The Effect of Schooling on Cognitive Skills

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Abstract: How schooling affects cognitive skills is a fundamental question for studies of human capital and labor markets. While scores on cognitive ability tests are positively associated with schooling, it has proven difficult to ascertain whether this relationship is causal. Moreover, the effect of schooling is difficult to separate from the confounding factors of age at test date, relative age within a classroom, season of birth, and cohort effects. In this paper, we use a fundamentally different identification approach compared to the previous literature. We exploit conditionally random variation in the assigned test date for a battery of cognitive tests which almost all 18 year-old males were required to take in preparation for military service in Sweden. Both age at test date and number of days spent in school vary randomly across individuals after flexibly controlling for date of birth, parish, and expected graduation date (the three variables the military conditioned on when assigning test date). We find an extra 10 days of school instruction raises cognitive scores on crystallized intelligence tests (synonym and technical comprehension tests) by approximately one percent of a standard deviation, whereas extra nonschool days have almost no effect. The benefit of additional school days is homogeneous, with similar effect sizes based on past grades in school, parental education, and father's earnings. In contrast, test scores on fluid intelligence tests (spatial and logic tests) do not increase with additional days of schooling, but do increase modestly with age. We discuss the importance of these findings for questions about the malleability of cognitive skills in young adults, schooling models of signaling versus human capital, the interpretation of test scores in wage regressions, and policies related to the length of the school year.

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1 Introduction

How schooling affects cognitive skill formation is an important question for studies of human capital. Cognitive skills, as measured by standard intelligence tests, are associated with relatively large returns in the labor market; a one standard deviation increase in cognitive test scores is associated with an average 7 percent increase in wages across 24 different studies (Bowles, Gintis, and Osborne, 2001). A sizable literature also suggests that cognitive ability plays a role in labor markets more broadly, including studies of employment, discrimination, wage inequality, and changes in the college wage premium.[1]

While scores on cognitive ability tests are positively associated with schooling, it has proven difficult to ascertain whether this relationship is causal. Schooling could affect cognitive ability, but it is equally plausible that cognitive ability affects schooling. Moreover, the effect of schooling is difficult to separate from the confounding factors of age at test date, relative age within a classroom, season of birth, and cohort effects.

A growing literature attempts to estimate the link between education and cognitive skills using school entrance cutoff dates as an instrumental variable.[2] The idea in these studies is that cutoff dates generate arguably exogenous variation in years of completed schooling for individuals of the same age. This approach solves some of the problems mentioned above, but also requires additional assumptions. Whether or not these assumptions are likely to hold has been discussed in several recent papers.[3] Another challenge for existing studies is that often age at the time of the cognitive test, school start date (and hence cumulative schooling), and birthdate are perfectly collinear, making it impossible to separately estimate the three effects. Moreover, studies based on a common test-taking date are not nonparametrically identified, but must impose some structure on how birthdate affects cognitive skills. This turns out to be empirically important, since as we verify in our dataset, birthdate is not randomly assigned.[4]

[1] For a sampling of papers, see Altonji and Pierret (2001), Bishop (1991), Blackburn and Neumark (1993), Blau and Kahn (2005), Cawley, Heckman, and Vytlacil (2001), Heckman, Stixrud, and Urzua (2006), Murnane, Willett, and Levy (1995), Neal and Johnson (1996), Taber (2001), and Lindqvist and Vestman (2011).

[2] See Bedard and Dhuey (2006, 2007), Black, Devereux, and Salvanes (2011), Cascio and Lewis (2006), Cascio and Schanzenbach (2007), Crawford, Dearden, and Meghir (2010), Datar (2006), Fertig and Kluve (2005), Fredriksson and Ockert (2005), Leuven et al. (2004), McEwan and Shapiro (2008), and Puhani and Weber (2005)

[3] See Bedard and Dhuey (2006), Cascio and Schanzenbach (2007), Dobkin and Ferreira (2007), and Elder and Lubotsky (2009).

[4] Research documenting the nonrandomness of birthdate in other settings includes Buckles and Hungerman (forthcoming), Bound and Jaeger (2000), Dobkin and Ferreira (2010), and Cascio and

In this paper, we use a fundamentally different identification approach. We exploit conditionally random variation in the assigned test date for a battery of cognitive tests which almost all 18 year-old males were required to take in preparation for military service in Sweden. Both age at test date and number of days spent in school vary randomly across individuals after flexibly controlling for date of birth, parish, and expected graduation date (the three variables used by the military to assign test date). This approach gives us a quasi-experimental setting to estimate the effect of schooling and age on cognitive test scores, without the need for either an instrument or assumptions about birthdate.

Our approach is also different in that we look at additional days of schooling within a grade level rather than different years of schooling across grade levels. We study individuals currently enrolled in the 12th grade to facilitate variation in days of schooling around an individual's 18th birthday. This means we focus on young men in the academic high school track, which requires 12 or 13 years of schooling, as opposed to the vocational track, which requires only 11 years. The quasi-random timing of enlistment generates substantial variation in both age and number of school days as of the test date; in our data, the standard deviation in age and school days are 108 days and 51 days, respectively. Because school days are unevenly distributed over the year, there is separate variation in both age and school days.

As long as an individual's test date is conditionally random, both age and number of school days will vary randomly across individuals after conditioning on birthdate, parish, and expected graduation. As a test of conditional randomness, we document that both age and number of school days are unrelated to family background characteristics and prior performance in school after flexibly controlling for the conditioning variables. We also show why failure to control for the conditioning variables could lead to biased estimates, as birthdate in particular is correlated with a variety of outcomes which are predictive of cognitive test scores.

Our first finding is that cognitive skills are still malleable when individuals are approximately 18 years old. This is true both for crystallized intelligence tests (synonyms and technical comprehension tests) and fluid intelligence tests (spatial and logic tests), two categories of tests commonly used by psychologists which we describe in more detail below.[5] This finding is important as the cognitive tests we analyze are similar to those used by the U.S. military, some potential employers,and college entrance exams. Our results suggest that even as late as age 18, these types of cognitive skills are not fully determined, and therefore cannot easily be compared

Lewis (2006).

[5]The commonly used Wechsler Adult Intelligence Scale (WAIS-III) has both a fluid intelligence portion (named performance IQ) and a crystallized intelligence portion (named verbal IQ).

across individuals who are different ages when they take the tests.

Our main set of results concerns the effect of extra days spent in school. We find 10 more days of school instruction raises cognitive scores by 1.1 percent of a standard deviation on the synonyms test and 0.8 percent of a standard deviation on the technical comprehension test. Extra nonschool days have virtually no effect on these two crystallized intelligence tests. To put the estimates in perspective, they imply an additional year of schooling (180 days) results in crystallized test scores which are roughly one-fifth of a standard deviation higher. In contrast, test scores on the fluid intelligence tests (spatial and logic tests) do not increase with additional days of schooling, but do increase modestly with age.

Our results are robust to a variety of alternative specifications, including different functional forms for the conditioning variables. However, if one were to erroneously exclude the conditioning variables for birthdate, parish, and expected graduation, the estimates change markedly. The biggest differences show up for the crystallized intelligence tests where the coefficient on school days falls by half for the synonyms test and to almost zero for the technical comprehension test.

From a policy perspective, one of the more interesting questions is whether the cognitive returns to extra schooling are heterogeneous. If either low or high ability individuals experience larger cognitive returns to schooling, then extra resources spent on an appropriately-targeted group of students could have a high individual and social return. Our last result is that the benefit of additional school days is homogeneous for a variety of pre-determined characteristics which are strongly correlated with cognitive ability. We find similar effect sizes based on past grades in school, parental education, and father's earnings. This suggests that extra schooling can benefit students from a variety of backgrounds.

Taken together, our findings have several important implications. They provide insight into questions about the malleability of cognitive skills, schooling models of signaling versus human capital, the interpretation of test scores in wage regressions, and policies related to the length of the school year.

The remainder of the paper proceeds as follows. Sections 2 and 3 discuss previous research, the difficulties in estimating a cognitive production function, and our identification strategy. In Section 4, we describe our setting and the Swedish data which make this study possible. Section 5 tests for the conditional randomness of test dates, while Section 6 presents our main results, robustness checks, and heterogeneity results. Section 7 discusses the importance of our findings and the final section concludes.

2 Previous Research

2.1 Literature Review

Researchers have long been interested in understanding the relationship between schooling, age, and cognitive ability. Ceci (1991) provides a detailed survey of 200 studies across several disciplines that investigate the relationship between schooling and the cognitive components of general intelligence. Most of the designs he reviews are observational in nature – for example, correlations between cognitive tests and completed years of schooling – and therefore likely to suffer from severe selection bias.[6] The worry in these studies is that students who have more schooling might also have higher cognitive ability for unobserved reasons.

Given the challenges inherent in observational studies, researchers have looked for sources of exogenous variation in schooling. Research in psychology, starting with Baltes and Reinert (1969) and continuing with Cahan and Davis (1987, 1989) to the present, has used a "between-grade level" discontinuity design. This approach uses the fact that admission to elementary school is determined by date of birth relative to a cutoff date. For example, in some states in the U.S., a child is eligible to start kindergarten in a given year as long as she is age 5 by September 1. These cutoff dates cause some students to be older than others when they start school, with at least some of the age gap persisting into higher grade levels. Using a regression discontinuity (RD) design, these studies compare the cognitive test scores of students born immediately before versus immediately after the cutoff date, since these students will have similar ages but different amounts of schooling. One problem with this approach is imperfect compliance, as some parents delay or accelerate school entry in violation of the enrollment cutoffs. If noncompliance is correlated with unobserved factors, this will bias the estimates. Most studies in psychology deal with this issue by excluding non-compliers; however, this results in a non-random sample which could also create a bias.

Recent work in economics recognizes the problem of noncompliance and uses assigned school start date (rather than actual start date) as an instrument in an RD framework. Research which takes this approach includes Bedard and Dhuey (2006, 2007), Black, Devereux, and Salvanes (2011), Cascio and Lewis (2006), Cascio and Schanzenbach (2007), Crawford, Dearden, and Meghir (2010), Datar (2006), Fertig

[6] Other observational designs Ceci reviews include the effect of school absences, delayed school entry, and dropping out of school on cognitive scores. Researchers have also examined historical trends in the schooling-IQ link. These observational studies generally find a positive relationship between schooling and the development of cognitive abilities. Many of the studies Ceci reviews are based on small samples (tens or hundreds of observations).

and Kluve (2005), Fredriksson and Ockert (2005), Leuven et al. (2004), McEwan and Shapiro (2008), and Puhani and Weber (2005).[7] These papers generally find a sizable link between schooling and cognitive ability. For example, Bedard and Dhuey's estimates for 19 different countries imply that students who are 11 months younger, and therefore have less schooling than their peers, score 4-12 percentile lower on standardized tests in fourth grade and 2-9 percentile lower in eighth grade.

