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Swedish Veterans after Bosnia: The Relationship Between Military Deployment and Labour Market Marginalisation

RESEARCH ARTICLE

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SCANDINAVIAN MILITARY STUDIES

ABSTRACT

While the well-being of the Swedish peacekeepers deployed to Bosnia in the 1990s has received a great deal of attention over the years, very little is known about how these military veterans have fared on the labour market after returning home. In this paper, I provide novel evidence on the relationship between military deployment to Bosnia and adverse outcomes on the labour market. The analysis is based on longitudinal administrative data for a sample of 2,275 young Swedish veterans who served as peacekeepers in Bosnia at some point during the years 1993–1999. I follow these veterans for up to 20 years after deployment. Using propensity score matching based on a rich set of covariates, I estimate the effects of deployment on three broad measures of labour market marginalisation: long-term unemployment, work disability, and social welfare assistance. I find no indication of long-term labour market marginalisation for the veterans. Even though the veterans experienced an increase in the risk of unemployment in the years immediately following their return from service, in the long run their attachment to the labour market is not affected negatively.

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INTRODUCTION

The Swedish peacekeepers who were deployed to Bosnia in the 1990s found themselves in the midst of a violent civil war marked by war crimes and atrocities, and were at times under severe pressure (see Sjöstrand, 2006, for some accounts). It has long been known that this kind of exposure to combat and traumatic events can have negative consequences for soldiers' mental health (Cesur et al., 2013; Dobkin & Shabani, 2009; Hyams et al., 1996). Consequently, the well-being of the military veterans who served in Bosnia has received a great deal of attention over the years. Many critical voices have indeed questioned the measures taken to re-integrate veterans into civilian society after returning home (see, for example, Holmström, 2007; Strömberg et al., 2013; Häggström, 2013; Tuulik, 2019). Yet, despite these concerns, very little is known about how these veterans fared on the labour market after returning home.

Concerns about the re-integration of formerly deployed veterans are warranted. Studies from the United States have shown that mental health conditions such as post-traumatic stress disorder (PTSD) are associated with poor work-related outcomes for both Vietnam-era veterans and more recent military combat veterans (Amick et al., 2018; Anderson & Mitchell, 1992; Ramchand et al., 2015; Savoca & Rosenheck, 2000; Smith et al., 2005). In this sense, military service in combat zones may impair veterans' ability to re-integrate into civilian life and the civilian labour market after returning home.

At the same time, previous studies on Swedish peacekeeping veterans tend to emphasise the physical and mental well-being of those who have served in international peace missions. Michel et al. (2003) assessed the self-reported mental health of a Swedish battalion serving in Bosnia before and after deployment and found no significant change over time. In another study, Michel et al. (2007) concluded that Swedish personnel serving in international peacekeeping operations from 1960 to 1999 did not have a higher risk of suicide compared to the general population. This result was later confirmed by Pethrus et al. (2017), who controlled for a range of psychological, psychiatric, and physical fitness factors and found that Swedish military veterans deployed between 1990 and 2013 did not differ from nondeployed comparators in terms of suicide or all-cause mortality. Similar patterns have been observed in other Nordic countries (Pethrus et al., 2022). Subsequent studies have also shown that Swedish veterans deployed to Afghanistan did not have an increased risk of being convicted of violent crime, nor did they consume more anxiolytics or antidepressants, or receive more psychiatric inpatient care, than non-deployed comparators (Pethrus, et al., 2019a). Indeed, Wisén and his colleagues (2021), who studied stress levels over time for a sample of Swedish peacekeepers deployed to Afghanistan, challenged the general view of military deployment as an inevitable source of negative stress and argued for a shift in focus towards potential positive aspects of deployment.

There are, however, some indications that veterans deployed to Bosnia in the 1990s experienced negative labour market outcomes after returning home from service. In a related study, Bäckström and Hanes (2023) studied the effect of peacekeeping on post-deployment earnings for Swedish veterans. Using rich administrative data, they followed a sample of more than 11,000 Swedish veterans, deployed for the first time during the period 1993–2010, for up to nine years after returning home. Even though the results showed that, overall, the veterans in the sample did not experience any large long-term earnings effects as a result of their service, the specific sub-sample of veterans who served in Bosnia in the 1990s appeared to have suffered a (transitory) earnings penalty after returning home. One possibility is that this adverse effect on earnings reflects aspects specific to the mission in Bosnia, which affected the veterans' ability to work and re-integrate into the civilian labour market. Another potential explanation for the findings, however, comes from the domestic labour market situation at the time of their return from service. Indeed, Bäckström and Hanes (2023) note that veterans who were deployed during the early years of the 1990s were unlucky enough to return during the midst of a deep recession in the Swedish economy. As such, the domestic macroeconomic environment might provide a simple economic explanation for the pattern of treatment effects across cohorts of military veterans returning from service (Angrist, 1998).

By studying the effect of deployment on qualitative aspects of long-term labour market outcomes, this study contributes to a comprehensive understanding of the marginalisation potentially experienced by Swedish peacekeeping veterans deployed to Bosnia in the 1990s. The main research question is: Did service as a peacekeeper in Bosnia in the 1990s lead to

adverse labour market outcomes after returning home? To answer this question, I follow a sample of 2,275 young Swedish veterans for up to 20 years after deployment and estimate the effect of deployment on three broad measures of labour market marginalisation: long-term unemployment, work disability, and social welfare assistance. In order to deal with the issue of selection bias, I use propensity score matching based on a rich set of covariates, including measures of cognitive ability, psychological capacity, military medical assessments, pre-deployment labour market and welfare history as well as a range of socio-demographic background variables.

The remainder of this paper is structured as follows. The following section presents a brief background on the Swedish mission to Bosnia in the 1990s. The third section describes the data and discusses the empirical approach used to answer my research question. In the fourth section, the empirical results are presented. The final section concludes the paper with a discussion.

THE SWEDISH MISSION TO BOSNIA

Violence and civil war throughout the Balkans began in 1991, when Slovenia and Croatia declared themselves independent from the Socialist Federal Republic of Yugoslavia. Not long after, the fighting spilled over into multi-ethnic Bosnia and Herzegovina.

In 1992, the United Nations Protection Force (UNPROFOR) was established. Although its mandate and reach initially related to Croatia, these were extended as the conflict intensified in Bosnia and Herzegovina. In 1995, around 31,000 men and women from a wide range of different countries were deployed as a part of the force (Findlay, 2002, p. 142). After the peace treaty of 1995, the forces of UNPROFOR were reflagged under the NATO-led Implementation Force (IFOR) and, later, Stabilization Force (SFOR).

Between 1993 and 1999, Sweden deployed 13 mechanised battalions in Bosnia as part of UNPROFOR, IFOR and SFOR. The first battalion (BA01) was deployed to Tuzla in the northeast part of Bosnia in September of 1993. The last Swedish battalion (BA99) was disbanded at the end of 1999 (although some functions remained active in the area). In total, more than 7,000 Swedish men and women were deployed as peacekeepers to Bosnia in the 1990s.

