THE EFFECT OF EDUCATION ON COGNITIVE ABILITY

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This paper analyzes whether schooling increases intelligence measured by intelligence quotient (IQ). We use a longitudinal dataset where the individuals have conducted IQ tests both at ages 10 and 20. We estimate the effect of schooling on IQ at age 20 conditional on IQ at age 10 and other measures of early cognitive ability to account for selection into noncompulsory schooling. Ordinary least squares estimates indicate that 1 year of schooling increases IQ by 2.9–3.5 points (about 0.2 SD *deviations), and instrumental variables estimates are similar.* (*JEL* I21, J24)

I. INTRODUCTION

An implicit assumption in the human capital literature is that education affects individuals' general and analytical skills, and not only achievements narrowly related to the curriculum. A general concern in the empirical literature on human capital investments is the extent to which the effect of observed investments reflects unobserved ability. If noncompulsory schooling is only a signaling device, general cognitive skills should not be affected by schooling choices. 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 intelligence quotient (IQ) tests as these are designed to test "thinking skills" or "intelligence."

This paper investigates whether formal schooling improves IQ scores. The empirical challenge is to isolate the effect of schooling on cognitive ability from the effect of latent ability. Latent ability is a strong predictor of schooling, at least in a signaling setting. It is thus essential to take selection into noncompulsory schooling into account in order to compare individuals who are initially seemingly identical. Hansen, Heckman, and Mullen (2004) use NLSY data on achievement and solve the selection problem by conditioning on estimated latent ability, utilizing the fact that the individuals have conducted the cognitive test at different ages (between 15 and 22 years of age) and that some have completed their schooling at the date of the test. Although this may be a reasonable approach, an approach that conditions on observed early cognitive ability as in Winship and Korenman (1997, 1999) may seem easier to interpret.

We use the Malmö Longitudinal Dataset, a dataset much richer on ability measures 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 sample consists of the population of third graders in the city of Malmö in 1938. Ten years later, a major effort was made

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ABBREVIATIONS

AFQT: Armed Forces Qualifying Test
GPA: Grade Point Average
IQ: Intelligence Quotient
IV: Instrumental Variable
OLS: Ordinary Least Squares

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by Husén (1950) to collect the results on the IQ test at military enrollment for all individuals in the initial sample. According to Husén, who was also involved in the construction of the latter test, the IQ tests are highly comparable.

In the empirical period, compulsory schooling began the year the child turned seven and lasted 7 years only. In addition to ordinary least squares (OLS), we use instrumental variable (IV) techniques in line with the literature on the return to education in the labor market. Measurement error in education might be a problem, children who believe they will gain most in their development of cognitive skills by remaining in school might be most likely to do so, and people investing in schooling might be more likely to invest more in cognitive skills in other ways. As instruments, we use average family income during childhood, the tracking decision after fourth grade, and the growth in the grade point average (GPA) from the end of third to the end of fourth grade as assessed by the teacher. The two latter variables are attractive because tracking of the students started in grade five, partly based on GPA, and the former variable is attractive in the value-added model formulation because causal evidence indicates that credit market constraints played a role in the empirical period. We present results using the instruments separately, which identifies the schooling effect on very different variations, and jointly in the same model.

How intelligence is determined is an old research question within psychology. Ceci (1991, 703) argues that "there is now considerable evidence for the importance of variation in schooling on IQ." In general, the approach taken to handle sorting into education tends to be rather weak, though some rather old studies investigate the effect of schooling using IQ tests at two different ages. 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 conditional model. All these studies find strong effects of schooling. However, they include few control variables if any, they do not pursue IV techniques, and they do not use schooling measured 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 1 year of schooling.

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, Hanushek, and Kain (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 input variable, namely, the quantity of schooling. We go a long way in responding to the criticism against the value-added modeling tradition raised by Todd and Wolpin (2003) using teacher evaluations as instruments for the IQ test result at age 10. Although 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, Hanushek, and Kain (2005), our results indicate that investments 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 von Wachter (2008) find no effect for Germany. The present paper analyzes in a direct way whether schooling affects skills, and we will allow for interaction effects between schooling and early cognitive ability.

Section II gives a closer description of the data. The identification issue is discussed in Section III, whereas the empirical results are presented in Section IV. Section V provides some concluding comments.

II. 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.

Military enrollment was compulsory for men at the time, and all enrolled had to take an IQ test. We use the score on this test conducted at the enrollment in 1947 and 1948 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 a voluntary basis (e.g., in officer training), they were seamen, or their state of health was regarded as inadequate for military service. Information from military enrollment was merged with the original dataset from 1938 by Husén (1950).

The ability measures from third grade include a thorough IQ test. The original purpose of 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 IQ test. Normally, they were in their tenth year of life.[1] The test was constructed after thorough testing of third graders the year before and consisted of four parts: word opposites, sentence completion, perception of identical figures, and disarranged sentences. The IQ tests are further described in the Appendix.

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 Raven's matrices. Involved in the construction of this test was educational psychologist Torsten Husén, who devoted a lot of work in order to make the test comparable with the Malmö test from 1938. The IQ test conducted by those who enrolled in the military in 1947 was of a slightly different kind, but Husén (1950) reports that the correlation coefficient between the tests in 1947 and 1948 was 0.91, indicating that both tests measure the same ability functions. Ninety-four percent of the normal-aged individuals conducted the test in 1948, whereas 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. Overall, the different IQ tests should be well suited for comparison with each other.[2]

There is additional information on early cognitive ability in the data. We will utilize GPA from third and fourth grade as well as a teacher rating index from third grade. Teachers were asked to give an 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 7 years of schooling in the Malmö region. At the time, the Swedish school system was comprehensive for only the first 4 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 five additional years. The tracking was based on GPA and individual wishes. Teacher grading in fourth grade was therefore important for the educational possibilities above the compulsory level. The lower secondary school was a prerequisite for enrollment in upper secondary school, which normally lasted 4 years.[3] Individuals that finished primary school either entered the labor market or continued with more vocational schooling, of which there were several kinds, generally lasting 1 or 2 years.

