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

By Erik Lindqvist and Roine Vestman*

We use data from the Swedish military enlistment to assess the importance of cognitive and noncognitive ability for labor market outcomes. The measure of noncognitive ability is based on a personal interview conducted by a psychologist. We find strong evidence that men who fare poorly in the labor market—in the sense of unemployment or low annual earnings—lack noncognitive rather than cognitive ability. However, cognitive ability is a stronger predictor of wages for skilled workers and of earnings above the median. (JEL J24, J31, J45)

For the vast majority of people, labor market earnings are 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., Richard J. Herrnstein and Charles Murray 1994). An alternative view holds that noncognitive abilities such as persistence, motivation, emotional stability, or social skills are equally or more important (e.g., Samuel Bowles and Herbert Gintis 1976; Christopher Jencks 1979; Bowles, Gintis, and Melissa Osborne 2001a; James J. Heckman, Jora Stixrud, and Sergio 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 modest fraction of the variance

in earnings.[1] On the other hand, the estimated effect of noncognitive ability on outcomes varies substantially in the literature and is often small compared to the effect of cognitive ability. However, inference about the importance of noncognitive ability is difficult due to a lack of valid measures. Most studies in psychology and economics use measures of noncognitive abilities and related personality traits based on self-reported questionnaires. Compared to IQ tests, such measures are less reliable and less precise (Lex Borghans et al. 2008). In addition, the valuation of cognitive and noncognitive ability is likely to differ across sectors and occupations.

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

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

By contrast, cognitive ability is a stronger predictor of wages than noncognitive ability. In our basic specification, a 1 standard deviation increase in cognitive ability predicts an increase in wages by 8.9 percent, compared to 6.9 percent for noncognitive ability. However, while log wages are linear in noncognitive ability, they are strictly convex in cognitive ability with a low marginal product for low levels of ability. Relatedly, we find that noncognitive ability has a higher return than cognitive ability for unskilled workers and managers, while skilled workers in nonmanagerial positions face a higher return to cognitive than to noncognitive ability. In sum, our results support the view that a certain level of noncognitive ability is a prerequisite

for avoiding failure in the labor market whereas cognitive ability is at least as important for achieving success.

Our paper is related to the small but expanding literature on personality and socioeconomic outcomes initiated by Bowles and Gintis (1976); Richard C. Edwards (1976); Paul J. Andrisani and Gilbert Nestel (1976); and Jencks (1979).[2] The majority of these papers use measures of personality based on self-reported questionnaires. For example, measures of self-esteem (Arthur H. Goldsmith, Jonathan R. Veum, and William R. Darity, Jr. 1997; Murnane et al. 2001), withdrawal and aggression (Melissa Osborne Groves 2005), Machiavellism (Charles F. Turner and Daniel C. Martinez 1977), and sense of personal control over outcomes in life (Andrisani and Nestel 1976; Greg J. Duncan and Jamen N. Morgan 1981; Rachel Dunifon and Duncan 1998) have been found to predict wages or occupational status. There is also an extensive literature on the predictive power of various personality measures from the psychology literature, such as the five factor model (see Borghans et al. 2008 for a survey and Gerrit Mueller and Erik Plug (2006) for a recent contribution in the economics literature).

Another strand of the literature infers noncognitive ability from observable choices or behaviors. Heckman and Yona 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, something which Heckman and Rubinstein (2001) attribute to low noncognitive ability. Relatedly, Heckman, Stixrud, and Urzua (2006) infer cognitive and noncognitive ability by a latent factor model estimated on NLSY data, whereas Peter Kuhn and Catherine Weinberger (2005) use leadership positions in clubs or sports teams in high school as indicators of leadership ability. More recently, Carmit Segal (2009) uses teacher evaluations of student classroom behavior in eighth grade as a measure of noncognitive ability. To the best of our knowledge, our paper is the first in this literature to consider a measure of noncognitive ability based on a personal interview.[3]

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

The paper proceeds as follows. Our data and measures of cognitive and noncognitive ability are presented in Section I. We discuss our estimation strategy in

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

[3] A number of papers have used the Swedish enlistment data in other contexts. Erik Grönqvist and Jonas Vlachos (2008) test how cognitive and noncognitive ability affect teacher performance. Martin Nordin (2008) studies how the returns to schooling interact with cognitive ability. Petter Lundborg, Paul Nystedt, and Dan-Olof Rooth (2009) show that controlling for the cognitive and noncognitive ability measures reduces the earnings premium associated with height.

[4] See, for example, Borghans et al. (2008). A related conceptual issue is whether noncognitive skills are most accurately described as skills or preferences. Bowles, Gintis, and Osborne (2001b) argue that character traits like persistence or dependability could be viewed as incentive-enhancing preferences which employers value in the face of incomplete labor contracts.

Section II and provide the results for wages, employment, and earnings in Section III. Section IV discusses how these results relate to the previous literature. Section V concludes the paper. Basic facts regarding the data and construction of variables are available in Appendix A at the end of the paper. Appendix B (sample selection), Appendix C (measurement error), Appendix D (additional results), Appendix E (occupational choice), and Appendix F (additional material regarding our skill measures) are available online.

I. Data

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

LINDA is a panel dataset that covers 3 percent of the Swedish population annually. The starting point for LINDA is a representative, random sample of the Swedish population in 1994 which has been tracked from 1968 to 2007. New individuals are added to the database each year to ensure that LINDA is also cross-sectionally representative.[5] Each wave of LINDA contains information on taxable income and social benefits (e.g., unemployment support) from the Income Registers in a given year. In addition, LINDA contains information on occupation, wages, and educational attainment from separate registers held by Statistics Sweden. For each year, information on all family members of the sampled individuals are added to the dataset, but we focus on the core sample of randomly selected individuals. We focus on labor market outcomes in 2006, but provide additional analysis using data from several years in Appendix B.

