WORKING PAPER 2021:2

Who recovers from a job loss? The importance of cognitive and non-cognitive skills

Matz Dahlberg
Linna Martén
Björn Öckert

IFAU
INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala.

IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

More information about IFAU and the institute's publications can be found on the website www.ifau.se

ISSN 1651-1166

Who recovers from a job loss? The importance of cognitive and non-cognitive skills[a]

Matz Dahlberg[b] Linna Martén[c] Björn Öckert[d]

January 24, 2021

Abstract

A well-functioning labor market is characterized by job reallocations, but the individual costs can be vast. We examine if individual's ability to cope with such adjustments depends on their cognitive and non-cognitive skills (measured by population-wide enlistment tests). Since selection into unemployment is a function of skills, we address the endogeneity of a job loss by exploiting the exogenous labor market shock provided by the military base closures in Sweden following the end of the Cold War. We find that labor earnings decrease and unemployment and social insurance benefits increase for displaced workers. In particular, individuals with high cognitive and, especially, non-cognitive skills face shorter unemployment spells than the individuals with low skills.

Keywords: Cognitive and Non-cognitive skills; Displaced workers; Unemployment; Plant closure; Defense draw down
JEL classification: J63, J65, H56

[a]We thank Karin Edmark, Marcus Eliasson, Peter Fredriksson, Erik Lindqvist, Eva Mörk, Erik Snowberg and seminar participants at Uppsala University, at the 2013 Nordic Summer Institute in Labor Economics, at the 2015 SOLE/EALE Meeting in Montreal, and the 2018 CESifo Conference on Employment and Social Protection in Munich for helpful comments on earlier drafts of this paper. An earlier version of the paper, Dahlberg et al. (2013), used data that contained less precise information when constructing treatment and control groups and that contained no skill measures.

[b]Uppsala University and IFAU, matz.dahlberg@ibf.uu.se, affiliated with CESifo, IEB at University of Barcelona, Uppsala Center for Labor Studies, and Uppsala Center for Fiscal Studies

[c]Uppsala University, linna.marten@sofi.su.se, affiliated with Uppsala Center for Labor Studies and Uppsala Center for Fiscal Studies

[d]IFAU and Uppsala University, bjorn.ockert@ifau.uu.se, affiliated with Uppsala Center for Labor Studies

Contents

1 Introduction

Job reallocation is a natural consequence of economic development and a well-functioning labor market, where more productive companies and plants grow at the expense of the less productive. Yet, as noted by several scholars (e.g., Eliason and Storrie, 2006; Huttunen et al., 2011) there are two sides of the story. While reallocations are beneficial from an economy point of view, they can be detrimental for those individuals directly affected by downsizing and closures. For example, several studies have found that displaced workers experience costly spells of unemployment and earnings declines (see e.g., Stern, 1972; Jacobsen et al., 1993; Couch and Placzek, 2010) and a deteriorated health (see e.g., Sullivan and von Wachter, 2009; Brand, 2015; Black et al., 2015).

It is well-established that the risk of losing a job is unevenly distributed in the population (see Hines et al., 2001, for a survey), and that individuals with lower skills face a higher risk of unemployment (see e.g., Lindqvist and Vestman, 2011; Öckert, 2011; Seim, 2013). We know much less about the importance of individuals' skills for the length of unemployment spells. To the extent that low-skilled individuals also experience longer unemployment spells, the societal costs of job reallocation may be substantial. To design effective policies, it is therefore important to know if the effect of a job loss, in terms of income loss and unemployment duration, is heterogeneous with respect to skills, and if that is the case, whether cognitive or non-cognitive skills are more important.

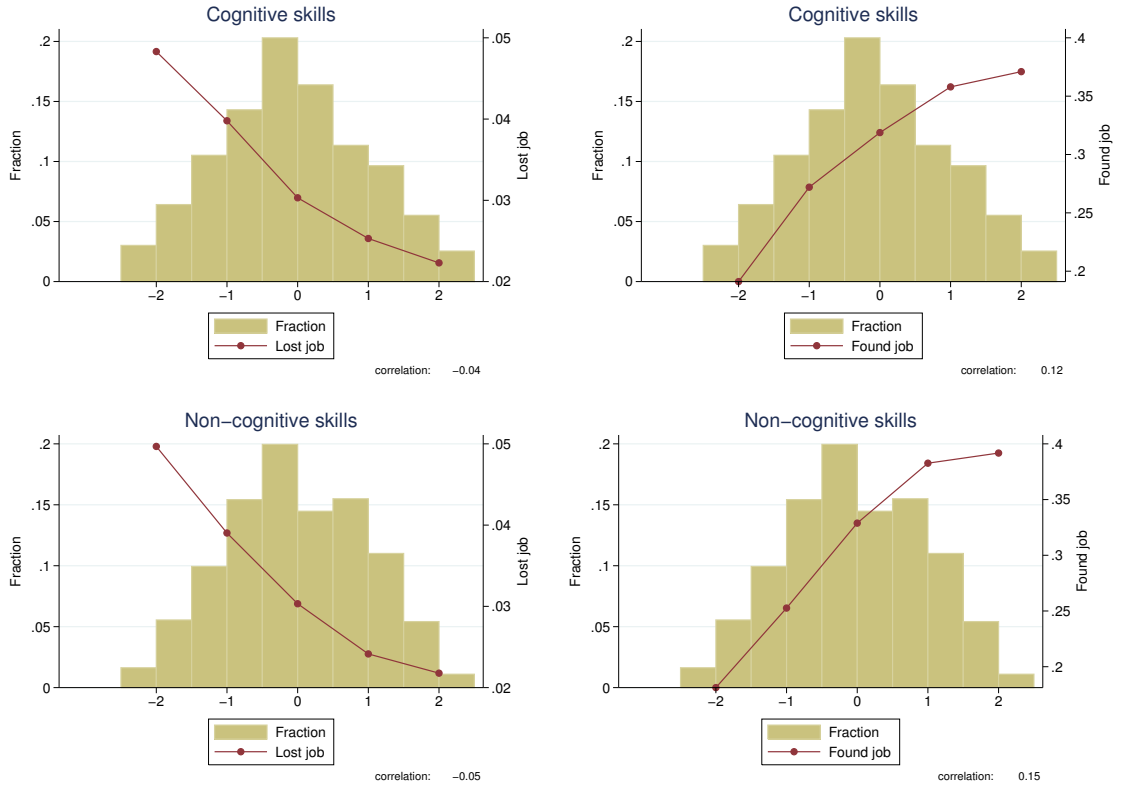
To provide descriptive evidence of the importance of skills for the transition to and from unemployment, respectively, Figure 1 displays how cognitive and non-cognitive skills are distributed among those who entered or exited unemployment in 2013.[1] The probability of entering into unemployment is highest in the lower part of the skill-distributions, while the opposite is true for those who exit unemployment. Figure 1 indicates that cognitive and non-cognitive skills might be important for the transition to new employment after a negative labor market shock. The description also shows that there is an important, non-random, selection of individuals' entry and exit from unemployment based on cognitive and non-cognitive skills. This pattern could simply be due to the fact that low skilled individuals work in sectors with more layoffs, or are discharged for performance-related issues, making it harder to find a new job.[2] It is also possible that high-skilled individuals have access to high quality networks, a more effective job search behavior, a higher

[1] Cognitive and non-cognitive skills are measured at military enlistment. See the data section for a full description of our data. In the introduction we use labor force participants with measures of cognitive and non-cognitive skills for *all* sectors, while we only use those employed in the military sector in the rest of the paper.

[2] Abowd et al. (2009) find that firm closures occur substantially more often in firms that hire a disproportionately high share of worker with low human capital.

flexibility in adopting new skills, or new potential employers simply favored them in the application process by (see e.g. Neal, 1998).

Figure 1: Exit and entry to unemployment over the skill distribution



Note: The sample is restricted to all men age 25-50 in 2013. Individuals who lost a job were registered at the Employment office during 2013, but not in January 2013. Individuals who found a job were registered at the Employment office during 2013, but not in December 2013 (and also had labor income). Points show the average share who lost or found a job within each standard deviation, and the sample is restricted to individuals who had labor income (for the job loss share) or were registered at the employment office (for the share who found a job) in 2013. Histograms are not restricted to individuals with a specific employment status, and excludes outliers (more than 2.5 sd). The skills measures are standardized by enlistment year for the full population, with mean 0 and standard deviation 1. Correlation coefficient between employment status and skill is displayed in the right corner.

The purpose of this paper is to examine whether there are heterogeneous effects of a negative labor market shock in terms of cognitive and non-cognitive skills. In doing this, we will, first, examine the average labor market effects for those affected by a job loss. Second, we will evaluate to what extent the effects depends on the individual's cognitive and non-cognitive skills. Since individuals are selected into unemployment based on skills, we will use the exogenous labor market shock provided by the substantial military base closures in Sweden following the end of the Cold War. The Swedish military sector experienced major cut-backs during this period. In 1995 the Swedish Armed Forces employed around 26 000 individuals, whereas only 16,000 were employed by 2009

(Hedin, 2011).

There is a large literature focusing on displaced workers and the economic difficulties that they face. In the short run, the cost of displacement is ascribed to forgone earnings during unemployment as well as the loss of firm- and industry-specific human capital (e.g. Hamermesh, 1987). Besides such mechanisms, the long-term effects, often referred to as unemployment scarring, are explained by the loss of general human capital and the tendency for employers to view individuals' labor market history as a signal of productivity (see e.g., Böheim and Taylor, 2002; Arulampalam et al., 2001). There are strong reasons to think that involuntary job losses determined by unobserved individual characteristics, such as human capital and productivity.[3] Hence, most studies focus on events such as mass layoffs or plant closures, where the separation is thought to be independent of a worker's quality. Yet, expectations of a forthcoming firm closure might cause a selection of the remaining employees, e.g. if workers with better labor market opportunities choose to quit prior to the shutdown. To deal with this problem most studies use a time window before the closure and define all separations during this period as displacements.[4] Several studies from both the US and Europe have found that displaced workers experience periods with earnings decline (see overview by von Wachter, 2010). Although the initial drop in income decreases over time, many studies find long-run effects, which compared to pre-displacement earnings varies between 13-25 percent in the US (Jacobsen et al., 1993; Couch and Placzek, 2010), 12 percent for Germany (Schmieder et al., 2010) and 7 percent for Sweden (Eliason, 2011). The decline in earnings could be due to unemployment, that individuals are leaving the labor force, or to the fact that there is a decline in the displaced workers re-employment wages. Some studies find that the long-term effect is mainly driven by lower wages (Schmieder et al., 2010), while others find that the losses are mainly due to periods of non-employment (Hijzen et al., 2010).

Several papers have showed that cognitive and non-cognitive abilities are important determinants of schooling, employment and earnings (see e.g., Herrnstein and Murray, 1994, Bowles et al., 2001, Heckman et al., 2006), but there is scarce evidence on the role played by cognitive and non-cognitive skills in the job reallocation process. Lindqvist and Vestman (2011) use enlistment information to examine the relationship between cognitive and non-cognitive skills and unemployment and labor market earnings. They find that men who fare poorly in the labor market lack non-cognitive rather

[3] Using displacement announcements, Seim (2013) shows that there is selection into displacement based on age as well as cognitive and non-cognitive skills.

[4] This method has been criticized for being arbitrary and to use ad-hoc definitions, thereby failing to fully capture the selection process (Schwerdt, 2011).

than cognitive ability. Conditional on being unemployed, individuals with high non-cognitive ability experience shorter spells,[5] while cognitive ability has no statistically significant association with the duration of unemployment. However, as is evident from Figure 1, individuals who are unemployed in a given period are not a randomly selected group, and high and low skilled individuals are likely to lose their job for very different reasons. The closest study to ours is Seim (2013), who examines displacement announcements from Sweden combined with enlistment data. Seim shows that individuals with low cognitive and non-cognitive skills increase their participation in job training programs following the displacement announcement, whereas there is no significant increase for high skilled respondents.

This paper makes three contributions. First, to our knowledge we are the first to study if individuals' unemployment duration following a plant closure are heterogeneous with respect to direct measures of skills.[6] We have access to information on the individuals' cognitive (IQ-tests) and non-cognitive (evaluations by psychologists) skills from the military draft in Sweden. Enlistment was compulsory for men in Sweden until 2010, and since we examine the effects of military base closures, all affected military personnel, and most of the affected civilian personnel, have done the enlistment tests. This puts us in a unique position for evaluating the role of cognitive and non-cognitive abilities following a job loss.

Second, compared to the existing displacement literature, we argue that we have a strong case for exogenous treatment. To deal with the selection of individuals who experience displacements, most earlier studies focus on events such as mass layoffs, displacement announcements or plant closures. These events are typically identified through administrative registers, which can be problematic given the limited information on how and why the layoffs occurred. In particular, individuals who are displaced following a mass layoff or a displacement announcement are unlikely to constitute a random sample, and displacement announcements do not necessarily result in a job loss. These problems are mitigated in our case. The end of the Cold War denoted the beginning of a new geopolitical landscape, and the government announced several defense bills, resulting in closures of multiple military bases. Although military downsizing was expected at this point in time, it was not known which bases would be affected until the bills were announced. Hence, we argue that the displacements that occurred were exogenous and unexpected, at least from the perspective of

[5] A one standard deviation increase in non-cognitive skills decreases the unemployment duration by approximately 10 days.

[6] That individual characteristics such as education, sex, and age correlate with post-displacement outcomes has been shown by Farber (2003). Carrington (1993) also find that those who switch industries following displacement have systematically larger earnings losses.

the employees, and the descriptive statistics, presented later in the paper, strongly supports this claim.

