

TOO YOUNG TO LEAVE THE NEST? THE EFFECTS OF SCHOOL STARTING AGE

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Abstract—Using Norwegian data, we examine effects of school starting age (SSA). Unlike much recent literature, we can separate SSA from test age effects using scores from IQ tests taken outside school at about age 18. We find a small, negative effect of starting school older but much larger positive effects of age at test. Also, starting older leads to lower earnings until about age 30. We find little impact of SSA on educational attainment, but boys who start older are less likely to have poor mental health at age 18. Additionally, starting school older has a negative effect on the probability of teenage pregnancy.

I. Introduction

DOES it matter at what age a child starts school? Older children do better on tests, but is this because they are older, and, in fact, is this success unrelated to the age they started school? Despite the dearth of convincing evidence, the popular press seems to suggest that there are benefits to “red-shirting” (holding back) children in kindergarten (see “When Should a Kid Start Kindergarten?” 2007).¹ But is this the case? Are the short-run benefits in terms of better performance just that: Short run? And are there costs associated with finishing school and starting work later?

Much research has shown a consistent pattern that children who start school later tend to score higher on in-school tests, even after accounting for the endogeneity of school starting age.² However, a key limitation in the interpretation of these correlations is the inability to distinguish between the effect of school starting age and a direct age-at-test effect, as they are perfectly collinear. As a result, it could be that children who start school when they are older do better simply because they are older when they take the test, or it could be that starting school at an older age leads to direct and persistent benefits.

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¹ Recent work by Deming and Dynarski (2008) documents the trend toward increasing school starting age in the United States and explores possible causes.

² This includes a cross-country study by Bedard and Dhuey (2006) and country-level studies by Fredriksson and Öckert (2006) for Sweden, Puhani and Weber (2007) for Germany, Strom (2004) for Norway, Crawford, Dearden, and Meghir (2007) for England, McEwan and Shapiro (2008) for Chile, and Elder and Lubotsky (2009) for the United States.

Using data on the population of Norway, we are able to separate these two effects using IQ test scores measured outside school, at the time of military enrollment when students are around age 18. The rule in Norway that children must start school the year they turn 7 provides a discontinuity in school starting age for children born around January 1 and provides an instrument for actual school starting age. Importantly, there is also variation in the mapping between year and month of birth and the year the test is taken, allowing us to distinguish the effects of school starting age from pure age effects. Cognitive scores around age 18 are particularly interesting as it is about the time of entry into the labor market or to higher education, and so these scores are more relevant to the labor market than are scores in kindergarten or elementary school.

Additionally, we study the effects of school starting age on longer-term outcomes including adult earnings, educational attainment, early fertility, and mental health. While this is methodologically less complicated than studying in-school tests because age of measurement and school starting age are not perfectly collinear, the literature has been hindered by a paucity of data. Given the complications created by school-leaving-age rules in the United States, European data are attractive when studying education and earnings.³ Educational attainment has been studied in the literature, and it has generally been found that older starters average modestly higher completed education.⁴ However, ultimately adult earnings are a very important outcome, and we add to the literature by being the first study to track cohorts of men and women from ages 24 to 35 and analyze how the impacts of school starting age change with age.⁵ Also, we examine the effects of school starting age on the probability of teenage childbearing by women.⁶ Finally, unlike the literature, we can control for sibling fixed effects to address the issue that different types of families may

³ In Norway, the law specifies that students must complete a certain number of grades. Studies using U.S. data have suffered from the fact that compulsory schooling laws specify minimum school-leaving ages rather than grades, so older starters have completed less education at the minimum dropout age. Therefore, historically, persons whose quarter of birth predicts starting later have on average lower schooling and lower earnings (Angrist & Krueger, 1991). Dobkin and Ferreira (2010) find that older starters also obtain slightly lower education in more recent U.S. cohorts.

⁴ See Puhani and Weber (2007) and Fertig and Kluve (2005) for Germany, and Fredriksson and Öckert (2006) for Sweden.

⁵ Earnings estimates are particularly valuable given the view (Angrist & Pischke, 2009) that the question of the effect of school starting age on elementary school test scores is “fundamentally unanswerable.”

⁶ McCrary and Royer (2006) show, using U.S. data, that school starting age is unrelated to teen pregnancy, but this may be because compulsory schooling laws imply that older starters are compelled to spend less time in school in the United States. Indeed the authors interpret their estimates as schooling effects. Black, Devereux, and Salvanes (2008b) find evidence that extra schooling reduces teen childbearing in Norway.

have children at different times of the year, perhaps to manipulate school starting age.

We find evidence for a small, negative effect of starting school later on IQ scores measured at age 18. In contrast, we find evidence of a large, positive effect of age at test, and these results are robust. We also find a short-run negative effect on earnings of beginning school at an older age; however, this effect has essentially disappeared by age 30. This pattern is consistent with the idea that starting school later reduces potential labor market experience at a given age for a given level of education; however, this becomes less important as individuals age. When we examine other outcomes, we find that school starting age has a significant effect on teenage pregnancy among girls but no strong effect on education of girls or boys. Additionally, the probability that boys have poor mental health at age 18 is slightly lower for later starters.

The paper unfolds as follows. Section II presents the relevant literature. Section III describes our methodology and contrasts it to other approaches in the literature. Section IV discusses relevant institutional details in Norway, and section V provides a data description. Sections VI to IX present our results, and section X concludes.

II. Relevant Literature

A primary distinction in the literature on school starting age is between the policy question (What is the optimal starting age for society?) and the individual decision (What is the optimal starting age for an individual given the starting age rules that exist?).⁷ Like most of the rest of the literature, our paper focuses not on the optimal policy but on the individual decision: the effects of school starting age for any one child, taking the school starting ages of other children as given. This is what is relevant to parents who are deciding the age at which to send their children to school. In Norway, as in many other countries, there is a rule that most parents follow, but there is leeway to start children earlier or later if parents believe that this is appropriate for their children.

⁷ Bedard and Dhuey (2008) tackle the policy question directly by using variation in school starting age within states over time in the United States and find a significant positive effect of increasing the school starting age on wages. Other work addresses the policy issue through a less direct route by trying to distinguish the effects of relative age versus absolute age; a policy change in school starting age would change only absolute, and not relative, ages. Elder and Lubotsky (2009), Cascio and Schanzenbach (2007), and Fredriksson and Öckert (2006) use idiosyncratic variation in the school starting ages of other children in the class to estimate the role of relative and absolute school starting age on in-class test scores. If random variation in the school starting ages of other children in the class affects child outcomes, it suggests that relative school starting age matters. However, the policy relevance is limited, compared to the direct approach of Bedard and Dhuey (2008), by the fact that the random variation in starting age by class is unaccompanied by curriculum variation and we would expect changes in curriculum to accompany policy changes to the school starting age.

Most of the literature has compared test scores of children who are in the same grade. While children are in school, researchers are faced with the identity that

$$\text{Age at test} = \text{School starting age} + \text{Years of schooling.}$$

As such, this literature has typically estimated the combined effects of school starting age (SSA) and test age (AGE). Despite the importance of the distinction, there is little solid evidence as to the role of SSA versus AGE in determining school test scores.