The first cohort for which we have enlistment data is men born in 1965 (enlisted in 1983 and 1984). In comparison to the Anglo-Saxon countries, many Swedes with higher education enter the labor market late in life. For this reason, we do not consider men born after 1974, implying that the youngest men in our data were 32 years old in 2006. We also exclude men born outside of Sweden; men with an incomplete record from the military enlistment or enlistment after 1993; self-employed men (defined as an annual business income above 10,000 Swedish krona (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.[6] We provide a robustness analysis of these sample restrictions in Appendix B.[7]

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[5] Per-Anders Edin and Peter Fredriksson (2000) provide a detailed account of the data collection process for LINDA.

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

[7] For natural reasons, it is not possible to conduct a robustness test for the 9 percent of conscripts for which we lack information on cognitive and noncognitive skills. These are men who were exempted from the draft altogether,

A. *Socioeconomic Variables in LINDA*

LINDA is complete with respect to taxable income and social benefits, but the wage registers are not complete for the private sector. In total, we have data on wages in 2006 for 12,570 workers which corresponds to 85.5 percent of our sample. The remaining group consists both of people with no or limited participation in the labor market (e.g., people who were unemployed 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.[8] Using wages from previous years, we are able to add information on wages for 1,401 men, bringing the total number to 14,038, or 95.5 percent of our sample. This imputation technique rests on the assumption that men whose wages were not observed in 2006 experienced no change in productivity between 2006 and the year of the latest wage observation. We provide robustness checks regarding the imputation of wages and estimation techniques that adjust for selection bias in Appendix B.

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

We construct five dummy variables for educational attainment from the information in LINDA: only primary school (9 years), secondary school (11–12 years), two years education beyond secondary school, university degree, and a PhD. 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 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.[9]

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

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

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

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

B. *The Enlistment Data*[10]

The military enlistment usually takes place the year a Swedish man turns 18 or 19 years old.[11] The enlistment procedure spans two days involving tests of medical status, physical fitness, cognitive ability, and an interview with a psychologist. For the period we consider, almost all men who were not given a low health rating were enlisted to the military service.[12] Importantly, it was not possible to avoid the military service by obtaining a low score on cognitive or noncognitive ability, though test scores predict the precise type of service to which conscripts were enlisted.[13] In total, 90 percent of the men in our sample were enlisted to the military service.

The majority of enlisted men start their military service upon graduation from secondary school. The mean age at the onset of the military service is 20.3 years in our sample, and only 2 percent of enlisted men were more than 22 years old (see Figure B1). About 10 percent of enlisted men do not enter into the military service. Attrition from the military service is unrelated to cognitive ability, but men with high noncognitive ability are significantly more likely to actually start the military service conditional on being enlisted. However, attrition is unrelated to educational attainment and wages conditional on skills (see Table B6). The duration of the military service time varies between 7 and 18 months depending on type of service. Service time is typically 7 or 8 months for privates (67 percent of enlisted men in our sample), 10 months for squad leaders (23 percent), and 12–18 months for men enlisted as platoon leaders (10 percent). The majority of men leave the military after the mandatory military service. In our sample, only 0.69 percent had a military career as of 2006.

Measure of Cognitive Ability.—The Swedish military has conducted tests of conscripts' cognitive skills since the mid 1940s. These tests have changed several times over the years, but the men in our sample all did the same test.[14] This test consists of four different parts (synonyms; inductions; metal folding; and technical comprehension). Each part contains 40 questions and is graded on a scale from 1 to 9. The results of these tests are then transformed to a discrete variable of general cognitive

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[10] 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 Swedish National Service Administration (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.

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

[12] A linear regression of a dummy for "enlisted to the military service" on a set of health classification dummies has an R^2 of 0.73. Among the men in the highest health category (A), 96.5 percent were enlisted compared to none of the men in the second lowest and lowest health categories (Y and Z).

[13] Once health status is controlled for, cognitive ability is not a statistically significant predictor of enlistment. The score on noncognitive ability is statistically significant at the 5 percent level, but the estimated effect is weak. We provide results regarding the selection into the military service in Appendix B.

[14] See Berit 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 (C. Spearman 1904). In this sense, the test of cognitive skills at the military enlistment differs from AFQT, which focuses more on "crystallized" intelligence, i.e., skills that are teachable (interview with Berit Carlstedt November 26, 2009). See M. Rebecca Kilburn, Lawrence M. Hanser, and Jacob A, Klerman (1998) for a description of the AFQT.

ability also ranging from 1 to 9. This variable follows a Stantine scale that approximates a normal distribution.

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

We also construct an alternative measure of cognitive ability from the sum of the scores on each subtest, which ranges from 4 to 36. The sum of the subscores is percentile rank-transformed and then converted by taking the inverse of the standard normal distribution to produce normally distributed test scores. This measure has a more continuous distribution and higher moments closer to a normal distribution with unit variance. The main reason to focus on the first rather than the second measure is that data on the subscores underlying the general score is only available for 13,278 out of 14,703 observations in our data. As shown in Appendix F, the results do not change appreciably depending on which measure we use.

Measure of Noncognitive Ability.—Like the test of cognitive skills, personality tests were introduced at the military enlistment in the early 1940s by Torsten Husén, a prolific writer in the field of military psychology. This development was inspired by the extensive testing procedure that Germany had built up during the 1930s 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. 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 United Nations missions (interview with Johan Lothigius 2004).

All the men in our data had their psychological profiles evaluated according to a procedure that was adopted in 1969 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. As a basis for the interview, the psychologist has information about the conscript's results on the test of cognitive ability, physical endurance, muscular strength, grades from school, and the answers to 70–80 questions about friends, family, and hobbies, etc. The interview is semi-structured in the sense that the psychologist has to follow a manual that states certain topics to be discussed, though specific questions are not decided beforehand. We provide more information regarding the enlistment interview in Appendix F.

The objective of the interview is to assess the conscript's ability to cope with the psychological requirements of the military service and, in the extreme case, war. The psychologists assign each conscript's military aptitude a score from 1 to 9, which follows the same Stantine distribution as the final test score for cognitive ability.[15] This score is in turn based on four different subscores which range from

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[15] In addition, leadership skills are estimated for those who score 5 or higher on the test of cognitive ability. In practice, the assessment of ability to cope with war stress and leadership skills are highly correlated in the data (0.88).

