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Whose Children Gain from Starting School Later? Evidence from Hungary

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ABSTRACT

Whose Children Gain from Starting School Later? Evidence from Hungary*

We look at the effect of school starting age on standardized test scores using data covering all grade four and grade eight students in Hungary. Instrumental variables estimates of the local average treatment effect suggest that children generally gain from starting school one year later and the effects are much stronger in the case of students coming from low-educated families. We test the robustness of the results by allowing for heterogeneity in the age effect, distinguishing between fields of testing, using discontinuity samples and relying on alternative data. The hypothesis that delayed entry has a stronger impact on low-status children is supported by the robustness checks. The observed patterns are most probably explained by the better performance of kindergartens, as opposed to schools, in developing the skills of low-status children.

JEL Classification: I21, I28, J24

Keywords: education, student test scores, enrolment age, identification

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1 Introduction

We look at the effect of school starting age on standardized test scores using data from Hungary's National Assessment of Basic Competencies (NABC), which covers all grade four and grade eight students in the country. Students are typically aged 10 – 11 and 14 – 15 at the time of testing because of the variation in their school starting age.[1]

We estimate the local average treatment effect (LATE) of delayed start using an instrumental variable (IV) model, which exploits the exogenous variation in school starting age driven by the cut-off date for enrolment and children's month of birth. We test the robustness of the results in four ways: by allowing for heterogeneity in the age effect, distinguishing between tests of literacy and numeracy, using discontinuity samples and relying on alternative data.

The impact of social background on academic test scores is nowhere as strong among the countries participating in PISA 2006 as in Hungary (Jenkins et al. 2008). In view of this fact, we are particularly interested in the question of how delayed start affects low-status students. Therefore, we estimate the models for the children of low- and high-educated mothers separately, taking advantage of the exceptional size of the NABC samples. Late entry is expected to have an equality-enhancing effect if pre-school institutions and/or families perform better in developing the skills of low-status children (relative to their high-status counterparts) than primary schools. In the Hungarian context, we expect that repeating the school preparation year in the less segregated environment of the compulsory kindergarten – rather than going to a low-quality school from age six – can help many disadvantaged and/or discriminated children catch up with their high-status classmates.

The LATE estimates strongly support our key hypothesis. Unlike the OLS results, which misleadingly indicate that late entrants perform below average, the IV estimates yield evidence that children generally gain from starting school one year later. The effects are significantly stronger in the case of students coming from low-educated families. For them, the LATE estimate exceeds 80 and 35 percent of the standard deviation of the composite cognitive-academic test score in fourth and eighth grades, respectively. The children of mothers with a tertiary degree benefit far less from a late start: the effects fall short of 30 percent and 20 percent in grades four and eight, respectively.

We find the between-group differences to be larger at age 10 – 11 than at age 14 – 15: the equity-enhancing effect of delayed start seems to fade away as children progress through school. However, given the practice of early tracking in the Hungarian school system, achievement at age 10 – 11 is of great importance.

When checking the robustness of the results, we first allow for heterogeneity in the age effect. The LATE identifies the age effect for children whose entrance age is influenced by the cut-off date and is only informative of the entire student population in case of model homogeneity, that is, if the age

[1] School starting age is strongly affected by the cut-off date for primary school enrolment and parental decisions. Children born after 31 May are expected not to start primary school in the year they reach age six. Furthermore, within certain limits, parents are allowed to request both early and delayed primary school enrolment.

effect for those affected by the instrument does not differ from the age effect for those who are selected into delayed enrolment voluntarily. Therefore, we further analyse the data following the control function approach proposed in Garen (1984) to produce consistent estimates of the average treatment effect (ATE), which measures the impact of delayed start for a randomly selected child. (To the best of our knowledge, we are the first to apply this model to the subject under examination.) The ATE estimates substantially lag behind the LATEs, suggesting that children born before the cut-off date and selected into delayed enrolment by their parents, kindergarten teachers and/or the body of educational counsellors benefit less from the postponement, on average, than do those, whose birthday falls after the cut-off date. Even so, most of the ATE estimates are significant for disadvantaged children. In their case, the estimates amount to about 40 percent of the standard deviation of the composite cognitive-academic test score in the fourth grade and 20 percent in the eighth grade. The ATE estimates for the high-status children are typically insignificant and fall short of 10 percent.

Second, we estimate LATE for reading and mathematics tests separately. The finding that the effect of delayed start is stronger for low-status children continues to hold.

Third, the LATE estimates remain significant and follow the same pattern in the discontinuity samples comprising children born in a four-month range around the cut-off date.

Finally, we repeat the analysis using the Hungarian sub-samples of PIRLS and TIMSS.[2] The LATE estimates follow the same pattern as those identified in the NABC (the effects are stronger for disadvantaged children) but most of the coefficients estimated for the small samples (no more than 3 – 5 percent of the NABC population) are statistically insignificant.

The paper is structured as follows: Section 2 discusses the methodological difficulties of estimating the impact of school starting age on academic achievement, gives an overview of the solutions proposed in the literature and introduces the IV and control function approaches. Section 3 introduces the Hungarian primary school enrolment cut-off date regulation and argues why the issue of delayed entry is important in a highly segregated school system. Section 4 describes the data sources. The LATE estimates and the results or the robustness checks are presented in Sections 5 and 6, respectively. Section 7 concludes.

