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

Erik Lindqvist† Roine Vestman‡

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Abstract

We use data from the military enlistment for a large representative sample of Swedish men 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. Unlike survey-based measures of noncognitive ability, this measure is a substantially stronger predictor of labor market outcomes than cognitive ability. In particular, we find strong evidence that men who fare badly in the labor market – in the sense of long-term unemployment or low annual earnings – lack noncognitive but not cognitive ability. We point to a technological explanation for this result. Noncognitive ability is an important determinant of productivity irrespective of occupation or ability level, though it seems to be of particular importance for workers in a managerial position. In contrast, cognitive ability is valuable only for men in qualified occupations. As a result, noncognitive ability is more important for men at the verge of being priced out of the labor market.

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

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†Research Institute of Industrial Economics (IFN), Stockholm, Sweden. E-mail: erik.lindqvist@ifn.se

‡Department of Economics, New York University. E-mail: roine.vestman@nyu.edu

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 abilites 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.[2] 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 a measure of noncognitive ability based on a personal interview conducted by a professional psychologist. Using this measure, we find that noncognitive ability is considerably more important than cognitive ability for success in the labor market. Moreover, our results suggest that the effect of cognitive ability on wages, unemployment and earnings has been overestimated in previous studies due to a lack of adequate controls for noncognitive ability.

We obtain our measures of cognitive and noncognitive ability by 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. Both the cognitive test score and the psychologists' assessment

[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.

[2] See Borghans et al. (2008 b) and Bowles, Gintis and Osborne (2001) for reviews and discussion about the previous literature on personality, noncognitive skill and economic outcomes.

are set on a discrete nine-point scale which approximates a normal distribution.

We argue that the psychologists' assessment offer a more precise measure of noncognitive ability than measures based on self-reported questionnaires. First, it is arguably easier to lie in a questionnaire than straight to another person. Second, many personal traits which may be difficult to accurately capture in a questionnaire are revealed in a personal encounter. The enlistment psychologists have thus access both to more extensive and more accurate information about conscripts' psychological status than what can be deducted from surveys. Our principal findings support this notion.

First, our measure of noncognitive ability is a substantially stronger predictor of wages than the survey-based measures previously employed in the literature. In a regression with a standard set of control variables, a one standard deviation increase in this measure predicts an increase in wages by about nine percent or one third of a standard deviation, compared to five percent for cognitive ability.[3] Not controlling for noncognitive ability gives an upward bias of more than forty percent on the estimated effect of cognitive ability on log wages.

Second, noncognitive ability is a much stronger predictor of employment status than cognitive ability. A one standard deviation increase in noncognitive ability predicts a decrease in the probability of receiving unemployment support by 3.3 percentage units, compared to 1.1 percentage units for cognitive skill. Moreover, men with high noncognitive ability have shorter unemployment spells while cognitive ability has no statistically significant effect on the duration of unemployment.

Finally, noncognitive ability is a stronger determinant of annual labor market earnings, in particular at the low end of the earnings distribution. For example, a one standard deviation increase in noncognitive ability predicts a decrease in the probability that annual earnings fall short of the tenth percentile of the earnings distribution by 4.7 percentage units. The corresponding figure for cognitive ability falls from 1.5 to 0.2 percentage units when noncognitive ability is controlled for.

Figure 1 and 2 provide graphical illustrations of the effect of cognitive and noncognitive ability on annual earnings. Figure 1 shows how the proportion of low income earners varies with noncognitive ability among men with average cognitive ability and, correspondingly, how the proportion of low income earners varies with cognitive ability among men with average noncognitive ability. As is clear from the figure, a very large proportion of men in the two lowest noncognitive ability categories have low earnings, despite average cognitive ability. Figure 2 shows the estimated partial effects of cognitive and noncognitive ability on the probability of earning less than the tenth percentile in a regression with a standard set of control variables.

[3] The set of control variables include region of residence, cohort, family background, enlistment into the military service, education and linear-quadratic terms in experience.

Apart from the higher proportion of low income earners among men with the lowest cognitive skill score (1), the proportion of men with low annual earnings does not change appreciably as cognitive ability increases. In contrast, an increase in noncognitive ability is associated with a lower proportion of low income earners throughout the skill distribution.[4]

Figure 1 and 2. Probability of annual earnings below 10th percentile

Fig. 1. At average of the other skill measure

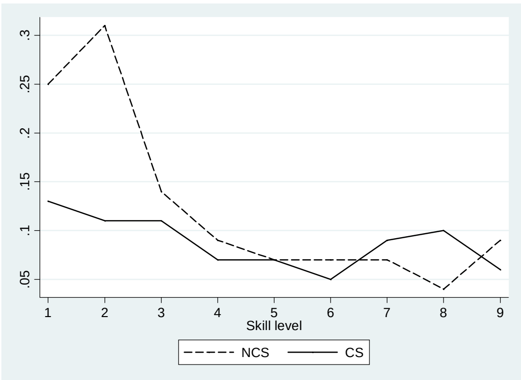
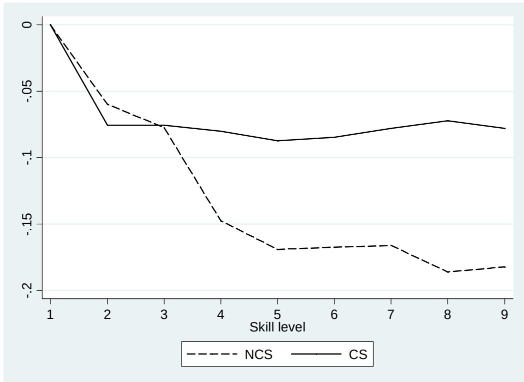


Fig. 2. Partial effects (with controls)



The particularly strong effect of noncognitive ability on unemployment and annual earnings is consistent with the institutional features of the Swedish labor market and the valuation of abilities across different types of workers. Though there is no minimum wage law in Sweden, workers with low productivity are priced out of the labor market due to union wage bargaining (Skedinger, 2008). Consequently, small changes in productivity can have a large effect on annual earnings if they make the difference between employment and unemployment. In our case, a closer look at the data reveals that while log wages are linear in noncognitive ability, they are strictly convex in cognitive ability with a low marginal product for low ability levels. This implies that noncognitive ability is particularly important for workers who have low productivity and who, consequently, are at risk of becoming unemployed.

The differential effects of cognitive and noncognitive ability on log wages suggest that abilities are rewarded differently across occupations. Dividing workers into three occupational groups (managers, qualified workers and unqualified workers), we find a clear selection pattern with respect to our ability measures. Though workers in unqualified occupations have a lower general level of ability, the difference is more pronounced for cognitive ability. Interestingly, workers in a managerial position have somewhat lower cognitive ability than workers in other qualified

[4] The results in Figure 1 and 2 have not been adjusted for measurement error in our skill measures. Since the measure of noncognitive skill has a lower reliability ratio than the cognitive skill measure, the pattern displayed in Figure 1 and 2 is therefore an *underestimation* of the relative importance of noncognitive skill.

occupations, but significantly higher noncognitive ability. This selection pattern is consistent with occupation-specific skill prices. For unqualified workers and managers, noncognitive ability has a significantly higher return than cognitive ability. In contrast, qualified workers in non-managerial positions have similar return to cognitive and noncognitive ability. In essence, we find that noncognitive ability is important regardless of occupation or level of ability, while cognitive ability is important only for workers in qualified occupations.

We believe these results are relevant for policy. In particular, by demonstrating the importance of noncognitive ability for unqualified workers, our paper strengthens the argument for social interventions like the Perry Preschool program or Head Start, which offer an enriched environment to children from a disadvantaged background. Previous research has found that these programs are successful in improving noncognitive abilities but has no effect on IQ (Heckman, 2000; Cuhna et al., 2006).

Our paper is related to the small but expanding literature on personality and socioeconomic outcomes initiated by Bowles and Gintis (1976), Edwards (1976) and Jencks (1979).[5] 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), withdrawal and aggression (Osborne, 2003) and Machiavellianism (Turner and Martinez, 1977) have been found to predict wages. 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 survey and Mueller and Plug 2005 for a recent contribution in the economics literature).

Another strand of the literature infer noncognitive ability from observable choices. 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 estimating a latent factor model estimated on NLSY data while Kuhn and Weinberger (2005) use participation in sports in high-school or a leadership position in clubs as indicators of leadership 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.[6] Moreover, by considering a wide set of labor market outcomes and more flexible functional forms, we show that noncognitive ability is much more important than cognitive ability for avoiding unemployment and poverty. This is a point

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

[6] Grönqvist and Vlachos (2008) use the measures of cognitive and noncognitive ability from the Swedish enlistment in a study of teacher performance.

not fully appreciated in the previous literature which have focused on estimating linear models of cognitive and noncognitive ability on log wages. We are, to the best of our knowledge, also the first to include measures of both cognitive and noncognitive ability in a model of occupational choice.

In line with the previous literature, we use "noncognitive ability" as a term for abilities which are distinct from the ability 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.[7] The words "ability" and "skill" are used interchangeably throughout this paper.

The paper proceeds as follows. Our data and measures of cognitive and noncognitive ability are discussed in Section 2. We discuss our basic estimation strategies in Section 3. The results for wages, employment and earnings are reported in Section 4. We consider occupational choice in Section 5. Section 6 concludes the paper.

2 Data

The data 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. We focus on labor market outcomes in 2006. The military service is mandatory only for men, and we exclude the small fraction of women for whom we have enlistment data.

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; men with a 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.[8]

[7] See, for example, Borghans et al. (2008). 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.

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

2.1 Socioeconomic variables in LINDA

The main data sources for LINDA are the Income Registers and the Population Census.[9] LINDA is thus complete with respect to different sources of taxable income and social benefits like unemployment support. In addition, LINDA contains information on occupation and wages from separate registers held by Statistics Sweden. The wage registers are not complete for the private sector. In total, we have data on wages in 2006 for 12,570 workers, or 85.5 % of the sample. The remaining group consists both of people with no or limited participation in the labor market (e.g., people who were unemployement 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.[10] Using wages from previous years, we are able to add 1,401 observations to the data, 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. In Appendix B, we report the results when we only consider wages reported in 2006 and when we use data on annual earnings to impute wages for men with no data on wages for the entire 2001-2006 period. We also discuss an estimation technique (median regression) where assumptions on imputed wages can be relaxed.

