2015

Long-Term Health Effects of Vietnam-Era Military Service: A Quasi-Experiment using Australian Conscription Lotteries

David W. Johnston
University of Wollongong

Michael A. Shields
University of Wollongong

Peter Siminski
University of Wollongong

Economics Working Paper Series 2015

http://business.uow.edu.au/aef/UOW188178.html

Long-Term Health Effects of Vietnam-Era Military Service: A Quasi-Experiment using Australian Conscription Lotteries

David W. Johnston, Michael A. Shields

Centre for Health Economics, Monash University, Melbourne, Australia

and

Peter Siminski

School of Accounting, Economics and Finance, University of Wollongong, Wollongong, Australia

WP 15-01

February 2015

Long-Term Health Effects of Vietnam-Era Military Service: A Quasi-Experiment using Australian Conscription Lotteries†

David W. Johnston*, Michael A. Shields* and Peter Siminski**

** Centre for Health Economics, Monash University, Melbourne, Australia*

*** School of Accounting, Economics and Finance, University of Wollongong, Wollongong, Australia; and IZA, Bonn, Germany*

10th February 2015

Abstract

This paper estimates the long-term health effects of Vietnam-Era military service using Australia's National conscription lotteries for identification. Our primary contribution is the quality and breadth of our health outcomes. We use several administrative sources, containing a near-universe of records on mortality (1994-2011), cancer diagnoses (1982-2008), and emergency hospital presentations (2005-2010). We also analyse a range of self-reported morbidity indicators (2006-2009). We find no significant long-term effects on mortality, cancer or emergency hospital visits. In contrast, we find significant detrimental effects on a number of morbidity measures. Hearing and mental health appear to be particularly affected. (*JEL* H56; I10; I13)

† Corresponding author: Peter Siminski: siminski@uow.edu.au We are grateful to Josh Angrist, Peter Haddad, Sonja Kassenboehmer, Malcolm Sim, Agne Suziedelyte, Alison Venn, as well as participants at the 5th Workshop on the Economics of Health and Wellbeing and at numerous departmental seminars for useful discussions and comments. We thank Daniel Thomason for excellent research assistance; the Department of Veterans' Affairs, the Australian Institute of Health and Welfare and the Sax Institute for access and assistance with de-identified data. Siminski acknowledges grant support from the Australian Research Council (DE120101642). The research was completed using data collected through the 45 and Up Study (www.saxinstitute.org.au). The 45 and Up Study is managed by the Sax Institute in collaboration with major partner Cancer Council NSW; and partners: the National Heart Foundation of Australia (NSW Division); NSW Ministry of Health; beyondblue; Ageing, Disability and Home Care, Department of Family and Community Services; and the Australian Red Cross Blood Service. We thank the many thousands of people participating in the 45 and Up Study. The views in this paper are the authors' alone, as are any errors of fact or omission. Conduct of the 45 and Up Study was approved by the University of New South Wales Human Research Ethics Committee. This project was approved by the University of Wollongong Human Research Ethics Committee, the Department of Veterans' Affairs Human Research Ethics Committee, and the Australian Institute of Health and Welfare Ethics Committee.

1. Introduction

Although it is nearly forty years since the Vietnam War ended, there remains considerable uncertainty about its health effects for veterans, despite an extensive body of multidisciplinary research (Autor et al., 2011).[1] One key reason for these mixed findings is the considerable empirical difficulty in identifying the long-term health consequences for veterans (see MacLean and Edwards, 2012). In addition to the lack of detailed data available to researchers on individual or even group-specific exposures, there is the issue of selection bias. Selection bias arises because individuals are not randomly assigned into military service (the "healthy soldier effect"), or to overseas deployment (the "healthy deployer" effect), making it difficult to select an appropriate control or comparison group for estimating unbiased causal effects. In recent years, a number of studies have attempted to overcome some of these difficulties by using a quasi-experimental IV framework (see Angrist, 1990; Angrist et al., 1996). This approach relies on the random assignment into military service that occurred through conscription (draft) lotteries. These studies have tended to suggest that many previous estimates of the health effects of Vietnam service were over-stated (Dobkin and Shabani, 2009; Angrist et al., 2010). In particular, Angrist et al. (1996), Angrist et al. (2010), Siminski and Ville (2011) and Conley and Heerwig (2012) found no significant evidence of heightened long-term mortality risk for Australian and US veterans. This contrasts with the higher mortality found for veterans in many earlier studies (see, for example, Watanabe and Kang, 1996; and Wilson et al., 2005a). So far, however, this quasi-experimental approach has not been widely applied to morbidity measures, with the exceptions of Dobkin and Shabani (2009) and Angrist et al. (2010). These two studies found little evidence of adverse long-term morbidity outcomes for US veterans.

The ongoing uncertainty regarding effects of the Vietnam War means that the long-term health and wellbeing of Vietnam veterans, who are now mostly in their 60s and 70s, remains an important issue for further research. Better estimates can help inform the appropriate nature and level of current health care and welfare services required by veterans, as well as what will be needed over the coming decades. The timeframe is even larger if we consider the health and wellbeing of the spouses and children of veterans as well (Levy and Sidel, 2009). More generally, learning about the potential long-term health consequences of previous military involvement can

[1] See, for example, the range of findings and discussions in: Hearst et al., 1986; Breslin et al., 1988; Centers for Disease Control Vietnam Experience Study, 1988a, 1988b; Boyle et al., 1989; Watanabe and Kang, 1996; DVA, 1999; Wilson and Horsley, 2003; Wilson et al., 2005a, 2005b, 2005c; Boehmer et al., 2004; Boscarino, 2006; MacLean and Elder, 2007; McLaughlin et al., 2008; O'Toole et al., 1996, 1999, 2009; and McBride et al., 2013. Some of the key health concerns for veterans that the literature has focused on are: poorer mental health, especially with respect to post-traumatic stress disorder (PTSD) and alcohol-related problems; greater risk of certain types of cancers resulting from chemical exposure in Vietnam (such as soft-tissue sarcoma); and a general heightened mortality risk, including from suicide and accidents.

provide some useful information for assessing the potential long-term health costs of more contemporary and future military operations (MacLean and Edwards, 2012). However, that said, lessons taken from one war for another should be applied to other periods with a degree of caution, given the context-specific differences in the nature of warfare and the differences in exposure to conflict, stress, trauma, infectious diseases, and chemicals. A consideration of institutions – including health care services, welfare provisions, and compensation entitlements – in place for returning veterans is also important for this understanding.

We overcome the empirical difficulties in identifying the long-term health consequences for veterans by using the assignment of young men into army service through the Vietnam-era national service conscription lotteries in Australia that took place between 1965 and 1972. The main contribution of this study is to apply the conscription lottery instrumental variable (IV) identification approach to a much wider range of health outcome measures, which capture both lifetime health (ever had a condition), and current health status and quality of life. In particular, we draw on morbidity and quality of life data from a large-scale population survey collected in 2006-2009 that does not explicitly reference military service, thus reducing concerns over justification bias (Bound, 1991; Angrist et al., 2010). We also use data on mortality (1994-2011), cancer episodes (1982-2008), and emergency hospital visits (2005-2010) from official administrative records. We believe these health outcomes represent the most comprehensive range of measures used in any one study within this quasi-experimental literature.

It is important to emphasise that the results we present apply only to Australian National Servicemen; they cannot necessarily be generalised to the health effects for regular soldiers who served in Vietnam. Also, the results are contingent on institutions - including the health care services, welfare entitlements and compensation schemes that have been available to Australian veterans over the last four decades. That is, it is impossible to estimate what would have been the health of national service veterans today if these services and provisions had not been made available. Our working assumption is that these large investments in the health and wellbeing of veterans, at least to some extent, will have mitigated some of the adverse health and wider consequences of war for veterans. This is consistent with the stated aims of the Commonwealth Department of Veterans' Affairs (DVA). For example, In 2013/14, the Australian Commonwealth Government allocated around $12.5 billion to the DVA, with the DVA providing a wide-range of programs and services split broadly into three components: Care, Compensation, and Commemoration. Finally, available data do not allow us to estimate short-run health effects of service.

The remainder of the paper is structured as follows: Section 2 provides further detail on the empirical challenges involved and describes the national service (conscription) lotteries that took place in Australia between 1965 and 1972. In Section 3 we outline the data sources we use, and in Section 4 we describe the empirical methodology we adopt. Our results are presented in Section 5, where several limitations of the analysis are also noted. Section 6 concludes.

2. Australian Vietnam-Era Conscription Lotteries

The introduction of National Service in 1964 by Prime Minister Menzies was central to Australia's effort to boost its regular army with National Servicemen (known as "Nashos") in order to meet projected manpower needs. Those enlisted were expected to complete two years of continuous full-time service in the army followed by three years of part-time service. Over this period, the civilian jobs of those enlisted were to be protected. From 1965 to 1972, around 804,000 young men were registered for the national service lotteries, in which 20-year olds were "balloted in" and therefore liable to be enlisted if their birth date corresponded to one of those drawn. Through 15 biannual lotteries, 63,735 men were conscripted to service in the army, with 19,450 serving in Vietnam after initial training in Australia (Davies and McKay, 2012). In most cases this was for a one-year tour of duty. The remainder who were not deployed to Vietnam served in the army in Australia. A comprehensive description of the conscription process, and a wider discussion of Australia's history of military conscriptions, can be found in Ville and Siminski (2011).

Despite the conscription lotteries, National Servicemen are not a random set of their birth cohorts. According to published estimates, only 28% of balloted-in men actually served in the military (Langford, 1997). Around 42% of all balloted-in men were rejected as not meeting the medical, psychological and educational standards required by the army; 15% were granted indefinite deferments at an earlier stage in the process because of marriage before date of call-up or by virtue of service as army reservists; 1.5% were exempted because of disability, conscientious objection, or by virtue of being theologians. At the end of the conscription period, another 7% of the total were unavailable for call-up because of deferments granted to students and apprentices; 4% were available for call-up but not required to do so; and 1.6% were suspected of breaching the National Service Act. The diverse nature of this selection, in particular the ineligibility of those who were medically unfit, emphasises the importance of controlling for the "healthy soldier" effect in this context. This means that any simple comparison between the health of the national servicemen and the general population likely will lead to incorrect inferences about the long-term health consequences of national service; that is, those who entered national service, on average, were healthier than the general population of 20-year-old men.