As part of the Nordic Battalion (Nordbat), the Swedish peacekeepers were at times under severe pressure and had several confrontations with the warring factions, who often made it clear that the UN peacekeepers were anything but welcome; Sjöstrand (2006) describes how ambushes were so common that the battalion ran out of spare tyres for the wheeled armoured personnel carriers. The conflict was also distinguished by war crimes and other atrocities. In Stupni Do, Swedish soldiers, in what used to be a quiet village in the mountains north of Sarajevo, were faced with the aftermath of one of the worst massacres of the war; the pyres built to burn the bodies of the victims were still burning as they entered what was left of the village in the late autumn of 1993 (see news articles by Ottaway, 1993, and Burns, 1993, for contemporary accounts of the Stupni Do massacre).

All Swedish soldiers that participated in the mission to Bosnia volunteered for service. The volunteers (typically former conscripts) were organised and employed in a separate voluntary force for international operations (the Utlandsstyrkan or International Military Force), rotated twice yearly (Ministry of Defence, 2007). The voluntary nature of service in international missions also comprised employed personnel of the Swedish Armed Forces (that is, professional officers). Any regular personnel of the Swedish Armed Forces who participated in international missions were on temporary leave of absence during their service in the International Military Force (Ministry of Defence, 2010).

Recruitment for the International Military Force was conducted in two recruitment periods per year. If a volunteer 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 that participate in an international military mission. After a first screening of

eligibility (which, among other things, involved the military grades from conscription service) 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 interviews were conducted with the selected applicants (Ministry of Defence, 2007). During a final selection stage that took place 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.[1]

METHODS

DATA

My data combine military personnel records with administrative data on transfers related to the Swedish social insurance system, allowing me to observe labour market outcomes for up to 20 years after deployment. I merged administrative data from several different sources: Statistics Sweden, the Swedish Armed Forces; the Swedish Defence Conscription and Assessment Agency, and the Military Archives. The data from the Swedish Armed Forces include information on Swedish military veterans deployed to Bosnia between 1993 and 1999. The data from Statistics Sweden cover the period 1990–2019 and include data on 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 schooling. The data from the Swedish Defence Conscription and Assessment Agency and the Military Archives include information from the enlistment tests for all individuals who underwent testing from 1990. All data analysis and processing used de-identified data available through Statistics Sweden's platform for access to microdata. The study has been reviewed and approved by the Swedish Ethical Review Agency (Etikprövningsmyndigheten, Dnr 2021-02153).

The treatment group consists of all former conscripts who underwent enlistment testing in Sweden during the years 1990–1999 and who were deployed to Bosnia (for the first time) at some point during 1993–1999. It is important to note that the way in which I constructed the sample guarantees that it mainly consists of young individuals. The main reason for the sample restriction is related to the availability of data; individuals who initiate their conscription service are identified by conscription compensation payments, which are only available from 1990. The reader should therefore keep in mind that the results from this study do not necessarily carry over to individuals in older age categories. In total, the treatment group consist of 2,275 individuals aged between 19 and 30 at the time of their first deployment.

To form a pool of potential controls among the untreated individuals, I matched each veteran from each calendar year to all former conscripts from the same birth-cohort who had not been deployed to any international military mission up to, and including, that calendar year. For example, a veteran who was born in 1973 and was deployed to Bosnia in 1994 was matched to observations of non-veterans who were also born in 1973 but who were not deployed (at all) up until, and including, 1994. In total, the pool of potential controls consists of 1,086,754 (weighted) observations on 232,886 unique individuals. A potential match was given a fictitious deployment date based on the deployment date of the veteran to whom he or she was matched. The fact that both the treatment group and the untreated group are drawn from the population of former conscripts means that the study population is homogeneous in the sense that all individuals have, at some point in time, been selected for conscription service. It also means that the potential effects from deployment are clearly separated from potential effects of pre-deployment military service.

I use three indicators of labour market marginalisation as outcomes: (1) long-term unemployment, (2) work disability, and (3) social welfare assistance. These measures have previously been used in epidemiological studies using similar Swedish data (Alaie et al., 2021; Niederkrotenthaler et al., 2014; Niederkrotenthaler et al., 2016). An individual is considered long-term unemployed if he or she was registered as (full-time or part-time) unemployed at the Swedish Public Employment Service, or enrolled in an active labour market programme, for 180

1 According to interview with Capt. Mats Kjäll (ret.), recruitment officer at the Swedish Armed Forces International Centre (Swedint) 1991–2001 (April 04, 2022).

days or more in the course of a year. Work disability is defined as either receiving any amount of disability pension or having 60 or more (net) days of sick leave during a year.[2] Social welfare assistance indicates whether the individual's household received any amount of temporary financial support (ekonomiskt bistånd) from the municipality during a year. This type of social aid is given to individuals who are unable to support themselves and is generally regarded as a last resort for individuals with financial difficulties.

DESCRIPTIVE STATISTICS

Figure 1 presents the number of individual first-time deployments in the sample from 1993 to 1999. The single year with the highest number of first-time deployments in my sample is 1998, when 450 of the 2,275 veterans were deployed.

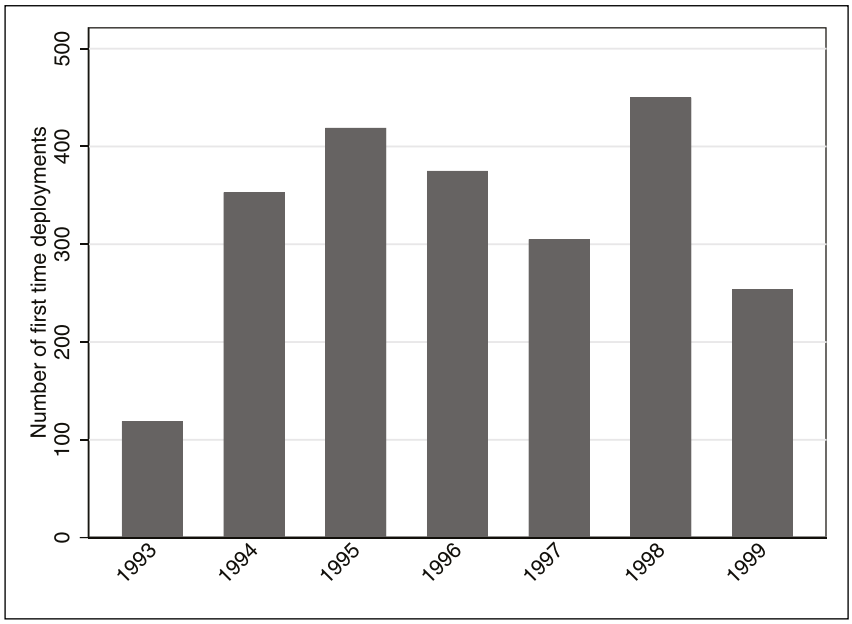

Figure 1 Number of deployments in the sample, by deployment year.