2 Methodological considerations

2A The effects of school starting age and the difficulties of identification

The effect of school starting age operates through a number of different pathways as discussed in Black et al. (2008), Cascio and Schanzenbach (2007), Datar (2006), Fredrikkson and Öckert (2006), Leuven at al. (2010) and McEwan and Shapiro (2007). (i) Children who delay enrolment are older at the time of testing and subsequently have more accumulated knowledge, implying an *age-at-test effect*. (ii) Delayed enrolment also increases children's absolute age of enrolment, whereby older children have the necessary cognitive, social, linguistic or physical maturity to perform better in each grade, implying an *absolute age effect* (iii) Delayed enrolment also increases a child's age relative to

[2] PIRLS: Progress in International Reading Literacy Study. TIMSS: Trends in Mathematics and Science Study.

his/her classmates, entailing a *relative age effect*. Relatively older students may benefit from delayed entry if the curriculum is geared towards the average student's level of development (Datar 2006). Furthermore, relatively older students may outperform younger ones by virtue of their relative maturity which permanently boosts their achievement – for example through self-confidence and attention that come from being the oldest in the class (Cascio and Schanzenbach 2007).

The major challenge in estimating the effect of school starting age on achievement is that students with delayed entry are not randomly selected if parents have some choice regarding the timing of primary school enrolment, as is the case in Hungary. In order to overcome the problem of self-selection, numerous empirical studies (including Bedard and Dhuey 2006, Black et al. 2008, Cascio and Schanzenbach 2007, Datar 2006, Elder and Lubotsky 2009, Fertig and Kluve 2005, Fredrikkson and Öckert 2005, Hámori 2008, Leuven et al. 2004, McEwan and Shapiro 2008, Puhani and Weber 2007 and Strøm 2004) exploit the exogenous variation in school starting age driven by the cut-off date regulation and the children's month of birth to estimate IV models. Our benchmark model will follow this identification strategy by using expected school starting age as an instrument for actual school starting age.

The IV estimates capture the LATE: the average causal effect of the treatment for those who comply with the assignment mechanism of the instrument i.e. compliers (Imbens and Angrist 1994).[3] In our case, the LATE identifies the effect of school starting age for those children, who start school later because their birthday falls after the cut-off date. The existence of heterogeneous treatment effects in the model implies that the LATE may not be informative for the entire student population (Angrist 2004, Angrist and Pischke 2009). In order to incorporate treatment effect heterogeneity, a control function approach is proposed by Garen (1984), an extension of the IV-model. The control function estimates the average treatment effect (ATE), that is, the gain to starting school later for a randomly chosen child.

In the absence of longitudinal data, we follow Bedard and Dhuey (2006), Elder and Lubotsky (2008) and McEwan and Shapiro (2007) in that we try to identify the persistence of the age effect and the underlying mechanism by comparing the estimates at two points in the school career. If the age-at-test effect dominates, the estimated impact fades over time since the knowledge accumulated in the early years will represent a smaller fraction of the stock of knowledge as children progress through school. By contrast, the absolute and relative age effects imply that late school entrants learn at a higher rate in each grade and perform better relative to their younger counterparts at both points in time.

[3] The local average treatment effect (LATE) framework (introduced by Imbens and Angrist 1994) partitions any population with an instrument into three instrument-dependent subgroups: compliers, always-takers and never-takers. The treated group, those children who start school at the age of seven, is composed of compliers (those children who start school at the age of seven because their birthday falls after the cut-off date) and always-takers (those children who start school at the age of seven voluntarily, irrespective of their birthday). The non-treated group is composed of compliers (those children who start school at the age of six because their birthday falls before the cut-off date) and never-takers (those children who start at the age six voluntarily, irrespective of their birthday). The LATE is not informative about the effect on school starting age on never-takers and always-takers because for these two groups the treatment status is unchanged by the instrument. (Angrist and Pischke 2009)

With the data at hand, we cannot examine the *lifetime effects* of school starting age.[4] However, given the features of early tracking and dead-ends in the Hungarian education system, performance at grades four and eight is important in itself, as it strongly influences the type and quality of secondary education, which in turn has a strong impact on the possibility of going on to further education and finding decent jobs.[5]

2B Estimation strategies

The simplest way to capture the effect of school starting age A_i^s on test score Y_i holding student, family and school background variables X_i constant is by estimating an *ordinary least squares (OLS)* regression similar to (1):

$$Y_i = \beta_1 + \beta_2 A_i^s + X_i'\beta_3 + \varepsilon_i, \qquad i = 1,...,n \quad (1)$$

In countries where there is teacher and parental choice concerning the date of school enrolment, actual school starting age A_i^s and the disturbance term ε_i may be correlated. It may be the case that (i) ambitious parents prefer early enrolment, (ii) wealthier parents are less sensitive to the additional costs of a longer compulsory education and hence may prefer a later start (iii) children with lower (higher) abilities start school a year later (earlier) than proposed by the cut-off date regulation. If the non-random pattern of enrolment is such that, on average, less able children enter school a year later, the OLS estimate β_2 for the effect of school starting age on test score will be downward biased.

Subsequently, recent empirical studies rely on IV estimation to identify the age effect, exploiting the exogenous variation in school starting age driven by the children's month of birth and the cut-off date regulation for enrolment. Accordingly, expected school starting age A_i^E, defined as the age when the child is supposed to start school according to the regulation is used as the instrument for actual school starting age A_i^S. The validity of the IV approach depends on two conditions: (i) $Cov\left(A_i^S, A_i^E\right) \neq 0$ (instrument relevance) and (ii) $Cov\left(\varepsilon_i, A_i^E\right) = 0$ (instrument exogeneity).

