ISSN 1503-299X

WORKING PAPER SERIES

No. 9/2006

THE EFFECT OF EDUCATION ON COGNITIVE ABILITY

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The effect of education on cognitive ability

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Abstract
We analyze whether the amount of schooling influences intelligence as measured by IQ tests. By use of a novel longitudinal dataset we are able to condition on early cognitive ability to account for selection into non-compulsory schooling when estimating the effect on cognitive ability at age 20. OLS estimates indicate that one year of schooling increases IQ by 2.8–3.5 points (about 0.2 standard deviations). When family income per family member and teacher evaluations of the individuals at age 10 are used as instruments for schooling and early cognitive ability, the return to schooling is estimated to 3.5–3.8 IQ points.

Keywords: Cognitive ability; Education production; Return to schooling
JEL code: I21; J24

* Comments from Edwin Leuven and Bjarne Strøm are gratefully acknowledged.

1. Introduction

A large debate in labor economics concerns whether schooling merely plays the role as a signaling device or as a way to improve on skills. If non-compulsory schooling is only a part of a signaling game, general cognitive skills should not be affected by schooling choices. An implicit assumption in the human capital literature is that education affects individuals' general and analytical skills, and not only in the narrow range of achievements related to the curriculum. In order to investigate whether education affects cognitive ability, it is necessary to test individuals after they have completed different amounts of schooling, using a test that does not favor individuals with specific types of education. Natural candidates in this regard are various types of IQ tests since these are designed to test 'thinking skills' or 'intelligence'.

Early cognitive ability, or latent ability, is a strong predictor of schooling, at least in a signaling setting. The empirical challenge is to isolate the effect of schooling on cognitive ability from the effect of latent ability. It is essential to take selection into non-compulsory schooling into account in order to compare individuals who are initially seemingly identical. Hansen et al. (2004) use NLSY data and solve the selection problem by conditioning on estimated latent ability, utilizing the fact that the individuals have taken the test at different ages (between 15 and 22 years of age) and that some have completed their schooling at the date of the test. While this may be a reasonable approach, an approach that conditions on observed early cognitive ability may seem easier to interpret. This is the approach in Winship and Korenman (1997, 1999), who also use the NLSY data, but they rely instead on early cognitive skills measured at different ages and by different tests across individuals. Both Hansen et al. (2004) and Winship and Korenman (1997, 1999) find sizable effects of schooling using the AFQT measure in the NLSY data.

In this paper, we use the Malmö Longitudinal Dataset, a dataset much richer on ability than the NLSY data. The data include the IQ test from the compulsory military enrollment at age 20, which we use as the outcome variable, in addition to a comparable IQ test and different teacher evaluations at age 10. The latter measures make it possible to utilize comparable early cognitive ability measures to take account of selection into education. The initial IQ test was taken before any tracking was done in school, and thus it is reasonable to believe that the individuals' ability had not yet been much affected by their school surroundings. The initial sample consists of the population of third graders in 1938, and a major effort was made by

Husén (1950) to collect the results of all individuals in the sample that took the test at military enrollment. According to Husén, the IQ tests are highly comparable.

The data include a relatively large variation in education because, at the time, compulsory schooling began in the fall in the year when a child turned seven, and lasted only seven years. Education is self-reported in 1948, and is not totally consistent with the more detailed information collected later. We have made an effort to correct for this difference, but, because of possible measurement error in education, we use instrumental variable techniques in line with the literature on the return to education in the labor market. As instruments, we use average family income during childhood and growth in the grade point average (GPA) from the end of third to the end of fourth grade as assessed by the teacher. The latter is attractive because tracking of the students started in grade five, partly based on GPA, and the former is attractive because causal evidence indicates that credit market constraints played a role in the empirical period.

How intelligence is determined is an old research question within psychology. Ceci (1991, p.703) argue that "there is now considerable evidence for the importance of variation in schooling on IQ". Some rather old studies investigate the effect of schooling using IQ tests at two different ages, typically using as the outcome variable the test taken at the enrollment into the military service compulsory for all males. For Sweden, Husén (1950) compares the change in IQ from ages 10 to 20 for different kinds of education using the Malmö Longitudinal Dataset and Härnqvist (1968) compares regression coefficients of IQ at age 18 on IQ at age 13 for different types of education. Lund and Thrane (1983) use Norwegian data from the 1950s collected at the ages of 14 and 19 in a similar way. Husén and Tuijnman (1991) use the Malmö Longitudinal Dataset in an analysis more similar to ours by use of a conditioning model. All these studies find strong effects of schooling. However, unfortunately, none of the studies use schooling measured purely as the number of years of education. Rather they categorize different educational types into different groups, making it hard to interpret the results in terms of return to one year of schooling. In contrast to the studies above we will use an explicit regression design.

There is another relevant line of literature for the present study. The so-called education production function literature aims at estimating how student achievement is determined. Usually, tests for students in compulsory schooling are used to investigate the effects of family background and different school inputs such as resources and peers, see for example Rivkin et al. (2005). The cumulative nature of the production process and the problem of unobserved individual characteristics such as innate ability have made the value-added approach popular. This approach conditions on a prior test score, or uses the growth in test scores as the dependent variable. The present analysis can be seen as following this tradition, but focusing on another variable, namely the quantity of schooling. We go a long way in responding to the criticism raised by Todd and Wolpin (2003) against the value-added modeling tradition by using teacher evaluations as instruments for the IQ test result at age 10. While the above literature typically finds that investment in terms of monetary inputs such as class size have at most a very small effect on student achievement, see for example Rivkin et al. (2005), our results indicate that investment in terms of time spent at school has a major impact on cognitive ability.

This paper is also related to the literature on the causal return to education in the labor market. Is a positive return in terms of earnings caused by education, or is it a result of individuals with greater innate ability choosing more schooling? Some papers utilize variation in compulsory schooling laws to generate exogenous variation in schooling. For example, Angrist and Krueger (1991) and Meghir and Palme (2005) find strong effects of prolonged schooling, clearly indicating that schooling improves skills valuable in the labor market. In contrast, Pischke and Wachter (2005) find no effect for Germany and argue that the most likely explanation is that German students have strong academic skills at the end of compulsory schooling. The present paper is a more direct analysis of whether schooling affects ability, and we will allow for nonlinear effects in order to test whether ability improves only up to a given point.

Section 2 gives a closer description of the data. The identification issue is discussed in Section 3, while the empirical results are presented in Section 4. Section 5 provides some concluding comments.

2. Data

The Malmö Longitudinal Dataset includes all children in third grade in the city of Malmö, Sweden, in 1938, originally 1,542 individuals. The data collected in the spring of 1938 include information on family background as well as different ability measures. Even though the fact that they all lived in Malmö at the time of the first test might reduce the representativity of the sample, it also reduces the need for conditioning variables in empirical analyses because it has been shown that levels of IQ actually vary place of abode, for example between urban and rural residents.

Military enrollment was compulsory at the time, and all men enrolled had to take an IQ test, similar to the US Armed Forces Qualifying Test (AFQT). In addition, the military enrollment data includes self-reported educational level. Information from military enrollment in 1947 and 1948 was merged with the original dataset by Husén (1950). Furthermore, educational information is also available from a questionnaire distributed in 1964, which was combined with central school registers.