We consider three different forms of social benefits related to lack of employment: unemployment support; early retirement benefits and social welfare benefits.[11] Individuals must be actively looking for a job in order to qualify for unemployment support whereas early retirement benefits requires that an individual is incapable of working full-time due to poor health. Both unemployment support and early retirement benefits are based on previous income. In contrast, eligibility for social welfare hinges on the individual's current economic circumstances. The reason why we do not only consider unemployment support is that individuals may substitute between different types of social benefits. In particular, people substitute from unemployment support to early retirement since unemployment support has a time limit and a lower reimbursement ceiling. For each type of benefit, we construct dummy variables that take the value one if a person received positive transfers in 2006.[12] We also construct measures for the fraction of the year 2006 spent in unemployment using data on total unemployment benefits and income

[9] Edin and Fredriksson (2000) provide a detailed account of the data collection process for LINDA.
[10] The wage data for 2006 is censored at 12,000 SEK. We use the same cutoff for the imputed wages.
[11] The Swedish terms are *arbetsmarknadsstöd*, *sjuk- och aktivitetsersättning* and *socialbidrag*.
[12] LINDA does not contain information on employment status at a particular point in time.

in previous years.[13]

We construct five dummy variables for educational attaiment 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 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.[14]

Direct information on family background are not available in LINDA, but we are able to construct variables on 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[15]

The military enlistment usually takes place the year a Swedish man turns 18 or 19.[16] 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 did not get a low health rating were enlisted to the military service.[17] Importantly, it was not possible to avoid the military service by obtaining a low score on cognitive or noncognitive skill.[18] However, the results on cognitive and noncognitive skill predict the precise type of service to which conscripts are enlisted.

[13] The details behind the construction of our measures are available in Appendix A.

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

[15] 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.

[16] 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.

[17] 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). In total, 90.0 % of the men in our sample were enlisted to the military service. Due to the end of the cold war, the size of the Swedish army has shrunk considerably and only a small fraction of Swedish men serve in the military today.

[18] Once health status is controlled for, the result on the test of 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; an increase in estimated noncognitive skills by one standard deviation predicts an increase in the probability of being enlisted by 0.53 percentage units.

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.[19] This test consists of four different parts (synonyms; inductions; metal folding and technical comprehension) which are each graded on a scale from 0 to 40. The results of these tests are then transformed to a discrete variable of general cognitive ability ranging from 1 to 9.[20] This variable follows a Stantine scale that approximates a normal distribution.[21]

We create two measures of cognitive ability based on the enlistment tests. First, we normalize the 1-9 measure of general cognitive ability to a distribution with zero mean and unit variance.[22] This measure is available for the entire sample and used in our main specifications. However, in regressions with higher order terms and adjustment for measurement error, the discreteness implied by the underlying nine-point scale turns out to be problematic as the higher moments do not fit a normal distribution with unit variance (see Section 3.2). We therefore construct an alternative measure of cognitive ability from the sum of the raw scores on each subtest, which ranges from 0 to 160. The sum of the subscores is percentile 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 13,064 out of 14,703 observations in our data. Our results in the linear case are similar regardless of which measure we use.

2.2.2 Measure of noncognitive ability

Like the tests 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.[23] This

[19] 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).

[20] The conversion is done in two steps. First, each 0-40 score in converted to a 1-9 score. The sum of these four scores (ranging from 4 to 36) is then converted to the final 1-9 score.

[21] 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).

[22] 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.

[23] Husén recognized already at an early stage that selection into the military service must be based both on an assessment of constripts' skills, such as intelligence, and of his character (Husén 1942 b). For example, Husén emphasized the important role for emotional stability (1942 a) 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.

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.[24] 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).[25]

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 to minor revisions. This procedure implies that conscripts are interviewed by a certified psychologist for about 25 minutes.[26] 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.[27]

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.[28] The psychologists assign each conscript's ability in this respect a score from 1 to 9, which follows the same Stantine distribution as the final test score for cognitive ability.[29] This score is in turn also based on four different subscores which range from 1 to 5. The subscores function only as a guide to the

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: Men who have not matured before entering the military are unlikely to do so while in the military.

[24] 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 1942 c). 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).

[25] *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).

[26] 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)

[27] The term for this type of interview in the psychology literature is *anamnestic*.

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

[29] 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).

psychologists – two conscripts with the same sequence of subscores could still get different final scores.[30]

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 use the second measure only in regressions with higher order terms. The two measures are highly correlated (.97) and the results do not change appreciably in the linear case 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 assume responsibility; independence; outgoing character; persistance; 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 the loss of personal freedom.[31] 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) argues 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 posess 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

[30] The definition of the subscores underlying the psychologists assessment is not publicly available information. However, as outlined below, we have extensive information on the basis for the psychologists' assessment from other sources.

[31] Husén (1946) describes the difficulties in adjusting to military life: In the military, individuals lose part of their individuality, as their ability to express some parts of their personality is limited. Moreover, the collective nature of military life implies that concripts lose a substantial part of their privacy. Finally, the need to become part of the strict military hierarcy implies that conscripts see their freedom of choice curtailed. Husén argues that men with exhibitionistic personality or a strong need to assert oneself will experience the loss of self as more painful.

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).[32] 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, 2003, p. 13).[33]

Our measure is different from the types of measures previously used in the literature on personality and labor market outcomes. Instead of measuring a specific trait, our measure capures a specific ability, i.e., the ability to function in the very demanding environment of armed combat. We argue that this ability is also likely to be rewarded in the labor market. Just like in the military, success in most work environments requires the 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, as opposed to for example the cognitive testing within the National Longitudinal Study of Youth, 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.

[32] 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).

[33] 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.

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. We consider different specifications of $f(c_i, n_i)$, but – like the previous literature – we focus on the linear case. That is, we consider the case when

$$f(c_i, n_i) = \beta_c c_i + \beta_n n_i.$$

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 three important issues to consider in the estimation of (1). First, we do not observe wage offers for the entire sample, which might give rise selection bias. This potential problem is not present in the case of unemployment and annual earnings which are observed for the entire sample. Second, the interpretation of the estimated parameters depends on the variables included in $\mathbf{X}_i$. Third, our estimates may be biased if c or n are measured with error.

We use two approaches for controlling for selection bias in our wage regressions. First, we test whether our results change when we exclude or include imputed wages. Second, we use three alternative estimation methods that control for selection bias under different conditions (median regression, Heckman two-step and Identification at infinity). To facilitate the reading, we discuss these approaches and the results in Appendix B. In essense, we find that our main results are unlikely to be driven by selection bias. In this Section, we instead focus on which covariates to include in $\mathbf{X}_i$ and measurement error.

3.1 Covariates

Since choices taken after the age of 18 are affected by skill endowment, such factors should not be included in a regression that aims to estimate the total effect of skills on wages. In contrast, a regression that aims to estimate skill prices should control for factors that are rewarded in the labor market and correlated with skills. We run regression (1) with two different specifications of $\mathbf{X}_i$. In the first specification, $\mathbf{X}_i$ contains dummy variables for region of residence, cohort, family background, enlistment into the military service and whether or not an individual has education above primary school.[34] In the second specification, we add to $\mathbf{X}_i$ the full set of indicator variables for educational attainment, and linear-quadratic terms in experience. The first regression thus gives the total effect of skills on outcomes, while the second regression estimates the effect which is independent of schooling and experience. In wage regression, estimation with the large set of control variables gives the price of cognitive and noncognitive skills in the labor market.

A difficult tradeoff is whether or not to control for type of military service. Though the military service is mandatory, conscripts have some freedom to affect the position they are assigned to provided that they fulfill the specific requirements for this position. On the one hand, both cognitive and noncognitive test scores have a direct effect on the type of military training to which conscripts are enlisted. In particular, conscripts who are considered to have high ability are more likely to be enlisted as squad or platoon leaders. 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. On the other hand, the fact that a worker with a high score on noncognitive or cognitive skill was not enlisted into a leadership position is a signal of an unwillingness to assume responsibility. Hence, controlling for type of position will imply that the identifying variation in skills is in fact correlated with an aspect of personality that can be presumed to have a negative effect on outcomes. For this reason, we have chosen only to include a dummy for enlistment into the military service in the basic specifications. The main results when type of military service is controlled for are reported in Table 12.

Another potential concern in 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.[35] The estimated effect of noncognitive skill on wages is somewhat lower when health status is controlled for, but this result is almost entirely driven by sample selection as health status classification is only available for about 50 % of our sample (results not reported).

Another 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. Since nine years of primary school is mandatory in Sweden, this implies that enlisted men may differ by a maximum of three years of schooling.[36] Not controlling for education up to the age of 19 may thus bias our estimates of (β_c, β_n). On the other hand, including a variable for educational attainment at the age of 19 may imply overcontrolling as

[34] We lack data on educational attainment for 47 observations.

[35] 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.

[36] A small proportion of Swedish men undergo the enlistment procedure at a more advanced age than 19, and may thus have obtained a higher level of schooling at the time of the enlistment (see Section 2).

causality runs in both directions. As reverse causality is likely to imply an upward bias, we include a dummy variable for no educational attainment beyond primary school also in the small set of covariates.

3.2 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.[37]

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 $v_c \sim N\left(0, \sigma_{v_c}^2\right)$ and $Cov\left(c^*, v_c\right) = 0$.[38] 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.[39] 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.

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 positively correlated (.389), classical measurement error in one skill measure will imply an upward bias of the estimated effect of the other skill measure.[40]

[37] 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.

[38] We further assume that all cross-moments between the true variables and the measurement errors are zero (see Appendix C).

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

[40] The positive correlation between cognitive and noncognitive ability may reflect an effect of noncognitive ability on cognitive test scores (see Borghans et al. 2008 c).