We use the educational information collected at the time of the second IQ test. The

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.

2. Our dependent variable is not directly comparable with the variable used in Winship and Korenman (1997, 1999) and Hansen, Heckman, and Mullen (2004). They use the U.S. Armed Forces Qualifying Test (AFQT), which is a qualification test for enlistment in the U.S. armed forces, and is therefore to a larger degree a test of specific skills than a traditional IQ test.

3. There were many different ways to gain an upper secondary diploma. One could stay for either 4 or 5 years in the lower secondary school before transferring to upper secondary, and one could also stay 3 or 4 years in the upper secondary school before taking the examination.

information was not simply self-reported on a questionnaire, but collected by the test-examiner together with marks in different subjects at school. The enlistments were instructed to bring their grade reports. Thus, it seems like the educational information from 1948 is almost without errors.

Educational information is also available from a questionnaire distributed in 1964, which was combined with central school registers. This information is less suitable in the present case as we cannot always be certain whether the reported education was acquired before or after military enrollment. There are, however, some limitations in 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 noncompulsory 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 6 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. However, it seems likely that the military test was taken before the end of the school year as very few report to have the diploma from upper secondary education. Many individuals reported in 1948 that they had some upper secondary school, but no 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, whereas individuals with diploma in 1948 are coded as having 13 years of education.[4]

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 income information for the years 1929, 1933, 1937, 1938, and 1942. Income for both fathers and mothers are utilized, though the number of mothers with income was rather low; see Palme and Sandgren (2008) for a closer description of how this measure is constructed. We utilize information on the number of siblings and the number of adults in the home of each individual in 1938. Month of birth is found to be related to student achievement in several recent studies; see, for example, Bedard and Dhuey (2006). In addition, to control for different learning environments up to third grade, we include fixed effects for class in school. The students have basically the same classmates in the first 4 years at school, and the boys in the data were enrolled in 43 different classes in 19 different schools.

Malmö was, and still is, the third most populous city in Sweden. The municipality consists of mostly urban areas, but also some rural parts. Manufacturing has always been an important part of the local economy, and one of the world's largest shipyards was located in the city in the 1950s and 1960s. Husén (1950) compares the results on the IQ test at military enrollment for the Malmö sample with the rest of the country. He concludes that the average score for the Malmö sample is very similar but marginally higher than the country average.[5]

4. Twenty-five individuals report lower educational attainment in 1964 than in 1948. One might wonder whether this reflects misreporting in 1948 or 1964. Excluding these observations from the models reported changes the results only to a very small degree and they are thus included in the analysis with the educational attainment reported in 1948.

5. Data from Statistics Sweden show that average earnings in 1982 for men in the Malmö sample was 103,000 SEK with standard deviation of 59,000. The average earning in 1982 of all men in Sweden born in 1928 was 93,000 SEK with standard deviation of 66,000. The slightly higher average wage in the Malmö sample is likely due to higher wages in urban than in rural areas. Thus, this information indicates that the sample is reasonably representative for Swedish teenagers in the 1940s.

TABLE 1
The Measures of Ability and Education

	Original Sample of Men			Sample Used		
	Observations	*M*	*SD*	Observations	*M*	*SD*
IQ test 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
GPA in third grade	799	3.49	0.57	637	3.48	0.58
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
Month of birth	834	6.45	3.45	652	6.53	3.42
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

Descriptive statistics for the original sample of boys are presented in Table 1. Both the mean IQ score at age 10 (IQ10) and age 20 (IQ20) are slightly below 100, which is explained by the fact that there are more overaged than underaged individuals in the sample and the overaged had a propensity to perform below average, see Husén (1950). Twelve 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.

Seventy-eight percent of the original sample enrolls in military service in 1947 or 1948. The descriptive characteristics for the original sample and the sample with IQ test results at military enrollment are very similar for all variables. For example, the difference in GPA and teacher rating is clearly below 1% of the standard deviations. Average educational attainment is 8.07 years when those dropping out of primary school are classified as having 6 years of education.

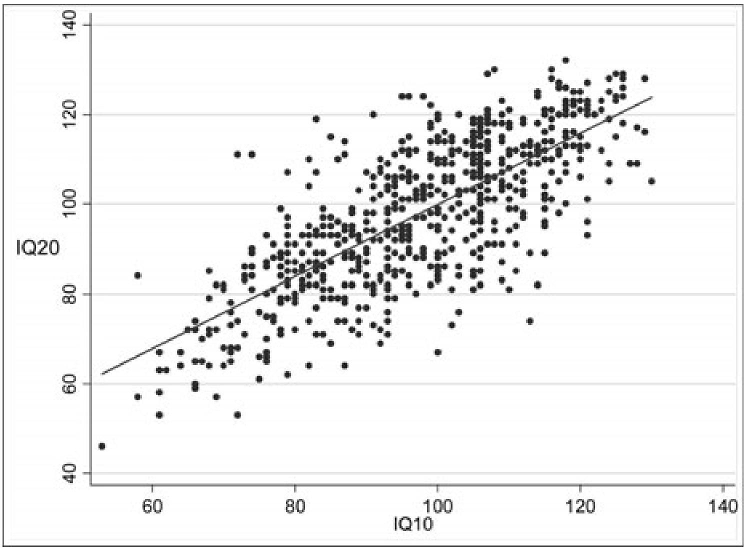
Correlation coefficients between the different measures of cognitive ability and the quantity of schooling for the sample used in the analysis are presented in Table 2. The correlation coefficients between the ability measures are in the range 0.61–0.75, with the highest coefficient for the correlation 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
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
GPA third grade			0.73	0.54
Teacher rating third grade (Rating)				0.51

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. Figure 2 presents the distributions of the IQ test scores for each level of educational attainment. Figure 2A 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. Figure 2B 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 distributions of individuals with 7 years of primary schooling are about the same at age 10 and age 20, but the mean IQ score decreases from 94.1 to 92.6 over the period. Table 3 also shows that this group comprises 55% of the sample, whereas some of the other attainment levels include rather few individuals. Educational attainment above the primary level is

FIGURE 1
The Relationship between IQ at Age 20 and Age 10



therefore grouped together pairwise in Figure 2. In particular, for education above 10 years, there is a pronounced upward movement in the IQ distribution.

