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The Labor Market Returns to Cognitive and Noncognitive Ability: Evidence from the Swedish Enlistment

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The Labor Market Returns to Cognitive and Noncognitive Ability: Evidence from the Swedish Enlistment*

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Abstract

We use data from the Swedish military enlistment to assess the importance of cognitive and noncognitive ability for labor market outcomes. The measure of noncognitive ability is based on a personal interview conducted by a psychologist. We find strong evidence that men who fare poorly in the labor market - in the sense of long-term unemployment or low annual earnings - lack noncognitive rather than cognitive ability. However, cognitive ability is a stronger predictor of wages for skilled workers and of earnings above the median.

Keywords: Personality; noncognitive ability; cognitive ability; intelligence; human capital.
JEL codes: J21, J24, J31.

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1 Introduction

For the vast majority of people, labor market earnings is the main source of income. It is therefore of vital importance for individuals and policy makers to understand which abilities or skills determine success in the labor market. In one view, cognitive ability is the single most important determinant of labor market outcomes (e.g. Herrnstein and Murray, 1994). An alternative view holds that noncognitive abilities such as persistence, motivation, emotional stability, or social skills are equally or more important (e.g., Bowles and Gintis 1976; Jencks 1979; Bowles, Gintis and Osborne 2001a; Heckman, Stixrud and Urzua 2006).

The existing evidence is not clearly in favor of either view. Though a large literature confirms that IQ and other measures of cognitive ability are robust predictors of labor market outcomes, they can only explain a small fraction of the variance in earnings.[1] On the other hand, the estimated effect of noncognitive ability on outcomes varies substantially in the literature and is often small compared to the effect of cognitive ability. However, inference about the importance of noncognitive ability is difficult due to a lack of valid measures. Most studies in psychology and economics use measures of noncognitive abilities and related personality traits based on self-reported questionnaires. Compared to IQ tests, such measures are less reliable and less precise (Borghans et al., 2008b). In addition, the valuation of cognitive and noncognitive ability is likely to differ across sectors and occupations.

In this paper, we investigate the effect of cognitive and noncognitive ability on labor market outcomes using unique data from the Swedish military enlistment. The enlistment is mandatory for all young Swedish men and spans two days with tests of health status, physical fitness and cognitive ability. In addition, each conscript is interviewed by a certified psychologist with the aim to assesses the conscript's ability to fulfill the psychological requirements of serving in the Swedish defense, ultimately in armed combat. The set of personal characteristics that give a high score include persistence, social skills and emotional stability. We argue that the psychologists' assessment offer a more precise measure of noncognitive ability than measures based on self-reported questionnaires. In particular, many personal traits which may be difficult to accurately capture in a questionnaire are

[1] See, for example, the studies by Bishop (1991); Murnane et al. (1995); Cawley et al. (1996); Neal and Johnson (1996); Altonji and Pierret (2001); Cawley et al. (2001) and Blau and Kahn (2005). Bowles, Gintis and Osborne (2001a) provide a summary and discussion of this literature.

revealed in a personal encounter. The enlistment psychologists have thus access to more extensive information about conscripts' psychological status than what can be deducted from surveys.

Using the ability measures from the military enlistment, we find that both cognitive and noncognitive skills are strong predictors of labor market earnings. However, noncognitive skills have a much stronger effect at the low end of the earnings distribution. At the tenth percentile, the effect of noncognitive skills is between 2.5 and 4 times the effect of cognitive skills depending on the exact specification. One reason for this result is that men with low noncognitive ability are significantly more likely to become unemployed than men with low cognitive ability. Moreover, conditional on becoming unemployed, men with high noncognitive ability experience shorter spells, while cognitive ability has no statistically significant effect on the duration of unemployment.

By contrast, cognitive ability is a stronger predictor of wages than noncognitive ability. In our basic specification, a one standard deviation increase in cognitive ability predicts an increase in wages by 8.9 percent, compared to 6.9 percent for noncognitive ability. However, while log wages are linear in noncognitive ability, they are strictly convex in cognitive ability with a low marginal product for low levels of ability. Relatedly, we find that noncognitive ability has a higher return than cognitive ability for unskilled workers and managers while skilled workers in non-managerial positions face a higher return to cognitive than to noncognitive ability. In sum, our results support the view that a certain level of noncognitive ability is a prerequisite for avoiding failure in the labor market whereas cognitive ability is at least as import for achieving success.

Our paper is related to the small but expanding literature on personality and socioeconomic outcomes initiated by Bowles and Gintis (1976), Edwards (1976), Andrisani and Nestel (1976) and Jencks (1979).[2] The majority of these papers use measures of personality based on self-reported questionnaires. For example, measures of self-esteem (Goldsmith, Veum and Darity 1997, Murnane et al. 2001), withdrawal and aggression (Osborne 2005), Machiavellism (Turner and Martinez 1977) and sense of personal control over outcomes in life (Andrisani and Nestel 1976, Duncan and Morgan 1981, Dunifon and Duncan 1998) have been found to predict wages or occupational status. There is also an extensive literature on the predictive power of various personality measures from the psychology literature, such as the five factor model (see Borghans et al. 2008b for a

[2] See Borghans et al. (2008b) and Bowles, Gintis and Osborne (2001a) for surveys of this literature.

survey and Mueller and Plug 2006 for a recent contribution in the economics literature).

Another strand of the literature infer noncognitive ability from observable choices or behaviors. Heckman and Rubinstein (2001) consider the Generational Educational Development (GED) program which allows high school dropouts to obtain a high school diploma. GED test takers earn lower wages than predicted by their cognitive ability, which Heckman and Rubinstein attribute to low noncognitive ability. Relatedly, Heckman et al., (2006) infer cognitive and noncognitive ability by a latent factor model estimated on NLSY data whereas Kuhn and Weinberger (2005) use participation in sports in high-school or leadership positions in clubs as indicators of leadership ability. More recently, Segal (2009) uses teacher evaluations of student classroom behavior in eighth grade as a measure of noncognitive ability. To the best of our knowledge, our paper is the first in this literature to consider a measure of noncognitive ability based on a personal interview.[3]

In line with the previous literature, we use "noncognitive ability" as a term for abilities which are distinct from the capacity to solve abstract problems and traditional measures of human capital such as training and experience. We acknowledge that this terminology is not perfect as most (or all) of the character traits considered as "noncognitive" involve some form of cognition.[4] The words "ability" and "skill" are used interchangeably throughout the paper.

The paper proceeds as follows. Our data and measures of cognitive and noncognitive ability are presented in Section 2. We discuss our estimation strategy in Section 3 and provide the results for wages, employment and earnings in Section 4. Section 5 discusses how these results relate to the previous literature. Section 6 concludes the paper. Basic facts regarding the data and construction of variables is available in Appendix A which is attached to the paper. Appendix B (sample selection), C (measurement error), D (additional results), E (occupational choice) and F (additional material regarding our skill measures) are only available online.[5]

[3] Grönqvist and Vlachos (2008) use the measures of cognitive and noncognitive ability from the Swedish enlistment in a study of teacher performance. Nordin (2008) use the cognitive ability measures in a study of how the returns to schooling interacts with cognitive ability.

[4] See, for example, Borghans et al. (2008b). Another form of criticism is offered by Bowles, Gintis and Osborne (2001b) who argue that character traits like persistance or dependability should not be viewed as skills, but are more accurately viewed as preferences which employers value in the face of incomplete labor contracts.

[5] [web adress]

2 Data

The data set used in this paper is obtained by matching a data set on socioeconomic outcomes for a representative sample of the Swedish population (LINDA) with data from the military enlistment. In addition, we have matched LINDA and the enlistment data with data from Statistics Sweden on grades and educational track in secondary school. The military service is mandatory only for men, and we exclude the small fraction of women for whom we have enlistment data.

LINDA is a panel data set that covers three percent of the Swedish population annually. The starting point for LINDA was a representative, random sample of the Swedish population in 1994 which has then been tracked back to 1968 and forward to 2007. New individuals are added to the data base each year to ensure that LINDA is also cross-sectionally representative.[6] Each wave of LINDA contains information on taxable income and social benefits (e.g., unemployment support) from the Income Registers in a given year. In addition, LINDA contains information on occupation, wages and educational attainment from separate registers held by Statistics Sweden. For each year, information on all family members of the sampled individuals are added to the data set, but we focus on the core sample of randomly selected individuals. We focus on labor market outcomes in 2006 but provide additional analysis using data from several years in Appendix B.

The first cohort for which we have enlistment data is men born in 1965 (enlisted in 1983 and 1984). In comparison to the Anglo-Saxon countries, many Swedes with higher education enter the labor market late in life. For this reason, we do not consider men born after 1974, implying that the youngest men in our data were 32 years old in 2006. We also exclude men born outside of Sweden; men with an incomplete record from the military enlistment or enlistment after 1993; self-employed (defined as an annual business income above 10 000 SEK); men who are not visible in any public records (zero earnings and no taxable transfers); men who received student support and men who worked in the agricultural sector. With these restrictions, our sample consists of 14,703 men distributed evenly over the 1965-1974 birth cohorts.[7] We provide a robustness analysis of these sample restrictions in Appendix B.[8]

[6] Edin and Fredriksson (2000) provide a detailed account of the data collection process for LINDA.

[7] Our largest cohort are men born in 1965 (1,626 observations) and our smallest cohort men born in 1974 (1,304 observations).

[8] For natural reasons, it is not possible to conduct a robustness test for the nine percent of conscripts

2.1 Socioeconomic variables in LINDA

LINDA is complete with respect to taxable income and social benefits, but the wage registers are not complete for the private sector. In total, we have data on wages in 2006 for 12,570 workers which corresponds to 85.5 % of our sample. The remaining group consists both of people with no or limited participation in the labor market (e.g., people who were unemployment or on long-term sick-leave) and men whose employers did not report wages. We use the wage data from five previous waves of LINDA (2001-2005) to impute wages for men for whom we do not observe the wage in 2006. We use the wage from the year closest to 2006 when wage data is available from several years and adjust for inflation.[9] Using wages from previous years, we are able to add information on wages for 1,401 men, bringing the total number to 14,038, or 95.5 % of our sample. This imputation technique rests on the assumption that men whose wages were not observed in 2006 experienced no change in productivity between 2006 and the year of the latest wage observation. We provide robustness checks regarding the imputation of wages and estimation techniques that adjust for selection bias in Appendix B.

We construct measures of unemployment using data on social benefits. Our first measure is a dummy variable equal to one if an individual received unemployment support sometime during 2006. As discussed further in the Results section, a potential drawback with this measure is that it does not cover men who received social welfare payments or disability insurance. We therefore construct an alternative unemployment measure which includes all major forms of income support directed to individuals who for some reason did not work. We also construct measures for the fraction of the year 2006 spent in unemployment using data on total unemployment benefits and income in previous years.

We construct five dummy variables for educational attainment from the information in LINDA: only primary school (9 years), secondary school (11-12 years), two years education beyond secondary school, university degree and a Ph.D. Further, we construct a measure

for which we lack information on cognitive and noncognitive skills. These are men who were extempted from the draft altogether, or who did not have to do the entire draft as they were extempted due to low health status. Only 15 percent of men with missing data on cognitive or noncognitive skills were eventually enlisted into the military service. As both non-enlistment and low health status are negatively correlated with socioeconomic status later in life, our final sample is thus somewhat positively selected compared to the entire population of Swedish men.

[9] The wage data for 2006 is censored at 12,000 SEK. We use the same cutoff for the imputed wages. This cutoff is not particularly restrictive. The year 2005, when the cutoff was set at 10,000 SEK, only 5 out of 12,425 men with a recorded (full-time equivalent) wage made between 10,000 and 12,000 SEK.

of potential labor market experience defined as the number of years between graduation and 2006, implying that two men with the same educational attainment and age can still have different levels of experience. We also construct three dummy variables for the three main regions in Sweden and dummy variables for the metropolitan areas of Sweden's three major cities.[10]

Direct information on family background are not available in LINDA, but we are able to derive family status, parental income and occupational choice by using information in the 1980 wave of LINDA. The details are available in Appendix A.

2.2 The enlistment data[11]

The military enlistment usually takes place the year a Swedish man turns 18 or 19.[12] The enlistment procedure spans two days involving tests of medical status, physical fitness, cognitive ability, and an interview with a psychologist. For the period we consider, almost all men who were not given a low health rating were enlisted to the military service.[13] Importantly, it was not possible to avoid the military service by obtaining a low score on cognitive or noncognitive ability, though test scores predict the precise type of service to which conscripts were enlisted.[14] In total, 90.0 % of the men in our sample were enlisted to the military service.

The majority of enlisted men start their military service upon graduation from secondary school. The mean age at the onset of the military service is 20.3 years in our sample and only 2 percent of enlisted men were more than 22 years old (see Figure B1). About 10 percent of enlisted men do not enter into the military service. Attrition from

[10]The regions are Götaland, Svealand and Norrland. The cities are Stockholm, Göteborg and Malmö.

[11]The discussion of the Swedish enlistment is based upon reports and literature from the Swedish armed forces (Försvarsmakten) and an interview with Johan Lothigius, chief psychologist at the SNSA (Pliktverket), August 25, 2004. In addition, both authors of this paper have undergone the military enlistment and between them spent more than two years in the Swedish Army.

[12]In our sample, 0.03 % did the military enlistment tests the year they turned 17, 73.68 % the year they turned 18, 24.61 % the year they turned 19, 1.30 % the year they turned 20 and 0.38 % the year they turned 21 or more.

[13]A linear regression of a dummy for "enlisted to the military service" on a set of health classification dummies has an R^2 of .73. Among the men in the highest health category (A) 96.5 % were enlisted compared to none of the men in the second lowest and lowest health categories (Y and Z).

[14]Once health status is controlled for, cognitive ability is not a statistically significant predictor of enlistment. The score on noncognitive ability is statistically significant at the five percent level, but the estimated effect is weak. We provide results regarding the selection into the military service in Appendix B.

the military service is unrelated to cognitive ability but men with high noncognitive ability are significantly more likely to actually start the military service conditional on being enlisted. However, attrition is unrelated to educational attainment and wages conditional on skills (see Table B6). The duration of the military service time varies between 7 and 18 months depending on type of service. Service time is typically 7 or 8 months for privates (67 percent of enlisted men in our sample), 10 months for squad leaders (23 percent) and 12-18 months for men enlisted as platoon leaders (10 percent). The far majority of men leave the military after the mandatory military service. In our sample, only 0.69 percent had a military career as of 2006.

2.2.1 Measure of cognitive ability

The Swedish military has conducted tests of conscripts' cognitive skills since the mid 1940's. These tests have changed several times over the years, but the men in our sample all did the same test.[15] This test consists of four different parts (synonyms; inductions; metal folding and technical comprehension) which are each graded on a scale from 1 to 9.[16] The results of these tests are then transformed to a discrete variable of general cognitive ability also ranging from 1 to 9. This variable follows a Stantine scale that approximates a normal distribution.[17]

We normalize the 1-9 measure of general cognitive ability to a distribution with zero mean and unit variance.[18] This measure is available for the entire sample and used in our main specifications.

We also construct an alternative measure of cognitive ability from the sum of the scores on each subtest, which ranges from 4 to 36. The sum of the subscores is percentile

[15] See Carlstedt (2000) for a detailed account of the history of psychometric testing in the Swedish military. She provides evidence that the test of intelligence is a good measure of general intelligence (Spearman 1904). In this sense, the test of cognitive skills at the military enlistment differs from AFQT which focuses more on "crystallized" intelligence, i.e., skills that are teachable (interview with Berit Carlstedt Nov. 26th 2009). See Kilburn et al (1998) for a description of the AFQT.

[16] There are 40 questions on each subtest, and the number of correct answers to these question give the subscore grade on the 1-9 scale. See Appendix F for more detailed definitions of each subtest.