Furthermore, the selection criteria for deployment to Vietnam also was complicated and non-random (Fett et al., 1984; Ville and Siminski, 2011). As highlighted by Fett et al. (1984): “Units in the infantry, artillery, engineers and armour were most needed in Vietnam, so a serviceman’s allocation to a particular corps was a critical factor. Corps allocation took into account skills, further psychological aptitudes and personal preference, amongst many other corps-specific factors. It has been suggested that many who expressed unwillingness to serve in Vietnam were able to avoid doing so.” Another description is provided by Ham (2007): “The great myth about conscription was that national servicemen were forced to serve in Vietnam. In fact, most battalion commanders offered to re-post those who did not want to go”. However, he notes that in some cases intimidation, and peer pressure, was used to “convince” some to go to Vietnam. This complex selection of which national servicemen were deployed emphasises the potential for biases, such as the “healthy deployer” effect, in the estimates of the long-term health effects of deployment. Thus simply comparing the long-term health outcomes of deployed and non-deployed servicemen also may give misleading estimates. This is highlighted in a prominent mortality study of deployed Australian Vietnam veterans, which uses a non-deployed comparison group (Fett et al., 1984). Although this observational study could not control for unobserved differences, it shows that controlling for observed characteristics (especially corps) substantially changes the estimates.[2] Similar selection concerns also apply to U.S. veterans, whose deployment was related to education (Armed Forces Health Surveillance Center, 2007; Conley and Heerwig, 2012). As Conley and Heerwig (2012) conclude, “We suggest that previous research, which has shown that Vietnam-era veterans experienced significantly higher mortality than nonveterans, might be biased by nonrandom selection into the military and should be further investigated”.

The National Servicemen deployed in Vietnam served alongside regular soldiers in the infantry battalions, making up about one-third of Australia’s Vietnam military personnel. They were most heavily concentrated in frontline rifle companies, of which about 40 percent were conscripts (Ham, 2007). They were largely confined to the Phuoc Tuy Province, southeast of Saigon. Australian soldiers engaged in several fierce battles, including Long Tan in August 1966. To a large extent, though, they were involved in small unit actions using counter-insurgency tactics. 500 Australians were killed in Vietnam, including 198 National Servicemen. The number of deaths in each year of the war followed an n-shaped pattern, with few deaths between 1962-4, increasing to a high in 1968-9, and then declining to zero by 1972. The number wounded or

[2] More recent studies which use the same approach (e.g. Wilson et al. 2005c) do not control for such characteristics.

injured followed a similar time-path reaching a peak in 1968-9 (see NcNeil and Ekins, 2003). On the Australian Vietnam War Commemoration website a description of the service reads: "Combat in Vietnam meant more than exposure to mortar and small arms fire. Even where there was no contact with the enemy, men could be wounded or killed by concealed landmines and booby traps. This type of warfare carried a heavy psychological burden, danger was ever-present and many of those who suffered no physical injury were nonetheless traumatised by the experience". A detailed history and portrait of the experiences of Australian soldiers in Vietnam can be found in Ham (2007) and Davies and McKay (2012). [3]

3. Methods

We use a well-established quasi-experimental technique that exploits the randomness of the conscription lotteries. In this section we outline the approach and contrast it to more typical (observational) data analysis techniques. First we discuss the approach used to estimate the effects of army service (regardless of deployment) and then the approach used to estimate the effects of deployment.

We estimate the effect of army service on the health (y) of person i from birth cohort j using a regression model in which health is expressed as a linear function of army service (a binary indicator r), six-month-birth-cohort fixed effects (α), and other individual determinants of health (μ):

$$y_{ij} = \beta r_{ij} + \alpha_j + \mu_{ij} \quad (1)$$

where the causal parameter of interest is β. A common approach is to estimate the parameters in equation (1) using Ordinary Least Squares (OLS), or with equivalent nonlinear specifications, such as logit or poisson. Such analyses usually include covariates to control for differences between veterans and non-veterans. However, this approach is likely to yield biased estimates even with a rich set of control variables, because veterans and non-veterans almost certainly differ in unobserved ways that are related to health outcomes. Such estimates are expected to be biased downwards, suffering from so called "healthy soldier" bias because, as previously noted, recruits needed to pass a rigorous medical examination to enter the military.

Our approach draws on the randomness of conscription lotteries, similar to Angrist (1990), Dobkin and Shabani (2009), Siminski (2013) and others. In the Australian context, where

[3] It is also acknowledged that many returning veterans faced a poor reception in the immediate years following the end of the Vietnam War, and many veterans showing, for example, signs of PTSD, were misdiagnosed. Ozer et al. (2008) provides a history of PTSD diagnosis.

eligibility for conscription was randomly assigned in a binary process by DOB (Ville and Siminski, 2011), the lotteries are akin to a set of clustered randomised controlled trials, albeit with a large element of incomplete compliance. In other words, the conscription lotteries only determined service for a sub-set of men. Many "balloted-in" men did not serve, while many balloted-out men did serve in the military. As in earlier Australian studies (Siminski and Ville, 2011; 2012; 2013; Siminski, 2013), we use a two-sample 2SLS procedure, which accounts for the non-compliance, and does not require all of the necessary variables to be included in a single database (Inoue and Solon, 2010).

In the first-stage regression (using the first dataset), army service is instrumented by the conscription ballot outcome (z): a binary indicator equal to one for men whose DOB made them eligible for conscription (i.e. "balloted-in"):

$$r_{ij} = \pi_j z_{ij} + \gamma_j + \varepsilon_{ij} \qquad (2)$$

The first-stage-effect π of the ballot outcome is allowed to vary between birth cohorts, as indicated by the j subscript. Thus we adopt an over-identified specification with 15 instruments for army service, one for each birth cohort. Because this effect varies considerably between cohorts, this specification generates more precise 2SLS estimates than a just-identified specification.

The second-stage regression (estimated using a separate dataset) is:

$$y_{ij} = \beta \hat{r}_{ij} + \alpha_j + \mu_{ij} \qquad (3)$$

where $\hat{r}_{ij}$ is the predicted value from (2). This estimator is immune to selection bias, because variation in $\hat{r}_{ij}$ comes exclusively from the effect of the conscription lottery outcome on the probability of military service within each birth cohort, and α captures any fixed differences between cohorts. The procedure yields an estimate of β which should be interpreted as a Local Average Treatment Effect (LATE) – or in other words, the average effect for men whose army service was determined by the ballot outcome.[4] This effect may differ from the corresponding effect for regular military service-people.

[4] More precisely, our over-identified model, characterised by mutually exclusive binary instruments yields an estimate that is equal to a weighted average of birth-cohort-specific LATEs (Imbens and Angrist, 1994).

We estimate the effect of deployment – as opposed to army service – using a very similar approach. Consider a modification of equation (1), whereby r (army service) is replaced with v (indicating service in Vietnam):

$$y_{ij} = \delta v_{ij} + \alpha_j + \mu_{ij} \tag{4}$$

A typical observational analysis would use a similar specification, estimated by OLS with a sample of former servicemen, usually with additional control variables. Such an approach avoids the "healthy soldier" effect, because all persons in the sample are soldiers. But it does not avoid other selection bias issues. Decisions around deployment are complicated and certainly non-random, as discussed in Section 2, so deployment may be correlated with unobserved determinants of health, resulting in potentially biased estimates.

Our approach is to re-estimate the 2SLS models under the assumption that non-deployed service had no effect on long-run health outcomes. Different from the observational data approach, our 2SLS model is estimated using a sample of all men in the population in the relevant birth cohorts. For a subset of these men, deployment ultimately was determined by the outcome of the conscription lottery. In addition, we replace r with v in the first-stage regression (2), and we limit the sample to the first 12 birth cohorts (because no conscripts from the last three cohorts were deployed).

The identifying assumption – that the health of non-deployed conscripts was unaffected by service – may be more credible for some outcomes than for others and warrants close scrutiny. However, it is partially testable. We do this by re-estimating the original 2SLS model with the sample restricted to the youngest three (i.e. the non-deployed) birth cohorts. In other words, we estimate the effect of army service for those age cohorts whose conscripts were not deployed anywhere outside of Australia. The results from this analysis are reported in Section 5.

4. Data

One important contribution of this study is the quality and breadth of our health outcomes data. We use administrative records of mortality, cancers, and emergency hospital visits, as well as survey-based responses on morbidity and quality of life. In the following subsections, we detail the data sources and describe specific methodological issues that arise from particular data structures and coverage.

4.1. Mortality

The mortality data come from the National Mortality Database (NMD), administered by the Australian Institute of Health and Welfare. NMD includes records for all deaths registered in Australia since 1964, though DOB was only recorded from 1994 onwards. Male deaths registered between 1994 and 2011 are included in our analysis. To correspond with the first-stage data, deaths for men who migrated to Australia after the age of 20 are excluded. Some 12% of 1994-2011 deaths are also excluded because of missing DOBs, which are concentrated in 1994-6.[5] The analysis includes a total of 59,886 deaths, corresponding to 7.7% of the study population. The main results in the body of the text focus on deaths from all causes; however, further analysis by cause of death is also undertaken.[6]

Because of privacy regulations, exact DOB was not obtained. Instead, we were provided with frequency tabulations of deaths by birth cohort, ballot outcome, and source of death. We use these to construct a frequency weighted second-stage database.[7] The main limitation of this approach is the inability to cluster standard errors on DOB. Clustering on DOB is appropriate, because the ballot outcome was assigned by DOB. Our approach here is to assume no inter-cluster correlation. This assumption is violated if the error terms are correlated within DOBs. In other words, these standard errors will be biased downwards if health is more similar within DOBs than within 6-month birth cohorts, conditional on ballot outcome. We provide evidence that this is a reasonable assumption using the 45 and Up Study data (described below). The 45 and Up Study database does include DOB, but clustering on DOB makes virtually no difference to the standard error estimates. We also show results using a more conservative strategy: a group means approach with data collapsed to the cohort-ballot outcome level, as used by Siminski and Ville (2011).

4.2. Cancers

The cancer data come from the Australian Cancer Database. With some exclusions, this database covers the universe of primary, malignant cancers diagnosed in Australia since 1982, with DOB

[5] The day of birth was set to "15" in the administrative system whenever the true day of birth was unknown, so we are unable to distinguish between genuine deaths among men born on the 15th of any month from deaths where the day of birth is missing. To account for this, men born on the 15th of any month were excluded from the mortality analysis completely (from the first- and second-stage regressions).