In order to separate the effects 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, whereas 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 are positively related to educational attainment. Regarding GPA, the association 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.

III. IDENTIFICATION

The main problem with simply relating test scores to educational attainment is the selection of the most able individuals into noncompulsory education. Heckman (2000) and Cunha et al. (2006) 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. (2006) 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, and $t = 0$ is the initial period. On linear form with errors η and notation i for individuals, the technology can be written

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

where $0 < \beta_t < 1$, $\gamma_t > 0$, and $\varphi_t > 0$. We will start the analysis without the interaction effect. 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." To highlight the importance of initial observations and the cumulative nature of learning, we rewrite Equation (1) for $\varphi_t = 0$ as

$$S_{it} = S_{i0} B_j + \sum_{j=1}^{t} \alpha_j B_{j+1} + \sum_{j=1}^{t} \gamma_j I_{ij} B_{j+1} + \sum_{j=1}^{t} \eta_{ij} B_{j+1} \tag{2}$$

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

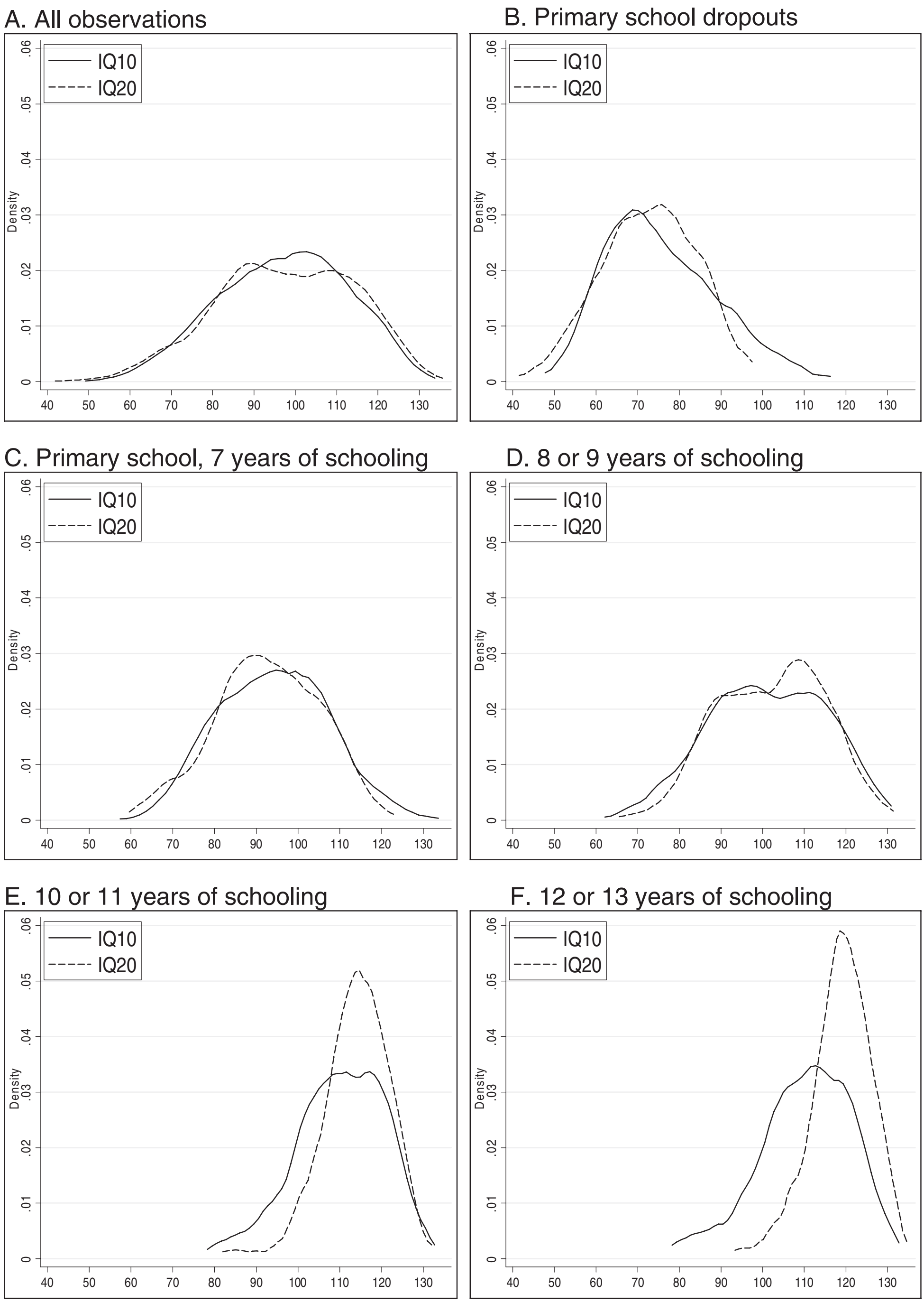


TABLE 3
Ability and Family Income by Educational Attainment

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

Notes: 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.

where S_{i0} is the initial skill,

$$(3) \qquad B_j = \prod_{j=1}^{t} \beta_j \quad \text{and}$$

$$B_{j+1} = \begin{cases} \prod_{k=j+1}^{t} \beta_k & \text{if } j \leq t-1 \\ 1 & \text{if } j = t \end{cases}$$

When the investment I_{it} is schooling, it can only take the values zero and unity and as such is an indicator variable. The number of years of education is given by $\text{SCHOOLING}_i = \sum_{j=1}^{t} I_{ij}$. A linear effect of SCHOOLING on S requires that $\gamma_j B_{j+1},\ j \in [1, t]$, is constant. This implies that $(\gamma_{j+1}/\gamma_j) = \beta_{j+1}$. The effect of investment at a given point in time has to be decreasing $(\gamma_{j+1} < \gamma_j)$ because $\beta_{j+1} < 1$. 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 Equation (2) 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 $B_j = 1$ 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.