3.2.1 Twin data

We use data from a sample of twins to calculate the reliability ratio of each skill measure. Here, we illustrate this method in the case of cognitive skills but the argument is the same in the case of noncognitive skill. Consider the equation

$$\Delta y_{MZ} = \beta_{MZ} \Delta c^*_{MZ} + \varepsilon, \tag{2}$$

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} which we denote $\widetilde{\beta}_{MZ}$.[41] Following Griliches (1979), we show in Appendix C that the assumptions above imply that the reliability ratio for the cognitive skill measure can be expressed as

$$\frac{\sigma^2_{c^*}}{\sigma^2_{c^*} + \sigma^2_{v_c}} = \frac{\widetilde{\beta}_{MZ}}{\beta_{MZ}} (1 - \rho_{MZ}) + \rho_{MZ} \tag{3}$$

where ρ_{MZ} is the correlation in c within monozygotic twin pairs. 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 get an analogous expression for dizygotic (fraternal) twins. That is, the estimates from the regression

$$\Delta y_{DZ} = \beta_{DZ} \Delta c^*_{DZ} + \varepsilon', \tag{4}$$

together with the within-twin correlation give the reliability ratio

$$\frac{\sigma^2_{c^*}}{\sigma^2_{c^*} + \sigma^2_{v}} = \frac{\widetilde{\beta}_{DZ}}{\beta_{DZ}} (1 - \rho_{DZ}) + \rho_{DZ}. \tag{5}$$

Since $\left(\widetilde{\beta}_{MZ}, \rho_{MZ}, \widetilde{\beta}_{DZ}, \rho_{DZ}\right)$ are directly observable in the data, the reliability ratio is identified under the assumption that $\beta_{MZ} = \beta_{DZ}$.

An implicit assumption in the formulation above is that there is no correlation in measurement error within twin pairs. This assumption could be violated if, for example, motivation affects performance and motivation for the military service is correlated within twin pairs. If this correlation is weakly above zero, the estimated reliability ratios are in fact upper bounds

[41] 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.

on the true reliability ratios.

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.[42] This sample covers 701 twin pairs with data on annual earnings and the enlistment skill measures.[43] As shown in Appendix C, the reliability ratios implied by these estimates are .8675 for cognitive and .70267 for noncognitive ability.[44] 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.[45] [46]

3.2.2 Measurement error in nonlinear models

As shown by e.g. Griliches and Ringstad (1970), estimation problems due to measurement error gets worse in models with higher order terms. In quadratic models, the effect of measurement errors is to flatten the curvature of the estimated function.[47] Hence, estimating more flexible functional forms of $f(c_i, n_i)$ puts higher demand on the data and on the specification of measurement error. In our case, the discreteness of the ability measures implies that the higher moments of the observed variables differ from what would be the case for normally (and thus continuously) distributed variables with unit variance. In particular, the fourth moments of cognitive and noncognitive ability are lower than three.[48] In order to obtain more continuously distributed ability measures with higher moments that better fit the assumed distribution, we use the alternative ability measures based on subscores in estimations with quadratic or interaction terms. These alternative ability measures fit better with our implied distributional

[42] The parameters are estimated by OLS regressions with annual earnings as the dependent variable. The results are very similar when we instead consider the log of annual earnings as the dependent variable.

[43] 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.

[44] We take the estimated reliability ratios to be true in our labor market outcome regressions. This may imply a downward bias on the estimated standard errors as the uncertainty regarding the true reliability ratios is not taken into account. However, all coefficients of interest in the linear case increase as a result of adjusting for measurement error and are strongly statistically even with no adjustment for measurement error.

[45] 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).

[46] We have performed robustness tests with a positive covariance in the measurement error in cognitive and noncognitive ability. This is the case that would apply if, for example, motivation for the military service affects performance on both tests positively. Assuming positive measurement error covariance increases the estimated effect of both cognitive and noncognitive ability (results not reported).

[47] See Kuha and Temple (2003) for a discussion of measurement error in quadratic models.

[48] The fourth moment of a normally distributed variable with mean zero and unit variance is $3\sigma^4 = 3$.

assumptions. We impose a few additional assumptions on the cross-moments in order to obtain a measurement error variance-covariance matrix where all off-diagonal terms are equal to zero. The details are available in Appendix C.

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

Table 2a gives the results for regression (1) using different estimators with log wages as the dependent variable. The results for the other two wage measures are discussed in Appendix B along with the other tests for selection bias. The first column of Table 2a reports the results for the OLS estimator with the small set of covariates and no adjustment for measurement error. In this case, an increase in cognitive skill by one standard deviations increases log wages by .086. The estimated wage premium to noncognitive skill is .067 log points. The relative size of cognitive and noncognitive skills is reversed once we include the full set of control variables for educational attainment and experience in the second column. Whereas the estimated effect of cognitive skill on wages is very sensitive to controlling for experience and education, the estimated effect of noncognitive skill is only affected to a small extent. The reason for this result is that cognitive skill is a much stronger predictor of educational attainment than noncognitive skill.[49] Hence, a considerable part of the total effect of cognitive skill at age 18 on wages in adult age reflects education, and not a direct price for skills.

Adjusting for measurement error in column 3 and 4 increases the estimated effect of noncognitive skill by more than 50 percent while the results for cognitive skill increase only slightly. As a result, the total effects of cognitive and noncognitive skills on log wages are similar at .089 and .102. The effect of noncognitive skill is 68 percent larger than the effect of cognitive skill when we control for education and experience: a one standard deviation increase in noncognitive skill increases log wages by .091 compared to .054 for cognitive skill. Table 2a also shows that the estimated effect of cognitive ability on log wages is sensitive to whether noncognitive ability is controlled for or not. Dropping noncognitive ability from the regression (column 8) increases the estimated effect of cognitive ability on log wages by 46 percent.

[49] A linear probability model of the likelihood of obtaining a university degree on our ability measures and the small set of control variables shows that a one standard deviation increase in cognitive ability predicts an increases in the probability of obtaining a university education by 16.3 percentage units compared to 4.5 percentage units for noncognitive skills.

Our estimates of noncognitive skill on wages are larger than what has been previously found in the literature. For example, Heckman et al. (2006) report .121 as the standardized OLS estimate for cognitive skill on log wages and .042 as the standardized OLS estimate for their measure of noncognitive skill.[50] In the specification most similar to our specification with the large set of control variables, Mueller and Plug (2006) find moderate standardized effects for the Big Five-factors agreeableness (−.036) neuroticism (−.022) and openness to experience (.033). Similarly, Borghans et al. (2008) review the evidence from the psychology literature and find the Big Five-factors to be weakly correlation with job performance. Borghans et al. (2006) report that a one standard deviation increase in sociability in childhood (age six) increase wages by about one percent on average, though this effect is larger in occupations where social skills are particularly important.

The difference between our estimates and the previous literature becomes even starker when we normalize our coefficients with respect to the variance in the dependent variable (i.e., log wages). This result is a consequence of the relatively compressed wage structure in Sweden. Adjusting for measurement error, a one standard deviation increase in noncognitive skill increases log wages by .317 standard deviations with the small set of covariates and .283 with the large set. These estimates can be compared to the studies surveyed by Bowles, Gintis and Osborne (2001) where the normalized effects of externality (Rotter scale), self-esteem and other personality measures are found to be to be between one fourth and half of our estimates. Our point estimates are in the same order of magnitude as the combined effect of seven psychological variables in Jencks (1979) of .245 (see Table 1 in Bowles, Gintis and Osborne, 2001).

As shown in column 7-12 of Table 2a, our measure of noncognitive skill is a strong predictor of outcomes also in terms of variance explained. In isolation, noncognitive skill explains 18.0 percent of the variation in log wages, compared to 20.0 percent for cognitive skill. Including both noncognitive and cognitive skill implies that 25.6 % of the variance in log wages is explained. In comparison, Mueller and Plug (2006) find that all factors in the five factor model combined explain five percent of the variation in log wages while Heckman (2006) find that their cognitive skill measure explains 9.0 percent of the variance of log wages compared to 0.9 percent for their noncognitive measure. Moreover, given that the large set of control variables are included as regressors, our noncognitive skill measure is a stronger predictor of log wages in terms of increase in variance explained than cognitive skill (see column 8-10 in Table 2a).

The analysis has so far centered on the log-linear model outlined in Section 3. Though a linear model has the advantage of giving results that are easy to interpret and compare to previous literature, it may not provide the full story. We therefore include quadratic terms and

[50] Heckman et al. (2006) compute their measure of noncognitive skill as a (standardized) average of the Rosenberg Self-Esteem Scale and Rotter Internal-External Locus of Control Scale.

an interaction effect between cognitive and noncognitive ability, using the alternative ability measures described in Section 2.2 and 3.2. The first two columns of Table 2b shows that the results for the linear case are very similar when we use the alternative measures of cognitive and noncognitive ability.

Table 2b shows that log wages are strictly convex in cognitive ability. This pattern remains stable regardless of whether or not we adjust for measurement error or include the interaction effect.[51] In contrast, log wages are linear in noncognitive ability in the specifications without adjustment for measurement error and strictly concave when we adjust for measurement error. Given the lower reliability ratio of noncognitive ability and the problems associated with measurement errors in quadratic terms, we are reluctant to draw strong conclusions from the implied nonlinearity in noncognitive ability. However, the results strongly suggest that the return to noncognitive ability is not increasing with ability level. Our results also indicate that their might be a positive interaction effect between cognitive and noncognitive ability, but this result is sensitive to including the large set of covariates and quadratic terms.

The strong convexity for cognitive ability implies that the marginal product of cognitive ability is close to zero for men at the low end of the cognitive ability distribution. In contrast, the marginal product of noncognitive skill is high also for men at the low end of the noncognitive skill distribution. This is important since, for men with low skills, small changes in productivity can make the difference between employment and unemployment. 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, implying that men with sufficiently low productivity are priced out of the labor market.[52] Consequently, we should expect the level of noncognitive skill to be a stronger predictor of labor market participation than cognitive skill.

4.2 Unemployment

As shown in Table 3, noncognitive skills is a much stronger predictor of receiving unemployment support some time during 2006 than cognitive skill. This is true regardless of whether educational attainment and experience are controlled for or not. For example, in a regression with the large set of control variables and adjustment for measurement error, a one standard deviation increase in noncognitive skills predicts a reduction in the probability of receiving unemployment support by 3.3 percentage units compared to 1.1 percentage units for cognitive skills. Table 3 also reveals that the estimated effect of cognitive ability on employment status is very sensitive to controlling for noncognitive ability. Dropping noncognitive ability from the regressions implies and increase

[51] The strict convexity for cognitive ability holds also in a non-parametric test where we enter a separate dummy variable for each value on the sum of subtests (see Appendix D).

[52] See Skedinger (2008) provide an overview of the Swedish mininum wage system.

in the estimated effect of cognitive ability on unemployment of about 100 percent. Table 4 gives the results for the probability of receiving any form of social assistance related to a weak position in the labor market. In this case, the estimated effect of noncognitive skill is even larger compared to cognitive skill.[53]

Including quadratic terms for skill endowment in column five and six of Table 3 reveals that the relationship between noncognitive skills and unemployment is strictly convex. This result is consistent with the estimates from the wage equation: Since noncognitive skill has a linear relationship with log wages, the relationship between noncognitive skills and unemployment weakens as the skill level increases and fewer workers are close to selecting out of the labor market.

