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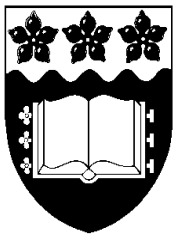


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# Employment Effects of Army Service and Veterans' Compensation: Evidence from the Australian Vietnam-Era Conscription Lotteries[1]

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**Abstract**

Exploiting the Australian National Service lotteries of 1965-72, I estimate the effect of Army service on employment outcomes. Population data from military personnel records, tax returns, veterans' compensation records and the Census facilitate a rich and precise analysis, identified by 53,000 compliers. The employment effect is confined to men who served

[1] I am grateful to Joshua Angrist, Rob Bray, Philip Clarke, Bob Gregory, Simon Ville and seminar participants at the Australian National University, the University of Wollongong, the Labour Econometrics Workshop and the Frontiers in Human Capital Research Workshop for useful discussions and comments. I thank Alison Haynes and Louise Rawlings for excellent research assistance; the Australian Bureau of Statistics, the Department of Veterans' Affairs, the Australian Tax Office and the Australian Institute of Health & Welfare for access and assistance with de-identified data; the Department of Veterans' Affairs and the Australian Research Council for grant support (LP100100417). The views in this paper are the author's alone, as are any errors of fact or omission.

in Vietnam and is very large, at -37 percentage points (95%CI: -32, -43) in 2006. The effect has emerged gradually since the 1990s, and is mirrored by veterans' Disability Pension effects. These results contrast with those for the USA, possibly reflecting differences in employment incentives associated with veterans' compensation.

## I. Introduction

In the context of the ongoing conflicts in Afghanistan and Iraq, there is continuing interest in the human and financial costs of war (Bedard and Olivier 2006; Stiglitz and Bilmes 2008). One aspect of this is the effect of military service on subsequent outcomes for veterans. In the case of the Vietnam War, many studies have considered effects on health and longevity (see for instance Angrist et al. 2010; Conley and Heewig 2009; Dobkin and Shabani 2009; Hearst et al. 1986). Other studies have considered effects on human capital and earnings (Angrist 1990, 1993; Angrist and Chen 2008).

As the majority of Vietnam veterans approach retirement age, attention has shifted recently to the potential work disincentives associated with veterans' compensation (Angrist et al. 2010; Autor and Duggan 2007; Duggan et al. 2010). Unlike social security, US Veterans' Compensation (VC) is neither means tested nor dependent on employment status. Thus, VC has provided opportunities for studying the behavioural responses associated with an almost pure income effect, with implications for the design of both VC and social security. Whilst earlier work was suggestive of a potentially large negative effect of VC on employment (Autor and Duggan 2007), more recently a consensus seems to have emerged that this effect is in fact small or insignificant (Angrist et al. 2010; Duggan et al. 2010), and perhaps manifest only for low-skill veterans (Angrist et al. 2010).

In this paper, I contribute to the debate by estimating employment effects for Australian Vietnam veterans, for whom compensation rates are fundamentally tied to employability. The Australian veterans' compensation system is similar in some ways to the US VC. Compensation includes cash payments and enhanced health insurance. These cash payments are notionally tied to the level of incapacity (on a scale from zero to 100%). However, veterans who are 'Totally and Permanently Incapacitated' receive the Disability Pension Special Rate (DP-SR), which is almost three times larger than the highest rate of general compensation.[2] In the USA, in contrast, 'Individual Unemployability' compensation is no greater than the highest rate of general compensation.[3] In both countries, veterans can combine such compensation payments with income support. In Australia, the difference between the highest rate of general compensation and DP-SR-plus-income-support is equal to 53% of average male full-time (before-tax) earnings. By 2009, about half of Australian Vietnam veterans were receiving DP-SR, almost all of whom began receiving DP-SR after 1990.

For identification of employment effects, I exploit Australia's conscription lotteries held between 1965 and 1972. Sixteen lotteries were held, each pertaining to a six-month birth cohort of 20-year-old men. Over 800,000 men registered for the ballots; 64,000 were enlisted in National Service, including over 19,000 who served in Vietnam (see Langford 1997 for an account of the scheme). For research purposes, these lotteries have several advantages over the corresponding

[2] The DVA Disability Pension is the Australian equivalent of US Disability Compensation. The DP-SR is received almost exclusively by veterans who are 'Totally and Permanently Incapacitated' (TPI). TPI veterans cannot work more than eight hours per week, but in practice most do not work at all. There is also an Intermediate Rate, which lies between the DP-SR rate and the 100% General rate, but few veterans receive the Intermediate Rate.

[3] See Angrist *et al.* (2010) and Autor and Duggan (2008) for details of the US VC system, including 'Individual Unemployability'.

US lotteries. First, the effect of operational service in Vietnam can be differentiated from non-operational service in Australia, due to large differences between cohorts in the probability of Vietnam service. In particular, nobody from the last four cohorts served in Vietnam. Second, randomization was by date of birth (DOB), achieved mechanically by using an actual lottery barrel (on loan from a lottery agency). There is no evidence of randomization failure as in the first US lottery (Fienberg 1971). Third, there was no GI Bill in Australia, and little or no effect of ballot outcome on educational attainment. This is useful for attempts to estimate the contribution of military experience to human capital. Fourth, the DOBs drawn were not published until 1997, so there is no risk of discrimination by employers on the basis of ballot eligibility. Fifth, there were no Random Sequence Numbers. DOBs were balloted in or balloted out immediately, so the ballot outcome could not influence behaviour prior to determination of eligibility. Finally, I show evidence of some external validity beyond the set of compliers who identify the estimates. (For discussions of the limitations of the US lotteries, see: Angrist 1990; Angrist et al. 1996; Angrist and Imbens 1997; Heckman 1997; Keane 2010; Rosenzweig and Wolpin 2000).

Apart from our own work on mortality (Siminski and Ville 2011-forthcoming), no previous studies have used the Australian conscription lotteries for identification. This is probably due to a paucity of data. However, in 2006 DOB was collected in the Australian Census of Population and Housing for the first time. The Australian Tax Office also has granted access to personal income tax return data. I combine these with military personnel records, administrative veterans' compensation records, and published contemporaneous population aggregates to construct unit-record population data for both the first- and second-stage regressions. By using a Two-Sample 2SLS procedure (Atsushi and Solon 2010), I have no need to link the data sources.

I find that the employment effect is confined to men who served in Vietnam and is very large, estimated at -37 percentage points in 2006 when most were in their late-50s. The effect is

relatively recent, emerging gradually since the early 1990s. I also find a corresponding trend in disability pension effects which largely mirror the employment effects, but are even larger. I discuss several factors that may have contributed to these striking results, focusing on the possible role of the Australian veterans' compensation system.

The remainder of the paper is structured as follows. Section II outlines the estimation strategy. Section III discusses the sources of data. The results are presented in Section IV, and Section V concludes. In Appendix A, I discuss the validity of the exclusion restriction. Appendix B provides a descriptive summary of trends in the DVA Disability Pension.

## II. Estimation Strategy

The primary aim here is to estimate the effects of Vietnam-era Army service (r) and Army service in Vietnam (v) on the probability that person *i* is employed in 2006 (y)[4]:

$$y_i = \alpha + \beta_r r_i + \beta_v v_i + \gamma' C_i + \mu_i \qquad (1)$$

C is a vector of 15 binary indicators representing six-month birth cohorts. r is likely to be correlated with μ, because medical screening of potential Army entrants is highly selective, and because men who seek out Army service may differ from those who do not. Selection for service in Vietnam also was non-random, so v is likely to be correlated with μ. Thus OLS estimation of (1) may not uncover the causal effects of service. To solve the problem of selection bias, I construct instruments that exploit the random assignment of the conscription lotteries. A single ballot outcome dummy (z) is a valid instrument. However, the effects of ballot outcome vary

[4] National Service was in the Army. In our preliminary analysis, we confirmed that the ballot outcome did not induce any men into other branches of the armed forces. The results are virtually unchanged (but slightly less precise) if Army service is replaced with military service.

between cohorts, so a more efficient strategy is to interact z with C. Let Z be a vector of 16 binary instruments, representing the ballot outcome in each cohort, respectively.[5] The two first-stage regressions are identified by the exclusion of Z from (1), along with considerable differences between cohorts in the proportion of conscripts sent to Vietnam. The first-stage regressions are:

$$r_i = \pi_0^r + \pi_1^r{}' Z_i + \pi_2^r{}' C_i + \varepsilon_i^r \tag{2}$$

$$v_i = \pi_0^v + \pi_1^v{}' Z_i + \pi_2^v{}' C_i + \varepsilon_i^v \tag{3}$$

Using a Two-Sample 2SLS procedure (Atsushi and Solon 2010), I estimate the first stage-regressions (2) and (3) by OLS with one dataset and the second-stage regression (1) with a different dataset by OLS, after I replace r and v with the fitted values from (2) and (3). That is, I use cross-sample fitted values of r and v. However, the first-stage coefficients come from population data and therefore are treated as known. For simplicity, I do not introduce heterogeneous treatment-effect notation, but I do note that the effect of Army service may vary between individuals and subpopulations. This approach estimates the average effects for men induced by the ballot outcome to enlist in the Army (balloted-in 'compliers').[6] In general,

[5] The term 'ballot outcome' refers to the outcome of the main conscription ballots. z = 1 for men 'balloted in' and zero for men 'balloted out'. A man is considered be 'balloted in' if his DOB was drawn in the ballot held for his birth cohort. A separate set of ballots were conducted for the presumably small number of men who were out of the country at the time of their main ballot (Langford, 1997). This is not a complication, because each ballot was independent and hence orthogonal, so the outcome of the main ballot was irrelevant for those who were temporarily absent.

