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## **I was only nineteen, 45 years ago: What can we learn from Australia's conscription lotteries?**

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# I Was Only Nineteen, 45 Years Ago: What Can we Learn from Australia's Conscription Lotteries?<sup>1</sup>

## Abstract

The Australian conscription lotteries of 1965-1972 are a unique and underutilised resource for studying the effects of army service and veterans' programs. Drawing on many data sources and 25 years of related US literature, we present a comprehensive analysis of this natural experiment, examining indicators of health, personal economic outcomes, family outcomes and educational attainment. We discuss the numerous potential mechanisms involved and the limitations of available data.

## 1. Introduction

The economic and social consequences of war have exercised the minds of policy makers and scholars over many years. World War Two has been a particular focus as the apogee of 'total war' where mass mobilisation of universally conscripted armed services and the aerial bombing of civilian communities inflicted massive human and physical destruction, with immense economic and social consequences (Marwick 1974, Milward 1977). The second half of the twentieth century, by contrast, witnessed limited regional conflicts with effects largely limited to battle locations. From the perspective of Western countries, these wars have been fought by regular military personnel, sometimes supplemented by selective conscription. Nevertheless, the economic and social consequences of military service remain pertinent policy-relevant questions. They inform assessments of the costs of modern warfare, an issue of central concern in the context of ongoing

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<sup>1</sup> The title is a reference to the song "I Was Only Nineteen" by Australian band *Redgum*.

conflicts in Afghanistan and Iraq (Bedard and Olivier, 2006; Stiglitz and Bilmes, 2008). They also inform appraisements of the adequacy and design of veterans' compensation. The Australian Government Department of Veterans' Affairs (DVA) has an annual research budget of around AU\$2.5 million, some of which has been dedicated to assessing the health and mortality effects of military service and subsequent rehabilitation and reintegration (examples of DVA-funded projects include Fett *et al.* 1984; Harrex *et al.* 2003; Sim *et al.* 2003; Sim *et al.* 2005; Wilson *et al.* 2005a, 2005b).

Our focus is Australia's involvement in the Vietnam War, one of the most significant events in Australia's recent history. When Australia's initially minimal commitment to the war expanded from 1965, the regular army was supplemented by conscripts selected by a ballot based on date of birth (DOB). Sixteen bi-annual ballots were conducted from 1965 to 1972. The randomness of the conscription ballots, designed to create a sense of fairness, but increasingly regarded as arbitrary and lacking legitimacy as the war progressed, has proved ideal for researching the causal effects of military service. It has allowed us to solve the problem caused by non-random selection, which typically affects observational data analyses. Men accepted into the army are typically much healthier than the general population. There are likely to be many other differences between army servicemen and other men. Some of these differences will be unobserved and hence ordinary least squares (OLS) regression cannot adequately overcome this selection problem. The conscription lotteries, however, solve the selection problem. They are almost identical to randomised controlled trials with imperfect compliance, which can be addressed using instrumental variable techniques. The intuition of the approach is to compare the outcomes of *all* men (not just veterans) whose DOBs were 'balloted in' to the outcomes of all men whose DOBs were balloted out. Any differences in their outcomes likely reflect the effects of army service or its downstream consequences such as veterans' compensation.

The United States had a broadly comparable conscription system in a similar era. Over the last 25 years, many US studies have exploited this opportunity to study the effects of military service on health, economic and social outcomes (these include Angrist 1990; Angrist *et al.* 1996; Angrist *et al.* 2010; Angrist *et al.* 2011; Angrist and Chen 2011; Conley and Heerwig 2009, 2011; Dobkin and Shabani 2009; Hearst *et al.* 1986; Hearst *et al.* 1991; Lindo and Stoecker 2010). Until recently, Australia's lotteries have not been utilised in this way. In recent years, several high quality data sources have emerged that facilitate such research. These include the Census of Population and Housing (which collected DOB for the first time in 2006), personal income tax data, and administrative data on mortality and veterans' compensation. We use these sources to study the

effects of army service on a broad range of outcomes, including health, personal economic outcomes, family outcomes and educational attainment. We have previously published studies on mortality (Siminski and Ville 2011) and employment (Siminski forthcoming). Here, our purpose is broader. We aim to provide a comprehensive account of what we have learned thus far from Australia's conscription lottery 'natural experiment' in the Vietnam War era. The majority of the results we show have not been published previously.<sup>2</sup>

We make several contributions to the Australian and international literature. We utilise Australia's conscription lotteries for identification. Only our own recent studies have done so previously. To our knowledge, this is also the first Australian study to estimate the effects of army service on a broad set of economic and social outcomes using any quantitative method. The institutional contexts of Australia's conscription lotteries also allow us to separately identify the effects of army service for those who served only in Australia and the effects of service for those who served in Vietnam. Several other advantages of the Australian conscription lotteries are detailed in Siminski (forthcoming).

The remainder of the paper is structured as follows. The following section describes the selection process for national service, compares the service experience of those who went to Vietnam with those who completed their service in Australia, and examines the range of repatriation benefits available to returning soldiers. Section 3 explains the methodology used to exploit the randomness of the ballots and describes the data used to examine effects. The subsequent sections present results for health and disability compensation (Section 4), employment, income and earnings (Section 5), domestic life (Section 6) and educational attainment (Section 7). Section 8 summarises and provides overall conclusions. Comparisons are drawn with the American context and results throughout the paper.

## **2. Institutional Context**

### *Selection*

Conscription into the armed services has been practised widely over a long period of history (Ville and Siminski 2011). In Australia there had been three conscription schemes prior to the Vietnam war era, in 1911-29, 1939-45, and 1951-9. Each of these was developed as a

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<sup>2</sup> The exceptions are results for mortality (Siminski and Ville, 2011), employment and disability compensation (Siminski, forthcoming). Brief discussions of effects on mean income, mean earnings and education are also contained in Siminski (forthcoming).

universal obligation for men of a specified age. However, in the last two years of the final scheme (1957-9), there was a shift to selective conscription that reflected reduced manpower needs and the desire to focus on more intensive training of fewer men. The process of selection used, a date of birth ballot, was revived in 1965. The context of its use differed somewhat second time around – the target group was 20 year old males (previously 18 year olds) and, rather than being solely a training scheme, it involved service in the regular army that could include wartime deployment in Vietnam. Bi-annual conscription ballots were conducted continually for eight years up to 1972, representing 16 natural experiments from which to analyse the effects of army service.

The nature of the selection process was largely unchanged throughout this era. Males were required to register for the ballot in January or July in the half-year of their twentieth birthday. Marbles, each marked with a number corresponding in serial order to a date in that six-month period, were placed in a barrel. Successive marbles were then drawn randomly from the barrel until a sufficient number had been selected to correspond with estimates of the army's manpower needs taking account of the size of the birth cohort and the extent of likely exemptions and deferments.<sup>3</sup>

Those whose dates of birth were withdrawn from the barrel were 'balloted-in', that is, *liable* to be enlisted in the army. Those balloted-out, the remainder, were permanently exempt from conscription. The probability of being balloted-in varied quite significantly across the 16 ballots, from a peak of 53 per cent in the first 1965 cohort to a nadir of 16 per cent in the second half of 1969. Being balloted-in did not, however, lead inevitably to army service. Far from it: of an estimated 224,706 men with balloted in dates of birth, only 63,735 (28 per cent) served in the army, a proportion that also varied between cohorts (Ville & Siminski 2011). A variety of exemptions and deferments explains why little more than a quarter of those balloted-in served. The main grounds for exemption were the failure of medical, psychological or aptitude tests. Members of religious orders and conscientious objectors to war were also exempt. Married men, volunteers, and serious criminals were offered indefinite deferments, while students, apprentices and trainees were granted temporary deferments for the duration of their course, subject to satisfactory progress (Fett et al: 191-4).

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<sup>3</sup> Langford (1997) provides an excellent summary of the Vietnam era National Service scheme.

Comparisons with the American conscription lottery suggest that Australia's procedure provided a cleaner natural experiment. The longer lead times of the annual American ballot provided greater scope for post-ballot draft avoidance behaviour. It seems likely that the use of a progressive system of selection through 'Random Sequence Numbers' may have influenced behaviour (either motivated by draft avoidance or in anticipation of being drafted) amongst men with dates of birth that fell in the uncertain middling section of the sequence of numbers. Finally, while dates of birth drawn remained unpublished until 1997 for all but the last five Australian lotteries, the American ballots were televised live. In the US context, this may have enabled discrimination against draft eligible men who had avoided conscription (especially by potential employers).

The basis on which National Servicemen were selected to serve in Vietnam is complicated, but was far from random (Fett *et al.*, 1984). Units in the Infantry, Artillery, Engineers and Armour were most needed in Vietnam so a serviceman's allocation to a particular corps was a critical factor. Corp allocation took into account skills, further psychological aptitudes and personal preference, amongst many other corp-specific factors. It has been suggested that many who expressed unwillingness to serve in Vietnam were able to avoid doing so (Fett *et al.*, 1984).

### *The Treatment(s)*

For the majority of the conscription era, National Service consisted of two years in the army, followed by three years in the army reserve.<sup>4</sup> But the nature of the National Service experience varied considerably between those who served in Vietnam and those who remained in Australia.

The initial conditions of training, discipline, and exposure to the army culture, graphically described by Paul Ham (2007) were relatively homogeneous. National servicemen began their service with about 10 weeks of training in general military skills such as weapons handling and achieving high levels of physical fitness. The next stage was allocation to one of the army's function-based corps (such as infantry, artillery, armour) for a further three months of training tailored to the skills and operational needs of the particular corps.