We also find that noncognitive skills predicts the duration of unemployment spells: As shown in Table 5, unemployed men with high noncognitive skills have a significantly higher hazard rate for obtaining a job, while the effect of cognitive skills is both economically and statistically insignificant. This result is robust to using different types of duration models and OLS. The size of the effect is substantial. When estimated with OLS, a one standard deviation increase in noncognitive skill decreases expected unemployment duration by .91 months.

4.3 Earnings

Since noncognitive skills is a strong predictor of both wages and labor force participation, it can be expected to have a strong impact on annual labor market earnings. As shown in Table 6a, this is indeed the case. In the regression with the large set of covariates and adjustment for measurement error, a one standard deviation increase in noncognitive skills predicts an increase in yearly earnings by 52,500 SEK (about 6,400 US dollars) or about one sixth of a standard deviation. The corresponding estimate for cognitive skills is 16,800 SEK (2,100 US dollars) or about six percent of a standard deviation. Similar to wages and employment status, we find that the estimated effect of cognitive ability is sensitive to controlling for noncognitive ability. As shown in Table 6b, we obtain similar results if we instead use the log of annual earnings truncated from below at 120,000 SEK as the dependent variable.

Another way to look at the data is to investigate how skill endowment affect the probability of belonging to certain groups of the income distribution. Table 7 shows that noncognitive skills have a much stronger effect than cognitive skills on the probability of belonging to the group of low income earners. Controlling for the large set of covariates, a one standard deviation in noncognitive skills decreases the probability of belonging to the 10 percent lowest income earners by 4.7 percentage units, compared to 0.2 percentage units for cognitive skills. The

[53] Another potential explanation for why is noncognitive ability is such a strong predictor of unemployment is that it predicts workers' reservation wage. We do not test this explanation here.

relative importance of cognitive skill increases as we consider the probability of belonging to the group of middle- or high-income earners, but it is never close to the effect of noncognitive skill.

A related way to study how skill endowment affect the distribution of income is to see how different quantiles of the earnings distribution vary with skill endowment. Table 8 gives the results for the 10th, 25th, 50th, 75th and 90th quantile (these regressions are not adjusted for measurement error). In line with our previous results, we see that noncognitive skill is more important than cognitive skill for outcomes at the lower end of the earnings distribution but that the effect of cognitive skills increases the higher the percentile of the income distribution we consider.

4.4 Summary of results

The analysis above has shown that noncognitive skill, as measured at the Swedish enlistment, is more important for labor market success than cognitive skill. Our results also point to two striking differences between the effect of cognitive and noncognitive skills on wages. First, unlike cognitive skills, noncognitive skills are not strongly related to educational attainment. Second, while log wages are concave or linear in noncognitive skills, they are strictly convex in cognitive skills. The marginal product of cognitive skill is thus low and the low end of the distribution of cognitive skill, but at high at the high end. Consistent with this finding, we also find that noncognitive skills is a much stronger predictor of selection into employment and of poverty. In the next section, we test whether differences in skill prices across occupations can explain the observed selection patterns and nonlinearities in productivity.

5 Differences across occupations

Due to differences in production technology, cognitive and noncognitive skills may be priced differently across sectors. In particular, we expect cognitive skills to be more important in highskilled occupations. For example, it seems reasonable that cognitive skills is more important for the productivity of a chemical engineer than a supermarket cashier. It is, in our view, less obvious a priori which relationship to expect for noncognitive skills.

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 "qualified" and the workers in the two lowest qualification levels as "unqualified". 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 qualified workers.[54]

Table 9 shows the average level of cognitive and noncognitive abilities across these three occupational categories. Two findings stand out. First, the average level of cognitive skills is highest among workers in qualified occupations, whereas managers have the highest average level of noncognitive skills. Second, the difference between qualified and unqualified workers is stronger in terms of cognitive than noncognitive skills. The difference between qualified and unqualified workers is .95 standard deviations in cognitive skills compared to .59 standard deviations in noncognitive skills.[55]

More formally, Table 10 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 qualified 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 qualified 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 qualified 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 $j = \{\text{manager, qualified, unqualified}\}$. 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 lowskilled occupations in case

[54] Further details underlying our classifications are available in the Appendix.

[55] All differences in average skills between occupational groups are statistically significant at the one percent level, expect 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.

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

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.[56]

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. 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 11. 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 unqualified occupations while workers in qualified occupations have a return to cognitive skill similar to the return to noncognitive skill.

There is a small previous literature on occupational choice and skill endowment. In line with our results, Schmidt and Hunter (2004) find that the importance of IQ rises with job complexity while Gould (2005) find relatively small differences across sectors. Consistent with our finding that noncognitive skills is not a strong predictor of skill level, Barrick and Mount (1991) find that the Big Five-factor conscientiousness does not vary much with job complexity.

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. (2008 b) find that while IQ is considerably more important for job performance than any of the Big Five-factors, the Big Five-factor conscientiousness is slightly stronger correlated

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

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. Borghans et al. (2008 a) document that a prefence for "directness" over "caring" is associated with work in managerial positions.

6 Concluding remarks

A key problem in the literature on personality and labor market outcomes is to obtain valid measures of noncognitive abilities. In this paper, we have used a measure of noncognitive ability based on a personal interview. In contrast to survey-based measures of noncognitive ability, this measure is a substantially stronger predictor of labor market success than cognitive ability. In particular, we find strong evidence that men who fare badly in the labor market in the sense of long-term unemployment or low annual earnings lack noncognitive but not cognitive ability. We point to a technological explanation for this result: Noncognitive ability is an important determinants of productivity irrespective of occupation or skill level, though it seems to be of particular importance for workers in a managerial position. In contrast, the return to cognitive ability is low for unqualified workers but high for workers in qualified occupations. As a result, noncognitive ability is more important for men at the verge of being priced out of the labor market.

Our results suggest that the emphasis put on cognitive ability by, for example, Herrnstein and Murray (1994) should be subject to serious qualifications. Since cognitive and noncognitive ability are positively correlated, a failure to control for noncognitive ability will lead to an upward bias on the effect of cognitive ability on outcomes. In our case, cognitive ability does not predict poverty once noncognitive ability is controlled for, while the estimated effect of cognitive ability on wages, unemployment and annual earnings is reduced substantially.

Previous research (e.g. Cunha et al. (2006), Cunha and Heckman (2007)) have suggested that noncognitive abilities can be substantially affected by early interventions. By demonstrating the particular importance of noncognitive ability for workers with low skills, our results thus reinforce the message put forth by these authors that early interventions for disadvantaged children could have large benefits.

The results in this paper are potentially important for a number related literatures. For example, the literature on skill-biased technological change and has so far focused on the increased importance of cognitive ability (e.g., Murnane et al 1995). Moreover, the genetic and cultural transmission of noncognitive ability could be important for understanding the intergenerational transmission of inequality (e.g., Bowles and Gintis 2002 and Björklund et al. 2006).

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:

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

For x percent of the observations the computed duration is greater than one and in these cases it is set 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 Definition of parental occupation in 1980

There are no direct information on occupation available in the 1980 wave of LINDA. We therefore 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".

7.5 Occupational choice

We use the information on occupation LINDA contains information on occupation (SSYK96) to assign workers into three broad occupation groups: managers, highskilled workers and lowskilled workers. We use the ten broadest occupational categories in the data, numbered from 0-9. We define men in group 0 (military work) and 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 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".

7.6 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 inlcude the municipalities Malmö, Lund, Trelleborg, Vellinge, Kävlinge, Staffanstorp, Lomma, Svedala, and Burlöv.

8 Appendix B: Tests for selection bias

As already discussed in Section 2, we do not observe wages for the entire sample. One reason we do not observe wages 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 will 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

$$\log w_i - \log w_i^r = \beta_c' c_i + \beta_n' n_i + \mathbf{X}_i \boldsymbol{\gamma}' + u_i > 0 \tag{6}$$

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.[57] We then divide total labor income in 2006 with this number to get an imputed monthly wage.[58] 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.[59] 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 $I_i = 1$,

[57] The details behind our construction of these measures are available in the Appendix.

[58] 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.

[59] 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).

$y_i = 0$ if $I_i = 0$ and $K_i = 0$ and $y_i = 10^8$ if $I_i = 0$ and $K_i = 1$.[60] 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.

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 non-linearity of the inverse Mill ratio to identify $(\beta_c, \beta_n, \gamma)$. 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 B1 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 one and two in Table 2. We first show that the results are very similar for the two other wage measures described in Section 2. We then consider the different methodological approaches outlined above. First, the results from median regressions is displayed in column five and six.[61] As described above, the advantage of median regression over OLS is that it is less sensitive to the imputed values of missing wages. 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 nine and ten give 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 larger than for cognitive ability already in the

[60] 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.

[61] 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.

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 interpreted with caution. In sum, the results in Table B1 do not indicate that our results are driven by selection bias.