[6] In the case of $\beta_v$, the approach estimates the average effect for men induced by the ballot outcome to serve in the Army *in Vietnam*.

compliers cannot be individually identified (Angrist et al. 1996). However, in this case balloted-in Army compliers are a majority subset (84%) of National Servicemen.[7]

I also show the results from specifications that include only one of the endogenous variables, including the estimates for each individual cohort using a set of just-identified models.

The model is then extended by interacting v with p, which represents age eligibility for the Service Pension (p = 0 if age < 60 years, 1 otherwise). Subject to a means test, veterans with 'qualifying service' (which essentially equates with service in Vietnam for the study population) are eligible for the Service Pension from 60 years of age. Eligibility for the otherwise equivalent social security Age Pension is from the age of 65. Service pension eligibility may encourage early retirement, due to both the income and substitution effects associated with pension means tests (Autor and Duggan 2007) and so $\beta_p$ is hypothesised to be negative.

$$y_i = \alpha + \beta_r r_i + \beta_v v_i + \beta_p v_i p_i + \gamma' C_i + \mu_i \quad (4)$$

Finally, I add a proxy for combat intensity in Vietnam (x), which varies between cohorts. Since this is only relevant for those who served in Vietnam, it is interacted with v.[8]

[7] Unlike in the US, there was no apparent incentive for balloted-in men to volunteer for service in the Regular Army, so balloted-in compliers are a subset of National Servicemen. However, not all National Servicemen are compliers. These include: men who were absent from Australia when their age group was required to register and were subject to the separate, latter ballots; men who failed to register for the ballots and were hence automatically considered for National Service call-up; National Service volunteers; and men who would have volunteered subsequently had they not been conscripted. Such men were not induced into the Army by the ballot outcome and hence do not contribute to the identification of the model.

[8] Since p and x vary only between cohorts, their main effects are already included in C.

$$y_i = \alpha + \beta_r r_i + \beta_v v_i + \beta_p v_i p_i + \beta_x v_i x_i + \gamma' C_i + \mu_i \quad (5)$$

I hypothesize that men who served in Vietnam in periods of greater combat intensity will be more likely to suffer from detrimental employment effects, and thus $\beta_x$ should be negative. However, I have limited confidence in the reliability of the combat intensity proxy, as I discuss in the sections that follow.

In addition to the 2SLS estimates, I report reduced-form estimates, which are more convenient in some components of the analysis.

The exclusion of z from the employment equation is valid if its only effect on employment outcomes is through inducement of Army service. In Appendix A, I consider several hypothesized violations of this condition in relation to draft-avoidance behaviour, but conclude that these are not a major concern.

## III. Data

For the first- and second-stage regressions, a number of databases were constructed from various sources, as described below.

### A. First-Stage Database

For the first stage, the population consists of men born in 1945-52 who were Australian residents when they turned twenty. Military personnel unit records were obtained from two sources: the Nominal Roll of Vietnam Veterans (NRVV) from the Department of Veterans' Affairs (DVA) and the Vietnam-Era database (VED) from the Australian Institute of Health & Welfare (AIHW). Both contain DOB.

The NRVV is believed to be of very high quality and contains records for all Vietnam veterans. Over several decades, the DVA has made a concerted effort to complete and refine its contents. However, personnel who did not serve in Vietnam are not included in the NRVV.

The VED contains records for all military personnel who served during the Vietnam War era. It is used for data on men who served during the Vietnam era but not in Vietnam. The VED has been used and refined by the AIHW & DVA for various health and mortality studies for nearly three decades. The VED version used in this study was also used by Wilson et al. (2005). Whilst successive modifications to the database are not well documented, the original source was a database from the Central Army Records Office of 'all Vietnam Veterans and non-veterans who served during the Vietnam conflict era' (AIHW 1992: 98). The estimated number of compliers, and their distribution between cohorts (see first-stage results in Section IV) conforms to prior expectations that are based on summaries of the National Service scheme (e.g. Department of Labour and National Service various years; Langford 1997).

For men who did not serve in the army ($r = 0$), records were synthesised using published population data (Australian Bureau of Statistics 2008).[9]

[9] Counts of 20-year-old men at June of each calendar year are published, subject to a definitional change in 1971 (see Australian Bureau of Statistics, 2008 for details). Within each single year of age, the proportion in a six-month birth cohort is assumed to equal the corresponding proportion in the 2006 Census, after excluding migrants who arrived after the age of 20. In practice, this proportion remains close to 0.5 (varying from 0.49 to 0.51). Within each cohort, the proportion of men whose DOBs were balloted-in is assumed to equal the proportion of DOBs that were balloted-in. Finally, the counts of Army servicemen (taken from military personnel records) are subtracted from the total residents in each cohort and ballot-outcome status.

The upper panel of Table 1 describes the first-stage data. The population consists of 868,605 men; 9.4% of them served in the Army during the Vietnam era and 3.4% of them served in the Army in Vietnam. Those who served in Vietnam spent an average of 261 days there. I do not have data on total time spent in the Army, although the terms of National Service stipulated a two-year commitment (which was reduced to 1.5 years in 1971). Of the Army servicemen who served in Vietnam, 342 or 1.2% died in Vietnam. Balloted-in men were much more likely to serve in the Army (27.1%) than those balloted out (3.2%). Similarly, they were much more like to serve in Vietnam (8.8% compared to 1.5%).

## B. Second-Stage (Outcomes) Data

As detailed below, several outcomes databases were constructed. None were linked to each other, or to the first-stage data on a unit-record level.

### *1. Census 2006*

In 2006 for the first time, the Census of Population and Housing collected DOB from all respondents. The estimation population is the set of men born in 1945-52, excluding those who arrived in Australia after the age of 20. Noting that each variable in the model is categorical, a unit-record database was reconstructed from frequency tabulations of employment status by cohort and ballot outcome. This covers every permutation of the variables in the second-stage regression.

There are 675,832 such men in the Census data, 22% fewer than in the first-stage database. Contributing to this attrition is mortality (9%), missing DOB in the Census (approximately 4%) and Census undercount (approximately 2%). The remaining 7% is presumably attributable to outbound migration. Such attrition is a threat to validity if it is correlated with ballot outcome. However, the existing evidence suggests that this is not a major threat. In the Census data, men with balloted-in DOBs make up 25.9% of the men in this age range, as compared to an expected

25.8% if the DOB frequency distribution were independent of ballot outcome within each cohort. Thus, balloted-in men are not disproportionately 'missing' from the Census data. Further evidence suggests that any excess mortality amongst those balloted in is likely to be small. Some 198 National Servicemen (176 of whom had 'balloted in' DOBs) died in the Vietnam conflict. This represents around 0.3% of National Servicemen, or 1% of National Servicemen who served in Vietnam, or 0.02% of the study population. A mortality analysis comparing National Service veterans to National Service non-veterans (Wilson et al. 2005) implies a similar number of (additional) post-service excess deaths. My own research, exploiting the conscription lotteries, finds no evidence of significantly elevated post-service mortality (in 1994-2007) associated with Army service (Siminski and Ville 2011-forthcoming).

The middle panel of Table 1 includes a descriptive summary of Census data. The employment rate for balloted-out men is considerably higher than for balloted-in men. Similarly, the disability rate is higher amongst balloted-in men. There is little difference in educational attainment by ballot outcome.

### *2. Personal Income Tax Data - 1993-2006*

A supplementary second-stage database was constructed from the full set of personal income tax return data for 1992-3, 1995-6, 1999-2000, 2002-3, 2005-6 and 2008-9. Not all residents submit tax returns (Leigh 2005) but almost all employees do. Employees who had any tax withheld or were liable to pay tax are required to submit a tax return, and the number of men in the database with positive wage-and-salary income corresponds closely to data from other sources.[10]

[10] For example, 642,253 men born in 1945-52 reported positive wages and salaries in 2001-2 on their tax returns, compared to the 631,358 men who worked for wages or salaries as estimated from HILDA data. For 2005-6, the counts are 576,477 (tax return data) and 531,237 (HILDA).