Note: The sample consists of 2,275 former conscripts who underwent enlistment-testing (and subsequently initiated their conscription service) in Sweden during 1990–1999 and who were deployed to Bosnia (for the first time) as peacekeepers at some point during 1993–1999.

Table 1 presents descriptive statistics on pre-deployment characteristics of veterans and the full pool of potential controls (descriptions of variables are found in Table A1 in the Appendix). Veterans are positively selected on a range of pre-deployment characteristics. In particular, at the time of enlistment-testing (typically performed at the age of 19), the veterans in my sample had higher cognitive ability, better psychological assessments and better military medical assessments than their birth-cohort peers. In the epidemiological literature, the observation that military personnel tend to be fitter and healthier than the general population (at enlistment) is commonly referred to as the *healthy soldier effect* (McLaughlin et al., 2008). Clearly, in this case, it is also true that they tend to have better cognitive and non-cognitive abilities than the general (conscription) population.

The veterans differ from their birth-cohort peers in the pool of potential controls in other aspects as well. The veterans tend to have both somewhat higher earnings and somewhat lower study participation than non-veterans during the years prior to deployment. Note, however, that the proportion of former students among the veterans is still large: 39% of the veterans received student grants in the year prior to deployment. This, together with the relatively low average age at deployment, and the modest pre-deployment income levels, suggest that many veterans in the sample, most likely had yet to establish themselves on the labour market. The differences with respect to pre-deployment labour market marginalisation are small, however; the prevalence of unemployment, social welfare assistance and work disability during the pre-deployment year are similar across the two groups.

Figure 2 shows how observed outcomes for the veterans in the sample evolve from a year before and up until 20 years after the first deployment. The veterans experienced a sharp increase in

2 The number of days of sick leave (nettodagar med ersättning för sjukdom/arbetsskada/ rehabilitering) does not include the initial 14 days of the absence period (sjuklöneperioden) for employed individuals.

	VETERANS	ALL POTENTIAL CONTROLS	MATCHED COMPARISONS
Cog. ability (1–4)	0.22	0.32	0.23
Cog. ability (5)	0.30	0.24	0.30
Cog. ability (6–7)	0.36	0.32	0.36
Cog. ability (8–9)	0.11	0.12	0.11
Psy. ability (1–4)	0.09	0.25	0.10
Psy. ability (5)	0.19	0.24	0.20
Psy. ability (6–7)	0.53	0.40	0.52
Psy. ability (8–9)	0.18	0.09	0.17
Med. test A	0.85	0.63	0.82
Med. test B	0.02	0.03	0.02
Med. test C/D	0.08	0.18	0.10
Female	0.01	0.01	0.01
Foreign	0.04	0.05	0.04
Primary school	0.07	0.09	0.03
Parent high educ.	0.39	0.38	0.39
Parents low educ.	0.09	0.11	0.09
Metro. area	0.27	0.32	0.27
Parental (t–1)	0.00	0.01	0.00
East Sweden	0.33	0.41	0.33
North Sweden	0.24	0.24	0.23
South Sweden	0.44	0.35	0.44
Study (t–1)	0.39	0.45	0.37
Study (t–2)	0.54	0.58	0.54
Study (t–3)	0.70	0.71	0.73
Social aid (t–1)	0.10	0.11	0.10
Social aid (t–2)	0.10	0.11	0.10
Social aid (t–3)	0.07	0.09	0.07
Unemployed (t–1)	0.18	0.19	0.19
Work disab. (t–1)	0.00	0.00	0.00
Earnings (t–1)	865.67	823.09	852.66
Earnings (t–2)	662.22	620.01	654.27
Earnings (t–3)	502.77	479.48	484.38
Age at deployment	22.17	22.17	22.17
Observations	2275	1086754	9039

Table 1 Descriptive averages.

Note: This table shows descriptive averages for the sample of 2,275 veterans deployed to Bosnia 1993–1999; a pool of potential controls consisting of non-veterans matched by birth-cohort and calendar year of observation only; and a matched comparison group of birth-cohort peers who did not serve. Variables measured one year before the deployment year, or at the time of enlistment testing, unless stated otherwise.

long-term unemployment after returning home. During the first year after the deployment year, around 20% of the veterans in the sample are categorised as long-term unemployed. This share decreases steadily throughout the follow-up period; during the 20th year after deployment around 1% of the veterans are categorised as long-term unemployed. Similarly, the share of veterans who receive social welfare assistance decreases steadily throughout the follow-up period. Work disability, on the other hand, tends to increase with follow-up time. The prevalence of work disability among veterans is very low, however; 20 years after returning home, less than 2% of the veterans in the sample are indicated as being in this category.

PROPENSITY SCORE MATCHING

Did service as a peacekeeper in Bosnia in the 1990s lead to adverse labour market outcomes after returning home? To give a definite answer to this empirical question, I would have to compare

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the observed outcomes for individuals who were deployed to Bosnia with the counterfactual outcomes that they *would have had* if they had *not* been deployed. A fundamental difficulty, not only for this question, but for all causal questions, is that one cannot observe the latter; data on counterfactual outcomes is inherently lacking and will continue to be so (Cunningham, 2021, pp. 127–128). Since the counterfactual cannot be directly observed, I need to *estimate* it in order to fill in the missing information (Gertler et al., 2016, p. 49).

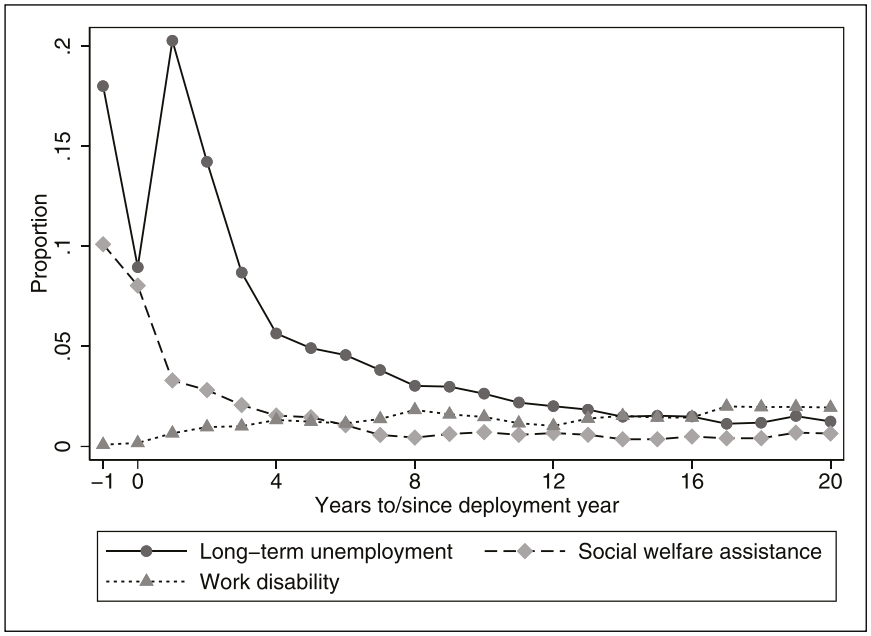

Figure 2 Observed outcomes for the sample of veterans deployed to Bosnia in the 1990s.