9 Appendix C: Measurement error

9.1 Validation from twin data

We describe our adjustment for measurement error in the context of cognitive skills, but the application to noncognitive skills is identical. Consider a simple model where the true level of cognitive skill (c^*) is unobservable but where there is a measure of c such that

$$c = c^* + v$$

where $v \sim N\left(0, \sigma_v^2\right)$ and $Cov\left(c^*, v\right) = 0$. Assuming that the correlation in v within twin pairs is zero, the correlation within MZ twins for c is

$$\rho_{MZ} = \frac{\sigma_{c_1 c_2}}{\sigma_c^2} = \frac{\sigma_{c_1^* c_2^*}}{\sigma_{c^*}^2 + \sigma_v^2}$$

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

$$\sigma_{c^*}^2 + \sigma_v^2 = 1$$

and

$$\rho_{MZ} = \sigma_{c_1^* c_2^*}.$$

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 $\sigma_{v_1 v_2}$ 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

$$\frac{\sigma^2_{c^*} - \sigma_{c^*_1 c^*_2}}{\sigma^2_{c^*} - \sigma_{c^*_1 c^*_2} + \sigma^2_v}.$$

The reliability ratio can be expressed as

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

Now, running a regression of within MZ twin differences in some outcome (like annual earnings) on within MZ twin pair differences in c gives the estimated effect

$$\widetilde{\beta}_{MZ} = \left(\frac{\sigma^2_{c^*} - \sigma_{c^*_1 c^*_2}}{\sigma^2_{c^*} - \sigma_{c^*_1 c^*_2} + \sigma^2_v} \right) \beta_{MZ}$$

where β_{MZ} is the true effect. Rearranging this gives

$$\frac{\widetilde{\beta}_{MZ}}{\beta_{MZ}} = \frac{\sigma^2_{c^*} - \sigma_{c^*_1 c^*_2}}{\sigma^2_{c^*} - \sigma_{c^*_1 c^*_2} + \sigma^2_v}.$$

and

$$\frac{\widetilde{\beta}_{MZ}}{\beta_{MZ}} = \frac{\sigma^2_{c^*} - \rho_{MZ}}{1 - \rho_{MZ}}$$

Rearranging this expression gives

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

or, equivalently

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

Note that the LHS of this expression is equivalent to the reliability ratio of c. Assuming that the true effects $\beta_{MZ} = \beta_{DZ}$ are the same for DZ twins, we get

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

Once we have obtained β_{MZ} (or, equivalently, β_{MZ}), we can also get the reliability ratio. 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

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

We now turn to robustness checks using different a different estimator (median regression) and dependent variable (log wages).

9.2 Measurement error covariance matrix

Assume that all cross-moments between the true variables and the measurement errors are zero. E.g. assume that $E[c^*v_n]=0$. In addition, assume that this also holds for higher moments, e.g. $E[c^*v_c^2]=0, E[(c^*)^2v_c]=0$ and $E[c^*n^*v_c]=0$ upto a total order of three, e.g. $E[x^ky^mz^l]$, for any variables x,y,z and $k+m+l\leq 3$ Also assume that $cov((c^*)^2,v_n^2)=0$ and $cov({v_c}^2,v_n^2)=0$. Though we focus on cognitive ability, the corresponding terms are identical for noncognitive ability.

9.2.1 Variance in measurement error for quadratic terms

Measurement error in quadratic terms is

$$
\begin{aligned}
v_{c^2} &\equiv c^2 - c^{*2} = (c^* + v_c)(c^* + v_c) - c^{*2} \\
&= 2c^* v_c + v_c^2.
\end{aligned}
$$

Notice that: $E[v_{c^2}] = E\left[2c^* v_c + v_c^2\right] = E\left[v_c^2\right] = \sigma_{v_c}^2$.We thus get

$$
\begin{aligned}
Var\left(v_{c^2}\right) &= Var\left(2c^* v_c + v_c^2\right) \\
&= E\left[\left(2c^* v_c + v_c^2 - Ev_{c^2}\right)\left(2c^* v_c + v_c^2 - Ev_{c^2}\right)\right] \\
&= E[4(c^*)^2 v_c^2 + v_c^4 + \sigma_{v_c}^4 + 4c^* v_c^3 - 4c^* v_c \sigma_{v_c}^2 - 2v_c^2 \sigma_{v_c}^2] \\
&= 4E\left[(c^*)^2 v_c^2\right] + E[v_c^4] + \sigma_{v_c}^4 - 2\sigma_{v_c}^2 E[v_c^2] \\
&= 4\sigma_{c^*}^2 \sigma_{v_c}^2 + 3\sigma_{v_c}^4 + \sigma_{v_c}^4 - 2\sigma_{v_c}^4 \\
&= 4\sigma_{c^*}^2 \sigma_{v_c}^2 + 2\sigma_{v_c}^4
\end{aligned}
$$

9.2.2 Covariance in measurement error between linear and quadratic terms

It follows from above that $Cov(v_c, v_{c^2}) = Cov(v_c, 2c^* v_c + v_c^2)$. Thus,

$$
\begin{aligned}
Cov(v_c, v_{c^2}) &= Cov(v_c, 2c^* v_c + v_c^2) \\
&= E[(v_c - Ev_c)(2c^* v_c + v_c^2 - Ev_{c^2})] \\
&= E[v_c(2c^* v_c + v_c^2 - \sigma_{v_c}^2)] \\
&= E[2c^* v_c^2 + v_c^3 - v_c \sigma_{v_c}^2] \\
&= E[2c^* v_c^2] + E[v_c^3]
\end{aligned}
$$

where $E[v_c^3] = 0$ since v_c is distributed as a standard normal. From the definition of variance, $E[2c^* v_c^2] = E[2c^*]E[v_c^2] + cov(2c^*, v_c^2) = E[2c^*]E[v_c^2]$ by assumption of independence with all higher moments. Also notice that $E[c^*] = 0$ according to our normalization. Thus:

$$
Cov(v_c, v_{c^2}) = 0.
$$

9.2.3 Variance in measurement error for interaction term

Measurement error in the interaction term is given by

$$\begin{aligned} v_{cn} &= cn - c^* n^* = c^* n^* - (c^* + v_c)(n^* + v_n) \\ &= c^* v_n + v_c n^* + v_c v_n. \end{aligned}$$

Further,

$$Var(v_{cn}) = E[(v_{cn} - Ev_{cn})(v_{cn} - Ev_{cn})].$$

Since

$$Ev_{cn} = E[c^* v_n + v_c n^* + v_c v_n] = 0$$

we obtain

$$\begin{aligned} Var(v_{cn}) &= E\left[(v_{cn})^2\right] \\ &= E\left[(c^* v_n + v_c n^* + v_c v_n)^2\right] \\ &= E\left[(c^* v_n)^2 + (v_c n^*)^2 + (v_c v_n)^2 + 2c^* n^* v_c v_n + 2n^* v_c v_n^2 + 2c^* v_c v_n^2\right] \\ &= E\left[(c^* v_n)^2 + (v_c n^*)^2 + (v_c v_n)^2\right] \\ &= cov((c^*)^2, v_n^2) + E[(c^*)^2]E[v_n^2] + cov((n^*)^2, v_c^2) \\ &\quad + E[(n^*)^2]E[v_c^2] + cov({v_c}^2, v_n^2) + E[v_c^2]E[v_n^2] \\ &= \sigma_{c^*}^2 \sigma_{v_n}^2 + \sigma_{n^*}^2 \sigma_{v_c}^2 + \sigma_{v_c}^2 \sigma_{v_n}^2 \end{aligned}$$

9.2.4 Covariance in measurement error between linear and interaction terms

Consider the case of the linear term in cognitive ability

$$\begin{aligned} Cov(v_c, v_{cn}) &= E[(v_c - Ev_c)(v_{cn} - Ev_{cn})] \\ &= E[v_c v_{cn}] \\ &= E[v_c (c^* v_n + v_c n^* + v_c v_n)] \\ &= E\left[v_c c^* v_n + v_c^2 n^* + v_c^2 v_n\right] \\ &= 0 \end{aligned}$$

9.2.5 Covariance in measurement error between quadratic and interaction terms

$$
\begin{aligned}
Cov\left(v_{c^2}, v_{cn}\right) &= E\left[\left(v_{c^2} - Ev_{c^2}\right)\left(v_{cn} - Ev_{cn}\right)\right] \\
&= E\left[\left(v_{c^2} - 1\right)\left(v_{cn}\right)\right] \\
&= E\left[v_{c^2} v_{cn} - v_{cn}\right] \\
&= E\left[v_{c^2} v_{cn}\right] \\
&= E\left[\left(2c^* v_c + v_c^2\right)\left(c^* v_n + v_c n^* + v_c v_n\right)\right] \\
&= E\left[2c^* v_c\left(c^* v_n + v_c n^* + v_c v_n\right) + v_c^2\left(c^* v_n + v_c n^* + v_c v_n\right)\right] \\
&= E\left[2c^* v_c c^* v_n + 2c^* v_c v_c n^* + 2c^* v_c v_c v_n + v_c^2 c^* v_n + v_c^2 v_c n^* + v_c^2 v_c v_n\right] \\
&= E\left[2c^{*2} v_c v_n + 5c^* v_c^2 v_n + v_c^3 n^* + v_c^3 v_n\right] \\
&= 0
\end{aligned}
$$

9.2.6 Reliability ratios of higher order terms

We calculate the reliability ratios assuming that both c and n are normally distributed with mean zero and unit variance. Let $v' = \begin{bmatrix} v_c & v_n & v_{c^2} & v_{n^2} & v_{cn} \end{bmatrix}$ be the vector of measurement error terms. The covariance between (observed) c and n, and between c^2 and n^2, is set equal to the covariance observed in the data for the subscore measures (.3235 and .2080, respectively). The variance-covariance measurement error matrix is then

$$
\sum_{vv'} = \begin{bmatrix}
\sigma_{v_c}^2 & 0 & 0 & 0 & 0 \\
0 & \sigma_{v_n}^2 & 0 & 0 & 0 \\
0 & 0 & 4\sigma_{c^*}^2\sigma_{v_c}^2 + 2\sigma_{v_c}^4 & 0 & 0 \\
0 & 0 & 0 & 4\sigma_{n^*}^2\sigma_{v_n}^2 + 2\sigma_{v_n}^4 & 0 \\
0 & 0 & 0 & 0 & \sigma_{c^*}^2\sigma_{v_n}^2 + \sigma_{n^*}^2\sigma_{v_c}^2 + \sigma_{v_c}^2\sigma_{v_n}^2
\end{bmatrix}
$$

which gives the variance-covariance measurement error matrix

$$
\sum_{vv'} = \begin{bmatrix}
0.1325 & 0 & 0 & 0 & 0 \\
0 & 0.2973 & 0 & 0 & 0 \\
0 & 0 & 0.49489 & 0 & 0 \\
0 & 0 & 0 & 1.0125 & 0 \\
0 & 0 & 0 & 0 & 0.3904
\end{bmatrix}
$$

To compute the reliability ratios, we also need the variances of the observed variables. For the quadratic terms, we obtain

$$\begin{aligned}
Var\left(c^2\right) &= E\left[\left(c^2 - Ec^2\right)\left(c^2 - Ec^2\right)\right] \\
&= E\left[\left(c^2 - 1\right)\left(c^2 - 1\right)\right] \\
&= E\left[c^4 - 2c^2 + 1\right] \\
&= E\left[c^4\right] - 2E\left[c^2\right] + 1 \\
&= 3 - 2 + 1 \\
&= 2
\end{aligned}$$