1 to 5. The subscores function only as a guide to the psychologists—two conscripts with the same sequence of subscores could still get different final scores.[16] 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 of four subscores and convert it into an approximately normally distributed variable using the same procedure as for cognitive ability. As for cognitive ability, the subscores are not available for the entire sample, and we therefore only report the results for the second measure in Appendix F. In practice, the two measures of noncognitive ability are highly correlated (0.97), and the results do not change appreciably depending on which measure we use.

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

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

Another explicit objective with the interview is to identify people who are particularly unsuited for 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, 9). The difficulty in assessing people with antisocial personality disorders is one

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

reason why the SNSA relies on interviews rather than questionnaires. In particular, psychopaths with high intelligence could trick a questionnaire and give answers that they know will increase their chances of obtaining military command (Andersson and Carlstedt 2003, 11). Other personality traits considered negative are difficulty accepting authority and to adjust to a different environment, and violent or aggressive behavior (Andersson and Carlstedt 2003, 13).

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

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

II. 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

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

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

$$(2) \qquad\qquad f(c_i, n_i) = \beta_c c_i + \beta_n n_i.$$

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

First, there is a direct correspondence between the distributional assumptions on c_i and n_i and the estimated functional form of $f(c_i, n_i)$. In our case, $f(c_i, n_i)$ is estimated under the assumption that cognitive and noncognitive ability are normally

distributed, in accordance with SNSA's variable construction.[17] We believe this assumption is a reasonable benchmark case which has the advantage of making our results comparable to other measures of ability, such as IQ, which are also normally distributed by assumption.

Second, our measures of cognitive and noncognitive ability are positively correlated (0.388), and the way we think about this covariance affects our interpretation of the estimates of β_c and β_n.[18] If the cognitive test score reflects noncognitive ability, controlling for cognitive ability will bias the estimated effect of noncognitive ability, and vice versa. Both directions of causality are plausible. On one hand, the psychologists know the conscripts' results on the test of cognitive ability before conducting the interview. The cognitive skill score could thus directly affect the psychologists' assessment of noncognitive ability. Using a fifth-order polynomial in the sum of subscores as a control for cognitive ability, we show in Appendix F that an increase in the final cognitive ability test score by one point on the 1–9 scale increases estimated noncognitive ability by 0.11 points on average. This would imply a correlation of 0.11 if it were the only source of covariance between cognitive and noncognitive ability. On the other hand, noncognitive skills have been shown to influence performance on tests of cognitive ability (Borghans, Huub Meijers, and Bas ter Weel 2008, and Segal 2008). Moreover, noncognitive abilities could facilitate the acquisition of cognitive abilities over the life-cycle (Flavio Cunha and Heckman 2007 and Cunha and Heckman 2008a). Hence, it seems plausible that both types of skills affect the measured level of the other skill measure, though it is uncertain which effect is most prevalent. To provide bounds on the potential biases, we estimate (1) with each ability measure taken out of the regression. This gives us an upper bound on the effect of cognitive or noncognitive ability while the regression with both measures included provides the lower bounds.

Third, our estimates may be biased if cognitive or noncognitive skills are measured with error. This is a particular problem for our measure of noncognitive ability as psychologists differ in their evaluation of identical conscripts. Barbro Lilieblad and Berit Ståhlberg (1977) estimated the correlation between SNSA psychologists' assessment of noncognitive skills to 0.85 after letting psychologists listen to tape recordings of enlistment interviews. The fact that psychologists make their own interviews could, in theory, both imply that the true correlation is higher or lower than 0.85. Following a procedure proposed by Gunnar Isacsson (1999), we use data on identical and fraternal twins to estimate the reliability ratios of our ability measures (see Appendix C for details). We find that the reliability for cognitive ability (0.868) is indeed substantially higher than for noncognitive ability (0.703). Since

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

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

our estimation of the reliability ratios relies upon a number of additional assumptions (e.g., uncorrelated measurement error within twin pairs), we will focus on the case with no measurement error correction.

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

Fifth, as we are estimating the partial correlations between our ability measures and outcomes, the interpretation of the estimated parameters depends on the variables included in the covariate vector, $\mathbf{X}_i$. Since the basic set of control variables includes variables that are predetermined at the time of the draft, this specification is silent on the exact mechanism by which skills affect outcomes.[19] Notably, selection into higher education is an important channel by which skills could affect labor market outcomes. To test for the importance of postdraft variables, we augment the basic specification of $\mathbf{X}_i$ with the full set of dummy variables for educational attainment and linear-quadratic terms in work experience.

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

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

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

affect our estimates, but the estimated effect of skills on wages are smaller when we control for type of military service.

III. 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.

A. *Wages*

The results for regression (1) with log wages as the dependent variable are presented in Table 1. Column 1 shows the results for the basic specification without controls for higher educational attainment or adjustment for measurement error. In this case, an increase in cognitive ability by one standard deviation predicts a wage increase by 8.6 log points compared to 6.6 log points for noncognitive ability.

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

As shown in columns 3 and 4, adjusting for measurement error has a strong effect on the estimated effect of noncognitive skill—the estimated effect increases 30 percent—whereas the estimated effect of cognitive skills remains essentially unchanged.

The estimated effects of cognitive and noncognitive ability both increase substantially when each ability measure is included separately in the regression (columns 5 and 6). As discussed in Section II, the regression with only cognitive ability gives the effect of cognitive ability on wages in case the covariance between the ability measures only reflects an effect of cognitive ability on measured noncognitive ability. Correspondingly, the regression with only noncognitive ability gives the effect of noncognitive ability that holds if the covariance is only due to an effect of noncognitive ability on measured cognitive ability.

The last column in Table 1 shows the results when we include quadratic terms and an interaction effect between cognitive and noncognitive ability. We find that log wages are strictly convex in cognitive ability but linear in noncognitive ability. The interaction term is positive and statistically significant, implying that the return to cognitive skill is increasing in noncognitive skills, and the other way around. Still, the regression model with higher order terms gives only a small increase in terms of variance explained.

