

## THE EFFECT OF SCHOOLING ON COGNITIVE SKILLS

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*Abstract*—To identify the causal effect of schooling on cognitive skills, we exploit conditionally random variation in the date Swedish males take a battery of cognitive tests in preparation for military service. We find an extra ten days of school instruction raises scores on crystallized intelligence tests (synonyms and technical comprehension tests) by approximately 1% of a standard deviation, whereas extra nonschool days have almost no effect. 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.

### I. 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 1 standard deviation increase in cognitive test scores is associated with an average 10% to 20% increase in wages in recent studies.<sup>1</sup> 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.<sup>2</sup>

While scores on cognitive 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 birth cohort.

To identify a causal effect, this paper exploits conditionally random variation in the assigned test date for a battery of cognitive tests that almost all 18-year-old men 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 used to assign test date). This quasi-experimental setting allows for estimation of the effect of schooling and age on cognitive test scores, without the need for an instrument. The quasi-random timing of enlistment generates substantial variation: the standard deviation in age and school days as of the test date are 108 days and 51 days, respectively, for individuals currently enrolled in the twelfth grade.

As a test of conditional randomness, we estimate 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. In particular, we document that birthdate is correlated with a variety of outcomes that are predictive of cognitive test scores.<sup>3</sup>

Our first finding is that cognitive skills are still malleable when individuals are 18 years old. This is true for both crystallized intelligence tests (synonyms and technical comprehension tests) and fluid intelligence tests (spatial and logic tests), two categories of tests that psychologists commonly use. We describe them in more detail below.<sup>4</sup> These cognitive tests are similar to those used by the U.S. military, some potential employers, and college entrance exams. Our results imply these test scores should not be compared across individuals of different ages when they take the tests.

The main set of results concerns the effect of extra days spent in school. We find that ten more days of school instruction raises cognitive scores by 1.1% of a standard deviation on the synonyms test and 0.8% on the technical comprehension test. Extra nonschool days have virtually no effect on these two crystallized intelligence tests. To put the estimates in perspective, a linear extrapolation implies that an additional 180 days of schooling (one more year of schooling) results in crystallized test scores that 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.

<sup>3</sup>Other research documenting the nonrandomness of birthdate includes Buckles and Hungerman (2013), Bound and Jaeger (2000), Dobkin and Ferreira (2010), and Cascio and Lewis (2006).

<sup>4</sup>The commonly used Wechsler Adult Intelligence Scale has both a fluid intelligence portion (named performance IQ) and a crystallized intelligence portion (named verbal IQ).

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<sup>1</sup> See table 2 in Hanushek and Rivkin (2012) and Hanushek et al. (2013). Earlier studies found smaller effects on the order of 7% (Bowles, Gintis, & Osborne, 2001).

<sup>2</sup> For a sampling of papers, see Altonji and Pierret (2001), Bishop (1991), Blackburn and Neumark (1993), Blau and Kahn (2005), Cawley, Heckman, and Vytlačil (2001), Heckman, Stixrud, and Urzua (2006), Murnane, Willett, and Levy (1995), Neal and Johnson (1996), Taber (2001), and Lindqvist and Vestman (2011).

The baseline estimates 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, the coefficient on school days falls by half for the synonyms test and to almost 0 for the technical comprehension test. There is also no evidence of nonlinear effects in school days or age within the range of our data. Finally, the benefit of additional school days is homogeneous for a variety of predetermined characteristics that are strongly correlated with cognitive ability. We find similar effects based on low versus high past school grades, parental education, and father's earnings.

Taken together, our findings have several important implications. They provide insight into 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. Our findings indicate that schooling has sizable effects on cognitive ability in late adolescence, and not just at very young ages as the prior economics literature has emphasized. The magnitude of the effects is sizable, implying that between 25% and 50% of the return to an extra year of school in wage regressions can be attributed to the increase in cognitive ability. From a policy perspective, our results suggest that increasing the length of the school year could be an effective approach to improving students' cognitive performance.

Our study is related to a growing literature that estimates the links between schooling and test scores. An older literature in psychology uses the fact that admission to elementary school is determined by date of birth relative to a cutoff date to compare test scores of similarly aged children with different levels of schooling.<sup>5</sup> Recognizing that school starting age could affect cognitive skills through a variety of channels, newer research by economists uses cutoffs as instrumental variables to identify the effect of age at school entry on cognitive skills.<sup>6</sup> Several of these papers suggest that cutoffs may not be valid instruments for education levels and that failure to account for both age at test and age at school start may lead to biases.<sup>7</sup>

Studies using school cutoff dates find a range of estimates, from small negative effects of starting school older (Black, Devereux, & Salvanes, 2011), to modest effects of an additional year of high school (Cascio & Lewis, 2006), to large positive effects of school exposure for 5-year-olds

(Cornelissen, Dustman, & Trentini, 2013). Our estimates, when comparably scaled, are two-thirds the size of what Cascio and Lewis find for minorities (they find no effect for whites) and roughly one-fourth to one-third of what Cornelissen et al. find for young children. Our findings could diverge from these other studies because school starting age is not the same as days of school instruction, because cognitive gains and variation in schooling at the beginning of life are different from that at age 18, or because the one-third of students choosing the academic track in Sweden are a more select sample of high school students. In addition, the types of tests taken differ across studies.

Another literature looks at the length of the school year, using differences in mandated instructional time or school closure days due to weather shocks.<sup>8</sup> Studies using differences in mandated instruction time find estimates that vary from no effects to ones that are larger than those in this paper. Papers taking advantage of school closures due to snowfall find even larger effects, which are ten times bigger than ours.<sup>9</sup> Yet another literature uses structural modeling and concludes that while cognitive skills are malleable for young children, they become less so as children age.<sup>10</sup>

Our paper clarifies and adds to this prior literature in several important ways. First, our study identifies the effect of schooling on cognitive skills holding age, grade level, season of birth, and cohort constant. This means we can separate out the effect of schooling from these other factors.<sup>11</sup> Second, our research design provides a quasi-experimental setting, which does not require an instrument. Third, our study looks at 18-year-old adolescents, while most of the literature focuses on effects for young children. Fourth, the variation in instructional days we use for identification occurs during the course of a normal school year. Finally, our paper distinguishes between crystallized and fluid intelligence tests, a split that matters empirically and has important implications for future research.