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<sup>4</sup> Full time service was reduced to 18 months in 1971 and the part time element raised from three to three and a half years.



Suitability of skills and level of education played some role in the choice of corps. At the end of the corps training, servicemen were deployed to a particular service unit within that corps (Fett *et al.* 1984).

At this stage, there was a separation in the nature of the treatment between the majority who served out the remainder of their full time service in Australia and those whose unit was to be deployed in Vietnam. Only 18,654 National Servicemen, less than thirty per cent of their total numbers, went to Vietnam and this was normally for one year. For the majority of National Servicemen who, following training, completed their service in Australia, the experience was for many tedious and constricting as a result of repetitious training exercises, and conformity to army practices and the culture of discipline and hierarchy (Ham, 2007: 172-7).

Those selected for Vietnam underwent further and more demanding preparedness training, including a battle efficiency course at the jungle training centre at Canungra, which helped to weed out anyone not considered suitable for a Vietnam posting. One of the challenges of training was to mimic conditions in Vietnam as closely as possible in order to increase the efficacy of the army. Simulated Vietnamese villages were constructed and populated.<sup>5</sup> Another aspect of realism would be to inure recruits to the shock of experiencing live weapon fire. Recent research on the US suggests low rates of weapon usage, even in defence, in previous wars, led to a training regime of greater realism for Vietnam. This appears to have lifted firing rates substantially during the Vietnam War (Grossman 1996: 250) but has motivated research into the possibility that habituation to violence and death may have contributed to higher postwar rates of violent crimes committed by veterans (Lindo and Stoecker 2011). Australian recruits for Vietnam appear to have experienced realistic firing practice and participated in large mock battles at Canungra that included the use of live weaponry (O'Brien, 2002: 2).

It would seem that those who served in Vietnam experienced a more intense 'treatment' in certain respects, as detailed by Fett *et al* (1984: Appendix IX). They may have been exposed to a series of physical health risks such as tropical diseases, chemicals, and herbicides such

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<sup>5</sup> A number of images in the photographic collection of the Australian War Memorial illustrate these constructed villages, for example ELL/64/2150/NC.

as 'agent orange'. They were also susceptible to severe psychological stress – they lived with the constant fear of attack, some of course experienced combat directly, and they served in conditions remote from their family and isolated from normal society. Much of the work of the Australian army took the form of sorties through dense bush to track down the enemy rather than the large set piece engagements that appear to have been more common of American experience (Grey, 2008: 246). The risk of surprise attacks and ambushes in these circumstances and of detonating heavily concealed landmines added to the sense of nervousness and fear. The short term response may have played itself out through high intakes of alcohol and tobacco.

The Vietnam experience itself was also far from homogenous, varying between units and phases of the war. Those who served between 1966 and 1969 faced the greatest risks of death or disablement. These were the years of the Battle of Long Tan, the Tet Offensive, and intensive mine laying (Grey, 2008: 243-4). When the Vietnam veterans returned to Australia, rather than being treated as conquering heroes, similar to those who served in the two world wars, they encountered an environment of hostility to Australia's involvement in the Vietnam War. The popular anti-war movement had been growing in strength from the late 1960s as the Vietnam War and the conscription process lost their sense of legitimacy in Australian society (Hamel-Green 1975; Langley 1992; Caulfield 2007). Again there are clear parallels with the experiences of returning American troops. Arguably even more galling was the apparently lukewarm reception in government and military circles. The Returned Services League looked with disapproval on the lack of major engagements by Australians in Vietnam and refused to admit 'Nashos' since they were not volunteers (Damousi, 2001: 50; Doyle, Grey, Pierce, 2002: 79). In terms of official recognition, it was only in 1987 that Vietnam Veterans were recognised through a symbolic welcome home march, the Australian Vietnam Forces National Memorial in Canberra was not completed until 1992, nor the National Servicemen's Memorial until 2010. The returning soldiers thus had to contend with a broad cultural and societal disapproval on top of, and exacerbating, their own personal social and psychological dislocation. The sense of alienation and rejection felt by the returning Veterans has been widely recorded in popular and oral histories (McKay 1992; Dickins 1999; Hennessy 1997).

In order to understand fully the nature of the ‘treatment’ it is important to consider the range of repatriation benefits and programs provided to veterans in both Australia and the United States as a means of compensation for the effects of service. A system of repatriation benefits in Australia dates back to World War One and is focussed on disability and service pensions, health services, business and home loans, and workforce training (Lloyd & Rees 1994). Of particular note is the cash disability compensation provided by the DVA. Although known as the Disability ‘Pension’ (DP), it can be combined with income support. DP is not taxable and not means tested. At 20 September 2011, the highest rate of DP (the ‘Special Rate’ or DP-SR) is \$1,143.80 per fortnight. DP-SR is paid to veterans who are ‘totally and permanently incapacitated’ (TPI). The DP has several noteworthy features. First is the ‘reverse criminal’ standard of proof. For DP claims made by veterans with operational service, DVA must accept the claim unless it can prove beyond reasonable doubt that the injury or disease was not caused by military service. Second, the Special Rate of DP is around 2.7 times higher than the 100% General Rate of DP. Third, DP-SR recipients cannot work more than 8 hours per week, whilst there are no work restrictions on General Rate DP recipients. Siminski (forthcoming) has argued that this represents a substantial work disincentive, which contrasts with the US scheme, possibly explaining the striking discrepancy in employment outcomes of Australia’s veterans compared to US Vietnam veterans (this discrepancy is discussed in Section 5). A range of other benefits were available to National Servicemen, irrespective of service in Vietnam. These included low interest business loans up to \$3000, or \$6000 for agricultural expenses, to re-establish a business or profession, housing grants under the Commonwealth Home Savings Grants scheme, and vocational training schemes for servicemen with no job to return to; those who held positions for at least 30 days before call up were entitled to return to them (Langford 1997: 365-6).

### 3. Methods, Data and First Stage Results

Despite the conscription lotteries, army service was not randomly assigned. If it were random, we could estimate the causal effect  $\beta$  of army service ( $r$ ) on a given outcome variable ( $y$ ) simply by estimating the following equation by Ordinary Least Squares (OLS), without even needing covariates:

$$y_i = \alpha + \beta r_i + \mu_i \quad (1)$$

Or if selection was randomly assigned within each of the sixteen six-month birth cohorts, we could estimate causal effects with the inclusion of sixteen cohort dummies ( $C$ ):

$$y_i = \beta r_i + \gamma' C_i + \mu_i \quad (2)$$

But OLS would not yield credible causal estimates, because of the range of factors that influence selection into the army. In particular, stringent medical standards rule out a large fraction of the population from serving in the military. Any number of other characteristics (many of which may be unobserved) may be correlated with the decision to seek out or avoid army service. Thus  $r$  is almost certainly correlated with the residual  $\mu$ .

Instead, we use Two Stage Least Squares (2SLS), exploiting the randomness of the lotteries. It is certainly not the case that all balloted-in men served in the army. Further, many balloted-out men served in the army. However, as we will show, balloted-in men were much more likely to serve in the army than balloted-out men within cohorts. There is a subset of this population ('compliers') for whom the ballot outcome determined whether they served in the army. We estimate Local Average Treatment Effects (LATEs) for this subpopulation (Imbens and Angrist 1994). The important assumption that all other men (non-compliers) were unaffected by the ballot outcome is discussed in (Siminski, forthcoming, Appendix A).

We could use a single binary ballot outcome indicator ( $z$ ) as an instrumental variable in a first-stage regression. However, more precise estimates are generated by exploiting differences between cohorts in the strength of the ballot outcome effect on military service. Thus we interact  $z$  with  $C$  as a set of 16 instrumental variables in a first stage regression:

$$r_i = \pi_1' z_i C_i + \pi_2' C_i + \varepsilon_i \quad (3)$$

In the second stage regression, army service ( $r$ ) in (2) is replaced with the fitted value from the first stage regression (3).

Next, for each outcome variable, we estimate a corresponding set of effects under the assumption that army service only affected those who served in Vietnam. Thus army service ( $r$ ) is replaced with army service in Vietnam ( $v$ ) in equations (2) and (3).

Finally, we explicitly estimate the effect of army service in Australia and the additional effect of army service in Vietnam through a model which includes both as endogenous variables:

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

Both  $r$  and  $v$  are instrumented by the 16 ballot outcome IVs in a pair of first stage regressions. They are separately identified by cohort differences in the relative strength of the ballot outcome effect on army service and army service in Vietnam, respectively.

We are not aware of any sufficiently large data sets that contain all the necessary variables to implement this strategy. This is overcome by using Two Sample 2SLS (Inoue and Solon 2010), which in principle follows Angrist (1990). This approach requires two data sets, one with first stage data (the endogenous variable, instruments and controls), the other with reduced form data (the outcome variable, instruments and controls). The approach is to estimate the 2<sup>nd</sup> stage regression with the reduced form database by OLS, after replacing the endogenous variables with the fitted values from the first stage regressions.

We have constructed a first stage database for the universe of men who were born in 1945-1952 and lived in Australia when aged 20, drawing on army personnel databases and contemporaneous population aggregates, as detailed in Siminski (forthcoming).

For the second stage, we utilise several data sets which respectively contain various outcome variables. In particular, we draw on the 2006 Census of Population and Housing, which was the first Australian Census that collected DOB, an essential variable for our approach. We use Census data to study disability, labour force status, income, marital status, family size, residential stability and educational attainment. We supplement this with mortality data from the AIHW (1994-2007), income tax return data to study earnings in 1992-93 and administrative veterans' compensation data (2009). Each second stage dataset covers the universe of (surviving) men in the birth cohorts of interest, excepting sample loss due to census under-enumeration, item non-response or failure to submit tax returns. These data are discussed in more detail in (Siminski and Ville 2011; Siminski forthcoming), which also include key descriptive statistics.