Third, following the end of the Cold War, reductions of military personnel and closures of military bases were carried out in many of the salient military powers, such as the UK, France and Germany. The closures were often expected to have tremendous negative consequences for the affected region (e.g. Warf, 1997), but most studies (see e.g., Hooker and Knetter, 2001; Andersson et al., 2007; Paloyo et al., 2010) find small or insignificant effects on local economic growth, employment and migration.[7] Yet, it is unclear if these results also apply to employees that were directly affected. To the best of our knowledge, this is the first paper studying the effect of military base closures using individual level panel data covering all military employees.[8]

We have access to population-wide register data with rich individual background information covering a fairly long period (1990–2013). We focus on the effects on labor earnings, employment related income, and disposable income, and also look at the number of days an individual is registered as a job applicant at the Employment office. The employment register allows us to exactly identify not only those who are employed by the military and at what bases they are employed (giving us a well-defined treatment and control groups), but also in what capacity they are employed. Military employees consist of two distinct groups, military personnel and civil personnel. This distinction is important since the two groups had different types of employment contracts, but also because the skill distribution for civil employees is more similar to the average population (see Figure A.4).

To estimate a causal effect, we employ a difference-in-differences approach where we compare military employees at bases that were affected by the bills in 1996, 1999, and 2004 to employees at unaffected bases. Using linked employer-employee data, we construct a panel of yearly register data for all individuals that were employed in the military sector two year before the bills were announced, and one year before a working group was given the task to draft a proposal of what bases to close down. This data allows us to follow individuals over several years, making it possible to separate the treatment effect over time, in order to fully capture its dynamics. By using a

[7] One potential explanation to the absence of negative effects is the fact that the opportunity cost of the military bases, for example land and buildings, has been overlooked. Many bases have been reused for civilian purposes, which can have positive implications for the community and local businesses and thereby contribute to economic growth.

[8] Jakobsson (2010) find that military employees in Swedish municipalities affected by the closures in 1999 on average perform better than non-military employees in the same municipality 5 years after the closures regarding factors such as unemployment and labor income. The data is cross-sectional, and given the initial difference between the groups, the difference cannot be interpreted as causal. Eriksson and Hallsten (2003) follow civil employees affected by closures in 1996 both before and after the closures. They find a lasting depreciation in several health indicators, primarily among males and regarding indicators related to anxiety. Given the lack of a control group the results cannot be given a causal interpretation.

relevant control group, consisting of military employees at units that were not affected by the reform, we isolate the causal effect of the closures on unemployment and labor income.

We find that, on average, labor earnings decrease and unemployment and social insurance benefits increase for those affected. Treated individuals with high cognitive and (in particular) non-cognitive skills face shorter unemployment spells than those with low skills. However, we do not, in general, find any heterogeneous treatment effects on the other outcomes. Given that low-skilled individuals fare the worst in the job reallocation process, it can be motivated to identify and direct different labor market policies towards those individuals during the recovery process. Policy makers might also want to consider policies aiming at generally improving the cognitive and non-cognitive skills of individuals in the lower end of the skill distribution. This could, for instance, be done by improving these skills through early childhood interventions (see discussion by Heckman, 2008).

The remainder of the paper proceeds as follows. The next section describes the institutional background, section 3 describes the data and measurements, section 4 formalizes the empirical strategy, section 5 presents the empirical results, and, finally, section 6 concludes.

2 Institutional background

The objective of the Swedish security policy changed drastically after the end of the Cold War. A foreign invasion aiming to occupy Sweden was no longer seen as possible, although attacks at more narrow objects in Sweden could not be dismissed. The primary focus of the armed forces shifted from the ability to halt a military incursion to participation in international peace-keeping interventions. These changes implied cut-backs in spending on the national defense as well as closure of a number of military bases. The government announced the military bases that were to be closed in a number of government bills.[9] In the first step, following the defense bill in 1996, the Swedish defense, which had previously focused on the threat of invasion, was said to transfer from an invasion defense to an adaptable defense. In 1999 and 2004 the next step was taken, as the Swedish defense was declared to move towards an interventionistic defense. The 1999 bill amounted to one of the largest reorganizations of the Swedish Armed Forces (SAF) in modern times. The bills in 1996, 1999, and 2004 resulted in the closure of a number of battalions, forces, regiments, and headquarters compounds. The closures occurred within 1-2 years after the announcement,

[9]The key government bills during this period were: Prop. (1991), Prop. (1995), Prop. (1999), Prop. (2004), and Prop. (2008). See the map in Figure A.3 for an overview of which bases were closed following the decisions in 1996, 1999, and 2004. The bill in 2008 did not result in any closures.

and all military units were closed in some municipalities, while only a few units were affected at other places .

Before deciding which bases to close down, the SAF was given the task to draft a proposal of how to reduce the work force and cut expenditures. The government declared what consideration should be taken into account. For instance, the bill in 1999 stated that military, economic, regional and environmental pros and cons should be evaluated. Due to military strategic concerns, there was a clear aim to have military bases geographically scattered over the country. At the same time, the fact that these considerations should be weighed against cost efficiency (e.g. necessary investments and synergy effects), environmental factors (e.g. availability of permits and training sites), and regional political considerations, made it difficult to predict which units would be closed down. Even places with recent investments (e.g. Ängelholm) or strategic positions (e.g. Gotland) were closed.[10] The proposals were then processed in the parliament, which caused increased uncertainty, not at least due to the fact that the Social Democratic government in office lacked a political majority. The initial proposals were modified on several occasions before the parliament adopted them.[11] Overall, it is clear that the decision of where to close down was based on factors unrelated to the workers' productivity. Although the bills were preceded by much debate and speculation, we argue that there was substantial uncertainty around which units would be affected, and that individuals employed by the military two years before the announcements could not foresee the upcoming bills at that time.

Individuals working in the military sector are employed with a military or civil contract. Military employees consist of individuals working as career officer, soldiers or mariners, whereas civil employees include, among others, mechanics, administrators, and health care staff. When the downsizing of the defense was implemented it was decided that military employees would not be dismissed due to redundancy in order to prevent an aging workforce, whereas civil employees could

[10]By the end of the 1990s the new battle airplane JAS 39 Gripen had just entered services within the Swedish Air Force, which required the Wings to adjust. Major investments had recently been made to accommodate the new airplanes at the F10 Wing in Ängelholm. Yet, the government decided to close down the F10 Wing in the 1999 bill, whereas they kept the nearby F17 Wing, which had not yet been prepared to accommodate the new airplanes. Also, even though the government had previously stated that military presence at Gotland (an island between Sweden and Russia) was necessary for strategic reasons, P18 was closed following the 2004 bill. Due to recent developments there are currently discussions to militarize Gotland once again.

[11]The decision in 1996 and 2000 was a compromise between the Social Democrats and the Centre Party. The government made several adjustments to the 1996 proposal, allegedly influenced by the intense lobbying from politicians in the municipalities that would initially be affected by the closures. The decision in 2004 was preceded by political turbulence as the Social Democrats lacked a majority for their proposal and the Left Party threatened to vote for the opposition's proposal if the military base in Arvidsjaur was closed. Arvidsjaur was finally degraded to a detachment belonging to Boden, but without any reduction of the size of the work force. The closures suggested in the SAF proposal preceding the last government bill, Prop. (2008), were never implemented, as the government decided to cut the grant for work material instead of reducing the work force.

be dismissed due to shortage of work. A number of rather generous initiatives to promote both civil and military personnel to leave voluntarily were launched already in the early 1990s, such as early retirement and career alternation programs.[12] All military personnel at the closing bases were given priority if they applied to vacancies at other bases, and otherwise offered a new position within SAF that wouldn't necessarily match their skills. They were only dismissed due to redundancy if they declined an offer to transfer to another military base. Furthermore, military employees with special contracts (*fullmaktsanställning*), whose military bases was about to close down, were offered severance pay of at least 6 months if they resigned. Civil employees who did not go for early retirement or career alternation programs were dismissed due to redundancy.

The closures generated objections and protests from both the public and politicians in the affected municipalities, since they were expected to have vast negative consequences. Local politicians attempted to overrule the decision, and at some of the affected places the inhabitants demonstrated against the decision to close their military base.[13] The government started adjustment programs for some municipalities after the closures, in particular following the 1999 and 2004 bills.[14] As far as we know there is only one evaluation of the programs following the 1999 bill, where Falkenhall (2004) concluded that they appeared not to have had any major impact.[15] It is obviously difficult to determine what long term effect the programs had on employment in the affected municipalities, but it is important to note that they were not directed towards the newly displaced workers. To

[12]Those employed with a military contract prior to 1992 had contracts that guaranteed stronger employment protection (*fullmaktsanställning*), meaning that they could not be dismissed due to redundancy. The ministry of defense was unwilling to dismiss other military employees due to redundancy since it would require discharging primarily younger individuals and probably induce a hiring freeze. Given that the average age of the workforce was already thought to be too high, this would only enhance the problem. Employees could get early pension income from age 55/58 with a military/civil contract (see Hallberg et al., 2015 for an analysis of this proposal on health outcomes). Employees older than 35 years (with a military contract) or with at least 15 years tenure (with a civil contract) who resigned from the SAF could continue receiving part of their wage (up to 2 years), conditional on starting to study, starting their own company, or doing an internship that was expected to lead to a stable employment. Between 1999 and 2010 somewhat more than 19,700 employees resigned from the SAF, of which only 1550 transferred to old age pension. The vast majority did so with some form of early age pension or due to their own request (Hedin, 2011). Programs that promoted career alternations were not very successful, and Blomsterberg and Kadefors (2009) argue that this can partly be explained by the fact that military servants have a strong professional identity and are thus unwilling to change occupation.

[13]These protests received much attention in the media, for example SvD (Nov 11, 1999), TT (Sept 30, 1999), TT (June 27, 1999), TT (Sept 23, 1999), and DN (Nov 1, 1999).

[14]Following the 1996 bill only Söderhamn was given support. The municipalities affected by the programs following the 1999 bill were Boden, Falun, Gotland, Härnösand, Hässleholm, Kiruna, Karlsborg, Karlskoga, and Sollefteå. There were no closures of military bases in Karlsborg and Karlskoga, but they were included in the programs since they had experienced downsizing of their military units and military industry. The affected municipalities following the 2004 bill were Arvidsjaur, Östersund, Kristinehamn and Gotland.

[15]The programs were supposed to relocate 1280 government jobs to the affected municipalities and, by grants to private companies, create 1000 private job opportunities. Evaluating the effects of these programs Falkenhall (2004) found that only 60 % of the government jobs had been relocated, whereas few private jobs had yet been realized. The grants directed at private companies were only paid out if new hiring occurred. By the end of 2002, Falkenhall (2004) found that the number of new hires only reached 62. The affected municipalities were also given general regional policy aid, and most of it was used to finance different projects, such as pilot studies. Falkenhall concluded that it was unclear if these would bring about permanent employment when the project ended or ran out of funds.

the extent that the programs were successful for our treated municipalities, our estimations should underestimate the full effect of the closures.

3 Data description and summary statistics

3.1 Data description

Our primary data is a linked employer-employee dataset (LOUISE) covering the full population 1990-2013, compiled for research purposes by Statistics Sweden. The dataset is collected on a yearly basis and contains information on individual characteristics such as income, employment, and education. The data links all individuals to their employers, providing information on which sector the individual is employed in and where the establishment is located. This information makes it possible to identify all individuals employed by the SAF (separately coded for army, marine, air force, management, common operation, and home guard). We define individuals as military employees if they are employed by army or air force, and these include both employees with a military and civil contract.[16] Information about municipalities is mainly compiled by aggregating individual level information from LOUISE, although the information about political majority comes from surveys made by the Swedish Association of Local Authorities and Regions.

We match our data set with assessments of both cognitive and non-cognitive skills from the military enlistment, which are available for the period 1969-2005 (full coverage from 1970). Most men enlist the year they turn 18 or 19 years. The procedure takes two days and includes tests of mental and physical fitness. All men take a cognitive test battery that consists of four different sub-tests; inductive ability, verbal comprehension, spatial ability, and technical understanding (including questions about chemistry and physics). The non-cognitive skills are assessed through an interview with a certified psychologist. The interviews lasts for about 20 minutes and focus on how the interviewee behaves rather than thinks. The psychological evaluation measure four different dimensions of non-cognitive skills; social maturity (extroversion, friendships, responsibility taking, independence), intensity (self-motivated, intensity and frequency of free-time activities), psychological energy (perseverance, ability to fulfill plans and remain focused), and emotional

[16] We are not using the marine since only one municipality (Härnösand) had a complete closure of all military activity and we lack similar control municipalities –Härnösand employed 200 individuals, whereas Karlskrona and Haninge (the marine units that remained during the full period) employed around 2000 each. We also exclude Linköping since their military base transferred from belonging to the army to belonging to the air force during the period. We also exclude Kristianstad since they have not, unlike other municipalities, registered SAF employees belonging to the army and the management as separate groups (all are coded as belonging to the management).

stability (anxiety inclination, ability to control/handle nervousness, stress).[17] s.

We use the sum of the four cognitive tests (measured on a 1-9 Stanine scale) as our main measure of cognitive skills. The psychological evaluation is graded on a five-point scale for each dimension, but the psychologist also makes an overall judgment of the "psychological fitness for military service" (on a Stanine scale). The overall judgment is not a direct function of the four dimensions, and in order to make use of all available information, we calculate the overall judgment for each unique combination of points from the four dimensions. We use this as our measure of non-cognitive skills. To ease the interpretation, we standardize each variable by enlistment year, with mean 0 and standard deviation 1 (for the full population).

We define treated individuals as those working within the army or the air force in a municipality where the SAF closed down all activity in the wake of the bills in 1996, 1999, and 2004, and sample them two years before the bills.[18] The control group are those working within the army or the air force at a military base that does not close down during the period covered by the dataset. However, note that military bases in the control group are partly affected by the downsizing. Given that military employees were offered to stay within the SAF, we believe that most of these employees were able to stay at their current military base.[19] To the extent this was not the case, we expect to underestimate the full effect of the military base closures.

Using the closures that took place in the wake of the bills, and given the length of our panel data, we can follow the individuals 6 years before and 9 years after the bill is announced. We sample the individuals in year -1, the year before the working group tasked with drafting a proposal is created, in order to avoid selection due to early leavers. We do not restrict the control group to be continuously employed during the post-period. Since test scores only have full coverage from 1970 the sample is restricted to individuals who are younger than 45 years in the year of a given bill, and we also drop individuals younger than 25 to increase the probability that they have finished their studies. In line with previous studies on displacement (e.g. Couch and Placzek, 2010) we also restrict them to have tenure, although we only restrict it to two years to avoid dropping too many observations.

