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EMPLOYMENT EFFECTS OF ARMY SERVICE AND VETERANS' COMPENSATION: EVIDENCE FROM THE AUSTRALIAN VIETNAM-ERA CONSCRIPTION LOTTERIES

Peter Siminski*

Abstract—Exploiting Australia's National Service lotteries of 1965 to 1972, 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 complying conscripts. The estimated employment effect is −12 percentage points (95% CI: −13, −11) overall, −37 for those who served in Vietnam and 0 for those who served only in Australia. It emerged in the 1990s, mirrored by veterans' disability pension effects. These results contrast with those for the United States, possibly reflecting employment disincentives associated with Australia's veterans' compensation system.

I. Introduction

THE human and financial costs of war can be large and are hence of considerable policy importance (Bedard & Olivier, 2006; Stiglitz & 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 Angrist, Chen, & Frendsen, 2010; Conley & Heerwig, 2009; Dobkin & Shabani, 2009; Hearst, Newman, & Hulley, 1986). Other studies have considered effects on human capital and earnings (Angrist, 1990, 1993; Angrist & Chen, 2011).

The direct effects of military service may be accompanied by indirect effects, through the channel of government programs for veterans. As the majority of Vietnam veterans approach retirement age, attention has shifted recently to the potential work disincentives associated with veterans' compensation (VC) (Angrist et al., 2010; Autor & Duggan, 2007; Autor, Duggan, & Lyle, 2011; Duggan, Rosenheck, & Singleton, 2010). In particular, the individual unemployability (IU) aspect of VC seems likely to act as a work disincentive (Angrist et al., 2010; Autor & Duggan, 2008).[1] However, there is little evidence of significant causal employment effects (Angrist et al., 2010; Angrist, Chen, & Song, 2011; Duggan et al., 2010), except among low-skill veterans (Angrist et al., 2010). Perhaps the work disincentives in the U.S. VC system are not as substantial as they appear.

In this paper, I contribute to this literature by estimating employment effects for Australian Vietnam veterans, for whom compensation rates are much more fundamentally tied to employability. The Australian veterans' compensation system is similar in some ways to the U.S. system. Compensation includes cash payments and enhanced health insurance. These payments are notionally tied to the level of incapacity (on a scale from 0 to 100%). However, veterans who are determined to be 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] This is not the case in the United States, where the VC payment rate for veterans with an IU determination is no greater than the highest rate for those who can work. In both countries, veterans can combine this compensation with income support. In Australia, the implicit (net) replacement rate of DP-SR plus income support is at least 64% (69%) for a coupled (single) veteran with average earnings and existing eligibility for the 100% general rate of compensation. Once granted, the payments continue until death. By 2009, about half of Australian Vietnam veterans were receiving DP-SR, almost all of whom began receiving it 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 system; see also Ville & Siminski, 2011). For research purposes, these lotteries have several advantages over the corresponding U.S. lotteries. First, the effect of service in Vietnam can be differentiated from service confined to Australia due to large differences between cohorts in the probability of Vietnam service. In particular, nobody from the final four cohorts served in Vietnam. Second, there was no GI Bill in Australia, and little or no effect of ballot outcome on educational attainment. This simplifies the interpretation of the results.

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[1] See Angrist et al. (2010) and Autor and Duggan (2008) for details of the U.S. VC system, including individual unemployability.

[2] The DVA disability pension is the Australian equivalent of U.S. disability compensation. The DP-SR is received almost exclusively by veterans who are determined to be totally and permanently incapacitated. These 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.

Third, the dates of birth (DOBs) drawn were not published until 1997, so there is no risk of discrimination by employers on the basis of ballot eligibility. Fourth, draft eligibility was determined immediately at the time of a ballot drawing. In the United States, there were long delays between assignment of random sequence numbers and draft eligibility determination, potentially influencing behavior in the interim, either to avoid the draft or in anticipation of being drafted. Fifth, Australia's casualty rates in Vietnam were considerably lower than those for the United States. This reduces the threat of bias due to attrition. It also reduces the likely magnitude of a health mechanism in the effect of army service on employment. Finally, I am able to show evidence of some external validity beyond the set of compliers who identify the estimates. (For discussions of the limitations of the U.S. lotteries, see Angrist, 1990; Angrist, Imbens, & Rubin, 1996; Angrist & Imbens, 1997; Heckman, 1997; Keane, 2010; Rosenzweig & Wolpin, 2000.)