Formally, in the IV approach, the first-stage regression involves a regression of A_i^S for individual i on the instrument A_i^E and the vector of control variables to obtain the fitted values $\hat{A}_i^S$:

$$A_i^S = \alpha_1 + \alpha_2 A_i^E + X_i'\alpha_3 + \varepsilon_{Si}, \qquad i = 1,...,n \quad (2)$$

where ε_{Si} is a random disturbance term which contains the unobserved determinants of children's actual school entry age such as physical, intellectual, mental and social maturity.

[4] There is mixed evidence on the long-run effects of school starting age such as highest educational attainment, wages and the probability of employment. See for example Angrist and Krueger (1992), Black at al. (2008), Dobkin and Ferreira (2010) and Fredriksson and Öckert (2005). See also Bertschy et al. (2009) for the effect of cognitive competencies measured while in compulsory education on transition to the labour market.

[5] See Appendix Figure A1 for an overview of the Hungarian school system.

The second stage involves a regression of test score Y_i for individual i on $\hat{A}_i^S$ and X_i:

$$Y_i = \beta_1 + \beta_2 \hat{A}_i^S + X_i' \beta_3 + \varepsilon_i, \qquad i = 1,...,n \quad (3)$$

where ε_i is a random disturbance term which contains the unobserved determinants of student performance such as ability.

As discussed above, the IV model identifies the LATE: the average causal effect of the treatment for those who comply with the assignment mechanism of the instrument (Imbens and Angrist 1994). The LATE may not be representative for the entire population i.e. inference for populations other than that affected by the instrument requires homogeneity assumptions (Angrist and Pischke 2009).

The *control function approach* (Garen 1984) produces consistent estimates of the causal effect for a randomly selected individual i.e. average treatment effect (ATE). The control function approach, in addition to the bias due to correlation between the unobserved determinants of test performance and actual school starting age, accounts for *unobserved heterogeneity in the age effect* and is therefore an extension of the IV approach. In essence, the control function approach makes assumptions about the covariances of the two unobserved components and the observed covariates, and includes additional terms in the test equation to capture these relationships. See Card (1999, 2001) for the application of the control function approach in the context of schooling models.

In order to incorporate heterogeneity in the age effect, the test equation can be rewritten as follows:

$$Y_i = \beta_1 + \beta_{2i} A_i^S + X_i' \beta_3 + \varepsilon_i, \qquad i = 1,...,n \quad (4)$$

$$Y_i = \beta_1 + \overline{\beta}_2 A_i^S + X_i' \beta_3 + \varepsilon_i + \left(\beta_{2i} - \overline{\beta}_2\right) A_i^S, \qquad i = 1,...,n \quad (5)$$

where $\overline{\beta}_2$ is the average age effect and $\varepsilon_i + \left(\beta_{2i} - \overline{\beta}_2\right) A_i^S$ is a composite disturbance term, which represents the two sources of unobserved heterogeneity: the first component of the disturbance term ε_i represents individual characteristics which affect the test score and $\left(\beta_{2i} - \overline{\beta}_2\right)$ represents the heterogeneity in the age effect i.e. β_{2i} is the individual deviation from the average effect $\overline{\beta}_2$. For simplicity of notation we denote the term $\left(\beta_{2i} - \overline{\beta}_2\right) \equiv \eta_i$.

In addition to the IV assumptions of instrument relevance and instrument exogeneity, the model assumes that the two unobserved heterogeneity components are mean independent (uncorrelated) of the instrument A_i^E:

$$E\left[\varepsilon_i \mid A_i^E\right] = 0, \qquad i = 1,...,n \quad (6)$$

and

$$E\left[\eta_i \mid A_i^E\right] = 0, \qquad i = 1,...,n \quad (7)$$

A further assumption is that the conditional expectations of the two unobserved heterogeneity components ε_i and η_i are linear in A_i^S and A_i^E. This assumption in combination with that in equations (6) and (7) yields:

$$E\left[\varepsilon_i \mid A_i^S, A_i^E, X_i\right] = \beta_4 \varepsilon_{Si}, \qquad i = 1,...,n \quad (8)$$

and

$$E\left[\eta_i \mid A_i^S, A_i^E, X_i\right] = \beta_5 \varepsilon_{Si}, \qquad i = 1,...,n \quad (9)$$

where ε_{Si} is defined in equation (2). Adding the two control functions to the test equation yields:

$$Y_i = \beta_1 + \overline{\beta}_2 A_i^s + X_i'\beta_3 + \beta_4 \hat{\varepsilon}_{Si} + \beta_5 A_i^s \hat{\varepsilon}_{Si} + \tilde{\varepsilon}_i, \qquad i = 1,...,n \quad (10)$$

Accordingly, the implementation of the control function regression consists of a two-stage procedure where consistent estimate of the error term $\hat{\varepsilon}_{Si}$ is first obtained from the OLS estimation of Equation (2) and in the second stage, equation (10) is estimated with OLS. The control function approach yields consistent estimates for the average effect of age on test score $\overline{\beta}_2$, which is equivalent to the ATE. Note that estimating the test equation with the additional regressor $\hat{\varepsilon}_{Si}$ but without the interaction of A_i^S and $\hat{\varepsilon}_{Si}$ is numerically equivalent to the standard IV estimation.