For reasons of confidentiality, we have not been given exact DOB in any of these second stage data sources. Instead we know 6-month birth cohort and whether DOB was balloted in or out. This is all we need for the point estimates, but it is not ideal for estimating standard errors. Ideally, we would cluster on DOB, since this is the unit by which conscription eligibility was randomly assigned. Since this is not possible, we considered two alternatives. The preferred approach is to use simple robust standard errors, ignoring clustering since any correlation within exact DOB is likely to be negligible.<sup>6</sup> The second, more conservative, approach is to use a group means procedure, where each group is a

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<sup>6</sup> In preliminary analysis of other second stage data sources where we do have exact DOB, we have found that clustering on DOB makes essentially no difference to the standard errors.

cohort-ballot outcome combination, giving at most 32 observations, effectively equivalent to around 25 observations after weighting by group size is taken into account. A conservative application of this approach is to take the higher of the robust and non-robust standard errors (Angrist & Pischke, 2009). The results are almost never sensitive to the method of calculating standard errors, including the level of significance in most cases. Education effects are arguably an exception to this, as will be discussed.<sup>7</sup>

A natural question is whether the first stage estimates (using data collected in 1965-1972) are appropriate to combine with reduced form data collected decades later. Do migration and mortality pose problems? Siminski (forthcoming) argues that mortality does not pose a major threat to the approach. To address migration, we exclude all men who arrived in Australia after the age of 20 where this is possible (Census and mortality data). Where this is not possible (veterans compensation data and tax data), we adjust the first stage data accordingly to include men who immigrated later, whose DOB distribution is assumed orthogonal to ballot outcome.

Figure 1 is a graphical depiction of the first stage results. It shows the proportion of men who served in the army during the Vietnam War era (upper panel) and in the army *in Vietnam* (lower panel) by birth cohort and ballot outcome. For each birth cohort, the first stage effect is the difference between the balloted-in and balloted-out proportions. The ballot outcome clearly induced a large proportion of balloted-in men into the army (between 19% and 33% for the first 14 birth cohorts). The effect is statistically significant for all but the youngest cohort. This is sensible, since the last cohort was never called up; the conscription system was abolished in December 1972 by the newly elected Whitlam Labor government. The ballot outcome also induced a subset of ‘army compliers’ to serve in Vietnam. These men were concentrated in the first 11 birth cohorts and indeed no men conscripted through the last 4 ballot drawings were sent to Vietnam. The corresponding regression results are presented in Table A.1. The Angrist-Pischke multivariate F-statistics are high, indicating a strong first stage.

#### **4. Health and Disability Compensation**

Much research has investigated the effects of army service in Vietnam on subsequent health outcomes, though most of it is subject to likely selection bias due to unobserved differences

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<sup>7</sup> The standard errors shown in the tables are simple robust standard errors. The only exception to this is the results for earnings effects, which were derived using the group means approach, since we were unable to access the earnings microdata. Group means standard errors are available on request from the authors for all other estimates.

between servicemen and comparison groups. Even with the benefit of the conscription lottery for identification, the interpretation of estimated health effects is particularly challenging. In this section we present and discuss 2SLS estimates of army service effects on various health and disability indicators, each of which is in some way problematic. The results are shown in Table 1. For each dependent variable considered, we show results from three models. The results in Column (1) are from a model with army service as the sole endogenous variable. Those in Column (2) are from a model with army service *in Vietnam* as the sole endogenous variable. Those in Column (3) are from a model which includes both endogenous variables.

Several studies have analysed post-service mortality amongst Australian Vietnam veterans (Crane et al. 1997; Fett et al. 1987a, 1987b; Wilson et al. 2005a, 2005b), but none exploited the conscription lotteries. A few studies have used the US lotteries for identification of mortality effects (Angrist et al. 1996; Conley and Heerwig 2009; Hearst et al. 1986). As an indicator of health, mortality has the advantage of being immune to several problems with other indicators to be discussed below. But it is also a blunt measure for relatively young populations with low death rates. This limits the precision of the estimates. Our analysis of mortality has been published elsewhere (Siminski and Ville 2011). We briefly summarise the results here. Ideally, we would study deaths during the full post-service period, but DOB is only available in mortality data from 1994 onwards. Panel A of Table 1 shows estimated effects on the probability of dying between 1994 and 2007. These are statistically insignificant and at first glance they appear precise. However, when expressed as approximate relative rates of mortality, the confidence intervals are arguably large: (95% CI: 0.91, 1.21 amongst all army compliers; or 0.77, 1.66 assuming only those who served in Vietnam were affected.) We will continue to monitor mortality using more recent data. The estimates will become more precise, since death rates approximately double with every five years of age amongst men aged in their 60s.

Panel B of Table 1 shows 2SLS estimates for self-reported disability in Census data. These are large and precise. They suggest that army service in Vietnam increased (self-reported) disability rates for compliers by around 15 percentage points. A limitation of such a measure is that it may be endogenous to qualification for disability benefits (Benítez-Silva et al. 2004; Bound and Waidmann 1992). In our view, it is difficult to determine the extent to which this effect can be attributed to army service in Vietnam and how much is due to access to veterans' compensation. Whilst we do not have access to other self-reported health measures, these are likely to suffer from similar problems (Angrist et al. 2010). However, it is noteworthy that related studies for the US find little or no effect on self-reported health and disability (Angrist et al. 2010; Dobkin and Shabani 2009), despite the fact that the US suffered considerably higher casualty rates in Vietnam compared to

Australian troops. This may suggest that the US was much more successful in rehabilitating its veterans, or it may reflect differences in the respective compensation systems, an issue we revisit below.

Panels C and D of Table 1 show estimated effects on veterans' disability compensation receipt at June 2009. Panel C shows the effect on the likelihood of receiving the Special Rate of disability compensation, paid almost exclusively to men who are 'Totally and Permanently Incapacitated'. Army service in Vietnam induced this payment for the majority (57 percentage points) of complying veterans. The effects on the receipt of any rate of disability compensation are shown in Panel D. These are most appropriately read from Column (3). Here there is a small but significant effect for conscripts who remained in Australia. For around 1.7 percentage points of these men, being balloted-in resulted in receipt of veterans' disability compensation in 2009. For those who served in Vietnam, the corresponding effect is 83.2 percentage points (equal to the sum of the coefficients of  $r$  and  $v$ ). This implies that 5 in 6 conscripts who served in Vietnam consequently received disability compensation in June 2009.

Overall, we think that the analysis of health we have presented is inconclusive and limited by the available data. We are currently seeking further health-related indicators, including clinical diagnostic data and health care utilisation data. However, such indicators are subject to their own limitations stemming from the fact that eligible veterans receive enhanced health insurance, including access to private health care through the Gold Card scheme. This may increase their health care utilisation. Similarly, the greater surveillance of their health may increase the likelihood of a given health condition being detected. Finally, even where actual health effects are identified, it is difficult to disentangle the (possibly negative) effects of army service from the (presumably positive) effects of veterans' health insurance and other benefits.

## **5. Employment, Income and Earnings**

Table 2 shows 2SLS estimates of the effect of being conscripted on economic outcomes in 2006. Panel A shows estimated employment effects that are negative, large and precise. Being conscripted reduced the probability of employment in 2006 by around 12 percentage points. Column (3) suggests that the effect was confined to men who served in Vietnam (-36 percentage points) and was essentially zero for those who served only in Australia. These results differ markedly from those of corresponding US studies, which also exploit a conscription lottery for identification. Estimates for the US are not statistically significant overall (Angrist et al. 2010; Angrist et al. 2011), though they are large and significant for low skill men only (Angrist et al. 2010). Siminski (forthcoming) examines



the employment effect in more detail and suggests that this discrepancy between countries may result from differences in the structure of the respective veterans' compensation systems. Siminski (forthcoming) also shows results which suggest that these effects also have some external validity.

Panel B shows that army service also lowered the probability of unemployment. Again, this appears to be confined to men who served in Vietnam. This result has several possible explanations. It may reflect differences in qualification criteria for the DVA Service Pension (Invalidity) versus the equivalent non-veteran pension (the Disability Support Pension). It could also be explained by an income effect: perhaps some veterans who receive veterans' compensation can afford to leave the labour market as an alternative to seeking work. A third potential explanation is the eligibility age for public retirement pensions, which is 60 years for male veterans and 65 for other males. The estimates are virtually unchanged when the sample is restricted to men aged less than 60. Nevertheless, when viewed in a lifecycle context, the prospect of receiving a pension 5 years earlier may still reduce the incentive to seek work for younger men. Taken together, the results from Panels A and B suggest that serving in Vietnam reduced the likelihood of being in the labour force in 2006 by around 39 percentage points.

Panels C and D show that the average effect on personal income is not significantly different from zero, regardless of how the model is specified. This suggests that the income foregone through reduced employment was completely offset by the veterans' compensation system, on average. However, a closer examination of the income data reveals some major distributional effects. Panel E shows that the probability of having a very low income (less than \$250 per week) was greatly reduced for men who served in Vietnam (by around 9%). Conversely, Panel F shows that the probability of having a relatively high income (more than \$800 per week, which is roughly the median for this sample) was also greatly reduced (by around 11%). Again, the effects seem to be confined to those who served in Vietnam.