[6] Our mortality analysis is an update of Siminski and Ville (2011), which shows corresponding results for deaths registered between 1994 and 2007. While our update includes only four more years of data, the death rate increases steeply in this age range, so the number of included deaths is 70% larger in this study.

[7] The mortality tabulations give the number of men in each combination of the explanatory variables in the second-stage regression for whom y= 1. We subtract these from the corresponding counts of all men from the first-stage database to derive counts of men for whom $y = 0$.

recorded throughout.[8] Our data include records for each cancer diagnosed between 1982 and 2008 for men in the relevant age cohorts. The database includes a random person identifier, year of diagnosis, ICD10 disease code and other clinical fields. These data were collapsed to the person-level with binary indicators for (one or more diagnosis of) each type of cancer considered.

Birth cohort and ballot outcome indicators were provided instead of exact DOB, due to privacy restrictions. Therefore, we follow a similar approach to that used for the mortality data – we construct a frequency-weighted second-stage database. Frequency weights for records with $y = 1$ are equal to the counts of men with each type of cancer in each combination of cohort and ballot outcome. Unlike with the mortality data, we could not exclude recent migrants from the cancer data. Given the long period of data coverage, it is not entirely clear which migrants to include in corresponding counts of men without a diagnosed cancer ($y = 0$). In the preferred analysis, we derive these by subtracting the counts of men with cancer from corresponding counts of all men from the 2006 Census, without excluding any migrants.[9] Men born on January 1 of any year are also excluded.[10]

The inability to exclude recent migrants from the second-stage database also means that the first-stage estimates will be biased upwards unless appropriate adjustments are made. For the analysis of cancer, the first-stage database is adjusted to include recent migrants (those arriving between the conscription lottery for their birth cohort and 2006) whose DOBs are assumed orthogonal to the ballot outcome (as in Siminski and Ville, 2013, among other precedent studies).[11]

4.3. Emergency Hospital Attendance

Our analysis of visits to hospital emergency departments (ED) draws on the Non-Admitted Patient Emergency Department Care Database (NAPEDCD). We received records for the universe of presentations at Australian EDs between July 2005 and June 2010. Triage category is included in the database. We exclude return visits, prearranged admissions, and patients in transit.

---

[8] Tumors diagnosed as benign, of borderline malignancy or in situ are not included. Only the first occurrence of a cancer is included; recurrences and metastases are not included. Basal cell carcinomas (BCC) and squamous cell carcinomas (SCC) of the skin are not included. Coverage of non-melanoma skin cancers, besides BCC and SCC is only complete from 2001. Coverage of myelodysplastic syndromes and some myeloproliferative cancers is complete from 2003 only.

[9] This approach ignores the fact that men who died prior to 2006 are included in the cancer counts. However, the mortality results we show suggest that death is orthogonal to ballot outcome, so this should not cause bias.

[10] Persons with DOB recorded as January 1 are over-represented in Census data; this is particularly apparent for recent migrants (who were not subject to the conscription ballots). This over-representation may or may not translate to cancer data. Thus a conservative decision was made to exclude January 1 DOBs from the cancer data (we asked the data custodians to exclude Jan 1 from data provided) as well as the Census counts.

[11] This inclusion of additional records in the first-stage database ensures that recent migrants (who were unaffected by the conscription lottery IVs) account for the same share of each birth-cohort in the first-stage database as observed in the 2006 Census.

In many ways the ED data were provided similarly to the cancer data. Indicators for birth cohort and ballot outcome were provided and exact DOB was not. Recent migrants are not excludable. So, we use the 2006 Census to count the number of relevant men in each combination of cohort and ballot outcome, as well as the migrant-adjusted version of the first-stage database. Presentations by men with a January 1 birthday were excluded, as with the cancer database.

The ED data do not include a person-identifier, so they cannot be collapsed to the person level. This likely affects the precision of the estimates if a subset of the population are regular ED users. With these restrictions, we resort to a group-means version of the second-stage regression, where the groups are combinations of birth-cohort (j) ballot-outcome (z). Thus there are at most 30 observations in the second-stage regression:

$$\bar{y}_{jz} = \beta\bar{r}_{jz} + \alpha_j + \mu_{zj}, \quad (5)$$

where $\bar{y}_{jz}$ is the ED presentation rate for men in cohort j and ballot outcome z (0 or 1), equaling the number of ED presentations (from NAPEDCD) divided by the number of men (from the 2006 Census), and $\bar{r}_{jz}$ is the predicted value from the first-stage regression. This regression is estimated by GLS, with weights equal to the number of men in each group.

4.4. Morbidity and Quality of Life

We use a number of outcome variables from the Sax Institute's 45 and Up Study (45 and Up Study Collaborators, 2008), which is believed to be the largest population-based cohort study conducted in the southern hemisphere. The full sample consists of over 267,000 people aged 45 years and above, residing in Australia's largest state, New South Wales (which includes Sydney). The estimation sample consists of 24,752 men with conscription-relevant DOBs, after excluding those who migrated to Australia after the age of 20.

The 45 and Up Study includes a broad-ranging health-related questionnaire, completed by participants between 2006 and 2009.[12] It covers physical and mental conditions, health-related actions, generalised health and quality of life. We were granted access to a confidential version of this file, which also includes respondents' DOBs. Importantly, unlike previous surveys of Australian veterans undertaken by the DVA or other welfare bodies, there are no questions in this survey about past or present military service, and no obvious reason for veterans to misreport

[12] The 45 and Up Study is a longitudinal cohort study, which includes an initial self-completed questionnaire linked to administrative health care utilisation data. We draw only on the cross-sectional questionnaire. 82% of respondents in the estimation sample completed the questionnaire between December 2007 and November 2008. A further 13% completed the questionnaire in February or March 2006.

their health. Consequently, the 45 and Up Study does not have a veteran indicator, so we follow the same two-sample 2SLS approach used with all of the other outcomes data.

5. Results

In this section we provide the 2SLS estimated effects of any army service (conscription) and army service in Vietnam (deployment) on a wide range of health indicators. Table 1 provides a detailed description, and also documents the approximate contemporary age range of the National Servicemen within the data windows (i.e. aged 20 in the period 1965-72). In each case, we first examine the three broad outcomes sourced from administrative data: (I) death from any cause; (II) cancer of any type; and (III) emergency hospital visits. Because these outcomes are generated from large administrative datasets, they provide the most precisely estimated effects. The next set of outcomes includes five summary health measures from the survey data: (1) (poor) general health; (2) (poor) quality of life; (3) physical dysfunction; (4) mental dysfunction; and (5) using prescription medication. The construction of these variables is described in Appendix A. Finally, we examine four outcomes that represent specific health conditions: (A) hearing loss; (B) diagnosed depression; (C) diagnosed anxiety; and (D) osteoarthritis. These health conditions have been singled out because they correspond to common conditions for which veterans are currently claiming disability benefits with the DVA (see Appendix Table A1 for details). We note that the data does not contain a direct indicator of whether an individual suffers from PTSD, but we focus particularly on diagnosed depression and anxiety as our most relevant indicators. PTSD is included in Fifth Edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-5) and proposes four diagnostic clusters for PTSD: these are re-experiencing, avoidance, negative cognitions and mood, and arousal. Re-experiencing, for example, includes flashbacks or other intense or prolonged psychological distress, and negative cognition and mood include persistent and distorted sense of blame of self or others.

We begin by showing results from the two first-stage regressions, which include 15 IVs representing the 15 cohort-specific ballot outcomes. Each IV is binary and equals one for men whose DOB made them eligible for conscription. These regressions are estimated using data from Vietnam-era military personnel records and contemporaneous population aggregates, as described by Siminski (2013). This database includes records for the universe of men born between 1945 and 1952 who lived in Australia at the age of 20 when their conscription lottery was conducted.

Figure 1 graphs the first-stage estimation results, with each data point representing the difference in the probability of army service or deployment between balloted-in and balloted-out men within each birth cohort, with 95% Confidence Intervals. Clearly, the ballot outcome induced a large proportion of men into the army. The estimated effects are statistically significant for each cohort individually (*t*-statistics range from 43 to 100) and jointly (*F*-statistic equals 4,238). The ballot outcome also induced a subset of men to serve in Vietnam: being balloted-in significantly increased the chance of being deployed for each of the first 12 cohorts. The *F*-test of joint statistical significance equals 989. As discussed in Section 3, we omit the last three cohorts from the deployment analysis given that no conscripts from these cohorts were deployed in Vietnam. This increases the first-stage *F*-statistic to 1,235.

5.1. Effects of Army Service

In Table 2, Column 2 presents the 2SLS estimated effects of army service during the Vietnam era (β in equation 3). Figure 2 displays the estimated effects relative to mean levels and shows the associated *p*-values. The point-estimates for outcomes based on administrative data (panel A) suggest that army service: increases the probability of death from any cause during 1994-2011 by 0.1 percentage points; reduces the probability of cancer of any type diagnosed in 1982-2008 by 0.2 percentage points; and reduces emergency hospital presentations in 2005-2010 by 0.028 attendances per person over a 5-year period (relative to mean rates of 7.6%, 11.4% and 0.550, respectively). None of these estimates are statistically significant. Importantly, the lack of statistical significance is due largely to small estimated effects, and not to large standard errors: the 95% confidence intervals for death from any cause, any cancer, and emergency visits equal (-0.0046, 0.0065), (-0.0106, 0.0061) and (-0.089, 0.034), respectively.

In addition, the small estimated effects for "deceased from any cause" and "cancer of any type" are not masking significant effects for particular causes of death or particular cancers. In Table 3 we present 2SLS estimated effects for disaggregated mortality and cancer outcomes. The results show that none of the eight cause-of-death estimates or the 16 cancer-type estimates is statistically significant at the 5% level. Estimates for soft-tissue sarcoma and rectal cancers are statistically significant at the 10% level, with positive and negative effects, respectively. However, we are cautious in interpreting estimates that are only statistically significant at the 10% level, given the multiple comparisons problem caused by the large number of hypothesis tests.