[17] The ideal Stantine distribution (with % of population in parentheses) is: 1 (4); 2 (7); 3 (12); 4 (17); 5 (20); 6 (17); 7 (12); 8 (7); 9 (4).

[18] We use the same normalization for all cohorts even though the exact mapping from the scores on each subtest to general cognitive ability has changed slightly over the years. The reason is that we lack data on enlistment year for 141 observation. The correlation between a normalization for all cohorts and a normalization by enlistment year is .999 for cognitive ability and .998 for noncognitive ability.

rank-transformed and then converted by taking the inverse of the standard normal distribution to produce normally distributed test scores. This measure has a more continuous distribution and higher moments closer to a normal distribution with unit variance. The main reason to focus on the first rather than the second measure is that data on the subscores underlying the general score is only available for 13,278 out of 14,703 observations in our data. As shown in Appendix F, the results do not change appreciably depending on which measure we use.

2.2.2 Measure of noncognitive ability

Like the test of cognitive skills, personality tests were introduced at the military enlistment in the early 1940's by Torsten Husén, a prolific writer in the field of military psychology.[19] This development was inspired by the extensive testing procedure that Germany had built up during the 1930's for the selection of officers and specialists, and by experiences from the United States (Husén 1941). The early attempts at designing adequate tests for different personality types were characterized by relatively advanced psychometric methods and a strong focus on evaluating their predictive power for performance in the military.[20] Important later sources of inspirations were the *The American Soldier Studies*, the first large-scale study about soldiers' attitudes and experiences of war, and the experiences of Swedish troops on UN-missions (Lothigius 2004).[21]

All the men in our data had their psychological profiles evaluated according to a procedure that was adopted in 1972 and kept unchanged up to 1995 when it was subject

[19] Husén recognized already at an early stage that selection into the military service must be based both on an assessment of constripts' intelligence and their character (Husén 1942b). For example, Husén emphasized the important role of emotional stability (1942a) for success in the military. Another common theme in Huséns early writings is that men will bring their personality in civilian life into the military service. For example, Husén (1946) emphasizes that men who have difficulties adjusting to their civilian environment will only see these difficulties magnify while in the military.

[20] In 1942, a wide range of tests was conducted on an entire cohort of conscripts (32,000 men) with the aim of acquiring expertise on how to conduct psychological tests (Husén 1942c). The tests of cognitive ability, physical fitness, but also of willpower and power of initiative. The reliability of each test was then evaluated by correlating the test scores with the commanding officer's assessment of the conscripts' military skills at various stages of the military service. Based on these experiences, a test of cognitive ability was introduced in 1944 together with more extensive tests of personality for applicants to the military academies (Husén 1946). By 1950, psychological stability and ability to adjust to the military environment were assessed for the majority of conscripts in a 10-20 minute interview (Husén 1951).

[21] *The American Soldier Studies* consisted of interviews with more than half a million soldiers on a diverse set of subjects, e.g. their attitudes toward the enemy, their mental health and their combat experiences (see Lazersfield 1949).

to minor revisions. This procedure implies that conscripts are interviewed by a certified psychologist for about 25 minutes.[22] As a basis for the interview, the psychologist has information about the conscript's results on the test of cognitive ability, physical endurance, muscular strength, grades from school and the answers to 70-80 questions about friends, family and hobbies, etc. The interview is semi-structured in the sense that the psychologist has to follow a manual that states certain topics to be discussed, though specific questions are not decided beforehand. We provide more information regarding the enlistment interview in Appendix F.

The objective of the interview is to assess the conscript's ability to cope with the psychological requirements of the military service and, in the extreme case, war.[23] The psychologists assign each conscript's military aptitude a score from 1 to 9, which follows the same Stantine distribution as the final test score for cognitive ability.[24] This score is in turn based on four different subscores which range from 1 to 5. The subscores function only as a guide to the psychologists – two conscripts with the same sequence of subscores could still get different final scores.[25] We create two measures of noncognitive ability based on the psychologists assessment of the potential conscripts. First, we normalize the 1-9 score to a distribution with mean zero and unit variance. Second, in order to get a more continuous variable, we take the sum the result on each subscore and convert it into an approximately normally distributed variable using the same procedure as for cognitive ability. As for cognitive ability, the subscores are not available for the entire sample and we therefore only report the results for the second measure in the online appendix. In practice, the two measures of noncognitive ability are highly correlated (.97) and the results do not change appreciably depending on which measure we use.

What character traits and abilities give a high score at the enlistment interview? According to the SNSA, a high ability to function in the military requires willingness to

[22] Psychologists have to undergo a four-week course prior to working for the SNSA. The educational requirements have increased over time. As of the mid-1970's, most psychologists had a bachelor's degree (Lilieblad and Ståhlberg 1977)

[23] Carlstedt (1999) shows that this score has predicted power for the commanding officers' assessment of conscripts' skills after completion of the military service.

[24] In addition, leadership skills are estimated for those who score at the average or above on the test of cognitive abilities. In practice, the assessment of ability to cope with war stress and leadership skills are based on rather similar criteria and highly correlated in the data (.88).

[25] The definition of the subscores underlying the psychologists assessment is not publicly available information. However, we provide additional information regarding the psychologist interview in Appendix F.

assume responsibility; independence; outgoing character; persistence; emotional stability, and power of initiative (Lothigius 2004). Another important aspect is the conscript's ability to adjust to the specific requirements of life in the armed forces, like loss of personal freedom. Motivation for doing the military service is not among the set of characteristics that are considered beneficial for functioning in the military (Lothigius, 2004). SNSA psychologists Andersson and Carlstedt (2003, p. 8) argue that there is no evidence that highly motivated individuals are also better suited for military service. In their view, selection based on the motivation for the military service would have a negative effect on the quality of conscripts.

Also worth to note is the importance attached to social skills. Citing previous research in psychology, Andersson and Carlstedt (2003, p. 9) argue that group cohesion is the single most important factor that influence soldiers' ability to cope with war stress. Soldiers overcome their anxiety and continue to fight not because of strong feelings of hostility toward the enemy but because they don't want to abandon their friends. Accordingly, the single most important cause of soldiers' mental breakdowns during combat is a breakdown of group cohesion. As a result, people who "do not posses the ability to function in a group and help create group cohesion are [...] unfit for combat." The importance of group cohesion is also stressed by *The American Soldier Studies*. Among the key findings from these studies were the low prevalence among combat troops of strong expressions of hostility toward enemy soldiers; the near universality of fear, and the importance of group obligations rather than ideological considerations in motivating soldiers for battle (Lazarsfeld 1949).

Another explicit objective with the interview is to identify people who are particularly unsuited for the military service. For instance, people with undemocratic values or an obsessive interest in the military are not considered fit for military service (Lothigius 2004). The same holds true for men with some kind of antisocial personality disorder, in particular psychopaths (Andersson and Carlstedt 2003, p. 9).[26] Other aspects of personality that are considered negative are difficulty in accepting authority, to adjust to a different environment and violent or aggressive behavior (Andersson and Carlstedt

[26] The difficulty in assessing people with antisocial personality disorders is one reason for why the SNSA relies on interviews rather than questionnaires. In particular, psychopaths with high intelligence could trick a questionnaire test and give answers that they know will increase their chances of obtaining military command (Andersson and Carlstedt 2003, p. 11).

2003, p. 13).[27]

Our noncognitive measure from the military enlistment is different from measures previously used in the literature on personality and labor market outcomes. Instead of measuring a specific trait, our measure captures a specific ability, i.e., the ability to function in the very demanding environment of armed combat. We argue that this ability is likely to be rewarded in the labor market. Just like in the military, success in most work environments requires an ability to socialize with co-workers, to cope with stress, to show up on time and to be able to deal with criticism and failure.

Apart from the measure of noncognitive skills, there are two additional advantages with our data. First, the fact that the enlistment procedure always takes place around the age of 18 or 19 mitigates the problem of reverse causality with schooling and labor market outcomes. Second, the size of the data set (more than 14,700 individuals) allows us to obtain precise estimates and explore labor market outcomes in detail.

3 Estimation

In this Section, we discuss our strategy for estimating how cognitive and noncognitive skills affect wages, unemployment and labor market earnings. Consider the equation

$$y_i = f(c_i, n_i) + \mathbf{X}_i \boldsymbol{\gamma} + \varepsilon_i, \tag{1}$$

where y_i is one of the three labor market outcomes, n_i is the normalized measure of noncognitive ability, c_i the normalized measure of cognitive ability and $\mathbf{X}_i$ a vector of control variables. In our basic specification, $\mathbf{X}_i$ contains dummy variables for region of residence, cohort, family background, enlistment into the military service and a dummy variable for whether or not an individual has some education above primary school. We consider different specifications of $f(c_i, n_i)$, but – like the previous literature – we focus

[27] The focus on avoiding the *martial misfits*, like neurotics and psychopaths, is present already in Husén writings from the 1940's. Husén (1946) argues forcefully that the military service itself is unlikely to change men to the better. Men with an anti-social personality will, if anything, become more anti-social. Neurotic men will see their symptoms worsened, etc. In a large study of conscripts' with particular discipline problems, Husén (1951) argues that a substantial share of indiscipline conscripts exhibit problems adjusting to also to civilian life.

on the linear case, i.e.,

$$f(c_i, n_i) = \beta_c c_i + \beta_n n_i. \tag{2}$$

As an extension, we add quadratic terms for c_i and n_i and an interaction term between c_i and n_i to $f(c_i, n_i)$.

There are five important issues to consider in the estimation of (1).

First, there is a direct correspondence between the distributional assumptions on c_i and n_i and the estimated functional form of $f(c_i, n_i)$. In our case, $f(c_i, n_i)$ is estimated under the assumption that cognitive and noncognitive ability are normally distributed, in accordance with SNSA's variable construction.[28] We believe this assumption is a reasonable benchmark case which has the advantage of making our results comparable to other measures of ability, such as IQ, which are also normally distributed by assumption.

Second, our measures of cognitive and noncognitive ability are positively correlated (0.388), and the way we think about this covariance affects our interpretation of the estimates of β_c and β_n.[29] If the cognitive test score reflects noncognitive ability, then controlling for cognitive ability will bias the estimated effect of noncognitive ability, and vice versa. Both directions of causality are plausible. On one hand, the psychologists know the conscripts' results on the test of cognitive ability before conducting the interview. The cognitive skill score could thus directly affect the psychologists' assessment of noncognitive ability. Using a fifth-order polynomial in the sum of subscores as a control for cognitive ability, we show in Appendix F that an increase in the final cognitive ability test score by one point on the 1-9 scale increases estimated noncognitive ability by 0.11 points on average. If this were the only source of covariance between cognitive and noncognitive ability, it would imply a correlation of 0.11. On the other hand, noncognitive skills have been shown to influence performance on tests of cognitive ability (Borghans et al.

[28] From a theoretical perspective, the true distribution of skill depends upon the difficulty of the relevant task. For example, the distribution of the ability to solve highly abstract mathematical problems is arguably different from the distribution of the ability to do basic calculus. Since we focus on general labor market outcomes rather than the ability to solve a specific task, it is hard to know which type of distribution is most relevant a priori.

[29] Few studies in the previous literature report correlation coefficients between measures of cognitive and noncognitive ability. In a working paper version of their 2006 paper, Heckman, Stixrud and Urzua (2005) report correlations between self-esteem (Rosenberg), locus of control (Rotter) and various measures of cognitive ability. For men, the noncognitive ability measures have correlations coefficients between 0.07 and 0.21 with the cognitive ability measures. The correlations are considerably higher for women (between 0.21 and 0.33). Olnek and Bills (1979) report correlations between IQ and various "noncognitive" abilities such as industriousness and emotional control in the range 0.20 to 0.28.

2008c and Segal 2008). Moreover, noncognitive abilities could facilitate the acquisition of cognitive abilities over the life-cycle (Cuhna and Heckman 2007). Hence, it seems plausible that both types of skills affects the measured level of the other skill measure, though it is uncertain which effect is most prevalent. To provide bounds on the potential biases, we estimate (1) with each ability measure taken out of the regression. This gives us an upper bound on the effect of cognitive or noncognitive ability while the regression with both measures included provides the lower bounds.[30]

Third, our estimates may be biased if cognitive or noncognitive skills are measured with error. This is a particular problem for our measure of noncognitive ability as psychologists differ in their evaluation of identical conscripts. Lilieblad and Ståhlberg (1977) estimated the correlation between SNSA psychologists' assessment of noncognitive skills to 0.85 after letting psychologists listen to tape recordings of enlistment interviews.[31] Using a data on identical and fraternal twins, we find that the reliability ratio for cognitive ability (0.868) is indeed substantially higher than for noncognitive ability (0.703). The details behind our estimation of the reliability ratios are available in Appendix C. Since our estimation of the reliability ratios relies upon a number of additional assumptions (e.g., uncorrelated measurement error within twin pairs), we will focus on the case with no measurement error correction.

Fourth, since we do not observe wage offers for the entire sample there might be a selection bias in our wage regressions. We use two approaches to control for selection bias. Our first approach is to test whether our results change when we exclude or include imputed wages. Our second approach is to use three alternative estimation methods that control for selection bias under different conditions (median regression, Heckman two-step and Identification at infinity). The details behind these methods are available in Appendix B.

Fifth, as we are estimating the partial correlations between our ability measures and outcomes, the interpretation of the estimated parameters depends on the variables included in the covariate vector, $\mathbf{X}_i$. Since the basic set of control variables includes variables that are pre-determined at the time of the draft, this specification is silent on the

[30] An alternative procedure would be to run a regression of one skill measure (e.g. noncognitive ability) on the other skill measure (e.g. cognitive ability) and then use the residuals as an ability measure.

[31] Since all psychologist listen to the same interviews, this correlation is not an exact measure of the true correlation between psychologists' assessment. The fact that psychologists make their own interviews could, in theory, both imply that the true correlation is higher or lower than 0.85.

exact mechanism by which skills affect outcomes.[32] Notably, selection into higher education is an important channel by which skills could affect labor market outcomes. To test for the importance of post-draft variables, we augment the basic specification of $\mathbf{X}_i$ with the full set of dummy variables for educational attainment and linear-quadratic terms in work experience.

A related issue is the role of schooling for the formation and measurement of cognitive and noncognitive skill. In our case, the far majority of conscripts undergo the enlistment procedure the year they turn 18 or 19 (the average age at the draft is 18.3). Since primary school in Sweden typically ends the year one turns 16, this implies that men who continue to secondary school have received about two more years of schooling at the time of the draft compared to men who drop out after primary school. Conscripts who dropped out after primary school score 0.94 standard deviations lower on the cognitive skill test and 0.85 standard deviations lower on noncognitive skills than those who continued to secondary school. From a theoretical perspective, these differences could reflect the selection of high ability men into secondary school or an effect of secondary school on skills. If the former case dominates, one should not control for educational attainment at the time of the draft in a regression that aims to estimate the total effect of skills on labor market outcomes. In contrast, not controlling for educational attainment at the time of the draft will bias the estimates if schooling affects skills. In practice, however, our choice of including educational attainment at the time of the draft has only a small (negative) effect on the estimated effects of cognitive and noncognitive ability (results available upon request).

We discuss a number of further concerns regarding omitted variable bias in Appendix D. First, as enlistment test scores affect the type of military training, the estimated effect of our skill measures might be confounded with the effect of different types of military service on labor market outcomes. A second concern is that the measure of noncognitive skills may function as a proxy for health status, which could have an independent effect on outcomes. Controlling for health status does not affect our estimates, but the estimated effect of skills on wages are smaller when we control for type of military service.

[32] The exception is region of residence which refers to the year 2006.

4 Labor market outcomes

In this section, we discuss the effect of cognitive and noncognitive skills on wages, unemployment and annual labor market earnings. We first consider wages.