Note: This figure shows how observed outcomes evolve over time for the sample of 2,275 veterans deployed to Bosnia 1993–1999, starting one year before their (first) deployment year. Year 0 refers to the calendar year when a veteran was deployed for the first time. An individual is considered long-term unemployed if he or she was registered as (full-time or part-time) unemployed at the Swedish Public Employment Service, or enrolled in an active labour market programme, for 180 days or more during a year. Work disability is defined as either receiving disability pension or having 60 or more (net) days of sick leave during a year. Social welfare assistance indicates whether the individual's household received any amount of temporary financial support during a year.

The aim of this paper is to estimate the average treatment effect on the treated (ATT) by letting the observed outcomes for a comparison group of non-veterans represent the counterfactual. Simply comparing the group of individuals who voluntarily signed up for an international peace mission to the group of individuals who chose not to is highly unlikely to produce good estimates of the ATT, however. The individuals who served as peacekeepers in Bosnia not only self-selected into service, all having volunteered, but were, further, actively screened and selected by the military organisation before deployment. Consequently, veterans and non-veterans are likely to be very different in ways that might influence their labour market outcomes, regardless of whether or not they served as peacekeepers. Indeed, previous research has shown that Swedish veterans are a selected sample of mentally and physically healthy individuals with above average levels of both cognitive and noncognitive ability (Pethrus et al., 2017; Pethrus, et al., 2019a; Pethrus et al., 2019b). Since these (pre-deployment) characteristics are likely to be correlated with (post-deployment) labour market outcomes (see Edin et al., 2022; Lindqvist and Vestman, 2011), simply comparing veterans to non-veterans in the general population is bound to lead to biased estimates of the effects from service. A range of previous studies, starting with seminal works by Angrist (1990) and Angrist and Krueger (1994), has shown that this sort of selection bias can be severe; Angrist and Krueger (1994) showed that the positive estimates of the earnings premium for World War II veterans reported by several earlier studies (e.g., Rosen and Taubman, 1982; Berger and Hirsch, 1983) were entirely due to non-random selection into the military.

One common approach that tries to deal with the selection issue when using observational data is propensity score matching (Rosenbaum & Rubin, 1983; Rosenbaum 2020). The basic idea behind propensity score matching is to compare individuals who, based on observable pre-treatment characteristics, had similar probabilities (i.e., similar propensity scores) of being in the treatment group, even though they differ in actual treatment status (Cunningham, 2021, pp. 208–209). 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 a weighted average outcome for the matched comparison group to represent the counterfactual (Angrist & Pischke, 2008, p. 69).

I matched on a rich set of pre-deployment characteristics that are likely to confound the relationship of interest by affecting *both* the probability of deployment *and* post-deployment labour market outcomes. First, I included a range of variables, measured during the enrolment process for military conscription service, 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 (see Ludvigsson et al., 2022). The overall aim of this enlistment procedure is to determine the individual's ability to perform military service and to screen individuals suitable for different types of positions. More specifically, I included information from the assessment of psychological ability, test scores on cognitive ability (i.e., general intelligence), and an overall assessment of medical and physical fitness. Second, in order to capture self-selection based on opportunity costs for service, I included a range of variables that measure pre-deployment labour market attachment. Importantly, in order to avoid matching on post-deployment events, all these variables are measured *before* the deployment year. Third, I included a range of basic sociodemographic variables such as gender, geographical location, and the education level of parents. Detailed descriptions of all covariates used in the matching model are found in Table A1 in the Appendix.

To perform the matching, I pooled all annual observations on the veterans and the nonveterans and estimated the probability that an individual is deployed to Bosnia over the period 1993 to 1999, conditional on the observed covariates and calendar time dummies, using a logit model. Table A2 in the Appendix reports the logit model estimates of this propensity score. The estimated conditional probability of deployment (i.e., the propensity score) was then used to construct a matched sample of veterans and non-veterans using nearest-neighbour matching with four neighbours and replacement. In the case of ties, i.e., that two or more control subjects have the same propensity score both, or all of them, are included. I matched within strata defined by the year of birth and calendar year. So, for example, veterans born in 1973 and deployed in 1994 was matched to similar individuals (in terms of the propensity score) among observations made in 1994 of non-veterans born in 1973. In total, the matched sample of non-veterans consists of 9039 (weighted) observations on 8804 unique individuals.

The matching procedure successfully created a comparison group that is very similar to the treatment group in terms of observable pre-deployment characteristics. Table 1 reports descriptive averages for the covariates in the matched comparison group. The standardised difference after matching is no larger than 0.10 for all covariates, which indicates that the covariates are balanced between the treated and the matched comparisons, i.e., that matching on the propensity score has produced a comparison group that is similar to the veterans in terms of observable pre-deployment characteristics.[3] Moreover, as shown in Figure 3, there is a high degree of overlap between veterans and the matched comparison group across the estimated propensity scores (i.e., there is *common support*; see Huntington-Klein, 2021, p. 303 ff. for a discussion of the common support assumption and covariate balance).

One serious limitation of my matching approach, however, is that it cannot account for unobserved characteristics. In order for the observed outcomes for the matched comparison group to be a valid measure of the counterfactual outcomes for veterans, I must assume that no unobserved differences exist between the two groups that both affect the probability of being deployed *and* future outcomes on the labour market. In the literature on matching, this assumption is often referred to as the *conditional independence assumption* (see Huntington-Klein, 2021, pp. 302–303).

This is indeed a strong assumption to make, especially since all veterans in my sample are volunteers who have self-selected into service on grounds I cannot fully observe. An important question when assessing the credibility of the conditional independence assumption in this setting is whether unobserved motivational factors (such as taste for military life or the desire to do good) are correlated with labour market outcomes. Even though studies using Swedish data are scarce, the evidence that does exist tends to emphasise non-pecuniary aspects (e.g., desire for adventure; meaningful personal experience) as the main source of motivation for Swedish peacekeeping soldiers (Hedlund, 2011). It is difficult to hypothesise about a possible

3 Moreover, the sample attrition is similar in the two groups. 20 years after deployment, 5.2% of the observations on the long-term unemployment variable are missing for the veterans. The corresponding number for the matched comparison group is 5.5%.