Given the difficulty with separating out the two effects, a number of recent papers try to infer the role of age versus school starting age by looking at either early test scores or changes in scores over time.⁸ Elder and Lubotsky (2009) show strong age effects in the fall of kindergarten year (about half a standard deviation), before children could have been much affected by formal schooling. Elder and Lubotsky (2009) and Cascio and Schanzenbach (2007) also show that effects of age at school entry on test scores tend to get smaller as children move to higher grades. Together these papers imply that the estimated starting-age effects partly reflect the endowment differences between students when school starts; they find little evidence that students learn more in school if they are older when they start. However, none of this work is able to directly disentangle the effect of age at test from that of school starting age.

While most of the literature controls for time in school and thereby measures the combined effect of SSA and AGE, another series of papers controls for age at test and thereby measures the combined effects of SSA and time in school (TIS). There is some evidence that when young children are tested at the same age, they score less well on in-school tests if they started school older and hence have spent less time in school (Cahan & Cohen, 1989; Elder & Lubotsky, 2009). However, the bulk of this evidence is for very young children in kindergarten and elementary school, and it is not clear that these findings generalize to older ages relevant to the labor market.

Most similar methodologically to this paper are those by Crawford, Dearden, and Meghir (2007) and Cascio and Lewis (2006), both of which rely on multiple sources of variation to identify the effect of school starting age on children's test scores. Crawford et al. (2007) use the fact that there is variation in school starting age across local education authorities (LEAs) in Britain to separately identify the effect of school starting age from age at test effects on in-school tests. While some LEAs have only one entry point (with one cutoff date), other LEAs have two entry points, with some children starting in September and some starting in January, or even three entry points, with children starting school in September, January, and April. So while the school start cutoff in Britain is September 1, August-born

⁸ For example, Datar (2006) finds that achievement changes between kindergarten and first grade are not highly correlated with age at school entry.

children start school in September in some LEAs and later in the year in others. Thus, the effect of SSA can be distinguished from that of test age by comparing August- and September-born children who are in LEAs that have different policies. They find that age at test is the biggest factor; however, a limitation of this methodology is that the different school starting policies may themselves be disruptive or lead to changes in curriculum and so may affect both August- and September-born children.⁹

Cascio and Lewis (2006) examined the role of schooling on student performance on the Armed Forces Qualifying Test (AFQT) in the NLSY using variation in school cutoff ages (arising from across state variation) as well as variation in the age at which individuals take the test. Unfortunately, likely due to a relatively small sample, the authors find very imprecise statistically insignificant effects of school starting age when controlling for age (they interpret these estimates as effects of schooling, but schooling and school starting age are perfectly collinear for in-school children, conditional on age). With our large sample size, we can precisely estimate these effects.¹⁰

Also, a recent literature examines the relationship between school starting age and longer-run outcomes such as educational attainment and earnings. While methodologically this is less complex because there is no link between date of measurement and time in school, the literature has been limited by the absence of good data. For example, the most thorough previous study of earnings, by Fredriksson and Öckert (2006), has only one year of earnings data and so cannot distinguish between cohort and age effects. This is the first study to track cohorts of men and women from ages 24 to 35 and analyze how the impacts of school starting age change with age.¹¹

A. Conceptual Framework

Parents care about their children's outcomes, and school starting age can affect these outcomes through a variety of mechanisms. Two of the more notable are by varying the absolute age and the relative age of a child in the class. Absolute age can affect student outcomes in the following ways. A later starting age is explicitly making a trade-off between a year at home or preschool versus entering the labor market a year earlier. Generally, entering the labor market earlier is considered better in terms of generating returns on investments in human capital; the trade-off is between this extra year in the labor market versus an extra year in preschool activities. In addition, the efficacy of schooling might depend on a student's absolute age. To the

extent that students learn "better" when they are older, this would lead to a positive effect of school starting age on children's outcomes.

Starting school later also affects students' relative age in the classroom. Relative age can affect outcomes on a number of dimensions. An older student may be bigger and do better on in-class exams (because of higher absolute age) than his or her peers, which could affect self-confidence and later performance. However, relatively younger students could learn more from their older peers, which would work in the other direction. This could also be true for other outcomes. Being the youngest in a class means your friends will be older and you might follow their behavior; to the extent that the older children (through an absolute age effect) are more likely to engage in risky behaviors, the relatively younger students might emulate them.¹² Because the theoretical predictions are ambiguous, the issue becomes an empirical question.

III. Methodology

A. Identification Strategy

We first describe the empirical strategy we use when our outcome variable is a long-term one such as earnings or completed years of education. We then describe the adjustments we make when we look at IQ as an outcome. When we look at IQ, we need to account for the fact that we control for age at test.

B. Long-Term Outcomes

Our equation of interest is:

$$Y_i = \alpha_0 + \alpha_1 SSA_i + X_i' \lambda + \varepsilon_i, \quad (1)$$

where Y is the outcome under study, SSA is the school starting age, and X is a vector of controls that includes year-of-birth indicators and a linear trend in month of birth. Because the school cutoff is at the beginning of the year, we redefine year of birth to run from July to June rather than from January to December (so the discontinuity is now at the middle of our redefined "year").¹³ The linear trend is centered at the discontinuity (ranging from 1 in July to 12 the following June). Together, the year-of-birth indicators and linear trend allow cohort effects such as secular increases in educational attainment over time.

Our exogenous variation in school starting age comes from variation in month of birth and the administrative school starting rule in Norway. During the period we study,

⁹ Although there is no direct evidence on this point, evidence from the United States suggests that student mobility between schools has on adverse impact on other children (Hanushek, Kain, & Rivkin, 2004).

¹⁰ Mayer and Knutson (1999) also find some evidence that quarter of birth matters for test scores in the CNLSY.

¹¹ Also using Swedish data, Skirbekk, Kohler, and Alexia (2004) show that January borns are more likely to delay childbearing than December borns. Unlike us, they do not specifically look at teenage childbearing.

¹² This argument is similar to that of Argys et al. (2006), who suggest that higher-birth-order children are more likely to engage in risky behaviors at young ages because they are influenced by their older siblings. Consistent with this, Black, Devereux, and Salvanes (2005) find that higher-birth-order women in Norway are more likely to have births as teenagers.

¹³ Fredriksson and Öckert (2006) also use this redefined-year approach in their Swedish study.

TABLE 1.—COMPLIANCE RATES BY MONTH OF BIRTH

	Early	On Time	Late
January	.10	.90	0.0
February	.04	.96	.01
March	.02	.97	.01
April	.01	.98	.01
May	.01	.98	.01
June	0.0	.98	.01
July	0.0	.98	.01
August	0.0	.98	.02
September	0.0	.97	.02
October	0.0	.96	.04
November	0.0	.93	.07
December	0.0	.85	.15

Each number in the "Early" column refers to the percentage of children in each birth month who started school before the year they turned 7 years old. Each number in the "On Time" column refers to the percentage of children in each birth month who started school the year they turned 7 years old. Each number in the "Late" column refers to the percentage of children in each birth month who started school after the year they turned 7 years old.

children were expected to start school in the calendar year they turn 7 so December borns start school a year earlier than children born in January. Therefore, we estimate equation (1) by 2SLS using the expected school starting age (ESSA) as an instrument for the actual school starting age. We measure the ESSA as equal to $7.7 - (\text{month of birth} - 1)/12$. This takes account of the fact that school starts in August, and the cutoff date is at the beginning of the year. Given that the ESSA is determined only by month of birth and not by parental choice, it seems reasonable to treat it as exogenous and use it as an instrument for the actual SSA.¹⁴

In Norway during our sample period, compliance with the school starting rule was high for the cohorts we study. This is not surprising as parents had to formally apply for an exception from the rule and the application had to be approved by health and school specialists as well as by the local government (Strom, 2004). The high compliance rates are reassuring as they imply that our IV estimates can be interpreted as an approximation to the average treatment effect of school starting age rather than the usual local average treatment effect (LATE) interpretation.¹⁵ As one might expect, compliance rates are lower for children born in December and January than for those born during the middle of the year (see table 1).