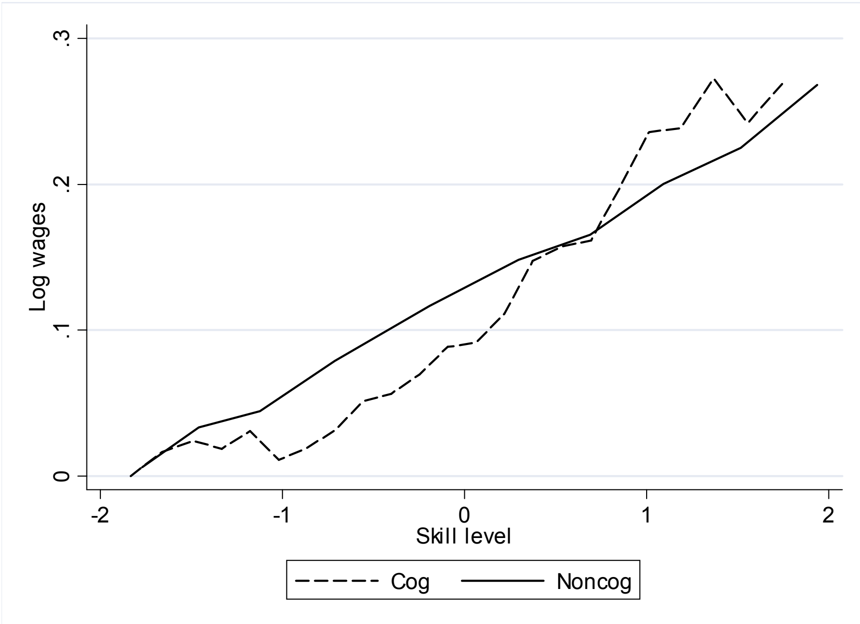
4.1 Wages

The results for regression (1) with log wages as the dependent variable are presented in Table 1. Column 1 shows the results for the basic specification without controls for higher educational attainment or adjustment for measurement error. In this case, an increase in cognitive ability by one standard deviations predicts a wage increase by 8.6 log points compared to 6.6 log points for noncognitive ability. To facilitate comparisons with previous literature, Table 1 also reports beta coefficients, which are standardized with respect to the variability in the dependent variable. In the basic specification, a one standard increase in cognitive ability predicts an increase in log wages by 0.265 standard deviations compared to .204 standard deviations for noncognitive ability.

The relative importance of cognitive and noncognitive skills is reversed once we control for educational attainment (column 2). The reason is that cognitive ability is a much stronger predictor of higher education than noncognitive ability. For example, cognitive ability is an almost four times stronger predictor of a university degree than noncognitive ability (results available upon request). This is an indication that our skill measures capture different types of skills.

As shown in column 3 and 4, adjusting for measurement error has a strong effect on the estimated effect of noncognitive skill – the estimated effect increases 30 percent – whereas the estimated effect of cognitive skills remain essentially unchanged. The estimated effects of cognitive and noncognitive ability both increase substantially when each ability measure is included separately in the regression (column 5 and 6). As discussed above, the regression with only cognitive ability gives the effect of cognitive ability on wages in case the covariance between the ability measures only reflects an effect of cognitive ability on measured noncognitive ability. Correspondingly, the regression with only noncognitive ability gives the effect of noncognitive ability that holds if the covariance is only due to an effect of noncognitive ability on measured cognitive ability. The last column in Table 1 shows the results when we include quadratic terms and an interaction effect between cognitive and noncognitive ability. We find that log wages are strictly convex in cognitive ability but linear in noncognitive ability. The interaction term is positive and statistically significant, implying that the return to cognitive skill is increasing in noncognitive skills, and the other way around. Still, the regression model with higher order terms gives only a small increase in terms of variance explained. Figure 1 presents results from a nonparametric estimation where we let each unique value of the alternative, more continuous, skill measures be represented by a dummy variable.[33] As is clear from the figure, the return to noncognitive skill does not change appreciably with skill level, while the return to cognitive skill is increasing in cognitive skill.[34]

Figure 1. Nonparametric estimation of wages



Nonparametric estimation using dummy variables for each value on the alternative ability measures based on the sum of subscores. Both skill measures have been truncated at +/- 1.96 standard deviations. The effect of each skill measure has been normalized to zero for a skill level of -1.96.

[33]Since the alternative skill measure for cognitive skill is based on a finer scale (4-36) than the measure for noncognitive skill (4-20), there are fewer observations for each unique value of cognitive skill, making the results noisier.

[34]See Figure D1 and D2 for a nonparametric estimation that shows the positive interaction effect between cognitive and noncognitive ability.

Table 2 presents results from two extensions to the basic regressions. First, we add as control variables grade point average and education track in secondary school and the full set of interaction terms between these variables. Education tracks differ in whether they prepare students for future study or whether they provide some form of vocational training, and regarding the focus of studies (e.g., natural science or humanities). In total, the men in our sample chose between 46 different education tracks. The estimated effect of cognitive ability is very sensitive to including the secondary school variables whereas the results for noncognitive ability are remarkably robust. Since performance in school is closely correlated with cognitive ability, these results probably understates the importance of cognitive ability for labor market outcomes. Yet it is reassuring that our results for noncognitive ability are not driven by a correlation between the psychologists' assessment and performance in school.

Second, we test whether cognitive and noncognitive ability are valued differently across occupations. Data on occupational status in 2006 is available in LINDA for $12,379$ workers. For all occupational groups except managers and military officers, our data contains information on the level of qualifications needed on the job. We classify workers in the two highest qualification levels (out of four) as "skilled" and the workers in the two lowest qualification levels as "unskilled". Managers are treated as a separate group. We exclude the small group of military officers as it is unclear whether they should be classified as managers or skilled workers.[35] Two findings stand out from a comparison of the mean values of skills across these occupational groups. First, whereas the average level of cognitive skills is highest among workers in skilled occupations, managers have the highest average level of noncognitive skills. Second, the difference between skilled and unskilled workers is much stronger in terms of cognitive than noncognitive skills.[36] The estimated skill prices are consistent with these selection patterns.[37] Noncognitive skill has a higher return than cognitive skill for managers and workers in unskilled occupations while workers in skilled occupations face similar returns to both types of skill.[38] The results remain

[35]Further details underlying our classifications are available in Appendix E.

[36]All differences in average skills between occupational groups are statistically significant at the one percent level, except for the difference between managers and highskilled workers in cognitive skills which is statistically significant only at the twenty percent level in a two-sided test.

[37]Since our aim in this case is to estimate how skill prices vary by occupational groups, we include the full set of dummy variables for educational attainment and linear-quadratic terms for experience as control variables.

[38]There is a small previous literature on occupational choice and skill endowment. In line with our

qualitatively similar when we estimate skill prices using econometric models that adjust for self-selection into different occupations (see Appendix E).

We discuss a number of issues related to sample selection in Appendix B. As shown in column 5 of Table B2, noncognitive skills is a strong predictor of observable wages while cognitive ability is not statistically significant. The estimated effect of noncognitive ability is about 0.02 log points larger when estimated using Heckman two-step, indicating that the effects we estimate for noncognitive ability may be biased downward. Table B4 shows that our results are similar when we include students, self-employed and workers in the agricultural sector, when estimating the results using the family members in LINDA instead of the core sample, or when using data from several years.

We discuss alternative skill measures in Appendix F. Table F3 and F5 presents results where we include each cognitive and noncognitive subscore separately, as well as all subscores jointly. Table F3 shows that the cognitive subscores are very similar as predictors of log wages and that the incremental R^2 from adding all measures jointly is small. This result is in line with the g-theory of intelligence, which argues that a single general factor explains a large proportion of the variance across intelligence tests (see, for example, Spearman 1904 and Heckman 1995). Table F5 shows that the results are similar also for each subscore of noncognitive ability.

4.2 Unemployment

As shown in Table 3, noncognitive skills is a stronger predictor of receiving unemployment support in 2006 than cognitive skill. The estimated effect of cognitive ability on the probability of receiving unemployment support is between -1.5 and -2.2 percentage units depending on whether noncognitive ability is included in the regression or not. The estimated effect of noncognitive ability is between -2.4 and -2.8 percentage units,

results, Schmidt and Hunter (2004) find that the importance of IQ rises with job complexity. In contrast, Gould (2005) find relatively small differences in IQ across sectors. Borghans et al (2008a) find that persons with a preference for a "direct" relative to "caring" style in interpersonal encounters select into occupations where directness is required (e.g., managers). There is also some previous evidence in support of the view that personality is of particular importance for workers in managerial positions. Surveying the psychology literature, Borghans et al. (2008b) find that while IQ is considerably more important for job performance than any of the Big Five-factors of personality, the Big Five-factor *conscientiousness* is slightly stronger correlated with leadership than IQ. Kuhn and Weinberger (2005) find that men who occupied leadership positions in high school are more likely to occupy a managerial position as adults and that the wage premium associated with high school leadership is higher in managerial occupations.

implying that the upper bound of the effect of cognitive ability is lower than the lower bound for noncognitive ability. As shown in Appendix D (Table D1), noncognitive ability is an even stronger predictor of unemployment relative to cognitive ability when we control for educational attainment or adjust for measurement error.

Since unemployment insurance benefits are subject to a time limit and are based on previous income, men with a permanently weak attachment to the labor market may not be eligible for unemployment insurance. We therefore construct an alternative measure of "unemployment" which also includes men who receive disability insurance or social welfare payments.[39] These men have a significantly weaker attachment to the labor force compared to those who just received unemployment support.[40] The relative importance of noncognitive ability increases when we use this alternative measure (column 4).

Table 3 also shows that, conditional on becoming unemployed, men with high noncognitive ability obtain a new job more quickly. This results hold regardless of whether we estimate the hazard rate of leaving unemployment (column 5) or the total duration of unemployment by OLS (column 6). Note that a higher hazard rate implies shorter unemployment spells. A one standard deviation increase in noncognitive skill decreases expected unemployment duration by 0.037 years, or about two weeks (column 6). The effect of cognitive skills on the job finding probability is neither economically nor statistically significant.

The relative importance of noncognitive ability for labor force participation is consistent with two different explanations. First, we showed above that noncognitive ability is a stronger predictor of wages in unskilled occupations. This suggests that men with low noncognitive ability may be priced out of the labor market.[41] Second, men with low noncognitive ability could have a higher reservation wage. It is beyond the scope of this

[39] Eligibility for disability insurance requires that an individual's capacity to work is permanently reduced by at least 25 percent. Like unemployment support, disability insurance is based on previous income. In contrast, social welfare is provided on a case-by-case basis and is not based on previous income. The aim of social welfare is to provide all Swedish citizens with a minimum standard of living.

[40] While average annual earnings in 2006 were 349,400 SEK for men who did not receive any kind of income support (88.5 percent of the sample), the corresponding figure was 144,200 SEK for men with unemployment support (9.2 percent), 50,400 SEK for men with social welfare benefits (1.8 percent) and 30,000 SEK for men who received disability insurance.

[41] Minimum wages in Sweden are set by negotiations between employers and trade unions and are binding mainly in the service sector. The level of social assistance granted to households with several children may also be higher than the minimum wage for service sector jobs. See Skedinger (2008) for a discussion of minimum wages in Sweden.

paper to distinguish between these two explanations.

4.3 Earnings

As shown in Table 3, the effect of cognitive and noncognitive skills on average earnings are similar to the effects found for wages. A one standard deviation increase in noncognitive ability predicts an increase in the conditional mean of earnings by 37,100 SEK (11 percent of average annual earnings) compared to 32,800 (10 percent) for cognitive ability.[42]

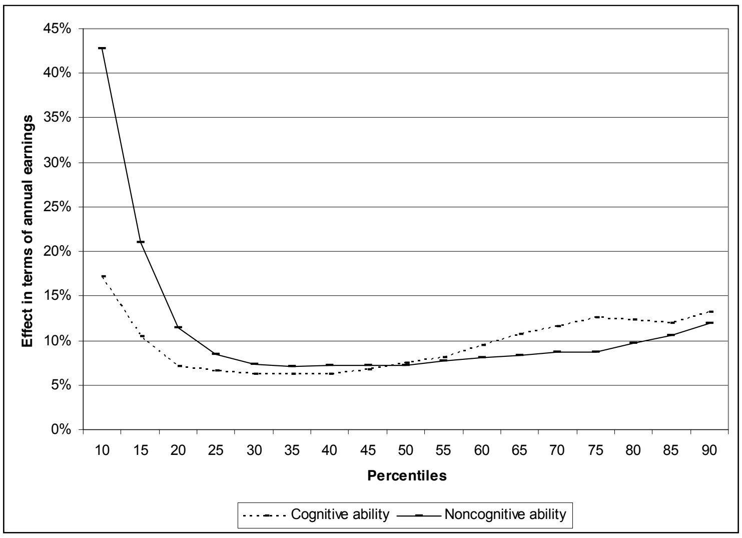
Though cognitive and noncognitive skills have similar effects on average earnings, they could still have differential effects at different quantiles of the earnings distribution. In particular, low annual earnings are strongly related to lack of employment for Swedish men. In our sample, 70 percent of men with earnings below the tenth percentile received some kind of income support related to lack of employment in 2006 (unemployment support, disability insurance or social welfare). The corresponding figure for the top 90 percent of earners is 6 percent. Since noncognitive ability is more important than cognitive ability for employment, we would expect it to have a stronger effect at the low end of the distribution of earnings.

We use the method developed by Firpo, Fortin and Lemieux (2009) to estimate the effect of cognitive and noncognitive ability on the unconditional quantiles of the annual earnings distribution. Unlike conditional quantiles (which do not sum up to the unconditional population counterparts), these estimates answer the question how an increase in the entire population's cognitive or noncognitive ability changes a certain quantile in the unconditional distribution of earnings. As is clear from Figure 2, noncognitive ability has a very strong effect on earnings at the low end of the earnings distribution. At the 10th percentile, an increase in noncognitive ability by one standard deviation increases annual earnings by 52,640 SEK which corresponds to 42.7 percent of annual earnings at the 10th percentile (123,300), or 16.5 percent of average annual earnings (319,800). By contrast, the effect of cognitive ability does not vary much throughout the distribution of earnings. The effect of noncognitive ability is stronger for earnings below the median

[42]Since we would have to truncate the earnings distribution in order to get meaningsful estimates if we were to take logs, we use the absolute value of earnings as the dependent variable in our earnings regressions. We show the results for the log of annual earnings truncated at 120,000 SEK in Table D1.

while cognitive ability is more important for earnings above the median.

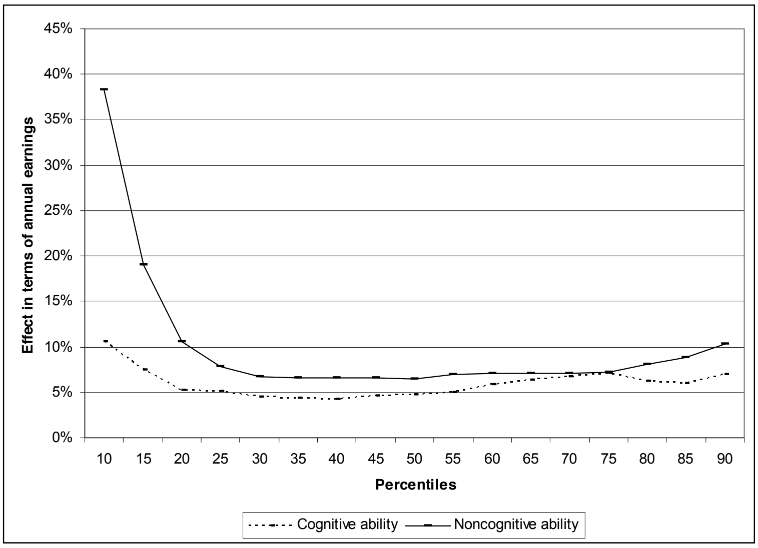
FIGURE 2. UNCONDITIONAL QUANTILES
Small set of covariates



Effect in absolute number (SEK) divided by annual earnings at each percentile. See Appendix D for details.