There is also a related literature which uses structural modeling to estimate the production function for cognitive and noncognitive skills (e.g., Cunha and Heckman, 2008; Cunha, Heckman, and Schennach, 2010; Hansen, Heckman, and Mullen, 2004).

2.2 Challenges for Existing Studies

While the previous literature makes important contributions to our understanding of the relationship between schooling and cognitive skills, it also faces several potential issues. First, school entry laws may not be valid instruments for educational attainment. As Bedard and Dhuey (2006) point out, relatively older children in the same classroom may be treated differently (e.g., be placed in more advanced programs) or experience fewer social problems; Cascio and Schanzenbach (2007) provide evidence that relative age harms disadvantaged children the most. Elder and Lubotsky (2009) document several pathways through which school entry laws could affect outcomes other than through educational attainment. They find that entrance start dates are correlated with school performance, grade repetition, and diagnoses of learning disabilities. Moreover, some studies have found that students starting school at a younger age are less likely to drop out of high school, at least in the U.S. where there are also age-based compulsory education laws (Angrist and Krueger 1991, Dobkin and Ferreira 2007).

Another challenge for many existing studies is that age at the time of the test, school start date (and hence cumulative schooling), and birthdate are perfectly collinear. The reason for this is the test date is usually the same for all individuals. By definition age at test equals test date minus birthdate, so if test date is fixed, independent variation in age can only be achieved through variation in birthdate. A similar relationship exists for cumulative schooling. As we explain in the next section, studies based on a common test-taking date are not nonparametrically identified, but must impose some structure on how birthdate affects cognitive skills.

Most papers do not separate out age from cumulative schooling; papers that do

[7] Prominent papers using school entry cutoffs in different settings include Angrist and Krueger (1991, 1992) and McCrary and Royer (2006). Sims (2008) looks at a law change in Wisconsin which prohibited schools from starting instruction before September 1.

are required to impose at least some restrictions on how birthdate affects cognitive skill formation. Black, Devereux, and Salvanes (2011) assume birthdate is random, at least after conditioning on family fixed effects. Cascio and Lewis (2006) and Crawford, Dearden, and Meghir (2010) use variation in cutoff dates across U.S. states and local areas in Britain, respectively, to separate out age from schooling. Their implicit assumption is that schooling cohort effects (which are a function of birthdate through cutoff dates) do not directly affect cognitive skills.[8]

As we empirically document later in our paper, a child's birthdate does not appear to be random and controlling flexibly for birthdate turns out to be important. Indeed, previous work has found that season of birth is associated with a variety of negative outcomes (see Buckles and Hungerman, forthcoming; Bound and Jaeger 2000; Dobkin and Ferreira 2010; and Cascio and Lewis 2006).[9] While there could be more than one reason for these associations, Buckles and Hungerman document that mothers of winter-born children in the U.S. are more likely to be teenagers, unmarried, and high school dropouts. A related pattern is also true for our Swedish data; as we document later, we find that higher socioeconomic women tend to have their children disproportionately in March and April (after the December 31 cutoff date for school entry in Sweden).

Our approach is fundamentally different from the literature which uses variation based on assigned school start dates. We take advantage of conditionally random variation in test dates. This setting gives us a quasi-experimental setting to estimate the effect of schooling and age on cognitive test scores, without the need for either an instrument or assumptions about birthdate. Our approach is also somewhat different since we look at additional days of schooling within a grade level rather than different years of schooling across grade levels. We explain our methodology in detail in the next section.

[8]In theory, an RD design does not need to make restrictions on season of birth effects, but some structure is often placed on these effects for precision, including modeling season of birth effects with quarter of birth or month dummies as in Cascio and Lewis (2006) or linearity as in Crawford, Dearden, and Meghir (2010). However, due to the very nature of cutoff dates, when they are used as an instrument, restrictions must be placed on schooling cohort effects.

[9]For example, Buckles and Hungerman report that season of birth is associated with schizophrenia, autism, menopausal severity, shyness, suicide, and life expectancy. Negative effects are usually correlated with births during the winter months.

3 Identifying Schooling's Effect on Cognitive Skills

3.1 Production Function for Cognitive Skills

Cognitive skill formation could depend on a variety of factors, including the current amount of schooling an individual has been exposed to and age. A general model for the production of cognitive skill, y_{it}, is given by:

$$y_{it} = f(S_{it}, A_{it}, X_{it}, B_i, P_i, G_i) \tag{1}$$

where for individual i taking a cognitive test on date t, S_{it} is days of schooling as of the test date, A_{it} is age on the test date, X_{it} is a vector of other (potentially) time-varying factors, B_i is birthdate, P_i is parish of residence (a small geographic area), and G_i is expected graduation (a dummy for whether a student plans on graduating the year they turn 18). Birthdate, parish, and expected graduation play an important role as conditioning variables in what follows, which is why we list them separately from other X_{it}'s. The formulation in equation (1) allows for the possibility that cognitive skills develop over time. It suggests that cognition could be malleable in response to general maturation with age as well as via formal instruction in schooling.

To allow for empirical estimation, we consider a production function which is additively separable in inputs and an error term e_{it}:

$$y_{it} = \alpha + \beta S_{it} + \gamma A_{it} + \delta X_{it} + g(B_i) + \sum_j \theta_j (P_i = j) + \pi G_i + e_{it} \tag{2}$$

where j indexes parishes. In the empirical work, we will consider various specifications for the function $g(\cdot)$ of birthdate.

The first concern for consistent estimation of β is reverse causation, as it is likely that completed schooling is a function of cognitive ability. This is problematic for datasets where individuals (or their parents) can choose or influence the amount of schooling to receive before taking the cognitive test.

The second challenge is that in many datasets schooling and age are perfectly collinear. Age at the time of the test equals cumulative school days plus cumulative nonschool days, so if all individuals take the test on the same date and start school on the same date, there is no independent variation in school days and nonschool days (and hence age) for individuals with the same birthdate. Another way of saying this is that since age equals test date minus birthdate, if test date is fixed, variation in age and schooling can only be achieved through variation in birthdate. This means that studies based on a common test-taking date (and a common school start date) are not nonparametrically identified, but must impose some structure on how birthdate affects cognitive skills.

A related set of problems arise because school days and age are both functions of birthdate in observational data.[10] As others have pointed out, and as we verify with our dataset, cognitive ability varies by date of birth (both via birth day and birth cohort effects). This means the omission of age controls will cause the estimates of β to be biased. More generally, any omitted variables related to birthdate and test date will also bias the coefficient on schooling.

3.2 Using Random Variation in Test Dates

The ideal experiment to estimate the effect of schooling on test scores would randomly vary days in school. While our setting does not directly manipulate the number of school days experimentally, it does provide (conditionally) random variation in the date individuals are assigned to take cognitive tests. This quasi-experimental setting allows for consistent estimation of the effect of schooling on test scores without the need for instruments or additional assumptions.

To begin, first consider the case where individuals are randomly assigned a test date. We will then discuss the additional issues that arise when test date is randomly assigned conditional on covariates. Remembering that age equals test date minus birthdate, random variation in test date provides random variation in age only after conditioning on birthdate. Likewise, recognizing that school days plus nonschool days equals age, school days are also only random after conditioning on birthdate. This discussion makes clear that random assignment of test date does not imply unconditionally random variation in either schooling or age at test date. But random assignment of test date, t, does imply random variation in schooling and age after conditioning on birthdate, so that schooling and age are independent of the error term in equation 2 conditional on birthdate:

$$\text{Random Assignment of } t \Rightarrow (S_{it}, A_{it})|B_i \perp e_{it}. \tag{3}$$

In our setting, the assignment of test date is random only after conditioning on covariates. As we explain in more detail in Section 4, in advance of military service, every enlistee took a battery of cognitive skill assessments. To facilitate this testing, the military was provided with information on an individual's name, date of birth, address (grouped by parish), and in some cases, expected graduation date. It used

[10] To see this, note that age = cumulative school days + cumulative nonschool days = test date - birthdate. Many studies do not distinguish between school days and age.

this limited information to assign a test date close to an individual's 18th birthday, taking into consideration transportation and other logistical issues (there are only 6 testing centers, each with limited capacity, throughout the country). They did not use any other information in assigning test dates besides birthdate, parish, and expected graduation.

This assignment process creates conditionally random variation in test taking dates, where the conditioning variables are now birthdate, parish, and expected graduation. Therefore, in our setting, schooling and age are independent of the error term after conditioning on date of birth, parish, and expected graduation:

$$\text{Random Assignment of } t|B_i, P_i, G_i \Rightarrow (S_{it}, A_{it})|B_i, P_i, G_i \perp e_{it}. \tag{4}$$

The assignment process provides a second reason for why birthdate must be flexibly accounted for. It also indicates that parish of residence and expected graduation must be conditioned on as well.

Since we have conditionally random assignment of test dates, we can separately identify cumulative school days from cumulative nonschool days (and hence age). This is true even for two individuals with the same birthdate, since variation in test dates implies differing amounts school and nonschool days. We have coded up the school year calendars in Sweden for our sample period to separate school days from nonschool days.

As equation 4 makes clear, since test dates are conditionally random, the only requirement for consistent estimation of equation 2 is that birthdate B_i, parish P_i, and expected graduation G_i are adequately accounted for. In our empirical implementation, we will control flexibility for these variables and explore robustness using alternative specifications. Due to the conditionally random assignment of test dates, it does not matter whether other pre-determined covariates X_{it} are included in the regression, an implication we test empirically. The reason to include other control variables is solely for efficiency gains in estimation.

While identification does not require any further assumptions, our formulation of the production function assumes the marginal effect of an additional day of school is the same, regardless of when a school day occurs during the year. The model also assumes the marginal effect of an additional nonschool day has a homogeneous effect. The first assumption means, for example, that a school day in September has the same effect as a school day in April. The second assumption means, for example, that a day of summer vacation has the same effect as a day during Christmas break.[11]

[11]When we test these assumptions empirically, we do not reject our specification, although it should be noted the tests have low power. With more data and identifying variation, each of these

4 Background and Data

Our empirical analysis is based on administrative register data obtained from the Swedish National Service Administration. These data contain information on every individual who enlisted in the military between 1980 and 1994. The reason for choosing this sample is the cognitive assessments administered by the military (our dependent variables) were based on the same battery of four tests during this time period. Our independent variables of interest, the number of school days and the number of nonschool days, are calculated from school calendars.