Apart from our own work on mortality (Siminski & Ville, 2011, 2012), no previous studies have used the Australian conscription lotteries for identification. This is probably due to a paucity of data. However, in 2006, DOB's were 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. As pioneered by Angrist (1990), I use a two-sample IV approach and thus have no need to link the data sources. In practice, however, I use the computationally simpler two-sample 2SLS procedure (Inoue & Solon, 2010).

I find a very large employment effect (−12 percentage points) in 2006, when most compliers were in their late 50s. However, this is completely driven by conscripts who served in Vietnam, for whom the effect is precisely estimated at −37 percentage points. The effect is relatively recent, emerging gradually since the mid-1990s. I also find a corresponding trend in disability pension effects that 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 the appendix, I discuss the validity of the exclusion restriction. Appendix B (online) provides a descriptive summary of trends in the DVA disability pension.

II. Estimation Strategy

The primary aim 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).[3] These binary indicators (r and v) are not mutually exclusive. In particular, $v = 1$ implies $r = 1$. In the main specification, β_r is the effect of army service in Australia, and β_v is the additional effect of serving in Vietnam:

$$y_i = \alpha + \beta_r r_i + \beta_v v_i + \gamma' C_i + \mu_i. \tag{1}$$

C is a vector of fifteen 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 nonrandom, so v is likely to be correlated with μ. Thus, OLS estimation of equation (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 plausibly valid instrument. However, the effects of ballot outcome vary by cohort, so a more efficient strategy is to interact z with C. Let Z be a vector of sixteen binary instruments, representing the ballot outcome in each cohort, respectively.[4] The two first-stage regressions are identified by the exclusion of Z from equation (1), along with considerable differences between cohorts in the proportion of conscripts sent to Vietnam. The first-stage regressions are

$$r_i = \pi_{r0} + \pi_{r1}{}' Z_i + \pi_{r2}{}' C_i + \varepsilon_{ri}, \tag{2}$$

$$v_i = \pi_{v0} + \pi_{v1}{}' Z_i + \pi_{v2}{}' C_i + \varepsilon_{vi}. \tag{3}$$

Using a two-sample 2SLS procedure (Inoue & Solon, 2010), I estimate the first-stage regressions (2) and (3) by OLS with one data set and the second-stage regression, equation (1), with a different data set by OLS, after replacing r and v with the fitted values from equations (2) and (3). That is, I use cross-sample fitted values of r and v. 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). In general, compliers cannot be individually identified (Angrist et al.,

[3] National service was in the army. In my preliminary analysis, I 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.

[4] The term *ballot outcome* refers to the outcome of the main conscription ballots. $z = 1$ for men "balloted-in" and 0 for men "balloted-out". A man is considered to be balloted-in if his DOB was drawn in the ballot held for his birth cohort. A separate set of ballots was 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.

1996). However, in this case, balloted-in army compliers are a majority subset (84%) of national servicemen.[5]

I also show the results from specifications that include only one of the endogenous variables, including estimates for each individual cohort using a set of just-identified models. In such models, the coefficients of r and v have different interpretations, discussed in the section IV. The analysis is also repeated for alternate dependent variables, including employment in other years and receipt of the veterans' disability pension.

The model is then extended by interacting v with p, which represents age eligibility for a veterans' age pension ($p = 0$ if age < 60 years, 1 otherwise).[6] Subject to a means test, veterans with qualifying service (which essentially equates with service in Vietnam for the study population) are eligible for this pension from 60 years of age. Eligibility for the otherwise equivalent social security age pension commences at the age of 65. Age pension eligibility may encourage early retirement due to both the income and substitution effects associated with pension means tests (Autor & Duggan, 2007), and so β_p is hypothesized to be negative:

$$y_i = \alpha + \beta_r r_i + \beta_v v_i + \beta_p v_i p_i + \gamma' C_i + \mu_i. \tag{4}$$

Finally, I add a proxy for combat intensity (x), based on within-cohort ratios of deaths in Vietnam to counts of men who served there (rescaled to have a mean of 0 and a range of 1). Since this is relevant only for those who served in Vietnam, it is interacted with v:[7]

$$y_i = \alpha + \beta_r r_i + \beta_v v_i + \beta_p v_i p_i + \beta_x v_i x_i + \gamma' C_i + \mu_i \tag{5}$$

β_x represents the additional effect on employment of service in Vietnam for men whose cohort experienced the greatest combat intensity, relative to the cohort with the lowest combat intensity.