Figure 3 shows the effect of skills on the unconditional quantiles when we add experience and education to the set of covariates. Holding education and experience fixed implies that the effect of cognitive ability on earnings goes down, in particular at the higher quantiles. The estimated effect of noncognitive ability is affected to a much smaller extent and the effect at the lower quantiles is almost exactly the same.

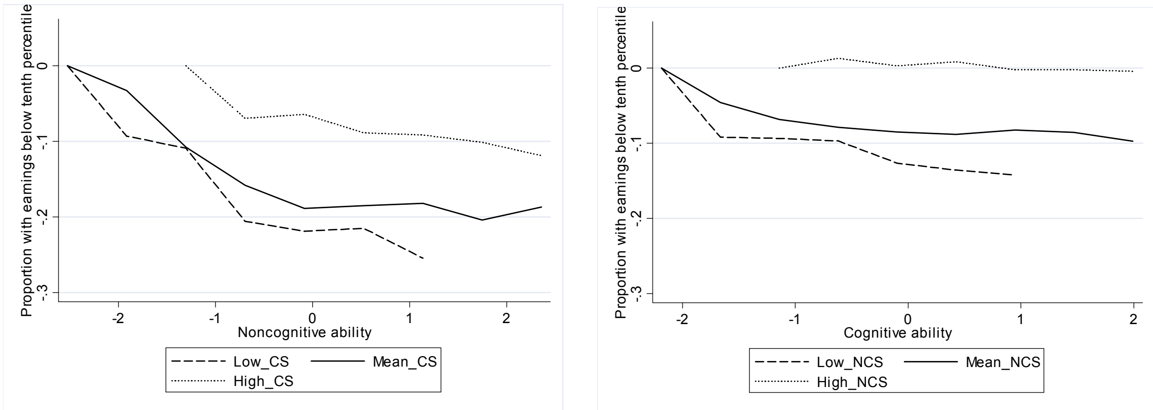
Figure 3. Unconditional Quantiles
Large set of covariates



Effect in absolute number (SEK) divided by annual earnings at each percentile. See Appendix D for details.

Our quantile regressions rely on the simple linear specification of regression equation (1). To obtain a more complete picture of how skills affect the probability of low earnings (below the 10th percentile), we employ a simple nonparametric estimation. We let each unique value of cognitive and noncognitive ability be represented by a dummy variable and estimate the effect of each skill measure on the probability of low earnings while fixing the opposite skill measure at low (1-3 on the 1-9 scale), medium (4-6) or high values (7-9). We exclude skill combinations for which there are fewer than 100 observations and normalize the effect to zero for the lowest skill value which is included in the regression. As shown in Figure 4, the proportion of men with low earnings is decreasing in noncognitive ability regardless of the level of cognitive ability, though the effect is largest for men with low cognitive ability. In contrast, the level of cognitive ability makes no difference for men with high noncognitive skills, and does only matter at the very low end of the cognitive ability distribution for men with low or average noncognitive skill.

Figure 4 and 5. Ability and low earnings



Samples restricted to "low" (1-3), "mean" (4-6) or "high" (7-9) values of the corresponding skill measure. All regressions include fixed effects for each value of cognitive and noncognitive skill included in the specific sample and the "small" set of covariates as defined in Table 1. Sample restricted to skill combinations with at least 100 observations. Effect normalized to zero for the lowest value included in the regression.

5 Relation to previous literature

In this section, we discuss how our results relate to the previous literature. We focus on the results for noncognitive ability. Our results for the effect of cognitive skills on wages is similar to what has been found in previous literature.[43]

Table A2 summarizes the results from previous studies of the association between personality and wages or earnings. Where possible, we report both coefficients that have been standardized with respect to the variance in the independent variables, and coefficients which are also standardized with respect to the variance in the dependent variable (beta

[43]Bowles, Gintis and Osborne (2001) present 65 estimates of cognitive ability measures in earnings regresssions from 24 different studies. The mean estimate was 0.07 for standardized regressions coefficients (normalized with respect to the variance in cognitive ability but not with respect to variance in earnings) and 0.15 for beta coefficients. Excluding noncognitive ability but controlling for the large set of covariates (which corresponds most closely with the specifications in previous literature on cognitive ability and earnings), we get a standardized coefficient of cognitive ability of 0.063 and a beta coefficient of 0.196. This particular specification is not reported in the paper, but is available from the authors upon request.

coefficients).

As is clear from Table A2, most studies use measures derived from surveys. Among these studies, there is a fairly high congruence in terms of the specific measures used. The "internal-external locus of control" scale developed by Rotter (1966) is used in four studies and very similar measures in another two ("personal efficacy" in Duncan and Morgan 1981 and "personal control" in Dunifon and Duncan 1998). The Rotter scale measures to what extent individuals believe that they can affect their own fate. A high external locus of control – the belief that events are determined by external forces – is negatively associated with wages.[44] A standard deviation increase in "external locus of control" decreases log wages by between 5-7 percent whereas beta coefficients vary between -0.05 and -0.15.[45] The outlier is Dunifon and Duncan (1998) where a one standard deviation increase in "personal control" (similar to internal locus of control) predicts a 14 percent increase in log wages. As noted by Dunifon and Duncan, this could partly be due to the advanced age of their sample population. Another potential explanation is that the cognitive ability measure available in PSID – a sentence completion test – is relatively imprecise control for cognitive ability. The study by Jencks (1979) also constitutes a special case. Along with results for several measures of self-assessed personality traits and behaviors, Jencks (1979) reports results for a measure of noncognitive skills based upon the linear combination of seven different traits and behaviors that maximizes predictive power. This measure is substantially stronger associated with earnings than any individual trait or behavior.

A few studies consider behavior in certain situations. Olnek (1979) and Segal (2009) use data on teacher assessments of classroom behavior while Edwards (1976) consider peer-group ratings of worker characteristics. Edwards (1976) finds a very strong association between within work-group wage differences and the extent to which workers internalize firm goals and values. However, since two thirds of the overall variance in wages in his sample reflect between-work group differences, it is difficult to know to which extent his result generalizes to other settings. Moreover, as wages were already set when peer group ratings were made, there is a potential problem of reverse causality.

Heckman et al. (2006) derive measures of cognitive and noncognitive skills both from

[44] As discussed by Jencks (1979), the direction of causality is not clear in this case: "Individuals may believe that they can control their lives because they face favorable circumstances, or because they possess other unmeasured characteristics that facilitate success" (p. 124).

[45] Note that some studies revert the scaling of these measures. For example, an increase in "personal control" (Dunifon and Duncan 1998) is similar to a decrease in "external locus of control".

survey data and from a structural model. Their structural model builds on a methodological framework developed by James Heckman and co-authors in a sequence of papers starting with Carneiro et al (2003) and Hansen et al (2004). In this framework, cognitive and noncognitive skill are modelled as latent factors which are distributed as mixtures of normals. Heckman et al. (2006) assume independence between cognitive and noncognitive skills, but this assumption is relaxed in future work (Cunha and Heckman 2008a and 2008b). The parameters are estimated and the factors extracted so that the best fit with data on test scores and a set of outcomes is obtained. The noncognitive skill factor derived this way is a significantly stronger predictor of wages than their alternative noncognitive skills measure based on the sum of Rotter Locus of Control Scale and Rosenberg Self-Esteem Scale.

Finally, Murnane et al. (2001) and Segal (2008) find that measures of coding speed – a proxy for motivation – from the ASVAB test battery are positively associated with labor market outcomes even after controlling for cognitive ability.[46]

As most studies in the previous literature report results when education is controlled, the results in Table A2 should be compared to our regressions with the large set of covariates, i.e., columns (2) and (4) in Table 1.[47] In general, our point estimates are large compared to the previous literature, in particular the beta coefficients which adjust for relatively compressed wage structure in Sweden.

Still, the main contribution of our analysis compared to previous literature lies not in the size of our point estimates in wage regressions, but in the importance of noncognitive ability for avoiding bad labor market outcomes, in terms of unemployment and low annual earnings. Since the previous literature has focused on the effect of noncognitive ability on wages rather than employment, it is difficult to know to what extent this result generalizes to other countries and measures of personality. At least compared to the US, the distribution of cognitive ability is relatively compressed in Sweden (Nickell 2004), which

[46]The main reason for the stronger point estimates in Murnane et al. (2001) is that the specification chosen from Segal (2008) includes educational attainment (results when educational attainment is controlled are not reported in Murnane et al.) The point estimate in Segal (2008) when education is not included as a covariate (0.092) is quite close to the estimate in Murnane et al. (2001). The remaining difference could be due to differences in sample selection, other control variables or the derivation of the coding speed measure.

[47]Whether one should compare to column (2) or (4) depends on whether the original study corrects for measurement error or not. Osborne-Groves (2005); Murnane et al. (2001); Goldsmith et al. (1997); and the structural model in Heckman et al. (2006) attempt some form of measurement error correction.

may explain why cognitive ability is less important than cognitive ability for labor force participation.[48] Our analysis nevertheless suggests that cognitive and noncognitive ability have distinctively different effects in the Swedish labor market.

6 Concluding remarks

Understanding why some succeed while other fail in the labor market is a key question in labor economics. In this paper, we investigate how skills measured at the Swedish military draft relate to labor market outcomes later in life. Our study differs from the previous literature in that we are able to use a measure of noncognitive ability based on a personal interview.

We find that cognitive and noncognitive skills have differential effects on labor market outcomes. Noncognitive ability is a stronger predictor of labor force participation, earnings at the low end of the earnings distribution and wages of unskilled workers. By contrast, cognitive ability is a stronger predictor of wages for skilled workers and earnings above the 50th percentile. In other words, cognitive ability appears to be somewhat more important for achieving success in the labor market, but noncognitive ability is much more important for avoiding failure.

The results in this paper are potentially important for a number of related literatures. For example, previous research (e.g. Cunha et al. 2006; Cunha and Heckman 2007) have suggested that noncognitive abilities can be substantially affected by early interventions. To the extent that their findings generalizes to our skill measures, our results indicate that disadvantaged children (who are likely to be unemployed or work in unskilled occupations) would also benefit more from improving their noncognitive than their cognitive ability. More generally, the genetic and cultural transmission of noncognitive ability could be an important channel for the intergenerational transmission of inequality (e.g., Bowles and Gintis 2002 and Björklund et al. 2006).

Another literature has investigated how cross-country differences in the distribution of

[48] There are a couple of papers which consider how cognitive ability relate to employment. Freeman and Schettkat (2001) find that the skill distribution on an adult literacy test is more compressed in Germany than in the US, but that this can only explain a small part of the gap in employment rate between these two countries. McIntosh and Vignoles (2001) find that numeracy is strongly related to employment in the United Kingdom (which has the same standard deviation of the PISA mathematics score as Sweden, see Nickell 2004).

cognitive ability relate to aggregated outcomes such as economic development (Hanushek and Woessman 2008) and income inequality (Nickell 2004). Due to the lack of measures of noncognitive ability which are consistent across countries, it is difficult to conduct similar studies for noncognitive ability. There is, however, no reason a priori to expect noncognitive ability not to be important also in this context.

7 Appendix A: Data

7.1 Construction of durations

We observe all the major transfers associated with absence from work. Those transfers are unemployment benefits, sick leave benefits and benefits during parental leave. It is however common that college educated workers have extra unemployment insurance for a limited period of time, which we do not observe. In following we abstract from those, assuming that they last for only a short period. The unemployment benefits from the government and the parental leave benefits are a function of earnings in the previous year while sick leave benefits are a function of the current wage rate (which we recalculate to the corresponding annual income). The replacement rates and ceilings that determine the size of the transfer are reported in the following table.

Benefit Policies (2006)

	Replacement rate	Ceiling (SEK)
Unemployment	80%	$240,900$
Sick leave	80%	$347,000$
Parental leave	69%	$347,000$

Note: The ceiling for sick leave and parental leave benefits was 297,000 until July 1 and SEK 397,000 after July 1. We use the average. The replacement rate for parental leave is variable decided by the parents. We set it to 6/7 of 80 %.

Based on the observed transfers in a given year and earnings in the previous year, the duration of an unemployment spell and the duration of leaves due to illness or parenthood is computed. In the case of sick leave the current wage rate is approximated by last year's income. Let variables denoted with stars ($*, **$) refer to last year's earnings truncated at each of the two ceilings reported in the table. A proxy for the duration of absence from work in 2006 is then calculated as follows:

$$\text{duration} = \frac{\text{unemployment benefits}}{0.8 \cdot \text{earnings}^{*}} + \frac{\text{sick leave benefits}}{0.8 \cdot \text{earnings}^{**}} + \frac{\text{parental leave benefits}}{0.69 \cdot \text{earnings}^{**}}$$

In case the computed duration exceeds one we set duration equal to one.

It is more difficult to infer durations for individuals with the previous year's earnings equal to zero, and we therefore treat these as missing observations when we use the imputed durations to impute wages or unemployment spells. As a robustness check in the analysis of unemployment spells, we also consider earnings prior to 2005 when we impute employment durations.

7.2 Imputation of wages

Based on the duration measure and reported earnings it is possible to impute wages for individuals with no observed wage rate in 2001-2006 as long as their earnings is observed. Note that the fraction of time worked in 2006 is given by (1− duration). Assuming that the individual works full-time the wage rate is:

$$w = \frac{\text{earnings}}{12 \cdot (1 - \text{duration})} \cdot 0.9385$$

where the last factor represents the average relation between the twelve times the wage rate and annual earnings in the sample.

7.3 Definition of parents in the wave of 1980

The oldest female in a household is defined as mother if she is at least 20 years old and if some other criteria are satisfied. Similarly, the oldest male may be defined is father if he is at least 20 years old and the remaining criteria are met. The remaing criteria concern civil status. If both a woman and a man satisfies the age criteria and both of them are married they are defined as mother and father, respectively. If only one of the two is reported as married or if one of the two is reported to be divorced then this person is defined as a parent and the other person is not defined as a parent. The household's income is defined as both parents' income if two parents are present, otherwise the household's income is defined as the mother's or the father's income.

7.4 Regional dummies

All municipalities in Stockholm county except for Norrtälje, Nykvarn, Nynäshamn and Södertälje are coded as belonging to greater Stockholm. Greater Gothenburg include the municipalities Göteborg, Kungälv, Stenungsund, Tjörn, Öckerö, Mölndal, Partille, Härryda, Lerum, Ale and Kungsbacka. Greater Malmö include the municipalities Malmö, Lund, Trelleborg, Vellinge, Kävlinge, Staffanstorp, Lomma, Svedala, and Burlöv.

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9 Interviews

An interview with the chief psychologist of the Swedish National Service Administration (Pliktverket), Johan Lothigus, in Karlstad Sweden conducted by Erik Lindqvist on August 25, 2004.

Table A1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Comment
Imputed wage 1 (W2)	14038	27978	12043	
Unemployment support	14703	0.092		
Any social assistance	14703	0.126		
Unemployment duration (if > 0)	1174	0.53		Set to zero in case of zero lagged earnings
Total wage income 2006	14703	319792	206140	
Cognitive skill (*c*)	14703	0.001	1.000	Normalized from 1-9 scores
Noncognitive skill (*n*)	14703	0.001	1.000	Normalized from 1-9 scores
Enlisted in the military	14703	0.900		
Geography: Gothenburg	14703	0.054		
Geography: Stockholm	14703	.089		
Geography: Malmo	14703	.201		
Geography: Southerns region (Götaland)	14703	.043		
Geography: Middle region (Svealand)	14703	.478		
Geography: Northern region (Norrland)	14703	.397		
Experience	13760	14.92	6.08	
Education: Primary school	14656	0.080		
Education: Secondary school	14656	0.556		
Education: Two years beyond secondary school	14656	0.094		
Education: University	14656	0.256		
Education: PhD	14656	0.013		
Family background: Household income in 1980	14673	1078	588	
Family background: Parents married in 1980	14673	0.791		
Grade point average in secondary school	11925	3.153	0.642	Grades are set on a 1-5 scale

Note: variables used only in Appendix B-F are reported in Table B1.