For ESSA to be a valid instrument for SSA, two conditions must be satisfied. First, it must be random which children are born in different months of the year; this could be violated if different types of families have children at different times of the year.¹⁶ We attempt to address this issue

¹⁴ Because we include a linear trend, using ESSA as an instrument is exactly equivalent in this specification to having a binary indicator for being born January or after as the instrument.

¹⁵ Consistent with recent popular press, we find that it is the better-educated mothers who are more likely to be noncompliers; however, counter to this anecdotal evidence on "redshirting," these mothers are actually more likely to start their children early ("When Should a Kid Start Kindergarten?", 2007).

¹⁶ Buckles and Hungerman (2008) show that in the United States, there are significant differences between the parental characteristics of summer- and winter-born children.

in a number of ways. As a robustness check, we include family characteristics in our regression and show that our resulting estimates are very close to estimates without these controls. In addition, and perhaps more convincing, we are able to include family fixed effects as a check on this possibility.

Second, it must be that there is no direct effect of being born at a particular time of the year on child outcomes. Although there is some evidence of small differences in health outcomes across season of birth (Bound & Jaeger, 2000), the balance of previous evidence is that these differences are not nearly large enough to make much difference. Importantly, our critical comparison is between December- and January-born children, so differences between summer- and winter-born children are largely irrelevant.¹⁷

C. IQ Scores as Outcomes

When we study IQ scores at age 18, we add a control for the age of the person at the time of the test (AGE):

$$IQ_i = \beta_0 + \beta_1 SSA_i + \beta_2 AGE_i + X_i' \delta + v_i. \quad (2)$$

In Norway, there is a relationship between month of birth and when a person is called to take the test. As an example, in some years, individuals who were born in January, February, and March were called to take the exam in one year, while individuals born after March (in the same year) were called to take the exam a year later. However, not all men take the test in the year in which they are supposed to do so. This type of deviation can occur due to illness or absence abroad, for example. As a result, age at the time of the exam is potentially endogenous. Conceptually similar to the case of school starting age, we use the age at which men were supposed to take the test as an instrument for the age at which they actually did take the test. This exploits discontinuities like the fact that those born in April 1961 are almost a year older when they take the test than those born in March 1961.

Conditional on age, school starting age is typically perfectly correlated with time in school when the outcome is measured while still in school. In Norway, many boys take military IQ tests while still in school. Our estimates for IQ therefore provide a lower bound on the benefits of starting school older, holding schooling constant. Later we evaluate the role played by time in school by providing separate estimates for those who had finished schooling by the time of the test.

¹⁷ We have verified that January- and December-born children are in fact similar along observable dimensions. Average mother's education is 10.783 for December born compared to 10.790 for January born, birth order is 1.932 compared to 1.936, and family size is 2.899 compared to 2.885. In Norway there is no annual child tax deduction, so there is no particular incentive to have children just before January 1. This is in contrast to other countries such as the United States (see Chandra & Dickert-Conlin, 1999).

IV. The Norwegian Child Care and School System

It is important to understand the institutional context of our study. Prior to the mid-1970s, labor market participation rates for married women were relatively low, with rates about 35% in the 1960s and about 40% in the early 1970s. This rose to over 70% by 1990. In addition, families faced a shortage of day care facilities during the 1960s and 1970s. As a result, prior to 1980, day care enrollment for children between the ages of 3 and 6 was around 10% or less, with a large increase during the 1980s to over 50%.¹⁸

While our data broadly cover children aged 6 between 1968 and 1994, most of our outcomes rely on children born earlier in the period. This suggests that during the time period relevant to our sample, most children were at home prior to enrollment in school, either with their mother or an informal child care provider such as a grandparent or a neighbor.

In terms of schooling, all compulsory education in Norway is free. Since 1997, schooling has been compulsory from age 6 to 16 (tenth grade). However, the cohorts we consider faced a school starting age of 7 and 9 years of compulsory schooling. Schools are generally run by the local municipality, and there is no tracking by ability during the years of compulsory schooling.¹⁹

V. Data

Our primary data source is the Norwegian Registry Data, a linked administrative data set that covers the population of Norwegians up to 2006 and is a collection of different administrative registers such as the education register, family register, and tax and earnings register. These data, maintained by Statistics Norway, provide information about educational attainment, labor market status, earnings, and a set of demographic variables (age, gender) as well as information on families.²⁰ To ensure that all individuals studied went through the Norwegian educational system, we include only individuals born in Norway. We have information on school starting age for cohorts born from 1962 onward, and our analysis focuses on the 1962–1988 cohorts.

The IQ and mental health data are taken from the Norwegian military records from 1980 to 2005. In Norway, military service is compulsory for every able young man; as a result, we have military data for men only.²¹ Before enter-

¹⁸ Up to 1980, most day care facilities were located in urban areas, and most catered to the children of working mothers. These facilities were relatively expensive and in short supply.

¹⁹ There are very few private schools in Norway, and only about 2% of all pupils attend them.

²⁰ See Møen, Salvanes, and Sørensen (2004) for a description of these data.

²¹ Norway has mandatory military service of between twelve and fifteen months (fifteen in the navy and twelve in the army and air force) for men between the ages of 18.5 (17 with parental consent) and 44 (55 in case of war). However, the actual draft time varies between six months and a year, with the rest being made up by short annual exercises.

ing the service, their medical and psychological suitability is assessed, typically between their eighteenth and twentieth birthday.

The IQ measure is the mean score from three IQ tests: arithmetic, word similarities, and figures (see Sundet, Barlaug, & Torjussen, 2004, Sundet et al., 2005, for details). The arithmetic test is quite similar to the arithmetic test in the Wechsler Adult Intelligence Scale (WAIS) (Sundet et al., 2005; Cronbach, 1964), the word test is similar to the vocabulary test in WAIS, and the figures test is similar to the Raven Progressive Matrix test (Cronbach, 1964). The IQ score is reported in stanine (Standard Nine) units, a method of standardizing raw scores into a 9-point standard scale that has a discrete approximation to a normal distribution, a mean of 5, and a standard deviation of 2.²² We have IQ scores on about 84% of the relevant population of men in Norway.²³

The mental health score is based on a psychologist's assessment of a patient's mental health (via an interview) aimed at determining suitability for military service. As most people are considered as having "no problems," we create an indicator equal to 1 if the individual is considered to have "no problems"; this constitutes 93% of the men in our sample.