We have also merged in data from administrative records maintained by Statistics Sweden in order to obtain more detailed demographic and background information on the enlistees. In particular, we have administrative records on completed years of schooling as of 2003, parental education as of 1999, father's earnings in 1980, and for a subset of cohorts, information on exit exam grades in math and Swedish when graduating from 9th grade. These variables will be utilized to test for random assignment and to explore whether there are heterogeneous returns to schooling on cognitive ability.

4.1 Logistics of the Enlistment Procedure

All males in Sweden, with a few exceptions, were required to show up at a military enlistment center on an assigned date around their 18th birthday during our sample period.[12] The enlistment process took one day, and involved filling out paperwork, a basic health screening, and a series of physical and cognitive tests. The tests were used to help assign individuals to various tasks upon entry into military service. Enlisted males generally began their military service, which lasted 11 months on average, after finishing any formal secondary school education.

Our approach exploits random variation in the timing of enlistment, and hence when individuals take the cognitive tests. The way the enlistment process works generates conditionally random variation in the number of days between an individual's 18th birthdate and the date of enlistment. This exogeneity is due to the fact that enlistees do not choose their date of enlistment; rather the military assigns enlistment dates which are conditionally random. Enlistees had strong incentives to

assumptions could be relaxed.

[12]Exceptions included individuals who have severe handicaps, are currently in prison, are institutionalized due to mental disorders, are non-citizens, or who live abroad (and can therefore postpone their enlistment date until they return to Sweden). This last group is primarily comprised of individuals who study abroad during secondary school, and therefore enlist the year they return to Sweden at age 19. Sweden ended compulsory military service in 2010.

comply with the assigned date of enlistment, with failure to show up resulting in fines and eventual imprisonment.[13]

The military was provided with two pieces of information about individuals – their birthdates and their parish of residence – which they used to assign enlistment dates.[14] Some enlistment offices also used information on expected graduation date. The variation in enlistment dates around an individual's 18th birthday is a result of logistical constraints faced by the military. The goal was to have all individuals enlist close to their 18th birthday, but there were transportation issues and capacity constraints at the local enlistment centers. The military arranged for transportation as needed, purchasing blocks of train tickets for enlistees or chartering buses in more rural areas. The enlistment offices are closed over Christmas break and for 2 months during the summer. The military had six regional offices, each with responsibility for a defined geographical area of Sweden. When planning the enlistment dates for the coming year, each office was given a list of all males turning 18 during the upcoming year. In addition to information on birthdate, the military was also given the enlistees address by the local parish. Based on these two pieces of information, the regional offices tried to assign enlistment dates close to individual's 18th birthday, but in a way which also satisfied the logistical constraints involved with travel, being able to process a limited number of individuals each day, and enlistment office closure periods.

Most enlistment offices did not use any information other than birthdate and parish to assign enlistment dates (and hence test-taking dates). However, some enlistment offices additionally used information on expected graduation date in some years. The apparent reason is that enlistment offices wanted to process enlistees far enough in advance of their commencement of military service. In Sweden during our time period, individuals in the academic track in upper secondary school (the group we focus on) took either three or four years to finish. Individuals in four year programs had an additional year of schooling to complete before they would begin serving in the military, so there was less time pressure to process them quickly. For enlistment offices with enough capacity, they processed virtually the entire list of

[13] Assigned enlistment dates were strictly enforced. For example, if an enlistee was sick on their assigned day, they still had to show up to the enlistment office unless they had a signed excuse note from a doctor.

[14] The enlistment procedure was established in a law passed in 1969. The legal statute tasked the county tax authorities to gather information on all Swedish males turning 17 each year and forward it to the military enlistment office by August 1 (Statute 1991:726, paragraph 6). The tax authorities in turn collected the required information from each parish, which keeps up to date records on the local population. The parish provided information on the name, birthdate, and address for all eligible males in their jurisdiction. Enlistment orders with an assigned date were sent out to each individual as a certified letter which had to be picked up from the local post office.

candidates they received from the tax authorities in the same calendar year.

However, for enlistment offices with more severe capacity constraints, they prioritized individuals who were in their last year of school. Since the tax authorities only provided information on birthdate and parish, these more heavily constrained enlistment offices sent out preliminary letters asking individuals whether they expected to graduate at the end of the current academic year. They then sent out formal enlistment orders with an assigned date to all individuals, where the assigned date was based on birthdate, parish, and expected graduation.

The enlistment offices using expected graduation dates did not save this information. However, we do observe a strong predictor of expected graduation, namely, the student's upper secondary school program. Most fields of study took three years to complete, but the technical studies program could take four years to complete. We therefore use the individual's self-reported school program at the time of enlistment as our measure of expected graduation.[15] Since there is no record of which offices used expected graduation to assign test dates or how the information was used from year to year, we fully interact the enlistment office, enlistment year, and school program indicator variables. We examine this proxy for expected graduation in more detail in what follows.

Figure 1 plots the distribution of the total number of days between an individual's enlistment date and birthdate. In the figure, we normalize the distribution of age at test date to be relative to age 18 (i.e., we subtract off 18 years). While most individuals enlist within six months of their birthdate, there is substantial variation within this time frame. The standard deviation of the difference in enlistment date and birthdate is 108 days. The positive skew in the distribution is a consequence of the military trying to process the list of individuals turning 18 within the calendar year combined with enlistment centers closing in the summers.

For our approach to work, it is important that we condition our estimates on the same set of variables as the enlistment offices. Doing so insures that we have a quasi-experimental design with conditionally random variation in the number of school and nonschool days, as discussed in section 3.2. We verified with several current and former administrators and psychologists at the Swedish Defense Agency that the only three variables provided to the military were name, date of birth, and address (and hence parish code, the only geographic information used to assign dates), and that some enlistment offices sent out a preliminary letter requesting

[15] There are five academic school programs: business, humanities, social sciences, natural sciences, and technical studies. The technical studies program is the most popular, with 41% of our sample choosing it as their field of study. Individuals could also expect to graduate a year later if they had previously studied abroad or repeated a grade, but these cases are relatively rare.

information about expected graduation date.[16] In the next section we provide empirical evidence that assignment date appears to be random after conditioning on birthdate, parish, and expected graduation. As we show later, the birthdate and parish conditioning variables matter empirically, as individuals born at different times of the year or living in different parishes score differently on cognitive tests. In contrast, the expected graduation conditioning variables make little difference to our estimates once the parish and birthdate controls are conditioned on.

4.2 Cognitive Tests

Cognitive skills are measured during the enlistment procedure using what is called the "Enlistment Battery 80." The tests are similar in style to the Armed Services Vocational Aptitude Battery (ASVAB) in the U.S. There are separate paper and pencil tests for synonyms, technical comprehension, spatial ability, and logic. Each of these four tests consists of 40 items presented in increasing order of difficulty and is slightly speeded (see Carlstedt and Mårdberg 1993).

In the synonyms test, a target word is presented and the correct synonym needs to be chosen among four alternatives. This test is similar to the word knowledge component of the ASVAB and is meant to measure verbal ability. The technical comprehension test is comprised of illustrated and written technical problems, with a choice of three alternative answers. It has similarities with the mechanical comprehension portion of the ASVAB. The test which measures spatial ability is referred to as the metal folding test. The goal is to correctly identify the three-dimensional object that corresponds to a two-dimensional drawing of an unfolded piece of metal. In the logic test, a set of statements, conditions, and instructions are presented and a related question must be answered using deductive logic. Example test questions can be found in Appendix Figure A1.

The four tests are meant to capture two different types of intelligence. The synonyms and technical comprehension tests are examples of crystallized intelligence tests, while the spatial and logic tests are examples of fluid intelligence tests. The distinction will be important when we discuss our findings, so we provide a brief explanation of these two types of intelligence.

Cattell (1971, 1987) originally developed the concepts of crystallized and fluid

[16] We verified this information with Berit Carlstedt, formerly employed at the National Defense College (on February 14, 2012), Bengt Forssten at the Swedish Defense Recruitment Agency (on October 11, 2011), Ingvar Ahlstrand at the Swedish Defense Recruitment Agency (on October 11, 2011), and Rose-Marie Lindgren, chief psychologist at the Swedish Defense Recruitment Agency (on March 16, 2012). Information about the preliminary letter requesting expected graduation date was obtained from Ove Selberg at the Swedish Defense Recruitment Agency (on June 20, 2012).

intelligence as discrete factors of general intelligence. Crystallized intelligence is supposed to measure the ability to utilize acquired knowledge and skills, and therefore is closely tied to intellectual achievement. Fluid intelligence, on the other hand, is meant to capture the ability to reason and solve logical problems in unfamiliar situations, and should therefore be independent of accumulated knowledge. Fluid intelligence is often measured by tests which assess pattern recognition, the ability to solve puzzles, and abstract reasoning. Crystallized intelligence tests are much more focused on verbal ability and acquired knowledge. Different tests have been designed by psychologists to capture each type of intelligence. For example, the commonly used Wechsler Adult Intelligence Scale (WAIS-III) has both a fluid intelligence portion (named performance IQ) and a crystallized intelligence portion (named verbal IQ).

4.3 School Days and Nonschool Days

The Swedish school system consists of compulsory primary school (from the ages of seven to 16) as well as an optional secondary school (from age 16 up to age 19). Generally, everyone born in the same calendar year starts primary school together in August the year they turn seven, so that those born in January will be the oldest within each schooling cohort.[17] Secondary school splits into two tracks: a two-year program consisting of vocational training and a three- or four-year academic program which prepares students for university studies. Since enlistment usually occurs in the months around an individual's 18th birthday, to facilitate variation in school days, we limit our sample to young men enrolled in the 12th grade (i.e., those in academic programs).[18] While focusing on this sample may limit the external validity of our findings, it does not affect internal validity.

In total, there are around 180 school days and 185 nonschool days over the year in Sweden, which corresponds closely to the number of school days in the US and many other EU countries (OECD 2011). Separating the effect of school days on cognitive ability from the effect of nonschool days relies on the fact that the two are not perfectly correlated across individuals. Based on school calendars for the period

[17]There are also a small number of individuals who start school earlier and those that are held back a year. According to Fredriksson and Öckert (2005) three percent of all children born from 1975 to 1983 started school earlier or later than intended. Unfortunately, we cannot distinguish these individuals in our sample, and hence, a small number of individuals will be included in the analysis that have already left school by age 18.

[18]In our data, 16% of young men drop out of school after finishing compulsory primary school, 50% study in a two-year vocational program, and 34% study in a three- or four-year academic program. While it is possible in theory to get variation in school days for individuals in vocational programs who enlist before June of their 11th grade year (their last year), there are not enough of these observations.