Table A2. Previous literature

Table A2 builds partly on Table 1 in Osborne-Groves (2005). We do not include studies that use multi-dimensional personality measures such as the "Big Five" in Table A2 (see Mueller and Plug 2006 for a recent test of how Big Five personality measures relate to labor market outcomes and Borghans et al. 2008b for a review of the literature on multi-dimensional personality measures and labor market outcomes). We also exclude studies which only focus on occupational status (e.g., Turner and Martinez 1977) or which use dichotomous measures of personality. Many of the papers in Table A2 report results from several different specifications. In this case, we report (when possible) results for white males with controls for educational attainment, family background and cognitive skill scores.

Study	**Dependent variable**	**Psychological variables**	**Type of measure**	**Normalized coefficients (b*σx)**	**Beta coefficients (b*σx/σy)**	**Data source**	**Country**	**Controls**[1]
Andrisani (1977)	Log wages 1971	External control (Rotter scale)	Survey	-0.072		NLS (young)	US	E
Andrisani (1977)	Log wages 1971	External control (Rotter scale)	Survey	-0.048		NLS (middle-age)	US	E
Andrisanni and Nestel (1976)	Log wages 1971	External control (Rotter scale)	Survey		-0.092	NLS (middle-age)	US	E
Duncan and Morgan (1981)	2- year change in hourly earnings	Personal efficacy[2]	Survey		-0.052	PSID	US	E
Duncan and Morgan (1981)	4-year change in hourly earnings	Personal efficacy	Survey		-0.165	PSID	US	E
Dunifon and Duncan (1998)	Log wages (average 1988-92)	Orientation towards challenge, personal control[3]	Survey	Challenge: 0.07 Personal control: 0.14		PSID	US	E, S, C
Edwards (1976)	Wage differences within work groups	Willingness to follow rules (Rules); Predictability and dependability (Dependability); Internalization of firm goals and values (Goals)	Peer-ratings		Rules: 0.14 Dependability: 0.10 Goals: 0.32	Government employees	US	E, S, C
Goldsmith et al. (1997)	Log wage 1980	Predicted self-esteem (Rosenberg)[4]	Survey	0.061	0.165	NLSY	US	E, S, C
Goldsmith et al. (1997)	Log wage 1987	Predicted self-esteem (Rosenberg)	Survey	0.078	0.149	NLSY	US	E, S, C

[1] E = educational attainment, S = socioeconomic background, C = cognitive ability.
[2] This measure is closely related to externality as measured by the Rotter scale.
[3] This measure is closely related to externality as measured by the Rotter scale.
[4] Self-esteem predicted with externality (Rotter scale).

Heckman et al (2006)	Log wage	Average of external control (Rotter) and Self-esteem (Rosenberg)	Survey	0.043		NLSY	US	E, C
Heckman et al (2006)	Log wage	Noncognitive ability	Structural model	0.112		NLSY	US	E[5]
Jencks (1979)	Hourly earnings.	Combination of measures[6]	Survey		0.245	Talent survey	US	E, S, C
Kuhn et al (2005)	Log wages	Leadership skills	Survey	0.037		Talent survey	US	E, S, C
Olnek (1979)	Log earnings	Cooperativness, Executive ability, Industriousness	Teacher assessment	Cooperativeness: -0.021 Executive ability: 0.081 Industriousness: -0.011		Kalamazoo	US	E, S, C
Murnane et al. (2001)	Log wage	Self-esteem (Rosenberg); Analytic speed (ASVAB)	Survey and test scores	Self-esteem 0.037 Analytic speed: 0.110	Self-esteem: 0.079 Analytical speed: 0.238	NLSY	US	C
Osborne Groves (2005)	Log hourly wages	External control (Rotter scale)	Survey	-0.055	-0.103	NLSYW	US	E, S, C
Osborne Groves (2005)	Log hourly wages	External control (Rotter) - instrumented	Survey	-0.067	-0.129	NLSYW	US	E, S, C
Osborne Groves (2005)	Log hourly wages	Agression, withdrawal	Teacher assessment	Aggression: -0.076 Withdrawal: -0.033	Aggression: -0.129 Withdrawal: -0.056	NCDS	GB	E, S, C
Segal (2008)	Log earnings	Motivation proxied by coding speed (ASVAB)	Test scores	0.064		NLSY	US	E, C
Segal (2009)	Log earnings	Misbehaviour[7]	Teacher assessment	-0.041		NELS	US	E, C

[5] Cognitive and noncognitive skills are orthogonal by construction.
[6] The combination of seven different variables related to self-assed traits and behaviors with maximum predictive power.
[7] Based on teacher assessments of 5 personal traits: absenteeism; disruptiveness; inattentiveness; tardiness and homework completion.

Table 1: Log wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cognitive skills	0.086***	0.050***	0.083***	0.049***	0.104***		0.087***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)		(0.003)
	0.265	*0.155*	*0.257*	*0.152*	*0.322*		
Noncognitive skills	0.066***	0.058***	0.086***	0.078***		0.092***	0.067***
	(0.003)	(0.003)	(0.004)	(0.004)		(0.003)	(0.003)
	0.204	*0.177*	*0.264*	*0.237*		*0.282*	
Cognitive skills sq.							0.014***
							(0.002)
Noncognitive skills sq.							0.001
							(0.002)
Noncognitive*Cognitive							0.014***
							(0.003)
Covariate set	Small	Large	Small	Large	Small	Small	Small
Measurement error correction	No	No	Yes	Yes	No	No	No
Observations	13974	13123	13974	13123	13974	13974	13974
R-squared	0.294	0.349	0.323	0.367	0.263	0.239	0.301

All regressions estimated using ordinary least squares. The dependent variable is log wage in 2006. Wage in 2006 has been imputed for 1,401 individuals using wage data from 2001-2005. Heteroskedasticity-robust standard errors are reported in parenthesis in columns (1)-(2) and (5)-(7). Standard errors in column (3)-(4) computed with bootstrap (50 replications). Three stars denote statistical significance at the one percent level, two stars at the five percent level and one star at the ten percent level in a two-sided test. Beta coefficients are reported in italics. All regressions include a constant, cohort dummies, an enlistment dummy, household income in 1980, a dummy for whether parents were married in 1980, six dummy variables for region of residence, and a dummy variable for no educational attainment above primary school (the "small" set of covariates). The regression in column (2) also includes a quadratic in potential post-education experience and dummy variables for secondary school, two years post-secondary schooling, university degree and a PhD (the "large" set of covariates). The measurement error correction in column (3) and (4) is based on a reliability ratio of .8675 for cognitive ability and .70267 for noncognitive ability. We adjust the coefficients in column (3) and (4) for the larger skill measure variance implied by measurement error.

Table 2: Extended wage regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cognitive skills	0.023***	0.089***	0.021***	0.052***	0.039***	0.056***	0.014***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.014)	(0.005)	(0.003)
Noncognitive skills	0.052***	0.066***	0.050***	0.058***	0.048***	0.046***	0.027***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.013)	(0.004)	(0.003)
Covariate set	Small	Small	Large	Large	Large	Large	Large
Observations	11480	11480	10915	10915	978	4962	5634
R-squared	0.353	0.279	0.370	0.333	0.341	0.267	0.064
Additional controls	GPA*	-	GPA*	-	-	-	-
Sample restrictions	-	GPA* observed		GPA* observed	Managers	Highskilled workers	Lowskilled workers
Sample mean of cognitive skills					.43	-.44	.50
Sample mean of noncognitive skills					.55	-.27	.32

GPA* denotes grade point average in secondary school interacted with dummy variables for type of educational track. See legend in Table 1 for further information

Table 3: Unemployment and earnings

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Exponential hazard	(6) OLS	(7) OLS	(8) OLS	(9) OLS
Dependent variable	Unemployment support	Unemployment support	Unemployment support	Any social assistance	Unemployment duration	Unemployment duration	Annual earnings	Annual earnings	Annual earnings
Cognitive skills	-0.015***	-0.022***		-0.023***	0.012	-0.003	32,791***	43,392***	
	(0.003)	(0.003)		(0.003)	(0.039)	(0.012)	(1,751)	(1,649)	
Noncognitive skills	-0.024***		-0.028***	-0.042***	0.130***	-0.037***	37,148***		46,999***
	(0.003)		(0.003)	(0.003)	(0.038)	(0.012)	(1,947)		(1,835)
Observations	14626	14626	14626	14626	1173	1173	14626	14626	14626
R-squared	0.030	0.025	0.028	0.060	-	0.023	0.168	0.144	0.148

All regressions estimated using the small set of covariates. The results in column (5) regards non-exponentiated coefficients (not hazard ratios). Annual earnings is denoted in SEK. See legend in Table 1 for further information.

1 Appendix B: Sample selection

This Appendix analyzes several issues related to sample selection. Table B1 gives the summary statistics for the additional variables used in Appendix B-F.

1.1 Missing wages

As already discussed in Section 2 in the paper, we do not observe wages for the entire sample. One reason is that employers do not report them to Statistics Sweden. If such workers are not systematically different from workers with observable wages in terms of the relationship between skills and wages, this will not bias our estimates. A more serious problem is that we do not observe wages for men who do not work. Though there is no minimum wage law in Sweden, the effective minimum wage is relatively high due to the strong influence of trade unions and the extensive welfare system. This implies that men with low productivity or a strong preference for leisure may be selected out of the labor market. Following Gronau (1974), suppose men select into the labor market in case the offered wage (w_i) exceeds the reservation wage (w_i^r), which is given by

$$\log w_i^r = \beta_{c2} c_i + \beta_{n2} n_i + \mathbf{X}_i \boldsymbol{\gamma}_2 + \varepsilon_{i2}.$$

Hence, we observe wages if and only if

$$\log w_i - \log w_i^r = \beta'_c c_i + \beta'_n n_i + \mathbf{X}_i \boldsymbol{\gamma}' + u_i > 0 \tag{1}$$

where $\beta'_c \equiv \beta_c - \beta_{c2}$, $\beta'_n \equiv \beta_n - \beta_{n2}$, $\boldsymbol{\gamma}' \equiv \boldsymbol{\gamma} - \boldsymbol{\gamma}_2$ and $u_i \equiv \varepsilon_i - \varepsilon_{i2}$. Let $I = 1$ denote the case when $w_i \geq w_i^r$ and $I = 0$ the case when $w_i < w_i^r$. A selection bias occurs in case u_i is correlated with ε_i. We use four different methods to deal with this potential problem.

Our first approach is to test if our results are sensitive to whether imputed wages are included or not. The results reported in the text considered the case when wages in 2006 were imputed from observed wages in the 2001-2005 period. Here, we consider the case when we exclude all imputed wages and when an imputed wage is added also for some of the men whose wage is unobservable in the entire 2001-2006 period. Using records on social benefits (unemployment benefits, pensions, sick leave and parental leave), we construct a measure on the number of months in employment in 2006. We then divide total labor income in 2006 with this number to get an imputed monthly wage.[1] We code this wage as missing in case it falls short of 12,000 SEK. Using wages imputed this way increases the number of observations by 175, bringing the total number to 14,213, or 96.7 % of our sample. The table below summarizes the different wage measures used in the paper.

Measure	Method	N
W1	Wages observed in 2006	12,570
W2	W1 + imputed from observed wages in 2001-2005 if W1 is missing	14,038
W3	W2 + imputed from annual earnings and social benefits in 2005-2006 if W2 missing	14,213

Our second approach is median regression. The advantage of median regression over OLS is that the results are only affected by the position of the imputed wage with respect to the conditional median.[2] Hence, the results from median regression are not sensitive to the exact value of imputed wages. If log wages are linear in c and n, median regression identifies the same parameter as OLS. To assign men with missing wages an imputed wage on the right side of the conditional median, we calculate the predicted values from a median regression of the logarithm of annual earnings on $(c_i, n_i, \mathbf{X}_i)$. Let w_i denote the wage from either of our three wage measures described in Section 2 and K_i denote an indicator variable equal to one in case actual earnings exceeds predicted earnings and equal to zero in case actual earnings falls short of predicted earnings. For each wage measue, we then create a new variable $y_i = w_i$ if $I_i = 1$, $y_i = 0$ if $I_i = 0$ and $K_i = 0$ and $y_i = 10^8$ if $I_i = 0$ and $K_i = 1$.[3] In other words, we assign men with missing wages and an annual earnings below the conditional median a wage below the conditional median, and men with missing wages but an annual earnings above the conditional median a wage above the conditional median.

[1] We multiply the imputed wage by a factor .9385 since the yearly labor market income implied by reported monthly wages only constitute 93.85 % of actual income as reported in tax records. The likely reason for this discrepancy is that some men work more than full-time.

[2] Bloomfield and Steiger (1983) provide the mathematical details for this result. Other papers that have used median regression to control for selection bias are Neal and Johnson (1996), Neal (2004) and Olivetti and Petrongolo (2008).

[3] The only reason we choose such a high value as 10^8 is to be certain that these wages are indeed above the conditional median.

Our third approach is to employ the two-step procedure proposed by Heckman (1976, 1979). As we do not have a valid instrument for selection into the labor market, we rely on the nonlinearity of the inverse Mill ratio to identify $(\beta_c, \beta_n, \boldsymbol{\gamma})$. Leung and Yu (1996) argue that the Heckman two-step estimator is effective even in the absence of an exclusion restriction in the selection equation, provided that at least one of the variables in the vector of covariates has enough variation to induce tail behavior in the inverse Mills ratio. As we will see, noncognitive ability is a strong predictor of participation in the labor market, suggesting that the Heckman two-step procedure could actually work for our purposes. Still, we view the results from Heckman two-step as a robustness check, not as our favoured specification.

We use as our fourth approach a simple variant of "identification at infinity" (Chamberlain, 1986; Heckman, 1990). The idea behind this identification strategy is to restrict the sample to a group of workers for whom the choice to select into the labor market is not affected by unobservable productivity (ε_i). To this end, we first run a probit regression of an indicator variable of observable wages on $(c_i, n_i, \mathbf{X}_i)$ and then run regression (1) for men whose covariates imply a high predicted probability of nonmissing wages. A drawback with this method is that inferences may not be valid for the entire sample.

Table B2 reports the results from different approaches to control for selection bias. These results are not adjusted for measurement error and should thus be compared to the standard OLS estimates of column (1) and (2) in Table 1. We first show that the results are very similar for the two other wage measures described above. We then consider the different methodological approaches outlined above. First, the results from median regressions is displayed in column (3).[4] Second, we employ a simple variant of "identification at infinity" by first running a probit regression of the probability of observed wages and then restricting the sample to men whose covariate values predicts this probability to be above 85 percent. Both quantile regression and "identification at infinity" give results close to the OLS estimates. Finally, column (5) gives the results from a Heckman two-step estimator. Even though these estimates are not corrected for measurement error, we find that the estimated effect of noncognitive ability is similar to the effect of cognitive ability already in the specification with the small set of covariates. However, as we do not have a credible exclusion restriction in the selection equation, these estimates should be

[4]The results for median regression, Identification-at-infinity and Heckman two-step for the two sample with directly observable wages and the second measure of imputed wages are reported in Table A2.

interpreted with caution.

1.2 Alternative samples and selection into the military service

The main analysis in the paper is conducted on the core members in LINDA (which is representative of the Swedish population) with a number of restrictions (self-employed, students and workers in the agricultural sector are excluded). Table B3 gives summary statistics for those who excluded from the core sample and for family members in LINDA. Table B4 gives summary statistics for men (in the restricted core sample) depending on enlistment status. Table B5 gives results for wages and earnings for alternative samples. Table B6 presents regressions for enlistment into the military service and attrition (i.e., those who were enlisted but did not serve). The low number of observations in column (1) of Table B6 is due to missing information on health status. Figure B1 is a histogram over the age at the onset of the military service. We use data on the cash transfers conscripts receive while in the military to derive the age at the onset of the military service. This information allows us to observe which year the military service starts, but not the exact date, implying that there is some uncertainty as to the exact age.