3 THE LOCAL CONTEXT

3A School starting age regulation in Hungary

According to the compulsory education law, children who turn six by 31 May are required to start primary school in September, while children born after the cut-off date are required to wait an additional year in order to enrol. The expected school starting age A_i^E is thus generated using the cut-off regulation c and birth month b_i for individual i and can be written as follows:

$$A_i^E = \begin{cases} \dfrac{72+9-b_i}{12} \ \ if \ 1 \le b_i \le c \\ \dfrac{84+9-b_i}{12} \ \ if \ c < b_i \le 12 \end{cases} \qquad i = 1,...,n \quad (11)$$

Given that the cut-off date is May, $c = 5$, A_i^E is between 6.33 years for the youngest children born in May and 7.25 years for the oldest children born in June. Children born in January start school at the age of 6.68 years, and there is a month-for-month decrease in A_i^E until May. Between May and June, A_i^E jumps up by 11 months, and falls again between June and December.

The compulsory education law allows for flexibility concerning the school starting age within certain limits. First, children may start school at the age of six if they turn six years old before 31 December. Second, children born between 1 September and 31 May may delay primary school enrolment by one year. Both early and delayed enrolment may be requested by the parents, and the final decision is made by schools based on the kindergarten teachers' recommendation and/or the opinion of a body of educational counsellors.[6] At this point, it is important to mention that both kindergarten and school education is free of charge, subsequently, there is no additional childcare cost imposed on parents whose children stay in kindergarten instead of starting primary school.

Table A1 in the Appendix presents the school enrolment patterns for the three datasets used in the analysis. As opposed to voluntary early enrolment, voluntary delayed enrolment is common in Hungary: 19 percent of the fourth graders in the NABC sample were enrolled in school a year later voluntarily. Voluntary delayed enrolment is more common among disadvantaged children than among non-disadvantaged ones. Appendix Figures A2 and A3 provide graphical illustrations of A_i^S and A_i^E for disadvantaged and non-disadvantaged eighth graders in the NABC. Compliance with the regulation is weaker in the first six months of the year than in the latter six months. Furthermore, the two months just after the cut-off date are characterized, on average, by early enrolment – a pattern, which is in line with the experience of other countries (see Puhani and Weber 2007 on Germany, for instance).

3B Why school starting age may matter for equality

Prior to starting primary school, the overwhelming majority of Hungarian children go to kindergarten. Attendance is compulsory from age five, and over 95 percent of the five-year-olds and 99 percent of the six-year-olds are actually enrolled. Most children attend the district kindergarten closest to their home: a recent survey (Office of Education 2010) found that only 8.2 percent apply to kindergartens outside their own district. (This figure includes children who go to kindergartens closest to their parents' workplace). Furthermore, the study found no evidence of segregation by social background within the institutions under examination.

When children leave kindergarten, they enter one of the most segregated school systems of Europe. As shown in Jenkins et al. (2008), analysing PISA 2006, the impact of family background on test scores is nowhere as strong within the OECD as in Hungary.[7] The same survey shows that Hungary has the highest ratio of *between schools* to *total* variance in student performance (OECD 2007). Furthermore, using TIMSS and PIRLS data, Csapó et al. (2009) demonstrate that a large part of what seems to be within-school variance at first sight comes from between-class and between-premises variance.

Large differences between schools and classes have evolved as a natural consequence of the *laissez-faire* regulations laid down at the fall of state socialism. Apart from a short period (2005 – 2009), children were allowed to apply to primary schools outside their districts, and schools were permitted

[6] Throughout the paper, delayed/early enrolment for reasons other than the date of birth will be referred to as "voluntary delayed/early enrolment".

[7] Moreover, the percentage of variance in student performance explained by students' socio-economic background is the highest in Hungary within the PISA 2009 sample (OECD 2010, Vol. II, Figure 3.2).

to admit children applying from elsewhere conditional on having admitted the local applicants.[8] In the NABC population, 29 and 31 percent started primary school outside their own districts. Students are further screened at age 10 and 12 when around 3 and 4 – 5 percent of them continue in eight- and six-year academic secondary schools, respectively (Horn 2010). Schools are administered by more than 3,000 local governments in Hungary while the number of actual school districts (municipalities connected by daily commuting) hardly exceeds 150, the number of NUTS-4 regions. The fact that there is no responsible actor at the level of the genuine school districts makes efficient action against segregation difficult, if not impossible (Varga 2009).

The practice of routing disadvantaged children to segregated schools and classes affects the Roma minority disproportionately. (The share of Roma among the children of low-educated parents amounts to 37 percent according to Kertesi and Kézdi 2010). Havas and Liskó (2005) estimate that while there was a twofold increase in the share of Roma children in primary schools between 1980 and 2003, the number of 100 percent Roma classes grew by a factor of eight. Furthermore, they found the share of Roma children to be 30 percent in normal classes, 15 percent in special classes for high-achievers and 70 percent in special classes for low-achievers.

The children of low-educated parents have poor chance of attending better schools for several reasons: they start with an obvious handicap at the formal and informal entry examinations, their financial resources are insufficient to cover the costs of commuting to a distant school and bear the expenses of extracurricular activities customary in middle-class schools. Furthermore, many of them are discriminated on the basis of skin colour. Staying in the less segregated environment of the kindergarten for a further year potentially reduces their handicap, and helps them keep up with their schoolmates on top of the general (age-at-test, absolute and relative) age effects discussed earlier.

4 Data

For the empirical analysis, data is drawn from three different surveys (NABC, PIRLS, TIMSS) of students tested at the end of the academic year. To arrive at the working sample for each of the data sources, we include only those students who started school between the ages of six and seven.[9] Excluding those who started school at an age younger than six or older than seven, amounts to dropping less than two percent of the samples. To distinguish between disadvantaged and non-disadvantaged subsamples, we use the mother's level of education, which is based on the International Standard Classification of Education (ISCED-97) in all datasets. The disadvantaged subsample consists of students whose mothers completed at most lower secondary education (ISCED2), which in Hungary amounts to completing eight years of primary school. The non-disadvantaged subsample consists of students whose mothers attained a tertiary degree (ISCED5 or ISCED6).