These distributional effects are presented in more detail in Figure 2, which shows estimated 2SLS effects on having an income within each individual Census income band. Each bar represents the estimate from a separate model, where the (binary) dependent variable represents having an income in each respective band. The probability of having a weekly income between \$400 and \$799 (roughly the second income quartile for this population) is increased by 20 percentage points. Unsurprisingly, the combined rate of DP-SR-plus-income support lies within this range (the single (coupled) rates were \$666.90 (\$625.65) per week). Most of the negative effect is concentrated in the \$150-249 band. This band contains the rates of both Unemployment benefits and the (civilian)

Disability Support Pension.<sup>8</sup> This suggests that some compliers would have been receiving income support regardless of their ballot outcome. But as veterans they receive higher payment rates.

Figure 2 also shows negative income effects extending high up the distribution, above average full time earnings.<sup>9</sup> The most likely interpretation is that the employment effect is not confined to low skill men. The effect does not seem to stem from early retirement pension eligibility, since the exclusion of men aged 60 or more changes the results little. But we cannot rule out a possible prospective behavioural response to earlier retirement pension eligibility (as discussed above in relation to unemployment effects).

We now consider the human capital consequences of army service by examining earnings effects using tax data. A particular focus of previous research is the extent to which army experience substitutes for civilian labour market experience. The seminal work of Angrist (1990) suggests that army experience during the Vietnam era was not subsequently rewarded in the US civilian labour market. Subsequent investigations reveal much heterogeneity between countries and categories of conscripts (as reviewed by Card and Cardoso 2011). Given the large negative employment effects that we have found for 2006, analyses of earnings effects for 2006 are likely to suffer from considerable selection bias. However, Siminski (forthcoming) found that the employment effect is relatively recent and close to zero in the mid-1990s. Thus earnings effects for the mid-1990s are informative. The almost complete lack of an education effect (shown in Section 7) is useful in simplifying the interpretation of these results. Panel G of Table 2 shows estimates for 1992-93, revealing no significant effect on annual earnings amongst men with non-zero earnings. On one hand, this result tells us little about the value of army experience in the civilian labour market. This is because the returns to an extra year of experience are approximately flat in middle age. Since conscripts were around 40 to 48 years old in 1992-93, it is possible that the effects of early career experience had dissipated. Angrist & Chen (2011) find corresponding results for the US. To investigate the experience effect more meaningfully for Australia would require earlier data. Unfortunately, personal income tax data for tax years prior to 1992-93 are unavailable. However, the approximately zero effects on both earnings and employment in 1992-93 do suggest that men's labour market outcomes were largely unaffected by being drafted, at least during middle age.

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<sup>8</sup> At the Census date, the weekly rates were: \$249.85 (Single rate of DSP), \$208.60 (Coupled rate of DSP), \$205.30 (Single rate of Unemployment Benefit); \$185.25 (Coupled rate of Unemployment Benefit). Thus the DSP rate for singles was actually on the boundary between this and the next band.

<sup>9</sup> Average male full time earnings at this time were \$1163 per week.

We also consider distributional effects on earnings in 1992-93. Whilst earnings is a continuous variable in tax data, we cannot estimate quantile treatment effects, since this requires the instruments, endogenous variables and outcome variables in a single data set (Abadie et al. 2002). We can however, attain similar insights in a two sample 2SLS approach, by estimating a series of conventional binary outcome IV models which consider the effect of army service on the probability that earnings lie in particular ranges of the (unconditional) earnings distribution.<sup>10</sup> This is equivalent to the approach used above for analysing income in Census data (where income is reported in bands). The results suggest a possible negative effect on the probability of high earnings. For compliers with positive earnings, army service reduced the probability of high earnings (top quintile) by about one percentage point (Panel H, column 1). However, this estimate is only marginally significant statistically ( $p=0.049$ ) and is small in comparison to the results for income in 2006. The results in column (3) are inconclusive on whether this effect is confined to veterans who served in Vietnam. There is no significant effect on the probability of having low earnings (Panel I). Figures A.1 and A.2 in the Appendix illustrate these distributional effects in more detail.

## 6. Domestic Life

Next we consider effects on marital and domestic outcomes.<sup>11</sup> As usual, the validity of the exclusion restriction rests on the assumption that the dependent variable was affected by the ballot outcome solely through the channel of induced army service. It is important to note that marriage appears to have been a potential draft avoidance strategy even after the ballot outcome was determined. But we are not aware of any evidence for men actually taking such actions (see Siminski forthcoming: Appendix A).

Whilst we hypothesised detrimental effects of army service on domestic outcomes, we do not find any evidence for this. Our 2SLS estimates are presented in

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<sup>10</sup> Since we are unable to access the full income tax data set, these (distributional) results were obtained from datasets that were synthesised using an interpolation procedure and distributional summary statistics from the full 1992-93 and 1995-96 tax data. More specifically, within each cohort/ballot outcome combination, we obtained exact percentile cut-offs (including the maximum and minimum) of the earnings distribution, and the exact number of tax returns between each such percentile. We then assumed a uniform distribution of log-earnings between each such percentile. The results are almost identical when we instead assume a uniform distribution of earnings in levels between percentiles.

<sup>11</sup> The results shown in this section are estimated with second stage data where non-UK migrants and those with missing country of birth are excluded. These exclusions represent about 16% of the relevant sample.

Table 3. We actually find a small but significant positive effect of army service on the probability of being married in 2006 (Panel A). We also find no significant effect on divorce (Panel B). This is consistent with related US studies which also find little evidence of effects on marital status (Angrist and Chen 2011; Conley and Heerwig 2011).<sup>12</sup> We note however that such ‘point in time’ marital status data do not account for the possibility of divorce followed by remarriage. We are currently investigating this further using administrative marriages data.

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<sup>12</sup> The Australian Vietnam Veterans Health study of 1998 also found that marriage and divorce rates did not differ greatly from Australian norms (AIHW and Commonwealth Department of Veterans’ Affairs, 1998)

Table 3 also shows no significant effect on family size (Panel C). The effects on household size (not shown) are similar and also insignificant. Finally, we also find no significant effects on residential stability (moved in the previous 5 years) (Panel D).

Overall, these results do not suggest that conscription into the army has diminished men's ability to engage in stable domestic lives. But these results have a number of possible interpretations. They may represent resilience and the ability to recover from traumatic early adulthood experiences. They may suggest that veterans' rehabilitation programs have been successful, or perhaps that veterans' benefits have sufficiently offset any detrimental long lasting effects of service on family stability. The results might also reflect a more tolerant approach by veterans' wives, if they attribute marital difficulties to the problems caused by their husbands' experiences in the army. More broadly, since National Service removed men from civilian life for two years in their early 20s, it likely had significant effects on the matching of servicemen with wives. For example, perhaps the women who chose to marry them are more likely to be accepting of any war-induced psychological problems. We are currently investigating these ideas further by studying the effects of ballot outcome on spouse characteristics.

## 7. Educational Attainment

There are several reasons to hypothesise that Australia's conscription scheme may have altered educational attainment amongst men born in 1945-1952. Education effects are of interest in their own right and also have considerable implications for the validity and interpretation of 2SLS effects of army service. The main potential mechanisms and their implications for using ballot outcome as an instrument for army service are discussed in turn:

1. Draft avoidance (a): University students and apprentices were eligible for temporary deferments of National Service eligibility (Langford 1997). Thus enrolling in university or taking up an apprenticeship *prior* to the ballot were potential draft avoidance strategies. Similar incentives were manifest in the corresponding scheme in the United States, for which there is clear evidence of a resulting inducement of university education (Card and Lemieux 2001). This mechanism is of interest in itself, but has no implications for the validity or interpretation of 2SLS estimates of army service effects.
2. Draft avoidance (b) Those balloted-in had an additional incentive to *continue* such studies, since deferments were re-assessed annually and subject to satisfactory progress. Renewals could continue until the completion of a degree and in some cases until completion of post-

graduate study. This mechanism may invalidate the exclusion of education from second stage regressions if education affects the outcome variable of interest.

3. Post-Service Education Subsidies: Upon discharge, National Servicemen were eligible for the National Service Vocational Training Scheme (NSVTS), which 'covered the cost of post-discharge training, including compulsory fees, travel fares, textbooks and equipment, and a living allowance for those studying full-time.' (Langford 1997). This scheme supported 12 months of full time training until October 1974, when the time limit was abolished as NSVTS was rolled into the National Employment and Training Scheme. Again, there is a rough US equivalent to this scheme, the more generous GI Bill which induced around 5% of Vietnam-era conscripts to subsequently complete a college degree (Angrist and Chen 2011). Since training benefits are a downstream effect of induced military service, their existence would not invalidate the exclusion of education from the second stage regression. Rather, it would change the interpretation of the 2SLS estimates to reflect both the direct effect of army service combined with the effect of the induced education.

Thus we examine in detail the extent to which Australia's conscription scheme influenced men's educational attainment. We begin by addressing the draft avoidance (b) and post-service educational subsidy mechanisms through reduced form regressions of ballot outcome on educational attainment. Whilst the two mechanisms should affect education in the same direction, post-service subsidies only covered vocational training. Thus draft avoidance (b) is the only hypothesised mechanism for an effect of ballot outcome on degree or high qualifications. Next, to address the draft avoidance (a) mechanism, we seek evidence for changes in the volume and gender composition of enrolments at universities and in apprenticeships around the time the conscription scheme was introduced. Following Card & Lemieux (2001) we also consider whether sex ratios in educational attainment are unusual amongst age cohorts eligible for conscription.