Figure 1 presents results from a nonparametric estimation where we let each unique value of the alternative, more continuous, skill measures be represented by a dummy variable. Since the alternative skill measure for cognitive skill is based on a finer scale (4–36) than the measure for noncognitive skill (4–20), there are fewer observations for each unique value of cognitive skill, making the results for cognitive skills noisier. As is clear from the figure, the return to noncognitive skill

TABLE 1—ESTIMATED EFFECT OF COGNITIVE AND NONCOGNITIVE SKILLS ON LOG WAGES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cognitive skills	0.086***	0.050***	0.083***	0.049***	0.104***		0.087***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)		(0.003)
Noncognitive skills	0.066***	0.058***	0.086***	0.078***		0.092***	0.067***
	(0.003)	(0.003)	(0.004)	(0.004)		(0.003)	(0.003)
Cognitive skills sq.							0.014***
							(0.002)
Noncognitive skills sq.							0.001
							(0.002)
Noncognitive × Cognitive							0.014***
							(0.003)
Covariate set	Small	Large	Small	Large	Small	Small	Small
Measurement error correction	No	No	Yes	Yes	No	No	No
Observations	13,974	13,123	13,974	13,123	13,974	13,974	13,974
R^2	0.294	0.349	0.323	0.367	0.263	0.239	0.301

Notes: All regressions estimated using ordinary least squares. The dependent variable is log wage in 2006. Wage in 2006 has been imputed for 1,401 individuals using wage data from 2001 to 2005. Heteroskedasticity-robust standard errors are reported in parentheses in columns 1–2 and 5–7. Standard errors in columns 3–4 computed with bootstrap (50 replications). All regressions include a constant, cohort dummies, an enlistment dummy, household income in 1980, a dummy for whether parents were married in 1980, six dummy variables for region of residence, and a dummy variable for no educational attainment above primary school (the "small" set of covariates). The regressions in columns 2 and 4 also include a quadratic in potential post-education experience and dummy variables for secondary school, two years post-secondary schooling, university degree and a PhD (the "large" set of covariates). The measurement error correction in columns 3 and 4 is based on a reliability ratio of 0.8675 for cognitive ability and 0.70267 for noncognitive ability. We adjust the coefficients in columns 3 and 4 for the larger skill measure variance implied by measurement error.

  ***Significant at the 1 percent level in a two-sided test.
   **Significant at the 5 percent level in a two-sided test.
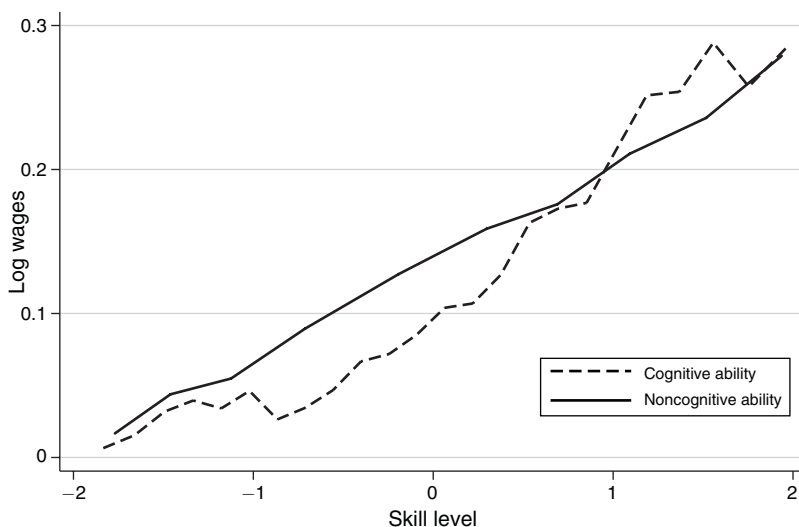   *Significant at the 10 percent level in a two-sided test.

FIGURE 1. NONPARAMETRIC ESTIMATES OF THE WAGE EQUATION

Notes: Nonparametric estimation using dummy variables for each value on the alternative ability measures based on the sum of subscores. Both skill measures have been truncated at $+/-2$ standard deviations. The effect of each skill measure has been normalized to zero for a skill level of -1.96.

Table 2—Estimated Effect of Cognitive and Noncognitive Skills on Log Wages, Extensions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cognitive skills	0.023***	0.089***	0.021***	0.052***	0.039***	0.056***	0.014***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.014)	(0.005)	(0.003)
Noncognitive skills	0.052***	0.066***	0.050***	0.058***	0.048***	0.046***	0.027***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.013)	(0.004)	(0.003)
Covariate set	Small	Small	Large	Large	Large	Large	Large
Observations	11,480	11,480	10,915	10,915	978	4,962	5,634
R^2	0.353	0.279	0.370	0.333	0.341	0.267	0.064
Additional covariates	GPA*		GPA*				
Sample restrictions		GPA* observed		GPA* observed	Managers	Skilled	Unskilled
Sample mean of cognitive skills					0.43	0.50	−0.44
Sample mean of noncognitive skills					0.55	0.32	−0.27

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

does not change appreciably with skill level, while the return to cognitive skill is increasing in cognitive skill. See Figures D1 and D2 for a nonparametric estimation that shows the positive interaction effect between cognitive and noncognitive ability.

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

Second, we test whether cognitive and noncognitive ability are valued differently across occupations. Data on occupational status in 2006 is available in LINDA for 12,379 workers. For all occupational groups except managers and military officers, our data contains information on the level of qualifications needed on the job. We classify workers in the two highest qualification levels (out of four) as "skilled" and the workers in the two lowest qualification levels as "unskilled." Managers are treated as a separate group. We exclude the small group of military officers as it is unclear whether they should be classified as managers or skilled workers. Further details underlying our classifications are available in Appendix E. Two findings stand out from a comparison of the mean values of skills across these occupational groups. First, whereas the average level of cognitive skills is highest among workers in skilled occupations, managers have the highest average level of noncognitive skills. Second, the difference between skilled and unskilled workers is much

stronger in terms of cognitive than noncognitive skills.[20] The estimated skill prices are consistent with these selection patterns.[21] Noncognitive ability has a higher return than cognitive ability for managers and workers in unskilled occupations, while workers in skilled occupations have a higher return to cognitive ability.[22] The results remain qualitatively similar when we estimate skill prices using econometric models that adjust for self-selection into different occupations (see Appendix E).