One interesting difference between our results and those from studies of U.S. war veterans from other conflicts (e.g. Bedard and Deschênes, 2006) is that Australian Vietnam-era army service seems to have had no effect on lung cancer: our estimate equals -0.001 with a *p*-value of 0.380. Correspondingly, using 45 and Up Study data from 2006-2009 (detailed in Section 4.2) we find that army service did not affect the probability of reporting ever having been a regular smoker (*p*-value = 0.226), currently smoking regularly (*p*-value = 0.146), or the number of cigarettes currently smoked (*p*-value = 0.877). Rather, the point estimates are negative for both the current-smoker and ever-smoker outcomes.

In Table 3 we also present estimated effects for disaggregated emergency hospital attendance outcomes. We separately show estimates for each triage level: 1) immediately life-threatening; 2) imminently life-threatening; 3) potentially life-threatening; 4) potentially serious; and 5) less urgent.[13] None of the five estimated effects are statistically significant.

Panel B of Table 2 presents the estimated effects for the five summary health measures (Column 2) based on responses from the 45 and Up Study. We find that army service did not significantly affect the probability of poor general health or the probability of poor quality of life. Although these estimates are less precise than those based on administrative data as shown in Panel A, relative to mean incidence levels the effects are not exceptionally large. For example, the effect on poor general health equals 3.2 percentage points, which is a 24% increase relative to a mean level of 13.2%. Similarly, we find statistically insignificant effects if we use alternative variants of these outcome variables that vary from 1 (excellent) to 5 (poor); the *p*-values for overall health and quality of life equal 0.182 and 0.449, respectively.

In contrast to the general health outcomes, our estimates for physical dysfunction and mental dysfunction are statistically significant at the 5% level. They suggest that army service increased physical dysfunction some 40 years later by 2.8 units (13% of a standard deviation) and mental dysfunction by 3.6 units (12% of a standard deviation). In other words, Vietnam-era army service increased the inability to accomplish common day-to-day activities due to both poor physical and mental health. The extent to which the significant effects may be driven partly by reporting bias is impossible to determine; however, given that these outcomes are constructed from questions regarding specific competencies, such as "climbing one flight of stairs" and "lifting or carrying groceries", we believe that any positive bias likely will be substantially less than the bias associated with more general non-specific health and disability questions. The final row in Panel B presents estimated effects on the probability of taking prescription medication in the past four weeks. The statistically insignificant estimate is 0.042 (that is, a 4.2 percentage point increase in medication use) and is small relative to the mean level of 0.487.[14]

Overall, the estimates in panels A and B suggest only modest long-term health effects of Vietnam-era army service. One explanation for this is that the treated sample includes a large

[13] Table 3 presents estimated effects for triage levels 1, 2 and 3 combined, which represents the occurrence of severe acute illness, rather than more mild illness routinely treated in the primary care GP sector (triage levels 4 and 5).

[14] One can also summarise the results in Panel B by estimating a single "standardised treatment effect", as will be described in detail for the mental health indicators in Section 5.3. The "standardised treatment effect" of army service on these summary measures of poor health is 0.100 (p=0.011). That is, army service increases the incidence of "poor health" by 0.1 standard deviations.

proportion of non-deployed conscripts, for whom service had no long-term health effects. An alternative explanation is that the estimates for the broad outcomes in panels A and B conceal large effects for specific health conditions. In panel C we investigate this possibility by examining outcomes associated with hearing loss, anxiety, depression, and osteoarthritis. We find that the estimates for hearing loss and doctor-diagnosed anxiety are statistically significant at the 1% and 5% levels, respectively. They suggest that army service increased the probability of hearing loss by 8.2 percentage points (15% relative to mean levels) and increased doctor diagnosed anxiety by 3.7 percentage points (41%). This strong evidence of hearing loss effects is consistent with Angrist et al. (2010). The estimated effects for depression and osteoarthritis are not statistically significant. The insignificant osteoarthritis effect may be partially due to the poor survey question, which only captures being recently treated for osteoarthritis.

5.2. Effects of Deployment to Vietnam

Any long-term health effects from army service were probably concentrated among conscripts deployed to Vietnam. Given that only 29% of conscripts were deployed, it is therefore important to present estimated effects for army service in Vietnam separately from the effects of any army service. Our approach, detailed in Section 3, is to re-estimate the 2SLS models under the assumption that non-deployed service had no effect on long-term health outcomes. This assumption is partially testable by estimating the effects of non-deployed service. We do this by re-estimating 2SLS models with the sample restricted to the last three birth cohorts, none of whom were deployed. Of the 12 estimated health effects, eight are positive, four are negative, and none are statistically significant at the 10% level.[15] The smallest *p*-value (0.138) is associated with the estimated effect for hearing loss, which equals 0.122 (24% relative to mean levels). This is perhaps unsurprising, given that hearing loss is a potential consequence of weapons training. Overall, the results generally support the assumption of no long-term health effects for non-deployed conscripts; however, some caution is required, especially when interpreting the effects on hearing loss.

The 2SLS estimated effects of deployment to Vietnam (δ) are presented in Column (3) of Table 2; Figure 2 presents the relative effects and *p*-values. Results for the administrative measures do not reflect the expectation that health effects from army service will be larger for deployed conscripts: each coefficient is less positive (more negative) than the coefficients in Column (2). However, this is not true for all causes of death and types of cancer: the estimate for

[15] The effects for the administrative based outcomes are precisely estimated and near-zero – specifically, 0.001, -0.000 and 0.030 for deceased, cancer and emergency hospital, respectively – whereas the effects for the survey based measures are much less precisely estimated.

soft-tissue sarcoma is larger for deployed conscripts (equals 0.002) and is statistically significant at the 5% level. This effect represents a 250% increase relative to mean population levels of 0.08%. This result is particularly interesting given the noted link between soft-tissue sarcoma and exposure to chemicals, including the herbicides used by the U.S. military in Vietnam between 1962 and 1971, generically known as "Agent Orange".[16] This finding is consistent with some previous studies; however, its statistical significance is not robust to correction for the multiple comparisons problem (e.g. Holm–Bonferroni correction method).

In contrast to the results in Panel A, the survey-based results in Panels B and C suggest that the estimated effects of deployment are larger than the effects of any Vietnam-era army service. Beginning with the summary health measures in Panel B, the results suggest that deployment increases poor quality of life (10 percentage points), physical dysfunction (49% of a standard deviation), and use of prescription medication (17.5 percentage points).[17,18] The largest effects for the specific health conditions in Panel C are hearing loss (22.4 percentage points) and diagnosed anxiety (10 percentage points). These effects are all relatively large compared to mean population levels; for the binary outcomes, the relative percentage effects equal 104%, 35%, 42% and 115% for poor quality of life, prescription medication, hearing loss and diagnosed anxiety, respectively. The effects of deployed service on poor general health and mental dysfunction are statistically significant at the 10% level. Overall, this set of results indicates that deployment has strong deleterious effects on self-reported health and quality of life some 40 years after Vietnam service.

In light of the small and insignificant objective health effects in Panel A, a concern raised in the literature (Angrist et al., 2010) is the extent to which the large subjective health effects in Panels B and C are driven by the high disability pension rates among deployed veterans. One possibility is that long-term receipt of a disability pension encourages a sickness or disability "identity", where "sickness could be used to seek potential advantages; to excuse failure and explain disappointment; to justify release from expected social roles and obligations; and to

[16] The US Institute for Medicine (2012) report provides a detailed review of the current state of knowledge relating to the mechanistic, animal and epidemiological evidence on the health effects of exposure to herbicides. The review concluded that there is "Limited or suggestive evidence of association" or "Inadequate or insufficient evident to determine association" for around 50 specific health conditions, while there is "Sufficient evidence" of an association for only soft-tissue sarcoma, non-Hodgkin's lymphoma, chronic lymphocytoc leukemia, Hodgkin's disease, and chloracne (p. 625-626).

[17] The prescription medication result is driven by a large effect on the use of prescription medication related to gastro-intestinal disorders (e.g. Nexium and Losec). For this sub-group of medications, the estimated effect of any army service is a 4.4 percentage point increase (significant at 5% level), and the estimated effect of army service in Vietnam is a 14.8 percentage point increase (significant at 1% level). The estimated effects for medications related to cardiovascular and mental disorders are not statistically significant.

[18] One also can summarise the results in Panel B by estimating a single "standardised treatment effect", as described in detail for the mental health indicators in Section 5.3. The "standardised treatment effect" of deployed army service on these summary measures is 0.359 (p=0.001). That is, deployment has a highly significant effect on these indicators. It increases the incidence of "poor health" by more than one-third of a standard deviation.

justify sympathy, assistance, and dependence" as suggested by Mechanic (1995, p.1208). That perspective is likely to foster actual feelings of poor health. Another possibility is that survey respondents partly justify disability pension receipt by knowingly exaggerating their ill-health to interviewers; this is the more common economic interpretation of justification bias (Bound, 1991). As discussed previously, it is impossible to determine the validity and extent of such mechanisms (at least with our data); nevertheless, our interpretation of the results in Table 2 is that army service in Vietnam significantly increased long-term morbidity and decreased quality of life outcomes. One motivation for this interpretation is the significant result for use of prescription medication. For this outcome, survey respondents were required to specify exact medication types and brand names (e.g. sertraline and Zoloft); thus it is likely to have low false positives rates.[19] Several other factors limit our concern about justification bias: the survey was not targeted to veterans and did not mention military service. Further, it was administered as a self-completion mail-out. Consequently, we believe the estimated effects to be relatively free from justification bias.

5.3. Mental Health Effects of Army Service and Deployment

Mental illness has been one of the most prominent ongoing health concerns for veterans, both in the immediate years after active service and later in life.[20] In particular, there is concern that war veterans may have a heightened risk of experiencing post-traumatic stress disorder (PTSD). Weiss et al. (1992) estimate that around 83,000 US Vietnam theatre veterans experienced PTSD approximately two decades after their exposure to traumatic stressors. Gates et al. (2012) report that the prevalence of PTSD for deployed military personnel is as high as 14-16%, compared to a population prevalence of about 7.8% (Kessler et al., 1995). Because of the prospect of serious mental health effects from Vietnam-era army service, we provide here some additional estimation results for mental health outcomes.

Table 2 shows the results for the mental health outcomes "mental dysfunction", "diagnosed anxiety" and "diagnosed depression" from the 45 and Up Study survey, which includes other mental health information that we also use. It includes ten questions from the Kessler Psychological Distress Scale (K10) measuring the frequency ("none of the time" to "all

[19] A potential concern with the prescribed medication outcome is that the estimated effect may be driven by differential access to the health care system between veterans and non-veterans. However, Australians have universal access to free primary health care regardless of veteran status (or economic situation). Many veterans do have access to enhanced pharmaceutical concessions. However, the price elasticity of demand for pharmaceuticals has been found to be small for people in this demographic group (Siminski, 2011), and so any resulting bias is likely to be small.