The remainder of the paper proceeds as follows. Section II discusses the cognitive production function and our identification strategy. Section III describes our setting and the data. Section IV tests for the conditional randomness of test dates, while section V presents our main results, robustness checks, and heterogeneity results. Section VI discusses the importance of our findings, and section VII concludes.

<sup>5</sup> For example, Baltes and Reinert (1969), Cahen and Davis (1987), and Cahan and Cohen (1989).

<sup>6</sup> See Bedard and Dhuey (2006, 2007), Black et al. (2011), Cascio and Lewis (2006), Cascio and Schanzenbach (2007), Cornelissen et al. (2013), Crawford, Dearden, and Meghir (2010), Datar (2006), Fertig and Kluge (2005), Fredriksson and Öckert (2005), Gormley and Gayer (2005), Leuven et al. (2010), McEwan and Shapiro (2008), Neal and Johnson (1996), and Puhani and Weber (2007). Angrist and Krueger (1991, 1992) and McCrary and Royer (2011) use school entry cutoffs in other settings.

<sup>7</sup> In related work, Elder and Lubotsky (2009) document that entrance start dates are correlated with a child's school performance and grade repetition, while Dobkin and Ferreira (2010) find that students starting school at a younger age are less likely to drop out of high school in the United States.

<sup>8</sup> Some examples from this literature include Dobbie and Fryer (2013), Eide and Showalter (1998), Fryer (2012), Hansen (2011), Krashinsky (2014), Lavy (2010), Marcotte (2007), Marcotte and Hemelt (2008), and Sims (2008).

<sup>9</sup> As Hansen (2011) recognizes, these relatively large effects may partly be because snow days represent unplanned, disruptive days to the school year calendar. See Goodman (2014) for a recent reinterpretation of the snowfall literature.

<sup>10</sup> See Cunha and Heckman (2007, 2008), Cunha et al. (2006), Cunha, Heckman, and Schennach (2010), and Hansen, Heckman, and Mullen (2004).

<sup>11</sup> Among the studies listed in note 6, Black et al. (2011), Cascio and Lewis (2006), Crawford et al. (2010), and Cornelissen et al. (2013) are able to separate out age at entry from age at test effects. Cascio and Schanzenbach (2007) are able to separate out relative age from age at entry.

## II. Identifying Schooling's Effect on Cognitive Skills

### A. Production Function for Cognitive Skills

Cognitive skill formation could depend on a variety of factors, including the 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

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

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 he turns 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.

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

$$y_{it} = \gamma_0 + \gamma_1 S_{it} + \gamma_2 A_{it} + \delta X_{it} + g(B_i) + \sum_j \theta_j (P_i = j) + \pi G_i + e_{it}, \quad (2)$$

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

The first concern for consistent estimation of  $\gamma_1$  is reverse causation, as it is likely that completed schooling is a function of cognitive ability. This is problematic for data sets 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 data sets, 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 for individuals with the same 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 arises because both school days and age are functions of birthdate in observational data.<sup>12</sup> As others have pointed out and as we verify with our data set, cognitive ability varies by date of birth (by both birth day and birth cohort effects). This means the omission of birthdate controls, or any variables related to test date, will cause the estimate of  $\gamma_1$  to be biased.

<sup>12</sup> 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.

### B. 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.

To begin, first consider the case where individuals are randomly assigned a test date. We 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 random only 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:

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

In our setting, the assignment of test date is random only after conditioning on additional covariates. As we explain in more detail in the next section, 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 this limited information to assign a test date close to an individual's eighteenth birthday, taking into consideration transportation and other logistical issues. This assignment process creates conditionally random variation in test-taking dates. Schooling and age are now independent of the error term after conditioning on date of birth, parish, and expected graduation:

$$\begin{aligned} &\text{Random assignment of } t | B_i, P_i, G_i \\ &\Rightarrow (S_{it}, A_{it}) | B_i, P_i, G_i \perp e_{it}. \end{aligned} \quad (4)$$

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 of school and 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. Due to the conditionally random assignment of test dates, it does not matter whether other predetermined 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 that 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 that 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.<sup>13</sup>

### III. 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 that 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 ninth grade. These variables will be used to test for random assignment and explore whether there are heterogeneous effects.

#### A. Logistics of the Enlistment Procedure

All men in Sweden, with a few exceptions, were required to show up at a military enlistment center on an assigned date around their eighteenth birthday during our sample period.<sup>14</sup> The enlistment process took two days 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 on entry into military service.<sup>15</sup> Enlisted males generally began their military service, which lasted eleven months on average, after finishing 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 eighteenth birthday 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 comply with the assigned date of enlistment; failure to show up resulted in fines and eventual imprisonment.<sup>16</sup>

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.<sup>17</sup> Some enlistment offices also used information on expected graduation date. The variation in enlistment dates around an individual's eighteenth birthday is a result of logistical constraints that the military faced. The goal was to have all individuals enlist close to their eighteenth 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 were closed over Christmas break and for two months in the summer. The military had six regional offices, each with responsibility for a defined geographical region of Sweden. When planning the enlistment dates for the coming year, each office was given a list from the local parish of all men turning 18 during the upcoming year, along with their addresses. The regional offices tried to assign enlistment dates close to an individual's eighteenth birthday, but in a way that also satisfied the logistical constraints involved with travel, being able to process a limited number of individuals each day, and enlistment office closure periods.