[17] For further details see Mood et al. (2012). Correlations for cognitive and non-cognitive measures and the underlying tests are displayed in Table A.18.

[18] According to the bill in 1999 the Wing in Uppsala would not be closed, but the year after the decision was reversed, and we thereby add Uppsala to the closures following the 1999 bill.

[19] Figure A.1 also show that the separation rate is similar before and after the closures in the control group. A bit more than 70 percent of the control group remained employed by the SAF in the last year, compared to around 35 percent of the treated individuals.

3.2 Summary statistics

Table 1 shows means and standard deviations of pre-determined varables for the military employees included in our analysis, divided by treatment status. We also run bivariate regressions to test for the difference between the groups. The individuals are very similar with respect to lagged outcome variables as well as socio-economic and demographic characteristics.

Table 1: Summary statistics: Military employees

	Control	Sd	Treated	Sd	p-value
Civil	0.29	0.46	0.27	0.44	0.57
Air Force	0.31	0.46	0.41	0.49	0.59
Women	0.0042	0.06	0.0058	0.08	0.64
Age	32.1	5.27	32.1	5.24	0.95
Immigrants	0.021	0.14	0.023	0.15	0.45
Education	4.75	1.67	4.66	1.78	0.65
Married	0.37	0.48	0.36	0.48	0.84
Children	0.48	0.50	0.46	0.50	0.49
Labor income	3001.2	1073.31	2966.8	1085.72	0.79
Days unemployed	6.29	40.70	7.24	44.12	0.64
Unemployed (%)	0.034	0.18	0.037	0.19	0.75
Disposable income	2010.6	747.76	2048.1	760.55	0.77
Social insurance	86.9	231.97	93.0	247.71	0.52
Non-cognitive skills	0.86	0.82	0.85	0.84	0.84
Cognitive skills	0.61	0.78	0.64	0.79	0.44
Observations	7643	.	2909	.	.

Note: All variables are measured at year -1. The table presents mean values as well as p-values from bivariate regressions examining if demographic characteristics affect the probability of treatment. Civil is a dummy for individuals expected to have a civil contract.[20] Immigrant is a dummy for individuals with a registered immigration year, Education level is measured on a 7-point scale (increasing in years of education), Children refers to individuals with children younger than 18, Unemployed (%) is a dummy for individuals registered at the Employment Office and Days unemployed measure the number of days registered. Employed refers to individuals working in November. All income variables are given in 100 SEK ($\approx$ \$ 12) with 2013 year value.[21]

Even though the individuals are very similar, a potential concern would be that they face very different labor market opportunities, given that they live in different municipalities. Hence, in Table 2 we also show the differences between the treated and untreated municipalities. It is evident that the municipalities are very similar, with similar employment and income levels. This tells us that on an aggregate level, municipalities that will eventually become treated are very similar to other

[20]While we do not know exactly what contract individuals had, we can use information about their occupation code (SSYK) as a proxy. We code individuals with SSYK 0110 (which includes officer, military, soldier etc) as military personnel, and everyone else as civil. SSYK is only available from 1995, for individuals without information about SSYK we code those with any form of military education as having a military contract.

[21]Labor income includes pre-tax wage, sickness allowance (if paid by employer), allowance for expenses, and severance pay. Disposable income is calculated by Statistics Sweden and individualized from household income. It constitute the net from all types of earnings and taxes. Social insurance refers to employment related income (consisting of unemployment benefits, sickness allowance, early retirement pensions etc.).

municipalities that also host military units.

Table 2: Summary statistics: Municipality

	Control	Sd	Treated	Sd	p-value
Women	0.49	0.01	0.49	0.01	0.21
Age	41.0	2.44	40.9	2.26	0.85
Immigrants	0.10	0.06	0.11	0.06	0.75
Education	3.10	0.43	2.96	0.38	0.26
Married	0.46	0.04	0.45	0.04	0.21
Children	0.39	0.04	0.40	0.03	0.73
Labor income	1494.6	146.15	1460.8	153.30	0.44
Disposable income	1495.2	202.75	1460.6	191.69	0.56
Social insurance	321.4	51.40	330.8	49.48	0.53
Employed	0.66	0.04	0.66	0.02	0.90
Population	27521.0	20476.62	36531.1	28431.11	0.19
SAF employees	917.0	501.90	700.1	387.43	0.12
Left majority	0.71	0.46	0.64	0.50	0.68
Observations	36	.	17	.	.

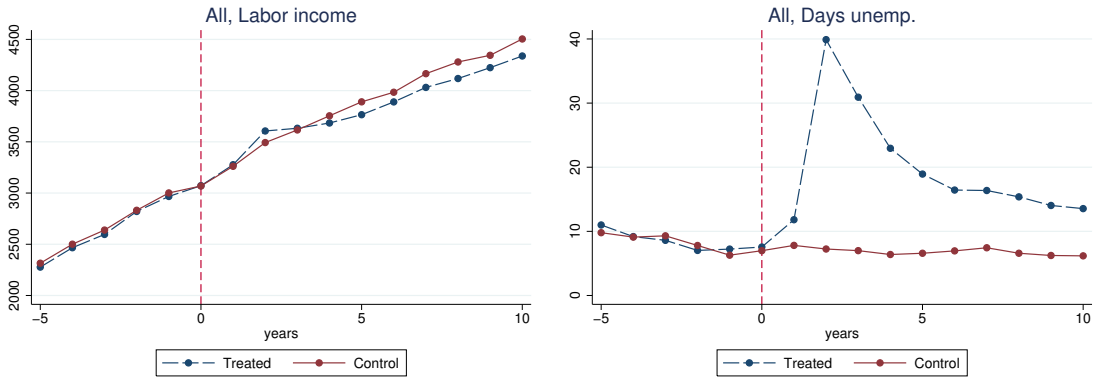
Note: All variables are measured at year -1. Left majority is a dummy variable for municipalities were the governing party/coalition only consists of the Left Party, the Social Democrats or the Green Party.

4 Econometric specification

The descriptive statistics suggest that the military bases that closed down were as good as randomly selected. However, even though there are no observed differences between treated and control bases, they may differ in unobserved factors. Therefore, we use a difference-in-differences approach in which we compare the change in labor market outcomes over time (before and after the defense bills) between treated and untreated individuals. Individuals working in the military bases preceeding the bills in 1996, 1999 and 2004 are pooled, and we re-center the timing of the bills (with year 0 representing the formation of the working group). Since we use a difference-in-differences setting, we need to confirm that the two groups develop similarly prior to the treatment. Figure 2 plots an event study of yearly labor income and days unemployed for the control and treatment group, respectively. It is clear that the groups track each other closely before year 0. We see a small increase in days unemployed the year the bill is announced (year 1), and a sharp increase in the following year, reaching almost 40 days in the treatment group, compared to 10 in the control group. The average number of days unemployed remains higher in the treated group compared to the control group during the whole decade following the bills. For labor income, we see a brief early increase (probably capturing severance pay) followed by a drop of up to 20 000 SEK in yearly

labor income for the remaining years.

Figure 2: Event studies of labor income and unemployment: full samples



Note: Income (2013 year value) is given in 100 SEK ≈ $ 12. Days unemployed refer to the number of days an individual is registered at the Employment Office in a given year. The government bills were announced in year 1.

The baseline model is given by:

$$y_{imbt} = \alpha_0 + \alpha_1 X_{im} + \sum_{t \geq -5}^{10} \beta_t D_{mt} + \lambda_{mb} + \kappa_{ibt} + \varepsilon_{imbt} \tag{1}$$

The outcomes of interest, y_{imbt}, represents yearly labor market income, days registered as unemployed, disposable income or social insurance, for individual i, in municipality m, bill b (1996, 1999, 2004), and year t (-5 to 10). We construct a dummy variable that indicates treatment, D_{imt}, and interact it with all year dummies to be able to estimate separate treatment effects, β_t, for all years. By presenting the effects both for the years before the defense bills were announced (year 1), and the nine subsequent years, we can show that the parallel trend assumption is fulfilled as well as examine all dynamics of the effects of the defense bills. We also include dummies for being in the Air Force and having a civil contract in X_{im}. In order to account for the fact that the composition of workers may change between bills, we include bill-by-municipality fixed effects, λ_{imb}.[22] Finally, κ_{ibt} are bill-by-year fixed effects that captures aggregate shocks that can affect labor market outcomes. Standard errors (ε_{imbt}) are clustered at the municipality m, to allow for correlation of the error term across different time periods and bills.

Equation (1) follows the typical econometric specification in the displacement literature. To exam-

[22]These are based on the municipalities where the individuals worked in year 0, and not where they work or live in subsequent years

ine whether there are any heterogeneous effects in terms of cognitive and non-cognitive abilities, we will re-estimate equation (1) and run a fully interacted model where all variables are interacted with one of our three different measures of cognitive and non-cognitive skills. We use either a dummy for being above the sample median in the skill distribution, the standardized measure of cognitive and non-cognitive skill, or the standardized measure transformed to percentiles (to avoid the influence of outliers).

5 Individual labor market effects of base closures

In this section we present our results showing the labor market effects for the individuals affected by the base closures. In section 5.1 we present the mean effects, in section 5.2 we present heterogeneous effects with respect to cognitive and non-cognitive skills, and in section 5.3 we provide separate analyses for military and civilian personnel employed at the military bases.

5.1 Mean effects

We estimate equation (1) on our four main outcomes, and the estimates are presented in Figure 3.[23] The pre-reform trends look very reassuring for all outcomes, and we can not reject the null hypothesis that the difference is equal to zero for any outcome. Hence, there are no indications of selection before the bills are announced.

There is no immediate negative effect from the military base closures on labor income; in the first year following the defense bills, i.e. year 2, and there is even a positive and significant effect (at the ten percent significance level). The absence of an immediate negative effect are quite likely explained by the fact that the closures were implemented in a stepwise manner, and that the military personnel who chose to leave their positions voluntarily could get severance payments during six months, which is counted as labor income in the income registers. There is a negative trend for most of the remaining period, with the estimates being significant at (at least) the ten percent significance level for the last four years of the post-reform period. The point estimates for the last years show that the individuals in the treated group earned 10,000–15,000 SEK less per year compared to those in the control group. These effects are economically significant and correspond to about 5 % of pre-treatment income.

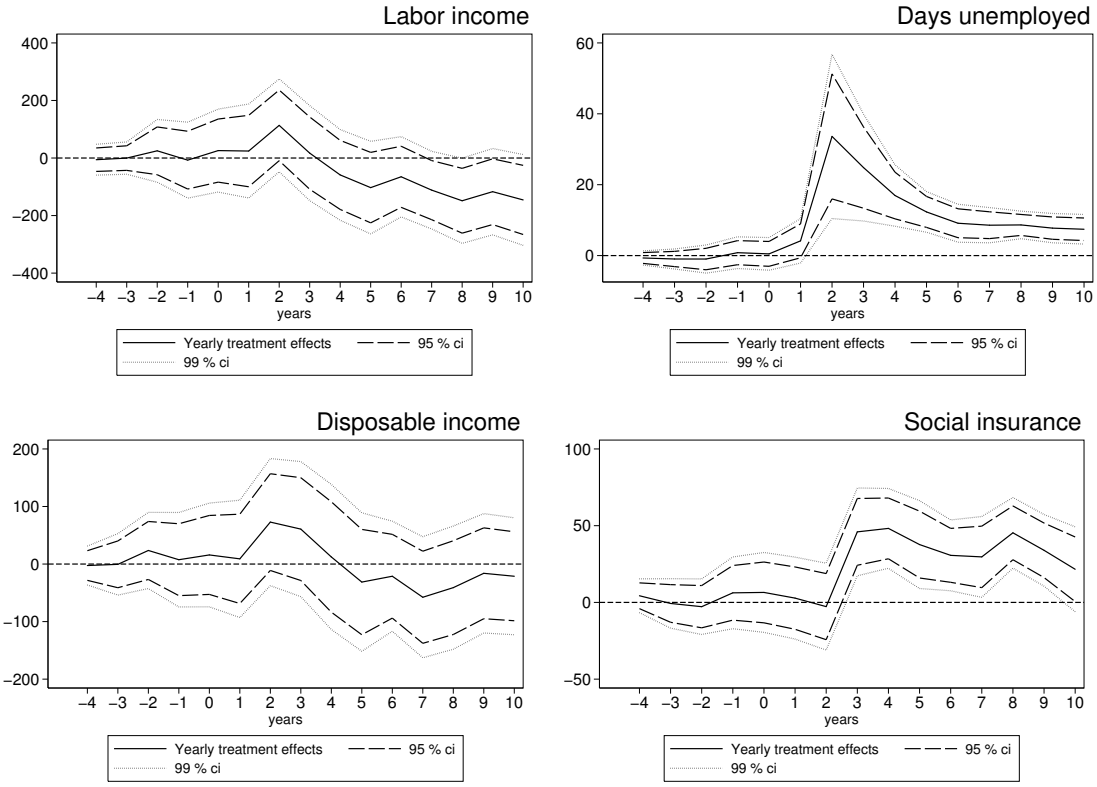
[23] The point estimates are shown in Table A.1, and we also present the results for the probability to be unemployed in the table. The results are qualitatively very similar between the two unemployment measures.

Turning to the unemployment variable, there is a pronounced and significant effect from the year following the bills (year 2) and onward. In year 2, the base closures caused the treated individuals to be unemployed for 34 days more than the control group, and in the last five years of the post-reform period the corresponding figure was 7–9 days per year. The results in Table A.1 show that the base closures caused the unemployment risk to be 15.7 percentage points higher among the treated individuals in year 2, and 3.5–5.5 percentage points higher in the last five years. The results for unemployment mirrors the results for labor income in the latter part of the period, indicating that at least part of the drop in earnings is due to increased unemployment.[24]

Given the results for labor earnings and unemployment, we would expect a drop in disposable income, but the point estimates are small and never statistically different from zero. This result is probably explained by the extended social safety net in Sweden, which dampens the negative effects on labor income and unemployment in terms of disposable income. Support for this story is also provided by the results for social insurance income (which, among other things, include unemployment and sickness benefits, and pensions from early retirement). The point estimates are significant from year 3 and onward and fairly stable over time, showing that the base closures caused the treated individuals to get around 3,000–4,000 SEK more per year in social insurance benefits. Individuals could only receive unemployment benefits after the six months of severance payments, and that might explain why we do not see an immediate increase of social insurance income.

