank W1-W5	0.04	0.06	0.02	0.07	0.09	0.02	0.00
	(0.19)	(0.23)	(0.15)	(0.26)	(0.28)	(0.12)	0.00
Rank O1-O3	0.10	0.10	0.10	0.10	0.12	0.07	0.10
	(0.30)	(0.30)	(0.29)	(0.30)	(0.33)	(0.26)	(0.30)
Rank 04-O10	0.10	0.11	0.08	0.08	0.14	0.06	0.11
	(0.29)	(0.32)	(0.27)	(0.26)	(0.35)	(0.25)	(0.31)
Observations	11542	5948	5594	2563	2507	3374	3098

Standard deviations in parentheses

Appendix Table 3: Sensitivity of Add Health Estimates to HRBADP Survey Controls

	(1) Property Crime	(2) Violent Crime	(3) White Collar Crime	(4) Drug Crime	(5) Arrested
Panel I: Effect of Combat Service					
HRBADP Controls Only					
Combat Zone Deployment	0.085***	0.077	0.036**	0.020	-0.003
	(0.029)	(0.048)	(0.017)	(0.022)	(0.018)
Full Controls					
Combat Zone Deployment	0.092***	0.064	0.037**	0.023	-0.001
	(0.034)	(0.053)	(0.017)	(0.021)	(0.018)
Panel II: Effect of Combat Engagement					
HRBADP Controls Only					
Combat Exposure	0.071**	0.127**	0.035**	0.033	0.001
	(0.034)	(0.062)	(0.018)	(0.025)	(0.022)
Combat Zone without Exposure	0.098***	0.033	0.037*	0.009	-0.007
	(0.035)	(0.054)	(0.022)	(0.024)	(0.022)
Full Controls					
Combat Exposure	0.076*	0.138**	0.029	0.049*	0.008
	(0.043)	(0.069)	(0.020)	(0.025)	(0.022)
Combat Zone without Exposure	0.114***	0.020	0.037	0.027	0.002
	(0.037)	(0.060)	(0.025)	(0.029)	(0.027)
Observations	480	480	480	480	480

Standard errors corrected for clustering on the school are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. All models control for age, age squared, race/ethnicity indicators, education indicators, military rank, and branch of service. Models also include missing dummy categories for each of the control variables. In every model estimated those who are deployed to a non-combat zone constitute the comparison group.

Appendix Table 4: Marginal Effects from Probit Estimates of the Effect of Combat Exposure on Crime (HRBADP Survey)

	(1)	(2)	(3)	(4)	(5)
	UCMJ punishment	Trouble Police	Arrested	Fight	Gang Member
Panel I: Military Controls Only					
Pooled Sample	0.021***	0.019***	0.001***	0.030***	0.003***
	(0.004)	(0.006)	(0.000)	(0.005)	(0.001)
Observations	11,542	11,511	11,542	11,512	11,386
Panel II: Military + Background Controls					
Pooled Sample	0.020***	0.019***	0.001***	0.029***	0.001***
	(0.004)	(0.005)	(0.000)	(0.005)	(0.000)
Observations	11,542	11,511	11,542	11,512	11,386
Panel III: Army					
Army Sample	0.031***	0.035***	0.000	0.040***	0.000*
	(0.009)	(0.009)	(0.000)	(0.008)	(0.000)
Observations	2,563	2,550	2,563	2,556	2,525
Panel IV: Marines					
Marines	0.022***	0.014	0.000	0.021**	0.003***
	(0.005)	(0.009)	(0.000)	(0.009)	(0.001)
Observations	2,507	2,501	2,507	2,498	2,465
Panel V: Navy					
Navy	0.012***	0.031***	0.000	0.040***	0.003
	(0.004)	(0.006)	(0.000)	(0.010)	(0.002)
Observations	3,374	3,371	3,374	3,367	3,328
Panel VI: Air Force					
Air Force	0.013	0.002	0.000	0.004***	0.000
	(0.009)	(0.009)	(0.000)	(0.001)	(0.000)
Observations	3,098	3,089	3,098	3,091	3,068

Standard errors corrected for clustering on the stratum are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. Each model controls for military rank, branch of service, branch specific major command indicators, education dummies, age, age squared, and race/ethnicity dummies. The sample is comprised of men only.

Appendix Table 5: The Impact of Combat Zone Deployment with Engagement (Enemy Firefight) on Selected Mechanisms (HRBADP)

	(1)	(2)	(3)	(4)
	TBI	PTSD	Binge Drinking	Drug Use
Combat Exposure	0.370***	0.068***	0.037***	0.032***
	(0.015)	(0.010)	(0.011)	(0.008)
Observations	11,253	11,427	11,073	11,514

Robust standard errors corrected for clustering on the stratum are in parentheses. Number of observations is in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels respectively. Each model controls for military rank, branch of service, branch specific major command indicators, education dummies, age, age squared, race/ethnicity dummies, and marital status indicators. The sample is comprised of men only.