I hypothesize that combat intensity will be associated with detrimental employment effects, and thus β_x should be negative. However, I have limited confidence in the reliability of this proxy, as discussed in the sections that follow.

The exclusion of z from the employment equation is valid if its only effect on employment outcomes is through inducement of army service. In the appendix, I consider several hypothesized violations of this condition in relation to draft-avoidance behavior 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 between 1945 and 1952 who were Australian residents when they turned 20 years old. 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 DOBs.

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 era but not in Vietnam. The VED has been used and refined by the AIHW and DVA for various health and mortality studies for nearly three decades. The VED version used in this study was also used by Wilson, Horsley, and van der Hoek (2005). While 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, p. 98). The estimated number of compliers and their distribution between cohorts (see the first-stage results in section IV) conform to prior expectations that are based on summaries of the National Service scheme (Department of Labour and National Service, various years; Langford, 1997).

For men who did not serve in the army ($r = 0$), records were synthesized using published population data (Australian Bureau of Statistics, 2008).[8]

[5] *National servicemen* are essentially the set of conscripts (although some men volunteered for national service). They are often identifiable in administrative data. They constitute a minority (30.9%) of Australia's military personnel who served in Vietnam, or 44.6% of army personnel who served in Vietnam. Unlike in the United States, 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, later 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 estimates.

[6] Officially known as *service pension (age)*. I use the term *veterans' age pension* since it is virtually identical to the civilian age pension and to avoid confusion with *service pension (invalidity)*, which is distinct.

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

[8] 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 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.

Table 1.—Descriptive Statistics

	Ballot outcome		
	In	Out	All
First-stage database			
Army service in Vietnam era (mean)	.271	.032	.094
Army service in Vietnam (mean)	.088	.015	.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
Census 2006			
Employed (mean)	.690	.729	.719
Disability (mean)	.060	.048	.051
Postschool qualifications (mean)	.483	.484	.484
Tertiary qualifications (mean)	.151	.152	.152
N	175,348	500,484	675,832
Personal income tax return database			
2008–2009			
Employed (mean)	.476	.499	.493
Log earnings (mean)	10.53	10.55	10.55
N	250,012	722,263	972,275
1992–1993			
Employed (mean)	.683	.680	.681
Log earnings (mean)	10.23	10.23	10.23
N	261,135	751,777	1,012,912

The upper panel of table 1 shows descriptive statistics from 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. Under the two-sample 2SLS procedure, it was not necessary to link records between these databases or to the first-stage database.

Census 2006. In 2006 for the first time, the Census of Population and Housing collected DOBs from all respondents. The estimation population is the set of men born from 1945 to 1952, 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 10%), and Census undercount (approximately 2%). The remaining 1% 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 among 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 nonveterans (Wilson et al., 2005) implies a similar number of (additional) postservice excess deaths. Exploiting the conscription lotteries, Siminski and Ville (2011) find no evidence of significantly elevated postservice mortality (1994–2007) associated with army service.

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 among balloted-in men. There is little difference in educational attainment by ballot outcome.

Personal income tax data, 1993–2009. A supplementary second-stage database was constructed from the full set of personal income tax return data for 1992–1993, 1995–1996, 1999–2000, 2002–2003, 2005–2006 and 2008–2009. Not all residents submit tax returns (Leigh, 2005), but almost all employees do. Employees who had any tax with-