1979-1994 we are able to calculate the exact number of school days and nonschool days between the day of enlistment and the 18th birthday for each individual in the data. The two longest periods of consecutive nonschool days are summer vacation (10 weeks) and Christmas break (2.5 weeks). There are also two other week-long school breaks during the spring semester, one in February (winter break) and one in the spring (Easter break), as well as ordinary weekends and other miscellaneous nonschool days. The timing of the February break varies geographically and the timing of the Easter break varies geographically and chronologically, facts we take into account when calculating school and nonschool days.

As Figure 2 shows, the quasi-random assignment of test dates generates substantial variation in the number of school days in our sample. As we did for Figure 1, the number of school days is normalized to be relative to one's 18th birthday. The standard deviation for school days in our sample is 51 days. A sizable amount of variation exists even after accounting for the conditioning variables used by the military to assign enlistment dates. Controlling for birthdate (birth week fixed effects), cohort (yearly fixed effects), parish (parish fixed effects), and expected graduation (enlistment office $\times$ enlistment year $\times$ school program fixed effects) in a linear regression, residual days of schooling has a standard deviation of 39 days.

4.4 Sample Restrictions

We make a few additional sample restrictions to be able to cleanly estimate the effect of schooling on cognitive skills. While the restrictions may limit the generalizability of our findings, they should not affect the internal validity of our estimates, since the restrictions are based on variables observed before enlistment dates are known.

First, we exclude non-native Swedes, defined as those who were born abroad or who have at least one parent born abroad. These cases constitute 15 percent of the population.[19] In our framework, separating school days from nonschool days requires that individuals be enrolled in school the year they are tested. We therefore restrict our estimation sample to all men who were enrolled in a three or four year academic program in high school. This means we will not study the effect of extra school days for those individuals who drop out of secondary school or enroll in two-year vocational training, since many of these individuals will already have completed school prior to enlistment.[20] We further restrict the sample to individuals turning

[19]Non-natives have a much lower enlistment rate since less than fifty percent are Swedish citizens, and only citizens were required to enlist.

[20]Categorization into academic tracks is based on two sources of information: self-reports at the time of enlistment and registrar information on completed level of education as of 2003. After limiting the sample to those enrolled in a three or four year program based on self reports, we

18 during the year they enlist. This restriction largely excludes students studying abroad when they are 18.[21] We further exclude the 1966 and 1967 birth cohorts since information on an enlistee's scores for the four cognitive tests is missing for two-thirds of observations in the administrative dataset. We also exclude individuals affected by the teacher strike in 1989, when school was canceled for most of November and December. Finally, we drop enlistees near the end of 1994 who take a new and different battery of cognitive tests.

After these restrictions, we are left with a sample of 128,617 native males who were attending secondary school at the time of enlistment and for whom there is full information for our key variables.

5 Conditional Randomness of Test Dates

As described in section 3.2, causal identification relies on test dates being conditionally random. As long as an individual's test date is conditionally random, both age at test date and number of days spent in school will vary randomly across individuals. As discussed in 4.1, date of birth, parish, and for some enlistment offices, expected graduation date, are the only variables used by the military to assign enlistment dates (and hence test dates). In this section, we provide empirical support that this is the case. We also show why failure to control for both birthdate, parish, and expected graduation date could lead to biased estimates, as these variables are correlated with a variety of outcomes which are predictive of cognitive test scores.

5.1 Tests for Conditional Randomness

If age at test date and number of school days are conditionally random, they should both be unrelated to background characteristics after flexibly accounting for the conditioning variables. It is particularly important that age and school days are not correlated with variables that predict cognitive skills, since these types of correlations can create a bias. In our dataset, we have several variables which are highly predictive of cognitive test scores: math and Swedish grades in 9th grade, mother's and father's education, and father's income.[22] The relationship between

then discard individuals with less than 12 years of completed education in 2003. The second step eliminates an additional 2% of observations.

[21]It is not uncommon for Swedes to study abroad for a year during secondary school. Roughly 16% of the population do not enlist until they are 19; this is largely due to study-abroad students returning to Sweden at age 19 and students in four-year programs whose enlistment processing was delayed as described in Section 4.1.

[22]Grades in math and Swedish each range from 1 to 5. Grades follow roughly a normal distribution; for example, in Swedish 3% score a 1, 29% score a 2, 45% score a 3, 21% score a 4,

these variables and cognitive scores is presented in Table 1.[23] The differences in cognitive test scores by background characteristics are large. For example, students with low math grades in our sample score almost half a standard deviation lower on the technical comprehension test compared to students with higher math grades (.51 - .03 = .48). Similarly, individuals whose fathers have less than 12 years of schooling score 0.15 standard deviation lower on the technical comprehension test. Large gaps by background characteristics are found for the other tests as well, regardless of whether the test is measuring crystallized or fluid intelligence. All of the differences by background characteristics in Table 1 are statistically different from each other at the 1% level.

Since each of the background variables are observed before enlistment, they should be uncorrelated with test date conditional on birthdate, parish, and expected graduation. To empirically test this, we regress age at test date and number of school days on each background characteristic, including the variables the military used to assign test dates as additional controls. For birthdate, we include 52 birth week dummies (one for each week of the year) and 13 birth cohort dummies. We also include roughly 2,500 parish dummies, which is the level of geographic detail the military uses to organize enlistment dates.[24] As explained previously, we do not directly observe expected graduation or which enlistment offices used this variable over time. Therefore, we use school program (i.e., field of study) as a proxy for expected graduation in four versus three years, and interact this with enlistment office and year. The estimates which control for the entire set of conditioning variables appear in column 5 of Tables 2 and 3. Table 2 reports results for age at test date, while Table 3 reports results for number of school days. As expected, whether one uses age or school days as the dependent variable, the estimated coefficients using this specification are small and statistically insignificant. The estimates are also not jointly significant in either table. These regressions provide strong empirical support for the claim that both age and school days are conditionally random.

In the other columns of Tables 2 and 3, we run a series of similar regressions as we did for column 5, except that we selectively exclude or include controls for birthdate,

and 3% score a 5 in the entire population. The fact that we have relatively more individuals in the high grade category reflects the fact that our sample is restricted to students who went on to a three year academic program.

[23]Each of the cognitive tests is normalized to be mean 0 and standard deviation 1 based on the entire population of test takers, and not just those in our sample. Since our sample is comprised of individuals enrolled in 12th grade (many individuals stop schooling before 12th grade), and this group has higher average cognitive test scores, this explains why the means reported in Table 1 are greater than zero for every subcategory.

[24]Parish boundaries change over time, so that there are closer to 1,500 parishes at any one time; we assign a unique dummy each time a parish's boundary changes.

parish, and expected graduation. The purpose of these columns is to show that age and school days are not unconditionally random. The first specification (column 1) includes no birthdate, parish, or expected graduation controls. The resulting coefficients are generally larger in magnitude compared to column 5 for both tables, and statistically significant in many cases. For example, students who earn high grades in Swedish in 9th grade are 4.8 days older and have 4.8 more days of school when they take the cognitive tests. Across the two tables, significant associations are found for math grades, mother's education, father's education, father's earnings, and whether background variables are missing in the dataset as well. The joint significance of these background variables is captured by the sizable and significant F-statistics appearing in the tables.

The next set of regressions (column 2) adds in dummies for each parish. These controls change many of the coefficients, but six out of nine estimates are large and statistically significant in both the age and school days regressions. Column 3 adds in controls for birth cohort and age (but not parish dummies). As in column 2, these controls alter many of the coefficients, but many of the coefficients remain statistically different from zero. Column 4 adds in expected graduation controls. The F-test continues to reject the null that these background characteristics are unrelated to age at test date and school days. Interestingly, the separate additions of parish, birthdate, and expected graduation controls suggest different types of correlations for these three sets of controls. Only when all three sets of controls are included simultaneously are the estimated coefficients close to zero and statistically insignificant, as shown in column 5 in both tables.

5.2 Non-Randomness of the Conditioning Variables

We have documented that age at test date and school days appear to be random only after conditioning on birthdate, parish, and expected graduation. To better understand why failure to control for these conditioning variables could cause a bias, in Figures 3 and 4 we show how background characteristics vary by season of birth. In these figures we use the universe of all enlistees, as opposed to our estimation sample which only includes individuals who were attending school at the time of enlistment. Figure 3 plots the average years of education for mothers and fathers, as well as father's earnings, by enlistee's month of birth. If births are distributed randomly throughout the year, there should be no variation in these variables through the year. Instead, the graphs reveal statistically significant differences by season of birth. Births in March and April have more educated and higher wage parents, especially compared to births in November and December. The pattern is not linear, with a slight uptick in September as well. The fact that parents of higher socioeconomic status avoid having their children near the end of the year is particularly interesting when one recognizes the cutoff date for school entry in Sweden is January 1. This cutoff date means that students born at the end of the year will be the youngest children in their class, which some researchers have argued hurts a child's academic and social development.

Figure 4 provides compelling evidence that academic performance is correlated with an individual's season of birth. Average grades in both Swedish and math are highest for individuals born near the beginning of the year and decline almost monotonically throughout the year. This pattern reveals that children who are the oldest in their class do substantially better than those who are the youngest, earning grades which are up to 5 percent higher on average. The patterns are striking, although we cannot say whether they are due to relative age within a classroom or differences in parental characteristics by season of birth.

The findings discussed in this section are important for more than just the present study. Our results indicate that birthdate is not randomly assigned, and that the way in which birthdate systematically varies is correlated with both age at test date, school days, and cognitive achievement. As other researchers have argued in different contexts (see footnote 4) this section provides a cautionary tale for research which uses season of birth variables as instruments.

For the current paper, the key result is that after conditioning on birthdate, parish, and expected graduation flexibly, both age at test date and number of school days appear to be randomly distributed. This provides confirmatory evidence that the military only uses these variables to assign enlistment dates and that we have a conditionally random experiment.

6 Results

This section describes our empirical specification, presents the main results, and conducts some robustness checks. It also explores whether there are heterogeneous returns to school days and age.

6.1 Are Cognitive Skills Malleable?

A first-order question is whether cognitive skills, as measured by the four tests, are fixed by age 18 or can develop further over time. We therefore begin our analysis by presenting results of the effect of age on test scores. If older test-takers are observed to have higher cognitive test scores, this provides strong evidence that cognitive skills are malleable. Recognizing that age at test date equals the cumulative number of school days plus nonschool days, in the next section we will separate out the marginal effect of extra school days on cognitive development.