We use results from the IQ test done at the time of compulsory military enrollment as the dependent variable in the analysis, thus excluding all females and the men who did not enroll for military service in 1947 or 1948. There were three main reasons for men not to enroll: they had already enrolled in the military service on voluntary basis (for example in officer training); they were seamen; or their state of health was regarded as inadequate for military service.

The ability measures from third grade include a thorough IQ test. The original purpose with the research that established the dataset was to study the relation between social background and cognitive ability. Thus, a lot of effort was put into the task of making this information reliable and accurate. Each child in third grade in any school within the county of Malmö is included in the dataset, and every single boy actually took the test. Not all of them were necessarily 10 years old, but normally were in their tenth year of life.[1] The test was constructed after thorough testing of third-graders the year before. The test consisted of four parts: word opposites, sentence completion, perception of identical figures, and disarranged sentences.

The IQ test taken in connection with the military enrollment in 1948 was of a similar kind to that in 1938. It consisted of four parts: synonyms, concept discrimination, number series and

[1] In the original sample of 834 boys, 14, 88, 717 and 15 boys were born in 1926, 1927, 1928 and 1929, respectively. In the sample of individuals with information on IQ at military enrollment, the respective numbers are 8, 60, 584 and 1. Regarding individuals born in 1929, the normal year of military enrollment would be 1949. The only boy born in 1929 with military enrollment in 1948 is excluded from the analysis.

Raven's matrices. Involved in the construction of this test was Torsten Husén, who devoted a lot of work to making the test comparable with the Malmö test from 1938. The IQ test taken by those who enrolled in the military in 1947 was of a slightly different kind, but the correlation between this test and the 1948 test was 0.91, indicating that both measure the same ability functions (Husèn, 1950). 94 percent of the normal-aged individuals took the test in 1948, while all the overaged in the sample did the test in 1947. All three tests (1938, 1947 and 1948) have been translated to the standard IQ scale with a mean score of 100 and a standard deviation of 15 units (Husèn, 1950). Thus, the different IQ tests should be well suited for comparison with each other.

There is additional information on early cognitive ability in the data. We will utilize grade point averages (GPA) from third and fourth grade as well as a teacher rating. In the teacher rating we use, the teachers gave a supposedly objective measure of general cognitive ability on a scale from one to five.

Regarding education, children started school in the fall the year they turned seven, and it was compulsory to complete at least seven years of school in the Malmö region. At the time, the Swedish school system was comprehensive for only the first four years. Thereafter, the pupils were streamed into two different tracks - either a vocational or a more academic track - similar to, for example, the German system today. The less academic primary school lasted three additional years and the more academic lower secondary school lasted four or five additional years. The tracking was based on choice and GPA. Teacher grading in fourth grade was therefore important for the educational possibilities above the compulsory level. It was also possible to transfer to the lower secondary school later than the fifth grade under certain circumstances, for example for pupils living far away from a lower secondary school. The lower secondary school was a prerequisite for enrollment in upper secondary school, which normally lasted four years. Individuals that finished primary school either entered the labor market or continued with more vocational schooling, of which there were several kinds, generally lasting one or two years.

We mainly use the educational information collected at the time of the second IQ test. The information from the survey in 1964 is less suitable, since we cannot always be certain whether the reported education was acquired before or after military enrollment. There are, however, some limitations to the information from 1948 that may be partly reduced by utilizing information from 1964. In both years, the information is on type and level of education rather than years of schooling. Using the information from 1964, Sandgren (2005) translates the information into years of schooling, based on an extensive search of the literature on the schooling system during the relevant time period. The information from 1948, however, is grouped into fewer types of education, making it somewhat harder to recode the data. This is particularly true for the 'primary education' group, which explicitly includes primary school dropouts and students with some minor post primary school education.

Two types of information from the survey in 1964 are used to correct the educational information from 1948. First, when the survey and register information from 1964 state that the individual did not complete primary education, we code schooling as six years. Second, upper secondary education is coded as 13 years of schooling, and with normal progression upper secondary education should have finished in the spring of 1948.[2] However, it seems likely that the military test was taken before the end of the school year since very few report to have the diploma from upper secondary education. Many individuals in 1948 report to have some upper secondary school, but without a diploma. We do not know whether these individuals had simply not finished their education at the time of the test or whether they are dropouts from upper secondary school. We use the information from 1964 to identify dropouts. Individuals without upper secondary diploma in either 1948 or 1964 are coded as having 11 years of schooling, individuals with diploma in 1964 but not in 1948 are assigned 12 years of schooling, while individuals with diploma in 1948 are coded as having 13 years of education.[3]

We also use some family background variables. Father's education is in the original data given in six categories: primary school, on-the-job training, apprentice training, vocational education, lower secondary education, and upper secondary school or higher education. Because the first three classifications seem somewhat ad hoc we construct a dummy variable equal to unity for the three latter types of education. Family income is constructed based on

[2] There were many different ways to gain an upper secondary diploma. One could stay for either four or five years in the lower secondary school before transferring to upper secondary, and one could also stay three or four years in the upper secondary before taking the examination.

[3] 25 individuals report lower educational attainment in 1964 than in 1948. One might wonder whether this reflects misreporting in 1948. Excluding these observations from the models reported below changes the results only to a very small degree and they are thus included in the analysis with the educational attainment reported in 1948.

income information for the years 1929, 1933, 1937, 1938 and 1942. Income for both fathers and mothers is utilized, though the number of mothers with income was rather low. See Palme and Sandgren (2005) for a closer description of how this measure is constructed. In addition, we utilize information on the number of siblings and the number of adults in the home of each individual in 1938.

Descriptive statistics for the original sample of boys and the sample with IQ test results at military enrollment are presented in Table 1. The mean IQ score in 1938 is slightly below 100, which is explained by the fact that there are more overaged than under-aged pupils in the sample and the overaged had a propensity to perform worse than the average pupil, see Husén (1950). Average educational attainment is 8.07 years when those dropping out of primary education are classified as having six years of education. 12 percent of the boys in the original sample are overaged, and the number of siblings varies from zero to eight with an average of 1.6. The descriptive characteristics of the original sample and the sample we use are very similar for all variables.

Table 1 here

Correlation coefficients between the different measures of cognitive ability and the quantity of schooling for the sample used in the analysis are presented in Table 2. The correlation coefficients between the ability measures are in the range 0.61 to 0.75, with the highest coefficient for the correlation between the IQ tests. The correlation coefficients between the ability measures at age 10 and education are about 0.5, indicating that there is a causal effect of early cognitive ability on subsequent educational choices. The correlation between education and IQ at age 20 is even stronger. One possible explanation for why the correlation increases over time is that education has a positive impact on cognitive ability.