TABLE 2.—FIRST-STAGE AND REDUCED-FORM ESTIMATES

	Army Service (r)		Army Service in Vietnam (v)		Employed in 2006 (Census) (y)	
One ballot outcome IV						
Ballot outcome	.2349***	(.0010)	.0715***	(.0006)	−.0261***	(.0013)
Full set of 16 ballot outcome IVs						
Ballot outcome, cohort 1	.3267***	(.0033)	.1198***	(.0024)	−.0457***	(.0052)
Ballot outcome, cohort 2	.3097***	(.0038)	.1183***	(.0028)	−.0484***	(.0055)
Ballot outcome, cohort 3	.2811***	(.0043)	.1179***	(.0032)	−.0575***	(.0058)
Ballot outcome, cohort 4	.2826***	(.0043)	.1382***	(.0034)	−.0456***	(.0056)
Ballot outcome, cohort 5	.2649***	(.0040)	.1262***	(.0031)	−.0415***	(.0053)
Ballot outcome, cohort 6	.2570***	(.0044)	.1350***	(.0035)	−.0539***	(.0057)
Ballot outcome, cohort 7	.2662***	(.0041)	.1233***	(.0031)	−.0414***	(.0053)
Ballot outcome, cohort 8	.2782***	(.0047)	.1111***	(.0034)	−.0372***	(.0057)
Ballot outcome, cohort 9	.2576***	(.0039)	.0872***	(.0026)	−.0259***	(.0050)
Ballot outcome, cohort 10	.2639***	(.0049)	.0605***	(.0029)	−.0122*	(.0057)
Ballot outcome, cohort 11	.2487***	(.0048)	.0318***	(.0022)	−.0143*	(.0056)
Ballot outcome, cohort 12	.2350***	(.0040)	.0061***	(.0012)	−.0030	(.0047)
Ballot outcome, cohort 13	.2107***	(.0033)	.0005	(.0008)	−.0007	(.0042)
Ballot outcome, cohort 14	.1899***	(.0034)	.0008	(.0007)	−.0022	(.0042)
Ballot outcome, cohort 15	.1275***	(.0030)	−.0013**	(.0004)	−.0006	(.0041)
Ballot outcome, cohort 16	.0011	(.0015)	−.0002	(.0001)	−.0003	(.0042)
F-statistics for IVs	3,973		927			
Angrist-Pischke multivariate *F*-statistic	1,781		931			
N	868,606		868,606		675,832	

This table reports estimated effects of ballot outcome on the probability of army service, army service in Vietnam, and employment in 2006, respectively. All regressions also include a constant and fifteen cohort dummies. Robust standard errors are in parentheses. Cohort 1 is the oldest six-month birth cohort. The slight variation in these first-stage results from Siminski and Ville (2011) is due to the exclusion of men born on the fifteenth of each month in Siminski and Ville (2011). $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

held 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.[9]

Nonresidents are excluded. Preliminary analysis revealed slightly elevated counts of men with DOBs listed as January 1, July 1, or October 10 in each year, and they are also excluded. When used to estimate employment effects, the five youngest cohorts are also excluded 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. Since recent migrants cannot be identified or hence excluded from this database, the first-stage database is adjusted accordingly when 2SLS effects are calculated using tax data.

The bottom panel of table 1 presents summary statistics from the first and last years of these data. The share of men with positive earnings differs little by ballot outcome in 1992–1993, but it is 2.3 percentage points lower for balloted-in men in 2008–2009. Mean log earnings in 1992–1993 are 10.23 regardless of ballot outcome. In 2008–2009, mean log earnings are slightly higher among balloted-out men.

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 (online) 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-statistics of 245 and 117, respectively. Similarly, the IVs are strongly significant in the overidentified models (lower panel of table 2) as evidenced by the large F-statistics. The coefficient of ballot outcome for the sixteenth cohort is close to 0 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 sixteenth cohort were due to be called up. For cohorts 13 to 15, ballot outcome is a strong predictor of r but not of v. Similarly, for cohorts 9 to 11, ballot outcome is a much stronger predictor of r than v relative to the first eight cohorts. This is useful because it allows me to differentiate the effect of service in Australia from the effect of 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

[9] For example, 642,253 men born between 1945 and 1952 reported positive wages and salaries in 2001–2002 on their tax returns, compared to the 631,358 men who worked for wages or salaries as estimated from HILDA data. For 2005–2006, the counts are 576,477 (tax return data) and 531,237 (HILDA).

are a subset of national servicemen, because there was no apparent incentive for balloted-in men to join the regular army rather than wait call-up for national service, unlike the U.S. experience (this is also evidenced by the insignificant coefficient of ballot outcome for cohort 16 in the army service regression in table 2; see also note 5).

In section III, I 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 does because of preenlistment 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 (my calculations from the 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 the coefficient of ballot outcome for cohort 1 in the army service regression in table 2). By 2006, they probably account for roughly 32.7% $\times$ (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.

