The Impact of Peacekeeping on Post-Deployment Earnings for Swedish Veterans

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Abstract

We study the effect of peacekeeping on post-deployment earnings for military veterans. Using Swedish administrative data, we follow a sample of more than 11,000 veterans who were deployed for the first time during the period 1993-2010 for up to nine years after returning home. To deal with selection bias, we use difference-in-differences propensity score matching based on a rich set of covariates, including measures of individual ability, health and pre-deployment labour market attachment. We find that, overall, veterans' post-deployment earnings are largely unaffected by their service. Even though Swedish veterans in the studied period tend to outperform their birth-cohort peers who did not serve, we show that this advantage in earnings disappears once we adjust for non-random selection into service.

Keywords: Military veterans; peacekeeping; earnings

JEL Codes: J01, J20, J45, H56

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1 Introduction

Sweden and the Swedish Armed Forces have a long history of contributing to international peace operations.[1] Since the 1950s, a large number of Swedish men and women have been deployed in many locations worldwide. There is an ongoing debate about the well-being of military veterans and the long-term consequences of deployment. Some have argued that military veterans are harmed by their service and that they struggle to re-integrate into civilian society (see, for example, Strömberg et al., 2013; and Häggström, 2013), whereas others have warned against letting negative aspects of service dominate the narrative (Neovius et al., 2014; Ramnerup, 2013).

In this paper, we contribute to this debate by presenting novel evidence on the effect of peacekeeping on future earnings. Using rich data from administrative sources, we follow a sample of more than 11,000 Swedish veterans, who were deployed for the first time during the period 1993-2010, for up to nine years after returning home. As such, our paper is the first to study labour market outcomes for Swedish military veterans.

We add to a relatively large international literature on the relationship between military service and labour market outcomes. One area of study focuses on the impact of military service during wartime, such as the Vietnam War. A well-known study by Angrist (1990) found that Vietnam War veterans experienced a negative effect on their earnings, attributed to the loss of civilian labour market experience. However, this negative effect appears to diminish over time (Angrist et al., 2011; Siminski, 2013). There is also evidence that more recent U.S. military veterans have had better labour market outcomes compared to earlier ones (Greenberg et al., 2022; Makridis & Hirsch, 2021). Another area of study examines the effects of peacetime conscription, with most studies focusing on European countries. These studies typically find that military conscription service has either negative or no effects on subsequent earnings (Bauer et al., 2012; Bingley et al., 2020; Grenet et al., 2011;

[1] Here, the term *peace operations* refers to military operations based on a mandate from the UN Security Council, including both *peace keeping* and *peace enforcement*. We use the term *peacekeepers* to describe those serving in all types of UN-mandated peace operations.

Hubers & Webbink, 2015; Imbens & Klaauw, 1995).

We contribute to this literature by focusing on peacekeeping. To our best knowledge, there are no studies that specifically investigate the effect of deployment to international peace missions on long-term earnings.[2] Military service in peace operations differs from war time service and peace-time conscription in many important aspects. Rather than being involved in a violent conflict as combatants, peacekeepers are impartial actors who "protect civilians, actively prevent conflict, reduce violence, strengthen security and empower national authorities to assume these responsibilities" (United Nations, 2021). Nonetheless, peacekeepers are commonly exposed to stressful events and violence in a way that distinguishes their experience from that of those who are going through peace-time military training.

There are also methodological reasons for studying earnings effects for Swedish veterans, specifically. When thinking about the effects from military service, it is important to distinguish between effects created by the military experience *per se* and those stemming from incentives created by the institutional arrangements regarding military service. In some countries, most notably the U.S., service in the military gives access to a range of government programmes aimed at veterans. These programmes might involve cash grants, as well as in-kind support, that, in the Nordic welfare states, are rarely tied to military service. In Sweden there is no specific compensation system aimed at military veterans; as Swedish citizens, they are fully covered by the general healthcare and social insurance system, and have access to the higher education system like anyone else. This allows us to study the effect of military deployment itself, net of the incentives created by various veteran compensation schemes and educational benefits, that are typical in many other countries.

[2] There are, however, some Nordic studies that look at other outcomes related to the labour market. Elrond et al. (2019) studied the labour market affiliation after deployment for Danish veterans returning in the years 2002-2012 and concluded the veterans fared better in the labour market within five years of returning home compared to non-deployed controls. Lyk-Jensen and Pedersen (2019) analysed the financial situation for a sample of Danish veterans deployed in 2002 and concluded that the veterans had lower net debt five years after return compared to a control group from the same birth-cohort.

Measuring the causal effects of military deployment using observational data is complicated by issues related to selection. All veterans, from the time period we study, volunteered for service and were typically civilians contracted for temporary international military service (Hedlund, 2011). They were not only self-selected into service based on motivation and preferences, but were also actively screened and selected by the military organisation before deployment. This dual selection process makes it highly likely that the characteristics of those who were deployed to an international peace mission differ from the characteristics of those who were not. Indeed, previous research has shown that Swedish peace veterans have higher cognitive ability, lower prevalence of mental health problems and better results in psychological assessments than the general population (Pethrus, Frisell, et al., 2019; Pethrus et al., 2017; Pethrus, Reutfors, et al., 2019). These pre-deployment differences are bound to induce selection bias, if we simply compare the earnings of those who served in a peacekeeping mission with the outcomes of those who did not; a wide range of earlier studies, starting with the seminal paper on U.S. Vietnam War veterans by Angrist (1990), have shown that failure to control for the selection mechanisms that determine service can lead to severely biased estimates of the effects of military service on labour market outcomes.

To deal with non-random selection into service, we estimate earnings effects from deployment using matching methods combined with difference-in-differences. Combining high quality administrative data from several sources, we have access to a rich set of individual characteristics measured before the soldiers were deployed to international service, including measures of cognitive ability, psychological capacity, military medical assessments, pre-deployment labour market history and demographic background variables. This approach not only allows us to adjust for observed pre-deployment differences between veterans and non-veterans, but also for selection bias stemming from unobserved heterogeneity that are constant over time, across individuals (Heckman et al., 1998; Heckman et al., 1997; J. A. Smith & Todd, 2005).

We find that veterans' post-deployment earnings are, on average, largely unaffected by their service. Even though Swedish veterans from the studied period tend to outperform their birth-cohort peers who did not serve, we show that this earnings advantage disappears once we adjust for non-random selection into service. For the full sample of veterans, the point estimates of the earnings effects are close to zero over the nine years of the follow-up period, with small enough confidence intervals to rule out any large long-term earnings effects. Even though the veterans in our sample spent time out of the civilian workforce, and most of them did not pursue a military career after deployment, they clearly managed to keep up in terms of earnings.

We do, however, find some indications that the zero effect for the full sample of veterans hides differences in effects between different missions. Veterans deployed to Bosnia in the early and mid-1990s appear to have suffered a (transitory) earnings penalty, whereas those deployed to Afghanistan in the 2000s appear to have experienced a earnings premium. We provide suggestive evidence that the earnings losses for those who served in the early and mid-1990s are due to differences in employment status after returning home.

The rest of this paper is organised as follows. In the next section, we briefly discuss some potential causal pathways trough which military deployment might affect the subsequent earnings of veterans. In Section 3, we present the institutional setting surrounding international military service during the studied period. In Section 4 and Section 5, we present the data and our empirical strategy. In the last two sections, we present the results and conclude the paper with a discussion.

2 Potential Mechanisms

Why would deployment to an international peace operation influence the labour market outcomes of veterans? There are several potential channels through which military deployment might have an impact on the subsequent labour market outcomes of those who have served. In summary, the literature suggests that military deployment may affect labour market outcomes through: (1) human capital accumulation (or depreciation); (2) negative effects on health; (3) signalling; and (4) institutional arrangements surrounding military service.

One starting point in thinking about military service in relation to civilian labour market outcomes is human capital theory (see seminal work by Becker, 1964; Mincer, 1974; Schultz, 1961). Human capital theory tells us to focus on skills and abilities acquired by the veteran and to ask whether or not these can be used to raise productivity in a civilian career; veterans who acquire skills during their service that are easy to use in civilian jobs are likely to be better off than those whose skills are not so easily translated to the civilian labour market (Humensky et al., 2013). Indeed, Kleykamp (2009) found that employers tend to prefer hiring veterans who have experience in fields that have civilian equivalents such as medical, clerical, and IT.

Although specific types of military training, such as leadership training (Grönqvist & Lindqvist, 2016), have been found to be beneficial to a civilian career, a range of previous studies show that, in general, time spent in the military appears to be a poor substitute for civilian labour market experience. Angrist (1990, p. 331) concludes that U.S. veterans that served in Vietnam ”earn less because their military experience is only a partial substitute for the civilian labor market experience lost while in the armed forces“. Lyk-Jensen (2018, p. 257), studying Danish conscripts, notes that “a clear consequence of military service is that it interrupts studies and delays entrance into the labor market”. Bingley et al. (2020, p. 39) show that the earnings penalty resulting from this disruption of the civilian career tends to be ”borne by men with the best labour market prospects”, highlighting differences in the opportunity costs of service across individuals. There are, however, some indication that military service might be beneficial for less advantaged groups. For example, in a recent study, Greenberg et al. (2022) find that military service significantly closes the earnings gap between groups by providing minorities with stable, well-paying jobs and opportunities for higher-paid post-service employment.

In the context of violent conflict, exposure to combat and traumatic events during deployment can have negative effects on the mental health of soldiers (see Dobkin and Shabani, 2009 and Cesur et al., 2013), which, in turn, may lead to adverse outcomes in the civilian labour market after return home (Amick et al., 2018; Anderson & Mitchell, 1992; Ramchand et al., 2015; Savoca & Rosenheck, 2000; M. W. Smith et al., 2005). For example, Armey and Lipow (2016, p. 771) show that exposure to violent combat made it less likely that U.S veterans from Afghanistan and Iraq would make use of their educational benefits and conclude that "exposure to actual combat and violence is detrimental and can handicap veterans' efforts to get on with their lives". However, in the context of our study, it is important to note that previous studies using Swedish data tend to emphasise the physical and mental well-being of former peacekeepers (Michel et al., 2003, 2007; Pethrus, Frisell, et al., 2019; Pethrus et al., 2017; Pethrus, Reutfors, et al., 2019; Pethrus et al., 2022).