[24]To get an impression on where the military personnel ended up, Table A.17 presents the top 15 sectors of employment in year 10 for the individuals in the treatment and control group, respectively.

Figure 3: Estimated effects of base closures: Full samples



Note: Income (2013 year value) is given in 100 SEK ≈ $ 12. Days unemployed refer to the number of days an individual is registered at the Employment Office in a given year. The government bills were announced in year 1.

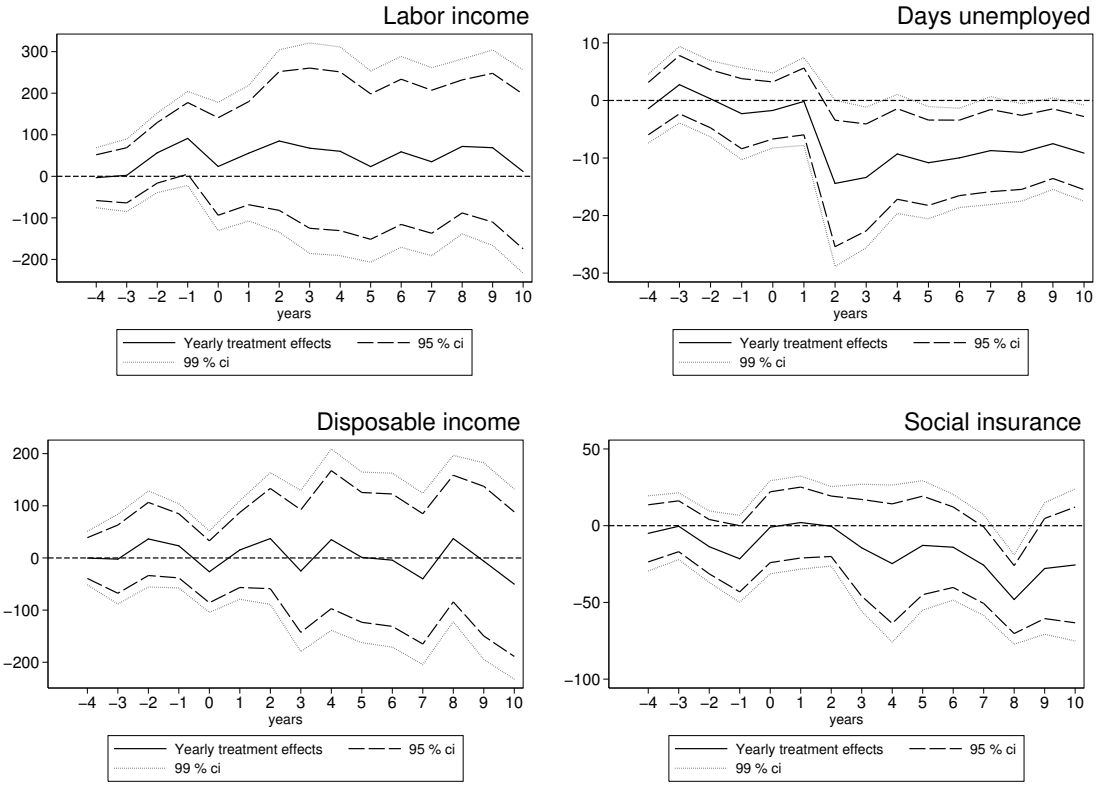
The standard errors are clustered at the municipality level in the main analysis, but given that we only have 29 municipalities a concern might be that we get biased estimates of the standard errors due to few clusters. We therefore estimated the model using wild bootstrap, and the estimates in Table A.2 show that the results are very similar. We also try excluding the municipality fixed effects in table A.3. The point estimates do not change much, but we lose some precision in the last years.

5.2 Heterogenous effects: cognitive and non-cognitive skills

The previous section showed that there were some detrimental effects for the personnel affected by the base closures. Next, we examine whether those with higher cognitive and non-cognitive skills fare better in the labor market following a negative labor market shock than those with lower skills. To investigate this, we estimate separate effects for high- and low-skilled individuals.

In Figure 4 (5) we present the treatment effect for individuals above the sample median for non-

Figure 4: Estimated effects of base closures: Non-cognitive skills



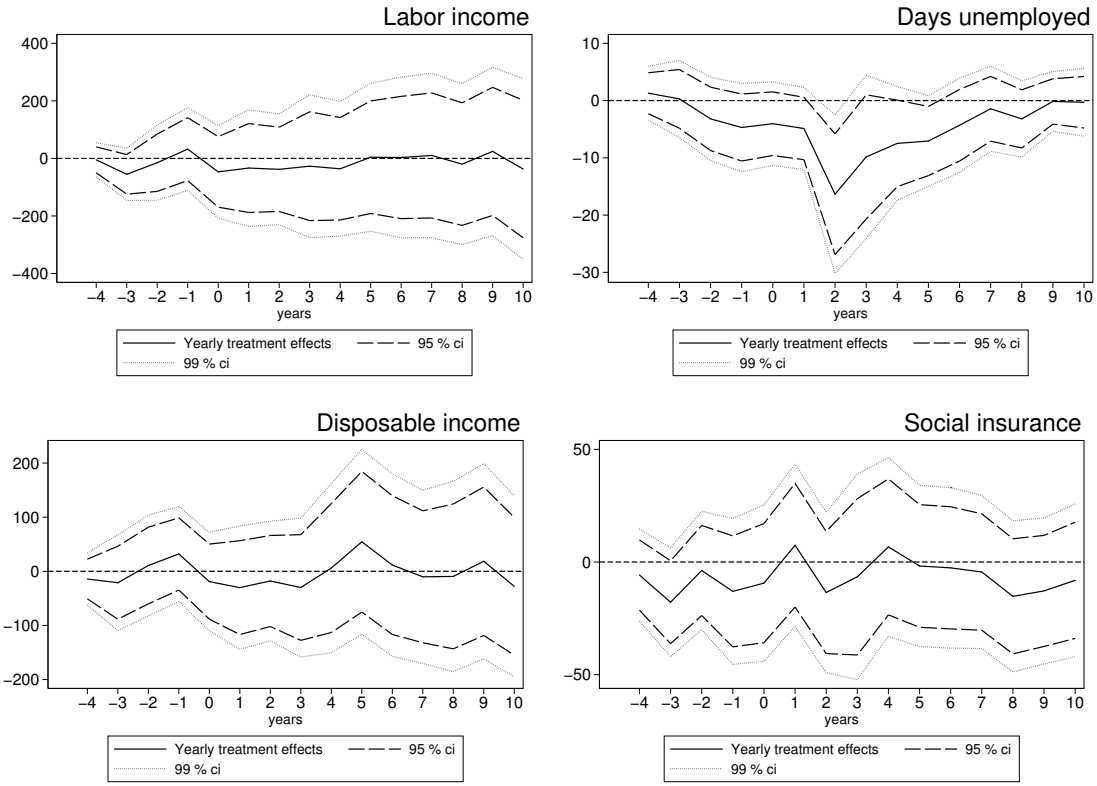
Note: Point estimates refers to effects for treated individuals with above median skills, relative to treated individuals with below median skills. Income (2013 year value) is given in 100 SEK ≈ $ 12. Days unemployed refer to the number of days an individual is registered at the Employment Office in a given year. The government bills were announced in year 1.

cognitive (cognitive) skills, compared to individuals below the median.[25] The pattern is very similar for these two skills; there are mainly no significant differences on earnings, disposable income or social insurance, but high-skilled individuals have fewer days of unemployment in a given year than the low-skilled individuals. The effect is most pronounced for those with low non-cognitive skills. In the first year after the bills were announced (year 2), the treated individuals with high non-cognitive skills were unemployed 15 days less than the treated individuals with low non-cognitive skills. In the last year of the follow-up period, the corresponding figure is still a substantial 9.4 days. This indicates that the average long-run effects of base closures are mainly driven by individuals with low non-cognitive skills. The results for those with high cognitive skills are similar to the effects for those with high non-cognitive skills in the short run, but are not equally important in the long run.

[25]The results are also presented in Table A.4 (A.7) in the Appendix. We also show the results when using a linear interaction term (Table A.5 and A.8) and a percentile ranked interaction term (Table A.6 and A.9).

It is hard to identify the exact mechanism behind these results. One plausible explanation is that non-cognitive skills that yield a high measure in the psychological evaluation (such as willingness to assume responsibility, initiative-taking ability, good social skills, ability to work in groups) also affect how active individuals are in applying for new jobs in general, and in applying for jobs that require them to adapt and learn new skills in particular. Hence, it can influence their search behavior. Another possibility is that there is a higher demand for individuals with high non-cognitive skills in the labor market. Since individual's test scores could affect what positions they were offered when they enlisted, employees with high cognitive skilled were also likely to hold different positions than low skilled employees.

Figure 5: Estimated effects of base closures: Cognitive skills



Note: Point estimates refers to effects for treated individuals with above median skills, relative to treated individuals with below median skills. Income (2013 year value) is given in 100 SEK ≈ $ 12. Days unemployed refer to the number of days an individual is registered at the Employment Office in a given year. The government bills were announced in year 1.

5.3 Separating military and civil personnel

Since employees with a military or civil contract were given different options when the military bases closed down, it is of interest to examine if the effects are different for these two groups.

Starting by looking at mean effects, we estimate equation (1) separately for military and civil employees. The results in Tables A.10 and A.11 show that the results are fairly similar for military and civil personnel. One striking differences is the fact that civilians have no significant effects on labor income, while there are significant and negative effects for military personnel. At the same time, civilians have more days of unemployment (about 50 days in year 2), than the military personnel (around 25 days in year 2). These results suggests that the civil personnel that find new employment get better paid jobs than earlier.

Turning to heterogeneous treatment effects, it is clear from Tables A.12–A.15, that the results are fairly similar (but less precise) for both groups compared to the heterogeneous treatment effects found for the full sample in section 5.2. We see significant effects for both groups of personnel for unemployment (mainly days of unemployment) and these are most pronounced for those with high non-cognitive skills. The similar pattern for individuals with a civil and military contract indicate that the results are not driven by the fact that all employees with a military contract had the option of transferring to another SAF base, whereas those with a civil contract had to apply for a new job.

One explanation to why high skilled individuals had an quicker recover, could have been that they were more mobile than low skilled individuals. By being prepared to move to find a job, high skilled individuals might have avoided periods of unemployment. In table A.16 we examine this hypothesis, by looking at the probability of living in another municipality 9 years after the bills (year 10). High skilled individuals and those who were employed at closing bases were more likely to move in general. In fact, the probability of living in another municipality was around 12 percentage points higher for the treated employees. Yet, there is no significant effect of the interaction term between skills and treatment status.

Another possibility is that high skilled individuals were more likely to stay in the military sector over the years, and thereby avoided periods of unemployment. To examine this, we also estimated the probability of being employed by SAF in year 10. The results, given in Table 3, show that the treated military and civilian personnel have left the military to a larger extent (42–46 percentage points) than the untreated personnel.[26] Among the control group, individuals with higher non-cognitive skills and civilians with higher cognitive skills were more likely to leave SAF. Among the treated individuals, on the other hand, those with higher skills have to a larger extent stayed

[26]Overall, 71 percent in the control group and 35 percent in the treatment group were employed by the Swedish Armed Forces in 2010 (see Table A.17).

Table 3: Probability of staying within SAF

	Military			Civil		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.418***	-0.426***	-0.462***	-0.435***	-0.447***	-0.461***
	(0.038)	(0.040)	(0.048)	(0.047)	(0.044)	(0.046)
Cognitive skills	0.001		0.003	-0.076***		-0.053***
	(0.011)		(0.010)	(0.013)		(0.013)
Cognitive*Treated	0.063**		0.058**	0.120***		0.084***
	(0.027)		(0.026)	(0.026)		(0.026)
Non-cognitive skills		-0.029***	-0.029***		-0.078***	-0.055***
		(0.010)	(0.010)		(0.014)	(0.016)
Noncognitive*Treated		0.055**	0.048**		0.121***	0.085***
		(0.021)	(0.018)		(0.022)	(0.022)
Observations	7421	7402	7402	2976	2955	2955

Note: Probability of having SAF as the main employer in year 10. Standard errors clustered at the municipality. Cognitive and non-cognitive skills are standardized by enlistment year for the full population.

within the military. The estimates in columns (3) and (6) show that these results hold also when we control for all skill-variables at the same time, but note that cognitive and non-cognitive skills are relatively highly correlated (see Graph A.2). Whether those with high cognitive and non-cognitive skills wanted to stay in the military to a larger extent, vacancies at other bases better matched their skills, or they were more flexible and willing to adapt to new positions is hard to say, but this result can explain some of the positive effects found on unemployment for those with high skills.

6 Conclusions

It is well-established that the risk of losing a job is unevenly distributed in the population, as low skilled individuals are over-represented among the unemployed (e.g. Lindqvist and Vestman, 2011). However, there is scarce evidence on the role played by cognitive and non-cognitive skills in determining how quick individuals recover from a job loss. To the extent that low-skilled individuals also experience longer unemployment spells, the societal costs of job reallocations may be substantial. We examine whether the effects from a negative labor market shock affected individuals with different cognitive and non-cognitive skills differently. Since selection into unemployment is a function of skills, we solve the endogeneity problem by using the exogenous labor market shock provided by the substantial military base closures in Sweden following the end of the Cold War. To measure cognitive and non-cognitive skills, we rely on the test results from the Swedish military draft.