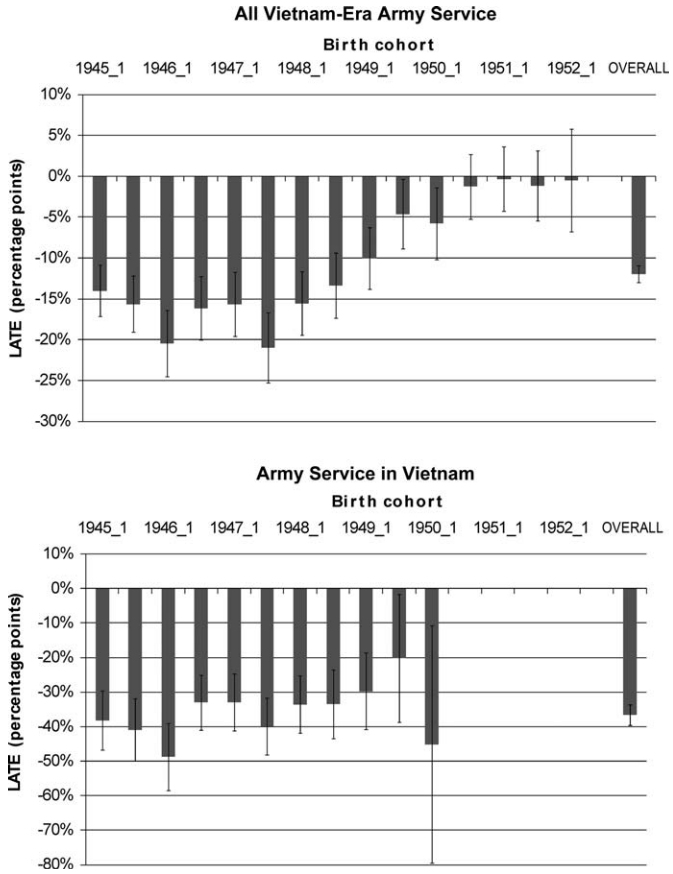
The reduced-form employment regression shows that ballot outcome is a significant negative determinant of employment for each of the first 11 cohorts (table 2).

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

Figure 1 shows 2SLS estimates of employment effect separately for each cohort. The youngest cohort is omitted, for whom ballot outcome is irrelevant in the first stage. The upper 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 0, 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 affected employment only for those who went to Vietnam. The five youngest cohorts are omitted because of the weakness in their 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,

Figure 1.—2SLS Employment Effects by Birth Cohort (Census 2006)



Vietnam veterans were age 60 or 61 and hence were eligible for a veterans' age pension), while other men (civilians of the same age, army veterans who did not serve in Vietnam, and younger Vietnam veterans) were not eligible for any age pension. The estimates for the last two cohorts shown are less precise, reflecting slightly weaker first stages.

The upper panel of table 3 shows 2SLS estimates of employment effects across cohorts. Columns 1 and 2 show results from just-identified specifications, each with a single endogenous variable and a single ballot-outcome instrument. Columns 3 to 7 show results from the overidentified specifications that use the full set of 16 IVs. In columns 1 and 3, army service (r) is the sole endogenous regressor. The estimated coefficient of r is $-.111$ using one IV and $-.120$ with all sixteen IVs, with very small standard errors. Thus, Vietnam-era army service reduced the employment rate of compliers by 11 or 12 percentage points. In columns 2 and 4, army service is excluded from the model, replaced with army service in Vietnam (v), which has an estimated coefficient of $-.365$ with one IV. With all 16 IVs, the point estimate is almost unchanged at $-.366$, but is more precise. Thus, army service in Vietnam reduced the employment of compliers who served in Vietnam by 37 percentage points, under the assumption that the employment effect was 0 for those who remained in Australia.

TABLE 3.—2SLS ESTIMATES OF EMPLOYMENT EFFECTS (CENSUS) AND VETERANS' DISABILITY PENSION EFFECTS (2006)