An alternative view of the potential link between military service and the civilian labour market might be that service does not improve any skills of value in the civilian sector, but provides civilian employers with information about individual traits and characteristics (on signalling, see seminal works by Arrow et al., 1973 and Spence, 1978). The signalling value of military service can be both positive and negative. On one hand, veteran status may signal to employers that an individual is reliable and productive (Kleykamp, 2009). On the other hand, employers may, for example, be concerned with high rates of post-traumatic stress disorder (PTSD) among combat veterans and the potential costs of dealing with PTSD in the workplace (Kleykamp, 2013). Unfortunately, in empirical applications, it is hard to distinguish between human capital formation and signalling; both approaches are consistent with the observation that experience from the military impacts the outcomes on the labour market (Hanes et al., 2010).

Government programmes aimed at veterans might also create work related incentives.

In the U.S., for example, the so-called GI Bill, which provides veterans with educational benefits, has been shown to have a strong positive on veterans' schooling (Angrist & Chen, 2011; Barr, 2015). Indeed, Kleykamp (2013) argue that individuals (in the U.S.) with relatively few opportunities for formal higher education might actually enlist as a means of earning benefits to pay for later education. Moreover, the compensation given to injured veterans might also create work related incentives, which affects labour market outcomes. Angrist et al. (2010) studied the long-term effects of Vietnam-era military service on health and work and conclude that employment consequences of war time injuries may have as much to do with incentives created by the disability compensation programme as with a medical inability to work. Similarly, Siminski (2013) studied the effect of Vietnam-era military service in Australia and conclude that the negative employment effects found was likely to be explained primarily by incentives embedded in Australia's veterans' compensation system.

3 Recruitment for Swedish Peace Operations

In total, around 20,000 Swedish men and women were deployed to international peace operations between 1993 and 2010. Early on, Sweden almost exclusively contributed to international peace operations organised by the UN. This policy changed in 1995, when Swedish participation in operations sanctioned, but not necessarily led, by the UN, became the guiding principle (Ministry of Defence, 2010b). Consequently, participation of Swedish troops in UN-sanctioned operations organised by NATO became common during the period we studied. In the 1990's the largest troop deployments took place in former Yugoslavia (UNPROFOR, KFOR, SFOR, IFOR) and Lebanon (UNIFIL). Engagements in Afghanistan (ISAF), and to some degree Liberia (UNMIL), gradually took over after 2000.

In the period we studied, participation in an international peace mission was strictly voluntary; the men and women who served abroad were typically former conscripts contracted for temporary international military service (Hedlund, 2011). International volunteers were organised and employed in a separate voluntary force for international operations (the International Military Force, or *Utlandsstyrkan*). Consequently, the military units that were deployed were, in principle, temporary units with temporary commanders (Ministry of Defence, 2007).[3]

Recruitment for the International Military Force was typically conducted in two periods per year. If a former conscript met the required standards for a specific international mission, he or she was eligible to sign a temporary employment contract for service in the International Military Force. Typically, the soldiers in the International Military Force served abroad for around six months, with the addition of several weeks of mission-specific training in Sweden before deployment. Swedish law (SFS 1994:2076) requires employers to grant a leave of absence to employees who participate in an international military mission. Deployed soldiers received monetary compensation according to a collective agreement that, in addition to an elevated entry level wage, included a range of supplements and allowances (see Ministry of Defence, 2007).

After a first screening of eligibility and a check against the police crime- and suspicion register, the applications were reviewed by the unit commander and some of his or her closest officers and applicants were interviewed (Ministry of Defence, 2007). During a final selection stage after application and acceptance for international service, the unit commanders evaluated the recruits during their mission specific training period prior to deployment. If someone was judged unfit for international service, that person could be rejected at this late stage, even though such separations were unusual.[4]

[3]Although the military manpower system in Sweden relied heavily on mandatory male military service throughout the 20$^{\text{th}}$ century, international military service has always been voluntary for conscripts. In fact, the Swedish Act on Liability for Total Defense Service does not allow conscripts to be used for international military operations. The voluntary nature of service in international missions also comprised employed personnel of the Swedish Armed Forces (e.g., professional officers). Regular personnel of the Swedish Armed Forces who participated in international missions were on a temporary leave of absence during their service in the International Military Force (Ministry of Defence, 2010a).

[4]According to interview with Capt. Mats Kjäll (ret.), recruitment officer at the Swedish Armed Forces International Centre (Swedint) from 1991-2001, on 2021-04-22.

The Swedish transition to an all-volunteer force, in 2010, meant the end of the International Military Force as a separate military entity. Since 2011, international operations have been organised under regular military units of the Swedish Armed Forces. Today, the voluntary elements of international service is also less pronounced; in 2010, the Swedish Armed Forces announced that, as a guiding principle, service in international operations had become an obligation of all employed personnel (Ministry of Defence, 2010a) .

4 Data and Sample

Our data combine military personnel records with administrative data on earnings, allowing us to observe outcomes for up to nine years after deployment. We used population-register longitudinal data, administrated by Statistics Sweden, containing annual earnings and transfers related to the Swedish social insurance system, as well as a range of basic demographic information, such as gender, family situation, region of residence, country of birth and level of schooling. The data originates from administrative records and covers the period 1990-2019. We linked this data to administrative data on veterans, obtained from the Swedish Armed Forces Veteran Centre, and from the military service records of the Swedish Defence Conscription and Assessment Agency and the Military Archives. All data analysis and processing used de-identified data available through Statistics Sweden's platform for access to microdata.[5]

Our study population consists of all individuals who underwent conscription testing in Sweden during the period 1990 to 2010 and subsequently initiated their military conscription service.[6] Based on this population of former conscripts, we constructed two groups: a treated group and an untreated group.

In order to be selected into our treated group, an individual must have been deployed

[5]The study has been reviewed and approved by the Swedish Ethical Review Agency (Etikprövningsmyndigheten) 2021-05-19.

[6]Individuals who initiated their conscription service were identified via conscription compensation payments (*värnpliktsersättning*).

in an international peace operation (involving the deployment of military troops) during the period 1993 to 2010. We focused on the first deployment that took place for each veteran during this time period.[7] The untreated group consists of individuals from the study population who had not been deployed to an international peace operation up until, and including, a specific year under consideration. For example, our treated group for 1994 consists of individuals who were deployed for the first time in 1994, whereas the group of untreated individuals had either not been deployed at all 1993 to 2010, or was deployed for the first time in 1995 or later. That is, in this case, we allowed for deployment of an untreated individual to occur 1995 or later as we did not want to condition on post-deployment events, which could possibly bias our estimates (see, e.g., Rosenbaum, 1984; Stuart, 2010; Wooldridge, 2005).

Using these criteria, we obtained a sample of 11,279 treated individuals (i.e., the veterans) and an untreated (i.e., non-veteran) sample consisting of 4,736,601 observations on 366,119 individuals. To form a comparison group of potential controls, we matched each veteran deployed in a specific calendar year to all untreated individuals from the same birth-cohort and for the same calendar year. For example, a veteran who was born in 1973, and was deployed in 1994, is matched to observations of individuals born in 1973 who, in 1994, had not been deployed up until, and including, 1994.[8] In total, the matched comparison group of potential controls consist of 4,518,789 (weighted) observations on 363,120 individuals.[9]

[7]Of the 11,279 veterans in our sample, 4,959 individuals had been deployed more than once during 1990-2019.

[8]Since we did not condition on future events, veterans can appear in the group of potential controls for time periods prior to their deployment. That is, an individual who was deployed for the first time in 1997 might have been selected as a matched comparison for an individual deployed in 1994. The opposite, however, was not possible: an individual deployed in 1994 could not be selected as a matched comparison for an individual that was deployed in 1997.

[9]Note that since we have repeated observations for each individual across calendar time, a single untreated individual might appear as a potential match for several treated individuals across deployment years. For example, a 1994 observation of an untreated individual born in 1973 is a potential control for a treated individual born in 1973 and deployed in 1994; an observation in 1995 of the same untreated individual is a potential match for treated individuals born in 1973 and deployed in 1995; and so on. Hence, due to the panel structure of the data, the number of potential control observations is larger than the number of unique individuals.

Figure 1 presents the number of individual first-time deployments in the sample over the years 1993 to 2010. The single year with the highest number of first-time deployments in the sample is 2001, when 864 of the veterans in our sample were deployed. Deployments to Bosnia, Kosovo and Afghanistan dominate and make up almost 90 per cent of the first-time deployments in the sample.

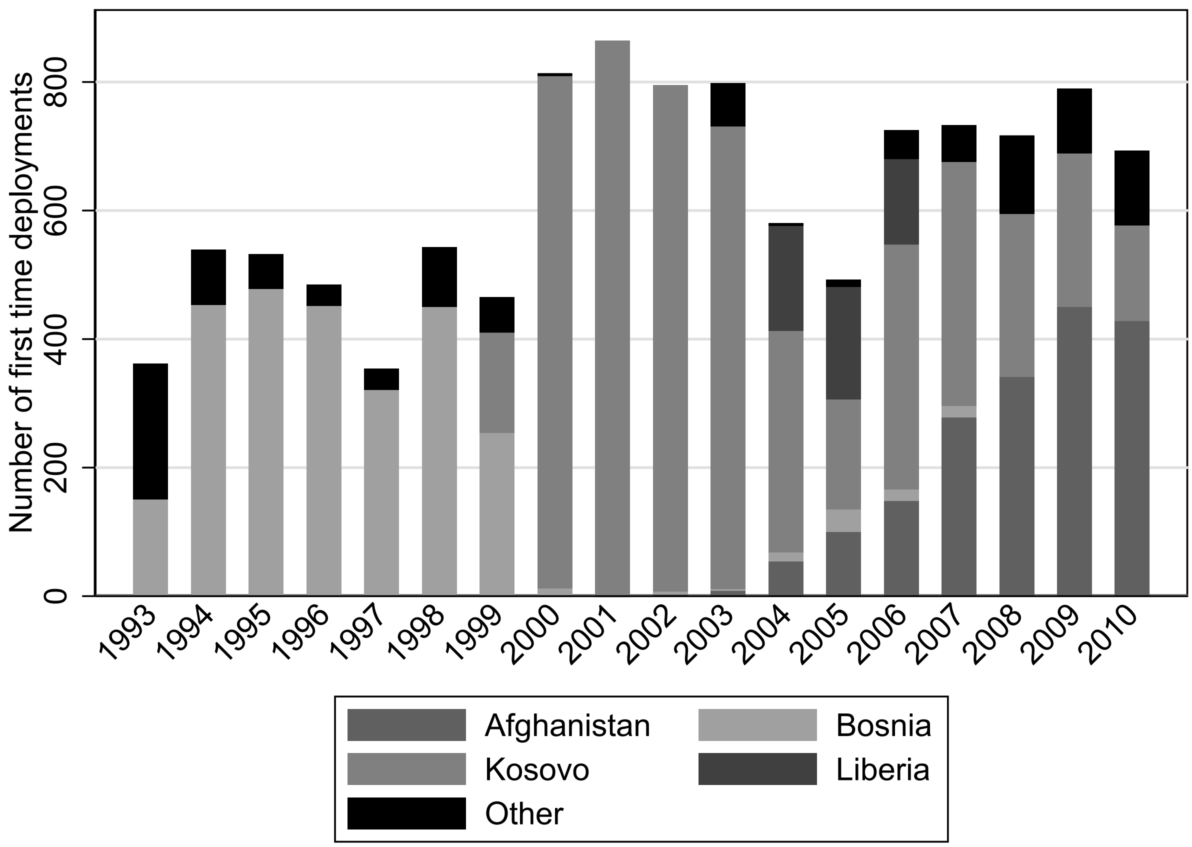

Figure 1: Number of deployments in the sample, by deployment year, and the distribution of deployments across geographical locations.