Our dependent variables are the test scores of the four cognitive ability tests. The raw test scores range from 1 to 40, corresponding to the number of correct answers on an exam. We standardize the scores to have a mean of zero and a standard deviation equal to one in the entire population of test takers (not just those in our sample) in order to facilitate comparisons across the four tests as well as with other studies. Our independent variable is age at test which by construction equals enlistment date minus birthdate. We divide the age variable by 100 in the regressions for ease of presentation.

Age at test date is exogenous only after conditioning on birthdate, parish of residence, and expected graduation year. Therefore, we include flexible controls for these variables in the analysis, using the same set of conditioning variables as in column 5 of Tables 2 and 3. We also include several pre-determined variables in the regressions, including controls for family size, parental education, parental age, father's earnings, grades in math and Swedish in 9th grade, and field of study in high school. As we show in a robustness table, these additional variables do not appreciably change the estimates, although they do decrease the standard errors by around 10 percent.

Figure 5 graphically depicts the coefficient estimates for age from each of the four cognitive test regressions. In each case, the aging effect is sizable and statistically significant. This provides strong evidence that both crystallized and fluid cognitive skills change over time, with older individuals doing substantially better on the tests. Individuals who are ten days older score approximately 0.4 percent of a standard deviation better on the synonym, technical comprehension, and logic tests. The estimate is half as large for spatial ability, which is a fluid intelligence test.

6.2 Main Results

The main focus of this paper is the effect of extra school days on cognitive development. Since age at test date equals the cumulative number of school days plus nonschool days, the previous section estimated the combined effect of the two types of days. In this section, we separate out the effect of an extra school day above and beyond a general aging effect.

Table 4 presents our baseline results. We use the same empirical specification as we did in the previous section, but add an additional independent variable which measures the number of school days. Remember that the age variable equals school days plus nonschool days. Therefore, the coefficient on the age variable represents the effect of aging by one day (regardless of type of day), while the coefficient on the school days variable captures the extra effect when one more of these days is spent in school.

For the crystallized intelligence tests, we find an extra 10 days of school instruction raises cognitive scores for synonym and technical comprehension tests by 1.1 percent and 0.8 percent of a standard deviation, respectively. To put these estimates in perspective, they imply an additional year of schooling (180 days in Sweden) results in test scores which are 21 percent of a standard deviation higher for synonyms and 14 percent for technical comprehension. This is the effect above and beyond any general aging effect, which is small and statistically insignificant for both of these tests.

The two tests which measure fluid intelligence show a different pattern. Both the spatial ability and logic tests show a statistically significant, but modest aging effect: individuals who are 10 days older perform between 0.3 and 0.5 percent of a standard deviation better. In contrast to the first two tests, the extra impact of an additional day of schooling is actually negative, although not statistically different from zero. Note that these negative coefficients do not mean that school days lower cognitive skills, since the total effect of a school day is the sum of the age coefficient and the school days coefficient. Rather the negative coefficients imply that school days improve performance on these two cognitive tests at a somewhat reduced rate relative to a nonschool day. While the standard errors are large enough to prevent precise conclusions, we interpret these results as evidence that schooling does not significantly contribute to the development of fluid intelligence, at least as measured by spatial or logical ability tests.

The contrast between the first two tests (synonym and technical comprehension) and the second two tests (spatial ability and logic) are particularly interesting when one remembers the distinction between crystallized and fluid intelligence. As discussed in Section 4.2, fluid refers to intelligence which can be applied to a variety of problems, while crystallized refers to intelligence which is more context specific. Fluid intelligence has been linked to the prefrontal cortex and regions of the brain responsible for attention and short-term memory. In contrast, crystallized intelligence is related to areas of the brain associated with long-term memory. Crystallized intelligence is thought to be more malleable over time as individuals acquire more knowledge and experience. But the relationship between each of these types of intelligence and schooling is not well-understood.

With the distinction between the two types of intelligence in mind, we return to the interpretation of Table 4. The synonym and technical comprehension tests, which capture crystallized intelligence in a manner similar to the Wechsler Adult Intelligence Scale, are strongly influenced by how much schooling an individual is exposed to. Our estimates suggest that schooling is an effective means to add to an individual's cognitive skills in this dimension, as might be expected. In contrast, it is interesting that the experiences gained on nonschool days seem to have very little effect on these measures of crystallized intelligence, even though nonschool experiences could in theory also be beneficial. In contrast, the spatial and logic tests we study capture fluid intelligence. Our results suggest that fluid intelligence is unaffected by additional amounts of schooling, even though it is modestly affected by general aging.

These are important findings in the literature, as the prior research in psychology which attempts to separate out schooling from aging on crystallized versus fluid intelligence has estimated correlations rather than causal effects (Cahan and Cohen 1989, Cliffordson and Gustafsson 2008, Stelzl et al. 1995). The key advantage of our design is that we use conditionally random variation, which allows for estimates based on quasi-experimental variation as discussed in 3.2. Our findings also suggest the common practice of averaging over both crystallized and fluid intelligence tests may be inappropriate for some applications, as the two types of tests are differentially affected by schooling and aging.

To illustrate the importance of flexibly controlling for birthdate and parish, in Table 5 we report results which do not include these conditioning variables. Except for the exclusion of the birthdate, parish, and expected graduation conditioning variables, the analysis in Table 5 mirrors that of Table 4. The difference in estimates are substantively important and point towards nontrivial omitted variable bias, as reported in panel B. The biggest differences show up for the crystallized intelligence tests. When the conditioning variables are erroneously excluded, the coefficient on school days falls by roughly half for the synonyms test, from 0.112 to 0 .059. For the technical comprehension test, the school days coefficient loses significance, dropping to almost zero (from 0.078 to 0.015). Using a Hausman specification test, these two differences are both statistically significant. While the estimated coefficients for the fluid intelligence tests change somewhat, the differences are not statistically significant. These findings demonstrate how failure to condition on birthdate, parish, and expected graduation variables change the estimates in ways that lead to incorrect conclusions about the effect of schooling on cognitive skills.

6.3 Robustness

Table 6 provides a variety of robustness checks. For simplicity, we average the two crystallized intelligence tests (synonyms and technical comprehension) and the two fluid intelligence tests (spatial and logic). As before, we normalize the averaged test scores to be mean zero and standard deviation one for the entire sample of test takers. The first panel in the table presents results similar to Table 4, using the two averaged test scores as the dependent variables instead of the four individual test scores. For crystallized intelligence, the coefficient is a large and statistically significant 0.111 for school days and close to zero for age, as expected given the more disaggregated results in Table 4. For fluid intelligence, the coefficient on school days is slightly negative and insignificant, while age has a modest but statistically significant coefficient of 0.040.

If test dates are conditionally random, it should not matter whether other pre-determined covariates (besides the conditioning variables of birthdate, parish, and expected graduation) are included in the regression. In panel B, we test this prediction empirically by excluding the control variables for father's earnings, parent's age and education, family size, and math and Swedish grades. As expected, the coefficients for both crystallized and fluid intelligence are very similar to those in panel A. This finding is not because the control variables do not predict test scores. The addition of these background controls increases the R-squared from 0.203 to 0.262 for crystallized intelligence and from 0.184 to 0.237 for fluid intelligence.

Panel C includes 365 birth day dummies as conditioning variables instead of 52 birth week dummies. The resulting estimates are similar to baseline. While not shown in the table, it is important to recognize that less flexible functions of birthdate can change the estimates. For example, including quarter of birth dummies instead of 52 birth week dummies drops the coefficient on school days in column 1 from 0.111 to 0.070; similarly, including age linearly drops the coefficient from 0.111 to 0.051.

Panel D uses a more parsimonious set of controls for an individual's residence. Instead of using approximately 2,500 parish dummies as conditioning variables, this panel uses 287 municipality dummies (parishes are embedded within the larger geographical unit of a municipality). This change results in only modestly different estimates.

In the next two panels, we explore our set of proxy variables for expected graduation. As a reminder, some enlistment offices conditioned on whether an individual stated that they were planning on graduating this year or next year. However, the military did not keep a record of this variable or which enlistment offices used it. Since different school programs (i.e., fields of study) could take three versus four years, we used this as our proxy for expected graduation, interacting it with enlistment office and enlistment year. In panel E, we see what happens when we completely omit these expected graduation conditioning variables. The estimates in both columns change little when omitting these proxy conditioning variables. As we demonstrated in Table 5, failure to jointly condition on birthdate, parish, and expected graduation changed many of the estimates; panel E indicates that it is not the expected graduation variables which drive the difference.

In panel F we use a different approach to assess the expected graduation conditioning variables. Two of the six enlistment offices were very efficient at processing enlistees. These two offices processed over 95% of enlistees during their 18th year. These enlistment offices did not appear to be capacity constrained, and were therefore unlikely to have sent out a letter asking about expected graduation date. Panel F estimates the baseline model with the expected graduation variables for these two offices.The estimates in panel F are similar to baseline, although the standard errors double since this is a smaller sample.

In the last panel, we limit the sample to enlistees processed within 6 months of their birthday to make sure that individuals who were processed very early or very late are not driving the results. While this restriction reduces the sample by about 12%, it does not appreciably change our estimates.

6.4 Heterogeneity

From a policy perspective, one of the more interesting questions is whether the return to schooling on cognitive ability is heterogeneous. In particular, do individuals with lower initial cognitive ability gain more from additional days of schooling or is the reverse true? If low ability individuals experience high cognitive returns to schooling, then extra schooling resources spent on this group could have a high individual and social return.

A priori, there is no clear answer to this question. Higher ability individuals may absorb new information and new ways of thinking relatively better in the school setting. Alternatively, if individuals have low initial cognitive ability due to a less enriching home environment (e.g., due to lower family income or lower parental education), then gains in cognitive ability could increase more rapidly in a structured learning environment.

While we do not observe baseline levels of cognitive ability in our dataset, we do observe a variety of pre-determined characteristics which are correlated with cognitive ability. Table 1 documents the raw differences in cognitive ability based on grades in 9th grade, parental education, and father's earnings. The gaps in cognitive ability are all statistically significant and relatively large. For example, individuals who earn high grades in school score approximately one-third to one-half of a standard deviation higher on the cognitive tests. Individuals whose parents are more educated and have high earnings also do better on cognitive tests. In Table 7, we analyze whether there are heterogeneous returns to schooling based on these pre-determined characteristics which are predictive of cognitive ability. As we did for the robustness table, we continue to focus on the average of the test scores measuring either crystallized or fluid intelligence.

Table 7 reports results which mirror the baseline specification in panel A of Table 6, but which allow for separate coefficients on the schooling and age variables by background characteristic. We begin our discussion of this table by focusing on the findings for crystallized intelligence.