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

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

B. *Unemployment*

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

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[20] All differences in average skills between occupational groups are statistically significant at the 1 percent level, except for the difference in cognitive skills between managers and high-skilled workers, which is statistically significant only at the 20 percent level in a two-sided test.

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

[22] There is a small previous literature on occupational choice and skill endowment. In line with our results, Frank L. Schmidt and John Hunter (2004) find that the importance of IQ rises with job complexity. In contrast, Eric D. Gould (2005) finds relatively small differences in IQ across sectors. Borghans, ter Weel, and Bruce A. Weinberg (2008) find that persons with a preference for a "direct" relative to "caring" style in interpersonal encounters select into occupations where directness is required (e.g., managers). There is also some previous evidence in support of the view that personality is of particular importance for workers in managerial positions. Surveying the psychology literature, Borghans et al. (2008) find that while IQ is considerably more important for job performance than any of the Big Five-factors of personality, the Big Five-factor *conscientiousness* has a slightly stronger correlation 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.

TABLE 3—ESTIMATED EFFECT OF COGNITIVE AND NONCOGNITIVE SKILLS ON EARNINGS
AND UNEMPLOYMENT

Dependent variable	Unemployment support (1)	Unemployment support (2)	Unemployment support (3)	Any social assistance (4)
Cognitive skills	−0.015*** (0.003)	−0.022*** (0.003)		−0.023*** (0.003)
Noncognitive skills	−0.024*** (0.003)		−0.028*** (0.003)	−0.042*** (0.003)
Observations	14,626	14,626	14,626	14,626
R^2	0.030	0.025	0.028	0.060

Dependent variable	Unemployment duration (5)	Annual earnings (6)	Annual earnings (7)	Annual earnings (8)
Cognitive skills	−0.006 (0.011)	32,791*** (1,751)	43,392*** (1,649)	
Noncognitive skills	−0.026** (0.011)	37,148*** (1,947)		46,999*** (1,835)
Observations	1,253	14,626	14,626	14,626
R^2	0.019	0.168	0.144	0.148

Notes: All regressions estimated with OLS using the small set of covariates. The results in column 5 regards nonexponentiated coefficients (not hazard ratios). Annual earnings are denoted in SEK. See legend in Table 1 for further information.

Appendix D (Table D1), noncognitive ability is an even stronger predictor of unemployment relative to cognitive ability when we control for educational attainment or adjust for measurement error.

Since unemployment insurance benefits are subject to a time limit and are based on previous income, men with a permanently weak attachment to the labor market may not be eligible for unemployment insurance. We therefore construct an alternative measure of unemployment which also includes men who receive disability insurance or social welfare payments.[23] These men have a significantly weaker attachment to the labor force compared to those who just received unemployment support.[24] The relative importance of noncognitive ability increases when we use this alternative measure (column 4).

Table 3 also shows that, conditional on becoming unemployed, men with high noncognitive ability obtain a new job more quickly. These results hold regardless of whether we estimate the total duration of unemployment by OLS (column 5) or the hazard rate of leaving unemployment (results available upon request). A 1 standard deviation increase in noncognitive skill decreases expected unemployment duration

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

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

by 0.026 years, or about 10 days. The effect of cognitive skills on the job finding probability is neither economically nor statistically significant.

The relative importance of noncognitive ability for labor force participation is consistent with two different explanations. First, we showed that noncognitive ability is a stronger predictor of wages in unskilled occupations. This suggests that men with low noncognitive ability may be priced out of the labor market.[25] Second, men with low noncognitive ability could have a higher reservation wage. It is beyond the scope of this paper to distinguish between these two explanations.

C. *Earnings*

As shown in Table 3, the effect of cognitive and noncognitive skills on average earnings are similar to the effect found for wages. A one standard deviation increase in noncognitive ability predicts an increase in the conditional mean of earnings by 37,100 SEK (11 percent of average annual earnings) compared to 32,800 (10 percent) for cognitive ability.[26]

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

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

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[25] Minimum wages in Sweden are set by negotiations between employers and trade unions and are binding mainly in the service sector. The level of social assistance granted to households with several children may also be higher than the minimum wage for service sector jobs. See Per Skedinger (2010) for a discussion of minimum wages in Sweden.

[26] Since we would have to truncate the earnings distribution from below in order to get meaningful estimates of the effect on log earnings, we use the absolute value of earnings as the dependent variable in our earnings regressions. We show the results for the log of annual earnings truncated at 120,000 SEK in Table D1.
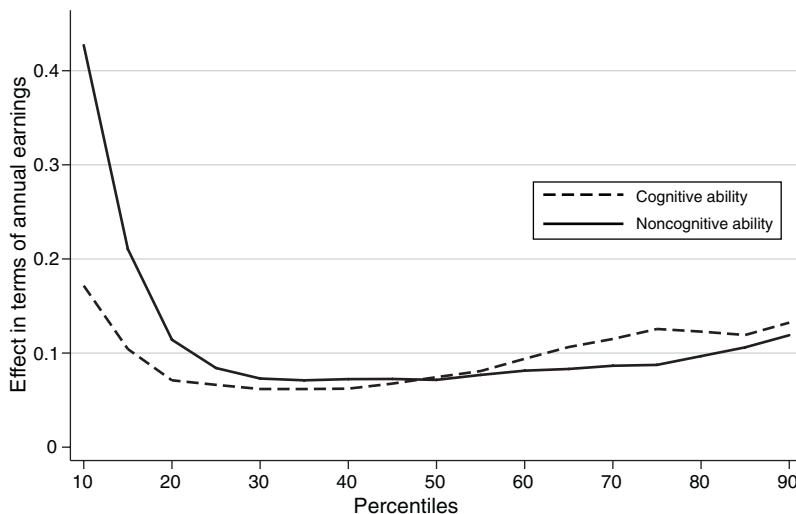
FIGURE 2. ESTIMATED EFFECT OF COGNITIVE AND NONCOGNITIVE SKILLS ON THE UNCONDITIONAL
QUANTILES OF THE EARNINGS DISTRIBUTION, SMALL SET OF COVARIATES