Earnings are measured as total pension-qualifying earnings reported in the tax registry and are available from 1986 to 2005. These are not top-coded and include labor earnings, taxable sick benefits, unemployment benefits, parental leave payments, and pensions. We identify full-time workers (defined as 30 or more hours per week) using the fact that our data set identifies individuals who are employed and working full time at one particular point in the year (in the second quarter in the years 1986–1995 and in the fourth quarter thereafter).²⁴ About 52% of our male sample are employed full time at age 24; this increases to 78% by age 35. The equivalent figures for women are 42% and 50%, respectively.

We measure educational attainment at the oldest age possible for each individual, that is, in 2006.²⁵ To get as close as possible to actual completed education, we do not include anyone in the education sample who is younger than 27 years old in 2006.

²² The correlation between this IQ measure and the WAIS IQ has been found to be .73 (Sundet et al., 2004).

²³ One concern is that missing IQ is nonrandom and is related to SSA. To examine this, we regressed an indicator for whether IQ is missing on SSA using the standard specification; while the OLS results are positive and significant, 2SLS estimates were small and insignificant. We got similar results when we examined missing earnings.

²⁴ An individual is labeled as employed if currently working with a firm, on temporary layoff, on up to two weeks of sickness absence, or on maternity leave.

²⁵ Our measure of child educational attainment is reported by the educational establishment directly to Statistics Norway, thereby minimizing any measurement error due to misreporting. This educational register started in 1970. See Møen, Salvanes, and Sørensen (2004) for a description of these data.

TABLE 2.—MEANS OF SELECTED VARIABLES

	Men	Women
School starting age (SSA)	7.3 (.3)	7.3 (.3)
Expected school starting age (ESSA)	7.3 (.3)	7.3 (.3)
Completed education	12.4 (2.4)	12.8 (2.5)
IQ test score	5.1 (1.8)	
Mental health (=1 if excellent)	.93 (.25)	
Teenage birth		.08 (.28)
Birth within 12 years of ESSA		.06 (.23)
Log earnings at age 24	11.37 (1.03)	11.07 (1.09)
Log earnings at age 35	12.57 (.74)	12.08 (.89)
Full-time employee (aged 24–35)	.65 (.48)	.48 (.50)
Social insurance receipt	.06 (.23)	.05 (.23)

Standard deviations are in parentheses.

We construct our teenage childbearing variable by restricting the sample to women who are at least 36 years old in 2006 and denoting a teen birth if they have a child who is at least 16 years old in 2006 who was born before the woman was aged 20.²⁶ On average in our sample, 8% of women have a birth as a teenager, and 6% have a birth within twelve years of the expected school start date.

Finally, we use register data on welfare receipt to construct an indicator for whether each individual was in receipt of social assistance at any point in the year he or she turned 35 years old. Social assistance is means tested and given only to the poorest members of society who have a tenuous connection to the labor market. As such, it is a useful indicator for poverty status. Only 5% of our sample received social assistance at age 35.

Table 2 presents summary statistics for our sample.

VI. Results for IQ Scores

Our results for IQ test scores are presented in table 3. We first present the OLS results (column 1), which suggest that SSA has a large, negative effect on military test scores. The coefficient of $-.8$ implies that going to school one year later reduces test scores by four-fifths of a stanine, or almost half a standard deviation. In contrast, the OLS estimates suggest no impact of age at test, which runs counter to our prior that older boys score higher on tests. Of course, the OLS estimates may be suffering from omitted variable bias, with less able children having their school entry, and possibly their test taking, delayed.

To address this issue directly, from this point forward, we treat SSA and age at test as endogenous variables and

²⁶ In order to know whether a woman had a teen birth, we need to observe both the mother and child in the administrative panel. The result is that the cohorts we use were born between 1963 and 1969.

use the 2SLS strategy described previously. As can be seen in table 3, the first-stage coefficient of ESSA (on SSA) is $.80$ for men, with a standard error of $.013$. The first-stage effect of predicted age at test on age at test is equally strong, with a first-stage coefficient of $.85$ with a standard error of $.024$. Clearly, there is no concern about weak instruments in this application.

In contrast to the OLS results, the 2SLS estimates show a strong, positive effect of age at test on IQ. The estimate implies that being one year older when taking the test increases the score by about $.2$; this is one-fifth of a stanine and about one-tenth of a standard deviation. Additionally, the effect of SSA is still negative and statistically significant but is much smaller, suggesting that starting school a year later reduces IQ scores by about $.06$, about one-twentieth of a stanine.

How do our results compare to those of other papers in the literature? Taken together, the age at test and SSA coefficients provide a prediction of what one would obtain if a boy started school a year later and, as a result of taking the exam with his school entry cohort, took the exam a year later. In this case, the estimated SSA effect would be the sum of the true SSA effect and the age-at-test effect. This equals $.16$, which is about 8% of a standard deviation. As such, our IQ findings are entirely consistent with findings in the literature of a significant positive effect of school starting age on test scores when boys are tested with others in their grade.

As a further validation of our results, we have also obtained data on tenth-grade performance in mathematics from 2002 to 2006 and so can estimate the effect of school starting age on in-class scores for the 1986 to 1990 cohorts (but, of course, cannot control for age). We find strongly statistically significant positive effects that imply that starting school one year later increases math test scores by about 20% of a standard deviation. This is comparable to findings in the literature (Bedard & Dhuey, 2006). The smaller effect for IQ scores is unsurprising given that our test takers are older and that the IQ tests probably measure fixed components of intelligence to a greater extent than in-school tests.²⁷

A. Additional Specifications

Discontinuity sample. The specification in equations (1) and (2) uses all months for identification of the SSA effect but allows other factors to affect IQ scores smoothly (linearly) through the discontinuity point.²⁸ As a robustness check, we also estimate our equation on the subsample of individuals born in either December or January, thereby using only the individuals born close to the discontinuity for identification. In this case, the linear trend is unidenti-

²⁷ We have verified that our IQ estimates are similar when only recent cohorts (born in the 1980s) are studied to make our sample more comparable with the Norwegian sample from Bedard and Dhuey (2006).

²⁸ Note that even using all months, the discontinuity in ESSA is necessary for identification because in the absence of the jump in January, ESSA would be perfectly correlated with the linear trend.