Assuming a linear effect of SCHOOLING, implementing the model on IQ tests at age 10 and 20, and allowing for other covariates X, Equation (2) can be written

$$(4) \qquad \text{IQ}_{i20} = \alpha + \beta \text{IQ}_{i10} + \gamma \text{SCHOOLING}_i + \phi X_i + \varepsilon_{i20}$$

This model formulation in effect takes 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.

Winship and Korenman (1997, 1999) follow a similar approach. They utilize that for a subsample of the NLSY79, including about 10% of the total number of individuals in the data, there exists information on early tests in addition to the Armed Forces Qualifying Test (AFQT). Winship and Korenman do not discuss how representative the subsample is. In addition, the measure of early cognitive skills is from different tests across individuals that are conducted at different ages. Our data include information for the same tests for each individual conducted at the same age. This is a general intelligence test in contrast to the qualification test in NLSY. We will condition on three different measures of early cognitive ability, including the IQ test that is similarly constructed as our dependent variable. In addition, we will present results from an instrument variable approach that in principle will handle unobservable variables in the schooling choice decisions, and we will investigate whether the interaction effect in Equation (1) is important.

Measurement error in relation to the true selection variable will bias the OLS estimate. There are two reasons why we do not think this is a serious problem in the present analysis. First, we use similar IQ tests 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 II, which makes it feasible to condition on other dimensions of ability than general intelligence.

We will, however, investigate the robustness of the OLS result using various approaches. First, we investigate whether the estimated return to education is sensitive to the inclusion of various control variables in the model. If it is not, this indicates that there is not a major problem related to unobservable variables. Second, we do a falsification test, and finally we pursue an IV approach. 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; Blackburn and Neumark 1993; Griliches 1977; 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 major problem in our study because we condition on early cognitive ability. Second, most studies, at least U.S. studies, are based on surveys, and self-reported educational attainment is vulnerable to measurement error.[6] The discussion in the previous section indicates that we cannot rule out that some measurement error exists in the education attainment variable. In addition, expectations about gains in cognitive ability may affect the education decision. Children who believe they will gain most in terms of cognitive skills by continued schooling might be most likely to have a high level of schooling, making the education variable endogenous. Moreover, it may be the case that people who invest the most in schooling also invest in cognitive ability in other ways. Thus, OLS estimates may be biased even though we have unusually relevant variables in the model.

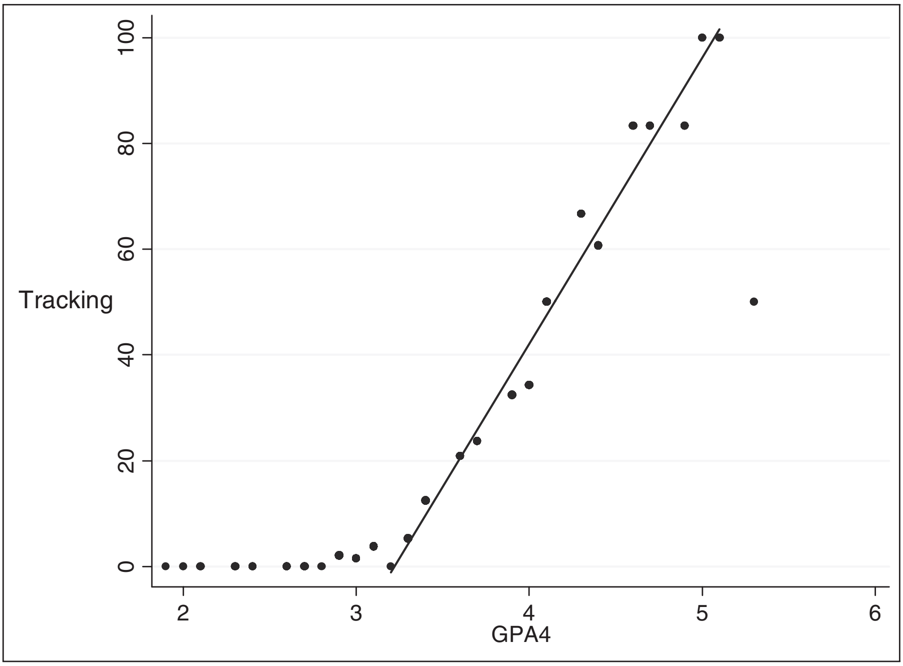
6. In a review of the literature, Card (1999) considers both these possibilities for biased estimates and concludes that the causal effect of education on earnings "is not much below the estimate that emerges form a simple cross-sectional regression of earnings on education" (p. 1855).

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 present extensive and generous student loan system had not yet been developed. Thus, education required that the family was able and willing 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 increases in the individual's educational attainment.

Teacher grading in fourth grade was important for the choice of education above the primary level. This choice had a major impact on educational attainment. Average years of schooling for individuals who continued primary education is 7.2, whereas for those enrolling in lower secondary education it is 10.7 years. Figure 3 shows the relationship between the share of students enrolling in lower secondary schools and GPA in fourth grade. The share of students enrolling in lower secondary education increases almost linearly in GPA in the range 3–5. There is no sharp formulaical relationship between GPA and enrollment in lower secondary education. Students with relatively low GPA who enrolled in lower secondary schools tend to have rich and well-educated fathers, whereas students with high GPA who did not enroll in lower secondary education have about average values of the family characteristics.

We will exploit these institutional characteristics in the empirical analyses. First, we will simply use the tracking as an instrument for years of schooling. Then, we identify the return to education on the situation at age 11, and any

FIGURE 3
The Share of the Students Enrolling Lower Secondary Education after the Fourth Grade Related to GPA in Fourth Grade



new information from age 11 on influencing schooling decisions will not confound the estimates. Secondly, in a more indirect way, we hypothesize that individuals with poor marks in third grade who had motivation for higher education, either intrinsically or by pressure from their parents, would 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 used as another instrument for educational attainment.