[8] In 2005 – 2009, the regulations tried to reduce schools' freedom of choice by putting a ban on formal entry exams and prescribing that priority should be given to children from the school's own district, in the first place, and socially disadvantaged children from other districts, in the second. Other applicants could be admitted on the basis of random draw. These regulations have been withdrawn by the government in office since April 2010.

[9] Estimation based on samples including all children independent of school starting age yields similar results.

4A National Assessment of Basic Competencies (NABC), 2006

The main results are based on the 2006 NABC, which covers all grade four and eight students in Hungary unless absent at the date of testing.[10] The NABC sample is exceptionally large: we have about 80,000 observations at each grade level (after we arrive at our working sample). A further advantage of the dataset is that it includes information on both the exact date of birth and grade repetition. Consequently, actual school starting age can be computed accurately.

As the main dependent variable, we use a composite cognitive-academic test score. At the grade four level, the composite test score is the sum of five separate test scores: reading, writing, arithmetic, combinative thinking and analytical skills. Each of the five test scores falls to the range of 0 – 100 points. At the grade eight level, the composite test score is the average of two test scores: literacy and mathematics. The average and standard deviation of the latter two tests is set at 500 and 100 points, respectively. As the range of the test scores differs between the two grades, we express the estimation results as the percentage of the standard deviation of the respective test scores.

The NABC contains a large set of background variables, which come from school and student questionnaires. Therefore, in addition to the specification where only the school starting age is used as a regressor (Specification 1), two alternative specifications are estimated. Specification 2 includes basic child, family, household and school level variables. Specification 3 includes seven additional controls for specific items in the *Early Adolescent Home Observation for Measurement of the Environment (EA HOME) Inventory* (Bradley et al. 2000) designed for the ages 10 to 15.[11] Following Kertesi and Kézdi (2009), these seven control variables are chosen in order to capture the child's variety of experience, instructional activities and learning materials. Table 1 and Appendix Table A1 provide a list of the control variables for the different specifications and summary statistics, respectively.

The differences concerning test scores, family characteristics, home environment, learning materials and instructional activities follow the expected pattern: disadvantaged children attain lower test scores, have more siblings, have fewer educational resources at home, are less likely to participate in extra-curricular activities, read less often for enjoyment, spend less time with their families going to exhibitions, concerts and other cultural events, and their fathers have lower education levels. Although the expected school starting age is identical for the two subsamples, delayed school entry is slightly more common for disadvantaged children.

Note that as kindergarten attendance is measured on the category level (none, 0 – 1, 1 – 2 and more than 2 years), we can control for insufficient pre-school education but not for variation within the top category, which comprises the vast majority of children. Within the disadvantaged subsample of fourth graders, 76 percent spent more than two years in kindergarten while the respective share was 93 percent within the non-disadvantaged group. The differences by school starting age are minimal: 2.5 and 2.2 percentage points in the two subsamples, respectively, implying that our estimates of the

[10] See Kertesi and Kézdi (2010) for a detailed description of the NABC dataset.

[11] The HOME Inventory (first developed and used by Elardo et al. 1975) was designed to measure the quality and quantity of stimulation and support available to a child in the home environment. The EA HOME contains 60 items clustered into 7 subscales: (1) physical environment, (2) learning materials, (3) modelling, (4) instructional activities, (5) regulatory activities, (6) variety of experience and (7) acceptance and responsivity.

effect of delayed start on academic performance are effectively *uncontrolled* for the duration of kindergarten attendance.[12] Given that the overwhelming majority of children aged six and seven go to kindergarten, as previously mentioned, we can take it for granted that late starters had longer records of pre-school education than their counterparts with similar social background. Therefore, the estimated coefficients capture the effect of longer kindergarten attendance in addition to the age-at-test, absolute and relative age effects.

Table 1 Specifications and data sources

Specification	Data source	Regressors
Specification 1	NABC, 2006	School starting age
Specification 2	NABC, 2006	School starting age, gender, years of kindergarten attendance, living with both parents, number of siblings, father's education, presence of computer at home, number of vacations in 2005, number of books at home, child has books, class size, class size squared, NUTS-3 region dummies at the school level
Specification 3	NABC, 2006	Specification 2 plus family plays music / sings together, family goes to the cinema / theatre / concerts, family goes to exhibitions / museums, family discusses what happens in school, child attends extra-curricular activities, child's reading habits, child has a desk
Specification 4	PIRLS, 2001	School starting age, gender, index of early home literary activities, number of people living at home, father's education, presence of computer at home, family has a car, number of books at home, child has books
Specification 5	TIMSS, 2003	School starting age, gender, number of people living at home, father's education, presence of computer at home, family has a VCR, number of books at home

4B Progress in International Reading Literacy Study (PIRLS), 2001

The second dataset used at the grade four level is the 2001 wave of the PIRLS, which is available for 35 countries.[13] For the empirical analysis, data from the Student Questionnaire (which contains the reading test scores and basic student background information) and the Home Survey (which contains demographic and socio-economic indicators) are merged. The outcome variable is the reading score, which is standardized so that the mean is equal to 500 and the standard deviation equals 100 when all countries are weighted equally. The control variables included in the regression model (Specification 4) are similar to the variables in Specification 2 (NABC data) and are listed in Table 1.