Table 2 about here

The relationship between the early and late IQ tests is illustrated in Figure 1. The regression line has a slope of 0.80 and is clearly significant, see Table 4 below. Figure 2 presents the distribution of the IQ test scores for each level of educational attainment. Panel A of Figure 2 includes all individuals in the sample used in the analysis and shows that the lower tail of the distribution is somewhat longer than the upper tail. Panel B shows that the ability distribution of the individuals dropping out of primary school moves to the left in the tails, but does not change much around the median. The mean IQ score for this group declined by 3.2 points, as shown in Table 3. The ability distribution of individuals with seven years of primary schooling change little, but the mean IQ score decreases from 94.1 to 92.6 over the 10-year period. Table 3 also shows that this group compromises of 55 percent of the sample, while some of the other attainments levels include rather few individuals. Educational attainment above the primary level is therefore grouped together pairwise in Figure 2. In particular for education above 10 years, there is a pronounced upward movement in the IQ distribution.

Figures 1 and 2 about here
Table 3 about here

In order to separate the effect of education and early cognitive ability, the variables must be sufficiently independent. With strong dependence, it is hard to isolate the effect of education from the effect of early cognitive ability, see Heckman and Vytlacil (2001) for a similar discussion. Figure 2 shows that the upper part of the ability distribution is spread across all educational levels except for primary school dropouts, while few individuals in the lower part of the distribution have 10–13 years of schooling.

Table 3 also shows that GPA, family income and the propensity for the father to have higher education increase with educational attainment. Regarding GPA, the increase is more pronounced for the scores in fourth grade than for the scores in third grade, which may be a result of the streaming decision into different tracks after the fourth grade.

3. Identification strategy

The main problem with simply relating test scores to educational attainment is the selection of the most able individuals into non-compulsory education. In this paper we take account of selection by conditioning on early cognitive ability. When the effect of education is conditional on early cognitive ability, there is no selection bias based on the early cognitive ability measure available. Measurement error in relation to the true selection variable will, however, still bias the estimates as pointed out by for example Hansen et al. (2004) We estimate the following equation.

$$IQ20_i = \alpha + \beta IQ10_i + \gamma SCHOOLING_i + \phi X_i + \varepsilon_i \tag{1}$$

where $IQ20_i$ is the IQ score at military enrollment for individual i, $IQ10_i$ is the IQ score in the third class, $SCHOOLING_i$ is the number of years of education, X_i is a vector of control variables such as the education of the father, and ε_i is an iid error term. γ is the coefficient of interest.

Hanushek (1979) and Todd and Wolpin (2003), among others, consider learning to be a cumulative process where achievement at a given point in time depends on the input histories and "endowed mental capacity" or "innate ability". Equation (1) can be interpreted within this tradition. As a measure of early cognitive ability, IQ10 captures heritable endowments and inputs into the learning process up to age 10, and SCHOOLING is simply interpreted as the quantity of a specific input acquired over time.

Heckman (2000) and Cunha et al. (2005) de-emphasize the importance of endowments and argue that a "common error in the analyses of human capital policies is the assumption that abilities are fixed at very early ages" (Heckman, 2000 p. 6). Instead, they argue that ability is created in a variety of learning situations from very early ages, and ability in turn fosters further learning. Cunha et al. (2005) formulate the technology of skill formation as $S_t = f_t(I_t, S_{t-1})$, where I_t is investment in the child at time t. On linear form with iid errors, the technology is

$$S_{it} = \alpha_t + \beta_t S_{it-1} + \gamma_t I_{it} + \eta_{it} \tag{2}$$

To highlight the importance of initial observations and the cumulative nature of learning, (2) can be written

$$S_{it} = \sum_{j=1}^{t} \alpha_j B_{j+1} + S_{i0} B_j + \sum_{j=1}^{t} \gamma_j I_{ij} B_{j+1} + \sum_{j=1}^{t} \eta_{ij} B_{j+1} \tag{3}$$

where S_{i0} is the initial skill,

$$B_j = \prod_{j=1}^{t} \beta_j \quad \text{and} \quad B_{j+1} = \begin{cases} \prod_{k=j+1}^{t} \beta_k & \text{if } j \le t-1 \\ 1 & \text{if } \quad j = t \end{cases} \tag{4}$$

(3) is identical to (1) if t denotes year and t = 10, X is included on linear form, and $\sum_{j=1}^{t} \gamma_j I_{ij} B_{j+1} = \gamma SCHOOLING_i$. When the investment variable is years of schooling, I_{it} can only take the values zero and unity and as such is an indicator variable. In this case

$\text{SCHOOLING}_{it} = \sum_{j=1}^{t} I_{ij}$ and a linear effect of SCHOOLING requires that $\gamma_j B_{j+1}$, $j \in [0,t]$, is constant. (3) can be written

$$S_{it} = \sum_{j=1}^{t} \alpha_j B_{j+1} + S_{i0} B_j + I_{i1}\gamma_1 \prod_{k=2}^{t} \beta_k + I_{i2}\gamma_2 \prod_{k=3}^{t} \beta_k + ... + I_{it-1}\gamma_{t-1}\beta_t + I_{it}\gamma_t + \sum_{j=1}^{t} \eta_{ij} B_{j+1} \quad (5)$$

For $\beta_j < 0$ and γ_j constant, the effect of investment is increasing in j and a linear effect of SCHOOLING requires that the effect of the schooling investment γ_j is decreasing. This can be seen as a result of skill depreciation over time without further investment since $\beta_j < 1$. Investment close in time has, then, a larger value than an early investment, all else equal. If the true effect of educational attainment on cognitive ability is decreasing, the effect of SCHOOLING may in fact turn out to be linear in our application. Under the assumption that individuals who leave the education system never return to take more education, the linearity of the effect of SCHOOLING can be tested by allowing for separate effects for each quantity of schooling by a dummy variable approach.

It also follows from (3) and (4) that when t increases, the effect of initial skills S_{i0} decreases. If the skill measures are close in time and β is close to unity, imposing the restriction $B_j = 1$, and thereby using the change in skills as the dependent variable, may be reasonable and not rejected by data. As the interval between the tests increases, an attractive feature in order to estimate the effect of schooling, the effect of lagged skills diminishes and such a restriction is more likely to be rejected by data.

In the literature on the return to education in the labor market, education is instrumented for two reasons. First, because more able individuals are more likely to have higher attainment, the estimated return in simple models may capture both the true return and the return to ability (Becker, 1964; Griliches, 1977; Blackburn & Neumark, 1993; Sandgren, 2005). Then the estimated return in a simple OLS model without ability measures is likely to overestimate the true return to education. This is not a problem in our study because we condition on ability. Second, most studies, at least US studies, are based on surveys, and self-reported educational attainment is to an unknown degree vulnerable to measurement error.[4] The discussion in the

[4] In a review of the literature, Card (1999) consider both these possibilities for biased estimates and concludes that the causal effect of education on earnings “is not much below the estimate that emerges form a simple cross-sectional regression of earnings on education” (p. 1855).

previous section indicates that measurement error in the education attainment measure is also a concern in our study.