Table 4 shows the estimated (reduced form) effects of ballot outcome on educational attainment, drawing on 2006 Census data. Column (1) shows effects on degree (or higher) attainment. Overall, the estimated effect of ballot outcome on degree attainment is statistically insignificant and precise (95% CI: -0.0010, 0.0030). When each cohort is analysed separately, the effect is only significant for one (the 8<sup>th</sup>) cohort. However, men in the younger cohorts had less incentive to remain at university until degree completion given that conscription was abolished in December 1972. When the youngest 4 cohorts are excluded, the estimated effect is 0.0031 and is significant ( $p=0.009$ ). It is

slightly larger again when restricted to the first 8 cohorts: 0.0035 ( $p=0.012$ ).<sup>13</sup> Thus perhaps one third of one percent of men balloted-in in the first 12 cohorts were induced to attain a degree qualification, presumably through draft avoidance behaviour. This equates to around 500 men or an increase in degree attainment of less than 0.1 percentage points amongst men born in 1945-1950.

Column (2) in Table 4 shows corresponding effects on attainment of Trade or Vocational qualifications. These are broadly similar to those for degrees, though less precise. The effect is insignificant overall, though the point estimate is larger when the sample is restricted to the oldest 8 cohorts. This suggests that few men were induced by the ballot outcome to attain vocational qualifications, despite the fact that both draft avoidance and NSVTS were potential sources of such an effect. Column (3) repeats the analysis with the combined outcome variable of any post-school educational qualifications. Here, it is clear that the ballot outcome did induce some additional education amongst the older cohorts.<sup>14</sup> The trade/vocational educational component of such educational inducement may reflect draft avoidance and/or subsidised vocational training for veterans. However, the lack of an effect on trade/vocational qualifications for the younger cohorts suggests that draft avoidance is the likely source of these effects for older cohorts. Nevertheless, the effects are small.

Next, following Card and Lemieux (2001), we plot the logarithm of male-female relative rates of educational attainment, focussing on comparisons between age cohorts eligible for conscription and those on either side of age eligibility (Figure 3). For this exercise, we only have educational attainment at the Census date (August 2006), and we only have age rather than DOB for females. The linear interpolation between ages 52 and 62 estimates the counterfactual of no conscription lottery.<sup>15</sup> There is no clear evidence of divergence from the trend for the conscription eligible cohorts, either for degree or higher, or for trade/vocational or higher attainment. There is some suggestion that the data points for degree attainment are above the trend line for the conscription cohorts. But this is only for relatively young cohorts, which is counterintuitive and inconsistent with the reduced form effects of ballot outcome.

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<sup>13</sup> When a group means procedure is used (groups defined by each combination of cohort and ballot outcome) to calculate more conservative standard errors, the estimate remains significant ( $p=0.022$ ) with the youngest 4 cohorts excluded, and marginally significant ( $p=0.072$ ) with the sample restricted to the first 8 cohorts.

<sup>14</sup> The statistical significance of the overall post-school qualifications effect across all 16 cohorts is sensitive to the standard error calculation method ( $p=0.040$  with ordinary robust standard errors;  $p=0.203$  using group means). With the sample restricted to the first 8 cohorts, the estimate is significant using either method ( $p<0.001$  with ordinary robust standard errors;  $p=0.026$  using group means).

<sup>15</sup> The ages of 53 years and 61 years span the thresholds for age eligibility.

Analysis of historical enrolment data leads to similar conclusions. There was no deviation around 1965 in either the number of male students enrolled at University, nor in the gender ratio of enrolments (Booth and Kee 2010: Table 2). Apprenticeships data also show no obvious deviation around 1965, including in annual counts of people in-training as well as counts of commencements and withdrawals/cancellations (NCVER 2010).

Overall, Vietnam-era conscription only had a surprisingly small effect on educational attainment. The effect appears to be confined to balloted-in men and is likely due to draft avoidance behaviour. The result was to induce degree attainment by less than 0.1 percentage points amongst men born in 1945-1950. This proportion is about 20 times smaller than the corresponding effect estimated by Card and Lemieux (2001) for the United States for similar age cohorts. There are several likely contributing factors to this discrepancy. Card & Lemieux's findings are mostly driven by draft avoidance behaviour prior to the introduction of the draft lotteries. In Australia, the ballot outcome immediately removed the relevance of draft avoidance behaviour for almost three quarters of men (those balloted out). Second, education was far more prevalent in this era in the USA than in Australia.<sup>16</sup> This may suggest that university was not a feasible option for many Australian men. Third, unlike their US counterparts, apprenticeships were an alternative draft avoidance strategy for Australian men, although again the extent of induced trade qualifications is similarly small.

## 8. Summary and Conclusion

Conscription lotteries are a powerful resource for estimating the effects of military service. However, the interpretation of such estimates is not straightforward because the mechanisms through which conscription may affect men's lives are numerous. There are at least five potential mechanisms: a) direct effects of operational service and combat; b) direct and indirect effects of veterans' compensation and repatriation programs; c) removal from civilian life, usually at early adulthood; d) army training; e) draft avoidance behaviour.

The Australian National Service lotteries have only recently been utilised for identification purposes. But their institutional context also facilitates differentiation between the potential mechanisms to a

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<sup>16</sup> Age-specific Australian university enrolment rates seem unavailable for this era. However, the ratio of total university enrolments to total population for Australia in 1970 is 0.009 (Booth and Kee, 2010, Table 2). This is 4.5 times lower than the corresponding ratio for the USA (Snyder and Dillow, 2011: Table 197). Similar conclusions are gleaned from data on completed qualifications. In 2006, 15.2% of men born in 1945-1952 (excluding those arrived in Australia after the age of 20) had a degree or higher qualification. This is less than half that of men born in 1950-52 in the USA at 2000 (Angrist and Chen, 2011).



large, perhaps unprecedented, extent. Between-cohort differences in the probability of service in Vietnam allow separate identification of effects for men who remained in Australia and for those who served in Vietnam. Important differences between Australia and the US inform assessments of the role of veterans' compensation design. There is also little evidence of non-ignorable draft avoidance behaviour in the Australian context (Siminski, forthcoming; Appendix A).

Drawing on many data sources, we have estimated the effects of conscription on a range of health outcomes, economic outcomes, family outcomes and educational attainment. In many cases, the outcome measures are not ideal and we believe there is much potential for further research using additional data sources and years of data.

The findings presented in this paper can be summarised as follows:

- The Australian conscription lotteries had very little effect on educational attainment, either through draft avoidance behaviour or through repatriation benefits. This removes one mechanism to consider in interpreting the other effects that have been estimated.
- Across the outcomes considered, effects seem to be confined to those men who served in Vietnam. The only exception is for DVA disability compensation receipt, which is also slightly elevated for conscripts who remained in Australia.
- The effects on individual economic outcomes are very large. For men who served in Vietnam, labour force participation at 2006 was decreased by 39 percentage points. In the mid-1990s, the employment effect was around zero. This is mirrored by a rise in receipt of the DVA Disability Pension (Special Rate). Associated with this is a major compression of conscripts' income distribution, but no effect on mean income. These results contrast sharply with corresponding results for the US, which Siminski (forthcoming) suggests may relate to differences in the incentives of the respective veterans' compensation systems.
- Health effects are difficult to measure and interpret. There are very large effects on self-reported disability and Disability Pension receipt. Mortality effects are not statistically significant, but are not estimated precisely. As these men enter age groups with higher mortality rates, estimated effects on relative mortality rates will become more precise.
- There is little evidence that conscription affected the probability of being married, family size, or residential stability (all at 2006). This is consistent with evidence from the USA, and suggests that the ability of veterans to live stable domestic lives has not been compromised. However, this result has other potential interpretations and does not account for the possibility of divorce followed by remarriage.



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Table 1 2SLS Estimates of Effects on Health and Disability Measures

Endogenous variables	mean of dependent variable	Estimated 2SLS effects			
		(1)	(2)	(3)	
<u>A. Dependent Variable: Died between 1994 and 2007</u>					
Army service ( <i>r</i> )	.044	.0012	(.0022)	.0021	(.0043)
Army service in Vietnam ( <i>v</i> )		.0022	(.0062)	-.0030	(.0121)
<u>B. Dependent Variable: Disabled (self-reported) at August 2006</u>					
Army service ( <i>r</i> )	.051	.050***	(.003)	.003	(.005)
Army service in Vietnam ( <i>v</i> )		.153***	(.008)	.147***	(.015)
<u>C. Dependent Variable: Receiving Veterans' Disability Compensation (Special Rate) at June 2009</u>					
Army service ( <i>r</i> )	.024	.184***	(.002)	-.003	(.003)
Army service in Vietnam ( <i>v</i> )		.570***	(.007)	.578***	(.011)
<u>D. Dependent Variable: Receiving Veterans' Disability Compensation at June 2009</u>					
Army service ( <i>r</i> )	.043	.281***	(.003)	.017***	(.004)
Army service in Vietnam ( <i>v</i> )		.857***	(.009)	.815***	(.014)

Notes: This table shows 2SLS estimates under various specifications. Column (1) shows effects of army service. Column (2) shows effects of army service in Vietnam under the assumption of zero effect for men who served only in Australia. Results in Column (3) are from a specification with both army service and service in Vietnam as endogenous regressors. Cohort dummies are included in each regression. Robust standard errors are shown in parentheses.  $N = 840,071$  in each regression in Panel A; 675,832 in Panel B; 752,088 in Panels C and D. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table 2 2SLS Estimates of Effects on Economic Outcomes