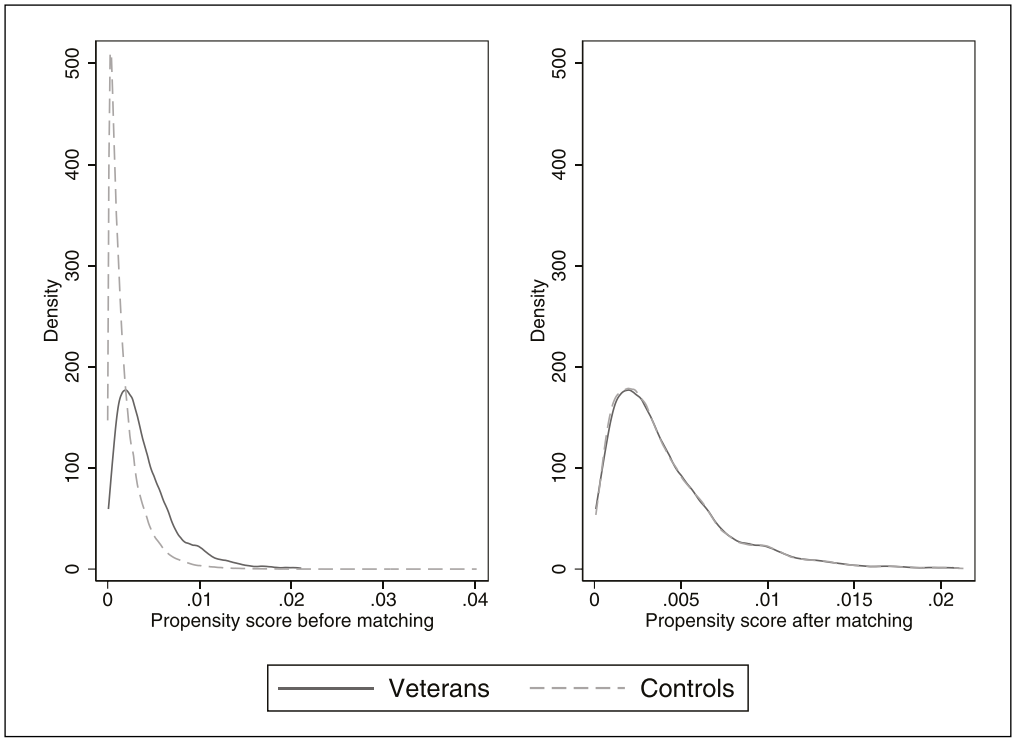

Figure 3 Distributions of the propensity scores before and after matching.

correlation between such motivators and potential labour market outcomes, however. Indeed, Hanes et al. (2010, p. 552) write that they "cannot find any reasonable argument that strong preference for the armed forces should be correlated with higher productivity in civilian life."

I argue, however, that the combination of rich data on individual ability and health, together with information on pre-deployment outcomes, means that the conditional independence assumption, although untestable, is reasonable in this case. In particular, my data lets me estimate the effect of deployment on lagged outcomes, which helps me make a case for plausibility (Imbens & Rubin, 2015).

Even so, it is important to keep in mind that this research design has limitations. The possible existence of unobserved confounders warrants caution when interpreting the results from this paper in a causal way. Importantly, the potential effects from unobservables on outcomes might not yet have emerged in the pre-deployment period for the young individuals in my sample. At the same time, it is also important to point out that, despite issues with identification, the situation on the labour market for these veterans can still be accurately described and compared with the situation for individuals who did not serve abroad, but who are comparable in a wide range of other relevant dimensions.

RESULTS

MAIN RESULTS

The left panel of Figure 4 shows how the outcome variables for the veterans evolved before and after their first deployment to Bosnia. The figures also show the corresponding outcomes for the matched comparison group of birth-cohort peers who did not serve in Bosnia. The right panel of the figure shows the estimate of the average treatment effect on the treated (ATT) for each outcome, together with 95% confidence intervals.[4]

Overall, the results show that deployment to Bosnia in the 1990s is *not* associated with an increased risk of labour market marginalisation in the long run. If anything, the veterans are *less* likely to experience adverse outcomes when compared to the matched comparison group.

4 Standard errors are 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). Bootstrap standard errors based on 500 replications are reported along with robustness checks in Table A3 in the Appendix. The standard errors reported by psmatch2 are very similar to the bootstrap standard errors. Hence, using the standard errors reported by psmatch2 does not change the overall conclusions made in this paper.