The typical instrument for education is parental education or the education of a sibling.[5] In our approach however, one has to be careful in the choice of instruments because parental background can also have an independent effect on the change in IQ between the ages of 10 and 20. Education production function estimates based on cross-section data typically find strong effects of family background variables such as parental education and income. Todd and Wolpin (2003) argue that an effect of family income is a symptom of a misspecified model because the usual argument for including income is that it is an index of other inputs. Income can be used to purchase inputs, including non-compulsory schooling. According to this view, income affects the quantity of schooling but not the outcome for a given schooling level, which are the conditions for valid instruments in our case. The idea is that parental income affects educational attainment in situations with credit rationing in the education market. In the sample period, education in Sweden was free of charge, but the extensive and generous student loan system valid today had not yet been developed. Thus, education required that the family be willing and able to pay living expenses. A larger family implies that the family income has to be divided among more individuals, and thus we use family income per family member as an instrument for schooling, taking into account both the number of siblings and the number of adults living at home at age 10. It is evident in Table 3 that the average family income per family member steadily increases with the individual's educational attainment.

Teacher grading in fourth grade was important for the choice of education above the primary level. Individuals with poor marks in third grade who had motivation for higher education, either intrinsically or by pressure from their parents, would therefore have greater incentives than others to increase their effort in the fourth grade. Thus, we expect the growth in GPA from the third to the fourth grade to include information on the motivation for higher education, and this variable is included as the second instrument for educational attainment.

[5] Family characteristics as instruments for education in earning functions may be criticized because they are correlated with unobserved ability. The instrumental variable estimator of the return to education will therefore be a biased estimate of the true return. This is not a major concern in our model because we explicitly condition on ability.

If cognitive ability is multidimensional, selection based on other dimensions of ability than those captured by the IQ10 variable may bias the estimates. This may also be seen as measurement error. There are two reasons why we do not think this is a serious problem in the present analysis. First, we use similar tests of cognitive ability as measures of both early cognitive ability and adult cognitive ability and serious efforts were made to make the tests as accurate and comparable as possible. Second, in the data there are additional measures of ability at an early age as described in Section 2. In some specifications below we condition on grade point average and teacher rating of the students in third grade in addition to the IQ test, thereby conditioning on other dimensions of ability in addition to the ones relevant for the IQ test.

Todd and Wolpin (2003) show that pretty strong assumptions on the cumulative learning process and the impact of ability endowment have to be fulfilled if true effects of educational inputs are to be identified by the value-added approach. Lagged achievement has to capture two different things empirically. One way to solve the problem proposed by Todd and Wolpin (2003) is to use earlier observations of achievement as instruments for lagged achievement, an approach that is common in the literature on dynamic panel data analyses, which also ideally will solve any problem related to measurement error.

In our data, we have information on only two IQ tests, but we will utilize information from the other achievement measures available from third grade to form instruments for IQ10 as an alternative to include these ability measures in the equation of interest. If only the objective IQ measure in third grade has a direct impact on IQ20, and not more subjective measures such as teacher rating and school achievement as measured by GPA, the latter measures are valid instruments for IQ10. This is a reasonable assumption since Husén (1950) argues strongly that the same types of ability are evaluated in both tests. De facto we use teacher evaluations from the same time period instead of lagged comparable test results as instruments, utilizing the fact that the tests are different but still correlated with the IQ test. The over-identification restrictions will be tested by the standard Sargan test.

4. Empirical results

Table 4 presents our basic model Equation (1) above estimated using ordinary least squares. The model in Column (1) includes only early cognitive ability and a dummy variable for overaged pupils in the third grade. The dummy variable has, as expected, a negative effect that is highly significant. The effect of the IQ test at age 10 is highly significantly lower than unity, indicating that restricting the coefficient to be equal to unity – as in models using the change in the IQ score as the dependent variable – is not in accordance with the data generating process.[6]

Table 4 about here

Column (2) in Table 4 includes educational attainment. The effect is equal to 3.5, or a bit more than 20 percent of a standard deviation. The model therefore predicts that an education of 12 years, compared to only primary school of seven years, increases IQ by about one standard deviation. Column (3) shows that the result is not sensitive to the inclusion of overaged individuals and individuals taking the IQ test in 1947 in the sample. In contrast, column (4) shows that the result is highly sensitive to the conditioning on early cognitive ability.

The model formulation in column (3) in Table 4 includes only two variables and is thus attractive to judge the relative importance of early cognitive ability and education. A model including only IQ10 explains 47 percent of the variation in IQ20, a model including only SCHOOLING explains 40 percent of the variation, while the parsimonious model explains 61 percent of the variation. Early cognitive ability therefore seems to be slightly more important than educational attainment. The same picture emerges when comparing the estimated coefficients. In column (3) in Table 4, the effect of increasing IQ10 by one standard deviation is 7.4, while the effect of increasing SCHOOLING by one standard deviation is 6.2. Compared to Winship and Korenman (1997, 1999), our standardized estimate of educational attainment is slightly larger and the estimate for early cognitive ability is slightly lower.

In column (5) in Table 4 we include controls for family income per family member and father's education, which lowers the estimate of schooling marginally. Notice that if measurement error in schooling gives a negatively biased coefficient of schooling, and family income is a strong predictor of schooling without any causal effect on IQ, there should be a

[6] If a dummy variable for whether the IQ20 test was taken in 1947 or 1948 is included in the model, it is highly insignificant with t-value below unity in all specifications. All the overaged individuals took the test in 1947, but only six percent of the normal-aged individuals. The dummy variable is therefore not included in the models reported.

positive effect of income in this specification even though the true effect is zero. The effect of family income is in fact negative, but clearly insignificant.[7] The effect of father's education is positively significant at five percent level as expected. In the next column, we include the other two ability measures from third grade, grade point average and teacher's rating of general cognitive ability. The effect of both variables is positive, which indicates that they capture some dimensions of ability relevant in the IQ test in 1948 but not in that of 1938. However, if there is measurement error in IQ10, one would expect similar effects because the ability variables are highly correlated. Only the teacher grading is significant at five percent level. Inclusion of the additional ability measures further reduces the estimated effect of schooling. Compared to the simple model in column (2), the effect of educational attainment is reduced by 16 percent when all control variables are included.[8]

So far it is assumed that the effect of education is linear. This can be tested by using a dummy variable approach. Figure 3 presents the results using primary school attainment as the comparison group together with the regression line from the linear model. The effect of schooling seems reasonably linear. The confidence interval of all dummy variables includes the effect following from the linear model, except the dummy variable for less than primary school for which the 95 percent confidence interval is marginally below the regression line. The estimates of nine and 10 years of education differs markedly (although the linear regression line is within two standard errors of the point estimate of both dummy variables), which may be a result of the fact that a majority of those coded with nine years of education went through vocational education, while those with 10 years of education mainly had more academic forms of education.[9]

Figure 3 about here

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[7] The variable family income per family member includes information on both family income, the number of siblings and the number of adults at home. When allowing for separate effects of these three variables, the effect of income is positive and the effect of the number of siblings is negative, but none of the variables has a significant effect at five percent level.

[8] Information on family background and the additional ability variables is missing for some individuals, but the reduced effect of schooling is not a result of a smaller sample in the latter model. When estimating the model in column (2) with the same observations as the model in column (6), the effect of schooling is equal to 3.59.

[9] In order to test for linearity, we replaced one of the dummy variables with the linear SCHOOLING variable and tested the joint significance of the remaining dummy variables by an F-test. The test statistic is $F(6, 640) = 2.31$ with a p-value of 0.03. Excluding dropouts, the effect of schooling in the simple linear model reduces from 2.47 to 2.41, and in the dummy variable approach the p-value of joint significance increases to 0.08.