Endogenous variables	mean of dependent variable	Estimated 2SLS effects				
		(1)	(2)	(3)		
<u>A. Dependent Variable: Employed (2006)</u>						
Army service ( <i>r</i> )	.719	-0.120***		-0.002	(.010)	
Army service in Vietnam ( <i>v</i> )				-0.366***	(.015)	-0.361***
<u>B. Dependent Variable: Unemployed (2006)</u>						
Army service ( <i>r</i> )	.028	-0.010***		-0.006	(.004)	
Army service in Vietnam ( <i>v</i> )				-0.026***	(.005)	-0.012
<u>C. Dependent Variable: Weekly Income (\$) (2006)</u>						
Army service ( <i>r</i> )	903	-11.50	(7.62)	1.88	(17.04)	
Army service in Vietnam ( <i>v</i> )				-36.56	(20.49)	-41.08
<u>D. Dependent Variable: Log(Income) (2006)</u>						
Army service ( <i>r</i> )	6.55	.004	(.010)	.003	(.020)	
Army service in Vietnam ( <i>v</i> )				.009	(.026)	.000
<u>E. Dependent Variable: Low Income (less than \$250 / week) (2006)</u>						
Army service ( <i>r</i> )	.160	-0.028***	(.004)	.002	(.009)	
Army service in Vietnam ( <i>v</i> )				-0.087***	(.012)	-0.093**
<u>F. Dependent Variable: High Income (\$800 or more / week) (2006)</u>						
Army service ( <i>r</i> )	.483	-0.035***	(.006)	.007	(.012)	
Army service in Vietnam ( <i>v</i> )				-0.113***	(.015)	-0.131***



<u>G. Dependent Variable: Log(Earnings) (1992-93)</u>							
Army service ( <i>r</i> )		.003	(.009)			-.018	(.019)
Army service in Vietnam ( <i>v</i> )	10.23			.022	(.021)	.065	(.046)
<u>H. Dependent Variable: High Earnings (top unconditional quintile) (1992-93)</u>							
Army service ( <i>r</i> )		-.011*	(.005)			-.010	(.011)
Army service in Vietnam ( <i>v</i> )	.200			-.026	(.015)	-.001	(.031)
<u>I. Dependent Variable: Low Earnings (bottom unconditional quintile) (1992-93)</u>							
Army service ( <i>r</i> )		-.003	(.005)			.009	(.012)
Army service in Vietnam ( <i>v</i> )	.200			-.014	(.015)	-.036	(.031)

Notes: See Table 1 notes. *N* = 675,832 in each regression in Panels A and B; 650,185 in Panels C, E and F; 633,904 in Panel D, 32 (group means) in Panel G, 689,374 in panels H and I.

Table 3 2SLS Estimates of Effects on Family Outcomes in 2006

Endogenous variables	mean of dependent variable	Estimated 2SLS effects					
		(1)	(2)	(3)	(4)	(5)	(6)
<u>A. Dependent Variable: Married</u>							
Army service ( <i>r</i> )		.012*	(.006)			.005	(.012)
Army service in Vietnam ( <i>v</i> )	.707			.033*	(.015)	.020	(.033)
<u>B. Dependent Variable: Divorced or Separated</u>							
Army service ( <i>r</i> )		-.001	(.005)			.004	(.010)
Army service in Vietnam ( <i>v</i> )	.185			-.007	(.013)	-.016	(.028)
<u>C. Dependent Variable: Number of People in Family</u>							
Army service ( <i>r</i> )		-.018	(.014)			-.006	(.033)
Army service in Vietnam ( <i>v</i> )	2.23			-.052	(.036)	-.037	(.087)
<u>D. Dependent Variable: Moved address in previous 5 years</u>							
Army service ( <i>r</i> )		.003	(.006)			.025*	(.012)
Army service in Vietnam ( <i>v</i> )	.284			-.007	(.015)	-.067*	(.033)

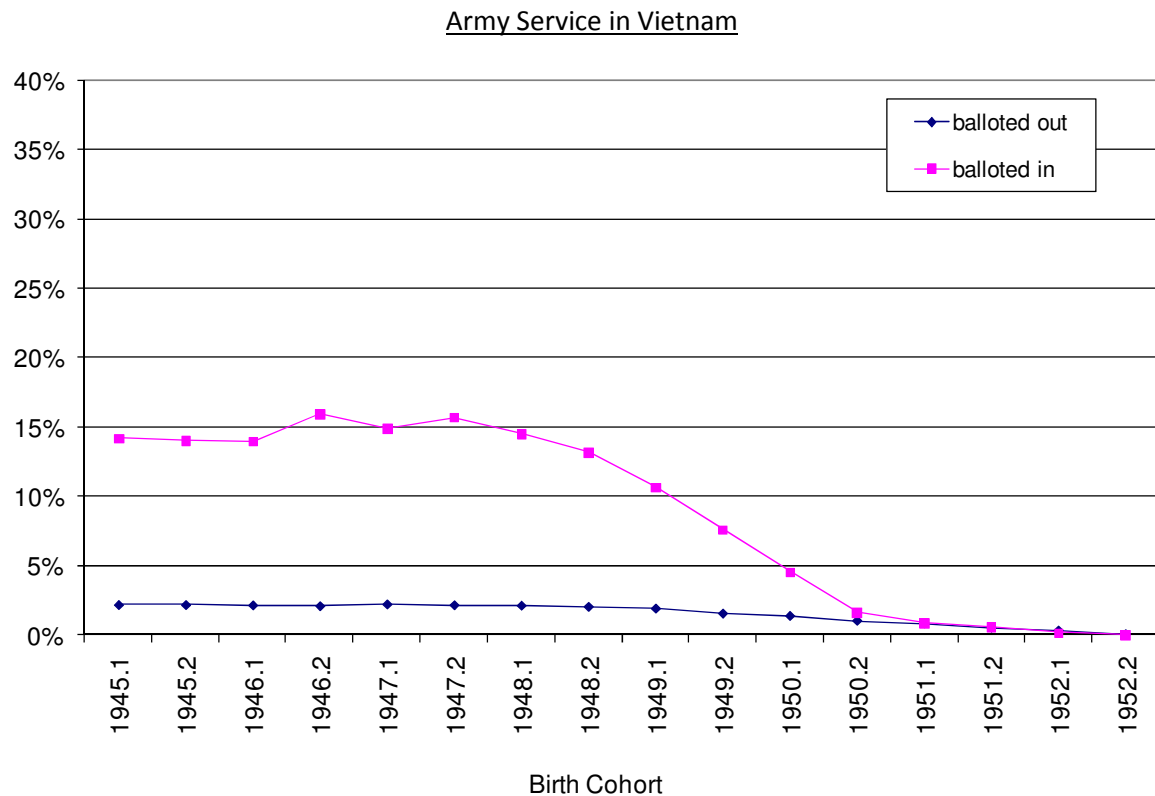
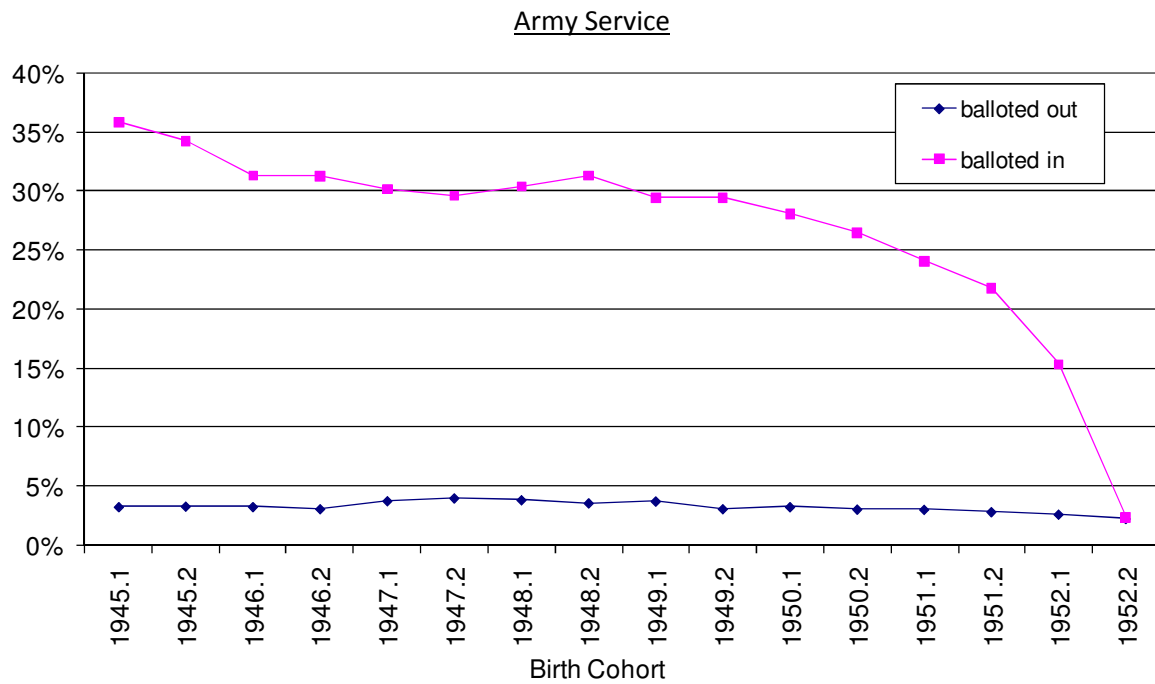
Notes: See Table 1 notes. *N* = 569,889 in each regression (see footnote 9).