[12] Within the disadvantaged subsample of fourth graders, 74.65 and 77.08 percent of those who started school at age six and age seven attended kindergarten for over two years, respectively. The corresponding figures for the non-disadvantaged subsample are 92.19 and 94.40 percent, respectively. For eighth graders the corresponding figures are 75.05 and 76.97 percent within the disadvantaged subsample, and 88.82 and 90.03 percent within the non-disadvantaged subsample, respectively.

[13] For an extensive description of the PIRLS dataset, testing procedure, scoring guide see Gonzalez and Kennedy (Eds.) (2003).

Columns (1) and (2) of Table A3 in the Appendix provide summary statistics, which confirm the picture outlined above based on the NABC data.

4C Trends in Mathematics and Science Study (TIMSS), 2003

The third dataset used in the analysis is the 2003 wave of the TIMSS, which has been conducted in 48 countries at the grade four and eight levels.[14] A drawback of the TIMSS data is that information on parental education is not available at the grade four level. Subsequently, we only use the eighth graders in our analysis. The outcome variable of interest is the mathematics score, the international mean of which is set at 500 and the standard deviation at 100. In the regression model, denoted as Specification 5, we use control variables similar to those in Specifications 2 (NABC) and 4 (PIRLS) (see Table 1). Columns (3) and (4) of Appendix Table A3 provide summary statistics of the variables used in the analysis, which differ by maternal education as expected.

5 Estimation Results

5A OLS versus IV estimates

The OLS and IV estimation results for the full sample of students are reported in Table 2, expressed as percentage of the standard deviation of the full sample test score. The OLS estimate in the regression model without controls (Panel A, Column 1, Specification 1) indicates a *negative* correlation between actual school starting age and test score: the disadvantage of delayed enrolment amounts to around 22 percent of the standard deviation of the composite cognitive-academic test score. With the inclusion of control variables, the OLS estimate decreases in absolute value.

The LATE coefficient estimate for Specification 1 (Panel B, Column 1) implies that delayed start *increases* the composite cognitive-academic test score by around 44 percent of the standard deviation. The finding that the OLS estimate is downward biased compared to the IV estimate is in line with the international literature analyzing grade four students. (See, for example, Bedard and Dhuey 2006 and Puhani and Weber 2007). A comparison of Specifications 1 – 3 implies that the IV estimates are robust to the inclusion of additional covariates. Note that the *F*-statistics (Appendix Table A4, Panel A, Column 1) testing the significance of the instrument in the first-stage regressions exceed the threshold level of 10 (Staiger and Stock 1997, Stock et al. 2002) thus there is no indication of weak instruments.

At the grade eight level, the downward bias of the OLS estimate, the robustness of the IV estimate as well as instrument relevance are confirmed (see Table 2, Column 2 and Appendix Table A4, Panel B, Column 1).[15]

[14] For an extensive discussion of the TIMSS dataset, the content and cognitive domains tested for mathematics, the test design and scoring guide see Martin (Ed.) (2005).

[15] Note that the downward bias of the OLS estimate is confirmed in the subsample analysis at both grade levels. All estimation results available upon request.

Table 2 OLS and IV estimation results, full sample, NABC

	Grade 4 (1)	Grade 8 (2)
A. OLS estimates	N = 83425	N = 81236
Specification 1	**- 22.03**	**- 22.15**
Specification 2	**- 7.26**	**- 14.57**
Specification 3	**- 7.19**	**- 13.87**
B. IV estimates		
Specification 1	**44.16**	**27.35**
Specification 2	**41.28**	**26.18**
Specification 3	**40.03**	**26.31**

Notes Estimation results expressed as percentage of the standard deviation of the full sample test score. Bold figures are significant. The dependent variable is the composite cognitive-academic test score for all specifications and grade levels. Control variables for the different specifications are listed in Table 1.

5B IV estimates, subsample analysis

Table 3 reports the IV estimates for subsamples of fourth and eighth graders distinguished by maternal education, expressed as the percentage of the standard deviation of the full sample test score.

The *grade four* LATE for disadvantaged children indicates a large positive effect of school starting age on academic performance, exceeding 80 percent of the standard deviation of the composite cognitive-academic test score for all specifications. Again, the inclusion of controls has little impact on the estimated age effect. The LATE estimates for children with highly educated mothers indicate that non-disadvantaged children who enter school a year later gain less from starting school later than their disadvantaged counterparts in grade four (around 27 percent of the standard deviation of the test score). The equality of the coefficient estimates across the two subsamples based on Specification 1 is rejected (see Appendix Table A5). Therefore, the estimation results support our key hypothesis that disadvantaged children have more to gain from starting school later than their non-disadvantaged counterparts.

Turning to *grade eight*, the LATE estimate for disadvantaged students remains statistically significant and large: around 35 percent of the standard deviation of the composite test score for the full specification (Specification 3). The conclusion that non-disadvantaged children gain less from starting school a year later in terms of academic competencies than their disadvantaged counterparts still holds, and the equality of the parameter estimates across the two subsamples can be rejected.

Comparing the magnitude of the grade four and eight results implies that the benefit of starting school at the age of seven instead of six fades as children progress through school. Note however that in the absence of longitudinal data, the comparison in time should be treated with caution.