<sup>15</sup> The cognitive tests were likely to be perceived as relatively low stakes, although enlistees could try to do well (or poorly) in an attempt to influence their military assignment. The tests were administered with strict protocols: tests could not leave the monitored room and answers were never provided to anyone. Because of the lack of strong incentives and the difficulty in passing on information, we do not believe students taking the test later in the year gained an advantage by talking to others who had already taken the test.

<sup>16</sup> Assigned enlistment dates were strictly enforced. For example, if an enlistee was sick on his assigned day, he still had to show up at the enlistment office unless he had a doctor's note. Enlistment orders with an assigned date were sent out to each individual as a certified letter, which he had to pick up at the local post office.

<sup>17</sup> The enlistment procedure was established in a law passed in 1969. The legal statute tasked county tax authorities with gathering information on all Swedish males turning 17 years old each year and forwarding 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 men in their jurisdiction.

<sup>13</sup> 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 assumptions could be relaxed.

<sup>14</sup> Exceptions included individuals exempted from military service (those with severe disabilities, currently in prison, or institutionalized, or who are noncitizens) and those who live abroad (and can therefore postpone their enlistment date until they return to Sweden). About 3% of individuals are exempted from military service. Sweden ended compulsory military service in 2010.

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 also 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 start of military service. In Sweden during our time period, individuals in the academic track in upper secondary school 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. Enlistment offices with enough capacity processed virtually the entire list of candidates they received from the tax authorities in the same calendar year.

Enlistment offices with more severe capacity constraints, however, prioritized individuals who were in their last year of school. 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: 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. We therefore use the individual's self-reported school program at the time of enlistment as our measure of expected graduation.<sup>18</sup> 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.<sup>19</sup>

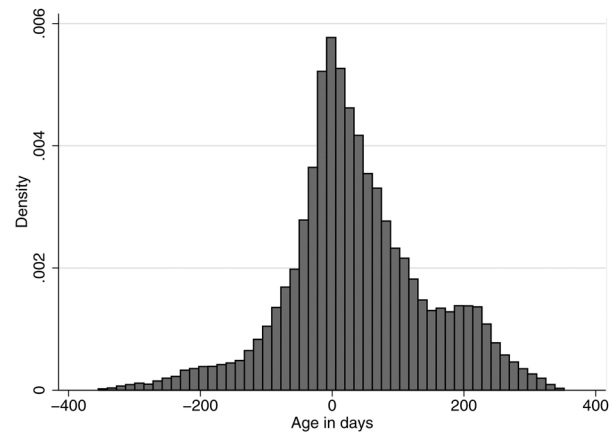
Figure 1 plots the distribution of the total number of days between an individual's enlistment date and birthdate. In the figure, we normalize the distribution of age at test date to be relative to age 18 (i.e., we subtract 18 years). While most individuals enlist within six months of their birthdate, 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.<sup>20</sup>

<sup>18</sup> There are five academic school programs: business, humanities, social sciences, natural sciences, and technical studies. The technical studies program constitutes 41% of our sample. Individuals also graduate a year later if they study abroad or repeat a grade, but these cases are relatively rare.

<sup>19</sup> We can, however, identify two of the six enlistment offices that empirically were never capacity constrained and processed almost all candidates when they were 18 regardless of expected date of graduation. As we report later, the estimates for these two offices are remarkably similar to our baseline results, although the standard errors are larger.

<sup>20</sup> The distribution of test taking in our estimation sample is: January (9.8%), February (9.9%), March (11.9%), April (9.7%), May (9.2%), June

FIGURE 1.—DISTRIBUTION OF AGE AT TEST DATE



The figure plots the distribution of the total number of days between an individual's enlistment date (when he takes the tests) and his birthdate. Age at test date is normalized to be relative to age 18.

For our approach to work, it is important that we condition our estimates on the same set of variables as the enlistment offices. 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 information about expected graduation date.<sup>21</sup> In the next section, we provide empirical evidence that assignment date appears to be random after conditioning on birthdate, parish, and expected graduation.

### B. Cognitive Tests

Cognitive skills are measured during the enlistment procedure using the Enlistment Battery 80. The tests are similar in style to the Armed Services Vocational Aptitude Battery (ASVAB) in the United States. There are separate paper-and-pencil tests for synonyms, technical comprehension, spatial ability, and logic. Each of these four tests consists of forty items presented in increasing order of difficulty and is slightly speeded (see Carlstedt & 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 has illustrated and written technical problems, with a choice of three alternative answers. It has

(1.5%), July (0.0%), August (4.6%), September (11.9%), October (12.8%), November (11.7%), and December (7.1%).

<sup>21</sup> We verified this information with Berit Carlstedt, formerly employed at the National Defense College (February 14, 2012), Bengt Forssten at the Swedish Defense Recruitment Agency (October 11, 2011), Ingvar Ahlstrand at the Swedish Defense Recruitment Agency (October 11, 2011), and Rose-Marie Lindgren, chief psychologist at the Swedish Defense Recruitment Agency (March 16, 2012). Information about the preliminary letter requesting expected graduation date was obtained from Ove Selberg at the Swedish Defense Recruitment Agency (June 20, 2012).

some similarities to the mechanical comprehension portion of the ASVAB. The test which measures spatial ability is referred to as a *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 is presented and a related question must be answered using deductive logic. Example test questions are found in appendix figure A.1 in the online supplement.

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 intelligence as discrete factors of general intelligence. Crystallized intelligence measures the ability to use acquired knowledge and skills and therefore is closely tied to intellectual achievement. Fluid intelligence captures 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 that 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 has both a fluid intelligence portion (named performance IQ) and a crystallized intelligence portion (named verbal IQ).