We find that the affected employees' labor earnings decrease, while the unemployment rate increase. We do not, however, find any significant effects on disposable income, which is probably explained by the extended social safety net in Sweden. The positive effects on social insurance benefits support such a story. When examining the heterogeneous treatment effects with respect to skills, we find no significant differences for earnings, disposable income or social insurance, but that high-skilled individuals have fewer days of unemployment following the base closures than the low-skilled individuals. This effect is most pronounced for those with high non-cognitive skills. In fact, our results suggest that the persistent effect of job displacement is almost entirely driven by individuals with low non-cognitive skills. As pointed out by Lindqvist and Vestman (2011), it is plausible that the non-cognitive skills that yield a high measure in the psychological evaluations (e.g., willingness to assume responsibility, good social skills, and ability to work in groups) are highly valued in the labor market and help ease the job reallocation process. Both in terms of affecting individuals search behavior, but also given the fact that these skills are likely to be favored by new employers.

The earlier literature on military base closures, which have focused on local aggregate effects, have found small or insignificant effects on outcomes such as local growth and unemployment (see e.g., Paloyo et al., 2010). Our results show that the individual labor market consequences for some of the affected personnel have been more detrimental than indicated by the aggregate estimates.

References

Abowd, J. M., McKinney, K. L., and Vilhuber, L. (2009). The link between human capital, mass layoffs, and firm deaths. In *Producer Dynamics: New Evidence from Micro data*, pages 447–472. University of Chicago Press.

Andersson, L., Lundberg, J., and Sjostrom, M. (2007). Regional effects of military base closures: the case of sweden. *Defence and Peace Economics*, 18(1):97–97.

Arulampalam, W., Gregg, P., and Gregory, M. (2001). Unemployment scarring. *Economic Journal*, 111(475):577–84.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2015). Losing heart? the effect of job displacement on health. *ILR Review*, 68(4):833–861.

Blomsterberg, M. and Kadefors, R. (2009). Utvärdering av karriärväxlingsprojektet slutrapport. *Department of Work Science, Gothenburg University.*

Böheim, R. and Taylor, M. (2002). The search for success: Do the unemployed find stable employment? *Labour Economics*, 19(6):717–35.

Bowles, S., Gintis, H., and Osborne, M. (2001). The determinants of earnings: A behavioral approach. *Journal of Economic Literature*, 39(4):1137–1176.

Brand, J. E. (2015). The far-reaching impact of job loss and unemployment. *Annual Review of Sociology*, 41:359–375.

Carrington, W. (1993). Wage losses for displaced workers: Is it really the firm that matters? *Journal of Human Resources*, 28(3):435–62.

Couch, K. and Placzek, D. (2010). Earnings losses of displaced workers revisied. *American Economic Review*, 100(1):572–589.

Dahlberg, M., Martén, L., and Persson, A. (2013). After the cold war: The effect of military base closures on individual labor market outcomes. Published in Persson, Anna, *Activation Programs, Benefit Take-up, and Labor Market Attachment*, Economic Studies 138, PhD Thesis, Uppsala University.

DN (Nov 1, 1999) (1999). Nedläggningsbeslut står fast. Dagens Nyheter.

Eliason, M. (2011). Income after job loss: The role of the family and the welfare state. *Applied Economics*, 43:603–618.

Eliason, M. and Storrie, D. (2006). Lasting or latent scars? swedish evidence on the long-term effects of job displacement. *Journal of Labor Economics*, 24(4):831–856.

Eriksson, M. and Hallsten, L. (2003). Tre år efter avvecklingen: arbete, hälsa och hälsoutveckling för uppsagd civilanställd personal inom försvaret. *Arbete och Hälsa, Arbetslivsinstitutet*, 2003(09).

Falkenhall, B. (2004). *Omställningsarbete efter försvarsnedläggningar*. Institutet för tillväxtpolitiska studier.

Farber, H. (2003). Job loss in the united states, 1981-2001. Working Paper 9707, National Bureau of Economic Research.

Hallberg, D., Johansson, P., and Josephson, M. (2015). Is an early retirement offer good for your health? quasi-experimental evidence from the army. *Journal of Health Economics*, 44:274 – 285.

Hamermesh, D. (1987). The cost of worker displacement. *Quarterly Journal of Economics*, 102(1):51–75.

Heckman, J. J. (2008). The case for investing in disadvantaged young children. Published in *Big ideas for children: Investing in our nation's future* (pp. 49-58). Washington, DC: First Focus.

Heckman, J. J., Stixrud, J., and Urzua, S. (2006). The Effects of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior. *Journal of Labor Economics*, 24(3):411–482.

Hedin, O. (2011). Försvarets förutsättningar - en ESO-rapport om erfarenheter från 20 år av försvarsreformer. *Expertgruppen för Studier i Offentlig Ekonomi*, 2011(2).

Herrnstein, R. J. and Murray, C. A. (1994). *The bell curve: Intelligence and class structure in American life*. Free Press, New York.

Hijzen, A., Upward, R., and Wright, P. (2010). The income losses of displaced workers. *Journal of Human Resources*, 45(1):243–269.

Hines, J., Hoynes, H., and Krueger, A. (2001). Another look at whether a rising tide lifts all boats. In *The Roaring Nineties: Can Full Employment Be Sustained?*, page . Russell Sage Foundation, New York.

Hooker, M. and Knetter, M. (2001). Measuring the economic effects of military base closures. *Economic Inquiry*, 39(4):583–598.

Huttunen, K., Møen, J., and Salvanes, K. G. (2011). How destructive is creative destruction? effects of job loss on job mobility, withdrawal and income. *Journal of the European Economic Association*, 9(5):840–870.

Jacobsen, L., LaLonde, R., and Sullivan, D. (1993). Earning losses of displaced workers. *American Economic Review*, 83(4):685–709.

Jakobsson, M. (2010). Militär avveckling. problem eller möjlighet? *licentiate thesis, Department of Social and Economic Geography, Uppsala University.*

Lindqvist, E. and Vestman, R. (2011). The labor market returns to cognitive and noncognitive ability: Evidence from the swedish enlistment. *American Economic Journal: Applied Economics*, 3(1):101–28.

Mood, C., Jonsson, J. O., and Bihagen, E. (2012). Socioeconomic persistence across generations: The role of cognitive and non-cognitive processes. In Ermisch, J., Jäntti, M., and Smeeding, T. M., editors, *From Parents to Children: The Intergenerational Transmission of Advantage*, pages 53–83. New York: Russell Sage Foundation.

Neal, D. (1998). The link between ability and specialization: An explanation for observed correlations between wages and mobility rates. *The Journal of Human Resources*, 33(1):173–200.

Öckert, B. (2011). Comment on christopher pissarides: Regular education as a tool of countercyclical employment policy. *Nordic Economic Policy Review*, pages 233–239.

Paloyo, A., Vance, C., and Vorell, M. (2010). The regional economic effects of military base realignments and closures in germany. *Defence and Peace Economics*, 21(5-6):567–579.

Prop. (1991). *Totalförsvarets utveckling till och med budgetåret 1996/97 samt anslag för budgetåret 1992/93.* Number 1991/92:102. Försvarsdepartementet, Stockholm.

Prop. (1995). *Totalförsvar i förnyelse.* Number 1995/96:12. Försvarsdepartementet, Stockholm.

Prop. (1999). *Det nya försvaret.* Number 1999/00:30. Försvarsdepartementet, Stockholm.

Prop. (2004). *Vårt framtida försvar.* Number 2004/05:5. Försvarsdepartementet, Stockholm.

Prop. (2008). *Ett användbart försvar.* Number 2008/09:140. Försvarsdepartementet, Stockholm.

Schmieder, J., von Wachter, T., and Bender, S. (2010). The long-term impact of job displacement in Germany during the 1982 recession on earnings, income and employment. *IAB Discussion Paper 2010:1*.

Schwerdt, G. (2011). Labor turnover before plant closure: Leaving the sinking ship vs. captain throwing ballast overboard. *Labour Economics*, 18:93–101.

Seim, D. (2013). Job displacement and labor market outcomes by skill level. Published in Seim, David, *Essays on Public, Political and Labor Economics*, PhD Thesis, Stockholm University.

Stern, J. (1972). Consequences of plant closure. *Journal of Human Resources*, 7(1):3–25.

Sullivan, D. and von Wachter, T. (2009). Job displacement and mortality: An analysis using administrative data. *The Quarterly Journal of Economics*, 124(3):1265–1306.

SvD (Nov 11, 1999) (1999). Var tredje militär ska bort. Svenska Dagbladet.

TT (June 27, 1999) (1999). Dalkarlar på marsch mot Stockholm. Tidningarnas Telegrambyrå.

TT (Sept 23, 1999) (1999). Försvarsnedläggningarna spär på utflyttningen. Tidningarnas Telegrambyrå.

TT (Sept 30, 1999) (1999). 5000 demonstrerade mot nedläggning av regemente i Kiruna. Tidningarnas Telegrambyrå.

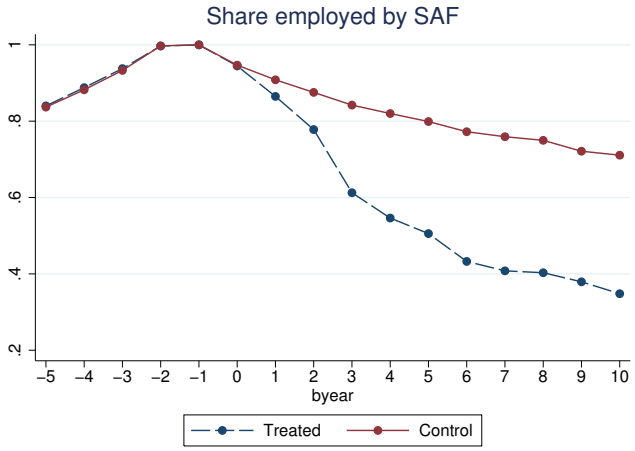
von Wachter, T. (2010). Summary of the literature on job displacement in the US and EU: What we know and what we would like to know. In *Wage Structures, Employment Adjustments and Globalization: Evidence from Linked and Firm-level Panel Data*, page . Applied Econometrics Association Series, Palgrave Macmillan.

Warf, B. (1997). The geopolitics/geoeconomics of military base closures in the USA. *Political Geography*, 16(7):541–563.

A Appendix

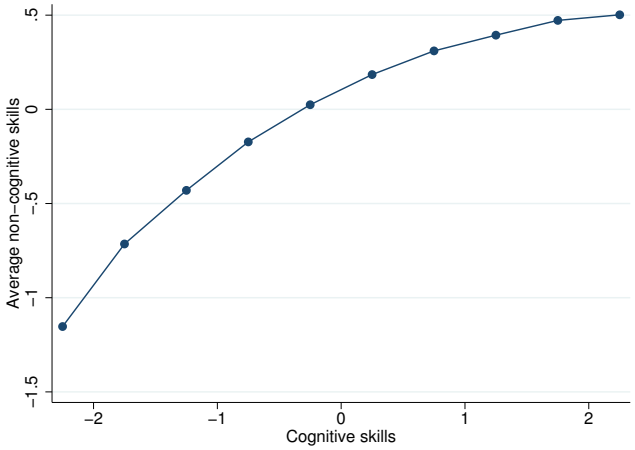
A.1 Figures

Figure A.1: Employed by the SAF



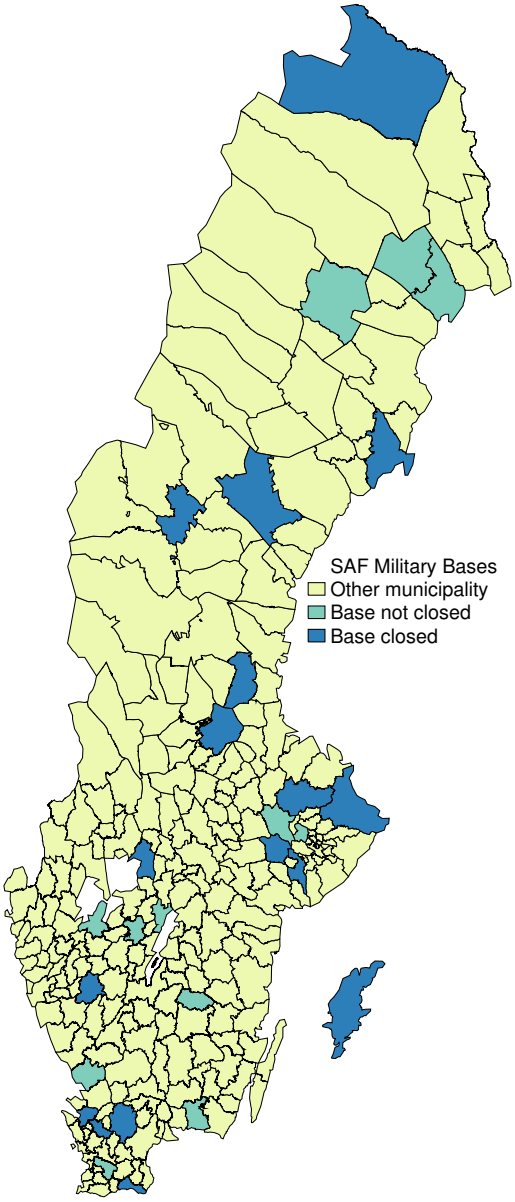
Note: Yearly share employed by the Swedish Armed Forces (SAF).

Figure A.2: Relationship between cognitive and non-cognitive skills.



Note: Average non-cognitive skill by cognitive skill for the full population. All variables are standardized by enlistment year, with mean 0 and sd 1.