Table 3 IV estimation results, NABC

	Grade 4 (1)	Grade 8 (2)
A. Disadvantaged subsample	N = 14973	N = 12332
Specification 1	**96.03**	**38.59**
Specification 2	**81.93**	**37.75**
Specification 3	**80.19**	**35.24**
B. Non-disadvantaged subsample	N = 16035	N = 17409
Specification 1	**26.25**	**21.38**
Specification 2	**27.63**	**19.79**
Specification 3	**27.15**	**21.94**

Notes Estimation results expressed as percentage of the standard deviation of the full sample test score. Bold figures are significant. The dependent variable is the composite cognitive-academic test score for all specifications and grade levels. Control variables for the different specifications are listed in Table 1.

6 Sensitivity Analysis

We check the sensitivity of our main result concerning between group differences in the age effect in four different ways: (1) using an alternative estimation approach, (2) estimating the test equation with two alternative outcome variables available in the NABC data, (3) using discontinuity samples and (4) carrying out the estimation using two alternative datasets, namely, the PIRLS and TIMSS.

6A Control function estimates

As discussed above, we further analyse the data following the control function approach proposed in Garen (1984) to produce estimates of the ATE, which are reported in Table 4.

Starting with the *grade four* results (Column 1), the ATE estimates are positive and statistically significant for disadvantaged children and statistically insignificant for non-disadvantaged ones. For both subsamples, the ATE estimates are lower than the corresponding LATE estimates. At the *grade eight* level (Column 2), the ATE estimates still imply a statistically significant benefit to starting school later for the average disadvantaged child, which is smaller in magnitude than in grade four. The benefit amounts to 42 and 16 percent of the standard deviation of the full sample composite academic-competencies test scores in grades four and eight, respectively. The ATE estimates for both subgroups are below the corresponding LATE estimates.

Overall, although smaller in magnitude, the ATE estimates confirm the LATE estimates along two lines. First, disadvantaged children have more to gain from starting school later than their non-disadvantaged counterparts in both grades. Second, the advantage of delayed enrolment seems to decrease as disadvantaged children progress through school. A comparison of the magnitudes of the LATE and corresponding ATE estimates suggests a *negative selection* into voluntary delayed enrolment.

Table 4 Control function approach estimation results, NABC

	Grade 4 (1)	Grade 8 (2)
A. Full sample	N = 83425	N = 81236
Specification 1	2.52	**10.38**
Specification 2	**16.34**	**14.12**
Specification 3	**15.31**	**14.52**
B. Disadvantaged subsample	N = 14973	N = 12332
Specification 1	**33.26**	12.67
Specification 2	**46.15**	**18.55**
Specification 3	**42.10**	**15.88**
C. Non-disadvantaged subsample	N = 16035	N = 17409
Specification 1	0.40	10.56
Specification 2	8.05	10.03
Specification 3	8.61	**12.80**

Notes Estimation results expressed as percentage of the standard deviation of the full sample test score. Bold figures are significant. Standard errors are computed by 500 bootstrap replications. The dependent variable is the composite cognitive-academic test score for all specifications and grade levels. Control variables for the different specifications are listed in Table 1.

6B Mathematics and reading test scores

As a further robustness check, we separate the composite test scores into literacy and mathematics components, and use the latter two measures as dependent variables. More specifically, the grade four "reading test score" is the sum of the reading and writing test scores, and the "mathematics test score" is sum of the test scores for arithmetic, combinative thinking and analytical skills. At grade eight, we simply use the original test scores for reading and mathematics.

The estimation results are presented in Table 5. First of all, note that school starting age has a significant effect on both reading and mathematics test scores in both grades for both subsamples. For disadvantaged children, we see a large benefit of delayed primary school enrolment in grade four, exceeding 84 and 35 percent of the standard deviation of the mathematics and reading tests, respectively. Whereas their benefit fades substantially by grade eight in terms of mathematics, it remains relatively stable across the two grades in terms of reading. The latter result also holds for the non-disadvantaged subsample.

Although our key hypothesis is confirmed by the estimation results, it must be pointed out, that in grade eight the benefit of delayed enrolment in terms of mathematical competencies is only slightly higher for disadvantaged children, as opposed to reading.

Table 5 IV estimation results, NABC, mathematics and reading scores as dependent variables

	Grade 4	Grade 8
	(1)	(2)
A. Full sample	N = 83425	N = 81236
Specification 1, mathematics score	**44.27**	**22.61**
Specification 2, mathematics score	**41.76**	**21.38**
Specification 3, mathematics score	**40.48**	**21.35**
Specification 1, reading score	**24.73**	**27.93**
Specification 2, reading score	**22.19**	**27.00**
Specification 3, reading score	**21.81**	**27.29**
B. Disadvantaged subsample	N = 14973	N = 12332
Specification 1, mathematics score	**95.89**	**27.63**
Specification 2, mathematics score	**85.37**	**26.13**
Specification 3, mathematics score	**84.05**	**23.52**
Specification 1, reading score	**54.77**	**43.75**
Specification 2, reading score	**37.87**	**43.70**
Specification 3, reading score	**35.83**	**41.67**
C. Non-disadvantaged subsample	N = 16035	N = 17409
Specification 1, mathematics score	**24.40**	**19.37**
Specification 2, mathematics score	**26.67**	**17.67**
Specification 3, mathematics score	**26.30**	**19.18**
Specification 1, reading score	**19.49**	**20.13**
Specification 2, reading score	**18.03**	**18.89**
Specification 3, reading score	**17.49**	**21.35**

Notes Estimation results expressed as percentage of the standard deviation of the full sample test score. Bold figures are significant. Control variables for the different specifications are listed in Table 1.