TABLE 3.—EFFECT OF SCHOOL STARTING AGE ON IQ TEST SCORES OF MEN

	OLS (1)	2SLS (2)	2SLS Discontinuity Sample (3)	2SLS with Fixed Effects (4)	2SLS Ten Years or Less of Education (5)	2SLS 12 or More Years of Education (6)
First stage		.800	.702	.802	.716	.786
School starting age		(.013)	(.014)	(.002)	(.020)	(.015)
Age at test		.849 (.024)	.704 (.043)	.873 (.003)	.940 (.011)	.944 (.009)
Second stage	-.760	-.060	-.039	-.065	-.0003	-.085
School starting age	(.027)	(.014)	(.014)	(.017)	(.030)	(.015)
Age at test	.004 (.014)	.208 (.012)	.206 (.033)	.192 (.013)	.174 (.021)	.217 (.012)
Observations	652,215	652,215	104,023	315,365	89,162	354,346
R ²	.0172	.0020	.0031	.0170	.0113	.0020

Sample includes cohorts born between July 1962 and June 1988. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in columns 1, 2, 4, 5 and 6 also include a linear trend that runs from July to the following June. In addition, column 4 contains family fixed effects and birth order dummies. The discontinuity sample includes only persons born in December or January. The sample used with family fixed effects includes only families in which there are at least two boys. Tests are administered to boys at approximately age 18. Standard errors in columns 1, 2, 3, 5, and 6 allow clustering by birth cohort (defined in months) and by family using the multiway clustering approach of Cameron, Gelbach, and Miller 2006. First-stage estimates reported refer to the effect of ESSA on SSA and the effect of projected age at test on age at test. Standard errors in parentheses.

fied and so is excluded from the estimating equation. The assumption underlying use of the discontinuity sample is that December and January observations are exchangeable, so that on average, their outcomes differ only because of the difference in their school starting ages. As can be seen in column 3 of table 3, consistent with the lower compliance rates in December and January, there is a lower first-stage estimate of .7 (.014) for ESSA when the discontinuity sample is used. The 2SLS estimates for the discontinuity sample are similar to those for the full sample.

Family fixed effects. There has been some recent evidence that parents may be manipulating the timing of births based on school starting age cutoffs. To the extent that this is true and that these parents may be different on other dimensions as well, our estimates may be biased.²⁹ To deal with this, we can also investigate the relationship between school starting age and long-run outcomes within families, thereby differencing out any time-invariant family qualities. To do so, we estimate the 2SLS regressions with additional dummy variables for each set of siblings. These specifications provide consistent estimates unless the timing of births among siblings is correlated with the counterfactual outcomes of the children. This seems unlikely, as child endowments are not known before birth but could arise if, for example, parents decide to strategically time the second child in response to indications that the first child had low ability. The fourth column of table 3 includes family fixed-effects estimates and controls for the birth order of the child. The number of observations is lower for these specifications because we exclude families in which there are not at least two boys with IQ scores. This specification gives very similar estimates to those without fixed effects.

Additional robustness checks. In the Appendix table 1, we report a set of alternative specifications to ensure that

our findings are robust to specification. These include allowing the linear trend to be different for each birth year, including a quadratic trend, allowing the linear trend to change slope in January, including a quartic in cohort defined at the monthly level, and including controls for maternal education, birth order, and family size. None of these specifications provides appreciably different estimates, and so we will focus on our basic full-sample 2SLS specification for the remaining outcomes.³⁰

B. Is the SSA Effect a Time-in-School Effect?

While the test is not administered in school (and is, in fact, unrelated to schooling), many individuals in our sample have not finished schooling at the time of the test. In this case, the estimated school-starting-age effect will encompass the fact that later starters have spent less time in school (since, for example, among individuals who ultimately complete college, those who started a year later will have not only a later school starting age but one year less of education at the time of the test—on average December borns have about 0.8 of a year more schooling than January borns at test time).³¹ To test the sensitivity of our results to this, we break our sample into those who, ex post, actually were finished with their schooling at the time of the test (those who have ten or fewer years of education in 2006) and those who had not completed their education at the time of the test (those who had at least twelve years of education in 2006).³²

One potential problem with this approach is that completed education may be endogenous because SSA affects educational attainment. However, as we will see later, there is no evidence in our data that male educational attainment is affected by SSA.

³⁰ While we do not report them, we have carried out similar specification checks for the other outcomes and found those estimates to be similarly robust to specification.

³¹ Leuven et al. (2006) find little evidence that time in school matters for Dutch kindergarten children.

³² Among those who ultimately completed twelve years of education, almost all (98%) had not finished schooling at the time of the test.

²⁹ See Crawford et al. (2007) for British evidence. Elder and Dickert-Conlin (2009) find very little evidence for this type of strategic birth timing in the United States.

The results for samples split by level of completed education are presented in columns 5 and 6 in table 3. When we restrict the sample to cases where both early and late starters are finished with their education by the time of the test, we get no statistically significant effect of school starting age and a slightly smaller (but still statistically significant) effect of age at test. Given that we have found relatively small effects of SSA on IQ in earlier specifications, this is consistent with even these small effects being largely explained by the fact that older starters have less schooling at the time of the test (given that the effect goes away in the sample where those with an earlier starting age have no education advantage). The estimates also suggest that a proportion of the estimated age effect is actually a time-in-school effect.³³

VII. Results for Earnings

Because SSA may affect labor supply and wages differentially across the age-earnings profile, it is important to look at the effect of school starting age within a cohort over time. For example, since late starters tend to have less work experience at any age and age-earning profiles are concave, this should imply that the effects of starting later get more positive (or less negative) as people get older. For this reason, we exploit the fact that we have panel data on earnings from 1986 to 2005 in order to examine how SSA effects change with age. To follow persons from age 24 (when some have not finished schooling) to 35 (at which point the marginal value of a year of extra labor market experience should be getting low), we use a sample born between 1962 and 1970. A crucial feature of our data is that we can follow cohorts (and even individuals) as they age and so can distinguish between cohort and age effects. In contrast, in their Swedish study, Fredriksson and Öckert (2006) have only one year of earnings data and so cannot make this distinction. As before, we redefine a birth year to include people born between July 1 and the following June and measure earnings at a particular age for all individuals in the redefined birth year at the same time.

³³ Note that we are attributing the difference in the estimated effect of SSA to time in school and not to heterogeneous treatment effects by educational attainment. This assumption is questionable as there is some heterogeneity in the SSA effect across educational attainment levels of twelve years and higher. An alternative approach is to use these estimates to provide a bound on the effect of SSA. To do so, we can consider a simple model for people who are still in school at the time of the test: Suppose $IQ = a \times SSA + b \times TIS + c \times AGE$. However, we regress IQ on just SSA and AGE so we get $IQ = a \times SSA + b \times (AGE - SSA) + c \times AGE = (a - b) \times SSA + (b + c) \times AGE$. If we make the weak assumptions that the AGE effect and the TIS effect are both nonnegative, then a lower bound on the SSA effect is $(a - b)$ because $b \geq 0$ and an upper bound of the SSA effect is $(a - b) + (b + c) = (a + c)$ because $c \geq 0$. This suggests that with very weak assumptions, we can bound the SSA effect for people in school at the time of the test. Using the group with education ≥ 12 (who are still in school at the time of the test) the bounds are $(-.09, .12)$. This tells us that the SSA effect is not very large in either direction, as is consistent with what we find when we use the group who had finished education by the time of the test.

In tables 4 and 5 (for men and women, respectively), we report estimates for earnings of all labor market participants (about 94% of the sample), the earnings of full-time employees, and the probability of working full time. We run each regression by age, and the reported coefficients are the effect of SSA on log earnings. The estimated SSA effect gives the effect of school starting age conditional on age, so (assuming no effect of SSA on educational attainment) it can be interpreted as the benefit of spending a marginal year before starting schooling rather than after finishing schooling.

The OLS estimates for men are negative, and the negative effect gets larger as men get older. This is inconsistent with the effects of SSA wearing off with experience but is probably explained by the fact that earnings at older ages provide more information about skills and late starters are negatively selected. Unsurprisingly, the 2SLS estimates are very different. For men, the main finding is that higher SSA leads to lower earnings until about age 30. This is true for both all earnings and the subsample of full-time workers.