If we have identified a causal effect of education, education achieved after the IQ test at age 20 should have no effect on that IQ test. In principle this hypothesis can be tested by using information on the quantity of schooling later in life as a falsification test of the model specification. Unfortunately, higher education attainment reported later than the age of 20 may simply reflect measurement error. However, we utilize information that seems truly reliable in this matter. First, individuals with 12 or 13 years of education in 1948 and higher attainment in 1964 are assumed to have taken the extra education after 1948. It was not feasible to have more than 12 or 13 years of education in 1948. Second, the survey from 1964 includes self-reported occupations from 1942. Unfortunately, there is clearly underreporting of being a student. However, we utilize the information from the individuals reporting themselves as having been students after 1948, and as having less than 12 years of schooling in 1948.

Extending the dummy variable model specification reported in Figure 3 with dummy variables for the number of years of education after 1948, none of the latter dummy variables are significant at 10 percent level.[10] This result indicates that the effect of schooling estimated above is a causal effect and not a selection effect in the sense that the most able individuals are self-selected into the highest educational attainment.

4.1. Instrumental variables estimation

The estimators above may be biased because of potential measurement error in the schooling variable and overloaded information requirement for early cognitive ability. Table 5 presents results from two-stage least squares models. All first-stage regressions are reported in Appendix Table A1. Firstly we only instrument schooling and continue assuming that IQ10 is exogenous. Columns (1) – (3) in Table A1 show that both instruments of schooling, family income per family member and the change in GPA from third to fourth grade, have a highly significant effect on schooling. An F-test clearly rejects the hypothesis of no joint effect of the instruments. Increasing family income by one standard deviation raises schooling by about 0.5 years, indicating that credit constraints may have played a role, although other interpretations are possible. In addition, IQ at age 10 is, as expected, highly significant. Increasing IQ by one standard deviation also raises schooling by about 0.5 years.

[10] Very few have more than four years of education after 1948, a group consisting only of individuals with less than 12 years of education in 1948, who are therefore grouped together. The estimated effects are 3.3, 2.5, 5.4 and 5.2 for one, two, three and four or more years of education after 1948, respectively.

Table 5 about here

Column (1) in Table 5 shows that the effect of schooling is equal to 3.96 when schooling is instrumented, that is about 15 percent higher than the comparable OLS specification. The dummy variable for whether the father has higher education is not significant when included in column (2) in Table 5, but has a strong effect on schooling.[11] The same is true for GPA and teacher rating in third grade. In order to give some indication of the validity of the instruments, Table 5 reports the results of the Sargan test for overidentifying restrictions. The test statistic is in fact close to zero, clearly indicating that the model is not misspecified. The extremely low value of the Sargan test statistic combined with very strong effects of the instruments in the first-stage regressions might seem surprising. One may question the power of the Sargan test, but the Sargan test is mainly regarded as having low power when a large number of instruments are included in the model.

The results so far indicate that there might be some measurement error bias in the OLS results. However, there is a danger of overestimating the coefficient for schooling when early cognitive ability is treated as exogenous. If early cognitive ability is endogenous and downward biased, as argued by, for example, Todd and Wolpin (2003), the effect of schooling will be overestimated because the variables are highly positively correlated. The first-stage results show that GPA in both third and fourth grade and the teacher rating in third grade are highly correlated with IQ10. In addition, the effect of family income is significant at five percent level, perhaps because the IQ of the parents increases their income as found in several studies, see for example Altonji and Pierret (2001) and Falch and Sandgren (2006). For the model of interest, the effect of early cognitive ability increases slightly and the effect of schooling decreases slightly compared to the previous models as expected. The same is true if father's education is included in the model. The p-values of the Sargan test for overidentifying restrictions still clearly indicate that the models are not misspecified.

In the reported models, GPA in fourth grade is in reality one of the instruments for IQ in third grade. A concern is whether the results are sensitive to the fact that one ability variable used

[11] This result indicates that father's education is a valid instrument for schooling. Extending the instrument set with this variable, the effect of schooling increases to 4.61, and the p-value of the Sargan test for overidentifying restrictions is equal to 0.25.

as instrument is measured later in time than the ability variable of interest. We have estimated models excluding the change in GPA from third to fourth grade from the instrument set. This does not alter the results.[12] Neither do the results seem sensitive to the exclusion of other instruments.[13]

4.2. Nonlinear effects

Heckman (2000) and Cunha et al. (2005) argue that skill formation is complementary in the sense that ability fosters further learning. Skills produced raise the productivity of subsequent investment in skills. In the formal modeling framework above where $S_t = f_t(I_t, S_{t-1})$, they argue that $\partial^2 S_t / \partial I_t \partial S_{t-1} > 0$. The hypothesis can be tested in our framework by including an interaction term between IQ10 and SCHOOLING. We rely on the OLS models in this section since the effect of schooling differ little between the simple models in Table 4 and the models instrumenting both SCHOOLING and IQ10 in Table 5. The results are presented in column (1) of Table 6. The interaction term in fact turns out to be negative and significant at five percent level, in contrast to the hypothesis put forward by Heckman (2000) and Cunha et al. (2005), but in line with the result of Winship and Korenman (1999). The model implies an effect of SCHOOLING of 4.6 for IQ10 equal to 70 and an effect of 2.7 for IQ10 equal to 130.

Table 6 about here

The negative interaction effect may be a result of other omitted nonlinearities. For example, it is reasonable that the return to early cognitive ability is declining. Column (2) in Table 6 includes a squared term of early cognitive ability, and the effect is negatively significant at five percent level. The models in columns (3) and (4) show that we are not able to distinguish between skill complementarity and declining return to early cognitive ability within the present dataset. The data seem to include too little information to be able to estimate nonlinear effects in a precise way.

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[12] Excluding the change in GPA from third to fourth grade from the instrument set in the model formulation column (5) in Table 5, the effect of IQ10 decreases from 0.62 to 0.60 and the effect of SCHOOLING increases from 3.72 to 3.85. The standard errors increase only marginally. The p-value of the Sargan test is equal to 0.56.

[13] For example, using only family income and GPA in third grade as instruments in the model formulation column (5) in Table 5, the effect of IQ10 and SCHOOLING is equal to 0.65 and 3.58, respectively, and clearly significant. Replacing family income per family member with the number of siblings (or, alternatively, the pure income measure) in the original instrument set, the effects are 0.60 and 3.89 (0.62 and 3.59), respectively, with a p-value of the Sargan test equal to 0.47 (0.94).

5. Conclusions

This paper clearly indicates that ability as measured by commonly used IQ tests is positively affected by education. Based on ordinary least squares we estimate the return to one year of schooling to be 2.8–3.5 IQ points on average in models where we condition on early cognitive ability to take selection into non-compulsory schooling into account. This estimate is likely to be biased downward if there is measurement error in educational attainment. Using instrumental variables the return to schooling is estimated to be 3.8–4.1 IQ points if schooling is treated as endogenous and to be 3.5–3.8 if both schooling and early cognitive ability are treated as endogenous. Our best estimate is a return to schooling of about 3.7 IQ points. About four years of schooling will on average increase IQ by about one standard deviation, which is a sizable effect. This effect is in the upper part of the range estimated by Winship and Korenman (1997, 1999) and above the estimates of Hansen et al. (2004).