Non-residents are excluded. Preliminary analysis revealed slightly elevated counts of men with DOBs listed as 1 January, 1 July, or 10 October in each year, and they are also excluded. The five youngest cohorts are also excluded in order to reduce noise, because the employment effects are confined to the older cohorts in the (higher quality) Census database, as will be shown. The results are not sensitive to these exclusions. For men with no earnings, records are synthesized using published contemporaneous counts of resident males by age, much like the construction of the first-stage database. Recent migrants cannot be identified, so the appropriate population for the first-stage analysis varies between years and is not immediately intuitive. Because my rationale for using tax data is to study trends over time, I avoid this complication by reporting only reduced-form results from that data.

The lower panel of Table 1 presents summary statistics from the first and last years of these data. The share of men with positive earnings does not differ by ballot outcome in 1992-3, but it is almost 4 percentage points lower for balloted-in men in 2008-9 (0.429 compared with 0.468). Mean log earnings in 1992-3 are 10.24 regardless of ballot outcome, and are slightly higher in 2008-9 for the balloted-out men (10.47 as compared to 10.44 for balloted-in men).

### *3. Veterans' Disability Pension Database*

I constructed a veterans' disability pension database from frequency tabulations of administrative data from the DVA, combined with resident population data, much as I did with the income tax data described above. Appendix B contains a detailed descriptive summary of trends in disability pension receipt.

## IV. Results

### A. First Stage Results

Table 2 shows results from the first stage regressions for r and v. The upper panel is from the regressions with a single ballot outcome instrument. The ballot outcome is estimated to have induced over 23% of balloted-in men into the Army, including over 7% into Army Service in Vietnam. The IVs are strongly significant in both regressions, with t-stats of 245 and 117, respectively. Similarly, the IVs are very strong in the overidentified models (lower panel of Table 2), with F-stats of 8,485 and 2,709, respectively. The coefficient of z16 is close to zero and is not statistically significant in either first-stage regression. This is sensible, because conscription was abolished in December 1972, shortly before men from the 16$^{th}$ cohort were due to be called up. The IVs z13, z14, and z15 are strong predictors of r, but not of v. Similarly, z9, z10, and z11 are much stronger predictors of r than v, relative to z1-z8. This is useful, because it allows me to differentiate the effect of service in Australia from the effect of operational service in Vietnam.

The first-stage results imply that around 53,674 men were induced by the ballot outcome to enlist in the Army. The number induced is 10,066 fewer than the total number of National Servicemen (63,740) (Langford 1997). Compliers are a subset of National Servicemen, because there was no apparent incentive for balloted-in men to join the Regular Army rather than await call-up for National Service, unlike the US experience (this is also evidenced by the insignificant coefficient of z16 in Table 2; see also footnote 7).

In Section III, it was argued that any correlation between mortality and ballot outcome is not large enough to be a substantial threat to the validity of the approach. I now consider a related but more subtle issue, drawing on the first-stage results. Veterans often have lower mortality rates than the general population, because of pre-enlistment health screening. Between 1966 and 2001, the mortality rate for National Servicemen was 27% lower than community norms (Wilson et al. 2005). This suggests that compliers (both balloted-in and balloted-out) account for a rising

share of each cohort over time, thereby increasing the size of the ballot-outcome effects in the first stage. Consider men who were 20-years old in 1965. Between 1965 and 2006, 11.6% of them had died (author's calculations from AIHW National Mortality Database). Extrapolating from Wilson et al.'s estimates, the corresponding proportion of National Servicemen who died after the Vietnam War is approximately 11.6% times 73% = 8.5%. Compliers account for 32.7% of the first birth cohort (see coefficient of z1 in Table 2). By 2006, they probably account for roughly 32.7% * (1 - 0.085) / (1 - 0.116) = 33.8% of the surviving population. Because death rates are lower for younger cohorts, the magnitude of this issue is smaller for younger cohorts. Given the small magnitude of bias and in the interest of transparency and simplicity, no adjustments have been made.

### B. 2SLS Employment Effects (Census 2006)

Figure 1 shows 2SLS employment effect estimates separately for each cohort. The youngest cohort is omitted, z16 is irrelevant in the first stage. The top panel shows that Army service had a major negative effect on the likelihood of employment, but only for some cohorts. In particular, for cohorts in which no men were induced to serve in Vietnam (those born in 1951 and 1952), the effect of Army service is close to zero, is not statistically significant, and is precisely estimated. This strongly suggests that the employment effect is confined to men who served in Vietnam.

The lower panel of Figure 1 shows corresponding estimates under the assumption that Army service only affected employment for those who went to Vietnam. The five youngest cohorts are omitted because of the weakness of z12-z16 in the Vietnam service first-stage. In the lower panel, there is much less variation between cohorts, although the effect is slightly larger for the first three cohorts. In these cohorts, Vietnam veterans were age 60 or 61, and hence were eligible for a Service Pension (Age), while other men (civilians of the same age, Army veterans who did

not serve in Vietnam, and younger Vietnam veterans) were not. The estimates for the last two cohorts shown are less precise, reflecting slightly weaker first stages.

Table 3 shows 2SLS estimates across cohorts. In the upper panel are the results of the just-identified specifications, using a single ballot-outcome instrument. The lower panel has results from the models that use the full set of 16 IVs. Column 1 presents the estimates from specifications with Army service (r) as the sole endogenous regressor. The estimated coefficient of r is -0.111 using one IV and -0.120 with all 16 IVs, with very small standard errors. In column 2, army service is excluded from the model, replaced with army service in Vietnam (v), which has an estimated coefficient of -0.365 with one IV. With all 16 IVs, the point estimate remains almost unchanged at -0.366, but is more precise.

The results in Column 3 are for the specification in equation (1), where the coefficients of r and v are both identified. The estimated coefficient of v is -0.361, almost unchanged by the inclusion of r. However, the coefficient of r is close to zero and is not statistically significant, despite a small standard error. This suggests that the negative employment effect of Army service is only manifest amongst men who served in Vietnam. This is consistent with the results in Figure 1, which show no employment effect for the cohorts in which conscripts were not sent to Vietnam. Further, r is not statistically significant in any of the other specifications that include v.

In column (4), I add the interaction of v and p (age eligibility for the Service Pension), which is statistically significant at the 1% level. This is not surprising, because it represents the effect of eligibility for an early retirement pension. What may be surprising is how small the effect is (-8.3 percentage points), suggesting that a clear majority of Vietnam veterans who were induced to retire early (before the age pension eligibility age of 65) did so before reaching service pension eligibility age (60 years).

In column (5), I add a proxy for combat intensity (x), defined as the ratio of deaths in Vietnam to men who served there for each cohort, rescaled to range from zero to one. This variable is interacted with v, because only those who served in Vietnam saw combat in that period. Other recent studies have used similar proxies for combat intensity (Costa and Kahn 2008; Rohlfs 2010). This variable is not statistically significant, but the estimate is imprecise. The reliability of this proxy is questionable, since there were relatively few deaths in most cohorts. Further, the probability of death in Vietnam is highest for the oldest cohorts, so it is difficult to know whether the variable is picking up combat intensity or age effects.[11] A better proxy would be based on combat injuries, not deaths, but it is not available. A complete database of combat injuries has been compiled (Fett et al. 1984), but it is unclear whether it still exists and its use by external researchers is prohibited by a specific Act of Parliament: the Epidemiological Studies (Confidentiality) Act 1981. A better measure of combat intensity may warrant further research. However, it is clear that there is actually little variation in employment effects between cohorts if Army service affected employment only for those who went to Vietnam (lower panel, Figure 1). I return to this issue in the analysis of Disability Pension data later in this Section.

The overidentification test (Table 3, column 1) confirms that the Army service LATEs differ between cohorts, which is consistent with the upper panel of Figure 1. For the other specifications (columns 2 to 5), the overidentification tests show no evidence of heterogeneous average effects between cohorts.

[11] Variations of this proxy also were tried (not shown), including versions that vary by ballot outcome and smoothed versions based on local averages with adjacent cohorts. None led to precise enough estimates. Another proxy considered was share of National Servicemen serving in infantry battalions, which had much higher casualty rates than other units. This was also futile, because there is almost no variation between cohorts (stable around 41%).

A broad literature suggests that employment effects should be larger for men with low potential earnings (see for example Angrist et al. 2010; Autor and Duggan 2003, 2006; Cai and Gregory 2004). That cannot be tested here, because there are no proxies for potential earnings available to interact with the instruments in the first-stage data. I do find that the share of the employment effect accounted for by low-skill occupations (labourers, and machinery operators and drivers) is considerably larger than the share of employment in those occupations (results available on request). But it is possible that compliers were concentrated in those occupations to begin with. This could be the result either of selection (high-skill balloted-in men more likely to avoid enlistment), or a causal effect of service. I find no evidence that Army service reduced average earnings (later in this Section). But, there are no data to test the selection argument. Men with poor health and low intelligence were screened out in the medical and aptitude tests. However, high skill may have been more resourceful in avoiding conscription, and they had a greater incentive to do so, precisely because of their potential earnings. So, I cannot confidently determine whether the employment effect is concentrated amongst low-skill men.