In the working paper version of this paper, we plotted estimates of the month of birth dummies from regressions of log earnings on month-of-birth and year-of-birth dummies. These are essentially descriptions of the reduced forms underlying the 2SLS estimates. For earnings at age 24, the jump between December and January is very apparent. The basis for finding no SSA effect on male earnings at age 35 is also obvious, as there is no jump between December and January (see Black, Devereux, & Salvones, 2008a).

Quantitatively, the initial negative effects are larger (about 10% at age 24) when all earners are included than when only full-time workers are included (about 5% effect at age 24). This is consistent with much of the earnings impact coming through a differential labor supply, with older school starters working fewer hours at younger ages. After about age 30, the 2SLS estimates for both men and women become close to 0 and are almost always statistically insignificant. Given the large sample sizes, the estimates are quite precise, and we can be confident that there is no large effect of school starting age on earnings or the probability of working full time in either direction once men or women are in their mid-30s.³⁴ The earnings estimates for women in their 20s are generally similar to those of men but are less precisely estimated.³⁵

³⁴ In our standard 2SLS specification, we estimate both the SSA effect and the linear trend. The linear trend gives the value of an extra month of age, conditional on SSA, and so is the return to potential experience provided there is no cohort effect conditional on the year-of-birth dummies. Although we do not report the estimates, we have verified that by age 35, the coefficient on the linear trend also becomes negligible and statistically insignificant. This is consistent with the return to experience being close to 0 by that age.

³⁵ One might still be concerned that 35 is too young an age to cease the analysis. We have information on ESSA, but not SSA, for cohorts born from 1950 onwards, and we have used the 1950–1965 cohorts to estimate the reduced forms all the way from ages 22 to 40. Black et al. (2008a) show that the ESSA effect between 36 and 40 is always close to 0 and never statistically significant. Given the generally high compliance rates, this suggests the SSA effect is also very small for these ages.

TABLE 4.—EFFECT OF SCHOOL STARTING AGE ON LOG EARNINGS AND LABOR FORCE PARTICIPATION OF MEN

	Age											
	24	25	26	27	28	29	30	31	32	33	34	35
Log earnings												
All OLS	-0.055 (0.010) [247,195]	-0.086 (0.009) [246,285]	-0.123 (0.009) [245,655]	-0.145 (0.010) [245,463]	-0.164 (0.011) [245,530]	-0.172 (0.011) [245,590]	-0.180 (0.011) [245,436]	-0.190 (0.011) [245,021]	-0.192 (0.012) [244,736]	-0.194 (0.011) [244,160]	-0.200 (0.012) [243,813]	-0.210 (0.012) [243,301]
2SLS	-0.092 (0.013) [247,195]	-0.099 (0.013) [246,285]	-0.096 (0.011) [245,655]	-0.065 (0.011) [245,463]	-0.039 (0.011) [245,530]	-0.023 (0.009) [245,590]	-0.010 (0.008) [245,436]	-0.006 (0.009) [245,021]	-0.003 (0.009) [244,736]	0.006 (0.009) [244,160]	0.003 (0.009) [243,813]	0.006 (0.007) [243,301]
Log earnings												
Full-time OLS	-0.056 (0.006) [129,665]	-0.072 (0.006) [136,231]	-0.087 (0.006) [146,345]	-0.088 (0.005) [158,515]	-0.094 (0.005) [168,983]	-0.100 (0.005) [176,943]	-0.100 (0.005) [182,648]	-0.111 (0.005) [186,348]	-0.109 (0.005) [188,428]	-0.109 (0.005) [189,254]	-0.115 (0.006) [189,282]	-0.120 (0.006) [188,861]
2SLS	-0.032 (0.007) [129,665]	-0.029 (0.006) [136,231]	-0.029 (0.005) [146,345]	-0.030 (0.005) [158,515]	-0.023 (0.005) [168,983]	-0.010 (0.006) [176,943]	-0.004 (0.005) [182,648]	-0.012 (0.005) [186,348]	-0.003 (0.005) [188,428]	-0.004 (0.006) [189,254]	0.008 (0.005) [189,282]	-0.004 (0.006) [188,861]
Whether work full time												
OLS	-0.007 (0.004) [129,665]	-0.018 (0.004) [136,231]	-0.028 (0.004) [146,345]	-0.044 (0.004) [158,515]	-0.045 (0.004) [168,983]	-0.044 (0.005) [176,943]	-0.046 (0.005) [182,648]	-0.044 (0.005) [186,348]	-0.047 (0.005) [188,428]	-0.047 (0.005) [189,254]	-0.045 (0.005) [189,282]	-0.047 (0.005) [188,861]
2SLS	-0.041 (0.006) [129,665]	-0.041 (0.005) [136,231]	-0.035 (0.005) [146,345]	-0.030 (0.006) [158,515]	-0.020 (0.006) [168,983]	-0.012 (0.006) [176,943]	-0.002 (0.005) [182,648]	0.003 (0.005) [186,348]	-0.002 (0.005) [188,428]	0.004 (0.005) [189,254]	0.005 (0.004) [189,282]	0.008 (0.005) [188,861]

Sample includes cohorts born between July 1962 and June 1970. All regressions include indicators for year of birth (defined as running from July to the following June) and a linear trend that runs from July to the following June. Standard errors allow clustering by birth cohort (defined in months) and by family using the multiway clustering approach of Cameron et al. (2006). The 2SLS estimates use ESSA as an instrument for SSA. Standard errors in parentheses. Number of observations in brackets.

TABLE 5.—EFFECT OF SCHOOL STARTING AGE ON LOG EARNINGS AND LABOR FORCE PARTICIPATION OF WOMEN

	Age											
	24	25	26	27	28	29	30	31	32	33	34	35
Log earnings												
All OLS	-0.135 (0.012) [223,449]	-0.155 (0.013) [221,934]	-0.172 (0.013) [220,888]	-0.173 (0.013) [220,497]	-0.198 (0.012) [220,418]	-0.212 (0.012) [220,571]	-0.209 (0.013) [220,681]	-0.207 (0.014) [220,993]	-0.213 (0.014) [221,639]	-0.211 (0.014) [222,258]	-0.206 (0.013) [223,185]	-0.204 (0.012) [224,220]
2SLS	-0.116 (0.014) [223,449]	-0.098 (0.013) [221,934]	-0.078 (0.012) [220,888]	-0.049 (0.014) [220,497]	-0.030 (0.011) [220,418]	-0.011 (0.011) [220,571]	0.004 (0.012) [220,681]	0.009 (0.011) [220,993]	-0.007 (0.011) [221,639]	-0.022 (0.012) [222,258]	-0.005 (0.010) [223,185]	0.001 (0.010) [224,220]
Log earnings												
Full-time OLS	-0.088 (0.008) [94,519]	-0.091 (0.006) [97,644]	-0.103 (0.007) [102,629]	-0.103 (0.006) [106,820]	-0.101 (0.005) [109,084]	-0.100 (0.006) [110,130]	-0.106 (0.007) [110,426]	-0.104 (0.008) [110,430]	-0.098 (0.006) [110,532]	-0.100 (0.007) [110,400]	-0.100 (0.006) [111,072]	-0.096 (0.006) [112,093]
2SLS	(0.010) [94,519]	(0.007) [97,644]	(0.008) [102,629]	(0.008) [106,820]	(0.007) [109,084]	(0.007) [110,130]	(0.007) [110,426]	(0.007) [110,430]	(0.007) [110,532]	(0.006) [110,400]	(0.007) [111,072]	(0.006) [112,093]
Whether work full time												
OLS	-0.042 (0.004) [94,519]	-0.052 (0.004) [97,644]	-0.061 (0.004) [102,629]	-0.057 (0.004) [106,820]	-0.061 (0.004) [109,084]	-0.069 (0.004) [110,130]	-0.070 (0.004) [110,426]	-0.068 (0.004) [110,430]	-0.069 (0.004) [110,532]	-0.074 (0.004) [110,400]	-0.074 (0.004) [111,072]	-0.075 (0.004) [112,093]
2SLS	-0.031 (0.006) [94,519]	-0.036 (0.005) [97,644]	-0.033 (0.006) [102,629]	-0.007 (0.005) [106,820]	0.007 (0.006) [109,084]	-0.004 (0.005) [110,130]	0.005 (0.005) [110,426]	0.006 (0.005) [110,430]	0.002 (0.006) [110,532]	-0.004 (0.005) [110,400]	-0.003 (0.005) [111,072]	-0.006 (0.005) [112,093]