Notes: Effect in absolute number (SEK) divided by annual earnings at each percentile. See
Appendix D for details.
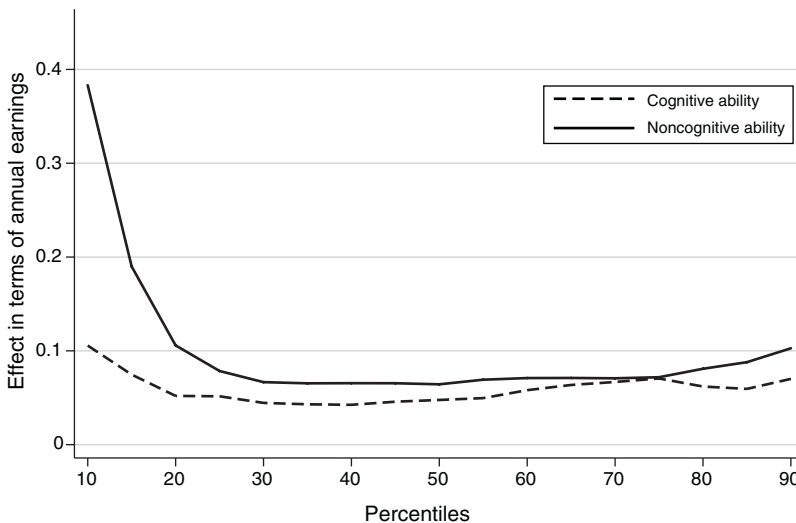
FIGURE 3. ESTIMATED EFFECT OF COGNITIVE AND NONCOGNITIVE SKILLS ON THE UNCONDITIONAL
QUANTILES OF THE EARNINGS DISTRIBUTION, LARGE SET OF COVARIATES

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

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

FIGURE 4. ESTIMATED EFFECT OF NONCOGNITIVE SKILLS ON THE PROBABILITY
OF LOW EARNINGS, BY COGNITIVE ABILITY

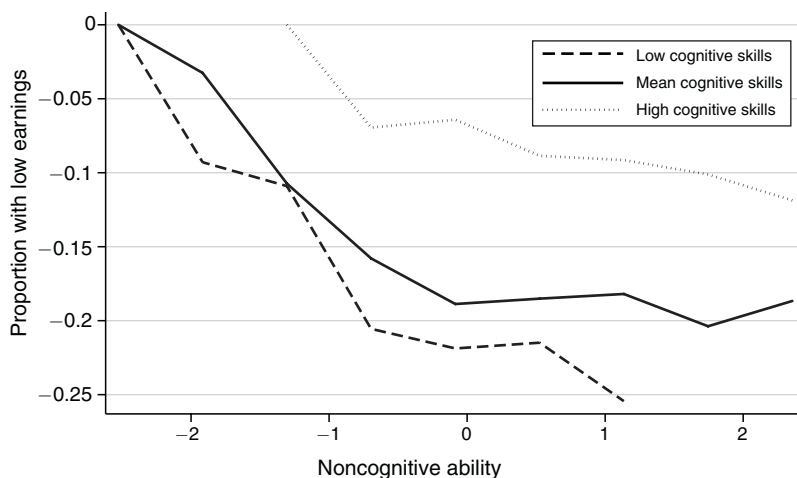*Notes:* Samples restricted to "low" (1–3), "mean" (4–6), or "high" (7–9) values of the corresponding skill measure. All regressions include fixed effects for each value of cognitive and noncognitive skill included in the specific sample and the small set of covariates as defined in Table 1. Sample restricted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

Our quantile regressions rely on the simple linear specification of regression equation (1). To obtain a more complete picture of how skills affect the probability of low earnings (below the tenth percentile), we employ a simple nonparametric estimation. We let each unique value of cognitive and noncognitive ability be represented by a dummy variable and estimate the effect of each skill measure on the probability of low earnings while fixing the opposite skill measure at low (1–3 on the 1–9 scale), medium (4–6), or high values (7–9). We exclude skill combinations for which there are fewer than 100 observations and normalize the effect to zero for the lowest skill value which is included in the regression. As shown in Figure 4, the proportion of men with low earnings is decreasing in noncognitive ability regardless of the level of cognitive ability, though the effect is largest for men with low cognitive ability. In contrast, Figure 5 shows that the level of cognitive ability makes no difference for men with high noncognitive skills, and only matters at the low end of the cognitive ability distribution for men with low or average noncognitive skills.

IV. Relation to Previous Literature

In this section, we discuss how our results relate to the previous literature. We focus on the results for noncognitive ability. Our results for the effect of cognitive skills on wages is similar to what has been found in previous literature.[27]

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[27] Bowles, Gintis, and Osborne (2001a) present 65 estimates of cognitive ability measures in earnings regressions from 24 different studies. The mean estimate was 0.07 for standardized regressions coefficients (normalized
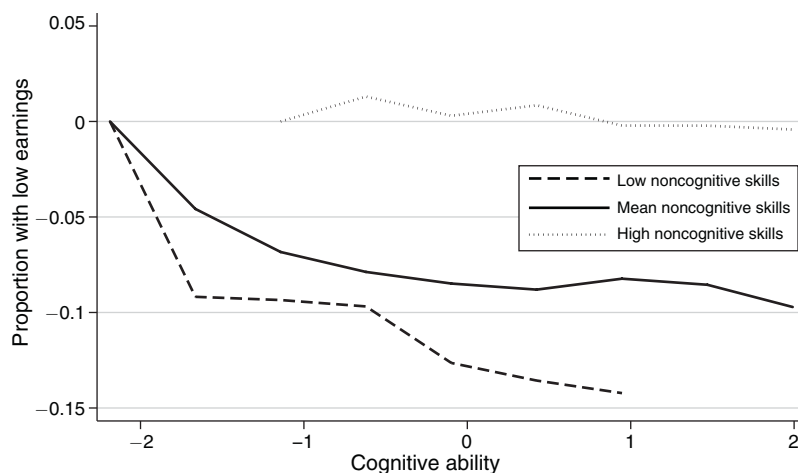
FIGURE 5. ESTIMATED EFFECT OF COGNITIVE SKILLS ON THE PROBABILITY
OF LOW EARNINGS, BY NONCOGNITIVE ABILITY