### C. Employment and Disability Pension Effects Over Time

Table 4 shows the reduced-form estimates of employment and disability pension effects for various years. The youngest five cohorts are excluded, as discussed previously.

The employment effect emerged gradually, beginning in the mid-1990s. The estimates are negative and statistically significant for each year from 1998-9 to 2008-9. They increase (become more negative) steadily throughout the period. The effect is largest for 2008-9 at -2.0 percentage points.[12] For 1992-3, the employment effect is actually positive (0.4 percentage points) and

[12] When analysed comparably (by excluding the last five cohorts and including recent migrants), the corresponding effect in the 2006 Census is larger (-3.1 percentage points). This is probably due to differences in the definition of being employed, including the treatment of self-

significant. Undoubtedly, some men were permanently unable to work immediately after being injured in Vietnam. However, their numbers are apparently too small to drive the results for the early 1990s. There is reason to be cautious in interpreting the positive effect for 1992-3, given how small it is and a relative instability in effect sizes between cohorts as compared to the Census results (results by cohort are available on request). Nevertheless, the tax data suggest that the employment effect was either small or zero as recently as the mid-1990s.

DP-SR effects also increased strongly between 1990 and 2009, by a factor of over 26, from a very small base (Table 4). The similarity of trends in DP-SR effects and employment effects strongly suggests that the two are linked. The DP-SR effects are considerably larger than the employment effects derived from tax data (Table 4) or from Census data (footnote 12). Thus, the growth in DP-SR more than accounts for the employment effect. This is probably because some compliers who receive DP-SR, and are hence not employed, would also not be employed in the counterfactual (i.e. if they had been balloted out), but would be ineligible for DP-SR.

It is impossible to precisely delineate the roles of age, cohort and period effects as drivers of the DP-SR trends because of multicollinearity (birth year + age = period). Figure 2 presents DP-SR recipients as a share of each cohort of compliers, collapsed into one-year groups, by year (in the upper panel) and by age (lower panel). Clearly, either age or period effects, perhaps both, contribute to the increase in DP-SR receipt. But the strong relationship between combat intensity and birth year (discussed above) makes it difficult to rule out cohort effects. It is notable, however, that the proportions converge across cohorts at older ages (lower panel), suggesting that cohort effects (e.g. differences in combat intensity) must be a minor contributor to DP-SR pension receipt.

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employment (excluded from the tax data employment definition) and differences in the reference period (one week in the Census, as compared to a full year in the tax data).

### D. External Validity

Compliers are likely to have different characteristics to men who joined the Army voluntarily. Further, compliers were limited to Army service, but other Vietnam veterans served in all branches of the armed forces. Excluding compliers, 33% of Vietnam veterans in the same age groups served in the Navy and 8% served in the Air Force. Figure 3 shows the proportion of compliers and other Veterans who served in Vietnam that received the DP-SR over this period (for simplicity, no adjustments are made for mortality). The growth in DP-SR for other Vietnam veterans is strikingly similar to that of compliers. This is a strong indication that the results have some external validity. The trends in disability pension take-up are not confined to conscripts, nor to Army servicemen.

### E. Earnings and Income

How did the dramatic changes in employment and disability pension receipt affect compliers' standard of living? I show reduced-form estimates for total income in the 2006 Census (since source of income is not available) and earnings in 1996 tax data.[13]

I find no significant effect on earnings in 1996. In logarithms, the point estimate is positive (0.006) and marginally insignificant (p=0.069). The effect also is insignificant in levels ($120 pa, SE = $112). Thus, there was no adverse effect on the mean earnings of compliers in 1996, although other income sources, such as the disability pension, are ignored.

[13] As shown previously, the coverage of men with non-zero earnings is very good in the tax data. However, the tax data are not reliable for studying income from all sources in the present context, because many men without earnings do not file tax returns. Given that there is no significant employment effect in 1996, I assume that an analysis of earnings in 1996 is not subject to selection bias. The youngest 5 cohorts are excluded for both years, as for the earlier analysis that drew on tax data.

Using equivalent specifications, I find no significant effect on mean personal income in 2006.[14] Specified in logs, the point estimate is 0.001, (SE = 0.003) or -$193pa in levels (SE = $121).[15] However, in the Census income is reported before tax, and the DVA Disability Pension is not taxable. Although not conclusive, the results suggest that the effects on mean (after tax) income likely were positive in both 1996 and 2006. But the data do not reveal whether the reduction in earnings over this period was offset by the increase in transfers.

## V. Conclusion

Using unit-record population data, I find huge negative effects of army service on employment for Australia's Vietnam veterans; effects that are confined to men who served in Vietnam. These effects have only emerged in the last 15 years, coinciding with even larger veterans' disability pension effects. I have shown evidence suggesting that the effects are not confined to those veterans who were induced to enlist by the conscription ballots.

These results are consistent with aging-related exacerbation of service-related health conditions. However, similar employment effects have not been found for the U.S.: my estimate for 2006 is -37 percentage points, while Angrist *et al.*'s (2010) estimate for the U.S. is just -0.5 percentage

[14] Only categorical income data were collected in the Census, so income is set to the midpoint of each category. The open-ended highest category is set to $2500 per week. The results are not sensitive to the assumed value, because ballot outcome is found to be orthogonal to the probability of having income in this category.

[15] However, the distribution of income is heavily compressed amongst balloted-in men in 2006. The probability of having income between $400 and $800 per week was much higher (reduced form effect = 0.022, SE 0.002) for balloted-in men. The probability of having higher or lower incomes was correspondingly reduced.

points, and it is not statistically significant.[16] Differences in Australian and U.S. involvement in Vietnam are unlikely to explain this discrepancy: considerably higher proportions of American soldiers serving in Vietnam were killed (1.7%) and wounded (8.9%), as compared to their Australian counterparts (0.9% and 5.2%, respectively) (U.S. DVA 2008; Wilson et al. 2005).

Thus an explanation must be sought elsewhere. It has been suggested that Australia's system of veterans' entitlements is amongst the most generous in the world (Clarke et al. 2003). Indeed, a far greater proportion of Australia's Vietnam veterans receive disability compensation than U.S. veterans. The 'reverse criminal' standard-of-proof for disability claims made by Australia veterans is very generous (see Appendix B). However, it seems likely that the results are explained primarily by the central role of employability in the compensation system. There is a very large difference in the Australian disability pension payment rate between veterans who can and cannot work, which is not present in the U.S. system. A simple policy change that might be worthy of further research is an increase in the highest general disability pension rate to equal the DP-SR rate. This would not disadvantage any veteran, but would remove the work disincentive.

A rough, conservative estimate of the present value of lost earnings for Vietnam veterans is $9.7bn (AU$2010, assuming a 5% discount rate), or almost $250,000 per soldier who served in Vietnam.[17] No detrimental effects of Army service on mean income are found, so this loss in earnings appears to have been fully compensated.

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[16] They did, however, find a large and significant (though imprecisely estimated) effect of around 10 percentage points for low-skill workers. There are two qualifiers to this comparison. Angrist *et al.*'s estimates are mean effects for Vietnam era veterans, not just for those who served in Vietnam, and are estimated using earlier data (2000), and hence for younger veterans.

[17] For this estimate, it is assumed that the employment effects estimated for compliers also apply to other Vietnam veterans from the 1945-52 birth cohorts. The estimate is based on the

The Australian conscription lotteries provide a natural experiment of the highest quality. Given the similarities of Australian and U.S. culture and institutions, further comparative research is likely to be informative. In future work, we aim to contribute to debates on the effects of army service on human capital accumulation, and on other outcomes including crime and marital stability.

## Appendix A: Draft Avoidance Behaviour and the Exclusion Restriction

The ballot outcome was randomly assigned, but its validity as an instrumental variable depends on the assumption that its only effect on employment was via inducement of army service. Here I consider several forms of draft avoidance behaviour as potential violations of this assumption. I conclude that there is little reason for concern.

### A. University Education and Apprenticeships

Balloted-in students, apprentices, and trainees were granted temporary deferments of National Service liability (Langford 1997). Prior to their ballot, men thus had an incentive to enrol in a course of study or apprenticeship. Such actions occurred prior to the ballot and hence were orthogonal to ballot outcome. However, after their ballot, the balloted-in men who had been granted temporary deferments had an incentive to continue their studies rather than dropping out, because temporary deferments were reassessed annually. If any such induced education improved long-run employment prospects, then the validity of the IV is challenged. However, analysis of Census data suggests this is not a significant issue. There is some evidence that

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employment effects from Census data, the trends estimated using tax data, and male average weekly earnings. Effects on older and younger Vietnam veterans, as well as possible effects prior to 1996 and after 2009, are excluded.

balloted-in men were slightly more likely to have post-high school educational attainment (the reduced-form estimate is 0.004; p=0.013). However, this effect appears to be confined to vocational qualifications (diplomas and certificates), whilst the effect on degree or higher qualifications is not statistically significant. Further, there is a second potential explanation for the slightly elevated attainment of vocational qualifications, which is not a threat to instrument validity: the post-discharge National Service Vocational Training Scheme covered fees, travel fares, textbooks, and equipment, as well as a living allowance for those studying full-time. This may have raised the attainment of vocational qualifications. It is possible to include education in the employment equation, but this has not been pursued, due to the weak education first-stage.