Sample includes cohorts born between July 1962 and June 1970. All regressions include indicators for year of birth (defined as running from July to the following June) and a linear trend that runs from July to the following June. Standard errors allow clustering by birth cohort (defined in months) and by family using the multiway clustering approach of Cameron et al. (2006). The 2SLS estimates use ESSA as an instrument for SSA. Standard errors in parentheses. Number of observations in brackets.

TABLE 6.—EFFECT OF SCHOOL STARTING AGE ON VARIOUS OUTCOMES

	Men		Women	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)
Education	-.756 (.028) [514,662]	.009 (.022)	-.741 (.027) [489,794]	.038 (.023)
Teenage birth			.003 (.002) [218,674]	-.018 (.003)
Birth within first 12 years of school			.019 (.002) [218,674]	.039 (.003)
Mental Health at age 18	-0.031 (0.003) [701,676]	0.005 (0.002)		
SSA receipt at age 35	0.019 (0.002) [263,293]	-0.0002 (0.002)	0.020 (0.002) [249,762]	-0.002 (0.002)

Sample cohorts differ by outcome; see text for details. All regressions include indicators for year of birth (defined as running from July to the following June) and a linear trend that runs from July to the following June. In addition, outcomes measured at age 18 include a control for age at measurement. Standard errors allow clustering by birth cohort (defined in months) and by family using the multiway clustering approach of Cameron et al. (2006). Standard errors in parentheses. Number of observations in brackets.

To get a sense of the magnitudes, we have estimated a Mincer regression on earnings of full-time men and found coefficients on experience and its square of .098 (.001) and $-.0031$ (.00003), respectively. These imply that the return to experience drops to 0 after about 16 years. Also, at 8% to 9%, the return to experience in the very early years in the labor market is more than large enough to explain our SSA effects on earnings in these years.

How big is the effect of SSA on lifetime earnings? We have done a simple back-of-the-envelope calculation assuming that SSA has no impact on earnings after age 35 and ignoring earnings before age 24. We find, using a discount rate of 5%, that the present discounted value (at age 24) of the lifetime earnings loss from starting one year older is 66,500 krona in 2007 currency (approximately U.S. \$10,000 or about one-fifth of average annual earnings).

VIII. Results for Other Outcomes

In table 6, we report the effect of school starting age on a number of other longer-run outcomes to paint a broader picture of the effects on overall well-being.

A. Education

Row 1 presents the education results for men and women. For both men and women, the OLS estimates are strongly negative. However, there is little evidence of a causal effect of school starting age on educational attainment, with 2SLS estimates being small and statistically insignificant.³⁶

B. Teenage Childbearing

In terms of teenage childbearing, we study two related outcome variables. The first is whether a woman has a child

as a teenager, and the second is whether a woman has a child within 12 years of her expected school starting age. While the former is the more standard measure of teenage childbearing, the latter is plausibly a better measure of whether early motherhood is likely to disrupt human capital accumulation and, hence, later earnings potential. Given that most of our sample completes at least 12 years of schooling and 12 is the modal level of schooling, this outcome variable measures whether women are likely to find it difficult to obtain the normal level of education because they have children.

While OLS estimates suggest a small, positive effect of SSA on teen childbearing, our 2SLS estimates show a statistically significant negative effect of school starting age on teenage pregnancy, with a coefficient of about $-.018$.³⁷ This implies that a three-month increase in school starting age reduces the probability of teenage pregnancy by approximately 0.5 ($.25 \times .018 \times 100$) percentage points.

When we instead consider the effect on the probability of having a birth within 12 years of the expected school start, the OLS effect of SSA is .019 (.002). The 2SLS effects of SSA are also positive and even larger: about .04 to .05. A three-month increase in school starting age will increase the probability of a birth within the first 12 years of school by about 1.2 ($.25 \times .05$) percentage points.³⁸ Our estimates suggest that although starting school older does reduce teenage pregnancy, it still increases the probability that a girl will interrupt her schooling to have a baby.

³⁷ This is consistent with recent work by Argys and Rees (2008) that finds that females with older peers are more likely to use drugs. We have verified that the average derivatives of the reduced forms from probit models are very similar to the linear probability estimate and have similar-sized standard errors.

³⁸ The main reason for this large, positive effect is that twelve years after the ESSA, January borns are almost one year older than December borns, and age is a prime determinant of fertility. The actual difference between January and December borns in fertility probability is less than what we would expect based purely on age effects and so is consistent with our finding that at any given age, later starters are less likely than early starters to have had a child.

³⁶ We have also studied the effect of SSA on an indicator for whether the individual has at least twelve years of schooling and found very similar results.

TABLE 7.—EFFECT OF SCHOOL STARTING AGE BY QUARTILE AND GENDER 2SLS

Dependent Variable	Men			Women		
	Lowest 25%	Middle 50%	Top 25%	Lowest 25%	Middle 50%	Top 25%
IQ test score	−0.067 (0.023) [163,088]	−0.064 (0.016) [326,076]	−0.088 (0.021) [163,051]			
Full-time employment at age 24	−.045 (.013) [65,836]	−.035 (.008) [131,611]	−.048 (.011) [65,846]	−.022 (.011) [62,464]	−.031 (.008) [124,824]	−.042 (.010) [62,472]
Full-time Employment at age 35	−.001 (.009) [65,833]	.011 (.007) [122,022]	.012 (.011) [65,814]	−.018 (.009) [62,441]	−.008 (.008) [124,884]	.006 (.010) [62,437]
Log earnings at age 24, all workers	−0.038 (0.021) [62,261]	−0.089 (0.016) [123,553]	−0.145 (0.029) [61,381]	−0.064 (0.025) [55,317]	−0.114 (0.016) [111,933]	−0.184 (0.029) [56,199]
Log earnings at age 35, all workers	−0.040 (.016) [60,987]	0.012 (.010) [121,809]	0.034 (.021) [60,505]	−.000 (.029) [56,055]	0.005 (.013) [112,242]	−0.008 (.017) [55,923]
Education	−0.030 (0.031) [128,794]	0.028 (0.025) [257,727]	−0.060 (0.041) [128,141]	0.055 (0.029) [122,835]	0.047 (0.025) [244,915]	−0.007 (0.046) [122,044]
Teenage pregnancy				−0.016 (0.005) [54,669]	−0.016 (0.004) [109,337]	−0.018 (0.008) [54,668]
Birth within first 12 years of school				0.013 (0.003) [54,670]	0.035 (0.003) [109,336]	0.070 (0.008) [54,668]
Mental health (= 1 if excellent)	.006 (.004) [175,421]	.003 (.002) [350,893]	.006 (.003) [175,362]			
Social assistance receipt at age 35	.002 (.004) [65,824]	.003 (.003) [131,646]	.004 (.005) [65,823]	−.001 (.003) [62,442]	−.003 (.003) [124,880]	.002 (.005) [62,440]