The correlation coefficients between the three instruments are in the range 0.1–0.4.[7] Thus, they will identify the effect of schooling on different types of variation when used separately. The exclusion restriction regarding family income is that the only way income can improve intelligence between age 10 and 20 is by paying the costs of formal schooling. For the tracking variable, one restriction is that it is years in school that affect intelligence, not the type of school. Regarding the last instrument, the assumption is that the change in GPA from third to fourth grade includes no information of potential improvements in intelligence, but improves the scope for more schooling. Notice that the growth in GPA is slightly negatively correlated with GPA in third grade. Variation in the estimated return to education across models using different instruments might indicate that at least some models are misspecified or that the return to education is heterogeneous. Similar effect in different model specifications, however, is in line with the case of a homogeneous average effect.

A concern for dynamic models like Equation (4) is that the error term is serially correlated. Then OLS estimates of β are inconsistent. In addition, Todd and Wolpin (2003) show that value-added specifications are vulnerable to missing variables. The common approach to solve the problem in the literature on dynamic panel data models is to use earlier observations

7. The correlation coefficients for family income are 0.39 and 0.09 with regard to tracking into lower secondary education and growth in GPA from third to fourth grade, respectively. The correlation coefficient between the latter two variables is 0.20.

TABLE 4
The Effect of Educational Attainment on Ability, Dependent Variable Is IQ20

	(1)	(2)	(3)	(4)	(5)	(6)
IQ10	0.73 (0.03)	0.54 (0.03)	0.53 (0.03)	—	0.53 (0.03)	0.42 (0.04)
Born early	−9.41 (1.47)	−6.71 (1.28)	—	−13.9 (1.54)	−4.09 (1.45)	−4.40 (1.56)
SCHOOLING	—	3.47 (0.23)	3.47 (0.25)	5.41 (0.26)	3.24 (0.25)	2.87 (0.29)
Father has higher education	—	—	—	—	3.37 (1.18)	3.21 (1.24)
Month of birth	—	—	—	—	0.27 (0.11)	0.36 (0.11)
GPA third grade	—	—	—	—	—	2.70 (1.16)
Teacher rating third grade	—	—	—	—	—	0.89 (0.58)
Fixed effects for class in primary school	No	No	No	No	Yes	Yes
R^2	0.588	0.696	0.610	0.519	0.730	0.731
Sample	All	All	Born in 1928 and tested in 1948	All	All	All
Observations	652	650	549	650	629	566

Note: Standard errors in parentheses.

of the dependent variable as instruments. 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 as Husén (1950) argues strongly that the same types of ability are evaluated in both IQ tests. De facto we use teacher evaluations from the same time period instead of lagged comparable test results as instruments, utilizing that the tests are different but correlated with the IQ test. The overidentification restrictions will be tested by the standard Sargan test.[8]

IV. EMPIRICAL RESULTS

A. Ordinary Least Squares

Table 4 presents our basic model Equation (4) above, estimated using OLS. The model in column (1) includes only early cognitive ability and a dummy variable for overaged students 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 tests scores as dependent variables—is not in accordance with the data generating process.[9]

Column (2) in Table 4 includes educational attainment. The effect is equal to 3.5, about 20% of a standard deviation. The model therefore predicts that an education of 12 years, compared with only primary school of 7 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 conducting the IQ test in 1947 in the sample.[10] 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% of the variation in IQ20, a model including only SCHOOLING explains 40% of the variation, whereas the parsimonious model explains 61%. Early cognitive ability therefore seems to be slightly

8. One critical assumption for the Sargan test is that at least one of the instruments has to be valid, which is a nontestable assumption. Another feature of the Sargan test is that the power decreases in the interdependence of the instruments and the number of instruments. In the present case, we use few instruments and they have low correlation.

9. 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. The dummy variable is therefore not included in the models reported.

10. This is the case for all model formulations below.

more important than educational attainment. The same picture emerges when comparing the estimated coefficients. The effect of increasing IQ10 by one standard deviation is 7.4, whereas the effect of increasing SCHOOLING by one standard deviation is 6.2. Compared with Winship and Korenman (1997, 1999), our standardized estimate of educational attainment is slightly larger and our standardized estimate for early cognitive ability is slightly lower.

In column (5) in Table 4, we include father's education, month of birth, and fixed effects for class in school. The estimated effect of schooling is one standard error lower in the extended model. The effect of father's education is positive and significant at 5% level as expected. The conditional effect of month of birth is also positive. This is consistent with the findings in Bedard and Dhuey (2006) that the effect of relative age is stronger at the fourth grade level than at the eighth grade level in cross-section models.

In column (6), we include the two other ability measures from third grade, GPA, and teacher's rating of general cognitive ability. The effects of both variables are positive, which indicates that they capture some dimensions of ability relevant in the IQ test at age 20 but not captured by the IQ test at age 10. If there is measurement error in IQ10, one would expect similar effects because the ability variables are highly correlated. Only GPA is significant at 5% level. Inclusion of the additional ability measures further reduces the estimated effect of schooling. Compared with the simple model in column (2), the effect of educational attainment is reduced by 17% when all covariates are included.[11]

So far, it is assumed that the effect of education is linear. This can be tested using a dummy variable approach. Figure 4 presents the results based on the model specification Equation (2) in Table 4 using primary school attainment as the comparison group. The figure also presents 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% confidence interval is marginally below the regression line. The estimates of 9 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 9 years of education went through vocational education, whereas those with 10 years of education mainly had more academic forms of education.[12]