The evidence that schooling affects general intelligence as thinking and reasoning is not in accordance with simple signaling models of educational attainment but in accordance with the view that a positive return to education in the labor market follows at least partly from increased general ability and not only from specific subject skills or signaling. The results also indicate that it is difficult to distinguish between the return to education and the return to ability in the labor market. The total return to education may include both a direct effect and an indirect effect via the impact on general ability.

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Table 1. The measures of ability and education

	Original sample of men			Sample used		
	Obser-vations	Mean	Standard deviation	Obser-vations	Mean	Standard deviation
IQ at age 20 (IQ20)	653	97.6	16.5	652	97.5	16.4
IQ test at age 10 (IQ10)	834	97.7	16.0	652	97.1	15.4
Grade point average (GPA) in third grade	799	3.49	0.57	637	3.48	0.58
Grade point average (GPA) in fourth grade	790	3.56	0.65	634	3.54	0.65
Change in GPA from third to fourth grade	786	0.07	0.42	632	0.06	0.42
Teacher overall rating (Rating)	765	2.89	1.22	595	2.90	1.20
Education attainment (SCHOOLING)	658	8.06	1.82	650	8.07	1.82
Born early	834	0.12	0.33	652	0.10	0.31
Log(family income)	774	8.31	0.54	619	8.27	0.52
Father has higher education	799	0.16	0.37	630	0.14	0.35
Number of siblings	786	1.56	1.56	623	1.58	1.57

Table 2. Correlation coefficients between ability variables and educational attainment

	IQ10	GPA	Rating	SCHOOLING
IQ at age 20 (IQ20)	0.75	0.61	0.61	0.68
IQ test at age 10 (IQ10)		0.62	0.65	0.50
Grade point average third grade (GPA)			0.73	0.54
Teacher rating third grade (Rating)				0.51

Table 3. Ability and family income by educational attainment

SCHOOLING	Observations	Mean IQ10	Mean IQ20	Mean GPA third grade	Mean GPA fourth grade	Mean family income	Father has higher education
6	52	75.8	72.6	3.03	2.95	3.14	0.06
7	360	94.1	92.6	3.30	3.32	3.66	0.06
8	15	106.5	106.6	3.77	3.87	4.33	0.13
9	85	100.6	101.6	3.56	3.68	4.75	0.16
10	66	110.6	113.7	3.97	4.24	5.11	0.23
11	5	103.0	117.4	3.88	3.94	7.12	0.40
12	55	110.7	118.4	4.13	4.31	11.79	0.52
13	12	107.9	120.5	4.20	4.48	10.57	0.50
All	650	97.2	97.5	3.48	3.54	4.81	0.14

Note. The number of observations is valid only for the two IQ scores. For GPA third grade, GPA fourth grade and family income there are 11, 16 and 54 missing observations, respectively. Family income is measured in thousands of 1938 SEK.

Table 4. The effect of educational attainment on ability, dependent variable is IQ20

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IQ10	0.73	0.54	0.53	-	0.54	0.44	0.43
	(0.03)	(0.03)	(0.03)		(0.03)	(0.04)	(0.04)
Born early	-9.41	-6.71	-	-13.9	-6.71	-7.21	-
	(1.47)	(1.28)		(1.54)	(1.37)	(1.48)	
SCHOOLING	-	3.47	3.47	5.41	3.30	2.90	2.81
		(0.23)	(0.24)	(0.26)	(0.26)	(0.30)	(0.32)
Family income per family member / 1000	-	-	-	-	-0.16	0.87	1.11
					(0.22)	(0.68)	(0.76)
Father has higher education	-	-	-	-	3.12	2.74	3.15
					(1.17)	(1.30)	(1.40)
GPA third grade	-	-	-	-	-	2.23	2.02
						(1.06)	(1.19)
Teacher rating third grade	-	-	-	-	-	0.81	0.83
						(0.53)	(0.60)
R^2	0.588	0.696	0.610	0.519	0.700	0.700	0.609
Sample	All	All	Born in 1928 and tested in 1948	All	All	All	Born in 1928 and tested in 1948
Observations	652	650	549	650	587	530	449

Note. Standard errors in parentheses.

Table 5. The effect of educational attainment by using 2SLS, dependent variable is IQ20

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IQ10	0.52	0.51	0.42	0.41	0.62	0.63	0.57
	(0.04)	(0.04)	(0.04)	(0.05)	(0.10)	(0.10)	(0.11)
Born early	-6.59	-6.51	-6.72	-	-5.00	-4.94	-
	(1.44)	(1.46)	(1.53)		(1.70)	(1.72)	
SCHOOLING	3.96	3.85	4.06	4.15	3.72	3.48	3.82
	(0.63)	(0.69)	(0.69)	(0.80)	(0.82)	(0.93)	(0.98)
Father has higher education	-	2.55	1.99	2.31	-	2.40	2.40
		(1.51)	(1.50)	(1.67)		(1.61)	(1.72)
GPA third grade	-	-	1.41	0.99	-	-	-
			(1.17)	(1.35)			
Teacher rating third grade	-	-	0.60	0.63	-	-	-
			(0.57)	(0.65)			
Endogenous variables	SCHOOLING	SCHOOLING	SCHOOLING	SCHOOLING	SCHOOLING IQ10	SCHOOLING IQ10	SCHOOLING IQ10
Sargan, p-value	0.99	0.94	0.77	0.99	0.82	0.91	0.92
R^2	0.691	0.694	0.692	0.594	0.681	0.683	0.589
Sample	All	All	All	Born in 1928 and tested in 1948	All	All	Born in 1928 and tested in 1948
Observations	581	571	524	443	533	524	443

Note. Standard errors in parentheses. Instruments for SCHOOLING are family income per family member and the change in GPA from third to fourth grade. Instruments for IQ10 are GPA in third grade and teacher rating in third grade.

Table 6. Nonlinear effects of educational attainment, dependent variable is IQ20

	(1)	(2)	(3)	(4)
IQ10	0.80	1.10	1.05	1.08
	(0.12)	(0.26)	(0.26)	(0.35)
Born early	-5.76	-5.88	-5.59	-
	(1.35)	(1.33)	(1.36)	
SCHOOLING	6.91	3.53	5.67	5.85
	(1.63)	(0.23)	(1.99)	(2.17)
IQ10 * SCHOOLING	-0.033	-	-0.020	-0.023
	(0.015)		(0.019)	(0.021)
IQ10 * IQ10 / 100	-	-0.29	-0.18	-0.19
		(0.13)	(0.17)	(0.21)
R^2	0.698	0.698	0.699	0.613
Sample	All	All	All	Born in 1928 and tested in 1948
Observations	650	650	650	549

Note. Estimated by Ordinary Least Squares. Standard errors in parentheses.