	Just-Identified Models (1 Ballot Outcome IV)		Overidentified Models Using All 16 IVs (Ballot Outcome by Cohort)				
Endogenous variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable: Employed in 2006							
Army service (r)	$-.111^{***}$		$-.120^{***}$		$-.002$	.001	.000
	(.005)		(.005)		(.010)	(.010)	(.011)
Army service in Vietnam (v)		$-.365^{***}$		$-.366^{***}$	$-.361^{***}$	$-.341^{***}$	$-.330^{***}$
		(.018)		(.015)	(.029)	(.030)	(.035)
Army service in Vietnam interacted with age >= 60 (vp)						$-.083^{**}$	$-.078^{*}$
						(.032)	(.034)
Army service in Vietnam interacted with combat intensity proxy (vx)							$-.038$
							(.071)
Overidentification test statistic			32.18	3.36	3.35	1.78	1.72
			$p = 0.006$	$p = 0.999$	$p = 0.998$	$p > 0.999$	$p > 0.999$
Dependent variable: receiving veterans' disability pension (special rate) in 2006							
Army service (r)	$.148^{***}$		$.161^{***}$		$-.003$	$-.004$	.000
	(.002)		(.002)		(.003)	(.003)	(.003)
Army service in Vietnam (v)		$.488^{***}$		$.498^{***}$	$.505^{***}$	$.498^{***}$	$.462^{***}$
		(.007)		(.007)	(.010)	(.011)	(.013)
Army service in Vietnam interacted with age >= 60 (vp)						$.030^{*}$	.013
						(.014)	(.015)
Army service in Vietnam interacted with combat intensity proxy (vx)							$.136^{***}$
							(.033)
Overidentification test statistic			67.24	1.86	1.84	1.61	0.69
			$p < 0.001$	$p > 0.999$	$p > 0.999$	$p > 0.999$	$p > 0.999$

This table shows 2SLS estimates under various specifications. Each column corresponds to an employment regression (upper panel) and a disability pension regression (lower panel). A constant and fifteen cohort dummies are included in each regression. The overidentification test is the grouped data test (Angrist & Pischke, 2009). The test statistic has a Chi-square 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 for columns 3 to 7, respectively. $N = 675{,}832$ in each employment regression and 777,201 in each disability pension regression. Robust standard errors are in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

The results in column 5 are for the specification in equation (1), where the coefficients of r and v are both identified. The estimated coefficient of v is $-.361$, which should now be interpreted as the additional effect of service in Vietnam (relative to the effect of serving only in Australia). The coefficient of r is close to 0 and is not statistically significant, despite a small standard error, and so its inclusion has almost no effect on the estimated coefficient of v. This suggests that the negative employment effect of army service is manifest only among 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 6, I add the interaction of v and p (age eligibility for the veterans' age 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 civilian age pension age of 65) did so before reaching the veterans' age pension age (60 years). In this specification, the coefficient of v is $-.341$, which represents the employment effect of service in Vietnam for those not yet eligible for a veteran's age pension.

In column 7, I add the combat intensity proxy (x), interacted with v. Other recent studies have used similar proxies for combat intensity (Costa & 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-related heterogeneity.[10] 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 if it does, its use by external researchers is constrained by 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).

The overidentification test (table 3, column 3) confirms that the army service local average treatment effects differ between cohorts, which is consistent with the upper panel of figure 1. For the other specifications (columns 4 to 7), the overidentification tests show no evidence of heterogeneous average effects between cohorts.

A broad literature suggests that employment effects should be larger for men with low potential earnings (see

[10] 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. All led to imprecise 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%).

Angrist et al., 2010; Autor & Duggan, 2003, 2006; Cai & 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 (laborers 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 of either 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 (as shown 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 men may have been more resourceful in avoiding conscription, and they had a greater incentive to do so because of their potential earnings. So I cannot confidently determine whether the employment effect is concentrated among low-skill men.

C. *Veterans' Disability Pension Effects*

The lower panel of table 3 repeats the analysis, this time with receipt of veterans' DP-SR in 2006 as the dependent variable. In most respects, the DP-SR effects mirror the employment effects but are even larger. This suggests that some compliers who receive DP-SR, and hence are not employed, would remain not employed in the counterfactual (if they had been balloted-out) but would be ineligible for DP-SR.

Like the employment effect, the DP-SR effect is confined to men who served in Vietnam. There is some evidence that the effects are larger for cohorts that experienced greater combat intensity. As argued previously, however, this proxy may be picking up age-related heterogeneity. There is little or no effect of veterans' age pension eligibility on DP-SR receipt.

D. *Employment and Veterans' Disability Pension Effects over Time*

Table 4 shows corresponding 2SLS estimates of employment and veterans' disability pension effects over time. Since table 3 suggests that such effects are confined to men who served in Vietnam, I now include army service in Vietnam as the sole endogenous regressor. The employment effect from Census data in table 4 is taken directly from table 3 (column 4), and the same specification is used throughout table 4, changing only the dependent variable. The discrepancy in the 2006 results using Census versus tax data may reflect differences in the definition of being employed, including the treatment of self-employment (excluded from the tax data employment definition) and differences in the reference period (one week in the Census compared to a full year in the tax data).