B. Robustness Checks

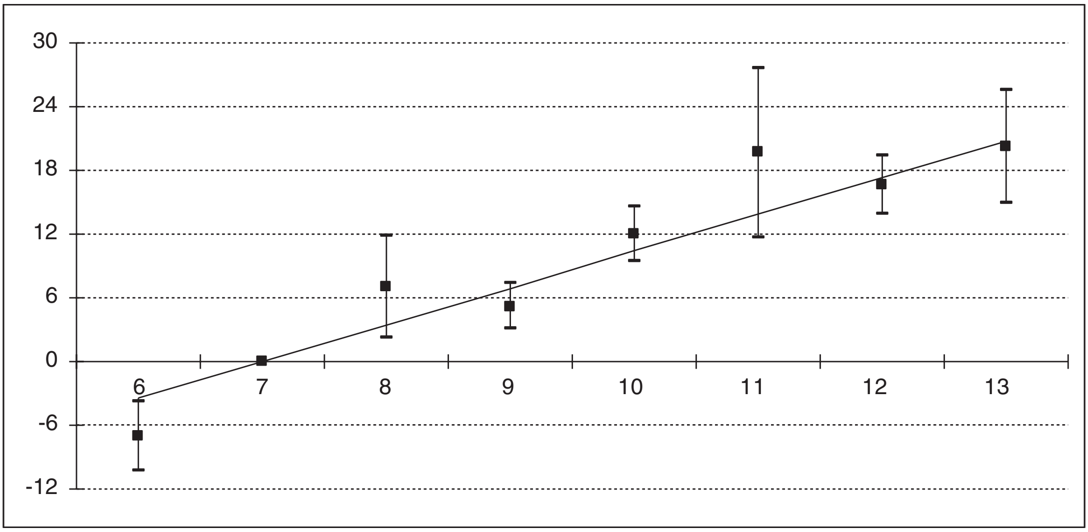
If we have identified a causal effect of education, education achieved after the IQ test at age 20 should have no effect. In principle, this hypothesis can be tested using information on the quantity of schooling later in life as a falsification test of the model specification. Unfortunately, higher education attainment reported later than the age of 20 may simply reflect measurement error. 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% level. 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

11. 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 3.54. The differences across models below are neither related to different samples.

12. 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.

FIGURE 4
Nonlinear Effect of SCHOOLING on Ability with 95% Confidence Interval



are self-selected into the highest educational attainment.

IV Estimation. Before turning to the IV estimations, we present some reduced form models in Table 5. The three first columns use as before IQ20 as the dependent variable, but exclude schooling from the model. The instruments we will use are included separately, and they have all highly significant effects.[13] When schooling is included in the models, however, the instruments become insignificant at 5% level in all three cases. Column (4) in Table 5 includes all instruments in addition to the schooling variable, in which case the effect of starting lower secondary education in the fifth year at school is marginally significant at 5% level. Nevertheless, the fact that the effects of the instruments almost abolish when schooling are included in the model indicates that they might be valid instruments.

The last part of Table 5 presents first-stage regressions. All instruments have a strong effect on schooling. The results in column (5) imply that increasing family income per family member by one standard deviation raises schooling by about 0.5 years. Credit constraints seem to have played a major role in the empirical period. The effect of starting lower secondary education is particularly strong, with a t-value of almost 30, and the results imply that lower secondary education on average implies three more years of schooling. Interestingly, the effect of IQ at age 10 is low in these regressions and significant at 5% level in only one case. GPA is more important for schooling than the IQ.

Table 6 presents results from two-stage least squares models. First, we only regard schooling as endogenous and continue assuming that IQ10 is exogenous. Columns (1)–(3) in Table 6 present the models corresponding to the first-stage regressions in Table 5 including the instruments separately. In all cases, the effect of schooling is highly significant, and 15%–33% larger than in the comparable OLS specification in column (6) in Table 4. Despite the fact that the correlation coefficients between the instruments are relatively low, the estimated effect of schooling is robust.

In order to give some indication on the validity of the instruments, column (4) in Table 6 includes all three instruments and reports

13. The variable family income per family member used in column (1) in Table 5 includes information on 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, both statistically significant at 5% level. The effect of the number of parents is close to zero.

TABLE 5
Reduced Form Regressions of IQ20 and Educational Attainment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	IQ20				SCHOOLING			
IQ10	0.45 (0.04)	0.44 (0.04)	0.43 (0.04)	0.41 (0.04)	0.014 (0.006)	0.010 (0.006)	0.007 (0.004)	0.005 (0.004)
Born early	−5.67 (1.78)	−5.15 (1.69)	−6.09 (1.58)	−4.89 (1.67)	−0.41 (0.24)	−0.31 (0.23)	−0.58 (0.15)	−0.49 (0.16)
Father has higher education	3.95 (1.43)	5.77 (1.31)	4.39 (1.25)	2.26 (1.34)	0.57 (0.20)	0.97 (0.18)	0.50 (0.12)	0.31 (0.13)
Month of birth	0.41 (0.13)	0.47 (0.12)	0.38 (0.12)	0.35 (0.12)	0.03 (0.02)	0.03 (0.02)	0.01 (0.01)	0.02 (0.01)
GPA third grade	4.60 (1.26)	6.75 (1.34)	2.65 (1.19)	2.78 (1.35)	0.67 (0.17)	1.24 (0.18)	0.07 (0.11)	0.26 (0.13)
Teacher rating third grade	1.77 (0.64)	1.19 (0.65)	1.08 (0.59)	0.83 (0.62)	0.28 (0.09)	0.16 (0.09)	0.10 (0.06)	0.04 (0.06)
Family income per family member/1,000	2.61 (0.70)	—	—	0.62 (0.68)	0.70 (0.10)	—	—	0.26 (0.07)
Change in GPA from third to fourth grade	—	4.47 (1.16)	—	0.90 (1.18)	—	1.16 (0.16)	—	0.26 (0.11)
Lower secondary education track	—	—	10.11 (1.16)	3.79 (1.85)	—	—	3.05 (0.11)	2.88 (0.12)
SCHOOLING	—	—	—	2.02 (0.47)	—	—	—	—
Fixed effects for class in primary school	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test for instruments	—	—	—	—	52.3	56.0	749.4	256.6
R^2	0.690	0.688	0.719	0.739	0.518	0.522	0.783	0.795
Observations	531	560	567	524	530	559	566	524

Note: Standard errors in parentheses.