Figure 1: The relationship between IQ at age 20 and age 10

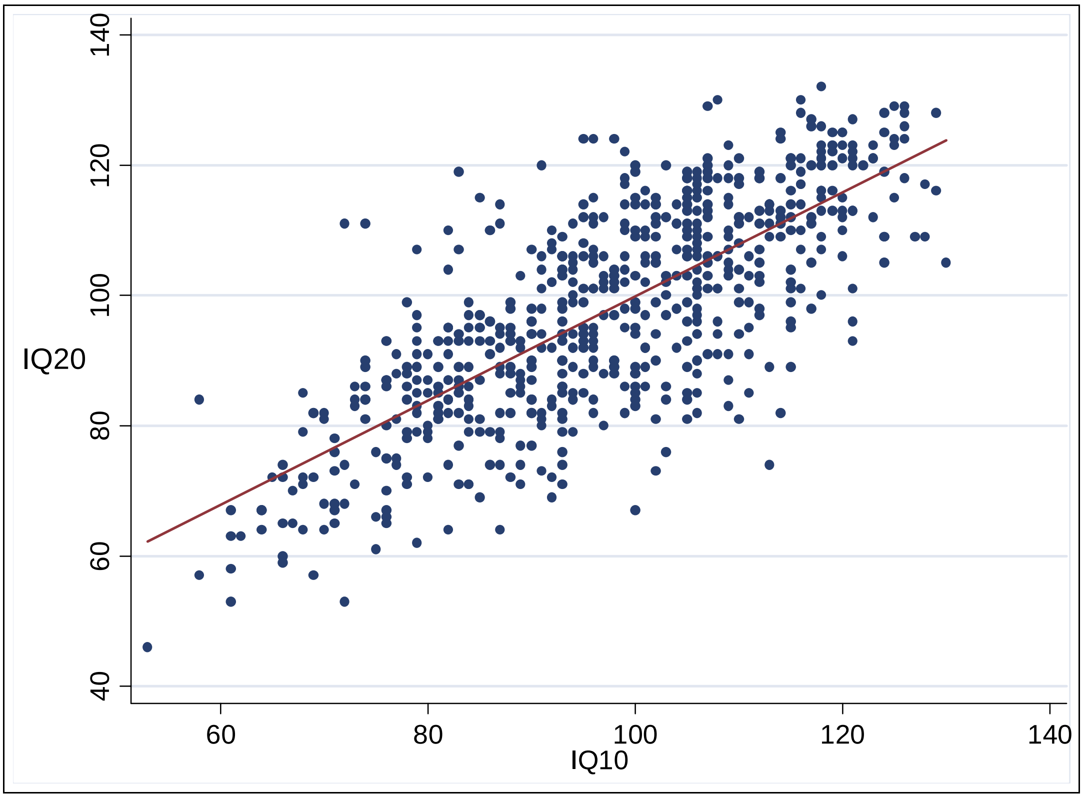
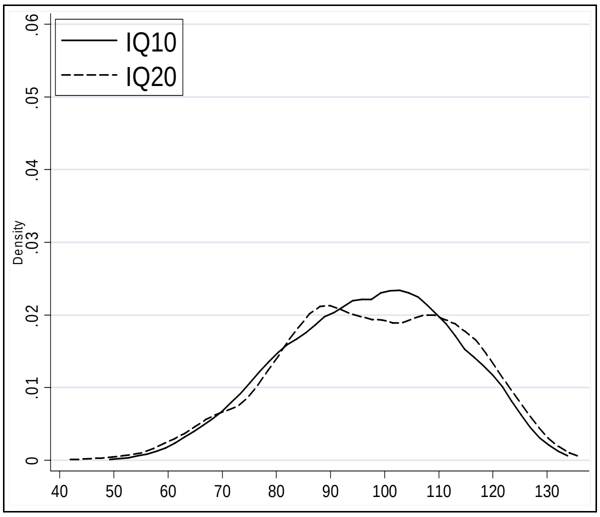
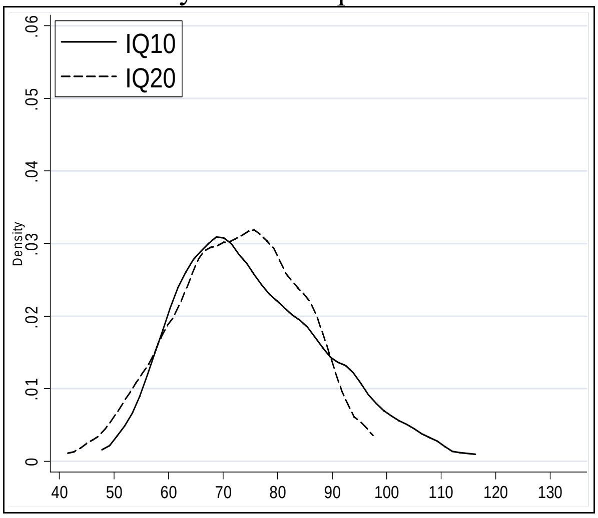