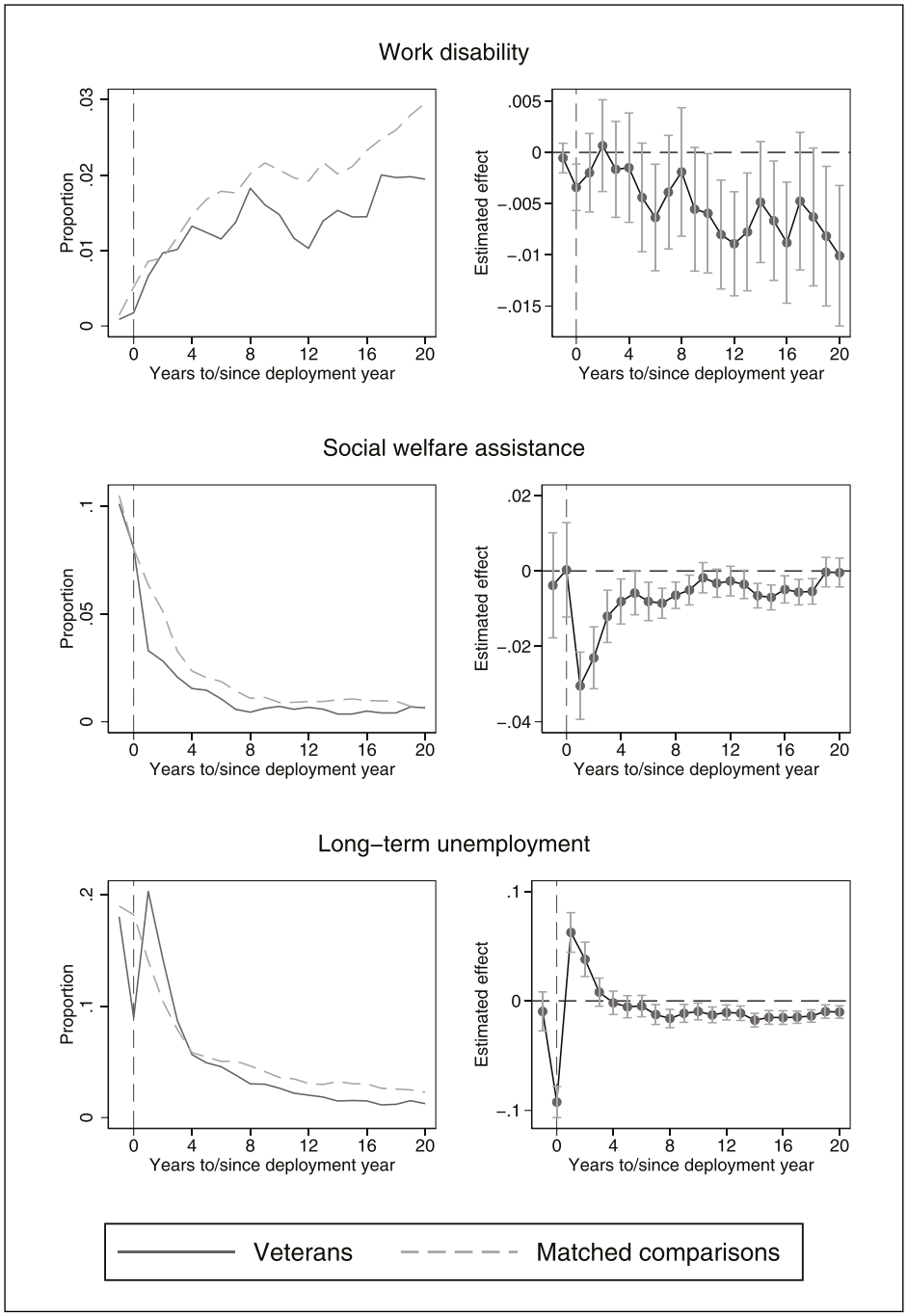

Figure 4 Impact of deployment to Bosnia on labour market marginalisation.

Note: The left panel of this figure shows the observed outcomes for a sample of 2,275 veterans deployed to Bosnia in the 1990s (solid line) together with the outcomes for the matched comparison group of birth-cohort peers who did not serve (dashed line). The right panel shows the estimated average treatment effect on the treated (ATT), together with 95% confidence intervals. All outcome variables are indicator variables (i.e., dummy variables). Year 0 refers to the calendar year when a veteran was deployed for the first time.

At the end of follow-up, 20 years after deployment, the veterans are as likely as the matched comparison group to be receiving social welfare assistance, and less likely to be in long-term unemployment or work disability. Note, however, that the confidence intervals surrounding the estimates of the effect on work disability are simply too large to rule out effects of limited practical importance.[5]

Even though there is no indication of labour market marginalisation of veterans in the long run, the results do reveal that the veterans experience an increased risk of unemployment in the short run. Compared to the matched comparison group, the prevalence of long-term unemployment is higher among the veterans in the two years immediately following the deployment year. The point estimates of the ATT indicate that the risk of being long-term

5 Note also that, for both the veterans and the matched comparison group, the risk of unemployment and receiving social welfare assistance decreases quite dramatically with follow-up time. Keeping in mind that the sample of veterans consist of young individuals, these patterns most likely reflect a process labour market establishment. Furthermore, during the early 1990s, Sweden witnessed a sharp increase in economic distress following the economic crisis at the time; these patterns therefore roughly mirror the way these measures have evolved for the Swedish general population. According to Statistics Sweden (2022), the number of full-year equivalents supported by social benefits and allowances rose by almost 60% between the years 1990 and 1994. Since then, the number has steadily decreased.

unemployed increases by around 6 percentage points during the first year after deployment (corresponding to a 45% increase). During the second year after return, the estimated effect has decreased to around 4 percentage points (corresponding to a 36% increase). From the third year after deployment, and up until the end of the follow-up period of 20 years, the veterans no longer demonstrate any increased risk of long-term unemployment.[6]

As a check for the plausibility of a causal interpretation of the results, note that the estimates of the ATT for the pre-deployment year are close to zero for all outcomes. In other words: the veterans and the matched comparison group are similar with respect to pre-deployment outcomes. Still, I am hesitant towards interpreting the negative effect on the risk of long-term unemployment for longer follow-up times in a causal way; the observed difference in pre-deployment outcomes between veterans and matched comparisons is, if small, not small enough to rule out the possibility that the effect reflects already existing differences in unemployment risk.

To verify that the findings are not driven by the choice of a particular matching strategy, I have also estimated the effects with different matching algorithms. The matching algorithms used are nearest neighbour with one, four and ten neighbours. Figure A3, in the Appendix, reports the results from these robustness checks. In summary, the choice of matching strategy does not affect the conclusions made.

RESULTS ACROSS DEPLOYMENT YEARS

So far, I have examined the effects of deployment to Bosnia on the full sample of young veterans who served there in the 1990s. However, it is important to note that this sample includes individuals who were deployed to Bosnia under very different conditions. For example, the level of conflict and degree of violence varied throughout the studied period, with some veterans being deployed before the ceasefire negotiated in 1995 and others afterwards.

Given these differences, it is possible that the results for the full sample mask variations in the effects of deployment on different battalions. In this section, therefore, I present the results separately by deployment year, in order to better understand the potential effects of deployment on veterans who served in Bosnia during different periods of the conflict.

When I estimated effects for veterans who were deployed in a specific year, the matching model was re-estimated on data for that specific year only. So, for example, propensity scores estimated from the subsample of veterans who was deployed to Bosnia in 1993 were estimated on data from 1993 only; propensity scores for veterans who were deployed to Bosnia in 1994 were estimated on data from 1994, only, and so on.[7]

The results suggest that the increased risk of unemployment in the first few years after returning home is driven by the subsample of veterans who were deployed to Bosnia during the early and mid-1990s. Figure 5 plots the estimated effects (across outcomes) 1 year and 20 years after deployment, respectively, by deployment year. Deployment during the early and mid-1990s is associated with higher risk of unemployment during the first year after returning home, compared to deployment in the later calendar years of the studied period. At the end of follow-up, 20 years after deployment, however, there is no longer any heterogeneity in effects across deployment years. Moreover, deployment in the early- and mid-1990s is not associated with an increased risk of work disability, nor social welfare assistance.

The results also suggest that the decreased risk of work disability (in the long run) and social welfare assistance (in the short run) is primarily driven by the veterans who were deployed in

6 One possibility, however, is that unemployed veterans leave the labour market altogether, and that lower unemployment rates for veterans in the long run reflect some sort of survival bias. To explore whether or not this type of worker discouragement (see, e.g., Dagsvik et al., 2013) occurs among the veterans, I compared employment rates for veterans and the matched comparison group 20 years after deployment. I used a simple measure of employment, consisting of a dummy variable equal to 1, if the individual's yearly gross labour earnings was larger than 0. The results from this additional analysis show that employment rates are very similar in the two groups. 20 years after deployment 97.3% of the veterans are in employment; the corresponding number for the matched comparison group is 96.6%. In other words, there is no evidence in support of the proposition that lower unemployment rates in the long run are driven by worker discouragement.