The variance in the interaction term is given by

$$Var\left(cn\right) = E\left[\left(cn - Ecn\right)\left(cn - Ecn\right)\right]$$

Since

$$\begin{aligned}
Ecn &= Cov\left(c,n\right) + E\left[c\right]E\left[n\right] \\
&= Cov\left(c,n\right)
\end{aligned}$$

we get

$$\begin{aligned}
Var\left(cn\right) &= E\left[\left(cn - Cov\left(c,n\right)\right)\left(cn - Cov\left(c,n\right)\right)\right] \\
&= E\left[\left(cn\right)^2 - 2Cov\left(c,n\right)cn + \left[Cov\left(c,n\right)\right]\right] \\
&= E\left(cn\right)^2 - 2E\left[Cov\left(c,n\right)cn\right] + \left[Cov\left(c,n\right)\right]^2 \\
&= E\left(cn\right)^2 - 2E\left[\left(.3235\right)cn\right] + \left(.3235\right)^2 \\
&= E\left(cn\right)^2 - \left(.3235\right)^2 \\
&= E\left(cn\right)^2 - 0.10465
\end{aligned}$$

As

$$\begin{aligned}
Ec^2n^2 &= E\left[c^2\right]E\left[n^2\right] + Cov\left(c^2,n^2\right) \\
&= 1 + Cov\left(c^2,n^2\right) \\
&= 1 + .2080 \\
&= 1.2080
\end{aligned}$$

we get

$$\begin{aligned} Var\,(cn) &= 1.2080 - 0.10465 \\ &= 1.103\,4 \end{aligned}$$

The estimated (theoretical) reliability ratios are then

$$\begin{aligned} c &: 0.8675 \\ n &: 0.70267 \\ c^2 &: \frac{2 - 0.49489}{2} = 0.75256 \\ n^2 &: \frac{2 - 1.0125}{2} = 0.49375 \\ cn &: \frac{1.1034 - 0.39043}{1.1034} = 0.64616 \end{aligned}$$

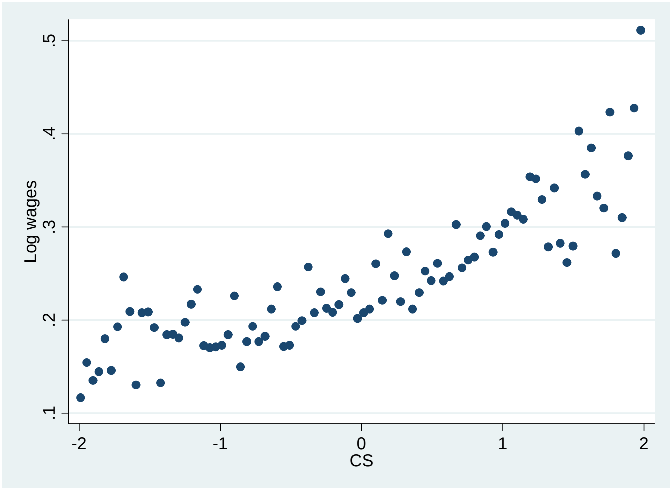
Actual variances in the of the higher order terms in the data when we restrict the sample to men with data on subscores from both cognitive and noncognitive ability:

$$\begin{aligned} \sigma^2_{c^2} &= 1.9124 \\ \sigma^2_{n^2} &= 1.8300 \\ \sigma^2_{cn} &= 1.0466 \end{aligned}$$

10 Appendix D: Alternative cognitive skill measures

Using the sum of subscores for cognitive ability (0-160), we create a dummy variable for each test score and use these as regressors in the standard regression with the large set of control variables and noncognitive skill. Figure D1 plots the estimated coefficients for each dummy variable in the range of -2 to $+2$ standard deviations from the mean. Note that the results are not adjusted for measurement error, which flattens the estimated curvature (Kuha and Temple 2001).

FIGURE D1: NONPARAMETRIC - CS



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12 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 1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max	Comment
Wage in 2006 (W1)	12570	28442	12202	12000	281448	
Imputed wage 1 (W2)	14038	27978	12043	12000	281448	
Imputed wage 2 (W3)	14213	27953	12027	12000	281448	Set to missing in case of zero lagged earnings
Welfare recipient	14703	0.018	0.133	0	1	
Retirement benefits	14703	0.026	0.159	0	1	
Unemployment support	14703	0.092	0.289	0	1	
Any benefit	14703	0.126	0.332	0	1	
Unemployment duration (if > 0)	1174	0.53	0.363	0,003	1	Set to zero in case of zero lagged earnings
Total wage income 2006	14703	319792	206140	0	4589613	
Cognitive skill (*c*)	14703	0.001	1.000	-2.186	1.992	Normalized
Cognitive skill (*c*) – alternative measure	13613	-.000	.999	-3.621	3.796	Based on sum of subscores
Noncognitive skill (*n*)	14703	0.001	1.000	-2.525	2.357	Normalized
Noncognitive skill (*n*) – alternative measure	11960	.000	.985	-2.958	3.102	Based on sum of subscores
Enlisted in the military	14703	0.900	0.300	0	1	
Enlisted as squad leader	14703	0.204	0.403	0	1	
Enlisted as platoon leader	14703	0.088	0.283	0	1	
Geography: Gothenburg	14703	0.054	0.227	0	1	
Geography: Stockholm	14703	.089	.284	0	1	
Geography: Malmo	14703	.201	.400	0	1	
Geography: "Götaland"	14703	.043	.203	0	1	
Geography: "Svealand"	14703	.478	.500	0	1	
Geography: "Norrland"	14703	.397	.489	0	1	
Experience	12752	14.39	5.96	0	25	
Education: Primary school	14656	0.080	0.272	0	1	
Education: Secondary school	14656	0.556	0.497	0	1	
Education: Two years beyond secondary school	14656	0.094	0.292	0	1	
Education: University	14656	0.256	0.436	0	1	
Education: PhD	14656	0.013	0.115	0	1	
Family background: Household income in 1980	14673	1078	588	0	573700	
Family background: Parents married in 1980	14673	0.791	0.406	0	1	
Family background: Father white-collar worker	10771	0.321	0.467	0	1	Coded from industry
Family background: Mother white-collar worker	10886	0.680	0.467	0	1	Coded from industry

Table 2A: Log wages (OLS) - Linear

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cognitive skills	0.086***	0.050***	0.089***	0.054***	0.155***		0.110***	0.079***		
	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)		(0.003)	(0.004)		
Noncognitive skills	0.067***	0.059***	0.102***	0.091***		0.165***	0.106***		0.107***	
	(0.003)	(0.003)	(0.005)	(0.005)		(0.003)	(0.004)		(0.005)	
Constant	10.044***	9.649***	10.133***	10.156***	10.177***	10.175***	10.175***	10.052***	10.202***	10.098***
	(0.012)	(0.026)	(0.015)	(0.023)	(0.002)	(0.003)	(0.002)	(0.024)	(0.028)	(0.024)
Covariate set	Small	Large	Small	Large				Large	Large	Large
Reliability ratio *c*	1.00	1.00	.8675	.8675	.8675		.8675	.8675		
Reliability ratio *n*	1.00	1.00	.70267	.70267		.70267	.70267		.70267	
Observations	13974	12235	13974	12235	14038	14038	14038	12235	12235	12235
R-squared	0.294	0.343	0.323	0.360	0.200	0.180	0.256	0.325	0.347	0.294

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Heteroskedasticity-robust standard errors in (1), (2) and (10), standard errors in (3) - (9) computed with nonparametric bootstrap. Wage measure: W2. Standard ability measures in all regressions.

Table 2B: Log wages (OLS) - Nonlinear

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Cognitive skills	0.084***	0.050***	0.079***	0.045***	0.080***	0.046***	0.079***	0.045***	0.082***	0.047***	0.085***	0.050***	0.082***	0.047***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Cognitive skills sq.			0.016***	0.008***			0.015***	0.007***	0.023***	0.014***			0.019***	0.011***
			(0.002)	(0.002)			(0.002)	(0.002)	(0.003)	(0.004)			(0.005)	(0.004)
Noncog. skills	0.104***	0.092***	0.069***	0.060***	0.068***	0.059***	0.069***	0.060***	0.109***	0.100***	0.102***	0.091***	0.111***	0.101***
	(0.005)	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.006)
Noncog. skills sq.			0.003	0.000			0.001	-0.000	-0.005	-0.013**			-0.016	-0.020*
			(0.002)	(0.002)			(0.002)	(0.002)	(0.005)	(0.005)			(0.011)	(0.012)
Cogn.*Noncog.					0.013***	0.006*	0.006*	0.003			0.019***	0.005	0.017	0.011
					(0.003)	(0.003)	(0.003)	(0.003)			(0.005)	(0.005)	(0.012)	(0.013)
Constant	10.120***	10.156***	10.055***	9.706***	10.066***	9.705***	10.056***	9.706***	10.114***	9.818***	10.107***	9.776***	10.131***	9.830***
	(0.011)	(0.028)	(0.014)	(0.032)	(0.013)	(0.031)	(0.014)	(0.032)	(0.021)	(0.033)	(0.014)	(0.039)	(0.023)	(0.045)
Covariate set	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large
Skill measure	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.	Alt.
Reliability ratio *c*	.8675	.8675	1.00	1.00	1.00	1.00	1.00	1.00	.8675	.8675	.8675	.8675	.8675	.8675
Reliability ratio *c-sq*			1.00	1.00			1.00	1.00	.75256	.75256			.75256	.75256
Reliability ratio *n*	.70267	.70267	1.00	1.00	1.00	1.00	1.00	1.00	.70267	.70267	.70267	.70267	.70267	.70267
Reliability ratio *n-sq*			1.00	1.00			1.00	1.00	.49375	.49375			.49375	.49375
Reliability ratio *cn*					1.00	1.00	1.00	1.00			.64616	.64616	.64616	.64616
Observations	11080	9743	11080	9743	11080	9743	11080	9743	11080	9743	11080	9743	11080	9743
R-squared	0.314	0.351	0.290	0.335	0.287	0.334	0.290	0.335	0.321	0.354	0.316	0.352	0.322	0.355

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Heteroskedasticity-robust standard errors in (3) - (8), standard errors in (1), (2) and (9) - (14) computed with nonparametric bootstrap. Wage measure: W2.