Figure 2. Changes in the distribution of IQ scores by educational attainment

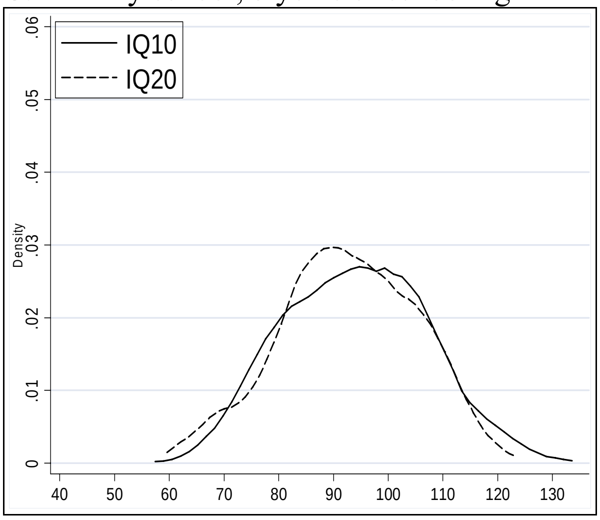
A. All observations



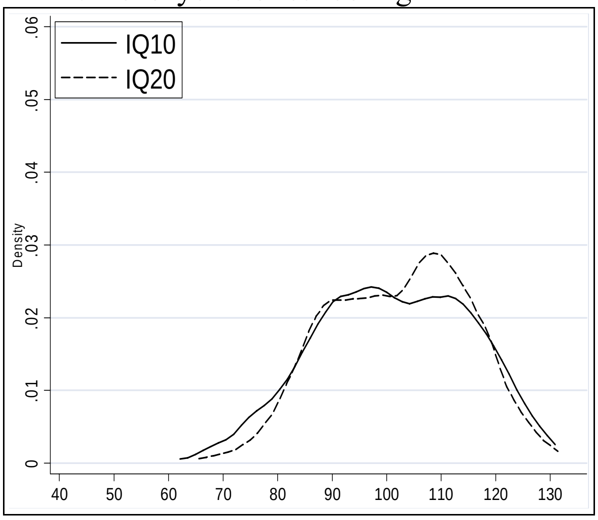
B. Primary school dropouts



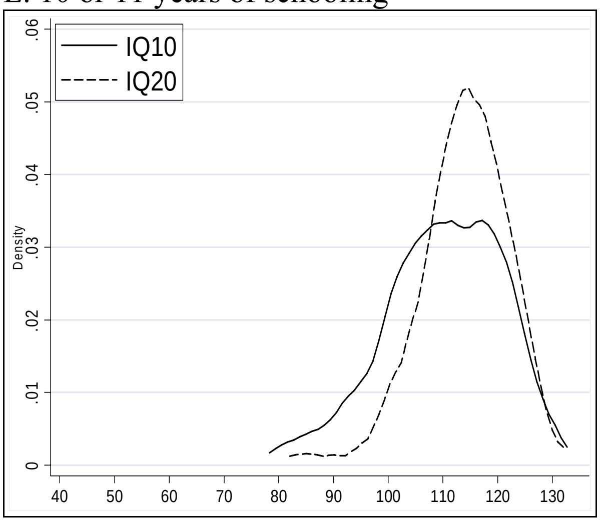
C. Primary school, 7 years of schooling



D. 8 or 9 years of schooling



E. 10 or 11 years of schooling



F. 12 or 13 years of schooling

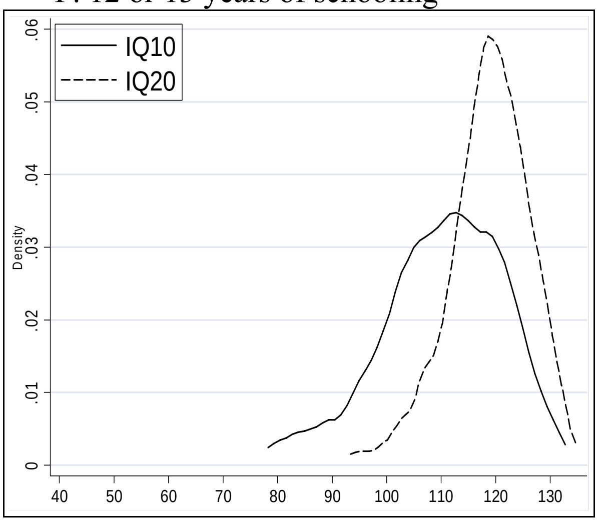


Figure 3. Nonlinear effect of SCHOOLING on ability with 95 percent confidence interval

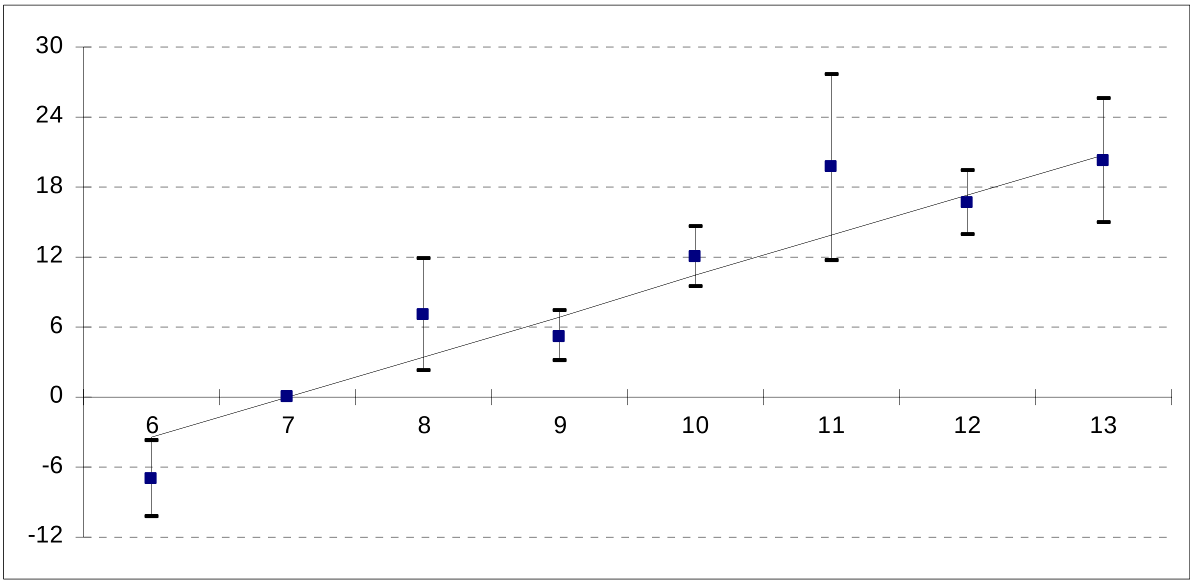


Table A1. First-stage regressions, dependent variable is SCHOOLING

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable	SCHOOLING	SCHOOLING	SCHOOLING	SCHOOLING	SCHOOLING	IQ10	SCHOOLING	IQ10	SCHOOLING	IQ10
Family income per	0.64	0.82	0.69	0.67	0.55	0.65	0.70	1.12	0.67	0.45
family member / 1000	(0.07)	(0.09)	(0.09)	(0.10)	(0.07)	(0.52)	(0.09)	(0.75)	(0.10)	(0.80)
Change in GPA from	0.49	0.44	0.93	0.89	1.08	5.39	0.98	5.16	0.93	4.54
third to fourth grade	(0.14)	(0.14)	(0.15)	(0.17)	(0.15)	(1.18)	(0.15)	(1.20)	(0.16)	(1.26)
GPA third grade	-	-	1.10	1.18	1.33	9.79	1.20	9.45	1.27	8.91
			(0.16)	(0.18)	(0.15)	(1.22)	(0.15)	(1.25)	(0.18)	(1.35)
Teacher rating third	-	-	0.12	0.08	0.16	3.87	0.16	3.93	0.12	4.10
grade			(0.08)	(0.09)	(0.08)	(0.60)	(0.07)	(0.61)	(0.09)	(0.66)
IQ10	0.045	0.041	0.010	0.010	-	-	-	-	-	-
	(0.004)	(0.004)	(0.005)	(0.006)						
Born early	-0.56	-0.51	-0.35	-	-0.49	-9.80	-0.44	-9.61	-	-
	(0.21)	(0.21)	(0.21)		(0.21)	(1.64)	(0.20)	(1.65)		
Father has higher	-	0.75	0.63	0.66	-	-	0.63	0.46	0.68	1.30
education		(0.19)	(0.18)	(0.20)			(0.18)	(1.51)	(0.20)	(1.53)
R^2	0.373	0.431	0.513	0.458	0.473	0.540	0.510	0.542	0.455	0.463
F-test for instruments	51.2	46.9	54.4	40.4	95.4	104.0	83.8	98.7	65.7	88.4
(degrees of freedom)	(2, 576)	(2, 565)	(2, 516)	(2, 436)	(4, 527)	(4, 527)	(4, 517)	(4, 517)	(4, 437)	(4, 437)
Sample	All	All	All	Born in 1928 and tested in 1948	All	All	All	All	Born in 1928 and tested in 1948	Born in 1928 and tested in 1948
Observations	581	571	524	443	533	533	524	524	443	443
Second step regression in Table 5 column	(1)	(2)	(3)	(4)	(5)	(5)	(6)	(6)	(7)	(7)

Note. Standard errors in parentheses.