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

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

As is clear from Table A2, most studies use measures derived from surveys. Among these studies, there is a fairly high congruence in terms of the specific measures used. The "internal-external locus of control" scale developed by Julian B. Rotter (1966) is used in four studies and similar measures are used in another two ("personal efficacy" in Duncan and Morgan 1981 and "personal control" in Dunifon and Duncan 1998). The Rotter scale measures to what extent individuals believe that they can affect their own fate. A high external locus of control—the belief that events are determined by external forces—is negatively associated with wages. A standard deviation increase in external locus of control decreases wages by between 5–7 log points whereas beta coefficients vary between −0.05 and −0.15.[28] The outlier is Dunifon and Duncan (1998) where a one standard deviation increase in

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with respect to the variance in cognitive ability but not with respect to variance in earnings) and 0.15 for beta coefficients. Excluding noncognitive ability, but controlling for the large set of covariates (which corresponds most closely with the specifications in previous literature on cognitive ability and earnings), we get a standardized coefficient of cognitive ability of 0.063 and a beta coefficient of 0.196. This particular specification is not reported in the paper, but is available from the authors upon request.

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

personal control (similar to internal locus of control) predicts a 14 log point increase in wages. As noted by Dunifon and Duncan (1998), this could partly be due to the advanced age of their sample population. Another potential explanation is that the cognitive ability measure available in PSID—a sentence completion test—is a relatively imprecise control for cognitive ability. The study by Jencks (1979) also constitutes a special case. Along with results for several measures of self-assessed personality traits and behaviors, Jencks (1979) reports results for a measure of noncognitive skills based upon the linear combination of seven different traits and behaviors that maximizes predictive power. This measure is substantially stronger associated with earnings than any individual trait or behavior.

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

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

Finally, Murnane et al. (2001) and Segal (2008) find that measures of coding speed—a proxy for motivation—from the ASVAB test battery are positively associated with labor market outcomes even after controlling for cognitive ability.[29]

As most studies in the previous literature report results when education is controlled, the results in Table A2 should be compared to our regressions with the large set of covariates, i.e., columns 2 and 4 in Table 1.[30] In general, our point estimates

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

[30] Whether one should consider our results in column 2 or 4 depends on whether the original study corrects for measurement error or not. Osborne Groves (2005); Murnane et al. (2001); Goldsmith, Veum, and Darity (1997); and the structural model in Heckman, Stixrud, and Urzua (2006) attempt some form of measurement error correction.

are large compared to the previous literature, in particular if we calculate beta coefficients which adjust for relatively compressed wage structure in Sweden. In the basic specification, a one standard increase in cognitive ability predicts an increase in log wages by 0.265 standard deviations compared to 0.204 standard deviations for noncognitive ability.

Still, the main contribution of our analysis compared to previous literature lies not in the size of our point estimates in wage regressions, but in the importance of noncognitive ability for avoiding bad labor market outcomes, in terms of unemployment and low annual earnings. Since the previous literature has focused on the effect of noncognitive ability on wages rather than employment, it is difficult to know to what extent this result generalizes to other countries and measures of personality. At least compared to the United States, the distribution of cognitive ability is relatively compressed in Sweden (Stephen Nickell 2004), which may explain why cognitive ability is less important than noncognitive ability for labor force participation.[31] Our analysis nevertheless suggests that cognitive and noncognitive ability have distinctively different effects in the Swedish labor market.

V. Concluding Remarks

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

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

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

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

Björklund, Mikael Lindahl, and Plug 2006). Two recent papers use Swedish enlistment data to study family effects on noncognitive ability. Grönqvist, Björn Öckert, and Vlachos (2010) estimate a father-son correlation for noncognitive ability of 0.43, which is close to the correlation for cognitive ability (0.48). David Cesarini (2010) finds that both shared genes and shared environment explain sibling correlations in cognitive and noncognitive ability, but that shared genes are more important for both ability measures.

Another literature has investigated how cross-country differences in the distribution of cognitive ability relate to aggregated outcomes, such as economic development (Eric A. Hanushek and Ludger Woessman 2008) and income inequality (Nickell 2004). Due to the lack of consistent measures of noncognitive ability across countries, it is difficult to conduct similar studies for noncognitive ability. There is, however, no reason a priori to expect noncognitive ability not to be important also in this context.

Appendix A: Data

A. *Construction of Unemployment Spells*

The main condition in order to be eligible for unemployment insurance benefits is a minimum of 70 hours of work per month for at least 6 months in the 12-month period preceding unemployment. For those who are eligible for unemployment insurance benefits, the value of the daily allowance is calculated based upon average earnings for months with at least 70 hours of work over the 12-month period preceding unemployment. The replacement rate in 2006 was 80 percent of earnings with a ceiling at 730 SEK per work day for the first 100 days of compensation and 680 SEK thereafter. The minimum daily allowance is 320 SEK. Only workers who are members of an unemployment benefit fund are eligible for compensation above the minimum daily allowance. More than 90 percent of the men in our sample who received unemployment support in 2006 were members of an unemployment benefit fund.

Since our data covers earnings and unemployment benefits per calendar year, we do not know the exact level of earnings that preceded an unemployment spell, implying that we cannot calculate the exact daily allowance. We instead use annual earnings in 2005 as a proxy for the level of earnings that preceeded unemployment. In case the imputed duration exceeds one, we set duration equal to one.