The first panel interacts the school days and age variables with indicators for whether the student had low or high math grades. The coefficient on schooling is similar for crystallized intelligence tests whether or not the student received low or high grades in math (.197 versus .190), even though the mean scores are very different based on math grades (see Table 1). The coefficients for age based on math grades are also not markedly different from each other. A very similar pattern holds when one allows for separate coefficients based on Swedish grades. One thing to remember for the results based on grades is that we only have information on grades for birth cohorts from 1972 to 1976; this different sample explains why the coefficient estimates are somewhat different in magnitude compared to the baseline results. We also find that mother's education does not markedly affect the coefficients on school days or age. Children of fathers who are highly educated have a somewhat larger coefficient for extra school days, but this difference is not statistically significant. Finally, looking at family income (as measured by father's earnings), we again find very little evidence for heterogeneous impacts for either school days or age.

Turning to the results for fluid intelligence in column 2, we again find little evidence for differential returns based on background characteristics. None of the pairwise comparisons are statistically different from each other at the 10% level. The coefficients jump around somewhat for math and Swedish grades, but this is not unexpected given the smaller sample size (and larger standard errors) for these two panels. The effects based on parental education and father's earnings appear to be fairly homogenous for both the schooling and age coefficients.

These results are interesting since they indicate that both high and low ability students benefit from additional schooling. While we do not have enough precision to rule out small differences by background characteristics, our interpretation of the results is the marginal return to extra school days is remarkably homogeneous, even for groups with very different abilities. From a policy perspective, our findings are suggestive that providing additional school resources or encouraging further study will aid a variety of students.

7 Discussion

Taken together, our findings have several important implications. In this section, we discuss what our results imply about the malleability of cognitive skills, human capital versus signaling models, the interpretation of schooling coefficients in wage regressions, and the potential benefits to increasing the length of the school year.

To begin, the results indicate that cognitive test scores are malleable into young adulthood and are therefore not comparable across individuals who had different levels of education or were of different ages when they took the test. Other countries use similar tests for military enlistees, such as the Armed Service Vocational Aptitude Battery (ASVAB) in the U.S. Cognitive tests are also used for some job applications and for college entrance exams (including the SAT and the GRE). In addition, academics use these types of tests as measures of cognitive ability in their research. Given the importance of these tests in so many different areas, it is critical to recognize they do not measure immutable intelligence.

To provide a better sense of the magnitude and relevance of our findings, we perform several simple calculations. In the remainder of this section, we focus on the crystallized intelligence tests since they were affected by extra schooling. While each of the calculations which follows is based on several assumptions and extrapolations, their purpose is to help quantify the role schooling plays in the production of cognitive skills.

Our first calculation suggests that not all of the gap in cognitive ability across education categories is due to signaling, as our findings suggest an important learning component. Extrapolating our estimate, an additional year of schooling (180 days) raises crystallized test scores by about one-fifth of a standard deviation. While one may be tempted to attribute wage gaps across education categories to self selection and sorting, our results indicate a sizable portion of the gap is likely due to schooling increasing human capital.

Our second calculation provides insight into the interpretation of schooling coefficients in standard wage regressions. Using prior estimates from the literature, a one standard deviation increase in cognitive ability is associated with roughly a

7% increase in wages. Combining this with our estimate of how schooling affects cognitive ability, an extra year of schooling is responsible for a 1.4% increase in wages solely due to an increase in cognitive ability. Stated another way, approximately 18% of the return to an extra year of schooling in wage regressions (which do not control for cognitive skills) is attributable to the increase in cognitive ability resulting from an extra year of schooling.[25]

Finally, our results suggest that increasing the length of the school year could improve cognitive ability and benefit students from a variety of backgrounds. Proposals to extend the school year in the U.S. typically suggest an extra 20 days be added to the school year, often with the explicit goal of helping students be more globally competitive.[26] Among OECD countries in 2009, the U.S. placed 14th out of 33 in a reading test administered by the OECD. If the school year was extended by 20 days starting in kindergarten and if our results can be applied cumulatively to other grade levels and be compared to the OECD test, the U.S. would improve its standing from 14th to 4th place in the rankings.[27]

8 Conclusion

While scores on cognitive ability tests are positively associated with schooling, estimating the causal effect has proven difficult due to reverse causality and the difficulty in separating out confounding factors such as age at test date, relative age in the classroom, and season of birth. The best studies to date link schooling to cognitive skills using school cutoff dates as an instrumental variable or use structural modeling, both of which solve some problems but also require additional assumptions. In this paper, we use a fundamentally different identification approach which exploits conditionally random variation in assigned test date. We take advantage of this quasi-experimental setting to estimate the effect of schooling and age on cognitive test scores, without the need for either an instrument or assumptions about birthdate.

Our key result is that additional schooling causally increases performance on crystallized intelligence tests. We find that 10 more days of school instruction

[25]This calculation assumes the return to a year of schooling is 8%, so that 1.4% / 8% = 18%.

[26]President Obama and Education Secretary Arne Duncan have both advocated for lengthening the school year to help American students compete globally. While the school year is currently about 180 days in both Sweden and the U.S., in many countries it is 200 days or more.

[27]The OECD administered the Programme for International Student Assessment (PISA) by giving standardized reading, math, and science tests to 9th graders. Twenty extra school days from kindergarten up to 9th grade results in an increase of approximately 200 school days, which implies 22% of a standard deviation increase on the synonyms test based on Table 4. Twenty-two percent of a standard deviation translates into an additional 22 points on the PISA, which would increase the U.S. ranking from 14th to 4th.

raises cognitive scores on synonyms and technical comprehension tests (crystallized intelligence tests) by approximately one percent of a standard deviation. This is a relatively large effect. It suggests that an additional year of schooling (180 days) raises crystallized test scores by approximately one-fifth of a standard deviation. Extra nonschool days have no effect on crystallized intelligence. In contrast, test scores measuring fluid intelligence (spatial and logic tests) do not increase with extra schooling, but do increase modestly with age. These findings have important implications for questions about the malleability of cognitive skills in young adults, schooling models of signaling versus human capital, the interpretation of test scores in wage regressions, and policies related to the length of the school year.

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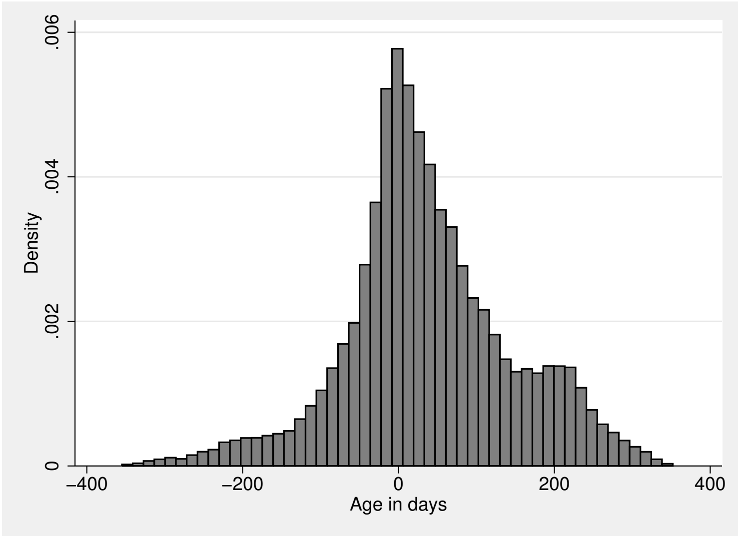


Figure 1. Distribution of age at test date.

Notes: Age as of the test date is normalized to be relative to age 18.

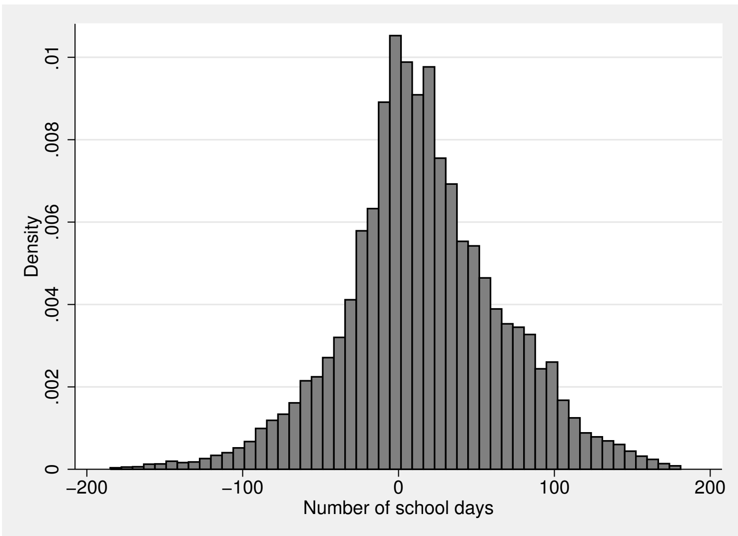


Figure 2. Distribution of number of school days.

Notes: Number of school days as of the test date is normalized to be on the same scale as age at test date, i.e., relative to age 18.