**Table 4 Reduced Form Effects of Ballot Outcome on Educational Attainment (2006)**

	Bachelor Degree or Higher		Trade or Vocational		Any post-school qualifications	
Explanatory variable	(1)		(2)		(3)	
<u>A. Effect of ballot outcome</u>						
Ballot outcome (all cohorts)	.0010	(.0010)	.0019	(.0013)	.0029*	(.0014)
Ballot outcome (cohorts 1-8)	.0035*	(.0014)	.0036	(.0019)	.0072***	(.0020)
Ballot outcome (cohorts 9-16)	-.0014	(.0015)	.0002	(.0019)	-.0012	(.0020)
<u>B. Effect of ballot outcome by cohort</u>						
Ballot outcome, cohort 1	.0033	(.0035)	.0129**	(.0049)	.0163**	(.0053)
Ballot outcome, cohort 2	.0037	(.0038)	.0041	(.0051)	.0078	(.0055)
Ballot outcome, cohort 3	.0078	(.0040)	.0082	(.0055)	.0160**	(.0059)
Ballot outcome, cohort 4	.0013	(.0040)	.0067	(.0054)	.0080	(.0058)
Ballot outcome, cohort 5	.0016	(.0038)	.0040	(.0052)	.0055	(.0055)
Ballot outcome, cohort 6	.0017	(.0042)	-.0033	(.0056)	-.0016	(.0059)
Ballot outcome, cohort 7	-.0019	(.0039)	.0028	(.0053)	.0009	(.0056)
Ballot outcome, cohort 8	.0125**	(.0045)	-.0101	(.0059)	.0024	(.0062)
Ballot outcome, cohort 9	.0017	(.0039)	.0069	(.0052)	.0085	(.0055)
Ballot outcome, cohort 10	-.0004	(.0048)	-.0109	(.0061)	-.0113	(.0065)
Ballot outcome, cohort 11	.0033	(.0047)	-.0010	(.0061)	.0023	(.0065)
Ballot outcome, cohort 12	.0037	(.0042)	.0048	(.0054)	.0085	(.0057)
Ballot outcome, cohort 13	-.0028	(.0038)	-.0064	(.0049)	-.0092	(.0051)
Ballot outcome, cohort 14	-.0071	(.0039)	.0045	(.0050)	-.0025	(.0052)
Ballot outcome, cohort 15	-.0025	(.0039)	-.0016	(.0049)	-.0041	(.0052)
Ballot outcome, cohort 16	-.0042	(.0041)	.0029	(.0052)	-.0013	(.0055)
<i>N</i>	675,832		675,832		675,832	

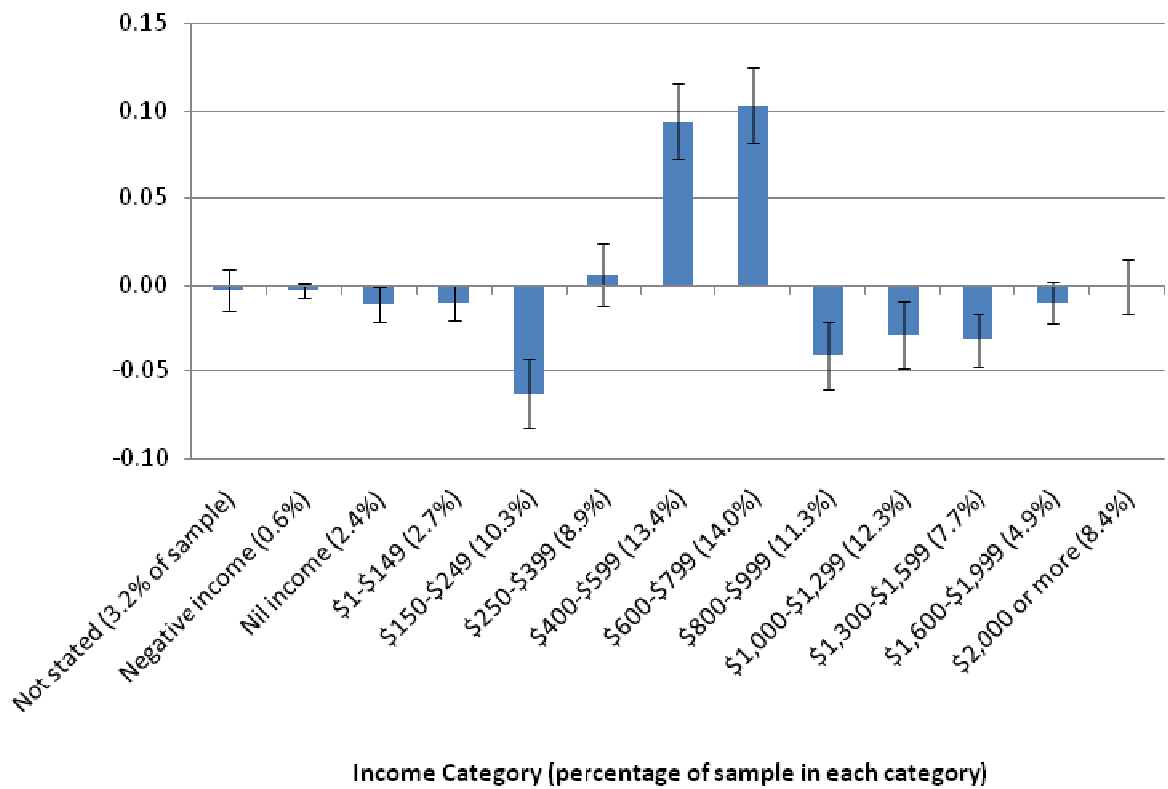
Notes: Each column shows reduced form effects of ballot outcome on educational attainment with a different dependent variable represented in each column. Panel A shows results from three separate regressions in each of the three columns (total of 9 regressions). Panel B shows results from 1 regression in each column (total of 3 regressions). Cohort dummies are included in each regression. Robust standard errors are shown in parentheses. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Figure 1 Graphical Depiction of First Stage Results – Share of Men with Army Service by Birth Cohort and Ballot Outcome



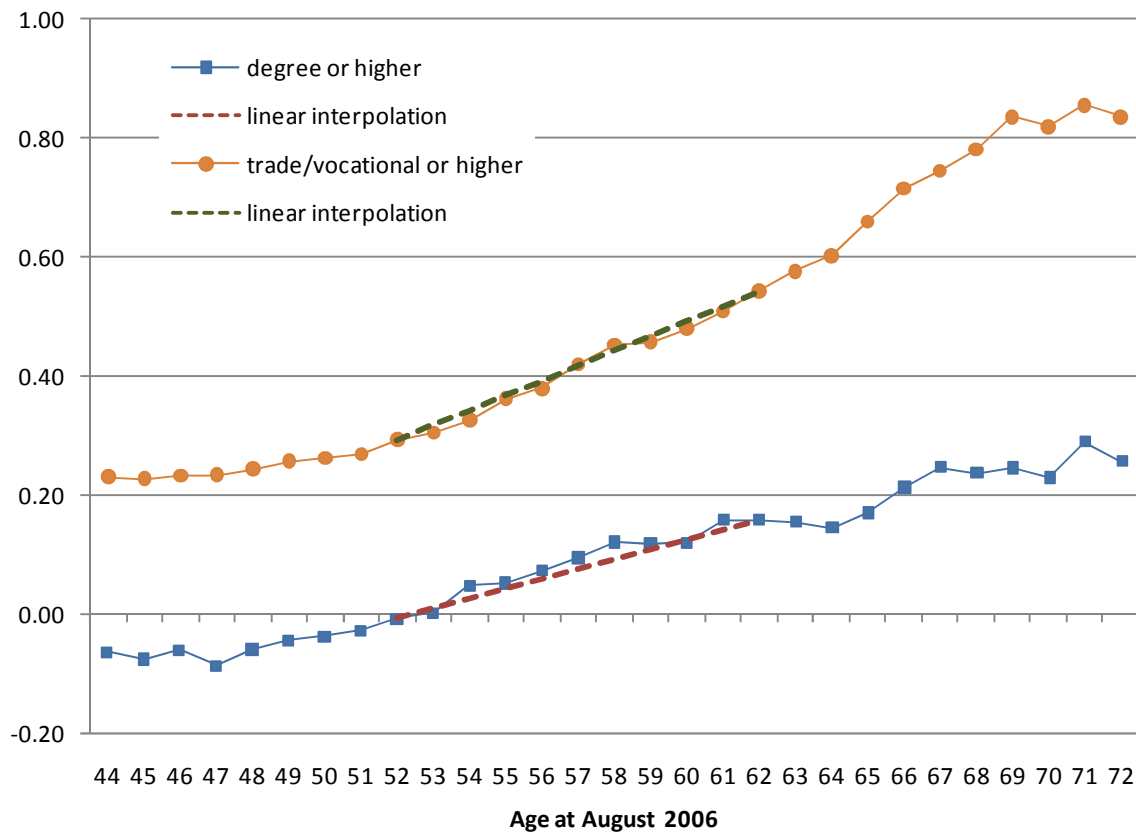
Notes: This figure shows the proportion of men with army service (upper panel) and army service in Vietnam (lower panel) by ballot outcome and birth cohort. The corresponding regression results are in Table A.1.

Figure 2 Estimated 2SLS Effects of Army Service in Vietnam on Probability of Income in each Census Band (2006)



Notes: Each bar shows the 2SLS estimate of the effect of army service in Vietnam on having personal income within each Census income band (with 95% CIs) (13 separate regressions, each with a binary dependent variable).

Figure 3 Male-Female Relative Educational Attainment by Age at August 2006



Notes: This figure follows the approach of Card & Lemieux (2001). It shows the log of male-female educational attainment differentials by single year of age (at August 2006) with linear interpolations for the draft eligible cohorts.