[20] In the Australian context, the Veteran Mental Health Strategy (2013) sets out a 10-year plan to support the mental health and wellbeing of veterans using the three principles of prevention, recovery and optimisation.

of the time") of ten mental health symptoms, such as "nervous", "restless or fidgety", "depressed" and "so sad that nothing could cheer you up". From this set of questions, we create two indices measuring anxiety and depression, respectively (details can be found in Appendix A). The 45 and Up survey also asks respondents how they would rate their memory, on a scale from excellent to poor. Impaired memory is a common symptom of mental illness, and of PTSD in particular. Finally, from the prescription medication question we create an indicator of whether an individual is currently taking anti-depressants.[21]

The seven outcomes represent all of the available information on mental health. The standard approach is to estimate separate effects for each, as we have done for the three mental health outcomes in Table 2. However, rather than interpreting effects on these outcomes separately, it is arguably more appropriate to estimate a single effect for the mental health "domain". We do this by estimating a "standardised treatment effect", following the approach in Finkelstein et al. (2012). This is an arithmetic average of treatment effects, estimated for each outcome, after standardising. More specifically, it is a linear combination of parameters from a stacked (second-stage) regression that includes one observation for each person-outcome combination. For example, to estimate the effect of army service on mental health, we run this regression:

$$y_{ijk} = \beta_k \hat{r}_{ij} + \alpha_{jk} + \mu_{ijk} \quad (6)$$

where y_{ijk} is the mental health of person *i* in cohort *j*, as measured by indicator $k \in (1, \ldots, 7)$. Each indicator k is standardised to have a mean of zero and a standard deviation of one. Interdependence within individuals is accounted for by clustering on date of birth. The standardised treatment effect $\sum_k \beta_k / 7$, represents the average effect of army service on mental health, expressed in standard deviation units.

Table 4 presents the results from this exercise. The first seven rows are the results for each non-standardised outcome. They suggest that any army service significantly increases mental dysfunction and diagnosed anxiety, while army service in Vietnam additionally increases anxiety symptoms (Kessler index), and the likelihood of poor memory. For each individual outcome, we report both per comparison *p*-values and family-wise *p*-values, adjusted to account for the multiple outcomes examined within the domain. Most interesting are the two standardised

[21] One mental health problem we are unable to adequately measure is alcohol dependence and abuse. Nevertheless, using data from the 45 and Up survey, we can show that army service and deployment did not significantly increase the number of alcoholic drinks consumed per week (*p*-values equal 0.086 and 0.089) nor the number of days per week alcohol is consumed (*p*-values equal 0.906 and 0.568). A range of other variables were considered (e.g. drinks every day and drinks more than 28 drinks per week) – the effects for which were also statistically insignificant.

treatment effect estimates. Any army service is estimated to worsen the mental health domain by 0.074 standard deviations. Army service in Vietnam is estimated to worsen the mental health domain by 0.256 standard deviations. The latter estimate is statistically significant at the 5% level (p=0.017). In contrast, Angrist et al. (2010) find no effect on mental health for US veterans. This discrepancy may be due to the richer mental health indicators in the 45 and Up Study as compared to the U.S. Census long form.

5.4. Caveats and Limitations

There are many challenges to estimating the long-term health effects of military service, so our analysis comes with several caveats and limitations. Perhaps most importantly, as with nearly all studies in the quasi-experimental literature, we do not observe individual-level information on combat, trauma, and chemical exposure, or even more aggregated battalion-level information. Thus we are unable to separately identify those National Servicemen who experienced the very worst aspects of war, and who therefore are most likely to have suffered the most severe long-term health effects. In other words, our average effect estimates are likely to mask considerable heterogeneity. Nevertheless, we do find some significant long-term health effects for the "average" veteran, and from this we conclude that many veterans will have been heavily affected by military service.

Second, while we are able to examine a wide-range of health outcomes, we do not have complete time coverage for some of them. In particular, we do not observe mortality before 1994. If veterans experienced higher mortality in the earlier years (e.g. through higher rates of suicide or accidents), then we are under-estimating the effects of Vietnam service. However, the Census data show that "balloted-in" men are not disproportionally "missing" from the population in 2006 (Siminski, 2013), which suggests this is not a major threat to validity. In addition, an early study focusing on deaths up to the end of 1981 found no significant difference between national service veterans and non-veterans, once differences in pre-service and service-related characteristics were accounted for (Fett et al., 1984).

Similarly, it is possible that our estimated cancer effects are biased downwards if veterans' cancers were disproportionally diagnosed prior to 1982, when the conscripted veterans were 32-37 years old – although, such cancers were observed if they re-occurred or metastasised within the data coverage period. We can partially test for this possibility by using information on reported cancer diagnoses in the 45 and Up Study. The estimated effect of army service on the ever-diagnoses of cancer equals -0.004 (p-value = 0.824). Similarly, the estimated effect of army service in Vietnam on the ever diagnoses of cancer equals -0.025 (p-value = 0.661). This evidence therefore suggests that our cancer estimates are not biased downward. Further, cancer diagnosis rates increase very steeply with age: 94% of all new cases for males in 1982-2008 were for those aged 40+. It is likely that relatively few cases were actually diagnosed prior to 1982 for these cohorts.

A third limitation is that for some health outcomes we are unable to obtain precise estimates of the effects of army service and deployment, leading to large confidence intervals (see Figure 2). This is a common problem within the literature. Clearly, this lack of precision also weakens our conclusions. However, it is important to note that the estimation sample sizes for the morbidity outcomes sourced from the 45 and Up Study are larger than those from many previous studies, and are comparable to those used by Bedard and Deschênes (2006) and Dobkin and Shabani (2009).

Fourth, as already noted a concern related to self-reported health measures that has been highlighted in the economics literature is the extent to which the large subjective morbidity effects are due to misreporting driven by the justification or self-rationalisation associated with high disability pension rates among deployed veterans (Angrist et al., 2010). While it is not possible to determine the extent of any misreporting in the 45 and Up Study, there are a number of reasons that point to our estimated effects being relatively free from justification bias. One reason for this interpretation is the significant result for prescription medication, which is likely to have low false positives rates. Other reasons are that the survey we used was not specifically targeted at veterans' issues, did not mention military service, and was administered as a confidential self-completion mail-out.

Finally, as we have noted previously, it is impossible to characterise what the health of veterans would be today if they had not had access to health care, disability pension entitlements, and compensation schemes that have been available through the DVA over the last three decades. These provisions aim to protect the health and wellbeing of veterans, so our results represent a net outcome within this institutional context. Consequently, any conclusions drawn from this analysis in terms of the potential long-term health costs of military service should be viewed within the environment that veterans faced after returning from service.

6. Conclusions

This paper contributes to the large multi-disciplinary literature on the health effects of Vietnam-Era military service in three important ways. First, it implements a quasi-experimental empirical approach based on Australia's National conscription lotteries held between 1965 and 1972. With this approach, it is possible to control for potentially large biases arising from non-random selection into army service and non-random selection into deployment to Vietnam. Second, it investigates a wide-range of health outcomes derived from both administration records (mortality, cancers, emergency hospital presentations) and from survey data (general health, physical and mental functioning, and specific health conditions). The quality and breadth of the health outcomes data represent an improvement on those used in previous quasi-experimental studies. Third, it provides estimated effects of any army service during the Vietnam Era (regardless of deployment) and of army service in Vietnam.

The positive conclusion from our analysis is that we find no significant effects of army service or army service in Vietnam on mortality (over the period 1994-2011), cancer diagnoses (1982-2008) and emergency hospital presentations (2005-2010). Relative to mean rates, our point estimates represent a mortality increase of 1%, a cancer risk decrease of 2%, and a decrease in emergency hospital presentations of 5%. The estimated effects of army service in Vietnam are also statistically insignificant and similarly small. However, we cannot definitively rule-out small detrimental health effects from Vietnam-Era army service, but the upper bands of the 95% confidence intervals (9%, 5% and 6% for mortality, cancer and emergency hospital presentations) are substantially lower than some previous point estimates, especially those from non-experimental studies. One potential exception, however, is a higher risk of soft-tissue sarcoma, although we are cautious of emphasising this result given the level of uncertainty around the estimate.

Importantly, many health issues cannot be well captured with our administrative data, and our subsequent analyses of survey data (2006-2009) suggests that army service and deployment has strong deleterious effects on self-reported health. We find that veterans are experiencing significantly lower levels of mental health and quality of life, as well as an increased risk of hearing loss. In terms of hearing loss, Army service is estimated to increase the risk by 8.2 percentage points (p-value = 0.005), or 15% relative to mean rates. This seems plausible given the potential for weapon and artillery fire to damage hearing, and is consistent with the findings in Angrist et al.'s (2010) study of US veterans. Also, hearing loss is the number one accepted condition for veteran disability benefits in Australia.

The estimated "standardised treatment effects" for mental health suggest that army service worsened long-term mental health by 7% of a standard deviation, and that army service in Vietnam worsened mental health by 26% of a standard deviation. In addition to being useful for the planning and provision of appropriate mental health care services for Vietnam veterans, this result adds support to studies that also identify substantial adverse mental health consequences for veterans of more recent military campaigns (see, for example, Cesur et al., 2013).

References

45 and Up Study Collaborators, "Cohort profile: the 45 and up study," *International Journal of Epidemiology*, 37 (2008), 941-947.

Angrist, JD. (1990). Lifetime earnings and the Vietnam era draft lottery: Evidence from social security administrative records. *American Economic Review*, 80, pp. 313–336.

Angrist, JD. and Chen, SH. (2011). Schooling and the Vietnam-era GI bill: Evidence from the draft lottery. *American Economic Journal: Applied Economics*, 3, pp. 96-118.

Angrist, JD., Chen, SH. and Frandsen, B. (2010). Did Vietnam veterans get sicker in the 1990s? The complicated effects of military service on self-reported health, *Journal of Public Economics*, 94, pp. 824–37.

Angrist, D., Imbens, GW. and Rubin, D. (1996). Identification of causal effects using instrumental variables. *Journal of the American Statistical Association,* 91, pp. 444–455.