Figure A.3: Municipalities in the treatment and control groups

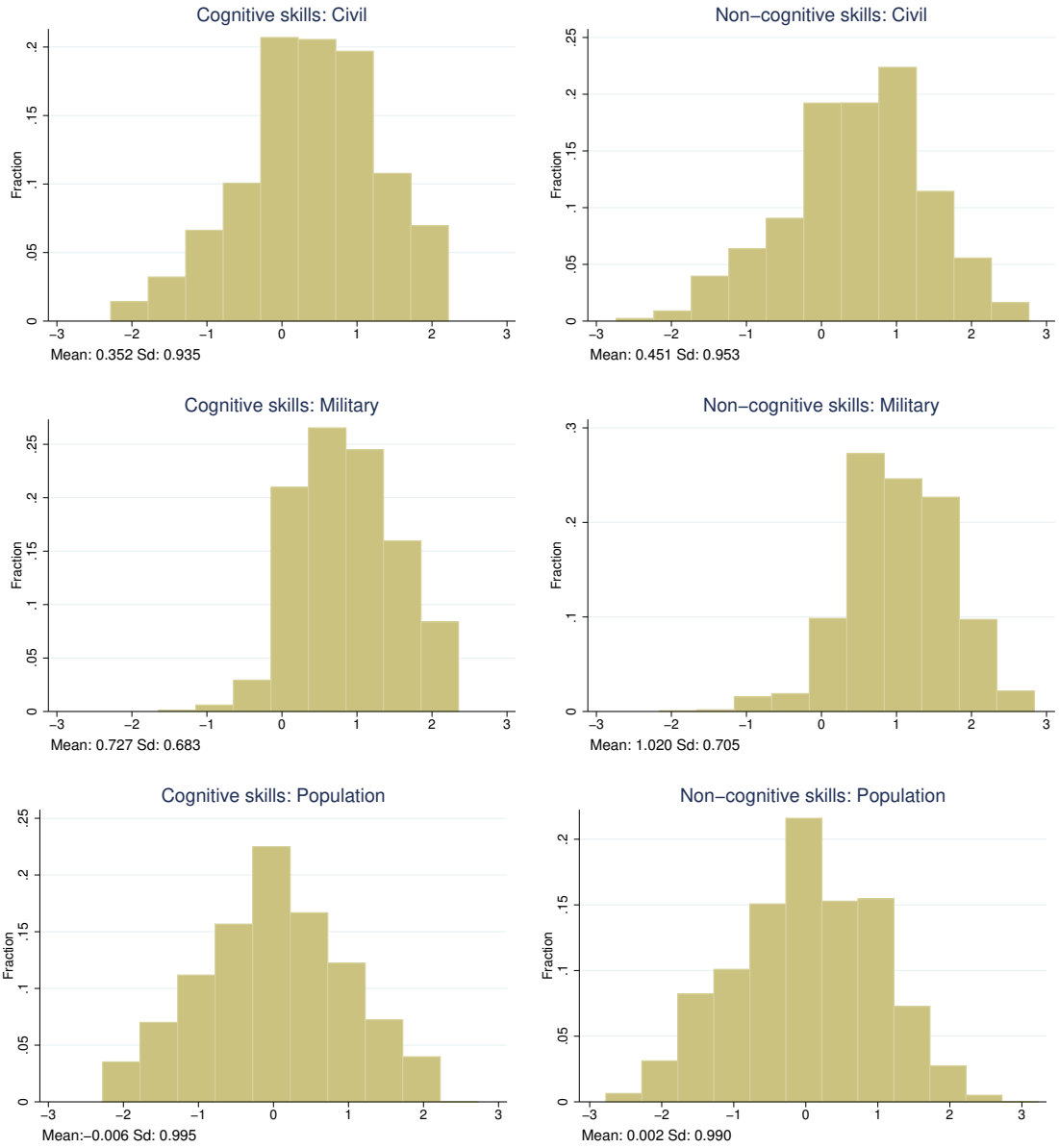


Treated:
Ystad
Södertälje
Umeå
Borås
Klippan
Söderhamn
Uppsala*
Ängelholm*
Hässleholm*
Falun*
Sollefteå*
Norrtälje*
Kiruna*
Östersund**
Kristinehamn**
Gotland**
Strängnäs**

Control:
Eksjö
Upplands-Bro
Boden
Halmstad
Skövde
Lund
Enköping
Karlsborg
Arvidsjaur
Luleå
Ronneby
Lidköping

Note: Only displays bases with army or air force. The treated units are those that closed following the defense bills in 1996, 1999 and 2004. */** Units at the military base in the municipality were closed due to the defense bill in 1999/2004

Figure A.4: Distribution of skills



Note: The skills measures are standardized by enlistment year (full population), with mean 0 and standard deviation 1. The population distribution is based on all men age 25-50 in 2013. The military employees (with a military or civil contract) are the individuals included in our analysis.

A.2 Tables

Table A.1: All

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4	-5.815	0.002	-0.674	4.345	-2.565
	(20.797)	(0.004)	(0.780)	(4.279)	(13.139)
-3	-0.243	-0.006	-0.959	-0.673	-0.503
	(21.782)	(0.006)	(1.097)	(6.261)	(20.729)
-2	25.056	-0.002	-0.973	-2.786	23.607
	(42.307)	(0.008)	(1.542)	(7.025)	(25.702)
-1	-7.333	0.002	0.822	6.248	7.545
	(51.233)	(0.008)	(1.734)	(9.080)	(31.859)
0	25.872	0.003	0.483	6.516	15.842
	(55.938)	(0.010)	(1.787)	(10.107)	(34.994)
1	24.287	0.054*	4.156*	2.811	9.053
	(63.338)	(0.027)	(2.436)	(10.362)	(39.584)
2	113.500*	0.157***	33.628***	-2.732	72.909
	(62.638)	(0.035)	(8.997)	(10.989)	(42.874)
3	17.125	0.122***	24.829***	45.972***	60.721
	(64.066)	(0.022)	(5.849)	(11.101)	(45.563)
4	-58.690	0.095***	16.951***	48.233***	12.346
	(61.196)	(0.015)	(3.357)	(10.100)	(48.931)
5	-102.841	0.071***	12.317***	37.693***	-31.312
	(62.472)	(0.011)	(2.226)	(11.112)	(46.702)
6	-65.183	0.055***	9.125***	30.671***	-21.157
	(54.167)	(0.009)	(2.080)	(8.960)	(37.135)
7	-111.554**	0.048***	8.576***	29.662***	-57.685
	(52.397)	(0.009)	(1.928)	(10.221)	(40.832)
8	-148.505**	0.042***	8.651***	45.349***	-40.983
	(57.459)	(0.009)	(1.508)	(8.938)	(41.559)
9	-116.973*	0.037***	7.747***	33.957***	-16.062
	(58.277)	(0.008)	(1.607)	(9.064)	(40.267)
10	-145.622**	0.035***	7.430***	21.598*	-21.198
	(61.289)	(0.007)	(1.621)	(10.737)	(39.444)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract.

Table A.2: Estimated effects of base closures: All (wild bootstrap)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4	-5.815	0.002	-0.674	4.345	-2.565
	(0.804)	(0.720)	(0.396)	(0.354)	(0.826)
-3	-0.243	-0.006	-0.959	-0.673	-0.503
	(1.000)	(0.374)	(0.378)	(0.898)	(1.000)
-2	25.056	-0.002	-0.973	-2.786	23.607
	(0.538)	(0.842)	(0.580)	(0.730)	(0.398)
-1	-7.333	0.002	0.822	6.248	7.545
	(0.910)	(0.818)	(0.620)	(0.538)	(0.830)
0	25.872	0.003	0.483	6.516	15.842
	(0.700)	(0.750)	(0.794)	(0.556)	(0.696)
1	24.287	0.054*	4.156	2.811	9.053
	(0.734)	(0.084)	(0.140)	(0.798)	(0.842)
2	113.500	0.157***	33.628***	-2.732	72.909
	(0.134)	(0.000)	(0.000)	(0.792)	(0.152)
3	17.125	0.122***	24.829***	45.972***	60.721
	(0.774)	(0.000)	(0.000)	(0.002)	(0.280)
4	-58.690	0.095***	16.951***	48.233***	12.346
	(0.348)	(0.000)	(0.002)	(0.000)	(0.822)
5	-102.841	0.071***	12.317***	37.693***	-31.312
	(0.122)	(0.000)	(0.000)	(0.002)	(0.524)
6	-65.183	0.055***	9.125***	30.671***	-21.157
	(0.246)	(0.000)	(0.004)	(0.000)	(0.604)
7	-111.554*	0.048***	8.576***	29.662***	-57.685
	(0.050)	(0.000)	(0.000)	(0.008)	(0.198)
8	-148.505**	0.042***	8.651***	45.349***	-40.983
	(0.032)	(0.000)	(0.000)	(0.000)	(0.370)
9	-116.973*	0.037***	7.747***	33.957***	-16.062
	(0.070)	(0.000)	(0.000)	(0.000)	(0.706)
10	-145.622**	0.035***	7.430***	21.598*	-21.198
	(0.030)	(0.000)	(0.000)	(0.076)	(0.650)
Observations	168001	168001	168001	168001	168001

Note: Bootstrapped p-value in parenthesis (1000 repetitions). Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract.

Table A.3: Estimated effects of base closures: All (no municipality FE)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4	9.902	-0.005	-1.989	4.647	5.709
	(32.873)	(0.013)	(2.344)	(6.492)	(44.379)
-3	15.381	-0.013	-2.273	-0.366	7.740
	(30.511)	(0.015)	(2.638)	(8.755)	(42.760)
-2	40.701	-0.009	-2.289	-2.490	31.891
	(47.940)	(0.017)	(3.012)	(9.860)	(33.466)
-1	8.309	-0.005	-0.494	6.542	15.827
	(55.946)	(0.015)	(2.868)	(11.909)	(36.568)
0	41.526	-0.004	-0.834	6.807	24.122
	(59.944)	(0.017)	(2.994)	(13.526)	(37.125)
1	39.929	0.047	2.840	3.105	17.334
	(66.645)	(0.035)	(3.957)	(13.547)	(41.395)
2	129.198*	0.150***	32.314***	-2.452	81.177*
	(64.013)	(0.041)	(9.923)	(14.614)	(42.487)
3	32.570	0.115***	23.522***	46.257***	69.055
	(65.247)	(0.026)	(6.336)	(13.802)	(45.476)
4	-43.549	0.088***	15.646***	48.518***	20.517
	(69.606)	(0.021)	(4.377)	(11.466)	(65.457)
5	-87.911	0.064***	11.011***	37.978***	-23.215
	(70.020)	(0.019)	(3.432)	(11.487)	(60.535)
6	-49.996	0.048***	7.822**	30.961***	-13.067
	(60.938)	(0.017)	(2.959)	(9.818)	(46.177)
7	-96.218	0.041**	7.257**	29.928**	-49.474
	(57.174)	(0.016)	(2.932)	(11.905)	(55.431)
8	-132.995**	0.035*	7.325**	45.586***	-32.666
	(60.339)	(0.018)	(2.924)	(10.860)	(56.497)
9	-101.436	0.030*	6.418*	34.238***	-7.881
	(63.422)	(0.017)	(3.158)	(11.765)	(59.873)
10	-130.090*	0.028	6.105*	21.894	-13.063
	(68.011)	(0.018)	(3.436)	(13.775)	(50.368)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract.

Table A.4: Estimated effects of base closures: Estimated effects of base closures: non-cognitive (median)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Non-cog	-3.238	0.012	-1.417	-4.997	-0.192
	(28.089)	(0.009)	(2.323)	(9.495)	(19.943)
-3*Non-cog	2.411	0.021**	2.751	-0.364	-2.293
	(33.890)	(0.010)	(2.584)	(8.452)	(33.381)
-2*Non-cog	56.480	0.008	0.282	-13.670	36.430
	(37.192)	(0.010)	(2.569)	(9.036)	(35.766)
-1*Non-cog	91.307**	0.006	-2.297	-21.571*	23.277
	(43.960)	(0.013)	(3.107)	(11.000)	(31.372)
0*Non-cog	23.656	0.003	-1.736	-0.989	-26.487
	(59.889)	(0.010)	(2.531)	(11.774)	(30.292)
1*Non-cog	55.663	0.005	-0.167	2.049	15.345
	(63.292)	(0.015)	(2.964)	(11.788)	(36.729)
2*Non-cog	85.049	-0.033	-14.500**	-0.375	37.179
	(85.195)	(0.021)	(5.642)	(10.042)	(49.032)
3*Non-cog	67.680	-0.038	-13.379***	-14.430	-25.129
	(98.349)	(0.024)	(4.741)	(16.084)	(59.970)
4*Non-cog	60.272	-0.042**	-9.410**	-24.729	35.115
	(97.469)	(0.020)	(4.035)	(19.870)	(67.503)
5*Non-cog	23.360	-0.028	-10.851***	-12.838	1.042
	(89.286)	(0.018)	(3.789)	(16.370)	(63.502)
6*Non-cog	58.964	-0.027*	-9.978***	-14.032	-4.249
	(89.161)	(0.015)	(3.348)	(13.387)	(64.769)
7*Non-cog	35.028	-0.022	-8.715**	-25.728*	-40.080
	(87.848)	(0.013)	(3.647)	(12.726)	(63.731)
8*Non-cog	71.943	-0.024*	-9.020**	-48.130***	37.128
	(81.687)	(0.013)	(3.282)	(11.297)	(61.952)
9*Non-cog	68.865	-0.025*	-7.511**	-27.905	-6.146
	(91.308)	(0.013)	(3.089)	(16.644)	(73.198)
10*Non-cog	11.488	-0.035**	-9.149***	-25.584	-50.268
	(94.910)	(0.014)	(3.252)	(19.237)	(70.707)
Observations	167366	167366	167366	167366	167366

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract. Point estimates refers to effects for treated individuals with above median skills, relative to treated individuals with below median skills.