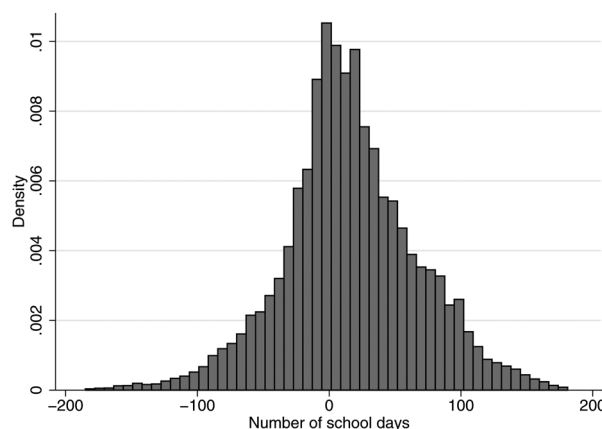
### C. School Days and Nonschool Days

The Swedish school system consists of compulsory primary school (from the ages 7 to 16) as well as an optional secondary school (from ages 16 to 19). Generally everyone born in the same calendar year starts primary school together in August the year they turn 7, so that those born in January are the oldest within each schooling cohort.<sup>22</sup> Secondary school splits into two tracks: a two-year program consisting of vocational training and a three- or four-year academic program that prepares students for university studies.

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 United States 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

<sup>22</sup> A small number of individuals start school earlier or are held back a year. According to Fredriksson and Öckert (2005), 3% of all children born from 1975 to 1983 started school earlier or later than intended. We cannot identify these cases in our sample, and hence, a small number of individuals will be included in the analysis who have already left school by age 18.

FIGURE 2.—DISTRIBUTION OF NUMBER OF SCHOOL DAYS



The figure plots the distribution of the total number of school days between an individual's enlistment date (when he takes the tests) and his birthdate. Similar to figure 1, number of school days as of the test date is normalized to be on the same scale as age at test date (that is, relative to age 18).

1980 to 1994, we are able to calculate the exact number of school days and nonschool days between the day of enlistment and the eighteenth 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 weeklong 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 eighteenth 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 that the military used 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.

### D. Sample Restrictions

We make several sample restrictions to be able to cleanly estimate the effect of schooling on cognitive skills. While focusing on this sample may limit the generalizability of our findings, it should not affect the internal validity of our estimates, since the restrictions are based on variables observed before enlistment dates are known or variables which are likely to be unaffected by enlistment dates.

TABLE 1.—MEAN COGNITIVE TEST SCORES BY BACKGROUND CHARACTERISTICS

	Crystallized Intelligence		Fluid Intelligence		N (5)
	Synonyms (1)	Technical Comprehension (2)	Spatial (3)	Logic (4)	
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					
Less than 12 years	0.74	0.22	0.76	0.80	63,912
12 years or more	0.91	0.40	0.94	0.94	57,761
D. Father's education					
Less than 12 years	0.72	0.22	0.76	0.80	45,776
12 years or more	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

Each of the cognitive tests is normalized to be mean 0 and standard deviation 1 for the entire population of test takers. Our sample comprises 18-year-old students enrolled in the academic track of high school, a group with higher average test scores (see appendix table A.1). Grades in math and Swedish are for ninth grade, range from 1 to 5, and follow a discretized normal distribution for the entire sample of ninth graders (including students who do not go on to the academic track). Grades are available only 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.

Separating school days from nonschool days requires that individuals be enrolled in school the year they are tested. Since enlistment usually occurs in the months around an individual's eighteenth birthday, we limit our sample to young men who are expected to be enrolled in the twelfth grade, that is, those in three- or four-year academic programs.<sup>23</sup> This means we cannot study the effect of extra school days for individuals who stop after compulsory schooling in ninth grade or enroll in two-year vocational training, since most of these individuals will already have completed school prior to enlistment.<sup>24</sup> As documented in appendix table A.1, the sample of students in academic programs have better grades, better-educated and wealthier parents, and substantially higher test scores. This means our results are unlikely to be externally valid for the broader sample of individuals. They also will not necessarily apply to women, as only men are required to enlist.

Next, we exclude nonnatives, defined as those who were born abroad or have at least one parent born abroad. This is because only citizens are required to enlist, and less than 50% of nonnatives are Swedish citizens. These cases constitute 15% of the population. We also restrict the sample to individuals turning 18 during the year they enlist.<sup>25</sup> 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 data set. 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 took a new and different battery of cognitive tests. After these restrictions, we are left with a sample of 128,617 observations.

#### IV. Conditional Randomness of Test Dates

If age at test date and number of school days are conditionally random, both should be unrelated to background characteristics after flexibly accounting for the conditioning variables of date of birth, parish, and expected graduation date. 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 data set, we have several variables that are highly predictive of cognitive test scores: math and Swedish grades in ninth grade, mother's and father's education, and father's income. The relationship between these variables and cognitive scores is presented in table 1.

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 fewer than twelve years of schooling score 0.15 standard deviation lower on the technical comprehension test. All of the differences by background characteristics in table 1 are statistically different from each other at the 1% level.

<sup>23</sup> Categorization is based on self-reports at the time of enlistment. While these self-reports are recorded after the enlistment date is known, an individual's schooling choices are unlikely to be affected by enlistment date. After limiting the sample to those enrolled in a three- or four-year program based on self-reports, we then discard individuals with fewer than twelve years of completed education. The second step eliminates an additional 2% of observations.

<sup>24</sup> In our data, 15% of young men stop after finishing compulsory primary school in ninth grade, 51% study in a two-year vocational program, and 34% study in a three- or four-year academic program.

<sup>25</sup> Roughly 16% of the population does 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 IIIA.