TABLE 6
IV Estimates of the Effect of Educational Attainment, Dependent Variable Is IQ20

	(1)	(2)	(3)	(4)	(4)	(6)	(7)
IQ10	0.40 (0.04)	0.40 (0.04)	0.41 (0.04)	0.41 (0.04)	0.50 (0.03)	0.64 (0.07)	0.65 (0.11)
Born early	−4.09 (1.72)	−3.97 (1.64)	−4.18 (1.57)	−4.29 (1.66)	−4.04 (1.57)	−2.92 (1.76)	−2.78 (1.64)
SCHOOLING	3.80 (0.94)	3.82 (0.94)	3.31 (0.37)	3.48 (0.37)	3.83 (0.33)	3.25 (0.42)	3.33 (0.99)
Father has higher education	1.75 (1.61)	2.04 (1.59)	2.74 (1.27)	1.96 (1.31)	2.30 (1.26)	1.94 (1.34)	2.40 (1.56)
Month of birth	0.30 (0.12)	0.33 (0.12)	0.34 (0.11)	0.32 (0.12)	0.24 (0.11)	0.29 (0.12)	0.26 (0.12)
GPA third grade	1.96 (1.35)	1.96 (1.33)	2.40 (1.17)	2.08 (1.21)	—	—	—
Teacher rating third grade	0.71 (0.67)	0.60 (0.66)	0.75 (0.59)	0.83 (0.61)	—	—	—
Dummy variables for class in primary school	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Endogenous variables	SCHOOLING	SCHOOLING	SCHOOLING	SCHOOLING	SCHOOLING	SCHOOLING, IQ10	SCHOOLING, IQ10
Instruments	Family income	Change in GPA	Tracking	All in previous columns	All in previous columns	All	Family income, GPA third grade
Number of instruments	1	1	1	3	3	5	2
Sargan test, p-value	—	—	—	0.82	0.95	0.98	—
R^2	0.731	0.725	0.729	0.734	0.728	0.717	0.720
Sample	All	All	All	All	All	All	All
Observations	530	559	566	524	571	524	577

Note: Standard errors in parentheses.

the Sargan test statistic for overidentifying restrictions. The test statistic is in fact close to zero, indicating that the instruments are valid.[14]

The effects of both GPA and teacher rating in third grade become smaller in the IV models and are typically insignificant at 5% level. Column (5) in Table 6 excludes the alternative ability variables from the model, which increases the return to education with one standard error as in the OLS model.

The results so far indicate that there might be some bias in the OLS results. However, there is a danger of overestimating the effect of 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 for models treating both IQ10 and schooling as endogenous are presented in Table A1. They show that GPA is highly correlated with IQ10. In addition, the effect of family income is significant at 5% level, perhaps because cognitive ability of parents is positively correlated with their income as found in several studies; see, for example, Altonji and Pierret (2001) and Falch and Sandgren (2008). The results for the model of interest are presented in column (6) in Table 6. The effect of early cognitive ability increases slightly and the effect of schooling decreases slightly compared with the previous models, as expected. The *p*-value of the Sargan test for overidentifying restrictions still clearly indicates that the model is not misspecified.

In the model reported, 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 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. One example for a model with a narrow instrument set is presented in the final column in Table 6. Neither do the results seem sensitive to other combinations of the instruments.[15]

Nonlinear Effects. Heckman (2000) and Cunha et al. (2006) 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, $\varphi > 0$ in Equation (1). The hypothesis can be tested in our framework by including an interaction term between ability and SCHOOLING. This is not a direct test of investment in children's skill at very early ages that Heckman (2000) and Cunha et al. (2006) argue have high returns. However, it is a test on the underlying mechanism why early investment has high returns in their model.

OLS results for all three ability measures we have utilized are presented in columns (1)–(3) in Table 7. All three interaction terms in fact turn out to be negative, in contrast to the complementarity hypothesis. At face value, the model in column (1) in Table 7 implies that the effect of SCHOOLING is equal to 3.8 and 2.2 for IQ10 of 70 and 130, respectively. Notice, however, that the interaction terms are insignificant at 10% level.

The negative interaction effects may be a result of other omitted nonlinearities. For example, the return to early cognitive ability might be declining. Column (4) in Table 6 includes a squared term of IQ10, and the effect is negative but insignificant at 10% level. In the model in column (5), both the interaction effect and the squared term are included without affecting the estimates qualitatively, but the results indicate that the present data seem to include too little information in order to estimate nonlinear effects.

14. The results for the model in column (4) in Table 5 may indicate that the tracking variable is questionable as an instrument. Excluding this variable from the instrument set, the *p*-value of the Sargan test statistic is 0.97. One may perhaps argue that in particular the change in GPA from third to fourth grade includes information on expected gain in IQ from age 10 to age 20. The variable may capture elements on possible educational signaling behavior. Excluding this variable from the instrument set, the *p*-value of the test statistic is 0.69. For the last combination of instruments, excluding family income, the *p*-value of the Sargan test statistic is 0.53. A related issue is whether father's education is a valid instrument as father's education has a strong effect on education and a weak effect on IQ20 (see Table 5). Extending the instrument set in column (4) in Table 6 with this variable, the effect of schooling increases to 3.65, and the *p*-value of the Sargan test is equal to 0.42.

15. For example, using three of the five variables in the instrument set, the return to education varies from 2.6 to 3.5 and is significant at 5% level in nine out of the ten cases. The *p*-value of the Sargan test is always above 0.6.