7 The composition, with respect to pre-deployment characteristics (such as cognitive ability, psychological evaluation and health status) is, with some exceptions, similar across deployment years. However, due to the way that the sample is constructed, veterans deployed in the early years tend to be younger. The average age of veterans deployed to Bosnia in 1993 was 21, whereas the average age for veterans deployed to Bosnia in 1998 was 23.

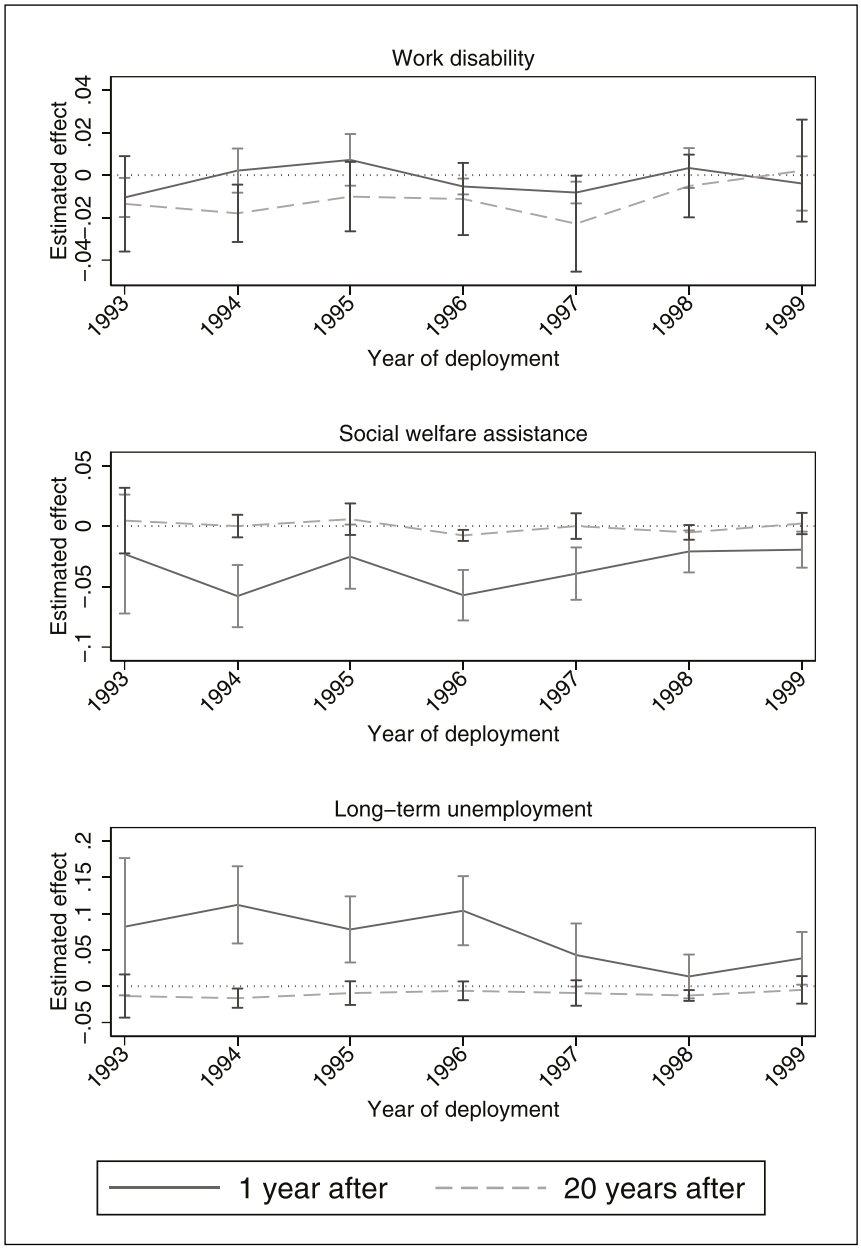

Figure 5 Impact of deployment to Bosnia on labour market marginalisation, 1 and 20 years after deployment, by deployment year.

Note: This figure shows the estimated effect of deployment on labour market outcomes for the sample of 2,275 veterans deployed to Bosnia 1993–1999, 1 year and 20 years after deployment, by deployment year, together with 95% confidence intervals. All outcome variables are indicator variables (i.e., dummy variables).

the early- and mid-1990s. However, the confidence intervals surrounding the estimates are large and it is therefore difficult to draw any definite conclusions regarding the heterogeneity in effects across deployment years for these outcomes.

DISCUSSION

In this paper, I study the relationship between military deployment to Bosnia in the 1990s and adverse outcomes on the labour market for up to 20 years after returning home.

I show that there is no indication of long-term labour market marginalisation for the sample of young veterans who served in Bosnia in the 1990s. As such, my results echo previous studies that highlight the well-being of Swedish peace veterans (Michel et al., 2003, 2007; Pethrus et al., 2019a; Pethrus et al., 2017; Pethrus et al., 2022; Wisén et al., 2021), as well as studies on the labour market affiliation of Danish soldiers returning from international deployment (Elrond et al., 2019; Nissen et al., 2016). For all follow-up times, the prevalence of work disability and social welfare assistance among the veterans in my sample is similar to, or lower than, that of the matched comparison group of birth-cohort peers who did not serve. And even though the veterans did indeed experience an increased risk of unemployment in the two years immediately following their return from service, there is no indication that in the long run their attachment to the labour market was affected negatively by their service. If anything, the results suggest that the veterans, for longer follow-up times, are at *lower* risk of long-term unemployment.

The temporary increase in unemployment during the first few years after returning home most likely represents a period of prolonged job search in the labour market rather than an inability to work. Indeed, the increased risk of unemployment during the first few years after returning home does not correspond to increases in the risk of receiving social welfare assistance or the risk of work disability.[8] Moreover, the increased risk of unemployment is concentrated on veterans who were deployed during the early- and mid-1990s. During these years, Sweden experienced a serious economic crisis, with a sharp drop in GDP and unemployment at levels not seen since the 1930s (see, for example, Englund, 1999 and Bergmark & Palme, 2003, for an overview). Consequently, the veterans who returned from Bosnia in these years were likely to have faced a very difficult situation when trying to re-integrate into the civilian labour market – a task that has been found to be particularly challenging for young workers (Rinz, 2022; Schwandt & Von Wachter, 2019). It is plausible, then, that any temporary increase in the risk of unemployment has derived from a combination of weak attachment to the civilian labour market following absence caused by deployment and harsh labour market conditions at the time of their homecoming. As such, and in line with Angrist (1998), the findings in this paper point to the macroeconomic environment as a simple explanation for the negative effect on earnings observed by Bäckström and Hanes (2023).

As with any study that uses non-experimental data to answer causal questions, caution is warranted when interpreting the results. Despite the high quality and richness of my data regarding observable pre-deployment individual characteristics, the control for common support in data, and the similarity between the treatment and control groups in terms of both pre-deployment outcomes and observable pre-deployment characteristics, there is still a risk of bias from unobserved heterogeneity affecting both the selection into deployment and subsequent labour market outcomes. Future studies should seek ways to improve the internal validity for this question. However, even without a causal interpretation, my study provides novel and valuable descriptive evidence that contributes to a comprehensive understanding of the situation for Swedish peace veterans who served in Bosnia.