Table 4.—2SLS Estimates of Employment Effects and Veterans' Disability Pension Effects, 1990–2009

Year	Employment Effect (Census Data) (1)		Employment Effect (Tax Data) (2)		Disability Pension (SR) Effect (3)	
1990					$.017^{***}$	(.001)
1993			$.047^{***}$	(.014)	$.038^{***}$	(.002)
1996			.026	(.014)	$.101^{***}$	(.003)
1999			$-.037^{*}$	(.015)	$.204^{***}$	(.004)
2002			$-.116^{***}$	(.016)	$.347^{***}$	(.006)
2006	$-.366^{***}$	(.015)	$-.208^{***}$	(.017)	$.498^{***}$	(.007)
2009			$-.235^{***}$	(.017)	$.570^{***}$	(.007)

This table shows 2SLS estimates of the effects of army service in Vietnam on employment or disability pension receipt at various points in time. Each estimate shown is from a separate regression. The specifications correspond to those shown for 2006 in column 4 of table 3. For column 2, 1993 represents the 1992–1993 tax year, similar to other years. Earlier data are not available at this time. Men with positive earnings in a given tax year are counted as employed for the regressions using tax data. The last five cohorts are excluded from the employment regressions using tax data. The discrepancy in the 2006 results using Census versus tax data may reflect differences in the definition of being employed, including the treatment of self-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). Disability pension (SR) receipt is in June of each year. Robust standard errors are shown in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

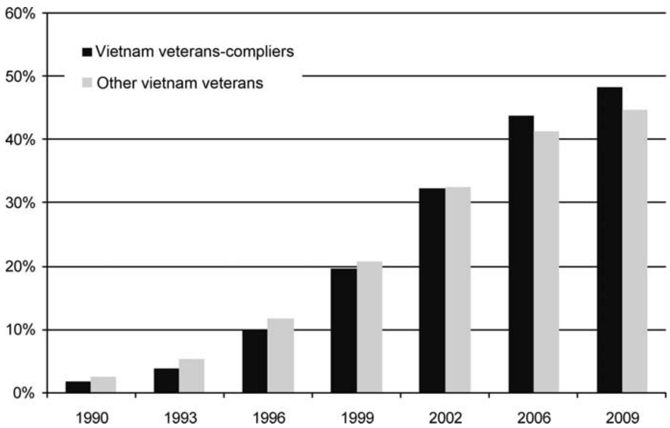
The results from tax data suggest that the employment effect emerged recently, beginning around the mid-1990s. The estimates are negative and statistically significant for each year from 1998–1999 to 2008–2009. They increase (become more negative) steadily throughout the period. The effect is largest for 2008–2009 at −23.5 percentage points. For 1992–1993, the employment effect is actually positive (4.7 percentage points) and significant. Undoubtedly some men were permanently unable to work immediately after being injured in Vietnam. However, their numbers are apparently too small to drive these results. There is reason to be cautious in interpreting the positive effect for 1992–1993, 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 0 as recently as the mid-1990s.

DP-SR effects also increased strongly between 1990 and 2009, by a factor of over 33, from a very small base. The similarity of trends in DP-SR effects and employment effects, coupled with the results in table 3, strongly suggests that the two are linked.

E. *External Validity*

Compliers are likely to have different characteristics from men who joined the army voluntarily. Further, compliers were limited to army service, but other Vietnam veterans served in all arms of the Defence Force. Excluding compliers, 33% of Vietnam veterans in the same age groups served in the navy and 8% served in the air force. Figure 2 shows the proportion of compliers and other veterans who served in Vietnam who 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 very similar to that of compliers. This is a strong indication that the results have some external validity. The trends in

FIGURE 2.—SHARE OF VIETNAM VETERANS RECEIVING DVA DISABILITY PENSION (SPECIAL RATE)



Population restricted to Vietnam veterans born between January 1, 1945, and June 30, 1950. Adjustments are not made for mortality, so all proportions are underestimates.

DP-SR take-up are not confined to conscripts or to army servicemen.

F. *Earnings and Income*

How did the dramatic changes in employment and disability pension receipt affect compliers' standard of living? Using the same 2SLS procedure as above, I estimate the effect of army service on income and earnings (table 5). Army service (r) is the endogenous regressor, since there is no reason to presume that these effects are confined to men who served in Vietnam. I consider total personal income in 2006 using the Census (source of income is not available) and earnings in 1996 tax data.[11]

For 1995–1996, I find no significant effect on earnings in logs or in levels, although both point estimates are positive. Thus, there is no evidence of an adverse effect of army service on mean earnings in 1995–1996. However, other income sources, such as the veterans' disability pension, are not included.