Notes: The sample consists of 11,279 former conscripts who underwent enlistment testing (and subsequently initiated their conscription service) in Sweden during 1990-2010 and who were deployed to an international peace mission (for the first time) at some point during 1993-2010.

The primary focus in this paper is the effect from deployment on annual gross labour earnings in 100s of SEK, deflated to 2019 prices, using the official national consumer price index provided by Statistics Sweden. The data on earnings originates from Swedish population-wide administrative records and is of high quality: it is not self-reported, nor top-coded, while zero earnings are distinguished from missing observations.

Figure 2 presents trajectories of (real) annual earnings for veterans and their comparison group of birth-cohort matches (i.e., potential controls), starting three years before (in the case of potential controls, contrafactual) deployment and ending nine years later. Note that the term *year* on the x-axis of the figure does not refer to calendar year but to the year in relation to the first deployment; year 1 is 1995, for those deployed 1994; 1997, for those deployed in 1996; and so on. On average, veterans tended to have higher earnings than non-veterans from the same birth-cohort, both before and after deployment, and throughout the follow-up period. Clearly, veterans from the studied period tend to outperform their non-veteran birth-cohort peers in terms of earnings.[10]

The data also reveals that veterans from the studied period were positively selected on a range of pre-deployment characteristics. Table 1 presents descriptive averages for pre-deployment characteristics and labour market status of veterans and the comparison group of potential controls. Definitions of these covariates are found in Table A1, in the Appendix. At the time of enlistment testing (typically performed at the age of 19), the veterans in our sample had higher cognitive ability, better psychological evaluations, and better military medical assessments than their non-veteran birth-cohort peers. During the years before deployment, they tended to be unemployed to a lesser degree; to be less likely to receive social welfare benefits; and less likely to be absent from work due to sickness or disability, or due to parenting, than their non-veteran peers in the group of potential controls. Also, note that, since matching is performed within birth-cohorts and calendar years, the average age in the two groups are identical.

[10]Most veterans in our sample were not employed in the military sector after deployment. In 2015, only 833 of the 11,279 veterans in our sample (7 per cent) worked in the military.

Table 1: Descriptive averages.

	Veterans	**All potential controls**	**Matched comparisons**	**Std.Diff.**
Cog. ability (1-4)	0.19	0.29	0.20	-0.01
Cog. ability (5)	0.25	0.24	0.25	0.01
Cog. ability (6-7)	0.41	0.35	0.42	0.00
Cog. ability (8-9)	0.14	0.13	0.14	0.01
Psy. ability (1-4)	0.07	0.22	0.08	-0.03
Psy. ability (5)	0.16	0.25	0.16	-0.01
Psy. ability (6-7)	0.56	0.43	0.56	0.01
Psy. ability (8-9)	0.20	0.09	0.19	0.02
Med. test = A	0.81	0.64	0.80	0.02
Female	0.04	0.02	0.03	0.07
Foreign	0.04	0.06	0.04	0.02
Primary-school	0.05	0.08	0.03	0.06
Parent high educ.	0.48	0.43	0.49	0.00
Parents low educ.	0.06	0.08	0.06	0.01
Married	0.00	0.01	0.00	-0.01
Metro. area	0.28	0.31	0.28	0.00
Student (uni.) (t=-1)	0.11	0.20	0.11	-0.01
Student (uni.) (t=-2)	0.10	0.16	0.10	0.00
Student (uni.) (t=-3)	0.08	0.12	0.08	0.01
Student (oth.) (t=-1)	0.17	0.18	0.16	0.04
Student (oth.) (t=-2)	0.35	0.34	0.34	0.02
Student (oth.) (t=-3)	0.51	0.50	0.51	0.00
Unemp. ben. (t=-1)	0.17	0.19	0.17	0.00
Unemp. ben. (t=-2)	0.13	0.16	0.13	0.00
Unemp. ben. (t=-3)	0.09	0.13	0.09	0.00
Social aid. (t=-1)	0.04	0.06	0.04	0.03
Social aid. (t=-2)	0.05	0.06	0.04	0.03
Social aid. (t=-3)	0.04	0.06	0.04	0.03
Sick. ben. (t=-1)	0.03	0.04	0.03	0.01
Sick. ben. (t=-2)	0.03	0.04	0.03	0.01
Sick. ben. (t=-3)	0.04	0.05	0.04	0.00
Parental. (t=-1)	0.02	0.05	0.02	-0.01
Parental. (t=-2)	0.01	0.04	0.01	-0.01
Parental. (t=-3)	0.01	0.03	0.01	0.00
Age at deployment	23.71	23.71	23.71	0.00
Observations	11 279	4 518 789	356 045	

Notes: Descriptive averages for veterans deployed for the first time during 1993-2010; a comparison group of non-veterans matched by birth-cohort and calendar year of observation only; and a matched comparison group of non-veterans. Standardised differences refer to the difference in means between the veterans and the non-veterans in the matched comparison group, expressed in units of the pooled standard deviation. Variable definitions are found in Table A1, in the Appendix.

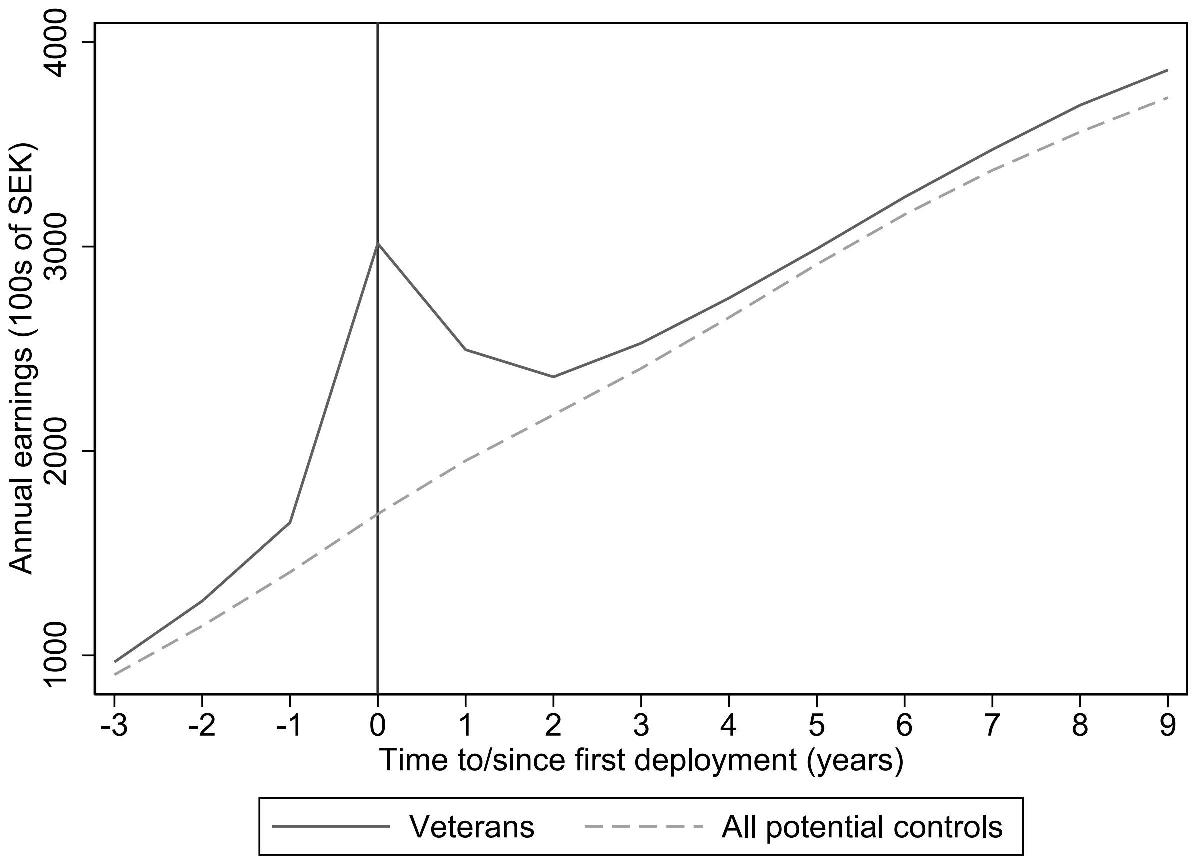

Figure 2: Observed annual earnings for the veterans and their potential controls from the same birth-cohort.

Notes: Average annual earnings in 100s of SEK, in 2019 prices, for veterans deployed for the first time 1993-2010, and a comparison group of non-veterans matched by birth-cohort and calendar year of observation only. Year 0 refers to the calendar year when a veteran was deployed for the first time. 100 SEK is approximately $10, £8 or €10.

5 Empirical Strategy

5.1 Causal inference and selection bias

The aim of this paper is to estimate the effect that deployment into a peace operation has on the subsequent earnings of those who were deployed. That is, we seek to establish the extent to which deployment caused a change in future earnings.

Taking the perspective of the Rubin causal model (Rubin, 1974, 1977), assume that each treatment (deployed; not deployed) comes with a specific outcome on earnings. Following

Angrist (1998), and Angrist and Pischke (2008), let Y_0 and Y_1 denote two outcomes: the earnings that a conscript would have had if he or she had not been deployed (Y_0) and the earnings the conscript would have had if he or she had been deployed (Y_1). Also, let the veteran status be denoted by a binary variable, D, that takes on value 1 if the conscript was deployed and 0 if he or she was not.