TABLE 2.—REGRESSION TESTS FOR CONDITIONAL RANDOMNESS

	Dependent Variable	
	Age in Days (1)	Number of School Days (2)
High math grades	-0.136 (0.875)	-0.855 (0.897)
High Swedish grades	-1.256 (0.845)	-1.011 (0.866)
Highly educated mother	0.492 (0.535)	0.781 (0.548)
Highly educated father	0.480 (0.589)	0.584 (0.603)
High father's earnings	0.582 (0.536)	0.496 (0.549)
Math grades missing	1.333 (4.041)	3.020 (4.140)
Swedish grades missing	1.301 (4.944)	0.677 (5.065)
Mother's education missing	0.785 (1.089)	0.536 (1.116)
Father's education missing	0.298 (0.775)	0.270 (0.794)
<i>F</i> -test ( <i>p</i> -value)	0.92 (0.502)	1.23 (0.274)

*N* = 128,617. High math and Swedish grades defined by a grade of 4 or 5 (on a scale of 1 to 5), highly educated mother or father defined by twelve or more years of education, and high father's earnings defined by earnings above the median. See notes to table 1. The regressions include the conditioning variables of parish dummies, birth year dummies, dummies for each birthday week within a year, and the complete interaction of enlistment office, enlistment year, and school program. \**p*-value < 0.05, \*\**p*-value < 0.10.

Since each of the background variables is 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 the set of background characteristics, including the variables the military used to assign test dates as additional covariates. For birthdate, we include 52 birth week dummies and 13 birth cohort dummies. We also include roughly 2,500 parish dummies, which is the level of geographic detail the military used to organize enlistment dates.<sup>26</sup> 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.

Table 2 shows that whether one uses age or school days as the dependent variable, the estimated coefficients on the background characteristics are small and statistically insignificant. The coefficients are also not jointly significant in either regression. These regressions provide empirical support for the claim that both age and school days are conditionally random. In contrast, if the conditioning variables of birthdate, parish, and expected graduation are excluded from the regressions, many of the coefficients are statistically significant.<sup>27</sup>

<sup>26</sup> Parish boundaries change over time, so there are closer to 1,500 parishes at any one time. We assign a unique dummy each time a parish's boundary changes.

<sup>27</sup> We ran similar regressions to those in table 2, but without the conditioning variables. For the age regression, six of nine coefficient estimates are statistically significant at the 10% level, and the joint *F*-test is 28.7

To better understand why failure to control for the conditioning variables of birthdate, parish, and expected graduation could cause a bias, appendix figures A.2 and A.3 show how background characteristics vary by season of birth. The first appendix figure shows that more educated and higher-earning parents are more likely to have children in March and April and less likely to have children near the end of the year. Since the cutoff date for school entry in Sweden is January 1, students born at the end of the year will be the youngest in their class, which some researchers have argued hurts a child's academic and social development. The second appendix figure reveals that average math and Swedish grades in ninth grade are highest for individuals born near the beginning of the year (and who are therefore the oldest in their class) and decline almost monotonically throughout the year. While the patterns are striking, we cannot say whether they are due to relative age within a classroom or differences in parental characteristics by season of birth.

The birthdate patterns we document are important for more than just this study. As researchers have argued in different contexts (see note 3), the patterns suggest due caution in using season of birth as an instrumental variable.

## V. Results

### A. Malleability of Cognitive Skills

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 compelling evidence that cognitive skills are malleable.

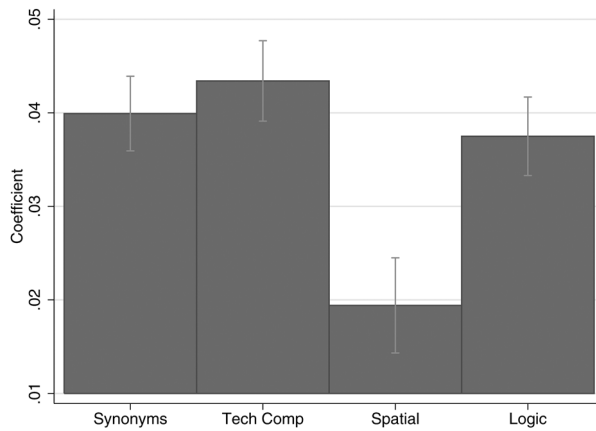
The consensus in psychology is that crystallized intelligence grows over time and does not start to decline until very late in life. In contrast, the current consensus for fluid intelligence is that it grows rapidly during childhood, peaks in early adolescence, and eventually declines in old age. There is some uncertainty about when exactly the peak happens for fluid intelligence, but it appears to be sometime around age 20 (see Tucker-Drob, 2009; and Salthouse, Pink, & Tucker-Drob, 2008).

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 0 and a standard deviation equal to 1 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 the age at test, which by construction equals enlistment date minus birthdate.

(*p*-value = .001). For the school days regression, seven of nine coefficients are statistically significant, and the joint *F*-test is 30.0 (*p*-value = .001).



FIGURE 3.—INCREASE IN COGNITIVE TEST SCORES FOR A 100-DAY INCREASE IN AGE



Coefficient estimates from separate regressions of the four cognitive test scores on age at test date, with 95% confidence intervals. Regressions do not include a variable for the number of school days but otherwise are similar to the specification used in table 3.

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 table 2. We also include several predetermined variables in the regressions, including controls for family size, parental education, parental age, father's earnings, grades in math and Swedish in ninth grade, and field of study in high school. These additional variables do not appreciably change the estimates, although they do decrease the standard errors by around 10%.

Figure 3 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% of a standard deviation better on the synonyms, technical comprehension, and logic tests. The estimate is half as large for spatial ability, which is a fluid intelligence test.<sup>28</sup>

The results in figure 3 apply to our main sample of enlistees enrolled in the academic track (i.e., the college preparatory track, which takes three or four years). This sample makes up roughly one-third of enlistees. The other enlistees are men who drop out in ninth grade after compulsory schooling ends or enroll in two-year vocational programs. While these samples do not have the necessary variation to estimate a school days effect, they can be used to estimate general aging effects. Results for these alternative samples are found in appendix table A.2. We find statistically significant aging effects on all four of our cognitive tests for both the ninth-grade

<sup>28</sup> Enlistees also take a psychological profile assessment. Older enlistees do significantly better, which could be one factor for the age-at-test increase in cognitive scores. It is difficult to interpret this psychological assessment, as the military does not release details on its content.