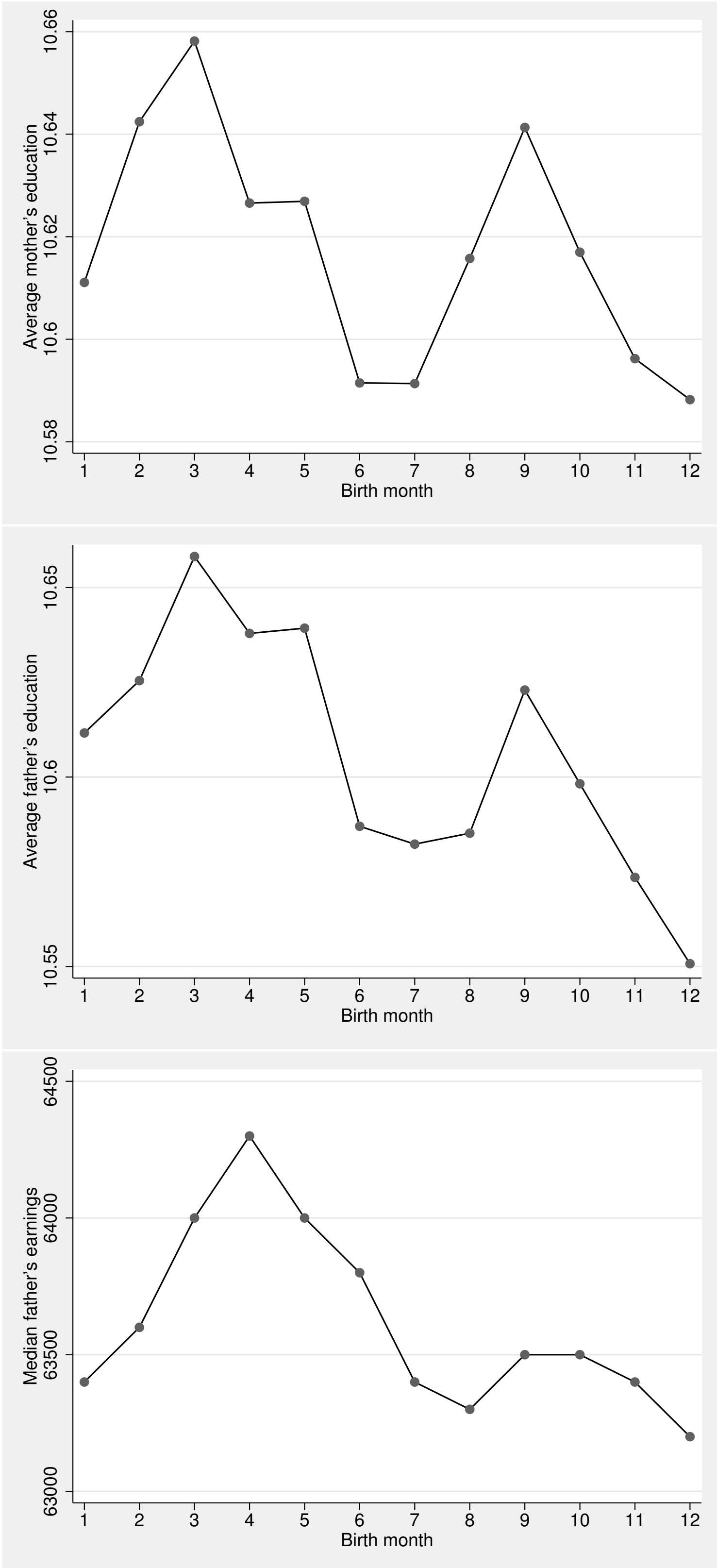


Figure 3. Socioeconomic background and month of birth.

Notes: Sample includes the universe of all enlistees. N=964,471 in the top graph, N=827,550 in the middle graph, and N=1,018,724 in the bottom graph.

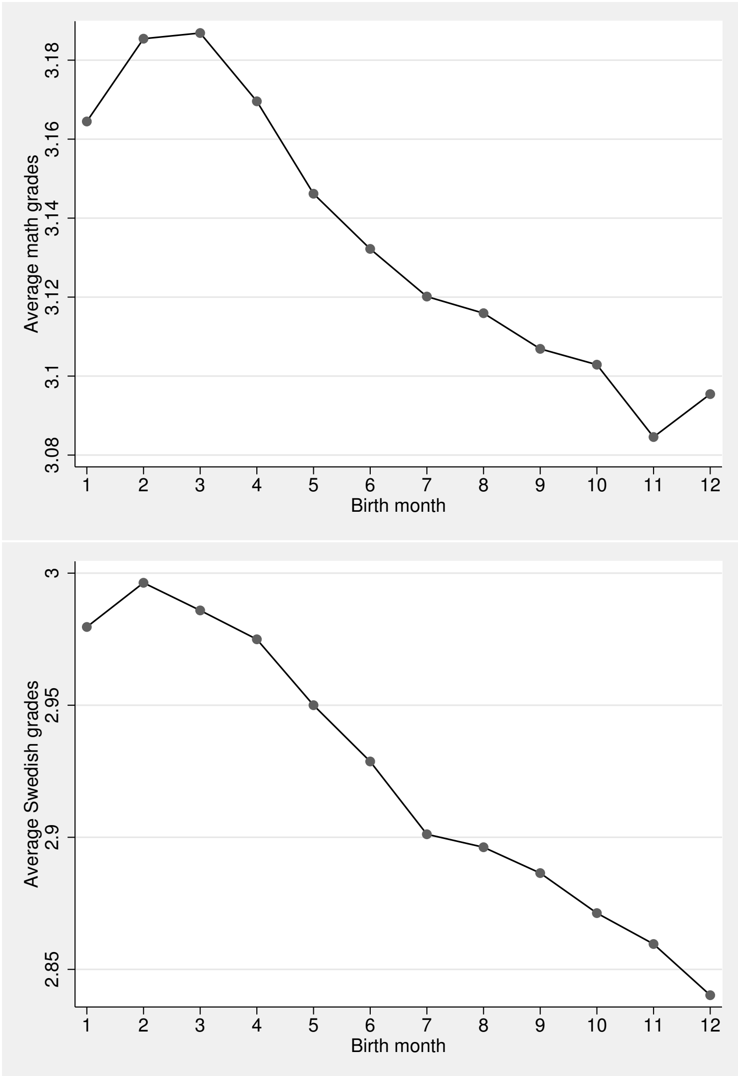


Figure 4. Grades in 9th grade and month of birth.

Notes: Sample includes the universe of all enlistees from the 1972 to 1976 birth cohorts (the cohorts for which grades are available). N=335,836 in the top graph and N=340,155 in the bottom graph.

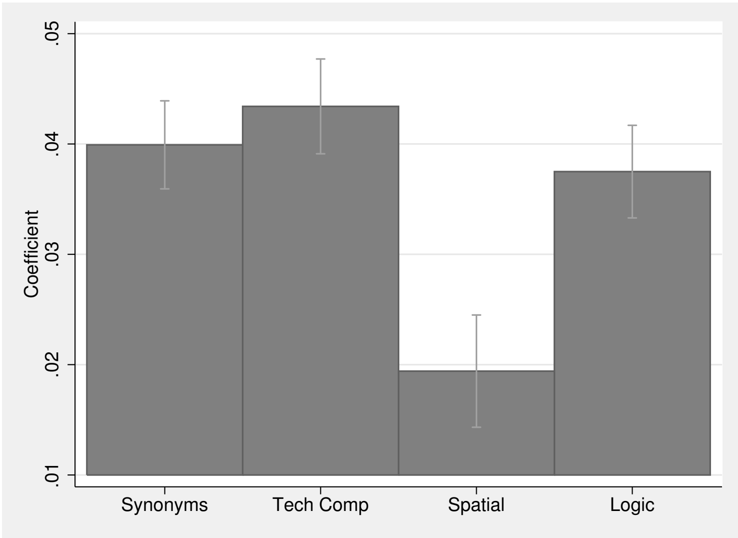


Figure 5. Increase in cognitive test scores for a 100-day increase in age.

Notes: Regression coefficients from separate regressions of cognitive test scores on age at test date, with ninety-five percent confidence intervals. Regressions do not include a variable for the number of school days, but do include the conditioning variables of birthdate, parish of residence, and expected graduation as well as controls for father's log earnings, mother's and father's age and age squared, and dummies for family size, mother's and father's years of education, and math and Swedish grades. When a covariate has a missing value for an observation (or is zero for earnings), we assign the mean value to the covariate and assign the value of one to a dummy variable which indicates whether the covariate is missing.

Table 1. Mean cognitive test scores by background characteristics.

	Crystallized Intelligence		Fluid Intelligence		
	Synonyms	Technical comp.	Spatial	Logic	N
	(1)	(2)	(3)	(4)	(5)
A. Math grades					
Low (1-3)	0.54	0.03	0.56	0.53	21,222
High (4-5)	0.86	0.51	1.05	1.09	27,447
B. Swedish grades					
Low (1-3)	0.43	0.14	0.68	0.55	21,581
High (4-5)	0.96	0.42	0.97	1.08	27,088
C. Mother's education					
$<$ 12 years	0.74	0.22	0.76	0.80	63,912
$\geq$ 12 years	0.91	0.40	0.94	0.94	57,761
D. Father's education					
$<$ 12 years	0.72	0.22	0.76	0.80	45,776
$\geq$ 12 years	0.88	0.37	0.91	0.91	67,376
E. Father's earnings					
Below the median	0.77	0.27	0.81	0.83	64,200
Above the median	0.89	0.35	0.89	0.90	64,417

Notes: Each of the cognitive tests is normalized to be mean 0 and standard deviation 1 for the entire population of test takers; our sample is comprised of 18 year old students enrolled in the academic high school track, a group with higher average test scores. Math and Swedish grades are from the 9th grade, with values ranging from 1 to 5, and are only available for the birth cohorts 1972-1976. Father's earnings come from the year 1980, when most fathers were between the ages of 27 and 60. All pairwise comparisons are statistically significant at the 1% level.

Table 2. Regression tests for conditional randomness: Age at test date.

	Dependent variable: Age in days				
	Conditioning variables				
	None (1)	Parish (2)	Birthdate (3)	Expected graduation (4)	All (5)
High math grades	-1.371	-0.071	-2.052**	0.684	-0.136
	(1.048)	(1.014)	(0.911)	(1.035)	(0.875)
High Swedish grades	4.836**	4.816**	-0.165	2.876**	-1.256
	(1.041)	(1.008)	(0.907)	(0.998)	(0.845)
Highly educated mother	-1.464**	-2.550**	0.891	-0.798	0.492
	(0.661)	(0.645)	(0.576)	(0.629)	(0.535)
Highly educated father	-0.271	-0.481	0.795	0.051	0.480
	(0.731)	(0.713)	(0.636)	(0.692)	(0.589)
High father's earnings	5.907**	2.134**	3.247**	2.088**	0.582
	(0.658)	(0.648)	(0.575)	(0.626)	(0.536)
Math grades missing	-8.190*	-8.599*	2.688	-2.796	1.333
	(4.691)	(4.858)	(4.105)	(4.461)	(4.041)
Swedish grades missing	1.286	-1.001	8.900*	13.032**	1.301
	(4.684)	(4.846)	(5.152)	(5.605)	(4.944)
Mother's education missing	6.878**	5.667**	1.059	2.365*	0.785
	(1.365)	(1.318)	(1.192)	(1.290)	(1.089)
Father's education missing	6.532**	4.742**	0.415	1.636*	0.298
	(0.959)	(0.929)	(0.846)	(0.917)	(0.775)
F-test	28.73	31.81	8.06	4.40	0.92
[p-value]	[0.000]	[0.000]	[0.000]	[0.000]	[0.502]

*Notes: N=128,617 in all columns. High math and Swedish grades defined by a grade of 4 or 5, highly educated mother or father defined by 12 or more years of education, and high father's earnings defined by earnings above the median. See notes to Table 1. Column 2 includes parish dummies, column 3 includes birth year dummies and dummies for each birthday week within a year, column 4 includes the complete interaction of enlistment office, enlistment year, and school program, and column 5 includes all of the controls of columns 2-4. **p-value<0.05, *p-value<0.10.*

Table 3. Regression tests for conditional randomness: Number of school days.