B. Marriage

Men who were 'married at the time that call-up for their age group commenced' were granted indefinite deferment of National Service liability (Australian Government 1966). Again, inducement of marriage prior to the ballot is orthogonal to ballot outcome. However, it appears that there was a small window of opportunity (approximately two to three months) for men to marry after the ballot, but before call-up. As an example, the first ballot was held on 10 March 1965. Most men were informed of the outcome 'within a fortnight', whilst call-up action commenced on May 31, 1965 (Australian Government 1966). In later years, similar rules prevailed (see for instance Department of Labour and National Service 1971). This appears to have allowed men to avoid conscription through marriage after the ballot outcome was known. However, to the authors' knowledge there were no such concerns raised at the time. The fact that the government policy on married men did not change substantively over this period provides further support for a lack of concern.

### C. Health

Balloted-in men were not liable for call-up if they failed medical or psychological examinations. The proportion of men who failed these examinations increased during the period. It has been suggested that one explanation for this is the availability of 'various tablets and medications' that helped men to fail the medical tests (Langford 1997). If such actions had long-run employment consequences for these men, then the exclusion of ballot outcome from the employment equation is invalid. To our knowledge there is no evidence in the historical records of such concerns being raised.

### D. Non-compliance and Prison

One way in which men dissented and did not comply with National Service obligation was to fail to register for the ballot. If detected, such men were automatically liable for call-up and most of them eventually conformed with the requirements. Only 14 men were imprisoned for failing to comply with a call-up notice (Langford 1997). At least some of these men probably did not register for the ballot, so not all of them would have had balloted-in birthdays. Thus, the ballot outcome had a negligible effect on imprisonment amongst those who were not induced into the Army.

### E. Leaving the Country or Going 'Underground'

Other ways to avoid the draft included hiding or fleeing the country. This represents a threat to validity if substantial numbers of men did so as a result of being balloted-in and/or called up and if this affected their long-run employment outcomes. In the US, the practice of avoiding the draft by moving to other countries such as Canada reportedly was widespread. This practice has not received substantial attention in the Australian literature, which suggests that it is less of an issue, presumably for geographical reasons. A few examples of hiding and/or moving overseas are given by Ham (2007: 271), sourced from Langley (1992). It is also unclear whether such

actions were taken by men who failed to register or by those balloted-in. The official history of Australia's involvement states that prior to 1968 'some young men were covertly evading service by devices such as travelling to New Zealand' (Edwards 1997: 217) but no reference is given for this claim. Amendments to the National Service Act were made in 1968, which prevented airlines and shipping companies from issuing tickets to men of conscription age without a departmental certificate (Edwards 1997: 217).

### F. Citizen Forces

As an alternative to facing the National Service ballot, men could elect to serve in the Citizen Forces (Army, Air Force or Navy Reserves) before their ballot (Department of Labour and National Service 1966; Australian Government 1966). As in the cases discussed previously, this is not a threat to validity because the choice to join the Citizen Forces was made prior to the ballot. However, there was a brief period (up to December 8, 1965) when balloted-out men could exploit a loophole. After finding out they were balloted-out, these men could resign from the Citizen Forces. The ballot outcome thus not only had the effect of inducing some men into National Service, but it also induced other men to *stay* in the Citizen Forces. This is only an issue for the first two cohorts, and only to the extent that involvement in the Citizen Forces affected long-run employment outcomes. Further, at December 31 1965, only 879 balloted-in men had their liability deferred due to enlistment in the Citizen Forces (Australian Government 1966), equal to 2.4% of men balloted-in in 1965. Thus, even for the 1945 birth-cohorts, this is a negligible issue.

## Appendix B: The DVA Disability Pension

This Appendix describes key elements of the DVA Disability Pension and trends in its receipt.

The DVA Disability Pension is roughly equivalent to VA Disability Compensation (DC) in the US. The Disability Pension - Special Rate (DP-SR) is comparable to VA Individual

Unemployability (IU) in the US. Almost all recipients of the DP-SR are 'totally and permanently incapacitated' and can work no more than eight hours per week. Less than 1% of 50-59-year-old DP-SR recipients report earnings to the DVA on their Service Pension assessment in a given fortnight. Thus, almost all DP-SR recipients are not employed. In contrast, Non-SR DP receipt does not involve any restrictions on employment. Receipt of DP can be combined with income support, from DVA or from the social security system. Whilst linked data are unavailable, it is likely that almost all Vietnam Veterans who are DP-SR recipients also receive the DVA Service Pension-Invalidity, unless they fail the means test. DP income is not included in the Service Pension income test.

Figure B.1 shows the number of male DP recipients (who were born in 1945-52) between the years 1990 and 2009, at June of each year.[18] The number of DP recipients more than tripled, from 10,691 at June 1990 to 32,550 at June 2009. The increase in DP-SR is far more striking. At June 1990, only 798 received the DP-SR; by June 2009, there were 17,806 recipients, a more than 22-fold increase. As a share of all DP recipients, DP-SR recipients increased from 7% in 1990 to 55% in 2009.

Table B.1 shows the main health conditions associated with DP receipt. It includes the top 10 ICD-10 conditions (most frequent accepted claims in 1990-2009), which account for half of all accepted claims. The upper panel shows the number of successful claims in five-year periods by condition and overall. More than 80% of all accepted claims were lodged after 1989. The number of accepted claims increased sharply over the 1990s and peaked in the early 2000s before declining. The single most prevalent condition is Post Traumatic Stress Disorder (PTSD), for which 17,399 successful claims were lodged. Of these, only 380 were lodged prior to 1990, reflecting the emergence of PTSD as a recognized psychological condition. The next three

[18] Men with a 'zero rated' DP are excluded. These men do not receive DP, but receive subsidised medical care for a non-service related disability.

conditions are hearing-related, together accounting for 32,289 accepted claims, and thus far exceeding PTSD. Only 9% of such claims were lodged prior to 1990. These first four conditions account for almost one third of accepted DP claims. The number of successful claims for all of the other top-10 conditions also grew markedly during the 1990s. 'Mental and Behavioural disorders due to Alcohol Dependence' was the third most prevalent condition amongst all accepted claims during 1995-9.

The middle panel of Table B.1 shows the proportions of DP recipients who received compensation for each of the top-ten conditions at various points in time. These do not sum to one, because many men receive compensation for more than one condition. The profile of DP recipients changed considerably over this period. By the early 2000s, around half of all DP recipients were receiving compensation for PTSD, about the same as for Sensorineural Hearing Loss. By the late 2000s, more than one quarter were receiving compensation for Tinnitus.

The lower panel of Table B.1 is similar to the middle panel, but is restricted to Special Rate DP recipients. It shows the proportions of DP-SR recipients who received compensation for each of the top-ten conditions. The two standout conditions are PTSD and Sensorineural hearing loss. In 1999 and later, almost three quarters of DP-SR recipients were receiving compensation for PTSD. By the mid-2000s, around two-thirds were receiving compensation for Sensorineural hearing loss.[19] The third and fourth ranked conditions in 2009 were Mental and Behavioural disorders due to Alcohol Dependence (24%) and Tinnitus (22%).

[19] The data show that before 2002, there were almost no claims for Sensorineural Hearing loss – *unspecified*. In contrast, from 2002 onwards, there were almost no claims for Sensorineural Hearing loss – *bilateral*. This appears to be a change in coding or diagnostic practice, and thus it is probably unlikely that men receive compensation for both of these conditions.

Why has DP-SR receipt increased so much since 1990? There is no single outstanding reason for this, but there are many factors that may have contributed. These include:

- *Aging of the Vietnam veteran cohort.* Aging exacerbates chronic conditions. Disability pensions also may become more attractive for men as they approach retirement. Figure 2 suggests that aging is an important factor contributing to the growth in DP-SR.

- *Standard of proof.* Since 1977, DVA must grant DP claims for veterans with 'operational service' (which equates to service in Vietnam in the present context) unless it proves beyond reasonable doubt that an individual's condition is not service related. This is effectively a 'reverse criminal' standard of proof. Whilst this change preceded the observed increase in DP-SR by over a decade, it may be a factor in the magnitude of its growth.