B. *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 as father if he is at least 20 years old and the remaining criteria concerning civil status are met. 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

Table A1—Summary Statistics

Variable	Obs	Mean	SD	Comment
Wage per month in 2006 (SEK)	14,038	27,979	12,042	Missing values imputed using wages from 2001–2005
Unemployment support	14,703	0.092		
Any form of social assistance	14,703	0.126		
Unemployment duration (if > 0)	1,262	0.507	0.346	
Total wage earnings in 2006	14,703	319,792	206,140	
Cognitive skill	14,703	0.000	1.000	Normalized from 1–9 score
Noncognitive skill	14,703	0.000	1.000	Normalized from 1–9 score
Enlisted to the military service	14,703	0.900		
Geography: Gothenburg	14,703	0.054		
Geography: Stockholm	14,703	0.089		
Geography: Malmö	14,703	0.201		
Geography: Southern region (Götaland)	14,703	0.043		
Geography: middle region (Svealand)	14,703	0.478		
Geography: Northern region (Norrland)	14,703	0.397		
Experience	13,760	14.92	6.08	
Education: primary school	14,656	0.080		
Education: secondary school	14,656	0.556		
Education: two years beyond secondary school	14,656	0.094		
Education: university	14,656	0.256		
Education: PhD	14,656	0.013		
Family background: household income in 1980	14,673	1,078	588	
Family background: parents married in 1980	14,673	0.791		
Grade point average in secondary school	11,925	3.153	0.642	Grades set on a 1–5 scale

Note: Variables used only in Appendix B–F are reported in Table B1.

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.

C. *Regional Dummies*

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

D. *Interviews*

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

TABLE A2—ESTIMATES OF THE EFFECT OF PERSONALITY ON WAGES OR EARNINGS FROM PREVIOUS LITERATURE

Study	Dependent variable	Psychological variables	Type of measure	Normalized coefficients ($b^*_{\sigma x}$)	Beta coefficients ($b^*_{\sigma x/\sigma y}$)	Data source	Controls[1]
Andrisani (1977)	Log wages 1971	External control (Rotter scale)	Survey	−0.072		NLS (young)	E
Andrisani (1977)	Log wages 1971	External control (Rotter scale)	Survey	−0.048		NLS (middle-age)	E
Andrisani and Nestel (1976)	Log wages 1971	External control (Rotter scale)	Survey		−0.092	NLS (middle-age)	E
Duncan and Morgan (1981)	2-year change in hourly earnings	Personal efficacy[2]	Survey		−0.052	PSID	E
Duncan and Morgan (1981)	4-year change in hourly earnings	Personal efficacy	Survey		−0.165	PSID	E
Dunifon and Duncan (1998)	Log wages (average 1988–92)	Orientation towards challenge, personal control[3]	Survey	Challenge: 0.07 Personal control: 0.14		PSID	E, S, C
Edwards (1976)	Wage differences within work groups	Willingness to follow rules (Rules); Predictability and dependability (Dependability); Internalization of firm goals and values (Goals)	Peer-ratings		Rules: 0.14; Dependability: 0.10; Goals: 0.32	Government employees	E, S, C
Goldsmith, Veum, and Darity (1997)	Log wage 1980	Predicted self-esteem (Rosenberg)[4]	Survey	0.061	0.165	NLSY	E, S, C
Goldsmith, Veum, and Darity (1997)	Log wage 1987	Predicted self-esteem (Rosenberg)	Survey	0.078	0.149	NLSY	E, S, C
Heckman, Stixrud, and Urzua (2006)	Log wage	Average of external control (Rotter) and self-esteem (Rosenberg)	Survey	0.043		NLSY	E, C
Heckman, Stixrud, and Urzua (2006)	Log wage	Noncognitive ability	Structural model	0.112		NLSY	E[5]
Jencks (1979)	Hourly earnings	Combination of measures[6]	Survey		0.245	Talent survey	E, S, C
Kuhn and Weinberger (2005)	Log wages	Leadership skills	Survey	0.037		Talent survey	E, S, C
Olnek and Bills (1979)	Log earnings	Cooperativeness, executive ability, industriousness	Teacher assessment	Cooperativeness: −0.021; Executive ability: 0.081; Industriousness: −0.011		Kalamazoo	E, S, C
Murnane et al. (2001)	Log wage	Self-esteem (Rosenberg); Analytic speed (ASVAB)	Survey and test scores	Self-esteem: 0.037; Analytic speed: 0.110	Self-esteem: 0.079; Analytic speed: 0.238	NLSY	C

(*Continued*)

TABLE A2—ESTIMATES OF THE EFFECT OF PERSONALITY ON WAGES OR EARNINGS FROM PREVIOUS LITERATURE
(*Continued*)

Study	Dependent variable	Psychological variables	Type of measure	Normalized coefficients $(b_{\sigma x}^*)$	Beta coefficients $(b_{\sigma x/\sigma y}^*)$	Data source	Controls[1]
Osborne Groves (2005)	Log hourly wages	External control (Rotter scale)	Survey	−0.055	−0.103	NLSYW	E, S, C
Osborne Groves (2005)	Log hourly wages	External control (Rotter) –instrumented	Survey	−0.067	−0.129	NLSYW	E, S, C
Osborne Groves (2005)	Log hourly wages	Aggression, withdrawal	Teacher assessment	Aggression: −0.076; Withdrawal: −0.033	Aggression: −0.129; Withdrawal: −0.056	NCDS	E, S, C
Segal (2008)	Log earnings	Motivation proxied by coding speed (ASVAB)	Test scores	0.064		NLSY	E, C
Segal (2008)	Log earnings	Misbehavior[7]	Teacher assessment	−0.041		NELS	E, C

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

[1] E = educational attainment; S = socioeconomic background; C = cognitive ability.

[2] This measure is closely related to externality as measured by the Rotter scale.

[3] This measure is closely related to externality as measured by the Rotter scale.

[4] Self-esteem predicted with externality (Rotter scale).

[5] Cognitive and noncognitive skills are orthogonal by construction.

[6] The combination of seven different variables related to self-assessed traits and behaviors with maximum predictive power.

[7] Based on teacher assessments of 5 personal traits: absenteeism, disruptiveness, inattentiveness, tardiness, and homework completion.

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