2 Appendix C: Measurement error

There are several reasons to expect both our measures of cognitive and noncognitive skill to be measured with error. For example, motivation for the military service is likely to affect performance on the test of cognitive skill and in the enlistment interview. The score of noncognitive ability is also subject to a particular form of measurement error since psychologists vary in their assessment of identical conscripts. Lilieblad and Ståhlberg (1977) estimated the correlation between the SNSA psychologists' assessment of noncognitive skills to be .85 after letting thirty SNSA psychologists listen to tape recordings of thirty enlistment interviews.[5] In a bivariate regression, classical measurement error leads to a downward bias of the estimated strength of the relationship between two variables. This is not necessarily the case in a multivariate context. Since our skill measures are

[5] Since all psychologist listen to the same interviews, this correlation is not an exact measure of the true correlation between psychologists' assessment. The fact that psychologists make their own interviews could, in theory, both imply that the true correlation is higher or lower than .85.

positively correlated (.388), classical measurement error in one skill measure will imply an upward bias of the estimated effect of the other skill measure.

Assuming classical measurement error, our measure of cognitive skills, c, is a function both of actual skills (denoted by c^*), and of a random error term, v_c. That is,

$$c = c^* + v_c$$

where $v_c \sim N\left(0, \sigma^2_{v_c}\right)$ and $Cov\left(c^*, v_c\right) = 0$.[6] We make the same assumptions regarding measurement error in noncognitive ability. Similar to Heckman et al. (2006), we thus view the measured level of cognitive and noncognitive ability as reflecting both true ability and measurement error.[7] However, note that the "true" ability in this context refers to the cognitive and noncognitive abilities valued by the Swedish military. These abilities may not perfectly coincide with the abilities sought after by employers in the civilian labor market.

We use data from a sample of twins to calculate the reliability ratio of each skill measure. Here, we illustrate this method (which is similar to Griliches 1979) in the case of cognitive skills but the argument is the same in the case of noncognitive skill. Assuming that the correlation in v within twin pairs is zero, the correlation within MZ twins for c is

$$\rho_{MZ} = \frac{\sigma_{c_1 c_2}}{\sigma^2_c} = \frac{\sigma_{c^*_1 c^*_2}}{\sigma^2_{c^*} + \sigma^2_v}$$

where $\sigma_{c^*_1 c^*_2}$ is the within-twin pair covariance in c^*. Without loss of generality, we can normalize the variance in c to one, implying that

$$\sigma^2_{c^*} + \sigma^2_v = 1$$

and

$$\rho_{MZ} = \sigma_{c^*_1 c^*_2}.$$

[6] We further assume that all cross-moments between the true variables and the measurement errors are zero.

[7] Heckman et al. (2006) use a model with latent factor structure to adjust for measurement error. Our approach is different, as outlined below.

Now consider the within-twin difference in observed cognitive skill

$$\begin{aligned}\Delta c &= \Delta c^* + \Delta v. \\ &= c_1^* - c_2^* + v_1 - v_2\end{aligned}$$

Since $\sigma_{v_1 v_2} = 0$ by assumption, the variance in Δc is

$$2\sigma_{c^*}^2 - 2\sigma_{c_1^* c_2^*} + 2\sigma_v^2,$$

implying that the reliability ratio for cognitive skills within MZ twin pairs is

$$\frac{\sigma_{c^*}^2 - \sigma_{c_1^* c_2^*}}{\sigma_{c^*}^2 - \sigma_{c_1^* c_2^*} + \sigma_v^2}$$

which can also be expressed as

$$\begin{aligned}&\frac{\sigma_{c^*}^2 - \rho_{MZ}}{\sigma_{c^*}^2 + \sigma_v^2 - \rho_{MZ}} \\ = &\frac{\sigma_{c^*}^2 - \rho_{MZ}}{1 - \rho_{MZ}}.\end{aligned}$$

Now consider the regression

$$\Delta y_{MZ} = \beta_{MZ} \Delta c_{MZ}^* + \varepsilon,$$

where Δy_{MZ} is the difference in some outcome (in our case annual earnings) within monozygotic (identical) twin pairs, and Δc_{MZ}^* the corresponding difference in true cognitive skill. By regressing Δy_{MZ} on Δc_{MZ} (the observed within twin-pair difference in cognitive skill), we obtain an estimate of β_{MZ}[8]

$$\widetilde{\beta}_{MZ} = \left(\frac{\sigma_{c^*}^2 - \rho_{MZ}}{1 - \rho_{MZ}} \right) \beta_{MZ}.$$

[8]Note that the parameter β_{MZ} does not have a causal interpretation as differences between twins in cognitive ability are likely to be correlated with other factors that enhance earnings.

Rearranging this expression gives

$$\sigma_{c^*}^2 = (1 - \rho_{MZ}) \frac{\widetilde{\beta}_{MZ}}{\beta_{MZ}} + \rho_{MZ}$$

or, equivalently

$$\frac{\sigma_{c^*}^2}{\sigma_{c^*}^2 + \sigma_v^2} = (1 - \rho_{MZ}) \frac{\widetilde{\beta}_{MZ}}{\beta_{MZ}} + \rho_{MZ}.$$

We can thus express the reliability ratio for cognitive ability as the sum of two terms: the correlation in observed test scores between MZ twins (ρ_{MZ}) plus one minus this correlation multiplied by a factor $\left(\widetilde{\beta}_{MZ}/\beta_{MZ}\right)$. The ratio $\widetilde{\beta}_{MZ}/\beta_{MZ}$ denotes the share of the within-twin variance in measured cognitive skill that reflect true differences in skill. We observe ρ_{MZ} and $\widetilde{\beta}_{MZ}$ but not β_{MZ}. To get an estimate of β_{MZ}, note that we get an analogous expression for dizygotic (fraternal) twins. That is, the estimates from the regression

$$\Delta y_{DZ} = \beta_{DZ} \Delta c_{DZ}^* + \varepsilon', \tag{2}$$

together with the within-twin correlation give the reliability ratio

$$\frac{\sigma_{c^*}^2}{\sigma_{c^*}^2 + \sigma_v^2} = \frac{\widetilde{\beta}_{DZ}}{\beta_{DZ}} (1 - \rho_{DZ}) + \rho_{DZ}. \tag{3}$$

Consequently,

$$\frac{\widetilde{\beta}_{MZ}}{\beta_{MZ}} (1 - \rho_{MZ}) + \rho_{MZ} = \frac{\widetilde{\beta}_{DZ}}{\beta_{DZ}} (1 - \rho_{DZ}) + \rho_{DZ}.$$

Now, assuming that the true effects are the same for DZ and MZ twins, i.e., $\beta_{MZ} = \beta_{DZ}$, we get

$$\beta_{DZ} = \beta_{MZ} = \frac{\widetilde{\beta}_{DZ} (1 - \rho_{DZ}) - \widetilde{\beta}_{MZ} (1 - \rho_{MZ})}{\rho_{MZ} - \rho_{DZ}}.$$

Let $\beta_{DZ} = \beta_{MZ}$ be denoted by β. Once we have obtained β, the reliability ratio is identified.

Estimates of the parameters $\left(\widetilde{\beta}_{MZ}, \rho_{MZ}, \widetilde{\beta}_{DZ}, \rho_{DZ}\right)$ have been provided to us by David Cesarini based on a sample from the Swedish Twin Registry restricted to the cohorts which are relevant in our case.[9] This sample covers 701 twin pairs with data on annual earnings

[9] The parameters are estimated by OLS regressions with annual earnings as the dependent variable.

and the enlistment skill measures.[10] From an OLS on annual earnings, we get for the noncognitive measure

$$\begin{aligned}\beta &= \frac{36569\,(1-0.5217) - 2338\,(1-0.6953)}{0.6953-0.5217} \\ &= 96651\end{aligned}$$

This gives us an reliability ratio for noncognitive skills which is

$$\frac{\widetilde{\beta}_{MZ}}{\beta}\,(1-\rho_{MZ}) + \rho_{MZ} = \frac{2338}{96651}\,(1-0.6953) + 0.6953 = 0.70267$$

Using the same formula and corresponding data for cognitive skills, we get

$$\begin{aligned}\beta &= \frac{14829\,(1-0.5027) - 6796\,(1-0.8004)}{0.8004-0.5027} \\ &= 20215\end{aligned}$$

We then get the reliability ratio

$$\begin{aligned}\frac{\widetilde{\beta}_{MZ}}{\beta}\,(1-\rho_{MZ}) + \rho_{MZ} &= \frac{6796}{20215}\,(1-0.8004) + 0.8004 \\ &= 0.8675\end{aligned}$$

Note that the lower reliability ratio for noncognitive ability is consistent with the lack of perfect congruence between the assessment of different psychologists. Using the estimated reliability ratios and assuming zero covariance between measurement errors, it is straightforward to adjust for measurement error.[11]

It should be noted, however, that our derivation of reliability ratios and measurement error correction relies on a number of quite strong assumptions. If measurement errors are positively correlated between twins (which seems likely) we would overestimate the

The results are very similar when we instead consider the log of annual earnings as the dependent variable.

[10]The twins in the Swedish Twin Registry data (both monozygotic and dizygotic) are somewhat positively selected in terms if cognitive and noncognitive ability compared to our sample (about .25 standard deviations for each measure). About 3 % of the twins from the Swedish Twin Registry can be presumed to be present in our data.

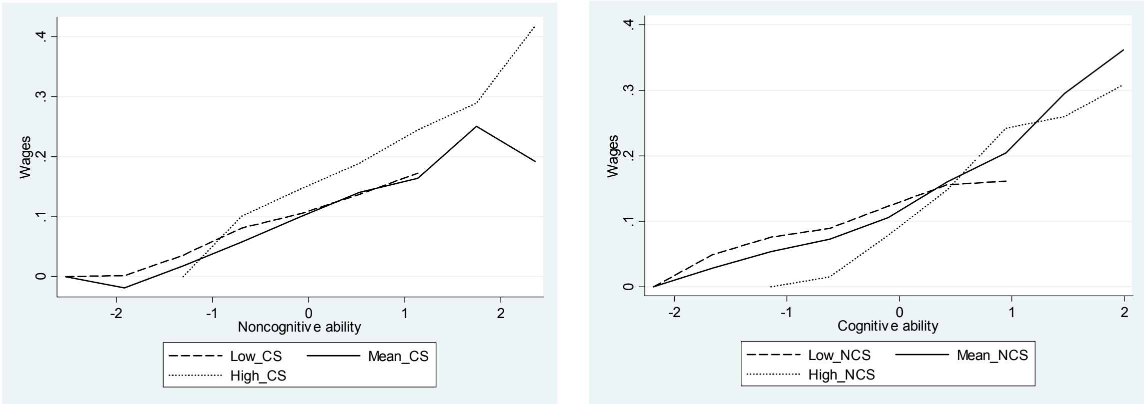
[11]We adjust for measurement error using Stata's [eivreg] command. For a textbook treatment of this method, see Kmenta (1997) or Draper and Smith (1998).

reliability ratios, creating a bias toward zero in the estimated effect of cognitive and noncognitive skills. Another important assumption is that there is no correlation between measurement errors in cognitive and noncognitive skills for a given conscript. Though we cannot test this assumptions, it seems plausible that there is some positive correlation in measurement errors. If this is indeed the case, we would again underestimate the effect of cognitive and noncognitive skills on outcomes.

3 Appendix D: Additional results

This Appendix presents various additional results. First, we present results from a nonparametric estimation of wages that shows the positive interaction effect between cognitive and noncognitive ability. We let each unique value of cognitive and noncognitive ability be represented by a dummy variable and estimate the effect of each skill measure on log wages while fixing the opposite skill measure at low (1-3 on the 1-9 scale), medium (4-6) or high values (7-9). We exclude skill combinations for which there are fewer than 100 observations and normalize the effect to zero for the lowest skill value which is included in the regression. As shown in Figure Figure D1, the return to noncognitive ability is higher for men with high cognitive ability than for men with low or average values of cognitive ability. Correspondingly, Figure D2 shows that the effect of cognitive ability is larger for men with high noncognitive ability.

FIGURE D1 AND D2. ABILITY AND LOG WAGES



Samples restricted to "low" (1-3), "mean" (4-6) or "high" (7-9) values of the corresponding skill measure. All regressions include fixed effects for each value of cognitive and noncognitive skill included in the specific sample and the "small" set of covariates as defined in Table 1. Sample restricted to skill combinations with at least 100 observations. Effect normalized to zero for the lowest value included in the regression.

Table D1 gives the for unemployment and earnings when controlling for the large set of covariates or adjusting for measurement error. Table D2 and D3 give the point estimates underlying Figure 2 and 3. In the figures, we transform the estimates by dividing by annual earnings at each percentile. We use the Stata program *rifreg* which is downloadable from Nicole M. Fortin's homepage[12] to estimate the unconditional quantiles.

Table D4 presents robustness tests for potential omitted variables.

First, we control for type of military service in column (1). The argument for including type of military service as a control variable is that test scores have a direct effect on the type of military training to which conscripts are enlisted. To the extent that the type of military service affects future wages, not including type of military service as a covariate in $\mathbf{X}_i$ will imply undercontrolling. However, though the military service itself is mandatory, conscripts have some freedom to affect the position they are assigned to, provided that they fulfill the specific requirements for this position. Hence, the fact that a conscript with a high score on noncognitive and cognitive skill was not enlisted into a leadership

[12] http://www.econ.ubc.ca/nfortin/hmpgfort.htm

position is a signal of an unwillingness to assume responsibility. Consequently, controlling for type of position implies that the identifying variation in our ability measures is in fact correlated with an aspect of personality that can be presumed to have a *negative* effect on outcomes, creating a bias toward zero. For this reason, we have chosen only to include a dummy for enlistment into the military service in the basic specifications. As shown in Table D4, controlling for type of military service decreases the estimated effects of noncognitive and cognitive ability as one would expect. In contrast, the effect of cognitive and noncognitive ability on unemployment is strengthened somewhat when controlling for type of military service (results available upon request).

Another potential concern is that the measure of noncognitive skills functions as a proxy for health status, which might have an independent effect on outcomes. There is, indeed, a positive correlation between noncognitive skill and health status classification at the enlistment in our data. In comparison, the correlation between cognitive skill and health status is much weaker.[13] However, as shown in columns (2) and (3), controlling for health status does not change the results appreciably once we restrict the sample to men for whom health status is observed. As evidence of psychopathology give conscripts a low health ranking (in addition to a low rating on noncognitive ability), the fact that our results are insensitive to controlling for health status also ensures that they are not driven by a few men with mental health problems.

Finally, we show in Table D4 that our results for wages remain essentially the same when controlling for age at draft (column 4).

4 Appendix E: Occupational choice

We use the information on occupation LINDA contains information on occupation to assign workers into three broad occupation groups (SSYK 96): managers, highskilled workers and lowskilled workers. We use the ten broadest occupational categories in the data, numbered from 0-9. We exclude men in group 0 (military work) and define group 1 (managerial work) as "managers". Group 2-9 has a qualification level attached to them (group 0 and group 1 are not assigned a qualification level), and we use this to classify workers as "highskilled" or "lowskilled". The qualification level goes from 1 (lowest) to

[13] A regression of noncognitive ability on the full set of dummy variables for health status classifications has an R^2 of .2361 compared to .0496 for cognitive ability.

4 (highest). We define workers in group 2 (qualification level 4) and group 3 (qualification level 3) as "highskilled", while workers in group 4-8 (qualification level 2) and 9 (qualification level 1) are defined as "lowskilled".