- *Other changes to veterans' benefits law.* Changes to the interpretation of veterans' compensation law are frequent and complex and a full investigation is beyond the scope of this paper. The most recent review of the veterans' benefits systems (Clarke et al. 2003)[20] highlights the possible roles of two specific legal cases, both in 1981. The *Bowman (1981)* case feasibly increased the scope of claims linking war-related impairment to employability (Lloyd and Rees 1994: 391). The *Law (1981)* case 'effectively conceded smoking in war-time as a causative element in entitlement' (Lloyd and Rees 1994: 358). The introduction of 'Statements of Principles' in 1994 may have also been a factor. These greatly clarified the circumstances in which DP claims would be accepted. It is possible that this led to claims that otherwise would not have been lodged.

- *PTSD.* Post Traumatic Stress Disorder emerged as a recognised psychological illness in the early 1980s. Between 1995 and 1999, it was the single largest illness category of

[20] Another review is due to be completed in late 2010 or 2011.

successful DP claims. Also since 1995, a majority of DP-SR recipients have been receiving compensation for PTSD. However, even in the peak period of 1995-9, PTSD accounts only for 19% of successful DP claims.

- *Cultural change*. The DVA (and the government more broadly) is known to exhibit a culture of gratitude towards veterans. But this was not always the case with respect to Vietnam veterans. In Australia, as in the US, public attitudes towards Vietnam veterans were not sympathetic in the latter stages of the war and for some time afterwards. This is most clearly demonstrated by the fact that the symbolic Vietnam Veterans Welcome Home Parade was not held until 1987. Similarly, relations between Vietnam veterans associations and the DVA were not always cordial in the early years (Ham 2007). The role of the Vietnam veterans association as a powerful and organized special interest group also may have grown over this time.

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**Table 1 Descriptive Statistics**

| | Ballot outcome | | |
|---|---|---|---|
| | In | Out | All |
| A. First Stage Database | | | |
| Army service in Vietnam Era (mean) | 0.271 | 0.032 | 0.094 |
| Army service in Vietnam (mean) | 0.088 | 0.015 | 0.034 |
| Days in Vietnam (mean for Vietnam Army servicemen) | 269 | 249 | 261 |
| Army Servicemen died in Vietnam (n) | 218 | 124 | 342 |
| N | 224,706 | 643,899 | 868,605 |
| B. Census 2006 | | | |
| Employed (mean) | 0.690 | 0.729 | 0.719 |
| Disability (mean) | 0.060 | 0.048 | 0.051 |
| Post-School qualifications (mean) | 0.483 | 0.484 | 0.484 |
| Tertiary qualifications (mean) | 0.151 | 0.152 | 0.152 |
| N | 175,348 | 500,484 | 675,832 |
| C. Personal Income Tax Return Database | | | |
| *2008-09* | | | |
| Employee (mean) | 0.429 | 0.468 | 0.458 |
| Log Earnings (mean) | 10.44 | 10.47 | 10.47 |
| N | 165,461 | 486,715 | 652,176 |
| *1992-93* | | | |
| Employee (mean) | 0.679 | 0.678 | 0.678 |
| Log Earnings (mean) | 10.24 | 10.24 | 10.24 |
| N | 174,758 | 511,363 | 686,121 |

**Table 2 First Stage Estimates**

| | Army service (r) | | Army service in Vietnam (v) | |
|---|---|---|---|---|
| | A. <u>One ballot outcome IV</u> | | | |
| z | 0.2349*** | (0.0010) | 0.0715*** | (0.0006) |
| | B. <u>Full set of 16 ballot outcome IVs</u> | | | |
| z1 | 0.3267*** | (0.0033) | 0.1198*** | (0.0024) |
| z2 | 0.3097*** | (0.0038) | 0.1183*** | (0.0028) |
| z3 | 0.2811*** | (0.0043) | 0.1179*** | (0.0032) |
| z4 | 0.2826*** | (0.0043) | 0.1382*** | (0.0034) |
| z5 | 0.2649*** | (0.0040) | 0.1262*** | (0.0031) |
| z6 | 0.2570*** | (0.0044) | 0.1350*** | (0.0035) |
| z7 | 0.2662*** | (0.0041) | 0.1233*** | (0.0031) |
| z8 | 0.2782*** | (0.0047) | 0.1111*** | (0.0034) |
| z9 | 0.2576*** | (0.0039) | 0.0872*** | (0.0026) |
| z10 | 0.2639*** | (0.0049) | 0.0605*** | (0.0029) |
| z11 | 0.2487*** | (0.0048) | 0.0318*** | (0.0022) |
| z12 | 0.2350*** | (0.0040) | 0.0061*** | (0.0012) |
| z13 | 0.2107*** | (0.0033) | 0.0005 | (0.0008) |
| z14 | 0.1899*** | (0.0034) | 0.0008 | (0.0007) |
| z15 | 0.1275*** | (0.0030) | -0.0013** | (0.0004) |
| z16 | 0.0011 | (0.0015) | -0.0002 | (0.0001) |
| F-stat for IVs | 8,485 | | 2,709 | |

This table reports estimated effects of ballot outcome on the probability of army service and army service in Vietnam, respectively. All regressions include a constant and 15 cohort dummies. Robust standard errors are in parentheses. N = 868,606 in each regression. z1-z16 represent the effect of ballot outcome for cohorts 1-16, where cohort 1 is the oldest. The slight variation in these results from Siminski and Ville (2011-forthcoming) is due to the exclusion of men born on the 15th of each month in the latter. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Table 3 2SLS Estimates of Employment Effects (Census 2006)**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | A. <u>One ballot outcome IV (z)</u> | | | |
| r | -0.111*** | | | | |
| | (0.005) | | | | |
| v | | -0.365*** | | | |
| | | (0.018) | | | |
| | | B. <u>Full set of 16 ballot outcome IVs (z1-16)</u> | | | |
| r | -0.120*** | | -0.002 | 0.001 | -0.000 |
| | (0.005) | | (0.010) | (0.010) | (0.011) |
| v | | -0.366*** | -0.361*** | -0.341*** | -0.312*** |
| | | (0.015) | (0.029) | (0.030) | (0.060) |
| vp | | | | -0.083** | -0.078* |
| | | | | (0.032) | (0.034) |
| vx | | | | | -0.038 |
| | | | | | (0.071) |
| Overidentification test statistic | 32.18 | 3.36 | 3.35 | 1.78 | 1.72 |
| | p=0.006 | p=0.999 | p=0.998 | p>0.999 | p>0.999 |

Notes: This table shows estimates from linear regressions of employment on cross-sample fitted values of r and v from the 1st stage. A constant and 15 cohort dummies are also included in each regression. The overidentification test is the grouped data test (Angrist and Pischke 2009, pp143-144). The test statistic has a $\chi^2$ distribution with degrees of freedom equal to the number of IVs minus the number of endogenous regressors, in this case 15, 15, 14, 13 and 12 degrees of freedom, respectively. N = 675,832 in each regression. Robust standard errors are in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Table 4 Reduced Form Employment and Disability Pension Effects (1990-2009)**

| Year | Employment Effect | | Disability Pension (SR) Effect | |
|---|---|---|---|---|
| 1990 | | | 0.002*** | (0.0001) |
| 1993 | 0.004** | (0.0014) | 0.004*** | (0.0002) |
| 1996 | 0.001 | (0.0013) | 0.009*** | (0.0003) |
| 1999 | -0.004** | (0.0014) | 0.018*** | (0.0004) |
| 2002 | -0.011*** | (0.0014) | 0.030*** | (0.0005) |
| 2006 | -0.018*** | (0.0015) | 0.042*** | (0.0006) |
| 2009 | -0.020*** | (0.0015) | 0.047*** | (0.0006) |

Notes: Employment effects are estimated using the personal income tax return database. The Last 5 cohorts are excluded from all years. 1993 represents the 1992-93 tax year, similarly for other years. Earlier data are not presently available. Men with positive earnings in a given tax year are counted as employed. Disability Pension (SR) receipt is at June of each year. Robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Table B.1 DVA Disability Pension Receipt – Top 10 conditions**

**Number of Accepted claims**

| ICD 10 | ICD Description | pre-1990 | 1990-94 | 1995-99 | 2000-04 | 2005-09 | Total |
|---|---|---|---|---|---|---|---|
| F43.1 | Post Traumatic Stress Disorder | 380 | 1945 | 7063 | 5595 | 2416 | 17399 |
| H90.3 | Sensorineural Hearing loss - bilateral | 2767 | 2428 | 6325 | 3246 | 28 | 14794 |
| H93.1 | Tinnitus | 115 | 305 | 1772 | 3431 | 3405 | 9028 |
| H90.5 | Sensorineural Hearing loss - unspecified | 65 | 37 | 16 | 4068 | 4281 | 8467 |
| M47.86 | Sponylosis, Other | 652 | 548 | 936 | 1372 | 2276 | 5784 |
| F10.2 | Mental and Behavioural disorders due to Alcohol Dependence | 34 | 254 | 2301 | 2004 | 1170 | 5763 |
| K58.9 | Irritable Bowel Syndrome | 315 | 379 | 1303 | 1153 | 343 | 3493 |
| M17.9 | Gonarthrosis, unspecified | 351 | 333 | 586 | 887 | 1599 | 3756 |
| K21.9 | Gastro-oesophageal reflux disease without oesophagitis | 46 | 122 | 1118 | 1274 | 524 | 3084 |
| I10 | Essential (primary) hypertension | 199 | 185 | 730 | 885 | 663 | 2662 |
| | *Total Top 10 conditions* | *4924* | *6536* | *22150* | *23915* | *16705* | *74230* |
| | Other | 22544 | 8455 | 15357 | 17704 | 13905 | 77965 |
| | *Total* | *27468* | *14991* | *37507* | *41619* | *30610* | *152195* |