TABLE 3.—EFFECT OF AGE AND SCHOOLING ON COGNITIVE SKILLS

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

$N = 128,617$  in all columns. Each of the cognitive tests is normalized to be mean 0 and standard deviation 1 for the entire population of test takers. Age is measured as of the test date and is calculated by summing up the 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 0 for earnings), we assign the mean value to the covariate and assign the value of 1 to a dummy variable that indicates whether the covariate is missing. \*\* $p$ -value  $< 0.05$ , \* $p$ -value  $< 0.10$ .

compulsory and vocational samples. While the estimates are somewhat smaller than those found for the baseline academic track sample, they are difficult to compare directly. The reason is that in these other samples, the general aging estimate captures the effect of a mixture of workdays and nonwork days.<sup>29</sup>

### B. Main Results

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 3 presents our baseline results. We use the same empirical specification as we did in the previous section, but add another 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 (and hence the effect of a nonschool day), while the coefficient on the school days variable captures the extra effect when one more of these days is spent in school. The school days coefficient is directly relevant for policy, as it captures the effect of replacing a nonschool day with a school day, holding age constant.<sup>30</sup>

For the crystallized intelligence tests, we find that an extra 10 days of school instruction raises cognitive scores for synonym and technical comprehension tests by 1.1% and 0.8% of a standard deviation, respectively. To put these estimates in perspective, they imply that an additional year of schooling (180 days in Sweden) results in test scores that are 21% of

<sup>29</sup> A day spent at work, particularly when an individual is young and receiving on-the-job training, could involve substantive learning and thereby increase both fluid and crystallized intelligence. While it would be interesting to test the effect of a work day versus a nonwork day, there is no way of separating out the two in our setting.

<sup>30</sup> To see this, consider the regression  $Y = \gamma_0 + \gamma_1 S + \gamma_2 A + e$ , where  $A = S + NS$  and  $S$  and  $NS$  represent school and nonschool days, respectively. Rewriting the production function in terms of the  $S$  and  $NS$  inputs,  $Y = \beta_0 + \beta_1 S + \beta_2 NS + e = \beta_0 + \beta_1 S + \beta_2 (A - S) + e = \beta_0 + (\beta_1 - \beta_2) S + \beta_2 A + e$ . This makes clear that  $\gamma_1 = \beta_1 - \beta_2$  is a relative coefficient and that  $\gamma_2 = \beta_2$ .

a standard deviation higher for synonyms and 14% for technical comprehension. This is the effect above and beyond any general aging effect, which is small and statistically insignificant for these tests.

The two tests that 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 ten days older perform between 0.3% and 0.5% 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 0. 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.

To help benchmark the size of our estimates, consider three recent studies on cognitive skills using Swedish data. Fredriksson, Öckert, and Oosterbeak (2013) find that increasing class size by 1 when a student is between the ages of 10 and 13 reduces test scores by 3% of a standard deviation at age 13, but has no significant effect by age 18. Lundborg, Nilsson, and Rooth (2014) use a compulsory school reform and find that one additional year of schooling for the mother raises the cognitive test score of her son around age 18 by 9% of a standard deviation. Finally, Fredriksson and Öckert (2005) find strong effects of age at school entry around age 7 on cognitive skills at age 13; a one-year increase in age when starting school and when taking the test increases scores between 20% and 30% of a standard deviation.

The contrast between the first two tests (synonyms and technical comprehension) and the second two tests (spatial ability and logic) is particularly interesting when one remembers the distinction between crystallized and fluid intelligence. As discussed in section IIIB, “fluid” refers to intelligence that can be applied to a variety of problems, while “crystallized” refers to intelligence that 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.

These are important findings in the literature, as the prior research in psychology that attempts to separate out schooling from aging on crystallized versus fluid intelligence has estimated correlations rather than causal effects (Cahan & Cohen, 1989; Cliffordson & Gustafsson, 2008; Stelzl et al., 1995). The key advantage of our design is that we use

TABLE 4.—CONSEQUENCES OF ERRONEOUSLY FAILING TO CONDITION ON PARISH, BIRTHDATE, AND EXPECTED GRADUATION VARIABLES

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

*N* = 128,617 in all columns. The regressions in panel A use the same specification as table 3 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.

conditionally random variation. Our findings also suggest that 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 4 we report naive OLS regressions that do not include these conditioning variables. Except for the exclusion of the birthdate, parish, and expected graduation conditioning variables, the analysis in table 4 mirrors that of table 3. The differences in estimates are substantively important and point toward 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 0 (from 0.078 to 0.015).

When a Hausman specification test is used, 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 changes the estimates in ways that lead to incorrect conclusions about the effect of schooling on cognitive skills.

### C. Robustness

Table 5 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 0 and standard deviation 1 for the entire sample of test takers. The first panel in the table presents results similar to table 3, using the two averaged test