Table A.5: Estimated effects of base closures: non-cognitive (linear)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Non-cog	-11.605	-0.002	-2.977*	4.148	-1.022
	(17.301)	(0.006)	(1.682)	(7.874)	(14.251)
-3*Non-cog	-2.655	0.008	-0.251	5.207	-7.224
	(18.753)	(0.008)	(1.951)	(6.560)	(18.247)
-2*Non-cog	1.147	0.000	-1.868	-0.882	15.731
	(24.486)	(0.009)	(2.378)	(7.468)	(21.505)
-1*Non-cog	18.845	-0.001	-3.273	-10.361	12.697
	(30.005)	(0.010)	(2.615)	(8.478)	(20.981)
0*Non-cog	3.756	-0.004	-2.724	-0.873	-7.962
	(36.291)	(0.008)	(2.100)	(8.754)	(18.019)
1*Non-cog	6.655	-0.008	-2.000	6.649	5.439
	(40.474)	(0.011)	(2.148)	(9.105)	(21.462)
2*Non-cog	7.152	-0.038**	-13.302***	4.826	17.954
	(51.179)	(0.017)	(4.570)	(8.974)	(27.773)
3*Non-cog	4.209	-0.030*	-10.245***	-8.218	-17.680
	(56.768)	(0.016)	(3.627)	(12.150)	(29.776)
4*Non-cog	-18.572	-0.030*	-6.993**	-14.478	-10.974
	(52.021)	(0.015)	(2.879)	(13.693)	(34.406)
5*Non-cog	-45.230	-0.021	-6.826**	0.118	-8.470
	(48.410)	(0.015)	(2.981)	(11.068)	(39.062)
6*Non-cog	-23.236	-0.019	-6.204**	-5.706	-14.918
	(47.563)	(0.013)	(3.027)	(12.508)	(31.358)
7*Non-cog	-28.711	-0.013	-6.129**	-17.686*	-29.186
	(52.427)	(0.011)	(2.583)	(8.802)	(33.617)
8*Non-cog	-6.864	-0.013	-5.567**	-23.874***	21.612
	(45.448)	(0.010)	(2.341)	(8.468)	(40.875)
9*Non-cog	7.462	-0.015	-5.401*	-18.669	12.681
	(50.030)	(0.009)	(2.716)	(11.164)	(42.274)
10*Non-cog	-36.999	-0.024**	-6.461***	-9.737	-42.219
	(59.661)	(0.009)	(2.283)	(12.682)	(44.093)
Observations	167366	167366	167366	167366	167366

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract. Point estimates refers to the interaction term between treated individuals and the skills measure.

Table A.6: Estimated effects of base closures: non-cognitive (percentile)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Non-cog	-0.476	0.000	-0.057	0.025	-0.148
	(0.540)	(0.000)	(0.041)	(0.204)	(0.411)
-3*Non-cog	-0.324	0.000	0.024	0.150	-0.399
	(0.629)	(0.000)	(0.043)	(0.185)	(0.591)
-2*Non-cog	0.089	0.000	-0.025	-0.104	0.382
	(0.759)	(0.000)	(0.054)	(0.186)	(0.761)
-1*Non-cog	0.755	0.000	-0.076	-0.377*	0.158
	(0.890)	(0.000)	(0.064)	(0.221)	(0.679)
0*Non-cog	0.111	-0.000	-0.061	-0.035	-0.529
	(1.119)	(0.000)	(0.049)	(0.237)	(0.551)
1*Non-cog	0.413	-0.000	-0.039	0.167	-0.020
	(1.262)	(0.000)	(0.049)	(0.232)	(0.667)
2*Non-cog	0.332	-0.001**	-0.358***	0.105	0.338
	(1.645)	(0.000)	(0.129)	(0.215)	(0.882)
3*Non-cog	0.378	-0.001*	-0.277***	-0.247	-0.730
	(1.847)	(0.000)	(0.091)	(0.308)	(0.961)
4*Non-cog	-0.091	-0.001**	-0.192**	-0.496	-0.342
	(1.718)	(0.000)	(0.075)	(0.340)	(1.059)
5*Non-cog	-0.772	-0.001	-0.194**	-0.110	-0.295
	(1.562)	(0.000)	(0.074)	(0.284)	(1.169)
6*Non-cog	-0.075	-0.001*	-0.183**	-0.205	-0.509
	(1.582)	(0.000)	(0.069)	(0.293)	(1.013)
7*Non-cog	-0.382	-0.000	-0.171***	-0.504**	-1.011
	(1.725)	(0.000)	(0.061)	(0.224)	(1.069)
8*Non-cog	0.244	-0.000*	-0.166***	-0.715***	0.332
	(1.504)	(0.000)	(0.054)	(0.223)	(1.241)
9*Non-cog	0.629	-0.000*	-0.140**	-0.509	0.324
	(1.695)	(0.000)	(0.061)	(0.315)	(1.401)
10*Non-cog	-0.619	-0.001***	-0.168***	-0.293	-1.249
	(1.810)	(0.000)	(0.056)	(0.353)	(1.281)
Observations	167366	167366	167366	167366	167366

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract. Point estimates refers to the interaction term between treated individuals and the percentile ranked skills measure.

Table A.7: Estimated effects of base closures: cognitive (median)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Cog	-4.998	0.016	1.288	-5.762	-14.244
	(22.970)	(0.009)	(1.826)	(7.947)	(18.672)
-3*Cog	-55.291	0.011	0.298	-17.842*	-21.073
	(35.085)	(0.012)	(2.617)	(9.380)	(34.385)
-2*Cog	-14.986	-0.004	-3.194	-3.757	10.703
	(50.794)	(0.012)	(2.822)	(10.194)	(36.137)
-1*Cog	32.381	-0.011	-4.703	-13.012	32.065
	(55.806)	(0.013)	(2.987)	(12.567)	(34.031)
0*Cog	-46.817	-0.006	-4.034	-9.361	-19.068
	(62.526)	(0.013)	(2.835)	(13.480)	(35.396)
1*Cog	-33.434	-0.030*	-4.876*	7.466	-30.139
	(78.766)	(0.017)	(2.782)	(14.000)	(44.215)
2*Cog	-37.908	-0.044*	-16.353***	-13.530	-17.901
	(74.719)	(0.022)	(5.364)	(13.823)	(42.924)
3*Cog	-26.981	-0.040	-9.845*	-6.565	-29.944
	(96.432)	(0.027)	(5.539)	(17.694)	(49.807)
4*Cog	-36.075	-0.025	-7.595*	6.715	6.009
	(90.856)	(0.020)	(3.925)	(15.391)	(60.754)
5*Cog	4.178	-0.020	-7.118**	-1.751	54.545
	(99.798)	(0.013)	(3.077)	(13.886)	(66.296)
6*Cog	3.171	-0.013	-4.318	-2.576	11.588
	(108.364)	(0.011)	(3.194)	(13.826)	(65.405)
7*Cog	10.374	-0.003	-1.431	-4.421	-10.153
	(110.848)	(0.012)	(2.888)	(13.179)	(62.168)
8*Cog	-19.784	0.003	-3.190	-15.225	-9.469
	(108.673)	(0.012)	(2.592)	(13.029)	(68.263)
9*Cog	24.473	0.004	-0.143	-12.812	18.551
	(113.517)	(0.012)	(2.021)	(12.572)	(69.960)
10*Cog	-36.771	0.012	-0.292	-8.118	-27.732
	(121.835)	(0.011)	(2.300)	(13.150)	(64.895)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract. Point estimates refers to effects for treated individuals with above median skills, relative to treated individuals with below median skills.

Table A.8: Estimated effects of base closures: cognitive (linear)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Cog	4.346	0.010	-0.694	-3.066	-6.209
	(12.474)	(0.008)	(1.730)	(8.581)	(8.833)
-3*Cog	-2.443	0.007	-0.860	-8.854	-14.835
	(20.421)	(0.009)	(2.237)	(7.973)	(15.581)
-2*Cog	20.768	-0.005	-3.444	-1.950	5.976
	(33.938)	(0.011)	(2.784)	(8.569)	(19.003)
-1*Cog	60.316	-0.010	-4.498	-11.752	34.677
	(37.088)	(0.012)	(3.013)	(10.017)	(22.842)
0*Cog	24.824	-0.005	-3.065	-2.549	5.219
	(40.838)	(0.011)	(2.540)	(11.944)	(24.396)
1*Cog	28.492	-0.020*	-4.244	7.428	1.659
	(49.449)	(0.012)	(2.698)	(12.194)	(27.972)
2*Cog	30.357	-0.040**	-13.455***	-4.636	12.908
	(44.405)	(0.015)	(3.803)	(12.910)	(27.613)
3*Cog	53.607	-0.048***	-13.025***	-21.768	0.299
	(56.848)	(0.017)	(4.199)	(15.939)	(32.569)
4*Cog	34.729	-0.042**	-9.367***	-15.098	2.480
	(51.549)	(0.015)	(2.714)	(13.552)	(35.079)
5*Cog	62.631	-0.030**	-8.691***	-11.737	44.294
	(51.364)	(0.012)	(2.562)	(10.144)	(38.024)
6*Cog	38.511	-0.020*	-5.112*	-6.845	10.711
	(60.673)	(0.010)	(2.497)	(9.673)	(38.888)
7*Cog	49.524	-0.008	-3.029	-13.350	8.694
	(61.782)	(0.010)	(2.517)	(11.346)	(37.994)
8*Cog	71.228	-0.004	-3.396	-19.439*	46.474
	(58.354)	(0.011)	(2.679)	(10.196)	(43.294)
9*Cog	71.177	-0.001	-1.494	-13.424	27.228
	(67.786)	(0.011)	(2.026)	(11.165)	(48.184)
10*Cog	34.422	0.003	-1.334	-18.735*	-7.733
	(70.330)	(0.010)	(2.129)	(10.961)	(44.574)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract. Point estimates refers to the interaction term between treated individuals and the skills measure.

Table A.9: Estimated effects of base closures: cognitive (percentile)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Cog	0.126	0.000	-0.015	-0.222	-0.162
	(0.364)	(0.000)	(0.040)	(0.171)	(0.271)
-3*Cog	-0.272	0.000	-0.014	-0.380**	-0.489
	(0.573)	(0.000)	(0.053)	(0.171)	(0.430)
-2*Cog	0.457	-0.000	-0.084	-0.169	0.275
	(0.874)	(0.000)	(0.066)	(0.174)	(0.503)
-1*Cog	1.540	-0.000	-0.117	-0.439**	0.944
	(0.946)	(0.000)	(0.072)	(0.214)	(0.589)
0*Cog	0.518	-0.000	-0.081	-0.183	0.160
	(1.070)	(0.000)	(0.063)	(0.267)	(0.635)
1*Cog	0.734	-0.001*	-0.103	0.069	-0.005
	(1.323)	(0.000)	(0.064)	(0.265)	(0.757)
2*Cog	0.620	-0.001**	-0.345***	-0.316	0.229
	(1.253)	(0.000)	(0.100)	(0.265)	(0.735)
3*Cog	1.201	-0.001**	-0.301**	-0.561	0.082
	(1.584)	(0.000)	(0.112)	(0.340)	(0.907)
4*Cog	0.858	-0.001**	-0.213***	-0.379	0.265
	(1.463)	(0.000)	(0.077)	(0.295)	(0.968)
5*Cog	1.731	-0.001**	-0.194***	-0.293	1.617
	(1.503)	(0.000)	(0.064)	(0.233)	(1.062)
6*Cog	1.091	-0.000*	-0.122**	-0.119	0.530
	(1.819)	(0.000)	(0.059)	(0.216)	(1.063)
7*Cog	1.352	-0.000	-0.061	-0.255	0.386
	(1.851)	(0.000)	(0.057)	(0.235)	(1.091)
8*Cog	1.594	-0.000	-0.070	-0.385	1.257
	(1.801)	(0.000)	(0.057)	(0.237)	(1.219)
9*Cog	1.870	-0.000	-0.031	-0.303	0.660
	(1.979)	(0.000)	(0.041)	(0.227)	(1.344)
10*Cog	0.914	0.000	-0.021	-0.475*	-0.048
	(2.068)	(0.000)	(0.047)	(0.232)	(1.204)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummies for Air Force and civil contract. Point estimates refers to the interaction term between treated individuals and the percentile ranked skills measure.

Table A.10: Estimated effects of base closures: Civil

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4	-20.196	-0.004	-0.795	10.695	-6.384
	(26.161)	(0.010)	(1.736)	(9.933)	(25.564)
-3	-6.477	-0.024	-3.657	-6.955	27.583
	(44.425)	(0.018)	(3.815)	(14.551)	(54.780)
-2	-16.640	0.003	0.474	13.148	-5.129
	(67.369)	(0.022)	(5.131)	(15.994)	(40.548)
-1	-75.771	0.014	5.807	39.248**	-44.965
	(73.354)	(0.021)	(5.252)	(19.074)	(37.876)
0	-72.487	0.020	4.520	39.445**	-34.595
	(63.717)	(0.021)	(4.533)	(15.239)	(35.433)
1	19.877	0.087**	9.829**	19.528	9.657
	(72.793)	(0.041)	(4.565)	(13.363)	(46.060)
2	90.668	0.211***	52.195***	9.580	16.857
	(81.169)	(0.050)	(14.444)	(15.672)	(48.450)
3	64.275	0.158***	34.254***	68.314***	49.526
	(89.147)	(0.031)	(7.674)	(21.420)	(53.594)
4	-50.019	0.119***	22.336***	70.841***	20.641
	(105.789)	(0.022)	(4.393)	(20.935)	(64.141)
5	-82.170	0.086***	14.901***	53.476***	-29.442
	(117.370)	(0.020)	(3.679)	(18.731)	(77.483)
6	-28.816	0.072***	12.063***	53.087***	8.123
	(111.614)	(0.018)	(4.250)	(17.032)	(63.737)
7	-101.503	0.051***	9.870***	62.580***	10.285
	(113.723)	(0.017)	(3.571)	(19.092)	(73.820)
8	-107.275	0.047***	12.343***	72.159***	38.426
	(110.476)	(0.017)	(3.372)	(17.828)	(78.633)
9	-94.214	0.052***	11.751***	55.884**	70.575
	(130.904)	(0.015)	(3.535)	(20.968)	(79.139)
10	-58.342	0.057***	13.896***	42.418*	96.332
	(135.751)	(0.016)	(3.665)	(23.262)	(77.590)
Observations	48176	48176	48176	48176	48176

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummy for Air Force.