For each individual i, we can therefore think of two *potential* outcomes:

$$\text{Potential outcomes} = \begin{cases} \mathrm{Y}_{1i} \text{ if } D_i = 1, \\ \mathrm{Y}_{0i} \text{ if } D_i = 0. \end{cases} \tag{1}$$

We are interested in the difference between Y_{1i} and Y_{0i}, which is the causal effect of deployment on earnings for individual i. In other words, in order to establish causality, we would like to measure earnings at the same point in time for the same individual in two parallel worlds (Angrist & Pischke, 2008). A fundamental difficulty, however, is that, in reality, we cannot do this; we cannot observe both Y_0 and Y_1 for the same individual at the same time. Instead, for each veteran, what we do observe in our data is Y_1, while Y_0 is unobserved.

The potential outcome Y_0 for veterans, therefore, represents a missing piece of information that needs to be filled in by estimating the counterfactual (Gertler et al., 2016, p. 49). By doing so, our aim is to estimate *the average treatment effect on the treated*, or *ATT*:

$$E(Y_{1i} - Y_{0i}|D_i = 1) = E(Y_{1i}|D_i = 1) - E(Y_{0i}|D_i = 1). \tag{2}$$

The main challenge is to find a comparison group that, on average, gives us a valid estimate of the counterfactual $E(Y_{0i}|D_i = 1)$. In a randomised controlled experiment, treatment is assigned randomly between individuals in the population. This procedure (if carefully implemented) ensures that potential outcomes are independent from the treatment variable and that the treated and untreated are similar in all aspects beside treatment. In this way, a randomised controlled experiment will provide a valid estimate of the counterfactual and capture the causal effect of the treatment (for an introduction to causal inference, see Cunningham, 2021; Huntington-Klein, 2021; and/or Angrist and Pischke, 2008).

Unfortunately, in this case, conducting a controlled experiment is unfeasible (impossible) for many good reasons. Since deployment is not randomly assigned among former conscripts, there may be all sorts of pre-deployment characteristics that make veterans systematically different from non-veterans. If these differences between veterans and non-veterans are also correlated with their potential labour market outcomes, a simple comparison of post-deployment average earnings between these two groups will be plagued by *selection bias*.

To see how selection might bias our estimate of the ATT, consider the (naïve) approach of a simple comparison of observed averages between veterans and non-veterans. Following Angrist (1998, pp. 252–253), this comparison, in terms of potential outcomes, can be written as:

$$\begin{aligned} & E(Y_1|D=1) - E(Y_0|D=0) \\ & = E(Y_1 - Y_0|D=1) + [E(Y_0|D=1) - E(Y_0|D=0)]. \end{aligned} \tag{3}$$

Equation (3) shows that a simple comparison of observed averages between veterans and non-veterans does indeed capture the average causal effect from deployment (the first term on the right-hand side of the equation), but it also captures potential selection bias (the second term on the right-hind side of the equation) stemming from the fact that the earnings of non-veterans might not provide a valid counterfactual of what veterans would have earned if they had not been deployed. The crucial message of Equation (3) is, therefore, that selection bias can generate an observed relationship between deployment and earnings, even though there might not be any causal link connecting them to each other.

5.2 The source of potential selection bias

In the context of Swedish peacekeeping missions, selection into service occurred in two dimensions. First, all veterans volunteered for service. Second, volunteers are screened and actively selected by the military before being deployed.

Veterans are most likely self-selected into service, based on motivation and preference. However, it is less than clear how to hypothesise a possible correlation between motivation for peacekeeping and labour market performance. Even though studies using Swedish data are scarce, the evidence that does exist paints a positive picture of the personal characteristics of those who volunteer. Rydstedt and Österberg (2013) studied the psychological characteristics of a sample of 427 Swedish conscripts (surveyed in three waves during basic training), of whom 35 per cent subsequently volunteered (or planned to volunteer) for international peace missions. The authors (p. 686) concluded that volunteers had ”higher stress tolerance, social competence, self-confidence, and confidence in their own capability to deal with stressful military situations“.

Moreover, individuals might choose to volunteer as a mean of avoiding poor labour market prospects in the civilian sector. This sort of self-selection might introduce negative selection bias in the estimated effect from peacekeeping on post-deployment earnings, even though previous studies tend to emphasise non-pecuniary aspects (e.g., the desire for adventure; meaningful personal experience) as the main source of motivation for Swedish peacekeeping soldiers (Hedlund, 2011).

Before being allowed to take part in a peace mission, volunteers are screened and actively selected by the military. Individuals who wish to participate in a peacekeeping mission need to have completed their conscription service with merit; be in good physical and mental shape; and pass a security clearance process. Moreover, volunteers are interviewed and evaluated by their commanders before being deployed abroad. Hence, regardless of any self-selection that might occur, it is most likely that this active selection on behalf of the military makes the group of veterans different from the group of non-veterans, in ways that would most likely introduce positive selection bias into our estimates.

Simply comparing the group of individuals who voluntarily signed up for an international peace mission to a group of individuals that chose not to (or were rejected by the military) is bound to be a poor idea for estimating the causal effect of deployment. Self-selection and active selection on behalf of the military means that veterans and non-veterans are likely to be very different from each other in ways that might influence their labour market earnings regardless of whether or not they were deployed. Indeed, previous studies of Swedish peacekeeping veterans (Pethrus, Frisell, et al., 2019; Pethrus et al., 2017; Pethrus, Reutfors, et al., 2019) have found that deployed military veterans are a selected group of mentally healthy individuals, with higher test scores for both cognitive and non-cognitive abilities, compared with the general population of former conscripts. Lindqvist and Vestman (2011) used data from Swedish military enlistment to study the correlation between these ability measures and labour market outcomes. They find cognitive and non-cognitive ability to be strong predictors of labour market earnings. More specifically, they show that non-cognitive ability had a relatively stronger effect at the low end of the earnings distribution, whereas cognitive ability is found to be a stronger predictor of wages for skilled workers and of earnings above the median. Hence, it is reasonable to suspect that Swedish peacekeeping veterans, being positively selected on these ability measures, would have had better labour market outcomes even if they had not been deployed.

5.3 Matching combined with difference-in-differences

The main methodological challenge we faced in this paper was to provide an estimate of the average treatment effect that is free of selection bias. In order to do deal with the selection issue, the effect from deployment on earnings was estimated by using nearest-neighbour matching on the propensity-score combined with difference-in-differences outcomes (Heckman et al., 1998; Heckman et al., 1997; J. A. Smith & Todd, 2005). This approach not only allowed us to account for observable pre-treatment differences between veterans and non-veterans, but also to adjust for potential selection bias due to unobserved differences that are constant across time for the two groups (and might affect earnings).[11]

The basic idea behind our matching approach is to find a group of non-veterans who are similar to the veterans in all relevant pre-deployment characteristics. In this way, the aim is to establish experimental conditions in a non-experimental setting by constructing an artificial comparison group (Blundell & Dias, 2009). Once this is done, the matching estimator produces covariate-specific treatment-control comparisons, weighted together to produce a single overall treatment effect (Angrist & Pischke, 2008, p. 69).

If there are a high number of covariates, conditioning on all of them will make matching difficult or even impossible (Heckman et al., 1999). As a solution to this problem, Rosenbaum and Rubin (1983) suggest the method of *propensity-score matching*. Instead of trying to match on all possible dimensions of the covariates, treated and untreated individuals are matched so that they have similar probabilities of treatment given observed values of the covariates. Rosenbaum and Rubin (1983) show that adjustment for the propensity-score is sufficient to remove selection bias due observed heterogeneity.

We matched on a rich set of pre-deployment characteristics that are likely to affect both deployment and labour market outcomes. We included a range of variables measured during the enrolment process for military service and that capture individual ability and physical fitness. During the enlistment test session (typically performed at age 19), the individuals perform a range of mental and physical tests, examinations, and interviews. The aim of this enlistment procedure is to determine the individual's ability to perform military service and to screen those suitable for different services. More specifically, when matching, we included the test score in the psychological ability evaluation (also referred to as non-cognitive ability), the test score on general cognitive ability (i.e., general

[11] See Stenberg et al. (2014) for an example of a study with a similar approach applied to Swedish data.

intelligence), and an overall assessment of medical and physical fitness. These measures of individual ability, which are described in detail by Ludvigsson et al. (2022), have previously been found to be highly predictive of earnings for Swedish men (Lindqvist & Vestman, 2011). Moreover, we included a range of variables in order to capture selection based on pre-deployment labour market status, such as indications of unemployment, studies, sickness benefits, social welfare benefits, and parental leave. In addition, we also included a range of basic socio-demographic variables, such as gender, marital status, education of parents and region of residence. Detailed descriptions of all covariates used in the matching model are found in Table A1, in the Appendix.

In practical terms, the application of propensity-score matching to our data was relatively straightforward. As a first step we pooled all annual observations of the veterans and all the non-veterans and estimated the probability that an individual was deployed to an international mission during the period 1993 to 2010, conditional on the observed covariates (and year dummies) using a logit model. Table A2, in the Appendix, presents our logit estimates of this propensity-score. The table shows, among other things, that individuals with high cognitive ability, high scores on the psychological evaluation, and in good physical condition are more likely to be deployed. So, overall, veterans are positively selected on observed characteristics related to individual ability.

This propensity-score was then used to construct a matched sample of veterans and non-veterans using nearest-neighbour matching with four neighbours and replacement.[12] In the case of ties, i.e., that two or more nearest neighbours have the same propensity-score, we included both or all of them. Importantly, in the matching procedure, we matched within strata defined by the year of birth and calendar year, so that veterans from a specific year, for example 1994, are always matched to observations of non-veterans in 1994 who were born in the same year as the veteran. That is, each veteran is matched to the four

[12] Even though there is no definitive rule in choosing the number of neighbours, Austin (2010) and Rosenbaum (2020) suggest using a relatively small number of matches. Rosenbaum (2020) argues that the gains in terms of precision are small once the number of matches exceeds 4:1.

non-veterans in the sample of possible matches from the same birth year who are closest in terms of the propensity-score, allowing for the possibility that a non-veteran is used as a match more than once, but for different veterans. In total, this matched comparison group of non-veterans consists of 356,045 (weighted) observations on 172,862 individuals.[13]

Table 1 reports descriptive averages for the covariates in the matched comparison group. Compared to the sample of potential controls, the matched comparison group is very similar to the treatment group in terms of observable pre-deployment characteristics. The standardised difference in means between the two groups after matching is below 0.10 for all covariates, which has been proposed as a threshold to denote meaningful imbalance in baseline covariates (see Austin, 2009). Moreover, as shown in Figure A1, in the Appendix, there is a high degree of overlap between the veterans and the matched comparison group in terms of propensity-scores. This means that we have successfully found a match for each veteran, since none of the veterans fall outside the area of common support (see Stuart, 2010; Cunningham, 2021; and/or Huntington-Klein, 2021 for a discussion on covariate balance and the common support assumption).