Table 3: Probability of unemployment support (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Cognitive skills	-0.015***	-0.012***	-0.012***	-0.011**	-0.025***		-0.020***		-0.015***	-0.012***	-0.013***	-0.010**
	(0.003)	(0.003)	(0.004)	(0.005)	(0.003)		(0.003)		(0.003)	(0.003)	(0.003)	(0.004)
Cognitive skills sq.									0.004*	0.002	0.003	0.000
									(0.002)	(0.002)	(0.003)	(0.003)
Noncognitive skills	-0.024***	-0.021***	-0.039***	-0.033***		-0.045***		-0.036***	-0.027***	-0.024***	-0.046***	-0.044***
	(0.003)	(0.003)	(0.004)	(0.005)		(0.004)		(0.004)	(0.003)	(0.004)	(0.006)	(0.007)
Noncognitive skills sq.									0.008***	0.007***	0.021***	0.021***
									(0.002)	(0.002)	(0.006)	(0.006)
Constant	0.217***	0.442***	0.116***	0.216***	0.160***	0.116***	0.255***	0.207***	0.173***	0.397***	0.122***	0.323***
	(0.016)	(0.039)	(0.014)	(0.024)	(0.015)	(0.014)	(0.024)	(0.025)	(0.018)	(0.043)	(0.021)	(0.058)
Covariate set	Small	Large	Small	Large	Small	Small	Large	Large	Small	Large	Small	Large
Skill measure	Std.	Std.	Std.	Std.	Std.	Std.	Std.	Std.	Alt.	Alt.	Alt.	Alt.
Reliability ratio *c*	1.00	1.00	.8675	.8675	.8675		.8675		1.00	1.00	.8675	.8675
Reliability ratio *c-sq*									1.00	1.00	.75256	.75256
Reliability ratio *n*	1.00	1.00	.70267	.70267		.70267		.70267	1.00	1.00	.70267	.70267
Reliability ratio *n-sq*									1.00	1.00	.49375	.49375
Observations	14626	12726	14626	12726	14626	14626	12726	12726	11553	10104	11553	10104
R-squared	0.030	0.042	0.033	0.044	0.026	0.032	0.038	0.044	0.033	0.043	0.039	0.048

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Heteroskedasticity-robust standard errors in (1) - (2) and (9) - (10) , standard errors in (3) - (8) and (11) – (12) computed with nonparametric bootstrap.

Table 4: Probability of any form of social assistance (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Cognitive skills	-0.023***	-0.016***	-0.018***	-0.014***	-0.042***		-0.030***		-0.024***	-0.017***	-0.019***	-0.014***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)		(0.004)		(0.003)	(0.004)	(0.004)	(0.004)
Cognitive skills sq.									0.009***	0.007***	0.008***	0.005
									(0.002)	(0.002)	(0.003)	(0.004)
Noncognitive skills	-0.042***	-0.035***	-0.068***	-0.056***		-0.077***		-0.060***	-0.042***	-0.037***	-0.075***	-0.069***
	(0.003)	(0.003)	(0.005)	(0.005)		(0.005)		(0.005)	(0.004)	(0.004)	(0.006)	(0.009)
Noncognitive skills sq.									0.016***	0.014***	0.043***	0.039***
									(0.003)	(0.003)	(0.006)	(0.008)
Constant	0.280***	0.630***	0.199***	0.292***	0.275***	0.199***	0.358***	0.280***	0.249***	0.570***	0.156***	0.443***
	(0.018)	(0.045)	(0.017)	(0.030)	(0.013)	(0.018)	(0.028)	(0.028)	(0.020)	(0.050)	(0.030)	(0.059)
Covariate set	Small	Large	Small	Large	Small	Small	Large	Large	Small	Large	Small	Large
Skill measure	Std.	Std.	Std.	Std.	Std.	Std.	Std.	Std.	Alt.	Alt.	Alt.	Alt.
Reliability ratio *c*	1.00	1.00	.8675	.8675	.8675		.8675		1.00	1.00	.8675	.8675
Reliability ratio *c-sq*									1.00	1.00	.75256	.75256
Reliability ratio *n*	1.00	1.00	.70267	.70267		.70267		.70267	1.00	1.00	.70267	.70267
Reliability ratio *n-sq*									1.00	1.00	.49375	.49375
Observations	14626	12726	14626	12726	14626	14626	12726	12726	11553	10104	11553	10104
R-squared	0.060	0.059	0.068	0.065	0.050	0.066	0.051	0.064	0.063	0.061	0.078	0.073

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Heteroskedasticity-robust standard errors in (1) - (2) and (9) - (10), standard errors in (3) - (8) and (11) – (12) computed with nonparametric bootstrap.

Table 5: Unemployment duration

	Exponential		Weibull		OLS		OLS	
Cognitive skill	1.012	0.928	1.012	0.928	-0.003	0.017	0.005	0.030*
	(0.038)	(0.043)	(0.038)	(0.043)	(0.012)	(0.014)	(0.017)	(0.018)
Noncognitive skill	1.139***	1.173***	1.137***	1.173***	-0.037***	-0.045***	-0.063***	-0.077***
	(0.043)	(0.049)	(0.043)	(0.049)	(0.012)	(0.013)	(0.023)	(0.021)
Covariate set	Small	Large	Small	Large	Small	Large	Small	Large
Reliability ratio *c*	-	-	-	-	1.00	1.00	.8675	.8675
Reliability ratio *n*	-	-	-	-	1.00	1.00	.70267	.70267
Observations	1173	926	1173	926	1173	926	1173	926
R-squared	-	-	-	-	0.023	0.040	0.028	0.049

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Heteroskedasticity-robust standard errors in OLS without adjustment for measurement error. Standard errors in OLS with measurement error adjustment computed with nonparametric bootstrap. Standard ability measures in all regressions.

Table 6A: Annual earnings in SEK (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Cognitive skills	32,791***	17,931***	31,293***	16,823***	51,407***		31,683***		29,856***	16,371***	29,023***	15,837**
	(1,751)	(2,173)	(2,107)	(2,702)	(1,913)		(2,948)		(1,968	(2,519)	(2,489)	(3,211)
Cognitive skills sq.									4,885**	1,668	7,653**	3,786**
									(1,369)	(1,517)	(2,149)	(1,782
Noncognitive skills	37,148***	33,118***	59,494***	52,490***		74,369***		57,569***	36,230*	31,918**	57,946***	52,139***
	(1,947)	(2,098)	(3,170)	(3,485)		(2,603)		(3,328	(1,939)	(1,998)	(3,358)	(3,679)
Noncognitive skills sq.									665	660	-3,453	-4,626
									(1,591)	(1,751	(4,536)	(3,874)
Constant	230,848***	35,534*	309,808***	255,835***	243,371***	308,657***	193,890*	270,305**	257,549***	49,272*	290,229**	101,541**
	(8,550)	(18,910)	(9,279)	(20,529.804)	(7,572)	(10,515)	(19,692)	(20,951)	(9,928)	(21,094)	(13,448)	(24,989)
Covariate set	Small	Large	Small	Large	Small	Small	Large	Large	Small	Large	Small	Large
Skill measure	Std.	Std.	Std.	Std.	Std.	Std.	Std.	Std.	Alt.	Alt.	Alt.	Alt.
Reliability ratio *c*	1.00	1.00	.8675	.8675	.8675		.8675		1.00	1.00	.8675	.8675
Reliability ratio *c-sq*									1.00	1.00	.75256	.75256
Reliability ratio *n*	1.00	1.00	.70267	.70267		.70267		.70267	1.00	1.00	.70267	.70267
Reliability ratio *n-sq*									1.00	1.00	.49375	.49375
Observations	14626	12726	14626	12726	14626	14626	12726	12726	11553	10104	11553	10104
R-squared	0.168	0.189	0.187	0.201	0.151	0.173	0.173	0.198	0.159	0.179	0.177	0.191

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Heteroskedasticity-robust standard errors in (1) - (2) and (9) - (10), standard errors in (3) - (8) and (11) – (12) computed with nonparametric bootstrap.

Table 6B: Log of annual earnings, sample truncated at 120,000 SEK (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Cognitive skills	0.082***	0.050***	0.084***	0.052***	0.122***		0.080***		0.074***	0.044***	0.074***	0.044***
	(0.003)	(0.004)	(0.005)	(0.005)	(0.004)		(0.005)		(0.004)	(0.004)	(0.005)	(0.005)
Cognitive skills sq.									0.013***	0.006**	0.020***	0.012***
									(0.002)	(0.003)	(0.004)	(0.004)
Noncognitive skills	0.075***	0.068***	0.116***	0.106***		0.154***		0.121***	0.077***	0.070***	0.124***	0.118***
	(0.004)	(0.004)	(0.007)	(0.007)		(0.005)		(0.005)	(0.004)	(0.004)	(0.007)	(0.007)
Noncognitive skills sq.									0.000	-0.001	-0.014*	-0.019**
									(0.003)	(0.003)	(0.008)	(0.009)
Constant	12.554***	12.109***	12.675***	12.614***	12.555***	12.671***	12.500***	12.658***	12.563***	12.195***	12.638***	12.325***
	(0.015)	(0.037)	(0.019)	(0.038)	(0.020)	(0.017)	(0.035)	(0.032)	(0.020)	(0.045)	(0.029)	(0.055)
Covariate set	Small	Large	Small	Large	Small	Small	Large	Large	Small	Large	Small	Large
Skill measure	Std.	Std.	Std.	Std.	Std.	Std.	Std.	Std.	Alt.	Alt.	Alt.	Alt.
Reliability ratio *c*	1.00	1.00	.8675	.8675	.8675		.8675		1.00	1.00	.8675	.8675
Reliability ratio *c-sq*									1.00	1.00	.75256	.75256
Reliability ratio *n*	1.00	1.00	.70267	.70267		.70267		.70267	1.00	1.00	.70267	.70267
Reliability ratio *n-sq*									1.00	1.00	.49375	.49375
Observations	13229	11678	13229	11678	13229	13229	11678	11678	10477	9280	10477	9280
R-squared	0.213	0.249	0.236	0.264	0.197	0.207	0.231	0.256	0.204	0.236	0.228	0.253

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Heteroskedasticity-robust standard errors in (1) - (2) and (9) - (10), standard errors in (3) - (8) and (11) – (12) computed with nonparametric bootstrap.