Armed Forces Health Surveillance Center (2007). Healthy deployers: Nature and trends of health care utilization during the year prior to deployment to OEF/OIF, Active Components, U.S Armed Forces, January 2002-December 2006. *Medical Surveillance Monthly Report*, 14, pp. 2-5.

Autor, DH., Duggan, MG. and Lyle, DS. (2011). Battle scars? The puzzling decline in employment and rise in disability receipt among Vietnam era veterans. *American Economic Review: Papers and Proceedings,* 101, pp. 339-344.

Bedard, K. and Deschenes, O. (2006). The long-term impact of military service on health: Evidence from World War II and Korean War Veterans. *American Economic Review*, 96, pp. 176–194.

Boehmer, TW., Flanders, MM., Boyle, CA. and Barrett, D. (2004). Postservice mortality in Vietnam veterans: 30 year follow-up. *Archives of Internal Medicine*, 164, pp. 1908-1916.

Boscarino. JA. (2006). Posttraumatic stress disorder and mortality among US army veterans 30 years after military service. *Annals of Epidemiology*, 16, pp. 248-256.

Boyle, CA., Decouflè, P. and O'Brien, TR. (1989). Long-term health consequences of military service in Vietnam. *Epidemiologic Review*, 11, pp. 1-27.

Breslin, P., Kang, H., Lee, Y., Burt, V. and Shepard, B. (1988). Proportionate mortality study of US Army and US Marine Corps veterans of the Vietnam war. *Journal of Occupational and Environmental Medicine*, 30, pp. 412-419.

Bound, J. (1991). Self-reported versus objective measures of health in retirement models. *Journal of Human Resources*, 26, pp. 10638.

Centers for Disease Control Vietnam Experience Study (1988a). Health status of Vietnam veterans: I. Psychosocial characteristics. *Journal of the American Medical Association*, 259, pp. 2701–7.

Centers for Disease Control Vietnam Experience Study (1988b). Health status of Vietnam veterans: II. Physical health. *Journal of the American Medical Association*, 259, pp. 2708–14.

Cesur, R., Sabia, JJ. And Tekin, E. (2013). The psychological costs of war: Military combat and mental health. *Journal of Health Economics*, 32, pp. 51-65.

Conley, D. and Heerwig, J. (2012). The long-term effects of military conscription on mortality: Estimates from the Vietnam era draft lottery. *Demography*, 49, pp. 841-855.

Davies, B. and McKay, G. (2012). *Vietnam: The Complete Story of the Australian War*. Crows Nest, New South Wales; Allen and Unwin.

Department of Veterans' Affairs and AIHW (1999). *Morbidity of Vietnam veterans: A study of the health of Australia's Vietnam veteran community*. Canberra, ACT; AIHW, Cat. no. PHE 20.

Dobkin, C., and Shabani, R. (2009). The health effects of military service: Evidence from the Vietnam draft. *Economic Inquiry*, 47, pp. 69–80.

Fett, MJ., Dunn, M., Adena, HA., O'Toole, BI. and Forcier, L. (1984). *The Mortality Report*, 3 vols. Canberra: AGPS.

Finkelstein, A., Taubman, S., Wright, B., Bernstein, M., Gruber, J., Newhouse, J., Allen, H. and Baicker, K. (2012). The Oregon Health Insurance Experiment: Evidence from the first year. *Quarterly Journal of Economics*, 127, pp. 1057-1106.

Gates, M., Holowka, D., Vasterling, J., Keane, T., Marx, B. and Rosen, R. (2012). Posttraumatic stress disorder in veterans and military personnel: Epidemiology, screening and case recognition. *Psychological Services*, 9, pp. 361-382.

Ham, P. (2007). *Vietnam: The Australian War*. Sydney, HarperCollins.

Hearst, N., Newman, T. and Hulley, S. (1986). Delayed effects of the military draft on mortality: A randomized natural experiment. *New England Journal of Medicine*, 314, pp. 620-624.

Hyer, L., Fallon, J., Harrison, W. and Boudewyns, P. (1987). MMPI overreporting by Vietnam combat veterans. *Journal of Clinical Psychology*, 43, 79-83.

Imbens, GW. and Angrist, JD. (1994). Identification and estimation of local average treatment effects. *Econometrica*, 62, pp. 467-75.

Inoue, A. and Solon, G. (2010). Two-sample instrumental variables estimators. *Review of Economics and Statistics*, 92, pp. 557-561.

Kessler, RC., Sonnega, A., Bromet, E., Hughes, M. and Nelson, CB. (1995). Posttraumatic stress disorder in the National Comorbidity Survey. *Archives of General Psychiatry*, 52, pp. 1048-1060.

Langford, S. (1997). 'Appendix: The National Service Scheme 1964–1972. In Peter Edwards (ed.), *A Nation at War*, Allen and Unwin.

Levy, BS. And Sidel, VW. (2009). Health effects of combat: A life-course perspective. *Annual Review of Public Health*, 30, pp. 123-36.

MacLean, A. and Elder, GH Jr. (2007). Military Service in the life course. *Annual Review of Sociology*, 33, pp. 175-196.

MacLean, A. and Edwards, R. (2010). The pervasive role of rank in the health of U.S veterans. *Armed Forces and Society*, 36, pp. 765-785.

MacLean, A. and Edwards, R. (2012). Health, aging and the post service life cycles of US veterans. In Jomana Amara and Ann Hendricks (ed.), *Military Medical Care: from Pre-Deployment to Post-Separation*, Abingdon, Routledge.

McBride, D., Cox, B., Broughton, J. and Tong, D. (2013). The mortality and cancer experience of New Zealand Vietnam war veterans: A cohort study. *BMJ Open*, 3, pe003379.

McLaughlin, R., Neilsen, L. and Waller, M. (2008). An evaluation of the effect of military service on mortality: Quantifying the healthy soldier effect. *Annals of Epidemiology*, 18, pp. 928-936.

McNeil, I. and Ekins, A. (2003). *On the offensive: the Australian Army in the Vietnam War, January 1967 – June 1968*. Sydney New South Wales, Allen and Unwin.

Mechanic, D. (1995). Sociological dimensions of illness behaviour, *Social Science and Medicine*, 41, pp. 1207-1216.

O'Toole, B., Marshall, R. and Grayson, DA. et al. (1996). The Australian Vietnam Veterans Health Study: I. Study design and response bias, II Self-Reported Health of Veterans compared with the Australian Population, and III. Psychological Health of Australian Vietnam Veterans and its relationship to combat. *International Journal of Epidemiology*, 25, pp. 307-340.

O'Toole, B., Marshall, R., Schureck, R. and Dobson, M. (1999). Combat, dissociation and posttraumatic stress disorder in Australian Vietnam veterans. *Journal of Traumatic Stress*, 12, pp. 625-640.

Ozer, EJ., Best, SR., Lipsey, TL. and Weiss, DS. (2008). Predictors of posttraumatic stress disorder and symptoms in adults: A meta-analysis. *Psychological Trauma: Theory, Research, Practice, and Policy*, S, pp. 3-36.

Siminski, P. (2013). Employment effects of army service and veterans' compensation: Evidence from the Australian Vietnam-era conscription lotteries. *Review of Economics and Statistics*, 95, pp. 85-97.

Siminski, P. and Ville, S. (2011). Long-run mortality effects of Vietnam-era army service: Evidence from Australia's conscription lotteries. *American Economic Review: Papers and Proceedings*, 101, pp. 345-349.

Siminski, P. and Ville, S. (2012). I was only nineteen, 45 years ago: What can we learn from Australia's conscription lotteries. *Economic Record*, 88, pp. 351–371.

Siminski, P. and Ville, S. (2013). Does the military train men to be violent criminals? New evidence from Australia's conscription lotteries. IZA, Bonn, DP No. 7152.

US Institute for Medicine (2012). *Veterans and Agent Orange: Update 2010*. Washington DC, Institute of Medicine of National Academies.

Ville, S. and Siminski, P. (2011). A fair and equitable method of recruitment? Conscription by ballot into the Australian army during the Vietnam War. *Australian Economic History Review,* 51, pp. 277-296.

Watanabe, KK. And Kang, HK. (1996). Mortality patterns among Vietnam veterans: A 24-year retrospective analysis. *Journal of Occupational and Environmental* Medicine, 38, pp. 272-278.

Military service in Vietnam and the risk of death from trauma and selected cancers. *Annals of Epidemiology*, 5, pp. 407-412.

Weiss, DS., Marmar, CR., Schlenger, WE., Fairbank, JA., Jordan, BK., Hough, RL. And Kulka, RA. (1992). The prevalence of lifetime and partial post-traumatic stress disorder in Vietnam theatre veterans. *Journal of Traumatic Stress*, 5, pp. 365-376.

Wilson, EJ. and Horsley, KW. (2003). Health effects of Vietnam. *ADF Health*, 4, pp. 59-65.

Wilson, EJ., Horsley, KW. and van der Hoek, R. (2005a). *Australian Vietnam Veterans Mortality Study*. Canberra: DVA.

Wilson, EJ., Horsley, KW. And van der Hoek, R. (2005b). *Cancer Incidence in Australian Vietnam Veterans Study*. Canberra: DVA.

Wilson, EJ., Horsley, KW. and van der Hoek, R. (2005c). *Australian National Service Vietnam Veterans Mortality and Cancer Incidence Study.* Canberra: DVA.