One serious shortcoming of the matching approach, however, is that it can only account for selection bias stemming from *observed* characteristics (see Blundell and Dias, 2009, pp. 600–601; Gertler et al., 2016, p. 155). Indeed, to identify the causal effects of deployment on labour market outcomes for veterans it is necessary to make the strong assumption that, conditional on the propensity-score, any remaining mechanism that determines who is deployed or not must be independent of future earnings. In other words, in order to obtain unbiased estimates of the causal effect from deployment, all pre-deployment heterogeneity that both influences selection for deployment *and* future earnings must be included among the set of covariates that we include in our matching model. This assumption is often referred to as the *Conditional Independence Assumption*, or *Selection on Observables* (see Angrist and Pischke, 2008, pp. 52–59 and Cunningham, 2021,

[13]Since we allow for ties in the matching procedure, the number of matched observations exceeds the 4:1 ratio.

p. 176).[14]

One way to (at least partly) deal with potential selection bias stemming from unobserved characteristics is to exploit the fact that we have access to data on outcomes measured *before* deployment. This makes it possible to combine matching with a difference-in-differences (Diff-in-Diff) approach, where we compare the *changes* in earnings between pre-deployment and post-deployment periods for the two groups. This allows us to account for any unobserved differences between the two groups that affect earnings and that do not change over time (Heckman et al., 1997; Blundell and Dias, 2009, pp. 604–605; Gertler et al., 2016, p. 148). More specifically, the outcome variable we studied is defined as:

$$\Delta Y_{it} = Y_{it} - \bar{Y}_i, \tag{4}$$

where Y_{it} is the annual earnings observed for individual i, t years after deployment, and $\bar{Y}_i$ is the annual earnings observed for individual i, two years before deployment (the baseline year). The choice of baseline year is motivated by the fact that veterans are on mission training before being deployed. Some veterans might have initiated their training during the calendar year before the deployment year. Using the year before deployment as the baseline might therefore lead to biased estimates of the earnings effect.

This specification allows us to relax the Conditional Independence Assumption. Even if veterans differ from non-veterans in important (and unobserved) ways, as long as these differences are stable over time, the Diff-in-Diff specification will eliminate selection bias. Instead, the key identifying assumption underlying the Diff-in-Diff specification is that *trends* in earnings would be the same for veterans and non-veterans in the absence of deployment; if the veterans in our sample had not been deployed, their average earnings would have changed in the same way as the average earnings for the group of matched

[14]Besides conditional independence and common support, an additional assumption underlying the propensity-score matching approach to causal inference is the so-called stable unit treatment value assumption, or *SUTVA*. Basically, this assumption states that the control group must be unaffected by the treatment (Caliendo & Kopeinig, 2008).

comparisons (see Blundell and Dias, 2009, p. 604; Angrist and Pischke, 2008, p. 230). The point of propensity-score matching in the Diff-in-Diff setting is to ensure that the comparison group of non-veterans serves as a valid counterfactual of the trends over time that the veterans would have experienced had they not been deployed to an international peace mission (Blundell & Dias, 2009; Huntington-Klein, 2021; Stuart et al., 2014).

Still, despite the richness of our data and our difference-in-differences approach, we cannot rule out the possibility that there might be unobserved characteristics that are correlated with deployment, and that also affects the age-earnings profile of the individuals. For example, volunteers may be less focused on earnings than non-volunteers, or they may be more risk-loving. These factors could lead to different age-earnings profiles between the two groups, biasing the estimates. Importantly, these traits might not yet have emerged in the brief pre-deployment time period we cover in our data. All in all, even though we go great lengths in adjusting for potential selection bias, a causal interpretation of our findings must be done with caution.

6 Results and Discussion

We first present results for the full sample of first-time veterans who were deployed at some time during the period 1993 to 2010. Figure 3 shows how annual earnings (expressed in 2019 prices) have evolved for veterans compared to the matched comparison group of non-veterans. The figure shows outcomes relative to the deployment year, where year 0 represents the year when the veteran was deployed for the first time. Hence, the figure shows earnings three years before deployment and up to nine years after deployment.

The annual earnings of veterans follow those of the matched comparison group closely throughout the follow-up period. During deployment, the annual earnings of veterans are much higher than the annual earnings of the matched comparison group. Two years after deployment, however, the veterans' annual earnings have converged with the matched

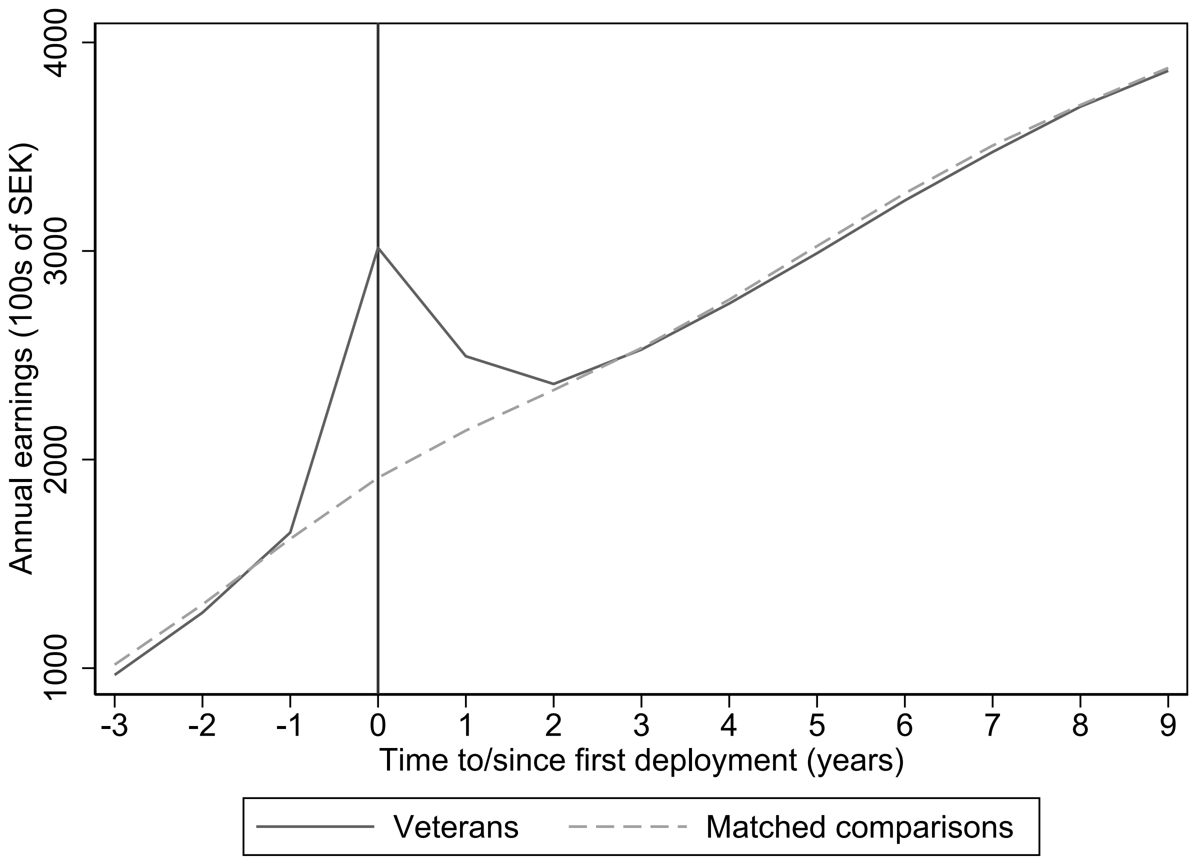

Figure 3: Observed annual earnings for veterans and the matched comparison group of non-veterans.

Notes: Average annual earnings in 100s of SEK in 2019 prices for veterans deployed for the first time 1993-2010 and the matched comparison group of non-veterans. Year 0 refers to the calendar year when a veteran was deployed for the first time. 100 SEK is approximately \$10, £8 or €10.

comparisons. In the following post-deployment years, the earnings of the two groups are very similar to each other. Figure 3 also shows that the matching procedure has created a comparison group that is very similar to the veterans in terms of pre-deployment earnings, both in terms of absolute value and trend. The latter is especially important to note since our identification strategy relies heavily on the assumption of parallel pre-deployment trends for the veterans and the matched comparison group of non-veterans.

Figure 4 shows the difference-in-differences estimates of the average treatment effect for veterans and tells a story similar to the results presented in Figure 3. Again, in conjunction with the deployment year, the results indicate a large and positive effect on veterans' annual earnings. Two years after deployment, however, the effect on the veterans' earnings has dropped considerably and stays close to zero throughout the rest of the follow-up period (i.e., up to nine years after deployment). The confidence intervals surrounding these point estimates are small, and rule out any long-term positive or negative effects of importance; in the last follow-up year, the confidence interval goes from an annual earnings premium of around 7,000 SEK (a 1.8 per cent increase), to an annual earnings penalty of around 600 SEK (a 0.2 per cent decrease). [15]

To verify the robustness of the results we also estimated the ATT with different matching strategies and different baseline years. The alternative matching strategies we use in the robustness check are nearest neighbour with one and ten neighbours. As an alternative baseline we use the average earnings over the second and the third pre-deployment year (i.e., years -2 and -3). Furthermore, standard errors are also estimated using bootstrap with 500 replications. The results from the robustness checks are given in Table A3, in the Appendix. We find that the estimates are similar across matching strategies and choice of baseline. Moreover, the bootstrap standard errors differ only marginally from the reported standard errors.

Summing up, the results suggest that, on average, the subsequent earnings of first-time veterans who were deployed at some time during the period 1993-2010 were largely unaffected by their service. The annual earnings of veterans closely follow those of the matched comparison group throughout the follow-up period. The observed earnings advantage that the veterans have over their birth-cohort peers disappears once we adjust for the non-random selection into service.