TABLE 5.—ROBUSTNESS CHECKS

	Crystallized Intelligence (Synonyms + Technical Comprehension) (1)	Fluid Intelligence (Spatial + Logic) (2)
A. Baseline		
School days/100	0.111** (0.023)	-0.019 (0.025)
Age in days/100	-0.002 (0.011)	0.040** (0.012)
B. No control variables (besides conditioning variables)		
School days/100	0.110** (0.023)	-0.020 (0.026)
Age in days/100	-0.002 (0.011)	0.040** (0.012)
C. Allow for nonlinear effects		
School days/100	0.116** (0.024)	-0.019 (0.027)
(School days/100) <sup>2</sup>	-0.007 (0.017)	0.003 (0.019)
Age in days/100	-0.002 (0.011)	0.042** (0.013)
(Age in days/100) <sup>2</sup>	-0.002 (0.004)	-0.003 (0.004)
D. Condition on 365 birth day dummies		
School days/100	0.117** (0.023)	-0.017 (0.025)
Age in days/100	-0.004 (0.011)	0.039** (0.012)
E. Condition on municipality dummies		
School days/100	0.097** (0.022)	-0.026 (0.025)
Age in days/100	0.004 (0.010)	0.043** (0.012)
F. Omit expected graduation conditioning variables		
School days/100	0.103** (0.024)	-0.010 (0.027)
Age in days/100	0.003 (0.011)	0.039** (0.012)
G. Limit sample to two enlistment offices with high efficiency		
School days/100	0.128** (0.049)	-0.036 (0.055)
Age in days/100	-0.001 (0.023)	0.063** (0.025)
H. Limit sample to enlistees processed within 6 months of birthday		
School days/100	0.115** (0.024)	-0.033 (0.027)
Age in days/100	-0.001 (0.011)	0.050** (0.013)

*N* = 128,617 in panels A–F, 36,587 in panel G, and 113,621 in panel H. See notes to table 3. \*\*\**p*-value < 0.05, \*\**p*-value < 0.10.

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 0 for age, as expected given the more disaggregated results in table 3. 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 predetermined 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  $R^2$  from 0.203 to 0.262 for crystallized intelligence and from 0.184 to 0.237 for fluid intelligence.

Panel C tests whether the effects of schooling and age are nonlinear. We add in school days squared and age squared to the regression and find little evidence for nonlinear effects, at least in the range of our data. The squared terms are small and insignificant, and do not markedly affect the first-order polynomial coefficients.

Panel D includes 365 birth day dummies as conditioning variables instead of 52 birth week dummies and yields estimates similar to baseline. It is important to recognize that less flexible functions of birthdate can change the estimates. While not shown in the table, using 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 birth day linearly drops the coefficient from 0.111 to 0.051.

Panel E uses a more parsimonious set of controls for 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 slightly different estimates.

In the next two panels, we explore our set of proxy variables for expected graduation. Panel F shows the estimates do not change much when omitting these expected graduation proxies as conditioning variables. In panel G, we use a different approach to assess the expected graduation conditioning variables. Two of the six enlistment offices processed over 95% of enlistees during their eighteenth 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 G limits the sample to these two enlistment offices with high efficiency and finds estimates that are similar to the baseline, although the standard errors double due to the smaller sample size.

In the last panel, we limit the sample to enlistees processed within six 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.

#### D. Heterogeneity

From a policy perspective, one of the more interesting questions is whether there are heterogeneous effects. If low-ability individuals experience high cognitive returns from additional days of schooling, then extra school day resources spent on this group could have a high individual and social return. A priori, it is not obvious which type of student benefits most. Higher-ability individuals may absorb new information and new ways of thinking relatively better in the school setting. Alternatively, if individuals have low initial

TABLE 6.—HETEROGENEOUS EFFECTS

	Crystallized Intelligence (Synonyms + Technical Comprehension)		Fluid Intelligence (Spatial + Logic)	
	Coefficient	SE	Coefficient	SE
	(1)		(2)	
<b>A. Math grades</b>				
Low Grades × School Days/100	0.197**	(0.050)	−0.001	(0.056)
High Grades × School Days/100	0.190**	(0.046)	−0.074	(0.052)
Low Grades × Age/100	−0.047**	(0.023)	0.036	(0.026)
High Grades × Age/100	−0.036*	(0.022)	0.071**	(0.024)
<i>N</i>	48,669		48,669	
<b>B. Swedish grades</b>				
Low Grades × School Days/100	0.199**	(0.050)	0.004	(0.056)
High Grades × School Days/100	0.188**	(0.046)	−0.078	(0.052)
Low Grades × Age/100	−0.034	(0.023)	0.039	(0.026)
High Grades × Age/100	−0.045	(0.021)	0.069**	(0.024)
<i>N</i>	48,669		48,669	
<b>C. Mother's education</b>				
Low Education × School Days/100	0.119**	(0.028)	0.008	(0.031)
High Education × School Days/100	0.116**	(0.029)	−0.040	(0.032)
Low Education × Age/100	−0.003	(0.013)	0.029**	(0.015)
High Education × Age/100	−0.007	(0.013)	0.049**	(0.015)
<i>N</i>	121,673		121,673	
<b>D. Father's education</b>				
Low Education × School Days/100	0.086**	(0.032)	0.008	(0.035)
High Education × School Days/100	0.130**	(0.028)	−0.001	(0.031)
Low Education × Age/100	0.014	(0.015)	0.033**	(0.017)
High Education × Age/100	−0.015	(0.013)	0.030**	(0.015)
<i>N</i>	113,152		113,152	
<b>E. Father's earnings</b>				
Low Earnings × School Days/100	0.103**	(0.028)	−0.014	(0.031)
High Earnings × School Days/100	0.120**	(0.027)	−0.023	(0.031)
Low Earnings × Age/100	0.002	(0.013)	0.040**	(0.015)
High Earnings × Age/100	−0.006	(0.013)	0.040**	(0.014)
<i>N</i>	128,617		128,617	

See notes to table 3. Grades are available only for the birth cohorts 1972–1976. \*\**p*-value < 0.05, \**p*-value < 0.10.

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 data set, we do observe a variety of variables that are correlated with cognitive ability: grades in ninth grade, parental education, and father's earnings (see table 1). In table 6, we analyze whether there are heterogeneous returns to schooling based on these predetermined characteristics. The specifications mirror the baseline regressions in panel A of table 5 but 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 in ninth grade. The coefficient on schooling is similar for crystallized intelligence tests whether or not the student received low or high grades in math, even though mean scores are very different by math grades (see table 1). The coefficients for age based on math grades are also not markedly different from each other. A similar pattern holds when one allows for separate coefficients by past Swedish grades. One thing to remember is that we have information on grades for birth cohorts only 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 also find little evidence for differential returns by background characteristics. None of the pairwise comparisons are statistically different from each other at the 10% level.