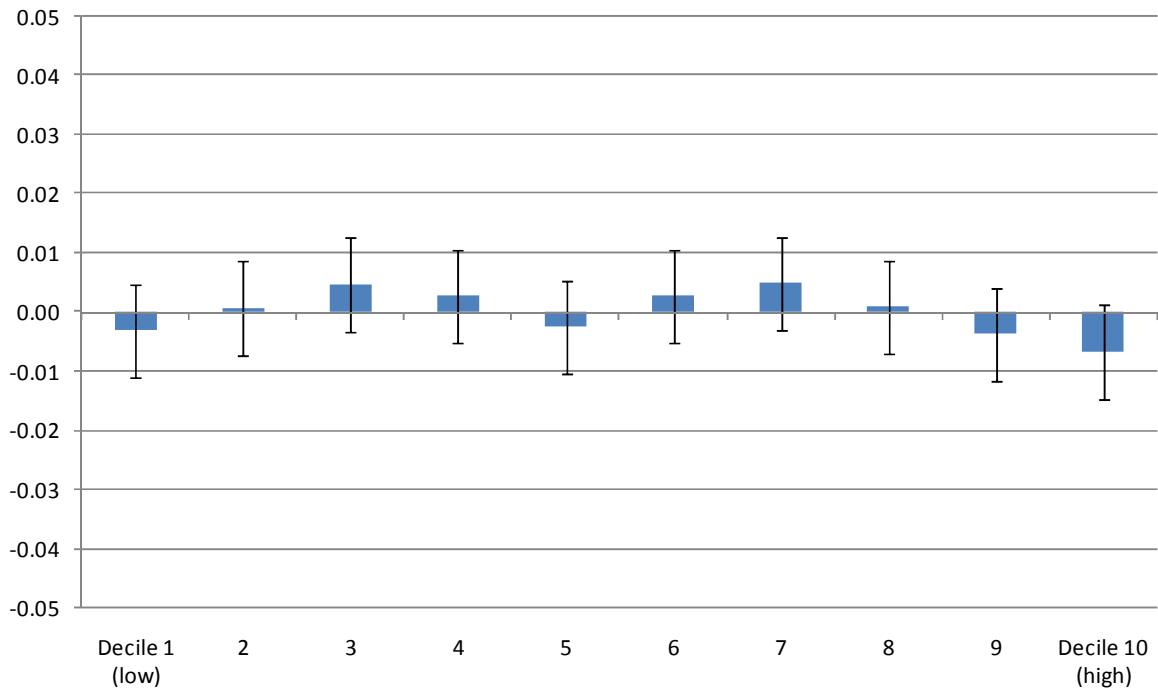
## Appendix: Additional Results

Table A.1 First Stage Regression Results

	Army Service ( $r$ )	Army Service in Vietnam ( $v$ )
Ballot outcome, cohort 1	.3267*** (.0033)	.1198*** (.0024)
Ballot outcome, cohort 2	.3097*** (.0038)	.1183*** (.0028)
Ballot outcome, cohort 3	.2811*** (.0043)	.1179*** (.0032)
Ballot outcome, cohort 4	.2826*** (.0043)	.1382*** (.0034)
Ballot outcome, cohort 5	.2649*** (.0040)	.1262*** (.0031)
Ballot outcome, cohort 6	.2570*** (.0044)	.1350*** (.0035)
Ballot outcome, cohort 7	.2662*** (.0041)	.1233*** (.0031)
Ballot outcome, cohort 8	.2782*** (.0047)	.1111*** (.0034)
Ballot outcome, cohort 9	.2576*** (.0039)	.0872*** (.0026)
Ballot outcome, cohort 10	.2639*** (.0049)	.0605*** (.0029)
Ballot outcome, cohort 11	.2487*** (.0048)	.0318*** (.0022)
Ballot outcome, cohort 12	.2350*** (.0040)	.0061*** (.0012)
Ballot outcome, cohort 13	.2107*** (.0033)	.0005 (.0008)
Ballot outcome, cohort 14	.1899*** (.0034)	.0008 (.0007)
Ballot outcome, cohort 15	.1275*** (.0030)	-.0013** (.0004)
Ballot outcome, cohort 16	.0011 (.0015)	-.0002 (.0001)
<i>F-stat for IVs</i>	8,485	2,709
<i>Angrist-Pischke Multivariate F-stat</i>	1,804	578
<i>N</i>	868,606	868,606

Notes: This table shows first stage regression results which are relevant to most of the 2SLS estimates shown throughout the paper. Each regression also includes a constant and 15 cohort dummies. Robust standard errors are in parentheses. Cohort 1 is the oldest 6-month birth cohort. These results correspond with the visual representation in Figure 1 and with Table 2 of Siminski (forthcoming). Where it is not possible to exclude recent migrants from the second stage databases (veterans compensation data and tax data), we adjust the first stage data accordingly to include men who immigrated later, whose DOB distribution is assumed orthogonal to ballot outcome. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

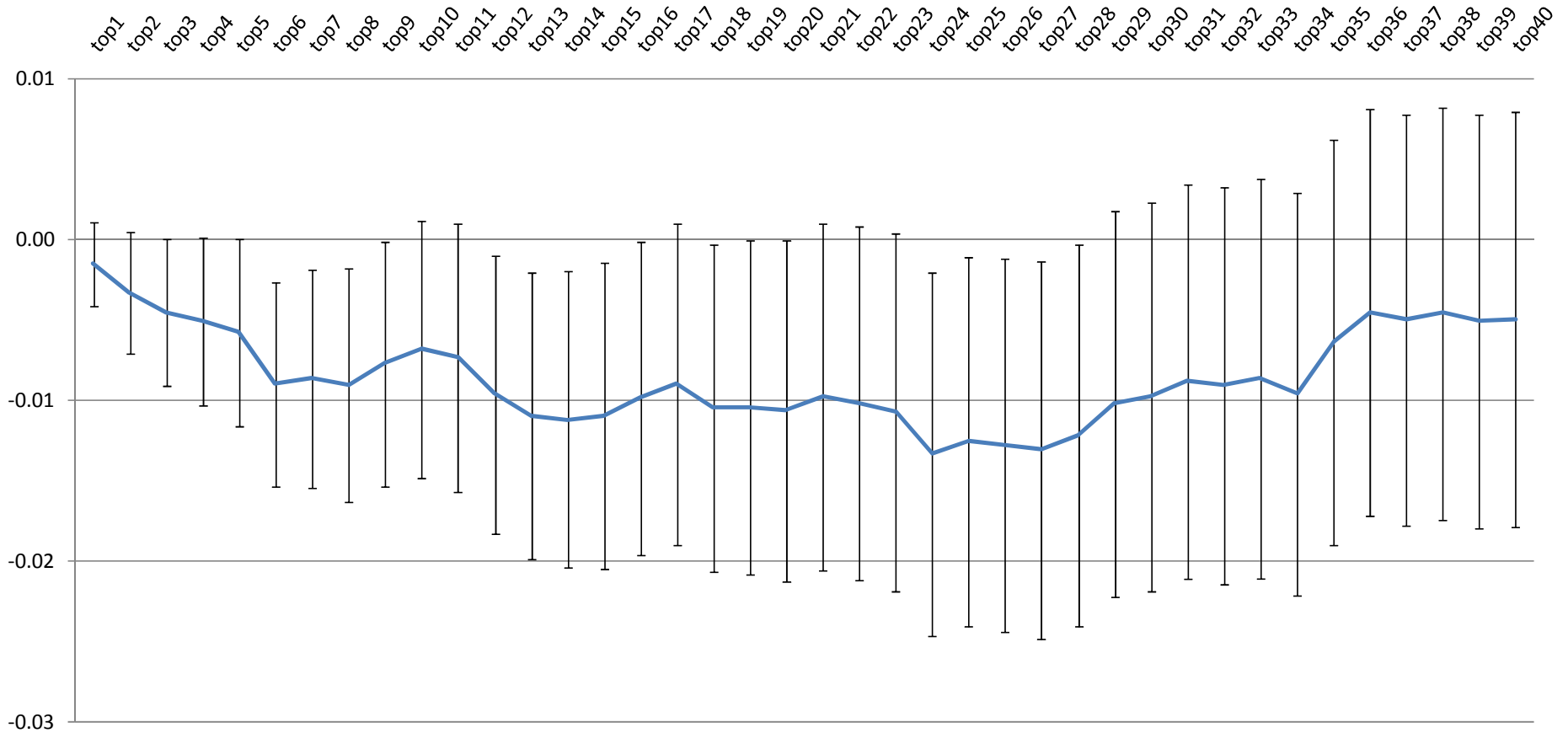
Figure A.1 Estimated 2SLS Effects of Army Service on Probability of Earnings in each Decile - Tax Return Data (1992-93)



Notes: Each bar shows the 2SLS estimate of the effect of army service on having income within each unconditional decile of the earnings distribution (with 95% CIs) (10 separate regressions, each with a binary dependent variable).  $N = 689,374$  in each model.



Figure A.2 Estimated 2SLS Effects of Army Service on Probability of High Earnings (Top x Per Cent of Unconditional Earnings Distribution) -Tax Return Data (1992-93)



Notes: This Figure shows 2SLS estimates of the effect of army service on the probability of having ‘high’ earnings in 1992-93 (with 95% CIs). It tests sensitivity to the threshold which defines ‘high’ earnings, using 40 separate models. The high earnings definition varies from the top 1% of earnings (labelled top1) to the top 40% (labelled ‘top40’). ‘Top20’ corresponds to the estimate in Table 2, Panel H, Column (1). ‘Top10’ corresponds to the far right column in Figure A.1.  $N = 689,374$  in each regression.