Figure 1: First-Stage Estimated Effects of Ballot-Outcome on Army Service and Deployment

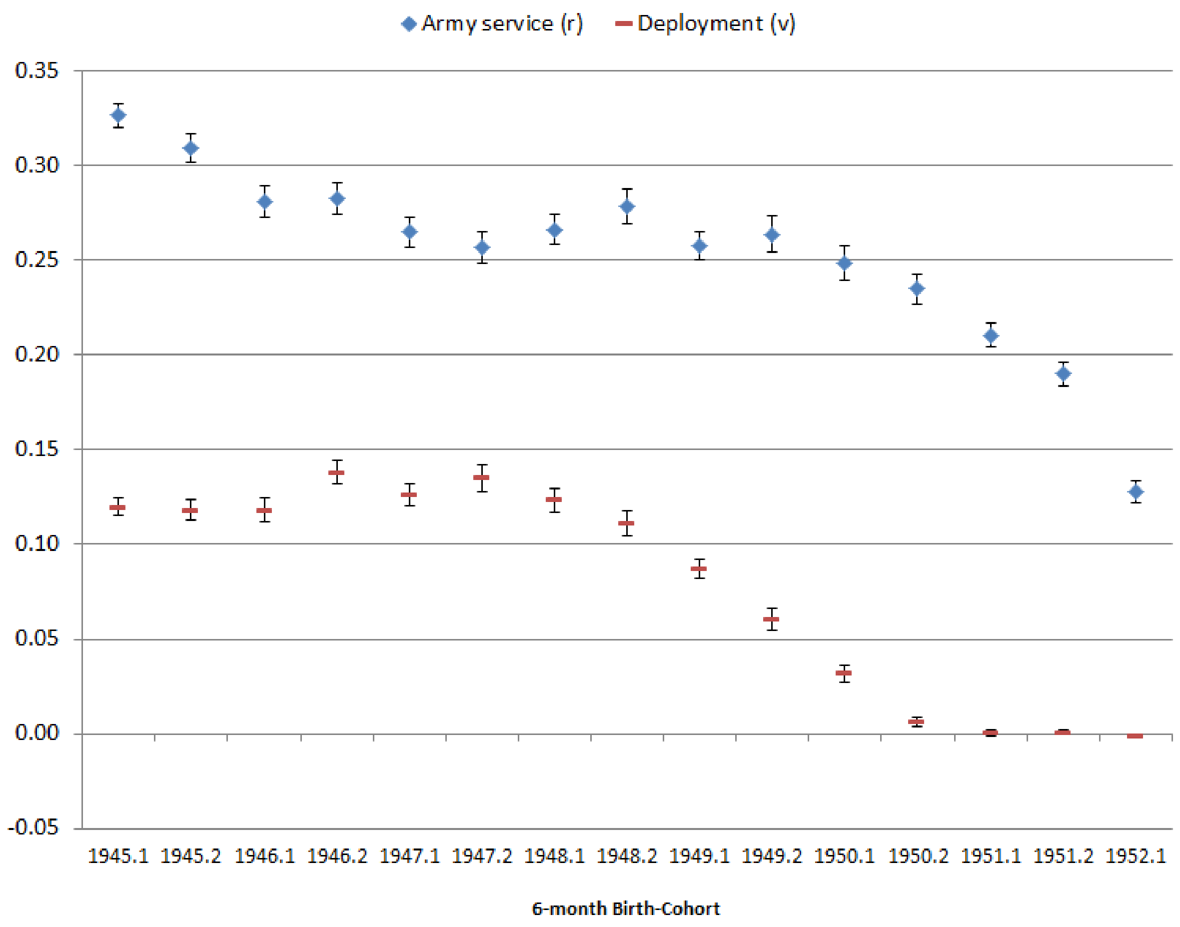

Figure 2: 2SLS Estimated Health Effects of Any Army Service Expressed Relative to Sample Means

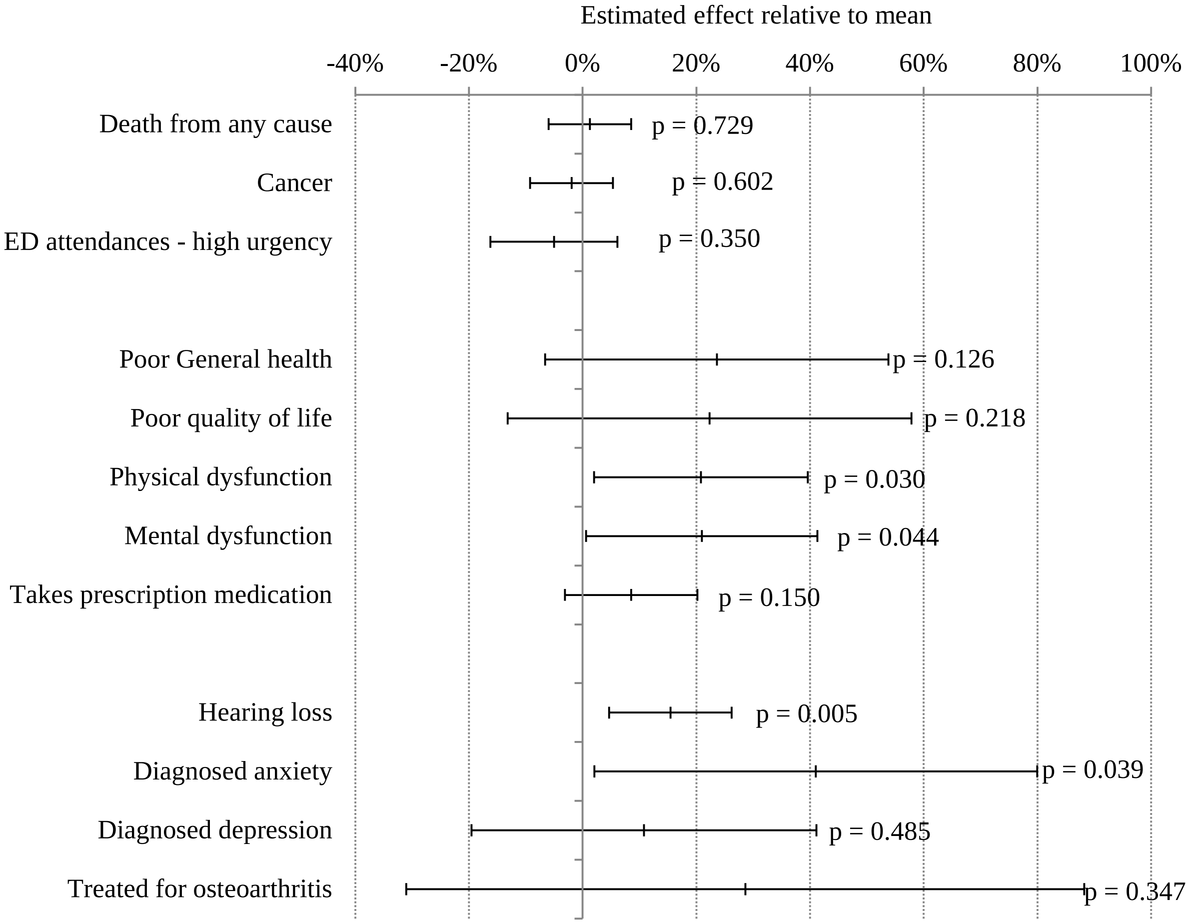

Figure 3: 2SLS Estimated Health Effects of Army Service in Vietnam Expressed Relative to Sample Means

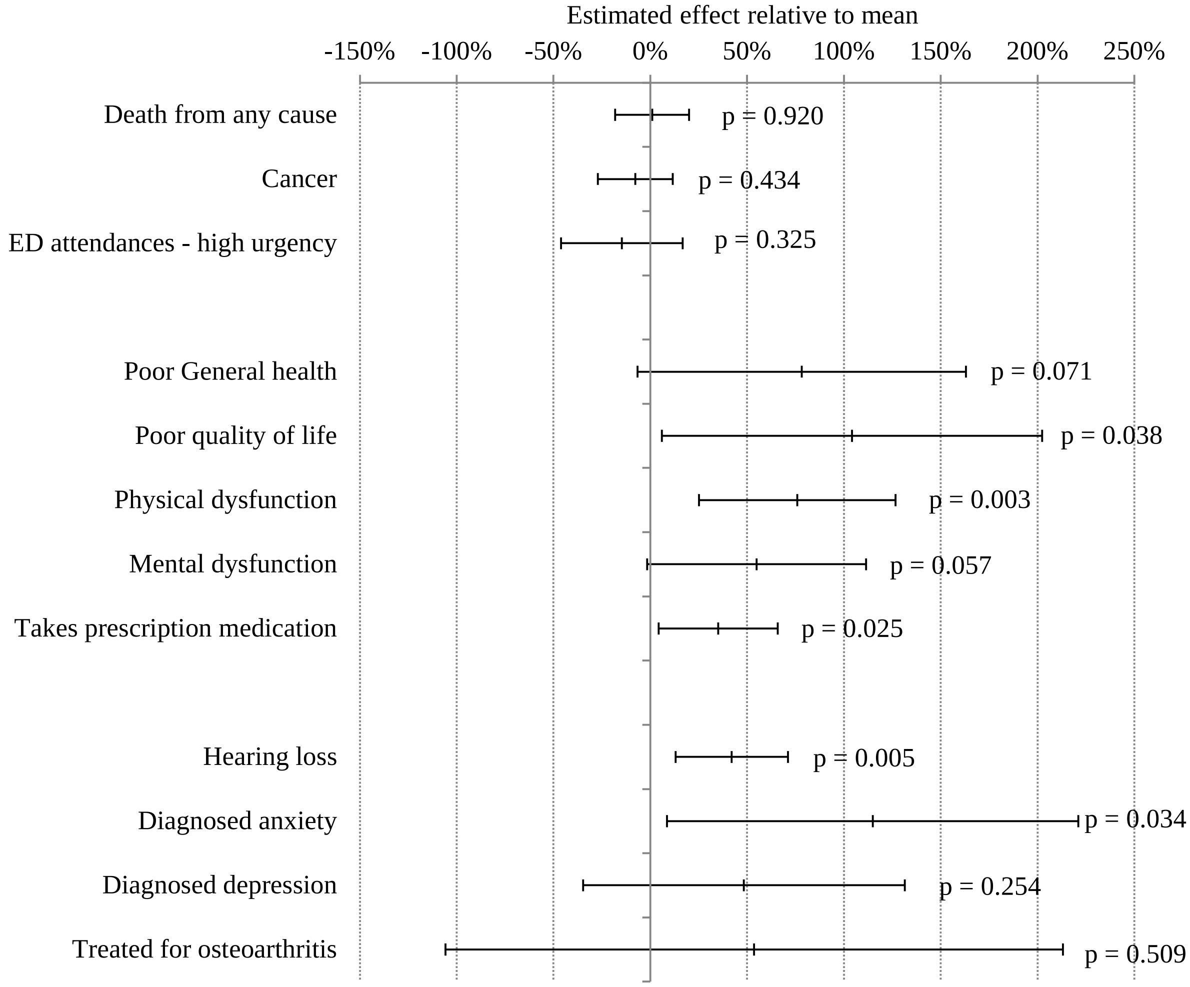

Table 1: Definitions and Descriptive Statistics for Health Outcome Measures

Definition	Data Source	National Servicemen Observation Age Range	Mean	Standard Deviation	Sample Size
Died in Australia (between 1994-2011)	National Mortality Database	42-66	0.0765	0.2659	782,329
Diagnosed with any form of cancer (between 1982-2008)	Australian Cancer Database	30-63	0.1143	0.3182	809,754
Triage 1, 2 or 3 presentations (between July 2005 - June 2010) per person	Emergency Department Database	53-65	0.5495	0.0306	30
Poor or fair overall health	45 and Up Study	54-64	0.1345	0.3412	24,176
Poor or fair quality of life	45 and Up Study	54-64	0.0966	0.2954	23,803
Reversed SF-36 Physical Functioning scale (0 = best, 100 = worst)	45 and Up Study	54-64	13.2483	20.9038	23,017
Reversed SF-36 Role Emotional scale (0 = best, 100 = worst)	45 and Up Study	54-64	17.3667	30.2158	23,917
Takes one or more prescription medications	45 and Up Study	54-64	0.4867	0.4998	24,349
Hearing loss	45 and Up Study	54-64	0.5278	0.4992	24,138
Ever told by doctor has anxiety	45 and Up Study	54-64	0.0900	0.2863	21,344
Ever told by doctor has depression	45 and Up Study	54-64	0.1435	0.3506	21,344
Treated for osteoarthritis in the previous month	45 and Up Study	54-64	0.0408	0.1979	24,752