[15] Standard errors were calculated using the user-written Stata package *psmatch2*. Importantly, the calculation of the standard errors using this package does not consider that propensity-scores are estimated, rather than known, when calculating standard errors (see Huntington-Klein, 2021, pp. 314–315). Therefore, we also estimated bootstrap standard errors and reported them along with robustness checks in Table A3, in the Appendix. The standard errors reported by psmatch2 are only marginally larger than the bootstrap standard errors. Hence, using the standard errors reported by psmatch2 does not change the overall conclusions made in this paper.

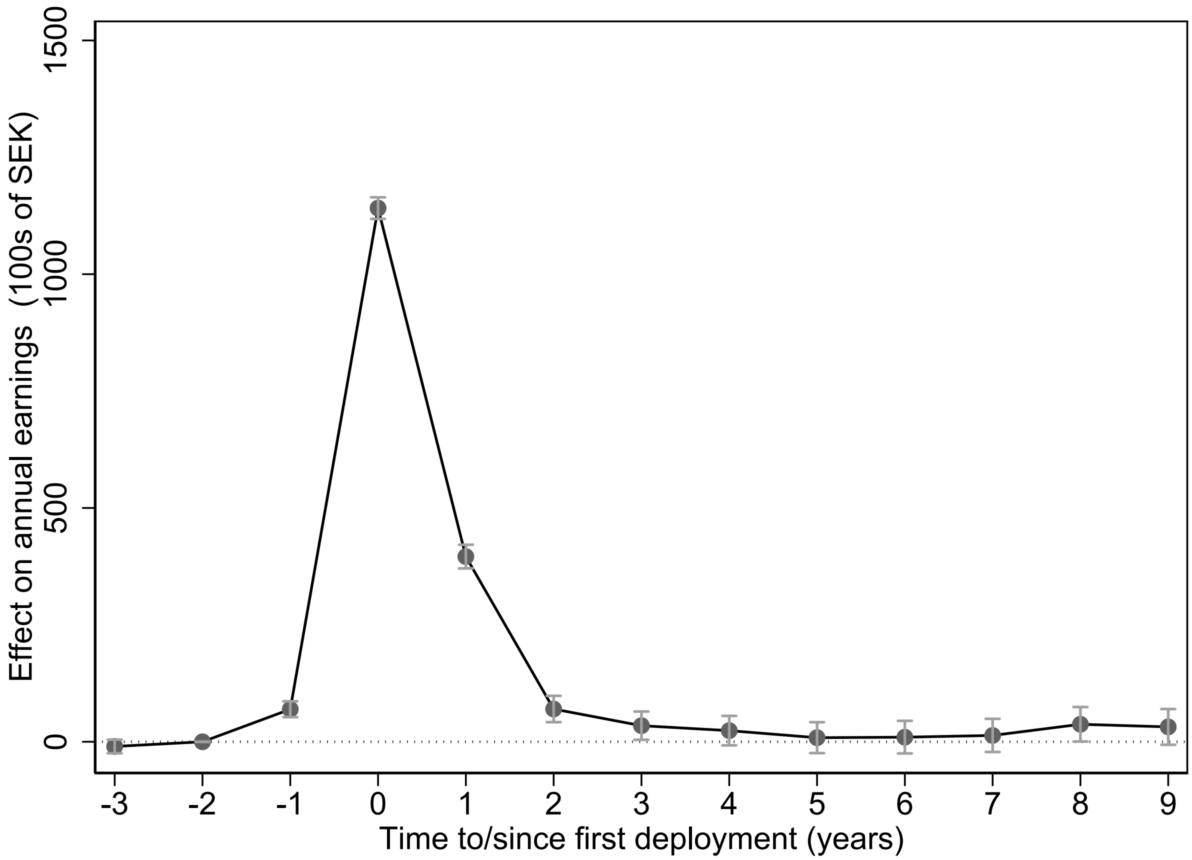

Figure 4: Impact of deployment on average annual earnings.

Notes: Matched difference-in-differences estimates of the average treatment effect from first-time deployment 1993-2010 on veterans' annual earnings (100s of SEK in 2019 prices) for up to nine years after deployment. Error bars represent 95% confidence intervals. Year 0 refers to the calendar year when a veteran was deployed for the first time. Baseline year is year -2. 100 SEK is approximately $10, £8 or €10.

6.1 Veterans by mission and deployment year

Above, we examine effects for the full sample of veterans. It is possible, however, that the results for the full sample hide differences in effects between subgroups of veterans. In this section, we therefore present results separately by mission and by deployment year.

When we estimated effects for veterans from a subgroup, the matching model was re-estimated on data from the relevant time period only. Also, note that when we present the results for subgroups of veterans, we do not attempt to deal with the potentially complicated confounding structure on the subgroup level. For example, observed differences between missions might reflect mission characteristics, as well as the composition of those who served with respect to age, gender, and so on. The analysis of the difference in effects across subgroups is purely descriptive and should thus be interpreted with caution.

Figure 5 presents results separately for the largest missions in our sample: Bosnia (1993-1999); Kosovo (1999-2010); and Afghanistan (2002-2010). The results indicate a negative earnings effect for veterans from Bosnia and a positive earnings effect for those from Afghanistan. The negative effect on annual earnings for veterans from Bosnia reaches its largest value five years after deployment when it amounts to around 9,000 SEK (roughly corresponding to 4 per cent decrease). The effect diminishes over time and is close to zero from the seventh year onwards. The positive earnings effect for veterans from Afghanistan diminishes slowly throughout the follow-up period and amounts to around 10,000 SEK nine years after deployment (roughly corresponding to a 2 per cent increase). We must stress, however, that the confidence interval surrounding the point estimates for both the Bosnia and Afghanistan missions are large, which means that, for most follow-up years, we cannot rule out earnings effects that are of very limited, or no, economic relevance.

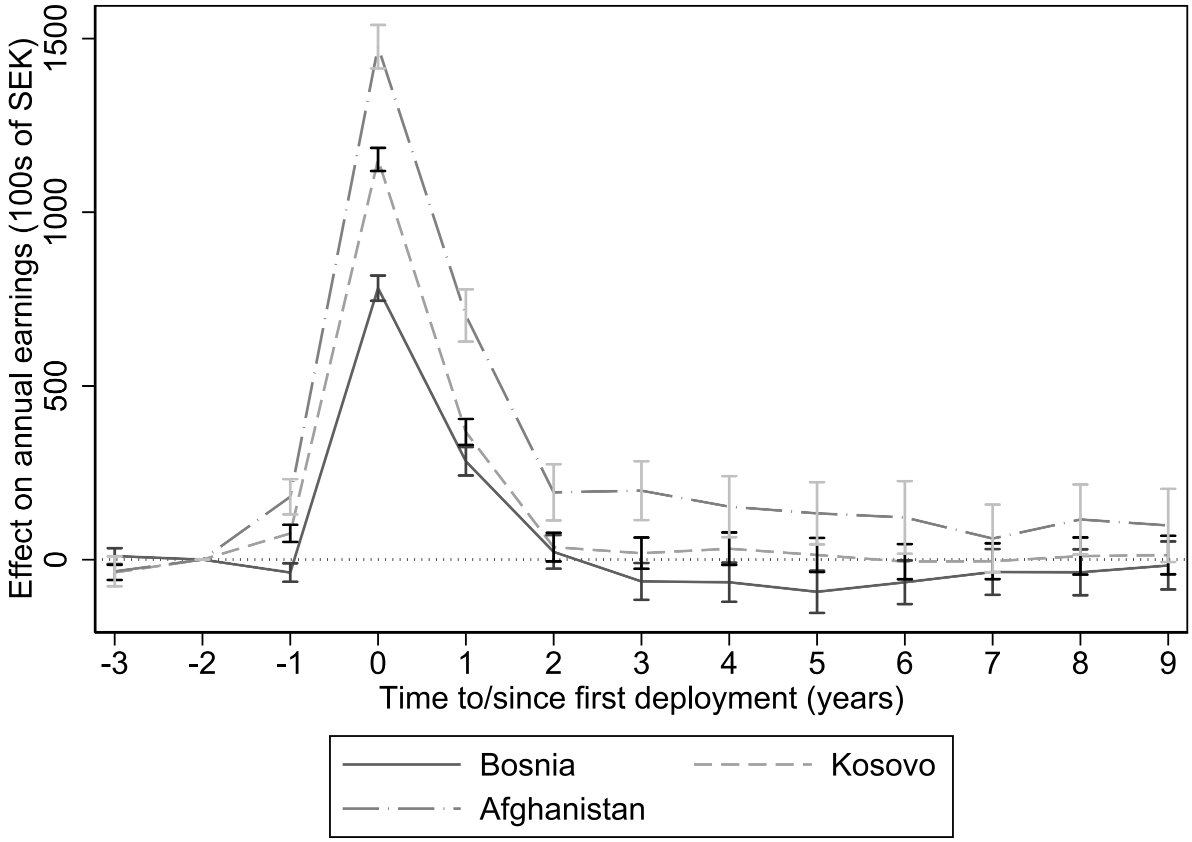

Figure 5: Impact of deployment on average annual earnings, by mission.

Notes: Matched difference-in-differences estimates of the average treatment effect from first-time deployment to various missions on veterans' annual earnings (100s of SEK in 2019 prices) for up to nine years after deployment. Estimates for veterans from Bosnia (1992-1999), Kosovo (1999-2010) and Afghanistan (2002-2010). Error bars represent 95% confidence intervals. Year 0 refers to the calendar year when a veteran was deployed for the first time. The baseline year is year -2. 100 SEK is approximately $10, £8 or €10.

Figure 6 plots the estimated effect on annual earnings five years after deployment, by deployment year. Since missions are carried out chronologically, the pattern of effects over deployment years mirrors those obtained for the different missions. Veterans deployed in the early 1990s suffered an earnings penalty, whereas veterans deployed in the early 2000s received an earnings premium. The effects for some of the years are sizeable:

veterans deployed in 1994 suffered an annual earnings loss of around 25,000 SEK (roughly corresponding to a 11 per cent decrease), whereas veterans deployed in 2006 received an earnings premium of around 33,000 SEK (roughly corresponding to a 11 per cent increase). Again, the confidence intervals surrounding the point estimates are large and for most deployment years we cannot rule out effects that are of very limited, or no, economic relevance.