Table E1 gives the marginal effects from a multinomial logit of occupational choice on $(c_i, n_i, \mathbf{X}_i)$. In the specification with the small set of control variables, high cognitive skills is a strong predictor of selection into a skilled occupation, while men with low cognitive skills are more likely to select into a lowskilled occupation. This pattern remains the same when education and experience are controlled for, though it is less pronounced. Men with high noncognitive skills are more likely to become managers or work in skilled occupations than workers with low noncognitive skills. The predictive power of noncognitive skills on occupational choice is insensitive to controlling for educational attainment.

We now turn to an estimation of the returns to skills across occupational groups. The key econometric problem in this estimation is that we only observe the wage in a given occupation for men who have selected into this occupation. For example, we do not observe the wage that managers would earn as skilled workers, and vice versa. If unobserved factors that influence occupational choice are also correlated with productivity in different occupations, then self-selection may bias our estimated skill prices.

More formally, we want to estimate the model

$$\log w_{ij} = \beta_{c,j} c_i + \beta_{n,j} n_i + \mathbf{X}_i \boldsymbol{\gamma}_j + \varepsilon_{ij}$$

where $j = \{\text{manager, skilled, unskilled}\}$. The econometric problem is that w_{ij} is only observed in case person i chooses occupation j. Let

$$w^*_{ij} = \mathbf{z}_i \boldsymbol{\delta}_j + \eta_{ij}$$

denote the utility individual i attaches to working in occupation j. Each individual chooses the occupation that maximizes his utility. For example, we only observe wages in unskilled occupations in case

$$w^*_{\text{unskilled}} > \max_{j \neq \text{unskilled}} \left(w^*_j \right).$$

McFadden (1973) showed that the model above leads to the multinomial logit model in case the error terms in the choice equations are independent and identically Gumbel distributed. Lee (1983) proposed a procedure to correct for selection bias in the multinomial case which is essentally an application of the Heckman (1979) selection model. Bourguignon et al. (2007) argues that the Lee (1983) procedure imposes strong assumption on the covariances between the error terms in the selection and the outcome equations. Instead, they propose an alternative estimator based on Dubin-McFadden (1984) but which allows for more general distributions for ε_{ij}, in particular the normal distribution. We consider both of these estimators.[14]

We use as instruments in the selection equation region of residence in 1980 and dummy variables for whether mother and father worked in a white-collar occupation in 1980. As there is no direct information on occupation available in the 1980 wave of LINDA, we use industry code (SNI69) of occupation as a proxy for occupation. This code is very detailed (five-digits), but we use the first two digits which indicate industry in a broader sense. We classify parents working in postal services and telecommunications; banking and finance; insurance; administration and consulting; public administration; education and culture as "white collar" and parents working in forestry; fishing; mining; ready-made clothing; pulp; chemical industries; other types manufacturing; energy; construction; retail; tourism; transportation; water and sanitation and repair services as "blue-collar".

Our identifying assumptions are thus that, controlling for parental income and $(c_i, n_i, \mathbf{X}_i)$, parents' occupational status and region of residence in 1980 will affect occupational choices only through preferences for different types of jobs. For example, children whose parents worked in a white-collar job could have a higher utility in white-collar jobs, but are not more productive once we include our full set of covariates and measures of cognitive and noncognitive skills. Since we do not observe occupational choice for all the men in the data, we include a fourth category of "no data on occupation" in the selection equation.

The estimated occupation-specific skill prices are displayed in Table E2. In general, the estimated skill prices are consistent with the more pronounced convexity in the return to cognitive skills and the selection patterns documented in Section 4. Noncognitive skill has a higher return than cognitive skill for managers and workers in unskilled occupations while workers in skilled occupations have a return to cognitive skill similar to the return to noncognitive skill.

[14] All estimations based on multinomial logit are conducted with the Stata *selmlog* command developed by Bourguignon et al. (2007).

5 Appendix F: Skill measures

This section provides additional material regarding our skill measures. We first present results for wages when using the sum of subscores instead of the normalized final (1-9) scores and when using each subscore separately. We run the basic wage regression with residualized ability measures. We then test whether psychologists are influenced by their knowledge of the conscript's cognitive test score. The second subsection gives the definitions of the cognitive skill subtest. Finally, we summarize an old manual for the enlistment interview. This manual regards the assessment of leadership ability which is different from the measure of general military aptitude we use as our noncognitive ability measure.

5.1 Additional analysis

As an alternative measure of cognitive ability, we percentile rank-transform the sum of subscores. We then convert it by taking the inverse of the standard normal distribution to produce normally distributed test scores. We present the results from wage regressions using these measure is Table F1. The results do not differ appreciably from those in Table 1.

We now consider each subscore separately. Table F2 shows that the pairwise correlations between our cognitive subscores range from 0.50 to 0.68. Table F3 shows the results from a regression of log wages on each cognitive ability subscore and the small set of covariates. The effect sizes are similar for all measures except mental rotation (a test of spatial ability) which is somewhat less strongly correlated with wages than the other measures. Including all measures in a single regression (column 5) leads only to a modest increase in variance explained. Table F4 presents the correlations between the noncognitive ability subscores. The correlations vary between 0.46 and 0.63. The results in wage regressions with each noncognitive subscore are presented in Table F5. Except for the second measure, which has a weaker association with wages, the estimated effects of these measures are very similar. Including all subscores jointly has only a modest effect on variance explained.

Table F6 presents results for the basic wage regression when using residualized ability measures. The first column presents the case when we use the residuals from a regression of noncognitive ability on cognitive ability as our measure of noncognitive ability. The point estimate of this measure is identical to the point estimate in column (1) of Table 1. The results for the non-residualized measure of cognitive ability is somewhat stronger than the corresponding result in Table 1 (column 5). Similarly, using a residualized measure for cognitive ability gives a somewhat stronger effect for noncognitive ability than just dropping cognitive ability from the regression (column 6, Table 1).

Finally, we use the entire draft data set for 1983-1993 to estimate the effect of the cognitive score on the psychologists' assessment of noncognitive ability. To control for underlying intelligence, we interact a fifth-order polynomial in the sum of subscores (4-36) with enlistment year. Our results, presented in Table F7, indicate that an increase by one step on the 1-9 cognitive test score scale increases the assessed level of noncognitive ability by 0.109 on the 1-9 scale, implying that the covariance between cognitive and noncognitive should in part be attributed to an effect of cognitive skills on estimated noncognitive ability. However, simulations show that this effect alone can only account for a small part of the total covariance: The effect of the cognitive score on the noncognitive would give a correlation of about 0.11 if it were the only source of covariance.

5.2 Definition of cognitive skill subtests

Definitions of the cognitive skill subtests are provided by Carlstedt and Mårdberg (1993, p. 355): "The Enlistment Battery is a paper and pencil test. The answers are marked on separate optical answering sheets. Each test consists of 40 items presented in increasing order of difficulty. The tests are slightly speeded.

The Enlistment Battery contains the following four tests:

Instructions (Test A). The first test of the battery, Instructions, contains verbally formulated instructions to make markings on an answering sheet that fulfill the conditions given by the instructions. 'The main principle for solving the item tasks is that it should be possible to solve them from the information given for each item. Difficulty is varied by the complexity of the instructions and by distractive negations or conditional clauses. In some of the items simple numerical operations are parts of the logical sequences. The test is meant to measure the combined ability of problem solving, induction capacity, and numerical ability' (Ståhlberg Carlstedt & Skold, 1981, p. 5). Thus, this test was designed to measure the primary factor Induction.

Synomyms (Test B). For each item of this test a target word is presented and the correct synonym should be chosen among four alternatives. The test is meant to measure the primary factor Verbal Comprehension.

Metal folding (Test C). Each task of this test is to find one three-dimensional object out of four that corresponds to a two-dimensional drawing of an unfolded piece of metal. The test was designed to measure the primary factor Spatial Ability.

Technical Comprehension (Test D). The items of this test all constitute illustrated technical and physical problems. One out of three solutions should be marked as the proper one. The test was constructed to measure a Technical Comprehension factor."

5.3 The interview procedure in the 1960's

Before 1969, the objective of the enlistment interview was to assess leadership ability for conscripts with high cognitive ability. The guidelines for this interview are no longer confidential (Militarpsykologiska institutet, 1956; Militarpsykologiska institutet, 1964). We provide a description of these guidelines below, which we hope will provide some context to the enlistment interview.

However, we want to emphasize that these guidelines should not be taken as a literal description of the assessment of "general military aptitude", which we use as our measure of noncognitive ability. The subscores underlying the leadership score are different from those underlying the score on military aptitude. Another key difference is that while the psychologists are instructed to take cognitive ability explicitly into account when assessing leadership ability, this is not the case for military aptitude.

The reason we choose the general aptitude score as our noncognitive measure, rather than the leadership measure, is that it is available for the entire sample and has a lower correlation with cognitive ability. We have, however, run our main regressions also for leadership ability and found that the results are almost exactly the same as for military aptitude.

5.3.1 Purpose and overall setting

The guidelines from the 1950's and 1960's state that approximately 60% of the draftees should be evaluated with respect to the ability to assume leadership in the military, which at the time was the primary purpose of the interview. The guidelines do however indicate that a second purpose is to judge whether a draftee is at all suitable for military training and a position in the military. Reasons for being unsuitable could for instance be lack of cognitive ability or signs of emotional instability.

The guidelines ask the interviewer to use on average 15 minutes per interview. The interview should be conducted in a separate room. As background information, the interviewer has school grades, general information about civil status, job experience, results from a questionnaire with open-ended answers, results from a multiple-choice questionnaire and the result from the test of cognitive skill.

5.3.2 The interviewer's role and communication style

The interviewer is reminded that the draftee may want to manipulate the outcome of the interview. To limit the effect of such attempts, the interviewer is asked to ignore the draftee's motivation for armed service per se and focus on the relevant personal factors. The guidelines do however state that motivation has been shown to correlate with fundamentally relevant factors. The interviewer should use a civilian tone and manner and is asked to convey the impression that one purpose of the interview is to be helpful rather than to screen draftees in terms of skills. For instance, advice on school choices or seemingly suitable civilian career paths may be given to draftee. Although time is short, the interviewer should not abruptly interrupt the draftee. Further, the guidelines states that the interview should be kept as a conversation with the use of a neutral language, without the use of leading questions or condemning language. The interviewer should avoid to advise the draftee on specific suitable military positions.

5.3.3 Factors for the evaluation

Five areas of the draftee's personal life must be covered during the interview. The interviewer grades the draftee on a 1-5 scale on each factor. On average, cognitive skill is expected to correlate with these scores, in particular at the lower end of cognitive skill distribution. The first topic is the draftee's experience from school. Aside from the academic achievements of the draftee, it is of interest to investigate how the draftee adapted to the school environment and his own perception of his school experience. Examples of noteworthy behavior are school quits, drop-outs or whether some classes were repeated. The second topic is work experience. Of main interest is the draftee's ability to function in the workplace. Examples of noteworthy events are repeated conflicts with managers or co-workers, whether the draftee had been fired or quit abruptly, etc. If the draftee has limited or no work experience, the interviewer should base his judgment on the draftee's career plans - whether such exist, whether they seem realistic, etc.

The third topic is how the draftee spends his leisure time. It is of interest whether the draftee is active and passionate about a hobby and to which extent the draftee has shown sustained interest in certain activities. Further, it is of interest whether the activities show tendency for extroversion or introversion, whether he participates in team sports, whether he seems to have rich interests and if there are signs that the draftee is able to adopt to given circumstances. Of particular interest are leadership roles. A mandatory question when covering this topic is 'What do you think of your ability to lead a group of peers?'.

The fourth topic is home environment and upbringing. Of particular interest are contacts with parents and siblings. Rather than judging the home conditions objectively, the interviewer is instructed to make notes of the draftee's subjective experience of the conditions, to investigate how the draftee may have adopted to difficult circumstances and to which extent the draftee is excessively dependent of the parents. A mandatory question is 'How often do you use alcohol?'. The fifth topic of the interview is emotional stability. Apart from issues that may have been raised during the conversation, the interviewer is asked to base the conversation on answers from the multiple-choice questionnaire if these answers indicate negative behavior. Overall, the draftee's maturity and self-knowledge is of interest.

6 References

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Table B1: Summary statistics for additional variables used in Appendix B-F (core sample)

Variable	Obs	Mean	Std. Dev.	Comment
Wage in 2006 (W1)	12570	28442	12202	
Imputed wage 2 (W3)	14213	27953	12027	Set to missing in case of zero lagged earnings
Cognitive skill measure based on sum of subscores	13173	.000	.996	Based on 4-36 scale
Noncognitive skill measure based on sum of subscores	11960	.000	.985	Based on 4-20 scale
Enlisted as squad leader	14703	0.204		
Enlisted as platoon leader	14703	0.088		
Family background: Father white-collar worker	10771	0.321		Coded from industry data
Family background: Mother white-collar worker	10886	0.680		Coded from industry data
Cognitive subscore 1: Logic ability	13278	5.19	1.94	
Cognitive subscore 2: Verbal ability	13439	4.99	1.76	
Cognitive subscore 3: Technological comprehension	13505	5.08	1.84	
Cognitive subscore 4: Metal folding	13439	5.22	1.92	
Noncognitive subscore 1	12011	3.14	0.72	
Noncognitive subscore 2	11999	2.94	0.87	
Noncognitive subscore 3	12007	3.15	0.68	
Noncognitive subscore 4	11997	3.03	0.67	

Table B2: Controlling for selection bias in regression of log wages

	(1) OLS	(2 OLS	(3 Median	(4) IAI	(5) Heckman two-step	
Wage measure	W1	W3	W2q	W2	W2	*Select*
Cognitive skills	0.086***	0.084***	0.083***	0.092***	0.088***	0.022
	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.021)
Noncognitive skills	0.065***	0.066***	0.059***	0.071***	0.085***	0.196***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.012)	(0.022)
Observations	12545	14150	14626	13760	14626	14626
R-squared	0.298	0.290	.	0.279	.	.

Heteroskedasticity-robust standard errors are reported in parenthesis in columns (1)-(2) and (4). Standard errors in column (4) computed with bootstrap (20 replications). All regressions include the small set of covariates. See Table 1 for more information.