Using equivalent specifications, I find no significant effect on mean personal income in 2006.[12] However, income in the Census is reported before tax, and the veterans' 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 explicitly whether the reduction in earnings over this period was offset by the increase in transfers.

TABLE 5.—2SLS ESTIMATES OF EARNINGS EFFECTS AND INCOME EFFECTS

	Dependent Variable			
	Earnings in 1995–1996 (1)	Log (Earnings in 1995–1996) (2)	Personal Income in 2006 (3)	Log (Personal Income in 2006) (4)
Army service (r)	319	.016	−598	.004
	(297)	(.011)	(464)	(.010)
N	697,487	697,487	650,185	633,904

This table shows 2SLS estimates of the effects of army service on annual earnings in 1995–1996 (using tax data) and on annual personal income in 2006 (using Census data). The samples are restricted to observations with positive earnings (columns 1 and 2), nonmissing income (column 3), and positive income (column 4). The specifications correspond to those shown in column 3 of table 3. Robust standard errors are shown in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

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 emerged only in the past fifteen years, coinciding with even larger veterans' disability pension effects. I have shown evidence suggesting that the effects are not confined to veterans who were induced to enlist by the conscription ballots.

These results are consistent with aging-related exacerbation of service-related health conditions. However, comparable estimates of employment effects for the United States are statistically insignificant and close to 0 (Angrist et al., 2010, 2011). 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. Department of Veterans Affairs, 2009; Wilson et al., 2005).

Thus, an explanation must be sought elsewhere. It has been suggested that Australia's system of veterans' entitlements is among the most generous in the world (Clarke, Riding, and Rosalky, 2003). Indeed, a far greater proportion of Australia's Vietnam veterans receives 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 online). However, it seems likely that the results are explained primarily by the central role of employability in the Australian compensation system. There is a very large difference in the Australian disability pension payment rate between veterans who can and cannot work that is not part of the U.S. system. A simple policy change that might be worthy of further research is an increase in the highest general veterans' disability pension rate to equal the DP-SR rate. This would not disadvantage

[11] As shown previously, the coverage of men with non-0 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. A similar assumption is not appropriate for other years.

[12] 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 $2,500 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. However, the distribution of income is heavily compressed among balloted-in men in 2006. The probability of having income between $400 and $800 per week is much higher (reduced form effect = 0.022, SE 0.002) for balloted-in men. The probability of having higher or lower incomes is correspondingly lower.

any veteran but would greatly reduce the work disincentive. I do not attempt to estimate the cost of such a reform, but I note that DP-SR recipients far outnumber recipients of 100% general rate compensation. This is particularly the case for Vietnam veterans, for whom the ratio is about 7:1, but also for veterans of other conflicts except World War II.

A rough conservative estimate of the present value of lost earnings for Vietnam veterans is $9.4 billion (AU$2010, assuming a 5% discount rate), or over $240,000 per soldier who served in Vietnam.[13] No detrimental effects of army service on mean income are found, so this loss in earnings appears to have been fully compensated.

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.

[13] For this estimate, it is assumed that the employment effects estimated for compliers also apply to other Vietnam veterans from the 1945–1952 birth cohorts. The estimate is based on the 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.

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APPENDIX

Draft Avoidance Behavior 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 behavior 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 enroll 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 drop 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 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), while 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 postdischarge national service vocational training scheme covered fees, travel expenses, 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. See Siminski & Ville (2012) for a more detailed analysis of schooling effects.

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 March 10, 1965. Most men were informed of the outcome within two weeks, and call-up action began on May 31, 1965 (Australian Government, 1966). In later years, similar rules prevailed (see 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 my knowledge, no such concerns were 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 my knowledge, there is no evidence in the historical records of such concerns being raised.

D. Noncompliance and Prison

One way in which men dissented and did not comply with the 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 fourteen 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 among 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 or called up and if this affected their long-run employment outcomes. In the United States, 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 or moving overseas are given by Ham (2007), 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 traveling 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).

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 (Australian Government, 1966; Department of Labour and National Service, 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 also induced other men to *stay* in the citizen forces. This is an issue only 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.