Table 7: Probability of annual labor market earnings above different percentiles of the annual earnings distribution (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Percentile	5		10		25		50		75		90		95	
Cognitive skills	0.003	0.000	0.010***	0.002	0.045***	0.031***	0.091***	0.060***	0.103***	0.061***	0.044***	0.024***	0.020***	0.011***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.007)	(0.007)	(0.006)	(0.005)	(0.006)	(0.004)	(0.004)	(0.002)	(0.003)
Noncognitive skills	0.039***	0.031***	0.056***	0.047***	0.100***	0.087***	0.138***	0.121***	0.105***	0.092***	0.062***	0.056***	0.037***	0.033***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)	(0.006)	(0.005)	(0.003)	(0.004)
Constant	0.896***	0.871***	0.847***	0.784***	0.708***	0.620***	0.500***	0.446***	0.259***	0.229***	0.116***	0.062*	0.059***	0.003
	(0.010)	(0.018)	(0.016)	(0.026)	(0.018)	(0.031)	(0.019)	(0.034)	(0.014)	(0.036)	(0.014)	(0.036)	(0.008)	(0.025)
Covariate set	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large
Reliability ratio *c*	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675
Reliability ratio *n*	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267
Observations	14626	12726	14626	12726	14626	12726	14626	12726	14626	12726	14626	12726	14626	12726
R-squared	0.046	0.035	0.055	0.043	0.092	0.086	0.157	0.166	0.204	0.235	0.144	0.170	0.092	0.112

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors computed with nonparametric bootstrap in parentheses. Standard ability measures in all regressions.

Table 8: Changes in conditional percentiles of annual earnings (quantile regression)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Percentile	10		25		50		75		90	
Cognitive skills	19,423***	13,642***	18,863***	12,079***	24,549***	14,494***	33,411***	18,636***	44,168***	21,449***
	(3,293)	(3,726)	(1,429)	(1,640)	(992)	(1,187)	(1,495)	(1,512)	(2,663)	(2,535)
Noncognitive skills	37,974***	33,458***	24,631**	22,381***	25,696***	21,832*	30,903***	26,074***	43,729***	35,804***
	(3,526)	(3,522)	(1,490)	(1,513)	(1,018)	(1,089)	(1,498)	(1,388)	(2,669)	(2,372)
Constant	83,645***	-31,302	194,329***	205,821***	280,943***	279,860***	355,318***	343,820***	445,262***	430,955***
	(15,105)	(28,754)	(6,768)	(13,023)	(4,689)	(9,519)	(6,977)	(12,114)	(12,395)	(20,816)
Covariate set	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large
Observations	14626	12726	14626	12726	14626	12726	14626	12726	14626	12726

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Coefficients not adjusted for measurement error.

Table 9: Occupational choice

	N	Cognitive skill	Noncognitive skill
Managers	1,011	.43	.55
Highskilled workers	5,185	.50	.32
Lowskilled workers	6,098	-.44	-.27

Table 10: Occupational choice, marginal effects from multinomial logit

	Managers		Highskilled		Lowskilled	
Cognitive skills	0.027***	0.020***	0.173***	0.116***	-0.200***	-0.137***
	(0.003)	(0.004)	(0.007)	(0.008)	(0.007)	(0.008)
Noncognitive skills	0.044***	0.045***	0.087***	0.066***	-0.131***	-0.111***
	(0.003)	(0.004)	(0.006)	(0.007)	(0.007)	(0.007)
Covariate set	Small	Large	Small	Large	Small	Large
Observations	12,274	10,831	12,274	10,831	12,274	10,831

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. Coefficients not adjusted for measurement error. Standard ability measures in all regressions.

Table 11: Occupation specific skill prices

	Managers				Qualified workers				Unqualified workers			
	(1) OLS	(2) OLS	(3) Lee	(4) BFG	(5) OLS	(6) OLS	(7) Lee	(8) BFG	(9) OLS	(10) OLS	(11) Lee	(12) BFG
Cognitive skills	0.041***	0.046**	0.059***	0.068**	0.056***	0.064***	0.080***	0.089***	0.012***	0.010***	0.016	0.026
	(0.015)	(0.017)	(0.019)	(0.028)	(0.005)	(0.006)	(0.008)	(0.012)	(0.003)	(0.004)	(0.013)	(0.020)
Noncognitive skills	0.051***	0.076***	0.107***	0.095**	0.047***	0.069***	0.061***	0.061***	0.027***	0.041***	0.033***	0.042***
	(0.013)	(0.021)	(0.031)	(0.048)	(0.004)	(0.005)	(0.007)	(0.013)	(0.003)	(0.005)	(0.010)	(0.011)
Constant	10.261***	10.405***	9.296***	10.014***	9.823***	10.132***	9.303***	9.430***	10.064***	10.062***	9.954***	10.024***
	(0.121)	(0.121)	(0.466)	(0.684)	(0.086)	(0.032)	(0.369)	(0.313)	(0.178)	(0.177)	(0.102)	(0.202)
Covariate set	Large	Large	Large	Large	Large	Large	Large	Large	Large	Large	Large	Large
Reliability ratio *c*	1.00	.8675	1.00	1.00	1.00	.8675	1.00	1.00	1.00	.8675	1.00	1.00
Reliability ratio *n*	1.00	.70267	1.00	1.00	1.00	.70267	1.00	1.00	1.00	.70267	1.00	1.00
Observations	943	943	595	595	4865	4865	2994	2994	5023	5023	2686	2686
R-squared	0.338	0.345			0.269	0.281			0.058	0.066		

*** p<0.01, ** p<0.05, * p<0.1. Standard errors computed with nonparametric bootstrap, except for OLS without measurement error adjustment which has heteroskedasticity-robust standard errors. Standard ability measures in all regressions.

Table 11, cont: Selection stage

No observation on occupation	Cognitive skill	-0.236***	(0.051)
	Noncognitive skill	-0.290***	(0.046)
	Region 1980: Gothenburg	0.400*	(0.217)
	Region 1980: Stockholm	0.203	(0.166)
	Region 1980: Malmö	0.721***	(0.237)
	Region 1980: Götaland	0.324	(0.205)
	Region 1980: Svealand	0.427**	(0.201)
	Mother white-collar 1980	-0.203**	(0.087)
	Father white-collar 1980	-0.072	(0.086)
Managers	Cognitive skill	-0.012	(0.062)
	Noncognitive skill	0.351***	(0.055)
	Region 1980: Gothenburg	-0.396	(0.281)
	Region 1980: Stockholm	0.144	(0.187)
	Region 1980: Malmö	0.174	(0.283)
	Region 1980: Götaland	0.563***	(0.213)
	Region 1980: Svealand	0.110	(0.220)
	Mother white-collar 1980	-0.200*	(0.103)
	Father white-collar 1980	-0.192*	(0.100)
Unqualified workers	Cognitive skill	-0.467***	(0.208)
	Noncognitive skill	-0.350***	(0.155)
	Region 1980: Gothenburg	0.268	(0.236)
	Region 1980: Stockholm	0.037	(0.182)
	Region 1980: Malmö	0.144	(0.178)
	Region 1980: Götaland	0.246	(0.073)
	Region 1980: Svealand	0.453**	(0.074)
	Mother white-collar 1980	-0.154**	(0.208)
	Father white-collar 1980	-0.099	(0.155)

Notes: The excluded category is qualified workers. The results for the large set of control variables have been excluded from the Table due to space considerations. Standard errors in parenthesis.

Table 12: Controlling for type of military service (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variables	Log wages (W2)	Log wages (W2)	Log wages (W2)	Log wages (W2)	Unempl. support	Unempl. support	Any social benefit	Any social benefit	Annual earnings	Annual earnings	Earnings >10 perc.	Earnings >10 perc.
Cognitive skills	0.075***	0.044***	0.087***	0.053***	-0.016***	-0.013***	-0.028***	-0.020***	31,142***	17,012***	0.020***	0.008*
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.006)	(2,751)	(3,159)	(0.004)	(0.005)
Noncognitive skills	0.052***	0.049***	0.097***	0.090***	-0.049***	-0.039***	-0.090***	-0.071***	58,799***	52,691***	0.079***	0.064***
	(0.003)	(0.003)	(0.005)	(0.006)	(0.005)	(0.006)	(0.007)	(0.007)	(3,870)	(3,584)	(0.007)	(0.007)
Constant	10.030***	9.640***	10.155***	10.179***	0.104***	0.209***	0.172***	0.275***	309,271***	255,844***	0.874***	0.804***
	(0.012)	(0.026)	(0.014)	(0.027)	(0.015)	(0.026)	(0.017)	(0.025)	(8,456)	(19,259)	(0.014)	(0.024)
Covariate set	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large
Reliability ratio *c*	1.00	1.00	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675	.8675
Reliability ratio *n*	1.00	1.00	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267	.70267
Observations	13974	12235	13974	12235	14626	12726	14626	12726	14626	12726	14626	12726
R-squared	0.301	0.346	0.324	0.361	0.035	0.045	0.073	0.068	0.187	0.201	0.061	0.048

*** p<0.01, ** p<0.05, * p<0.1. Standard errors computed with nonparametric bootstrap, except for OLS without measurement error adjustment which has heteroskedasticity-robust standard errors. Standard ability measures in all regressions.

Table B1: Controlling for selection bias in regression of log wages

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Median	(6) Median	(7) IAI	(8) IAI	(9) Heckman		(10) Heckman	
Wage measure	W1	W1	W3	W3	W2q	W2q	W2	W2	W2	*Select*	W2	*Select*
Cognitive skills	0.086***	0.051***	0.084***	0.049***	0.083***	0.050***	0.092***	0.052***	0.088***	0.025	0.050***	-0.018
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.021)	(0.003)	(0.026)
Noncognitive skills	0.065***	0.057***	0.066***	0.059***	0.059***	0.051***	0.071***	0.062***	0.087***	0.206***	0.062***	0.167***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.013)	(0.022)	(0.006)	(0.024)
Constant	10.094***	9.669***	10.051***	9.744***	9.976***	9.625***	10.085***	9.767***	9.943***	1.659***	10.140***	1.387***
	(0.012)	(0.027)	(0.012)	(0.029)	(0.013)	(0.028)	(0.012)	(0.033)	(0.050)	(0.086)	(0.042)	(0.237)
Covariate set	Small	Large	Small	Large	Small	Large	Small	Large	Small	Small	Small	Small
Observations	12545	11036	14150	12384	14626	12726	13760	12163	14629	14629	12729	12729
R-squared	0.298	0.346	0.290	0.339			0.279	0.336				

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses. Coefficients not adjusted for measurement error. Standard ability measures in all regressions.