TABLE 7
Nonlinear Effects of Educational Attainment, Dependent Variable Is IQ20

	(1)	(2)	(3)	(4)	(5)
IQ10	0.41 (0.04)	0.42 (0.04)	0.42 (0.04)	0.41 (0.04)	0.41 (0.04)
Born early	−3.77 (1.61)	−4.02 (1.59)	−3.77 (1.60)	−3.89 (1.59)	−3.67 (1.61)
SCHOOLING	3.10 (0.32)	3.09 (0.33)	3.18 (0.34)	2.91 (0.29)	3.04 (0.33)
Father has higher education	3.22 (1.24)	3.14 (1.24)	3.07 (1.24)	3.15 (1.24)	3.18 (1.24)
Month of birth	0.36 (0.11)	0.36 (0.11)	0.34 (0.11)	0.35 (0.11)	0.36 (0.11)
GPA third grade	2.84 (1.16)	2.86 (1.17)	2.77 (1.16)	2.79 (1.16)	2.85 (1.16)
Teacher rating third grade	0.91 (0.58)	0.83 (0.58)	0.84 (0.58)	1.00 (0.59)	0.97 (0.59)
IQ10 × SCHOOLING, centered	−0.028 (0.018)	—	—	—	−0.018 (0.021)
(GPA third grade) × SCHOOLING, centered	—	−0.50 (0.41)	—	—	—
(Teacher rating third grade) × SCHOOLING, centered	—	—	−0.39 (0.22)	—	—
IQ10 × IQ10/100, centered	—	—	—	−0.25 (0.16)	−0.16 (0.19)
Fixed effects for class in primary school	Yes	Yes	Yes	Yes	Yes
R^2	0.732	0.731	0.732	0.732	0.732
Sample	All	All	All	All	All
Observations	566	566	566	566	566

Notes: Estimated by OLS. Standard errors in parentheses.

V. CONCLUSIONS

This paper clearly indicates that ability as measured by commonly used IQ tests is positively affected by education. The point estimate is robust to various model formulations. Based on OLS where we condition on early cognitive ability to take selection into noncompulsory schooling into account, we estimate the return to 1 year of schooling to be 2.9–3.5 IQ points on average. This estimate is biased if there is measurement error in educational attainment or selection based on unobservable factors. Using IV the return to schooling is estimated to be 3.3–3.8 IQ points if schooling is treated as endogenous and about 3.3 if both schooling and early cognitive ability are treated as endogenous. Overall, the results imply that four to five additional years of schooling on average increases 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, Heckman, and Mullen (2004), who all use achievement on a qualification test. We do not find any support of the complementary hypothesis where cognitive ability raises the productivity of subsequent investment in skills.

The evidence that schooling affects general intelligence, such as thinking skills 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.

APPENDIX

The IQ Tests

The IQ test conducted in 1938 was designed by PhD student Siver Hallgren with the purpose to explore the relationship between social background and cognitive ability. To enable this, two tasks had to be performed on the same sample: collection of social data and testing. Hallgren's argument for examining a full cohort of students was explicitly to avoid the selection problem. Another important issue was to choose a suitable age for the study. He argued that tests of cognitive ability are less reliable the younger the children are, but on the other hand he needed to do the test before the age of school tracking. After fourth grade the main tracking was done in the Swedish school system at the time, the choice between primary school and lower secondary school. However, private schools enrolled students from third grade,

TABLE A1

First-Stage Regressions, Dependent Variables Are SCHOOLING and IQ10

	(1)	(2)	(3)	(4)
Dependent variable	SCHOOLING	IQ10	SCHOOLING	IQ10
Born early	−0.52 (0.16)	−5.59 (1.84)	−0.63 (0.23)	−6.94 (1.82)
Father has higher education	0.31 (0.13)	0.25 (1.50)	0.53 (0.19)	−0.15 (1.51)
Month of birth	0.02 (0.01)	0.18 (0.13)	0.02 (0.02)	0.09 (0.13)
Family income per family member/1,000	0.26 (0.07)	0.54 (0.75)	0.75 (0.09)	1.19 (0.74)
Change in GPA from third to fourth grade	0.28 (0.11)	3.94 (1.31)	—	—
Lower secondary education track	2.89 (0.12)	1.09 (1.42)	—	—
GPA third grade	0.31 (0.12)	10.0 (1.44)	1.30 (0.11)	15.0 (0.86)
Teacher rating third grade	0.06 (0.06)	3.49 (0.68)	—	—
Fixed effects for class in primary school	Yes	Yes	Yes	Yes
F-test for instruments (df)	247 (5, 478)	77.1 (5, 478)	126 (2, 530)	164 (2, 530)
R^2	0.794	0.621	0.504	0.563
Observations	524	524	577	577
Second step regression in Table 6 column	(6)	(6)	(7)	(7)

Note: Standard errors in parentheses.

and to minimize the impact of this potential segregation Hallgren chose to test the full cohort of third graders in the city of Malmö, as early as possible in third grade.

The 1938 test is closely described in Hallgren (1939) and also by Husén (1950, chapter 1). Individual testing was practically impossible as it would have taken much too long. The students would then be tested after different amounts of schooling, and the test would have time to become known among the students. Thus, a group-test-scale had to be used. A group-test scale suitable for Sweden for the relevant age group did not exist, and Hallgren therefore had to construct and standardize a new scale. A thorough work was done in this regard as documented in Hallgren (1939), including a careful reading of the literature available and testing the scale on 860 ten-year-old children the year before. The tests were then performed class by class during 2 weeks in February 1938 by two test examiners. Great care was taken to make the instructions easily understood, and two parallel tests were used to avoid cheating between neighbors in the classroom. The tests were always taken before lunch, as afternoon tests tend to give worse results.

The test consisted of four parts: opposites, missing words, identical figures, and disarranged sentences. No mathematical part was used as, among psychologists, it was not considered suitable for such young children. Mathematical tests have been proven to correlate bad with other tests of intelligence, and also with the teachers' general approximation of the children's intelligence. Furthermore, it has repeatedly shown to have a very low diagnostic power (Hallgren 1939, 13 and 17). Hallgren's ambition was clearly to construct a general intelligence test, standardizing all results to the IQ scale.

The 1948 test at military enrollment was constructed by Torsten Husén, who had been the opponent on Hallgren's (1939) thesis. He made strong efforts to make the two tests as comparable as possible as described in Husén (1950), but they had to differ to some extent as the test takers were 10 years older. The 1948 test also consisted of four parts: synonyms, concept discrimination, number series, and Raven's matrices. Husén carefully collected the test results for the individuals in the Malmö sample, even though not all of them enrolled in 1948. Still, some men are missing in the data, but he concluded that this did not alter the representativity of the sample (Husén 1950, 46). This is also evident from Table 1.

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