Another margin to look for heterogeneous effects is by school quality. While it is difficult to measure school quality, three commonly used metrics are the teacher-student ratio, average years of teacher experience, and the share of teachers with a university degree. We create indicator variables for these three school quality measures based on whether a school region is above the median value.<sup>31</sup> In appendix

<sup>31</sup> There were 140 high school regions in Sweden during our sample period. See Fredriksson and Öckert (2008) for more details on these Swedish school quality measures. If we define indicator variables based on whether a school region is below the 25th percentile, between the 25th and 50th percentile, or greater than the 75th percentile, we find similar results.

table A.3, we estimate similar specifications as we did for table 6. Since the regressions include fixed effects for year and parish (which are subsets of school regions), identification comes from within-regional variation in school quality over time. For the crystallized intelligence tests, we find similar returns to extra school days for both high and low values of the school quality measures for a region, suggesting that an additional day of schooling is worth the same regardless of where it occurs. There is also no differential effect by school quality for the fluid intelligence tests. One caveat to keep in mind is that we are looking only at the academic track of high school, and cognitive ability does not vary much by the school quality measures for this sample; in contrast, it does vary substantially among students in the academic versus vocational tracks of high school (see appendix table A.1).

Taken together, these results 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 that 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 will aid a variety of students enrolled in the academic track. Whether these results are externally valid for students in the vocational track or who stop after compulsory schooling ends in ninth grade is an open question.

## VI. Discussion

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 ability is malleable into young adulthood and is 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 United States. 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.

The fact that we find different effects for the crystallized versus fluid intelligence tests is an important distinction. It is tempting to group many types of tests under the “cognitive” skill label and make conclusions as though they are valid for the universe of cognitive skills. Our findings indicate this

would be a mistake. We find synonyms and technical comprehension tests (crystallized intelligence tests) are affected by days of schooling but not nonschool days. In contrast, the fluid intelligence tests measuring spatial and logic skills increase with age, with logic skills increasing at a more rapid rate. One possible explanation for why researchers using mathematics or reading comprehension and reading recognition tests find effects for younger children, but not for older children, may be due to the type of “cognitive” test being administered (see, e.g., Cunha et al., 2010).

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 are affected by extra schooling. While each of the calculations is based on several assumptions and extrapolations, their purpose is to help quantify the role schooling plays in the production of cognitive skills. It is also important to remember that our results apply to young men who enroll in the academic track of high school, so the calculations in this section may not be externally valid for other groups (such as women or vocational students).

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 that a sizable portion of the gap is likely due to the fact that schooling increases human capital.

Our second calculation provides insight into the interpretation of schooling coefficients in standard wage regressions. When we use prior estimates from the literature, a 1 standard deviation increase in cognitive ability is associated with roughly a 10% to 20% increase in wages. Combining this with our estimate of how schooling affects cognitive ability, an extra year of schooling is responsible for between a 2% and 4% increase in wages solely due to an increase in cognitive ability. Stated another way, one-fourth to one-half of the return to an extra year of schooling in wage regressions (which do not control for cognitive skills) could potentially be attributable to the increase in cognitive ability resulting from an extra year of schooling.<sup>32</sup>

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 United States typically suggest an extra twenty days be added to the school year, often with the explicit goal of helping students be more globally

<sup>32</sup> This calculation assumes that the return to a year of schooling is 8%. The calculations in this paragraph are only suggestive, in part because our estimates are identified by some students taking tests earlier in the school year than others, and not from differences in completed education.

competitive.<sup>33</sup> Among OECD countries in 2009, the United States placed fourteenth out of 33 in a reading test administered by the OECD. If the school year was extended by twenty days starting in kindergarten and if our results can be applied cumulatively to other types of education and grade levels (under the assumption that gains in earlier grade levels are at least as large as in twelfth grade) and if our synonyms test can be compared to the OECD reading test, the United States would improve its standing from fourteenth to fourth place in the rankings.<sup>34</sup> While this last calculation requires heroic assumptions, it illustrates the potential effectiveness of increasing the length of the school year and suggests further studies are warranted.

## VII. 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. In this paper, we exploit conditionally random variation in assigned test date to estimate the effect of schooling and age on cognitive test scores.

Our key result is that additional schooling causally increases performance on crystallized intelligence tests. We find that ten more days of school instruction raises cognitive scores on synonyms and technical comprehension tests (crystallized intelligence tests) by approximately 1% of a standard deviation. This is a relatively large effect. It suggests that an additional 180 days of schooling (an additional year of schooling) 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.

As with any study, there are some limitations that are important to keep in mind. First, our study is restricted to 18-year-olds in the academic track in high school, most of whom are planning on continuing on to college. Our setting does not allow us to estimate schooling effects for those who stop after compulsory schooling in ninth grade or who enroll in two-year vocational training programs during high school. It does not allow us to say anything about women either. Second, the analysis assumes additional school days in September are the

same as additional school days in May. Given the source of our identifying variation, we do not have enough precision to estimate separate effects early versus later in the school year.

Nonetheless, our estimates and their magnitude are important for policy. The results demonstrate that schooling has sizable effects on cognitive ability as late as age 18, suggesting that schooling interventions can be effective beyond primary school. Our 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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<sup>33</sup> President Obama and Secretary of Education 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 United States, it is 200 days or more in many countries.

<sup>34</sup> The OECD administered the Programme for International Student Assessment (PISA) by giving standardized reading, math, and science tests to ninth graders. Twenty extra school days from kindergarten up to ninth 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 3. Twenty-two percent of a standard deviation translates into an additional 22 points on the PISA, which would increase the U.S. ranking from fourteenth to fourth.

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