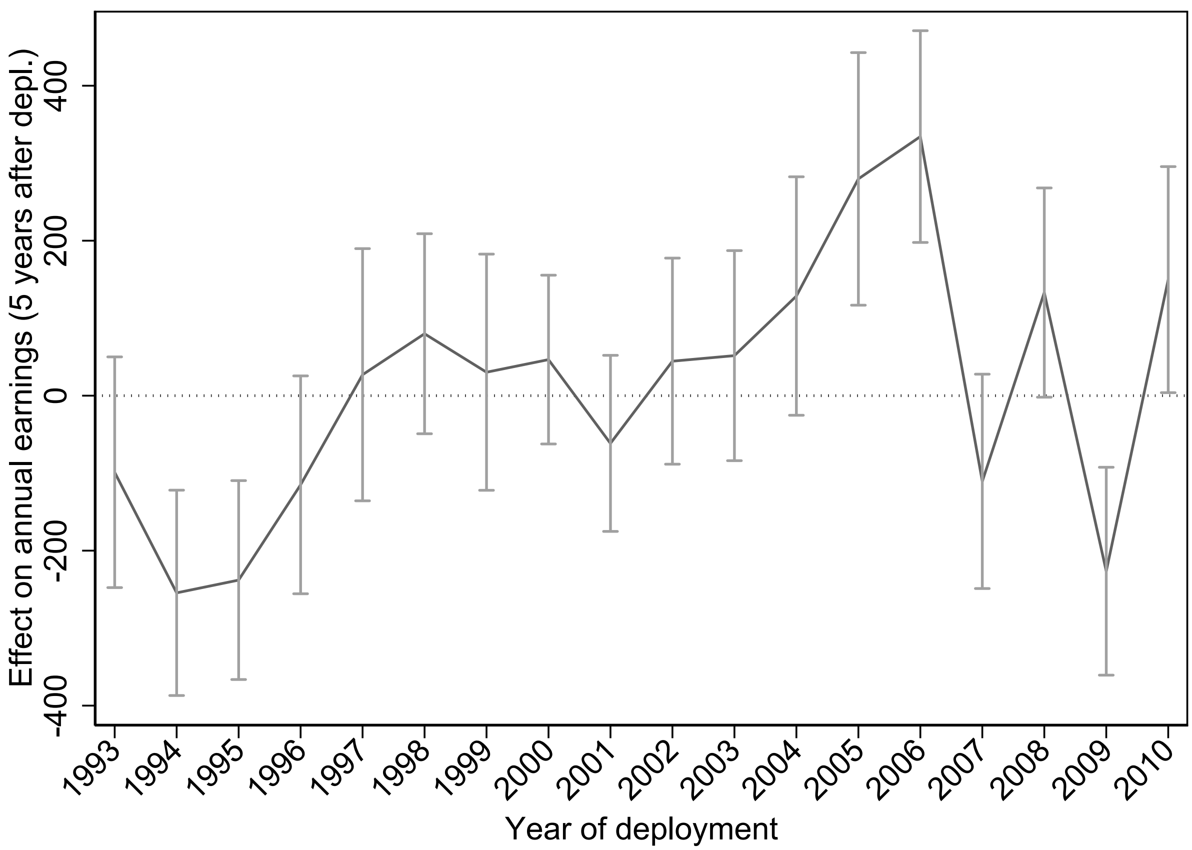

Figure 6: Impact of deployment on average annual earnings five years after deployment, by deployment year.

Notes: Matched difference-in-differences estimates of the average treatment effect from first-time deployment on annual earnings (100s of SEK in 2019 prices) five years after deployment. Separate results by deployment year. Error bars represent 95% confidence intervals. 100 SEK is approximately \$10, £8 or €10.

We can only speculate about the reason for the pattern of effects across missions and deployment years. One possibility is that the pattern, if treatment effects are heterogeneous, reflects a change in the composition of those who were deployed (e.g., increased female participation over the studied time period). Another possibility is that the differences in effects across deployment years are driven by the characteristics of the missions that were carried out during that specific year (e.g, the type of military work conducted during the mission, or the level of conflict and violence). A third possibility involves the macroeconomic situation at the time of deployment. It is interesting to note that the time-series pattern of effects roughly corresponds to conditions in the Swedish labour market. In the beginning of the 1990s, the Swedish economy was in a deep crisis and unemployment rates were at a historical high. In the beginning of the 2000s, and up until the financial crisis of 2008, on the other hand, the conditions on the labour market were much better. Hence, the soldiers returning from service were faced with very different situations on the Swedish labour market.

In principle, since annual earnings reflect both wage levels and the number of hours worked, variations in earnings across subgroups of veterans can be driven both by differences in productivity and differences in the probability of employment. We therefore explored whether deployment affected the probability of employment after returning home, by estimating the effect on employment probability in the fifth calendar year after returning home, across deployment years.[16]

The results from this additional analysis are presented in Figure 7. The effects on employment probability across deployment years roughly mirror those for earnings in Figure 6. Even though the effect is estimated with large confidence intervals, the point estimates show that deployment during the early and mid-1990s is associated with a negative effect on employment probability, whereas deployments in the later years of the studied period tend to have a positive effect. For example, veterans deployed in 1995

[16]We use a simple measure of employment: it consists of a dummy variable that is equal to one, if the individual's yearly gross labour earnings was larger than zero.

faced a 5 percentage point increase in the risk of being without employment five years after return home. Considering that the observed average employment rate for veterans deployed in 1995 was around 94 per cent five years after deployment, this effect is sizeable. Thus the negative earnings estimates for veterans deployed in the early and mid-1990s appear to be driven (at least partly) by a decrease in the number of hours worked.

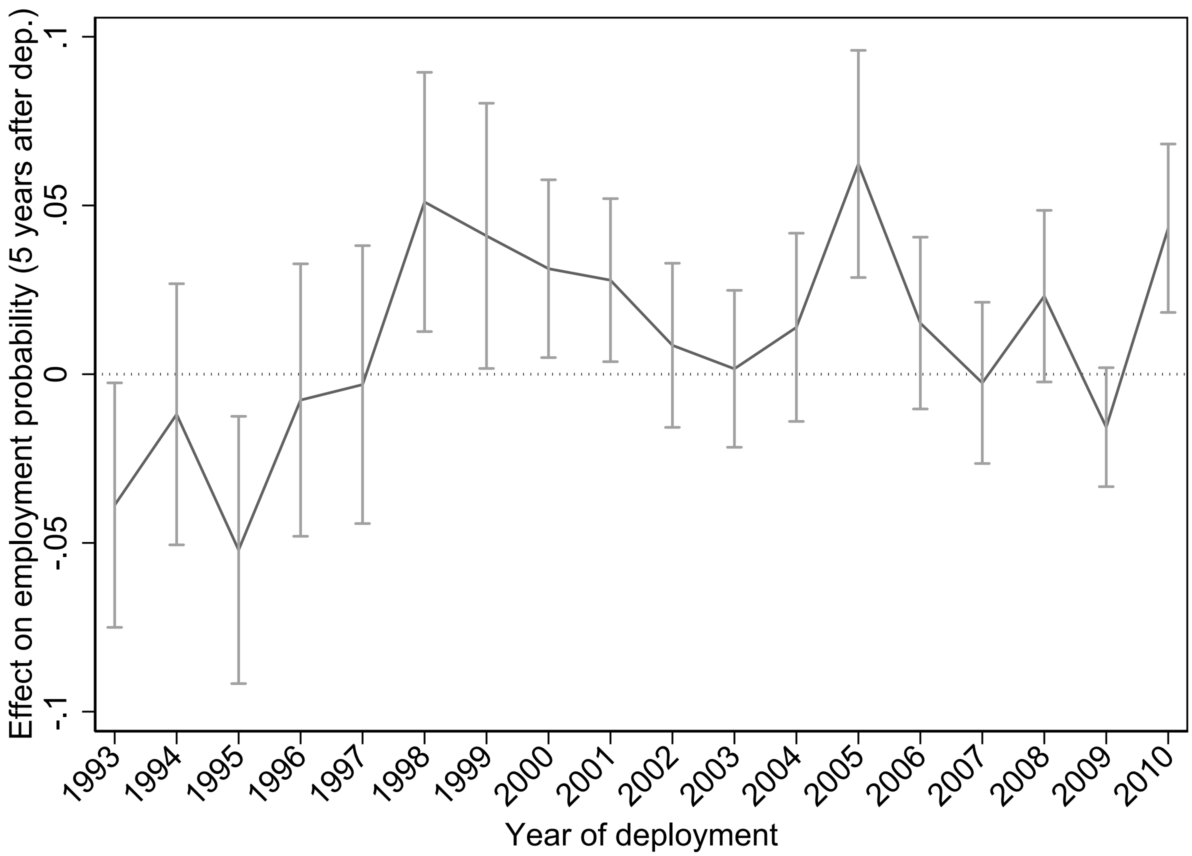

Figure 7: Impact of deployment on the probability of being in employment five years after deployment.

Notes: Matched difference-in-differences estimates of the average treatment effect from first-time deployment on employment probability five years after deployment. Separate results by deployment year. Error bars represent 95% confidence intervals.

7 Summary and Concluding Remarks

In this paper, we present novel evidence on the effects of peacekeeping on post-deployment earnings for Swedish veterans who were deployed during the period 1993 to 2010.

Overall, we find that veterans' post-deployment earnings are largely unaffected by their service. For the full sample of veterans, the point estimates of the earnings effects are close to zero, with sufficiently small confidence intervals to rule out any large long-term earnings effects. Even though Swedish veterans in the studied time period tend to outperform their non-veteran birth-cohort peers in terms of earnings, we show that this earnings advantage is the result of non-random selection into service. The earnings premium associated with deployment to a peacekeeping mission disappears once we adjust for non-random selection into service.

One interpretation of the absence of an effect on post-deployment earnings is that military experience as a peacekeeper is a valid substitute for civilian labour market experience (Makridis & Hirsch, 2021). Even though the veterans in our sample spent time out of the civilian workforce, and most of them did not pursue a military career after deployment, they clearly managed to keep up in terms of earnings.[17]

There are some indications, however, of heterogeneous effects across missions and deployment years. We find that veterans deployed to Bosnia in the early and mid-1990s appear to have suffered a (transitory) earnings penalty, whereas veterans deployed to Afghanistan in the 2000s appear to have experienced a (relatively persistent) earnings premium.

We provide suggestive evidence that the earnings losses for those who served in the early and mid-1990s are due to differences in employment status after returning home. The most plausible explanation for this finding is the macroeconomic environment at

[17] Whether or not the monetary compensation that the veterans received during deployment accurately compensated them for the risk of death or injury during deployment is beyond the scope of this paper, but is nonetheless an important topic for future research. See Armey et al. (2022), for a recent study of the U.S. combat exposure compensation policy.

the time of return. The cohorts who served during the early years of the 1990s were unlucky enough to return during the midst of a deep recession in the Swedish economy. Entering the labour market during a downturn has been found to be especially challenging for young workers (Rinz, 2022; Schwandt & Von Wachter, 2019). It is therefore not surprising to find that veterans who returned during these years seem to have struggled in establishing themselves on the labour market. At the same time, it is important to point out that the effect on employment probability might reflect aspects other than the macroeconomic environment, for example adverse effects on health and the ability to work, or increases in the demand for leisure. Our future work will shed light on the underlying mechanisms.