	Dependent variable: Number of school days				
	Conditioning variables				
	None (1)	Parish (2)	Birthdate (3)	Expected graduation (4)	All (5)
High math grades	-1.942*	-0.656	-2.600**	-0.310	-0.855
	(1.057)	(1.030)	(0.920)	(1.053)	(0.897)
High Swedish grades	4.751**	4.922**	-0.541	3.492**	-1.011
	(1.051)	(1.025)	(0.916)	(1.015)	(0.866)
Highly educated mother	-1.174*	-2.375**	1.156**	-0.474	0.781
	(0.667)	(0.656)	(0.582)	(0.639)	(0.548)
Highly educated father	0.235	0.362	1.313**	0.360	0.584
	(0.738)	(0.724)	(0.643)	(0.703)	(0.603)
High father's earnings	6.075**	2.031**	3.391**	2.365**	0.496
	(0.664)	(0.689)	(0.581)	(0.636)	(0.549)
Missing math grades	-10.521**	-9.301*	-0.365	-1.584	3.020
	(4.732)	(4.937)	(4.146)	(4.535)	(4.140)
Missing Swedish grades	3.510	-0.680	14.988**	16.051**	0.677
	(4.725)	(4.925)	(5.204)	(5.698)	(5.065)
Missing mother's education	6.637**	5.499**	0.767	2.348*	0.536
	(1.377)	(1.339)	(1.204)	(1.312)	(1.116)
Missing father's education	6.824**	4.926**	0.700	1.803*	0.270
	(0.968)	(0.944)	(0.855)	(0.932)	(0.794)
F-test	29.96	32.56	10.86	5.90	1.23
[p-value]	[0.000]	[0.000]	[0.000]	[0.000]	[0.274]

*Notes: N=128,617 in all columns. High math and Swedish grades defined by a grade of 4 or 5, highly educated mother or father defined by 12 or more years of education, and high father's earnings defined by earnings above the median. See notes to Table 1. Column 2 includes parish dummies, column 3 includes birth year dummies and dummies for each birthday week within a year, column 4 includes the complete interaction of enlistment office, enlistment year, and school program, and column 5 includes all of the controls of columns 2-4. **p-value<0.05, *p-value<0.10.*

Table 4. The effect of age and schooling on cognitive skills.

	Crystallized Intelligence		Fluid Intelligence	
	Synonyms	Technical comprehension	Spatial	Logic
	(1)	(2)	(3)	(4)
School days / 100	0.112**	0.078**	-0.011	-0.022
	(0.023)	(0.025)	(0.029)	(0.024)
Age in days / 100	-0.011	0.008	0.025*	0.048**
	(0.011)	(0.012)	(0.014)	(0.011)

*Notes: N=128,617 in all columns. Age is measured as of the test date and is calculated by summing up t he number of school days and nonschool days. All specifications include conditioning variables for birthdate, parish, and expected graduation as described in the text, as well as controls for father's log earnings, mother's and father's age and age squared, and dummies for family size, mother's and father's years of education, and math and Swedish grades. When a covariate has a missing value for an observation (or is zero for earnings), we assign the mean value to the covariate and assign the value of one to a dummy variable which indicates whether the covariate is missing. **p-value<0.05, *p-value<0.10.*

Table 5. The consequences of erroneously failing to condition on parish, birthdate, and expected graduation variables.

	Crystallized Intelligence		Fluid Intelligence	
	Synonyms	Technical comprehension	Spatial	Logic
	(1)	(2)	(3)	(4)
A. Excluding all conditioning variables				
School days / 100	0.059**	0.015	-0.033	-0.028*
	(0.016)	(0.019)	(0.012)	(0.017)
Age in days / 100	0.003	0.027**	0.034**	0.029**
	(0.008)	(0.009)	(0.010)	(0.008)
B. Difference compared to Table 4 using a Hausman test				
School days / 100	0.053*	0.063**	0.022	0.006
	(0.028)	(0.031)	(.031)	(.029)
Age in days / 100	0.014	0.019	0.009	-0.019
	(0.014)	(0.015)	(0.017)	(0.014)

*Notes: N=128,617 in all columns. The regressions in panel A use the same specification as Table 4 except they exclude the parish, birthdate, and expected graduation variables. In panel B, standard errors based on Hausman (1978) are reported under the null hypothesis that both estimators are consistent, but the estimator excluding the conditioning variables is more efficient; under the alternative, the estimator excluding the conditioning variables is inconsistent. **p-value<0.05, *p-value<0.10.*

Table 6. Robustness checks.

	Crystallized Intelligence (synonyms + tech. comp.) (1)	Fluid Intelligence (spatial + logic) (2)
A. Baseline		
School days / 100	0.111**	-0.019
	(0.023)	(0.025)
Age in days / 100	-0.002	0.040**
	(0.011)	(0.012)
B. No control variables (besides conditioning variables)		
School days / 100	0.110**	-0.020
	(0.023)	(0.026)
Age in days / 100	-0.002	0.040**
	(0.011)	(0.012)
C. Condition on 365 birth day dummies		
School days / 100	0.117**	-0.017
	(0.023)	(0.025)
Age in days / 100	-0.004	0.039**
	(0.011)	(0.012)
D. Condition on municipality dummies		
School days / 100	0.097**	-0.026
	(0.022)	(0.025)
Age in days / 100	0.004	0.043**
	(0.010)	(0.012)
E. Omit expected graduation conditioning variables		
School days / 100	0.103**	-0.010
	(0.024)	(0.027)
Age in days / 100	0.003	0.039**
	(0.011)	(0.012)
F. Limit sample to two enlistment offices with high efficiency		
School days / 100	0.128**	-0.036
	(0.049)	(0.055)
Age in days / 100	-0.001	0.063**
	(0.023)	(0.025)
G. Limit sample to enlistees processed within 6 months of birthday		
School days / 100	0.115**	-0.033
	(0.024)	(0.027)
Age in days / 100	-0.001	0.050**
	(0.011)	(0.013)

*Notes: N=128,617 in panels A-E, 36,587 in panel F, and 113,621 in panel G. In panel F, high efficiency defined as an office which processes over 95% of enlistees during their 18th year. See notes to Table 4. **p-value<0.05, *p-value<0.10.*

Table 7. Heterogeneity by background characteristics.

	Crystallized Intelligence (synonyms + tech. comp.) (1)		Fluid Intelligence (spatial + logic) (2)	
	coeff.	**s.e.**	**coeff.**	**s.e.**
A. Math grades				
Low grades× school days	0.197**	(0.050)	-0.001	(0.056)
High grades× school days	0.190**	(0.046)	-.074	(0.052)
Low grades× age	-0.047**	(0.023)	0.036	(0.026)
High grades× age	-0.036*	(0.022)	0.071**	(0.024)
N	48,669		48,669	
B. Swedish grades				
Low grades * school days	0.199**	(0.050)	0.004	(0.056)
High grades× school days	0.188**	(0.046)	-0.078	(0.052)
Low grades× age	-0.034	(0.023)	0.039	(0.026)
High grades× age	-0.045	(0.021)	0.069**	(0.024)
N	48,669		48,669	
C. Mother's education				
Low education× school days	0.119**	(0.028)	0.008	(0.031)
High education× school days	0.116**	(0.029)	-0.040	(0.032)
Low education× age	-0.003	(0.013)	0.029**	(0.015)
High education× age	-0.007	(0.013)	0.049**	(0.015)
N	121,673		121,673	
D. Father's education				
Low education × school days	0.086**	(0.032)	0.008	(0.035)
High education × school days	0.130**	(0.028)	-0.001	(0.031)
Low education × age	0.014	(0.015)	0.033**	(0.017)
High education × age	-0.015	(0.013)	0.030**	(0.015)
N	113,152		113,152	
E. Father's earnings				
Low earnings× school days	0.103**	(0.028)	-0.014	(0.031)
High earnings× school days	0.120**	(0.027)	-0.023	(0.031)
Low earnings× age	0.002	(0.013)	0.040**	(0.015)
High earnings× age	-0.006	(0.013)	0.040**	(0.014)
N	128,617		128,617	

*Notes: See notes to Table 4. Grades are only available for the birth cohorts 1972-1976. **p-value<0.05, *p-value<0.10.*

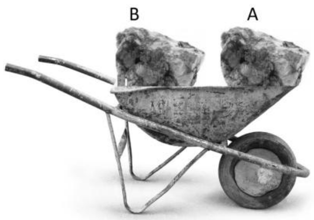
A. Synonyms

HOPFOGNING SKRÄP VÅGKAM GRENUTTAG

Ett av de ord som står här ovanför betyder ungefär samma sak som BRÅTE. Klicka i rutan vid det ordet.

Translation: *One of the words above is a synonym for BRÅTE. Select the circle below that word.*

B. Technical Comprehension

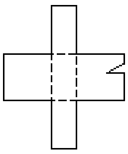


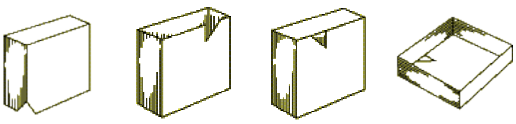
A B C

På vilket stätt är det lättast att köra stenblocket I skottkärran, A eller B? Om det är lika lätt, sätt ett streck under C.

Translation: *Which position for the stone block makes it easiest to push the wheelbarrow, A or B? If equally easy, select C.*

C. Spatial





Här ser du en utvikt papperfigur. Den streckade linjen visar hur den ska vikas. Din uppgift är att tänka ut vilken av de fyra bilderna här ovan som är en bild av samma pappersfigur, fast hopvikt. Klicka i rutan under den bild som visar pappersfiguren hopvikt.

Translation: *On top is an unfolded paper figure. The dashed lines show how it should be folded. Your task is to figure out which of the four pictures is a picture of the same paper figure on top, but folded. Choose the box under the picture that shows the correct folded figure.*

D. Logic

Om summan av antalet ord i denna mening är större än antalet bokstäver i det fjärde ordet i meningen, markera då rutan med nej. Markera i annat fall tredje rutan.

blå nej röd ja

Translation: *If the sum of the number of words in this sentence is greater than the number of letters in the fourth word in the sentence, select the circle which says no ("nej"). Otherwise, select the third circle.*

Figure A1. Sample test questions.

Note: Taken from http://rekryteringsmyndigheten.se/trmPublic/IProvet/inskrivningsprovet.htm.