Table B3: Summary statistics for non-core sample

Sample	Variable	Obs	Mean
Excluded from core sample*	Cognitive ability	1179	-0.014
	Noncognitive ability	1179	-0.069
	Earnings in 2006	1179	135,300
Family sample – restricted**	Cognitive ability	24,918	0.053
	Noncognitive ability	24,918	0.130
	Earnings in 2006	24,918	341,800

*Excluded due to student status, self-employment or work in agricultural sector
**The "restricted" sample excludes self-employed, workers in the agricultural sector and students

Table B4: Summary statistics by enlistment status (core sample)

Sample	Variable	Obs	Mean
Enlisted	Cognitive ability	13,228	0.05
	Noncognitive ability	13,228	0.11
	Earnings in 2006	13,228	327,300
Not enlisted	Cognitive ability	1,475	-0.45
	Noncognitive ability	1,475	-1.03
	Earnings in 2006	1,475	252,400

Table B5: Regression results for alternative samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Annual earnings	Log wages (W2)	Annual earnings	Log wages (W1)	Log wages (W1)	Log wages (W1)	Annual earnings	Log wages (W2)
Cognitive skills	31,359***	0.083***	37,769***	0.100***	0.101***	0.089***	33,781***	0.087
	(1,724)	(0.003)	(1,452)	(0.003)	(0.003)	(0.002)	(1,461)	(0.003)
Noncognitive skills	35,826***	0.065***	41,401***	0.065***	0.056***	0.065***	37,541***	0.068
	(1,886)	(0.003)	(1,691)	(0.003)	(0.003)	(0.003)	(1,617)	(0.003)
Cognitive skills sq.					0.009***			
					(0.002)			
Noncognitive skills sq.					-0.000			
					(0.002)			
Cognitive*Noncognitive skills					0.018			
					(0.003)			
Sample	Unrestricted core sample	Unrestricted core sample	Restricted family sample	Restricted family sample	Restricted family sample	Restricted core sample	Restricted core sample	Restricted core sample, only men who enlisted
Year	2006	2006	2006	2006	2006	2004-2006	2004-2006	2006
Observations	15,798	14,807	24,872	11,983	9,309	37,294	43,882	12,651
R-squared	0.153	0.286	0.138	0.273	0.293	0.278	0.171	0.287

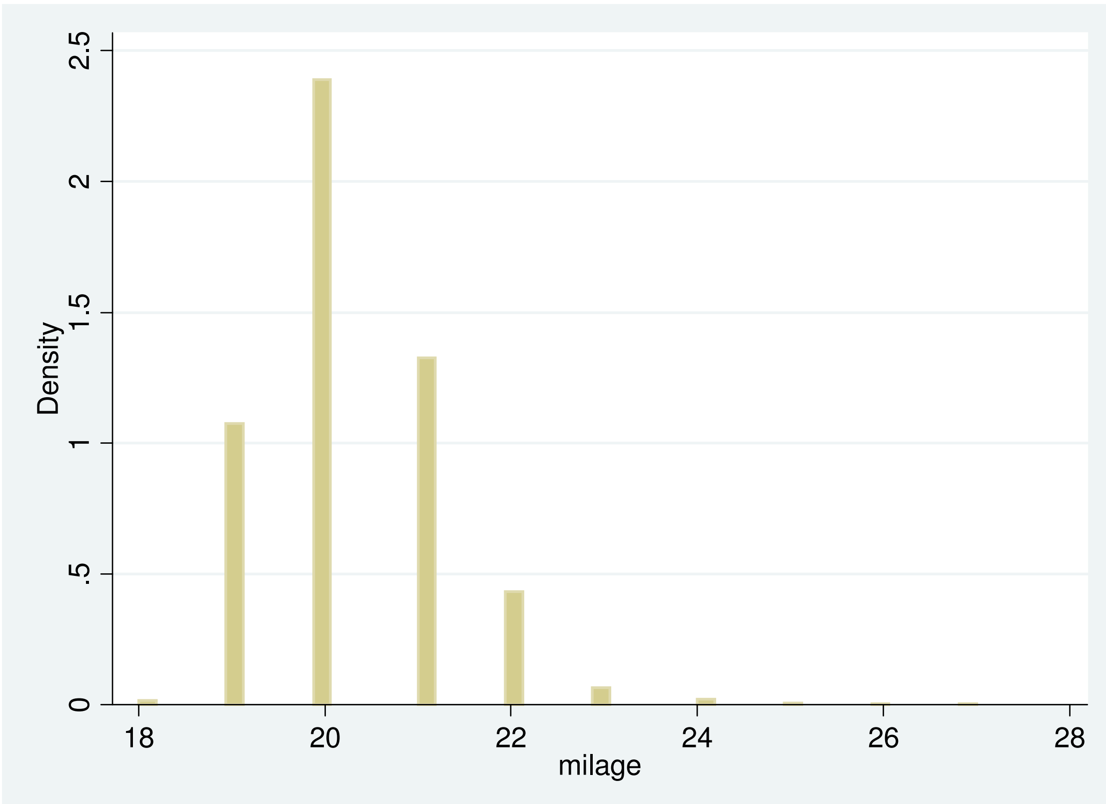
All regression are estimated with OLS with the small set of covariates, except for (3) and (4) which do not include control variables for family background (parental income and marriage status) and (8) which does not include enlistment status. Earnings and log wages for 2004 and 2005 have been adjusted for inflation to 2006 price levels. Robust standard errors in parenthesis. Standard errors have been clustered at the individual level in regression (6) and (7). The “restricted” sample excludes self-employed, workers in the agricultural sector and students

Table B6: Selection into the military service

	(1) OLS	(2) OLS
Dependent variable	Enlistment	Attrition
Cognitive skills	-0.000	-0.004
	(0.002)	(0.004)
Noncognitive skills	0.005**	-0.031***
	(0.003)	(0.003)
Education: Primary school		-0.011
		(0.028)
Education: Secondary school		-0.027
		(0.026)
Education: Two years beyond secondary school		-0.035
		(0.027)
Education: University		-0.012
		(0.025)
Observations	7552	13133
R-squared	0.727	0.008

Attrition from the military service occurs when a conscripts is enlisted but does not begin the military service. Regression (1) entails the full set of health status dummies but no additional covariates. Excluded education category in (2) is a PhD. Robust standard errors in parenthesis.

Figure B1: Histogram of age at the onset of the military service



“milage” refers to age at the onset of the military service.

Table D1: Unemployment and earnings – additional analysis

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS
Dependent variable	Unemployment support	Unemployment support	Annual earnings (SEK)	Annual earnings (SEK)	Log of annual earnings
Cognitive skills	-0.012***	-0.011***	18,121***	27,146***	0.082***
	(0.003)	(0.004)	(2,045)	(2,125)	(0.003)
Noncognitive skills	-0.021***	-0.033***	32,730***	49,871***	0.075***
	(0.003)	(0.004)	(1,987)	(2,575)	(0.004)
Covariate set	Large	Small	Large	Small	Small
Measurement error correction	No	Yes	No	Yes	No
Sample restriction	No	No	No	No	Earnings above 120,000 SEK
Observations	13732	14626	13732	14626	13229
R-squared	0.042	0.033	0.198	0.187	0.213

See Table 1 in the main text for definitions of the “small” and “large” set of covariates, and details for the measurement error correction.

Table D2: Quantile regression, small set of covariates

	10	15	20	25	30	35	40	45	50
Cognitive skills	21,151***	19,090***	15,446***	15,628***	15,452***	16,228***	17,062***	19,302***	22,169***
	(5,578)	(2,807)	(1,476)	(1,299)	(1,163)	(1,127)	(1,119)	(1,152)	(1,075)
Noncognitive skills	52,640***	38,540***	24,815***	19,839***	18,235***	18,667***	19,830***	20,705***	21,302***
	(5,246)	(2,885)	(1,806)	(1,307)	(1,289)	(1,222)	(1,248)	(1,195)	(1,172)
	55	60	65	70	75	80	85	90	
Cognitive skills	25,026***	30,406***	36,184***	41,297***	47,808***	50,141***	53,165***	66,409***	
	(1,398)	(1,441)	(1,540)	(1,887)	(2,534)	(2,899)	(3,462)	(5,570)	
Noncognitive skills	23,767***	26,324***	28,257***	31,073***	33,288***	39,494***	47,307***	59,705***	
	(1,350)	(1,315)	(1,610)	(1,650)	(2,073)	(2,552)	(2,849)	(5,172)	

Unconditional quantiles estimated using [rifreg] command by Firpo, Fortin and Lemieux (2009) with a Gaussian kernel function and the optimal bandwith according to Silverman (1986) (14,155.918). All regressions include the “small” set of covariates as defined in Table 1. Standard errors estimated by bootstrap (50 replications). Sample size is 14,626 observations.

Table D3: Quantile regression, large set of covariates

	10	15	20	25	30	35	40	45	50
Cognitive skills	13,025***	13,673***	11,296***	12,168***	11,130***	11,306***	11,658***	13,080***	14,193***
	(4,965)	(2,696)	(1,958)	(1,583)	(1,343)	(1,193)	(1,199)	(1,029)	(1,016)
Noncognitive skills	47,224***	34,762***	23,000 ***	18,527***	16,660***	17,185***	17,975***	18,704***	19,165***
	(5,420)	(3,259)	(1,837)	(1,303)	(1,165)	(1,230)	(911)	(1,098)	(1,233)
	55	60	65	70	75	80	85	90	
Cognitive skills	15,364***	18,809***	21,640***	24,005***	26,866***	25,310***	26,522***	35,205***	
	(1,228)	(1,424)	(1,555)	(1,828)	(2,067)	(2,120)	(3,539)	(5,070)	
Noncognitive skills	21,438***	22,991***	24,172***	25,406***	27,377***	33,038***	39,226***	51,565***	
	(1,144)	(1,252)	(1,711)	(1,621)	(2,112)	(2,237)	(3,102)	(4,635)	

Unconditional quantiles estimated using [rifreg] command by Firpo, Fortin and Lemieux (2009) with a Gaussian kernel function and the optimal bandwith according to Silverman (1986) (14,369.705). All regressions include the “small” set of covariates as defined in Table 1. Standard errors estimated by bootstrap (50 replications). Sample size is 13,732 observations.

Table D4: Robustness for omitted variables (log wages)

	(1)	(2)	(3)	(4)
Cognitive skills	0.075***	0.079***	0.079***	0.085***
	(0.003)	(0.003)	(0.003)	(0.003)
Noncognitive skills	0.052***	0.059***	0.059***	0.067***
	(0.003)	(0.003)	(0.004)	(0.003)
Sample restrictions		Health status observed	Health status observed	
Additional controls	Type of military service	-	Health status	Age at draft
Observations	13974	7198	7198	13845
R-squared	0.301	0.283	0.285	0.294

Dependent variable is the log of wage measure W2. All regressions include the "small" set of covariates as defined in Table 1 and are estimated using OLS. Heteroskedasticity-robust standard errors in parenthesis.

Table E1: Occupational choice, marginal effects from multinomial logit

	Managers		Skilled		Unskilled	
Cognitive skills	0.027***	0.021***	0.173***	0.120***	-0.200***	-0.141***
	(0.003)	(0.004)	(0.007)	(0.008)	(0.007)	(0.008)
Noncognitive skills	0.044***	0.045***	0.087***	0.070***	-0.131***	-0.116***
	(0.003)	(0.003)	(0.006)	(0.007)	(0.007)	(0.007)
Covariate set	Small	Large	Small	Large	Small	Large
Observations	12,274	11,575	12,274	11,575	12,274	11,575

Standard errors in parentheses. We exclude men who work in the military. See Table 1 for more information.

Table E2: Occupation specific skill prices

	Managers		Skilled occupations		Unskilled occupations	
	(1) Lee	(2) BFG	(3) Lee	(4) BFG	(5) Lee	BFG
Cognitive skills	0.058**	0.067**	0.081***	0.082***	0.022**	0.034*
	(0.023)	(0.029)	(0.009)	(0.011)	(0.009)	(0.017)
Noncognitive skills	0.107***	0.092*	0.061***	0.056***	0.038***	0.044***
	(0.029)	(0.054)	(0.007)	(0.015)	(0.006)	(0.010)
Observations	611	609	3036	2993	2975	2975
R-squared	0.362	0.366	0.288	0.283	0.069	0.073

Dependent variable is log wages (W2) in all regressions. All regressions include the large set of covariates as defined in Table 1. Standard errors calculated with bootstrap (50 repetitions). Specification (1), (3) and (5) use the selection correction method in Lee (1983) while (2), (4) and (6) use Bourguignon et al. (2007). All specifications are estimated using the [selmlog] STATA software developed by use Bourguignon et al. (2007).

Table E2, cont: Selection stage

No observation on occupation	Cognitive skill	-0.265	0.049
	Noncognitive skill	-0.306	0.044
	Region 1980: Gothenburg	0.368	0.214
	Region 1980: Stockholm	0.243	0.163
	Region 1980: Malmö	0.695	0.236
	Region 1980: South	-0.087	0.162
	Region 1980: Middle	-0.383	0.198
	Mother white-collar 1980	-0.190	0.084
	Father white-collar 1980	-0.074	0.084
Managers	Cognitive skill	-0.012	0.061
	Noncognitive skill	0.344	0.054
	Region 1980: Gothenburg	-0.348	0.274
	Region 1980: Stockholm	0.192	0.186
	Region 1980: Malmö	0.166	0.283
	Region 1980: South	0.486	0.181
	Region 1980: Middle	-0.084	0.219
	Mother white-collar 1980	-0.220	0.102
	Father white-collar 1980	-0.206	0.100
Unqualified workers	Cognitive skill	-0.466	0.042
	Noncognitive skill	-0.343	0.039
	Region 1980: Gothenburg	0.302	0.203
	Region 1980: Stockholm	0.012	0.152
	Region 1980: Malmö	0.200	0.231
	Region 1980: South	-0.186	0.149
	Region 1980: Middle	-0.477	0.175
	Mother white-collar 1980	-0.167	0.071
	Father white-collar 1980	-0.107	0.073

The excluded category is skilled workers. The results for the “large” set of control variables have been excluded from the Table due to space considerations.

Table F1: Log wages – skill measures based on the sum of subscores

	(1)	(2)	(3)	(4)	(5)	(6)
Cognitive skills	0.084***	0.048***	0.090***	0.104***		0.085***
	(0.003)	(0.003)	(0.004)	(0.003)		(0.003)
Noncognitive skills	0.065***	0.057***	0.098***		0.090***	0.066***
	(0.003)	(0.003)	(0.005)		(0.003)	(0.003)
Cognitive skills sq.						0.009***
						(0.002)
Noncognitive skills sq.						-0.001
						(0.002)
Noncognitive*Cognitive						0.014***
						(0.003)
Covariate set	Small	Large	Small	Small	Small	Small
Measurement error correction	No	No	Yes	No	No	No
Observations	10761	10090	10761	12542	11396	10761
R-squared	0.290	0.342	0.318	0.262	0.235	0.295

This table replicates Table 1 except for the measure of cognitive and noncognitive which are based on the sum of subscores.

Table F2: Pairwise correlations between cognitive subscores

	Logic ability	Verbal ability	Technological comprehension	Metal rotation
Logic ability	1.0000			
Verbal ability	0.6840	1.0000		
Technological comprehension	0.5445	0.5023	1.0000	
Metal folding	0.6062	0.5206	0.5753	1.0000

Table F3: Log wages – Cognitive subscores

	(1)	(2)	(3)	(4)	(5)
Logic ability	0.041***				0.023***
	(0.001)				(0.002)
Verbal ability		0.040***			0.011***
		(0.001)			(0.002)
Technological comprehension			0.042***		0.022***
			(0.001)		(0.002)
Metal folding				0.033***	0.004**
				(0.001)	(0.002)
Observations	12967	12967	12967	12967	12967
R-squared	0.236	0.218	0.227	0.211	0.254

Dependent variable is the log wages (W2). Each ability measure is set on a 1-9 scale (the measures are not normalized). All regressions are estimated with OLS and include the small set of covariates. See Table 1 for more information.

Table F4: Pairwise correlations between noncognitive subscores

	Score 1	Score 2	Score 3	Score 4
Score 1	1.0000			
Score 2	0.4588	1.0000		
Score 3	0.6219	0.5319	1.0000	
Score 4	0.6270	0.4672	0.5661	1.0000

Table F5: Log wages – Noncognitive subscores

	(1)	(2)	(3)	(4)	(5)
Score 1	0.106***				0.056***
	(0.004)				(0.005)
Score 2		0.061***			0.016***
		(0.003)			(0.004)
Score 3			0.105***		0.044***
			(0.004)		(0.005)
Score 4				0.103***	0.037
				(0.004)	(0.006)
Observations	11445	11432	11441	11432	11396
R-squared	0.221	0.195	0.215	0.210	0.237

Dependent variable is log wages (W2). Each ability measure is set on a 1-5 scale (the measures are not normalized). All regressions are estimated with OLS and include the small set of covariates. See Table 1 for more information.

Table F6: Log wages - residualized measures

	(1)	(2)
Cognitive skills	0.111***	
	(0.002)	
Noncognitive skills		0.100***
		(0.003)
Cognitive skills - residualized		0.086***
		(0.003)
Noncognitive skills - residualized	0.066***	
	(0.003)	
Observations	13974	13974
R-squared	0.294	0.294

The dependent variable is log wages (W2). The residualized measures are the residuals from a regression of the residualized measure on the other skill measure. Both regressions include the small set of covariates as defined in Tabke 1.

Table F7: Effect of cognitive ability score on measured noncognitive ability score

Final cognitive ability score (1-9)	0.109***
	(0.008)
Observations	494,981
R-squared	0.170

Dependent variable is noncognitive ability (1-9). The regression includes a fifth order polynomial of the sum of cognitive ability subscores interacted with draft year.