Table A.11: Estimated effects of base closures: Military

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4	-1.459	0.004	-0.648	1.897	-1.185
	(25.940)	(0.004)	(0.656)	(4.739)	(15.576)
-3	1.099	0.001	0.007	1.757	-13.040
	(25.656)	(0.006)	(0.897)	(6.902)	(18.363)
-2	39.154	-0.004	-1.644	-8.735	32.229
	(44.562)	(0.007)	(1.045)	(5.934)	(25.335)
-1	15.725	-0.003	-1.241	-5.944	24.859
	(48.691)	(0.007)	(1.073)	(7.513)	(31.133)
0	60.274	-0.004	-1.224	-5.684	32.226
	(59.702)	(0.010)	(1.396)	(10.288)	(39.084)
1	22.401	0.041	1.810	-3.390	5.651
	(73.220)	(0.025)	(1.986)	(11.442)	(45.818)
2	118.611	0.136***	26.351***	-7.641	90.385*
	(70.331)	(0.032)	(7.307)	(12.205)	(46.724)
3	-2.843	0.107***	20.998***	36.926***	61.769
	(72.013)	(0.019)	(5.366)	(10.418)	(53.323)
4	-63.975	0.084***	14.665***	39.122***	5.931
	(63.287)	(0.016)	(3.342)	(9.694)	(54.447)
5	-112.359*	0.064***	11.104***	30.901**	-35.415
	(65.728)	(0.009)	(2.154)	(12.858)	(49.370)
6	-80.515	0.047***	7.787***	21.558*	-35.658
	(60.434)	(0.008)	(1.860)	(11.967)	(44.052)
7	-117.207**	0.046***	7.851***	16.585	-86.127**
	(54.597)	(0.008)	(1.822)	(10.981)	(39.117)
8	-165.400**	0.039***	6.993***	34.407***	-73.500
	(64.841)	(0.008)	(1.339)	(10.147)	(43.253)
9	-126.041**	0.030***	5.974***	24.879**	-50.570
	(60.574)	(0.008)	(1.548)	(9.666)	(45.564)
10	-179.903**	0.025***	4.750***	12.953	-67.806
	(67.368)	(0.007)	(1.390)	(10.776)	(47.295)
Observations	119825	119825	119825	119825	119825

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummy for Air Force.

Table A.12: Civil: non-cognitive (percentile)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Non-cog	-0.569	0.000	-0.032	0.428	-0.066
	(0.905)	(0.000)	(0.085)	(0.366)	(0.835)
-3*Non-cog	0.132	0.001	0.093	0.204	1.419
	(1.162)	(0.000)	(0.100)	(0.478)	(1.856)
-2*Non-cog	-0.420	0.000	-0.029	0.159	0.258
	(1.374)	(0.000)	(0.110)	(0.483)	(0.837)
-1*Non-cog	0.337	0.000	-0.113	-0.173	0.533
	(1.484)	(0.001)	(0.126)	(0.498)	(0.894)
0*Non-cog	0.591	-0.000	-0.078	-0.133	0.487
	(1.594)	(0.001)	(0.112)	(0.615)	(0.895)
1*Non-cog	1.298	0.001	-0.005	0.340	1.589
	(1.936)	(0.001)	(0.130)	(0.554)	(1.190)
2*Non-cog	1.761	-0.001	-0.436**	0.128	1.748
	(2.274)	(0.001)	(0.200)	(0.582)	(1.270)
3*Non-cog	2.095	-0.001	-0.313*	-0.333	0.927
	(2.615)	(0.001)	(0.169)	(0.705)	(1.533)
4*Non-cog	-0.274	-0.001*	-0.324**	-0.368	2.034
	(2.298)	(0.001)	(0.129)	(0.760)	(1.758)
5*Non-cog	-1.968	-0.001	-0.254**	0.009	0.588
	(2.736)	(0.001)	(0.113)	(0.576)	(2.418)
6*Non-cog	-1.556	-0.001	-0.256**	0.171	0.942
	(2.719)	(0.001)	(0.120)	(0.683)	(1.922)
7*Non-cog	-1.380	-0.001	-0.210*	-0.825*	1.525
	(3.109)	(0.001)	(0.120)	(0.471)	(2.380)
8*Non-cog	-1.095	-0.000	-0.229*	-0.761	4.833*
	(2.228)	(0.001)	(0.120)	(0.467)	(2.828)
9*Non-cog	0.203	-0.000	-0.155	-0.951	5.314
	(3.078)	(0.001)	(0.155)	(0.646)	(3.127)
10*Non-cog	-2.355	-0.001	-0.154	-0.204	1.119
	(3.204)	(0.001)	(0.144)	(0.607)	(3.248)
Observations	47843	47843	47843	47843	47843

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummy for Air Force.

Table A.13: Military: non-cognitive (percentile)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Non-cog	-0.666	-0.000	-0.052*	-0.189	-0.287
	(0.782)	(0.000)	(0.027)	(0.218)	(0.441)
-3*Non-cog	-0.743	-0.000	-0.001	0.002	-1.174
	(0.899)	(0.000)	(0.032)	(0.210)	(0.698)
-2*Non-cog	-0.197	0.000	0.021	-0.150	0.075
	(0.954)	(0.000)	(0.032)	(0.187)	(1.188)
-1*Non-cog	0.271	0.000	0.011	-0.268	-0.604
	(1.098)	(0.000)	(0.032)	(0.218)	(0.890)
0*Non-cog	-1.080	0.000	0.010	0.239	-1.497*
	(1.377)	(0.000)	(0.033)	(0.228)	(0.769)
1*Non-cog	-0.041	-0.000	0.027	0.193	-0.690
	(1.280)	(0.000)	(0.041)	(0.267)	(0.696)
2*Non-cog	-0.425	-0.001	-0.163*	0.161	-0.670
	(1.832)	(0.000)	(0.093)	(0.251)	(1.119)
3*Non-cog	0.106	-0.000	-0.179**	-0.100	-1.598
	(2.115)	(0.000)	(0.087)	(0.301)	(1.233)
4*Non-cog	0.149	-0.000	-0.078	-0.492*	-1.237
	(1.916)	(0.000)	(0.067)	(0.286)	(1.193)
5*Non-cog	-0.027	-0.000	-0.136*	-0.128	-0.625
	(1.606)	(0.000)	(0.071)	(0.332)	(1.294)
6*Non-cog	1.058	-0.000	-0.114*	-0.288	-0.785
	(1.706)	(0.000)	(0.067)	(0.346)	(1.233)
7*Non-cog	0.285	-0.000	-0.127**	-0.213	-1.489
	(1.840)	(0.000)	(0.056)	(0.247)	(1.155)
8*Non-cog	1.460	-0.000	-0.095*	-0.619*	-0.677
	(1.666)	(0.000)	(0.051)	(0.308)	(1.111)
9*Non-cog	1.229	-0.000	-0.081	-0.247	-0.892
	(1.881)	(0.000)	(0.060)	(0.293)	(1.239)
10*Non-cog	0.955	-0.000	-0.101	-0.255	-1.286
	(1.763)	(0.000)	(0.063)	(0.340)	(1.039)
Observations	119523	119523	119523	119523	119523

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummy for Air Force.

Table A.14: Civil: cognitive (percentile)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Cog	0.666	0.000	-0.005	-0.180	-0.173
	(0.660)	(0.000)	(0.074)	(0.298)	(0.479)
-3*Cog	1.959	0.000	-0.052	-0.784*	1.888*
	(1.178)	(0.000)	(0.083)	(0.407)	(1.052)
-2*Cog	1.246	-0.000	-0.137	-0.371	1.062
	(1.376)	(0.001)	(0.131)	(0.557)	(1.117)
-1*Cog	2.016	-0.000	-0.145	-0.964	1.471
	(1.287)	(0.001)	(0.148)	(0.605)	(1.029)
0*Cog	1.487	-0.000	-0.120	-0.374	1.648
	(1.489)	(0.001)	(0.128)	(0.614)	(1.020)
1*Cog	1.130	-0.000	-0.099	0.105	2.056
	(2.074)	(0.001)	(0.142)	(0.548)	(1.540)
2*Cog	2.537	-0.001*	-0.447**	-0.529	2.058
	(2.657)	(0.001)	(0.164)	(0.645)	(1.598)
3*Cog	2.635	-0.002*	-0.449**	-1.037	1.692
	(3.147)	(0.001)	(0.171)	(0.649)	(1.768)
4*Cog	1.051	-0.001	-0.260**	-0.568	2.226
	(3.183)	(0.001)	(0.118)	(0.486)	(2.081)
5*Cog	1.794	-0.001	-0.188*	-0.499	3.075
	(3.461)	(0.001)	(0.102)	(0.455)	(2.344)
6*Cog	0.360	-0.000	-0.068	-0.222	1.907
	(3.832)	(0.001)	(0.106)	(0.531)	(2.071)
7*Cog	-0.128	0.000	-0.080	-0.777*	1.866
	(3.609)	(0.001)	(0.112)	(0.428)	(2.265)
8*Cog	1.620	0.000	-0.123	-1.449***	2.663
	(3.640)	(0.001)	(0.126)	(0.444)	(2.035)
9*Cog	2.136	0.000	0.015	-1.136**	4.051
	(4.014)	(0.001)	(0.094)	(0.506)	(2.962)
10*Cog	0.690	0.000	0.032	-1.453***	1.838
	(4.300)	(0.001)	(0.092)	(0.337)	(2.253)
Observations	48176	48176	48176	48176	48176

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummy for Air Force.

Table A.15: Military:: cognitive (percentile)

	(1) Labor inc.	(2) Unemp.(%)	(3) Days unemp.	(4) Social ins.	(5) Disp. inc.
-4*Cog	-0.244	0.000	-0.002	-0.385**	-0.071
	(0.482)	(0.000)	(0.026)	(0.175)	(0.385)
-3*Cog	-1.473**	0.000	0.015	-0.402*	-1.451**
	(0.695)	(0.000)	(0.031)	(0.207)	(0.637)
-2*Cog	-0.255	-0.000	-0.023	-0.122	-0.210
	(1.062)	(0.000)	(0.029)	(0.195)	(0.678)
-1*Cog	0.815	-0.000	-0.048	-0.127	0.462
	(1.240)	(0.000)	(0.033)	(0.197)	(0.775)
0*Cog	-0.695	0.000	-0.012	-0.022	-0.824
	(1.326)	(0.000)	(0.036)	(0.264)	(0.820)
1*Cog	0.438	-0.000	-0.047	0.020	-0.931
	(1.672)	(0.000)	(0.048)	(0.310)	(1.056)
2*Cog	-0.479	-0.000	-0.168*	-0.308	-0.920
	(1.351)	(0.000)	(0.083)	(0.209)	(0.876)
3*Cog	0.778	-0.001	-0.156	-0.360	-0.724
	(1.717)	(0.000)	(0.093)	(0.301)	(0.885)
4*Cog	0.757	-0.001*	-0.138*	-0.308	-0.581
	(1.917)	(0.000)	(0.068)	(0.332)	(1.210)
5*Cog	1.753	-0.001*	-0.159**	-0.250	0.994
	(1.920)	(0.000)	(0.065)	(0.284)	(1.315)
6*Cog	1.624	-0.000*	-0.109*	-0.090	0.109
	(1.997)	(0.000)	(0.061)	(0.299)	(1.323)
7*Cog	2.110	-0.000	-0.022	0.029	0.138
	(1.951)	(0.000)	(0.045)	(0.274)	(1.320)
8*Cog	1.837	0.000	0.000	0.116	1.180
	(2.114)	(0.000)	(0.049)	(0.379)	(1.499)
9*Cog	1.878	-0.000	-0.004	0.069	-0.399
	(1.996)	(0.000)	(0.038)	(0.324)	(1.294)
10*Cog	1.461	0.000	0.018	-0.052	-0.252
	(1.944)	(0.000)	(0.059)	(0.327)	(1.092)
Observations	119825	119825	119825	119825	119825

Note: Standard errors clustered at the municipality (29 clusters). Control variables: dummy for Air Force.

Table A.16: Probability of moving

	Military			Civil		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.118***	0.137***	0.132***	0.114***	0.123***	0.120***
	(0.037)	(0.040)	(0.041)	(0.028)	(0.027)	(0.025)
Cognitive skills	0.059***		0.052***	0.138***		0.094***
	(0.014)		(0.014)	(0.015)		(0.018)
Cognitive*Treated	0.005		0.006	-0.002		-0.001
	(0.024)		(0.023)	(0.025)		(0.026)
Non-cognitive skills		0.061***	0.055***		0.137***	0.096***
		(0.015)	(0.015)		(0.008)	(0.014)
Noncognitive*Treated		-0.014	-0.015		-0.004	-0.002
		(0.022)	(0.022)		(0.018)	(0.018)
Observations	7421	7402	7402	2976	2955	2955

Note: Probability of living in another municipality year 10. Standard errors clustered at the municipality. Cognitive and non-cognitive skills are standardized by enlistment year for the full population.

Table A.17: Sector of employment (year 10): Top 15

	Control		Treated	
	Percent	N	Percent	N
Institutional care	0.52	39	1.05	30
Other industries	16.31	1230	34.14	978
Consultant, computer	1.37	103	2.30	66
Renting or administer real estate	0.48	36	0.94	27
Consultant, organization	0.84	63	1.36	39
Consultant, technical	1.06	80	3.14	90
Public administration	0.80	60	2.41	69
Administration, health care, education, culture etc.	0.21	16	1.15	33
SAF	71.05	5359	34.76	996
Police department	0.68	51	1.75	50
Education, elementary school	0.84	63	2.02	58
Education, high school	0.41	31	1.22	35
Education, high school, voccational training	0.40	30	1.36	39
Education, post high school/university	4.65	351	11.45	328
Education, other	0.41	31	0.94	27

Note: Based on 4-digit industry code. The category other industries include all individuals not included in any of the other listed categories.

Table A.18: Correlation matrix: cognitive and non-cognitive skills

	Cognitive	Spatial	Verbal	Technical	Inductive	Non-cognitive	Social	Intensity	Psychological	Emotional
Cognitive	1									
Spatial	0.801***	1								
Verbal	0.796***	0.480***	1							
Technical	0.820***	0.614***	0.553***	1						
Inductive	0.862***	0.600***	0.691***	0.611***	1					
Non-cognitive	0.370***	0.261***	0.316***	0.306***	0.357***	1				
Social	0.377***	0.273***	0.334***	0.302***	0.355***	0.802***	1			
Intensity	0.189***	0.121***	0.146***	0.170***	0.195***	0.798***	0.456***	1		
Psychological	0.324***	0.230***	0.284***	0.260***	0.314***	0.793***	0.613***	0.540***	1	
Emotional	0.334***	0.244***	0.283***	0.278***	0.313***	0.813***	0.621***	0.472***	0.555***	1

Note: Correlations for different skills measures based on the full population 2013. All variables are standardized by enlistment year, with mean 0 and sd 1. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*