For all outcome variables except earnings and full-time employment at age 24, the reported quartile is the quartile of the distribution of the predicted value of the outcome variable where mother's education, family size, and birth order variables are used to predict the outcome. For earnings at age 24, the reported quartile is the quartile of the distribution of predicted earnings at age 35. For full-time employment at age 24, the reported quartile is the quartile of the distribution of predicted full-time employment at age 35. Standard errors allow clustering by birth cohort (defined in months) and by family using the multiway clustering approach of Cameron et al. (2006). Standard errors in parentheses. Number of observations in brackets.

C. Mental Health

School starting age appears to be negatively correlated with mental health. OLS results show that children who start school later are less likely to be classified as “without problems” by a psychologist. However, again, this appears to be driven entirely by selection. 2SLS results suggest a significantly positive effect of school starting age on mental health, although the magnitude is very small: a one-year increase in school starting age increases the probability of being classified as “without problems” by half a percentage point.

D. Receipt of Social Assistance

Finally, there is no significant effect of SSA on social assistance receipt at age 35.³⁹ This is consistent with our earnings results and suggests that SSA has no persistent effects on income.

IX. Heterogeneous Effects of SSA

One concern might be that by looking at the entire sample, we are missing important differences across the distribution of children. For example, children from poorer

families may be more at risk and hence suffer most from being young in school; wealthier families may be better able to offset any negative school effects. On the other hand, the advantage of school environment over home environment could be greater for children from poorer backgrounds.⁴⁰

To examine this directly, we regressed selected outcomes (by gender) on a variety of family background characteristics (mother's education, family size, and birth order) and obtained a predicted value for each individual. Using this predicted value as our index of family background (essentially just a weighted average of the three family background characteristics), we divide the sample into four quartiles and present the results separately for the first quartile, the second and third quartiles, and the fourth quartile. Table 7 presents these results.

As can be seen, there is little evidence of heterogeneous effects when the outcome variables considered are IQ, mental health, teen pregnancy, educational attainment, or social assistance receipt. However, the effect of SSA on the prob-

³⁹ We find no evidence of an effect of SSA on social assistance at any age but, for brevity, report only social assistance receipt at age 35.

⁴⁰ The conclusions about heterogeneity are quite mixed in the existing literature. For example, Elder and Lubotsky (2009) find, using U.S. data, that starting later has particularly large positive impacts on early in-school scores of children of high-income parents. However, using data from the United Kingdom, Crawford et al. (2007) do not find significant differences in the SSA effect on in-school scores between students eligible and ineligible for free lunch.

ability of giving birth within 12 years of starting school is higher for less advantaged groups (note that with the teenage pregnancy outcomes, the highest quartile is the most disadvantaged, as the outcome is a negative one).⁴¹

The effect of SSA on male earnings at age 35 is statistically negative for the bottom quartile but insignificantly positive for the other quartiles. This difference is statistically significant and suggests that starting school later has a negative impact on the earnings of men from disadvantaged backgrounds but no negative effect on other men. Interestingly, the estimates for earnings at age 24 show the opposite pattern (note that for this outcome, we split into quartiles based on predicted earnings at age 35 because many people are not fully engaged in the labor market at age 24). At age 24, the effects of starting school later are most negative for higher quartiles. This can be rationalized by the fact that the higher quartiles disproportionately include those who complete a lot of education and starting later is likely to cause them to be out of the labor market at age 24. We see no significant heterogeneity across the distribution when we look at the probability of being employed full time among 24 year olds or 35 year olds.

X. Conclusions

Much research has shown a consistent pattern that children who start school at older ages tend to score higher on in-school tests. This finding suggests that school starting age may have significant effects on the outcomes of adults. In this paper, we find that for the most part, this is, not the case. Despite the fact that the effects of SSA on in-school tests in Norway are as large as those in the United States (Bedard & Dhuey 2006), the long-run effects of SSA seem modest. For men, there appear to be no long-term effects on education or earnings, and the effects on military test scores are very small when one allows for age-at-test effects. For women, there is little evidence of large impacts on educational attainment. An intriguing result is that starting early increases the likelihood of giving birth as a teenager but reduces the likelihood of giving birth within twelve years of starting school. Overall there is not much evidence to suggest strong reasons for parents to hold their children out of school or to time the births of their children to influence school starting age.

The question of the optimal school starting age for society remains. Although starting older does not appear to have appreciable benefits for individual children (and reduces lifetime earnings on average), this does not imply that a policy change that caused all children to start at an older age would have these effects. Bedard and Dhuey (2008) have shown that in the United States, policies to increase starting ages have led to higher earnings on aver-

age. More work is required to assess the robustness of this result across countries and cohorts (the 1997 change in school starting age from 7 to 6 in Norway is a fruitful topic for future research).

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⁴¹ This result is to some extent a mechanical one. The probability of giving birth within the first twelve years of school is only 0.02 in the bottom quartile, and so the effect of SSA on this probability cannot be larger than .02.

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APPENDIX

Robustness Checks

IV ESTIMATES FOR THE EFFECT OF SCHOOL STARTING AGE ON IQ SCORES

	Slope of Linear Trend Allowed Change in January	Cohort Specific Trend	Quadratic Trend	Instrumenting SSA with Month of Birth Dummies	Including Quartic Function of Birth Cohort (Measured in Months)	Including Family Controls (Mother's Education, Birth Order, Family Size)	Including Family Controls, Discontinuity Sample
School starting age	−0.061 (0.014)	−0.063 (0.013)	−0.060 (0.014)	−0.062 (0.014)	−0.061 (0.013)	−0.072 (0.013)	−0.051 (0.013)
Age at test	0.209 (0.012)	0.226 (0.012)	0.210 (0.012)	0.209 (0.012)	0.216 (0.013)	0.204 (0.012)	0.195 (0.030)
Observations	652,215	652,215	652,215	652,215	652,215	652,215	104,023
R^2	0.002	0.002	0.002	0.002	0.002	0.120	0.121

Sample includes cohorts born between July 1962 and June 1988. All regressions include indicators for year of birth (defined as running from July to the following June). The discontinuity sample includes only persons born in December or January. Standard errors allow for clustering by birth cohort (defined in months) and by family using the multiway clustering approach of Cameron et al. (2006). Standard errors are in parentheses.