APPENDIX

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, B, C/D	Dummies for the individual's military medical serviceability. "A" represents medical requirements for military training in a combat position with the highest demands on physical mobility (e.g., ranger units); "B" represents medical requirements for combat units with high mobility (e.g., armoured units); "C/D" represents medical requirements for combat units with reduced mobility (e.g., infantry units) or technical positions in combat units (e.g., vehicle mechanic). Measured on enlistment test day.
Female	Dummy for whether the individual is female.

(Contd.)

8 In fact, the risk of receiving social welfare assistance *decreases* in the years immediately following the deployment year. The finding that the risk of being unemployed and the risk of receiving social welfare assistance go in different directions might seem contradictory at first sight. However, considering the fact that social welfare assistance is only given to individuals who have exhausted all possibilities for self-sufficiency (including financial wealth), it is not surprising that the number of veterans who receive social welfare assistance decreases during the first years after returning home. Soldiers on mission receive compensation according to a collective agreement that, in addition to an elevated entry-level wage, includes a range of supplements and allowances. The most plausible explanation for this finding is that the accumulation of wealth during deployment meant that the veterans were less likely to qualify for social welfare assistance during the first years after returning home. Whether or not this accumulation of wealth also affects job search behaviour and, by doing so, the duration of unemployment, (see, e.g., Lentz and Tranaes, 2005; Lentz, 2009; and Chaumont and Shi, 2022) is an interesting topic that, unfortunately, is beyond the scope of this paper.

VARIABLE	DESCRIPTION
Foreign	Dummy for whether the individual is born outside Sweden and/or has two parents born outside Sweden.
Primary school	Dummy for whether the individual's highest education is primary school. Measured 1 year before deployment.
Parent high educ.	Dummy for whether the individual has at least one parent with postsecondary 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.
Metro. area	Dummy for whether the individual is residing in a metropolitan area, as defined by the Swedish Association of Local Authorities and Regions (SKR). Measured 1 year before deployment.
East/North/South	Dummy for regional residence. Measured 1 year before deployment.
Parental leave	Dummy for whether the individual received any parental leave benefits. Measured 1 year before deployment.
Study	Dummy for whether the individual received any student grants. Measured 1, 2 and 3 years before deployment.
Unemp.	Dummy for whether the individual was registered as (full-time or part-time) unemployed with the Swedish Public Employment Service, or enrolled in an active labour market program, for 180 days or more during a year. Measured 1 year before deployment.
Social aid	Dummy for whether the individual's household received any social welfare assistance during a year. Measured 1, 2 and 3 years before deployment.
Work disab.	Dummy for whether the individual received disability pension or had 60 or more (net) days of sick leave during a year. Measured 1 year before deployment.
Earnings	Annual earnings in 100s of SEK expressed in 2019 prices. Measured 1, 2 and 3 years before deployment.

	COEF.	S.E.
Cog. ability (1–4)	–0.369	(0.0600)
Cog. ability (6–7)	–0.0953	(0.0528)
Cog. ability (8–9)	–0.289	(0.0769)
Psy. ability (1–4)	–0.686	(0.0852)
Psy. ability (6–7)	0.454	(0.0558)
Psy. ability (8–9)	0.796	(0.0712)
Med. test A	1.148	(0.0964)
Med. test B	0.657	(0.176)
Med. test C/D	0.259	(0.118)
Female	0.493	(0.182)
Foreign	0.00251	(0.108)
Primary school	–0.988	(0.0900)
Parent high educ.	–0.0522	(0.0469)
Parents low educ.	–0.203	(0.0759)
Metro. area	–0.247	(0.0515)
Parental (t–1)	–2.487	(0.579)
North Sweden	0.171	(0.0612)
South Sweden	0.451	(0.0495)
Study (t–1)	–0.728	(0.0584)
Study (t–2)	–0.377	(0.0632)
Study (t–3)	0.167	(0.0616)

(Contd.)

Table A2 Logit model estimation of the probability of deployment to Bosnia, conditional on observed pre-deployment characteristics.

Note: This table shows the 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 to Bosnia, and 0 otherwise. Subsequent observations of first time veterans are censored. The coefficients represent the contribution of each listed covariate to the log odds that an individual is deployed. The predictions from this model are used to construct the propensity score used for matching. Standard errors in parentheses.

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	COEF.	S.E.
Social aid (t–1)	0.166	(0.0827)
Social aid (t–2)	0.143	(0.0877)
Social aid (t–3)	–0.0661	(0.0920)
Unemployed (t–1)	–0.0657	(0.0632)
Work disab. (t–1)	–1.771	(0.708)
Earnings (t–1)	–0.000118	(0.0000325)
Earnings (t–2)	0.00000379	(0.0000125)
Earnings (t–3)	0.00000396	(0.00000854)
Year = 1993	–0.710	(0.107)
Year = 1995	0.0587	(0.0736)
Year = 1996	–0.213	(0.0773)
Year = 1997	–0.660	(0.0817)
Year = 1998	–0.421	(0.0770)
Year = 1999	–1.035	(0.0907)
Age	–0.0425	(0.0168)
Constant	–5.324	(0.396)
Observations	1201188	

NUMBER OF NEIGHBOURS			
	1	4	10
Social aid (t + 1)	–0.029	–0.030	–0.032
	(0.006)	(0.005)	(0.004)
	[0.008]	[0.006]	[0.005]
Social aid (t + 20)	–0.002	0.000	0.000
	(0.003)	(0.002)	(0.002)
	[0.003]	[0.002]	[0.002]
Unemp. (t + 1)	0.056	0.063	0.066
	(0.011)	(0.009)	(0.009)
	[0.012]	[0.011]	[0.009]
Unemp. (t + 20)	–0.014	–0.010	–0.009
	(0.004)	(0.003)	(0.003)
	[0.005]	[0.004]	[0.003]
Work disab. (t + 1)	–0.004	–0.002	–0.002
	(0.003)	(0.002)	(0.002)
	[0.003]	[0.002]	[0.002]
Work disab. (t + 20)	–0.011	–0.010	–0.009
	(0.005)	(0.003)	(0.003)
	[0.006]	[0.004]	[0.004]

Table A3 The impact of deployment to Bosnia on labour market marginalisation, 1 and 20 years after deployment, using bootstrapped standard errors and different matching algorithms.

Note: This table reports estimates of the ATT 1 and 20 years after deployment using different matching algorithms. Alternative matching algorithms include: 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. Standard errors in parentheses. Bootstrapped standard errors based on 500 replications are reported in brackets.

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COMPETING INTERESTS

The author has no competing interests to declare.

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