Table 2: 2SLS Estimated Health Effects of Vietnam-Era Army Service

	Sample Means	Estimated 2SLS Coefficients			
		All Conscripts		Deployed	
	(1)	(2)		(3)	
A) Administrative Measures					
Deceased from any cause	0.076	0.001	(0.003)	0.001	(0.008)
Cancer of any type	0.114	-0.002	(0.004)	-0.009	(0.012)
Emergency hospital attendance	0.550	-0.028	(0.029)	-0.082	(0.080)
B) Summary Health Measures					
Poor general health	0.135	0.032	(0.021)	0.106^{+}	(0.058)
Poor quality of life	0.097	0.022	(0.018)	0.100^{*}	(0.048)
Physical dysfunction	13.25	2.756^{*}	(1.270)	10.27^{**}	(3.499)
Mental dysfunction	17.37	3.640^{*}	(1.804)	9.296^{+}	(4.887)
Prescription medication	0.487	0.042	(0.029)	0.175^{*}	(0.078)
C) Common Health Conditions					
Hearing loss	0.528	0.082^{**}	(0.029)	0.224^{**}	(0.079)
Diagnosed anxiety	0.090	0.037^{*}	(0.018)	0.103^{*}	(0.049)
Diagnosed depression	0.144	0.015	(0.022)	0.069	(0.060)
Osteoarthritis	0.041	0.012	(0.012)	0.023	(0.035)

Notes: Figures are estimated coefficients from separate 2SLS models. Sample sizes and description of health outcome variables are presented in Table 1. Standard errors are presented in parentheses. +, * and ** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Table 3: 2SLS Estimated Health Effects of Vietnam-Era Army Service

	All Conscripts		Deployed Conscripts	
	(2)		(3)	
A) Causes of death				
Circulatory	-0.001	(0.001)	-0.003	(0.004)
External	-0.001	(0.001)	-0.001	(0.002)
Digestive	0.001	(0.001)	0.002	(0.002)
Respiratory	0.000	(0.001)	-0.000	(0.002)
Nervous	0.000	(0.000)	-0.000	(0.001)
Infectious	-0.000	(0.000)	-0.001	(0.001)
Other causes	-0.001	(0.001)	-0.001	(0.002)
B) Cancer types				
Prostate	0.002	(0.002)	0.005	(0.006)
Melanoma	0.001	(0.002)	0.002	(0.005)
Lung	-0.001	(0.001)	-0.004	(0.004)
Colon	-0.001	(0.001)	-0.004	(0.003)
Non-Hodgkin	0.000	(0.001)	0.004	(0.003)
Rectum	-0.002+	(0.001)	-0.004	(0.003)
Kidney	0.000	(0.001)	0.001	(0.002)
Lip	-0.000	(0.001)	-0.002	(0.002)
Unknown	0.001	(0.001)	0.002	(0.002)
Bladder	-0.000	(0.001)	0.000	(0.002)
Brain	-0.000	(0.001)	-0.000	(0.002)
Stomach	-0.001	(0.001)	-0.003	(0.002)
Testicular	0.000	(0.001)	-0.001	(0.001)
Lymphleukemia	0.000	(0.001)	0.001	(0.002)
Hodgkins	0.000	(0.000)	0.000	(0.001)
Soft Sarcoma	0.001+	(0.000)	0.002*	(0.001)
C) Emergency hospital attendance				
Triage level 1	-0.003	(0.002)	-0.004	(0.006)
Triage level 2	-0.001	(0.012)	-0.005	(0.028)
Triage level 3	-0.024	(0.020)	-0.074	(0.055)
Triage level 4	-0.023	(0.018)	-0.092+	(0.047)
Triage level 5	-0.016	(0.014)	-0.018	(0.042)

Notes: Figures are estimated coefficients from separate 2SLS models. Sample sizes and description of health outcome variables are presented in Table 2. Standard errors are presented in parentheses. +, * and ** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Table 4: 2SLS Estimated Mental Health Effects of Vietnam-Era Army Service and Deployment

	All Conscripts (1)		Deployed (2)	
	Estimates (standard errors)	p-values [family-wise]	Estimates (standard errors)	p-values [family-wise]
Mental dysfunction	3.640	0.044	9.296	0.057
	(1.804)	[0.207]	(4.887)	[0.266]
Diagnosed anxiety	0.037	0.039	0.103	0.034
	(0.018)	[0.207]	(0.048)	[0.189]
Diagnosed depression	0.015	0.485	0.069	0.254
	(0.022)	[0.546]	(0.060)	[0.330]
Kessler anxiety index	0.882	0.249	3.623	0.087
	(0.765)	[0.546]	(2.112)	[0.293]
Kessler depression index	0.505	0.559	2.821	0.228
	(0.863)	[0.546]	(2.340)	[0.330]
Poor memory	0.024	0.285	0.102	0.091
	(0.022)	[0.546]	(0.060)	[0.293]
Anti-depressant use	0.011	0.338	0.030	0.331
	(0.012)	[0.546]	(0.031)	[0.330]
Standardised treatment effect	0.075	0.056	0.256	0.017
	(0.039)		(0.107)	

Appendix A – Construction of Survey-Based Health Indicators

This Appendix describes the construction of the survey-based health indicators used in our analysis. The health indicators not discussed here are self-explanatory.

"Poor general health" equals: one for people who reported "fair" or "poor" "overall health"; zero for other valid responses.

"Poor quality of life" equals: one for people who reported "fair" or "poor" quality of life; zero for other valid responses.

"Physical dysfunction" is a reversed version of the SF-36 Physical Functioning scale. It is derived from ten items – self-reports on the extent to which health limits: "vigorous activities (e.g. running, strenuous sports"; "moderate activities (e.g. pushing a vacuum cleaner, playing golf)"; "lifting or carrying shopping"; "climbing several flights of stairs"; "climbing one flight of stairs"; "walking one kilometre"; "walking half a kilometre"; "walking 100 metres"; "bending, kneeling or stooping"; and "bathing or dressing yourself". For each item, a response of "yes, limited a lot" was assigned 1 point; "yes, limited a little" – 2 points; "no, not limited at all" – 3 points. The sum of the scores was then reversed and re-scaled so as to have a possible range of 0-100, with 100 being the worst possible outcome. The variable was set to missing for respondents with less than five valid responses to these ten items. For respondents with (at most five) missing or invalid responses, the non-missing items were similarly summarised to also have a possible range of 0-100.

"Mental dysfunction" is a reversed version of the SF-36 Role Emotional scale. It is derived from three items – self-reports on whether, during the past 4 weeks, the person had problems with work or daily activities because of "emotional problems (such as being depressed or anxious)", with binary responses to: "cut down on the amount of time you spent on work or other activities"; "achieved less than you would have liked to"; and "did work or other activities less carefully than usual." The "yes" responses were counted, and re-scaled so as to have a possible range of 0-100, with 100 being the worst possible outcome. The variable was set to missing for respondents with less than two valid responses to these items. For respondents with one missing or invalid response, the non-missing items were similarly summarised to also have a possible range of 0-100.

The anxiety index was derived from four items: the reported frequencies of feeling “nervous”; “so nervous that nothing could calm you down”; “restless or fidgety”; and “so restless than you could not sit still”. The depression index was derived from four other items: the reported frequencies of feeling “hopeless”, “depressed”, “so sad that nothing could cheer you up” and “worthless”. For each item, a response of “none of the time” was assigned 1 point; “a little of the time” – 2 points; “some of the time” – 3 points; “most of the time” – 4 points; ‘all of the time” – 5 points. Each index was calculated as the sum of the scores on each relevant item, re-scaled so as to have a possible range of 0-100, with 100 being the worst possible outcome. (In “version 1” of the questionnaire – covering 14% of the estimation sample – the item “so depressed that nothing could cheer you up” was used in place of “so sad that nothing could cheer you up”.)

APPENDIX TABLE

Table A1: Top 20 Conditions for Veterans of the Vietnam War, September 2013

Sensorineural Hearing Loss	21,015
Posttraumatic Stress Disorder	17,872
Tinnitus	10,404
Alcohol Dependence and Abuse	6,009
Solar Keratosis	5,224
Lumbar Spondylosis	4,494
Gastro-Oesophageal Reflux	4,202
Osteoarthrosis	3,591
Tinea of the Skin	3,431
Chronic Bronchitis & Emphysema	3,183
Ischaemic Heart Disease	3,145
Irritable Bowel Syndrome	3,091
Erectile Dysfunction	3,011
Non Melanotic Malignant Neoplasm of the Skin	2,980
Hypertension	2,888
Depressive Disorders	2,620
Psychoactive Substance Abuse	1,982
Diabetes Mellitus	1,649
Malignant Neoplasm of the Prostate	1,490
Anxiety Disorder	1,059

Notes: These are counts of veterans with each type of condition, as accepted by the DVA for the purpose of disability compensation.
Source:
http://www.dva.gov.au/aboutDVA/Statistics/Documents/2013_September/Top20_Sep2013.pdf