**Share of DP recipients receiving compensation for each condition at June**

| ICD 10 | ICD Description | 1990 | 1994 | 1999 | 2004 | 2009 |
|---|---|---|---|---|---|---|
| F43.1 | Post Traumatic Stress Disorder | 0.051 | 0.159 | 0.417 | 0.494 | 0.504 |
| H90.3 | Sensorineural Hearing loss - bilateral | 0.279 | 0.347 | 0.505 | 0.480 | 0.421 |
| H93.1 | Tinnitus | 0.011 | 0.026 | 0.095 | 0.185 | 0.266 |
| H90.5 | Sensorineural Hearing loss - unspecified | 0.006 | 0.007 | 0.005 | 0.141 | 0.253 |
| M47.86 | Sponylosis, Other | 0.067 | 0.081 | 0.094 | 0.115 | 0.170 |
| F10.2 | Mental and Behavioural disorders due to Alcohol Dependence | 0.004 | 0.019 | 0.115 | 0.151 | 0.166 |
| K58.9 | Irritable Bowel Syndrome | 0.031 | 0.047 | 0.089 | 0.104 | 0.102 |
| M17.9 | Gonarthrosis, unspecified | 0.035 | 0.043 | 0.049 | 0.061 | 0.095 |
| K21.9 | Gastro-oesophageal reflux disease without oesophagitis | 0.005 | 0.011 | 0.057 | 0.085 | 0.090 |
| I10 | Essential (primary) hypertension | 0.019 | 0.026 | 0.049 | 0.065 | 0.077 |
| | number of DP recipients | 10,691 | 14,481 | 22,112 | 29,402 | 32,550 |

**Share of DP (Special Rate) recipients receiving compensation for each condition at June**

| ICD 10 | ICD Description | 1990 | 1994 | 1999 | 2004 | 2009 |
|---|---|---|---|---|---|---|
| F43.1 | Post Traumatic Stress Disorder | 0.218 | 0.451 | 0.737 | 0.762 | 0.759 |
| H90.3 | Sensorineural Hearing loss - bilateral | 0.237 | 0.397 | 0.583 | 0.569 | 0.518 |
| H93.1 | Tinnitus | 0.010 | 0.035 | 0.123 | 0.178 | 0.219 |
| H90.5 | Sensorineural Hearing loss - unspecified | 0.011 | 0.008 | 0.006 | 0.079 | 0.153 |
| M47.86 | Sponylosis, Other | 0.068 | 0.085 | 0.116 | 0.121 | 0.154 |
| F10.2 | Mental and Behavioural disorders due to Alcohol Dependence | 0.010 | 0.049 | 0.200 | 0.229 | 0.243 |
| K58.9 | Irritable Bowel Syndrome | 0.070 | 0.111 | 0.158 | 0.161 | 0.153 |
| M17.9 | Gonarthrosis, unspecified | 0.024 | 0.033 | 0.048 | 0.054 | 0.071 |
| K21.9 | Gastro-oesophageal reflux disease without oesophagitis | 0.013 | 0.026 | 0.097 | 0.115 | 0.120 |
| I10 | Essential (primary) hypertension | 0.054 | 0.065 | 0.086 | 0.095 | 0.105 |
| | number of DP-SR recipients | 798 | 2,411 | 7,736 | 14,861 | 17,806 |