While our study uses high-quality and rich data and employs careful methods to address selection bias, a causal interpretation of our results must be done with caution. Future research should explore alternative approaches to identify causal effects from deployment. Despite its limitations, however, our study provides valuable insights into the long-term evolution of earnings for Swedish peacekeeping veterans and their comparison with non-veterans who are similar in a wide range of relevant dimensions.

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Appendix

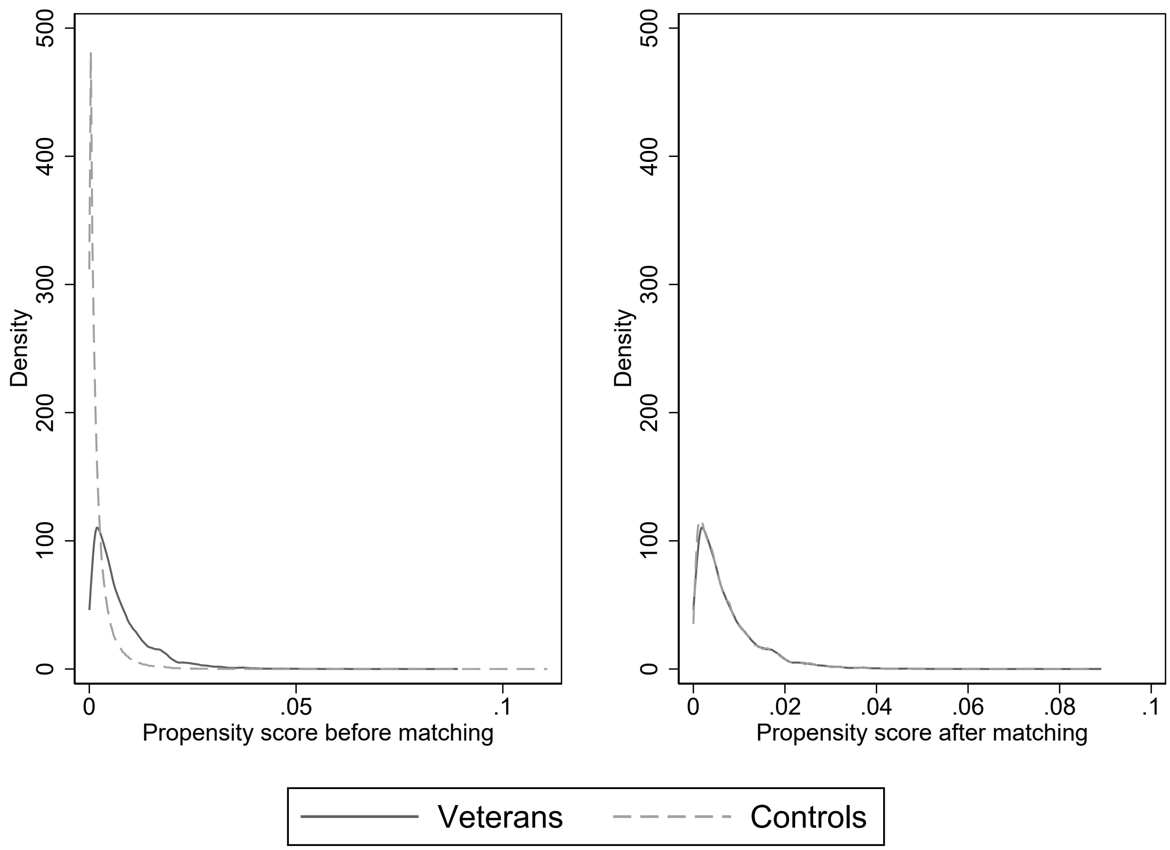

Figure A1: Distributions of the propensity-scores before and after matching.

Notes: Kernel density estimates of the distribution of the propensity-score before and after matching. Treated individuals are veterans deployed for the first time 1993-2010.

Table A1: Definitions of variables.

Variable	Description
Cog. ability (several categories)	General cognitive ability measured on the military enlistment test day. Discrete Stanine units (1-9), where 9 is the highest score. Weighted overall test score (g-factor) from four subtests of verbal, spatial, logic inductive and technical ability. Dummies for whether the individual scored 1-4; 5; 6-7; or 8-9.
Psych. ability (several categories)	Psychological assessment of the individual's ability to fulfill the psychological requirements of military service and armed combat, based on a personal interview conducted by a psychologist on the military enlistment test day. Discrete Stanine units (1-9), where 9 is the highest score. Dummies for whether the individual scored 1-4; 5; 6-7; or 8-9.
Med. test = A	Dummy for whether the individual fulfilled the military medical requirements for military training in a combat position with the highest demands on physical mobility. Measured on enlistment test day.
Female	Dummy for whether the individual is female.
Foreign	Dummy for whether the individual was born outside Sweden and/or has two parents born outside Sweden.
Primary-school	Dummy for whether the individual's highest completed education is primary-school. Measured 1 year before deployment.
Parent high educ.	Dummy for whether the individual has at least one parent with post-secondary education, three years or longer. Measured 1 year before deployment.
Parents low educ.	Dummy for whether both parent's highest education is primary school or below. Measured 1 year before deployment.
Married	Dummy for whether the individual is married. Measured 1 year before deployment.
Metro. area	Dummy for whether the individual resid in a metropolitan area, as defined by the Swedish Association of Local Authorities and Regions (SKR). Measured 1 year before deployment.
Parental leave	Dummy for whether the individual received any parental leave benefits. Measured 1, 2 and 3 years before deployment.
Student (uni.)	Dummy for whether the individual was registered at a university and received student aid. Measured 1, 2 and 3 years before deployment.
Student (oth.)	Dummy for whether the individual received student aid but was not registered at a university. Measured 1, 2 and 3 years before deployment.
Unemp. ben.	Dummy for whether the individual received any unemployment benefits or payments from active labour market training programmes. Measured 1, 2 and 3 years before deployment.
Social aid	Dummy for whether the individual's household received any social aid payments. Measured 1, 2 and 3 years before deployment.
Sick. ben.	Dummy for whether the individual received any benefits associated with sickness absence, occupational injury or rehabilitation. Measured 1, 2 and 3 years before deployment.

Table A2: Logit model estimation of the probability of deployment to a peacekeeping mission 1993-2010, conditional on observed baseline characteristics.

	Est.	**S.E**
Cog. ability (1-4)	-0.269	(0.0290)
Cog. ability (6-7)	0.0955	(0.0242)
Cog. ability (8-9)	0.0126	(0.0328)
Psy. ability (1-4)	-0.544	(0.0423)
Psy. ability (6-7)	0.626	(0.0266)
Psy. ability (8-9)	1.108	(0.0324)
Med. test = A	0.657	(0.0244)
Female	0.893	(0.0476)
Foreign	-0.0425	(0.0489)
Primary-school	-1.289	(0.0472)
Parent high educ.	0.106	(0.0205)
Parents low educ.	-0.164	(0.0408)
Married	-1.592	(0.278)
Metro. area	-0.266	(0.0216)
Stud. (uni.) (t=-1)	-1.022	(0.0390)
Stud. (uni.) (t=-2)	-0.173	(0.0471)
Stud. (uni.) (t=-3)	-0.420	(0.0459)
Stud. (oth.) (t=-1)	-0.955	(0.0322)
Stud. (oth.) (t=-2)	-0.282	(0.0307)
Stud. (oth.) (t=-3)	0.0380	(0.0297)
Unemp. ben. (t=-1)	0.0402	(0.0309)
Unemp. ben. (t=-2)	-0.103	(0.0369)
Unemp. ben. (t=-3)	-0.294	(0.0384)
Social aid. (t=-1)	0.0365	(0.0547)
Social aid. (t=-2)	0.123	(0.0560)
Social aid. (t=-3)	0.0232	(0.0538)
Sick. ben. (t=-1)	-0.333	(0.0594)
Sick. ben. (t=-2)	-0.110	(0.0563)
Sick. ben. (t=-3)	0.167	(0.0519)
Parental. (t=-1)	-1.342	(0.0963)
Parental. (t=-2)	-0.543	(0.118)
Parental. (t=-3)	0.149	(0.115)
Age at deployment	-0.202	(0.00426)
Constant	-1.239	(0.121)
Observations	4747880	

Notes: Output from a logit regression on pooled cross-sections of annual data for the full sample of treated and untreated individuals. The dependent variable is 1 if an individual was deployed for the first time, and 0 otherwise. Subsequent observations on first-time veterans are censored. The coefficients represent the contribution of each listed covariate to the log odds that an individual is deployed (year dummies are included, but not shown). The predictions from this model are used to construct the propensity-score used for matching. Standard errors in parentheses. Variable definitions are found in Table 2.

Table A3: The impact of deployment on annual earnings five and nine years after deployment using different matching algorithms and baseline year.

Outcome	**Nearest neighbour (1)**	**Nearest neighbour (4)**	**Nearest neighbour (10)**	**Nearest neighbour (4) w/ alt. baseline**
Annual earnings (t=5)	11.6	8.8	7.2	13.3
	(18.5)	(16.8)	(16.2)	(16.4)
	[19.9]	[18.1]	[17.2]	[17.7]
Annual earnings (t=9)	36.1	31.8	20.8	36.4
	(21.7)	(19.5)	(18.6)	(19.0)
	[21.7]	[20.3]	[19.2]	[19.7]

Notes: Matched difference-in-differences estimates of the ATT on annual earnings (100s of Swedish SEK) five and nine years after deployment using alternative matching algorithms and baseline year for calculating matched Diff-in-Diff outcomes. Alternative matching algorithms include the following: nearest neighbour matching using 1 nearest neighbour; nearest neighbour matching using 4 nearest neighbours (the preferred matching algorithm); and nearest neighbour matching using 10 nearest neighbours. The alternative baseline is the average earnings over the second and third years that came before deployment (the preferred baseline is the earnings in the second year before deployment). Standard errors in parentheses. Bootstrapped standard errors based on 500 replications are reported in brackets. 100 SEK is approximately \$10, £8 or €10.