Source: Unpublished data provided by the Department of Veterans' Affairs

**Figure 1 2SLS Employment Effects by birth cohort (Census 2006)**

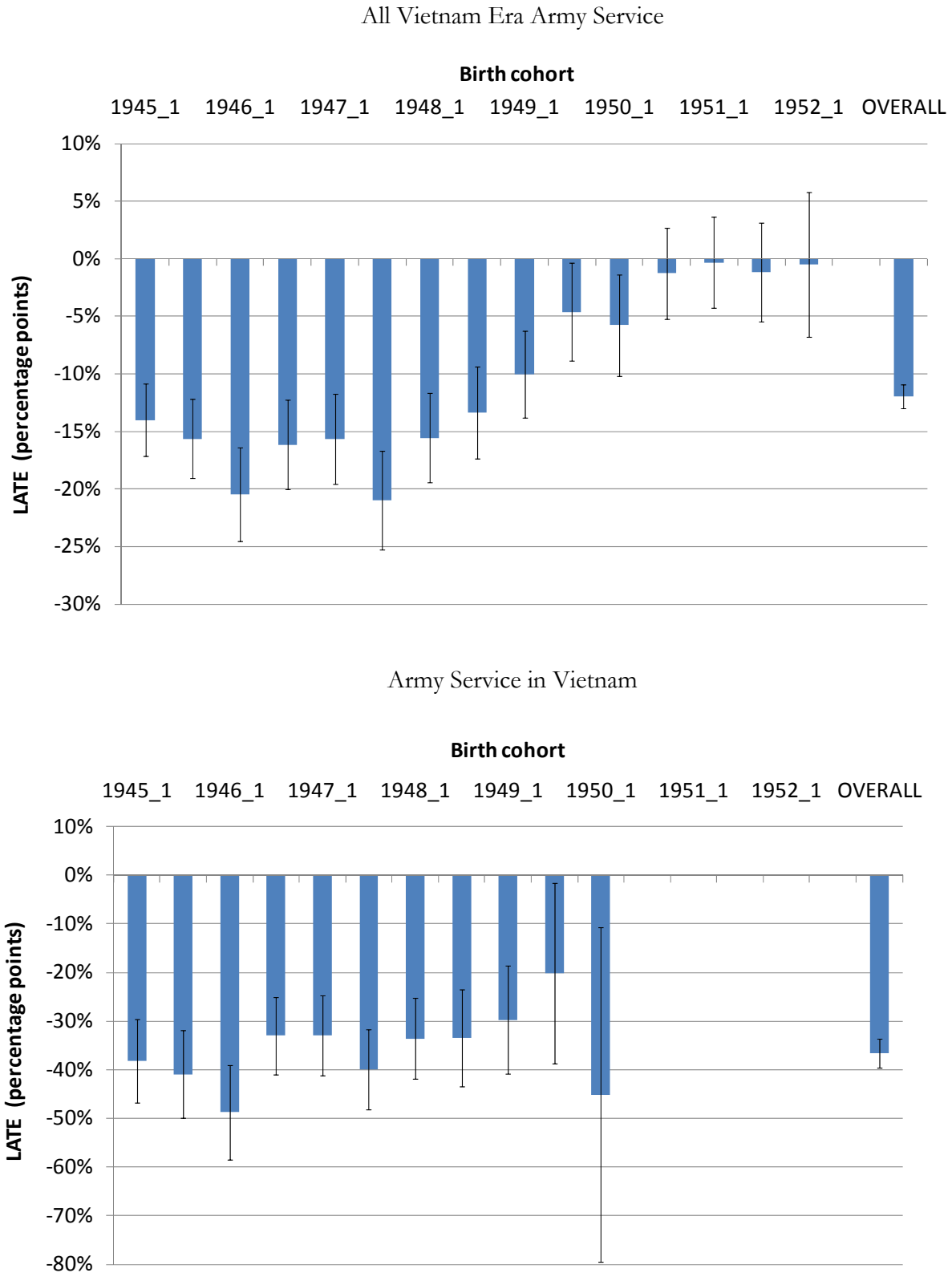
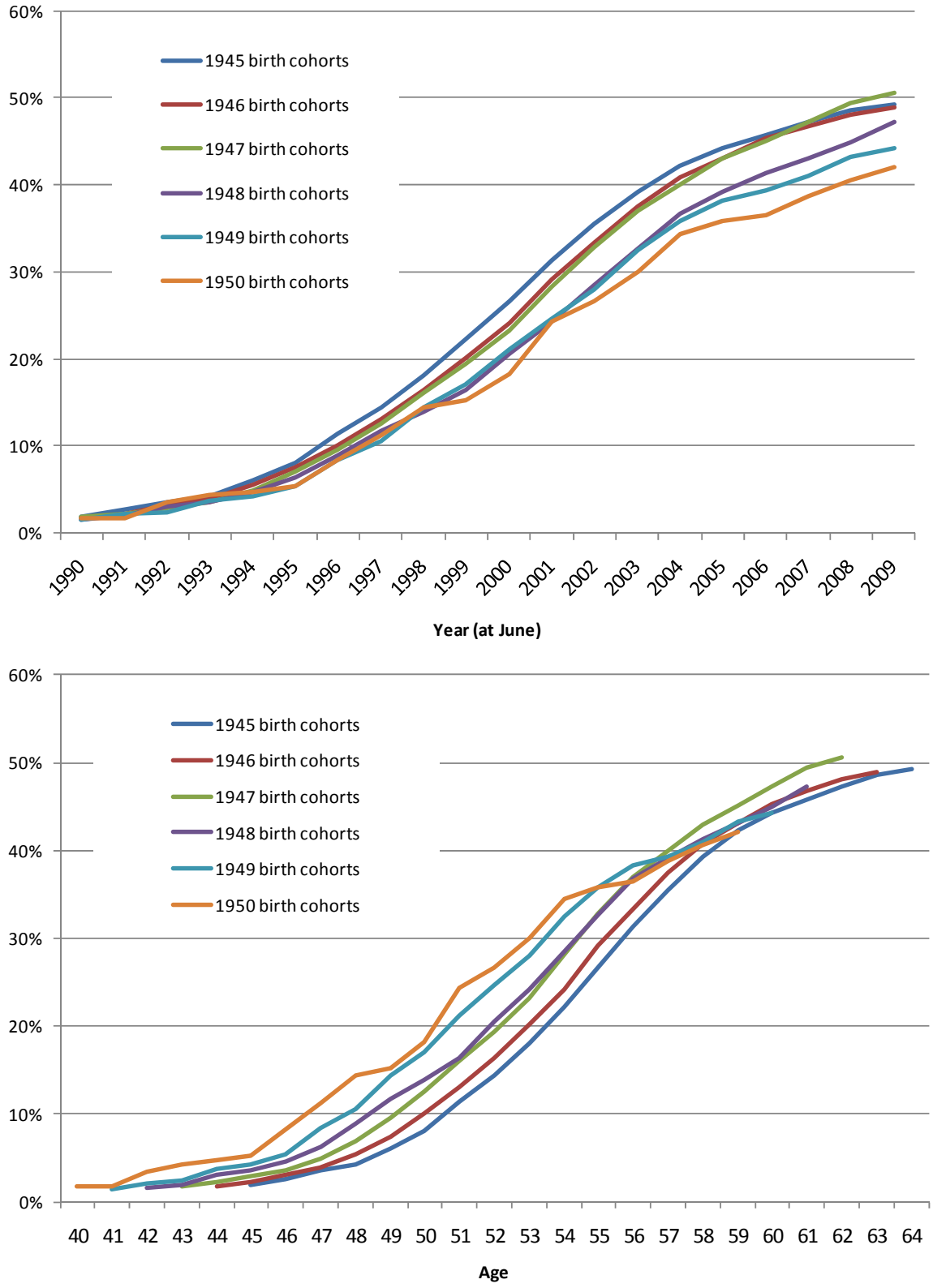
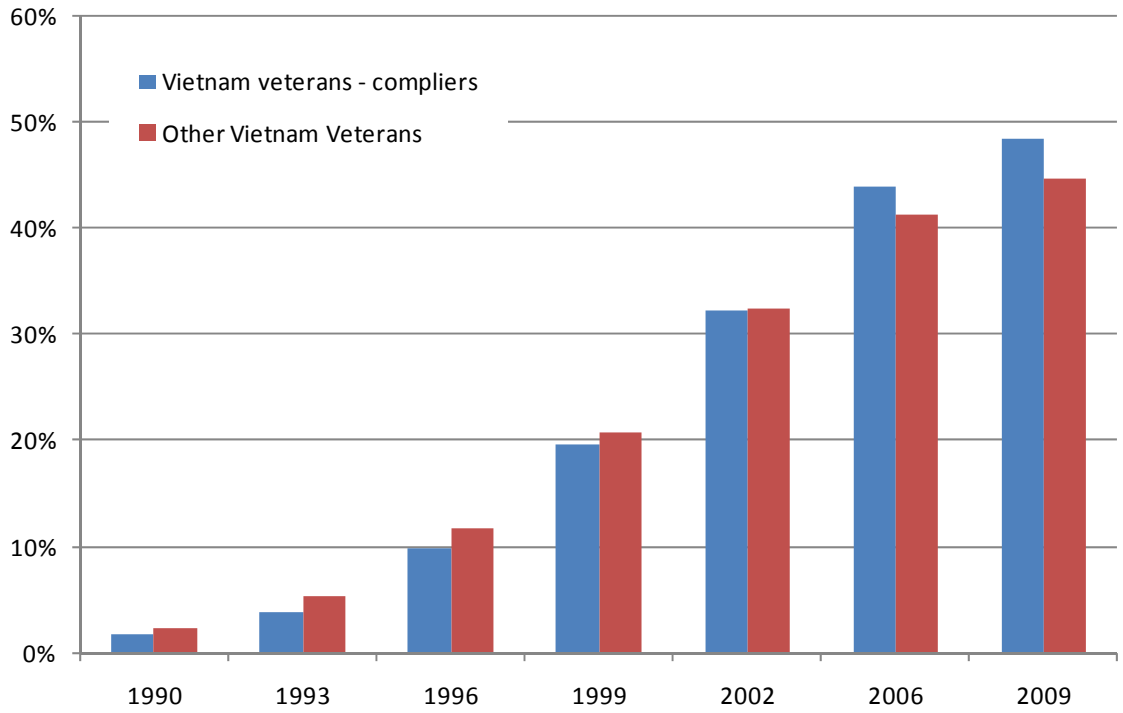


**Figure 2 Share of Vietnam Veteran 'Compliers' receiving Disability Pension (Special Rate) by Cohort, Age and Year**



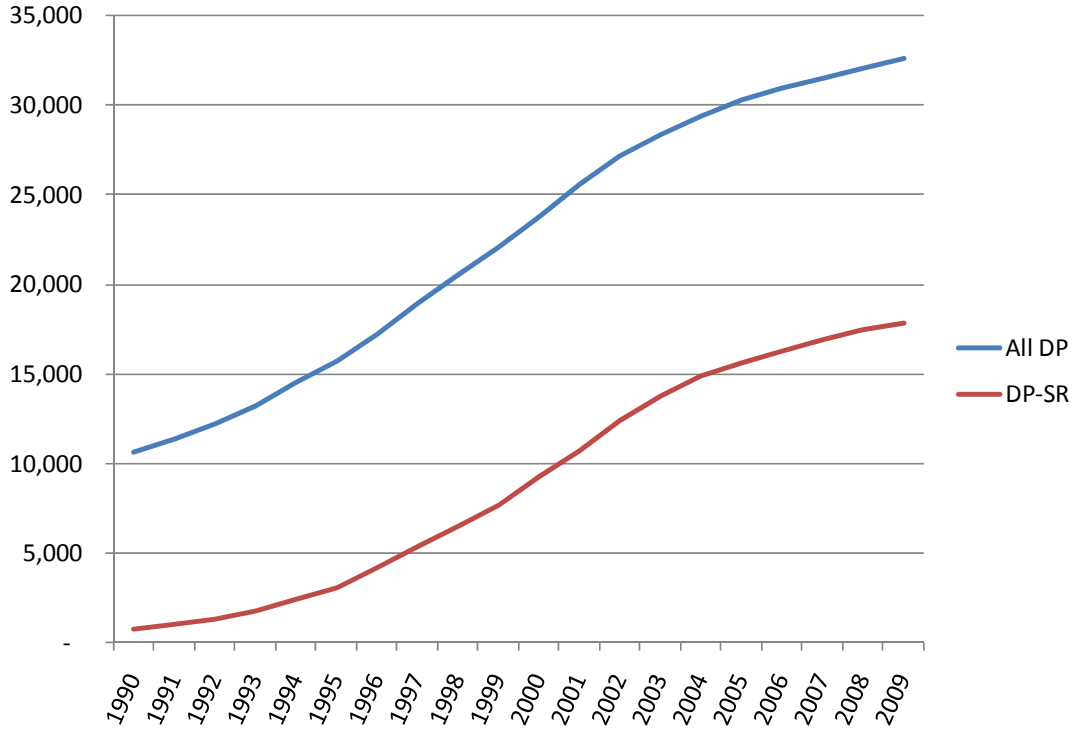

Notes: Population restricted to men born between 1 Jan 1945 and 30 Jun 1950. Adjustments are not made for mortality, so all proportions are underestimates.

**Figure 3 Share of Vietnam Veterans Receiving DVA Disability Pension (Special Rate)**




Notes: Population restricted to those born between 1 Jan 1945 and 30 Jun 1950. Adjustments are not made for mortality, so all proportions are underestimates.

**Figure B.1 Number of Male DVA Disability Pension recipients (born 1945-1952) at June, 1990-2009**




Source: Unpublished data provided by the Department of Veterans' Affairs