6C Discontinuity samples

A common critique of using expected school starting age as an instrument is the possible direct effect of month of birth on educational outcomes, which would invalidate the instrument, as argued in Bound et al. (1995) and Bound and Jaeger (2000) among others.[16] In order to check the robustness of the results for the subsamples, we use discontinuity samples (as for example Elder and Lubotsky 2009, Puhani and Weber 2007, Strøm 2004) i.e. subsamples of students born two months before and after the cut-off date. Using discontinuity samples has the advantage that (a) the possibility of birth timing is limited and (b) even if month of birth directly affects test scores, this association will not lead to bias as long as the children born close to the cut-off date are similar in unobservable characteristics (Elder and Lubotsky 2008). The four-month window is chosen to assure enough

[16] The non-randomness of the month of birth cannot be considered an established result. For instance, Angrist and Kruger (1992) cite studies providing opposing evidence: one concluding that “genetic-season-of birth effect exists because genetically inferior individuals are less able to contain their sexual passions in the summer”, and an opposing one claiming that “the seasonal pattern of children’s birth is unrelated to the wealth and marital status of their parents”.

observations for subsample analysis. The results based on the discontinuity samples for fourth and eighth graders are reported in Table 6. Despite the relatively large sample sizes (ranging between 4,126 and 5,936), the coefficients are generally less precisely estimated than those reported for the full subsample, as we are only using about one third of the observations. Note that in the discontinuity samples the instrument is still strong enough (*F*-statistic is larger than the threshold value of 10 as shown in Table A4 of the Appendix). The general conclusions drawn based on the discontinuity sample estimates remain identical to those based on the full sample: disadvantaged children gain more from delayed primary school enrolment than their non-disadvantaged counterparts, but the benefit is smaller in grade eight.

Table 6 IV estimation results, NABC, discontinuity samples: born April – July

		Grade 4 (1)	Grade 8 (2)
	A. Full sample	N = 26775	N = 27313
Specification 1		**34.00**	**25.47**
Specification 2		**32.37**	**24.78**
Specification 3		**32.37**	**25.50**
	B. Disadvantaged subsample	N = 4879	N = 4126
Specification 1		**51.49**	**38.99**
Specification 2		**46.56**	**37.97**
Specification 3		**47.20**	**35.88**
	C. Non-disadvantaged subsample	N = 5132	N = 5936
Specification 1		**25.71**	13.60
Specification 2		**31.49**	**14.22**
Specification 3		**33.10**	**17.87**

Notes Estimation results expressed as percentage of the standard deviation of the full sample test score. Bold figures are significant. The dependent variable is the composite cognitive-academic test score for all specifications and grade levels. Control variables for the different specifications are listed in Table 1.

6D PIRLS and TIMSS data

As a final robustness check, the regression analysis is carried out using the widely used datasets in the existing international studies. The estimation results based on the PIRLS and TIMSS data are reported in Table 7. The coefficient estimates, although four out of six are statistically insignificant because of the small sample sizes, are in line with the estimation results based on the NABC dataset: those estimated for disadvantaged students are larger than those for non-disadvantaged children. To put the Hungarian estimates into perspective, note that the LATE estimate for German grade four students based on the PIRLS data is around 40 percent of the standard deviation of the test score (in Puhani and Weber 2007). Furthermore, Bedard and Dhuey (2006) find statistically significant LATE for a number of OECD countries based on the TIMSS data, ranging from around 13 percent to around 35 percent of the international standard deviation of the mathematics test score for the full sample of grade eight students in Italy and New Zealand respectively.[17]

Table 7 PIRLS and TIMSS results

	LATE
A. PIRLS, grade four	
Specification 4, Full sample, N = 4452	**27.90**
Specification 4, Disadvantaged subsample, N = 729	**89.65**
Specification 4, Non-disadvantaged subsample, N = 926	23.74
B. TIMSS, grade eight	
Specification 5, Full sample, N = 3158	9.93
Specification 5, Disadvantaged subsample, N = 430	18.56
Specification 5, Non-disadvantaged subsample, N = 784	8.40

Notes Estimation results expressed as percentage of the standard deviation of the full sample test score. Bold figures are significant. The dependent variable is the reading and mathematics test score with the PIRLS and TIMSS data, respectively. Control variables for the different specifications are listed in Table 1.

7 Closing remarks

We found that low-status children gain significantly more from starting school later than their high-status counterparts, and this result proved to be robust to changes in the method of estimation, field of testing, choice of sample and data. The finding that late starters *generally* gain is not new and can be potentially explained by the facts that they are older at testing, more productive in attaining a curriculum geared at the average child and older than their classmates at any point in time. However, these mechanisms do not seem to explain the sizeable between group differences found in our data. The higher efficiency of kindergartens in developing the relative skills of low-status children appears to us as the only plausible explanation of why late starters perform much better within the low-status group, and why their advantage decreases over time.

Such an explanation is consistent with the findings of education research. While the Hungarian school system follows the 'Prussian tradition' in being curriculum-oriented and responding to those falling behind by punishment, segregation and exclusion (routing to class repetition, directing the laggards to special classes and less demanding schools), kindergartens put stronger emphasis on the development of basic competencies and do so in a playful and more cooperative environment. (See a general overview in Nagy 2009).[18] We think that our results are indicative of this contrast, and call for more inclusive and less segregated education in the primary school.

[17] The standard deviation for the Hungarian samples of the PIRLS and TIMSS is lower than the standard deviation in the German sample of the PIRLS and international TIMSS data thus the difference between the estimates based on the latter data and the Hungarian one is larger when expressed as percentage of the standard deviation.

[18] The comprehensive kindergarten network came into being much later than the school system and its program was strongly influenced by the Montessori method.

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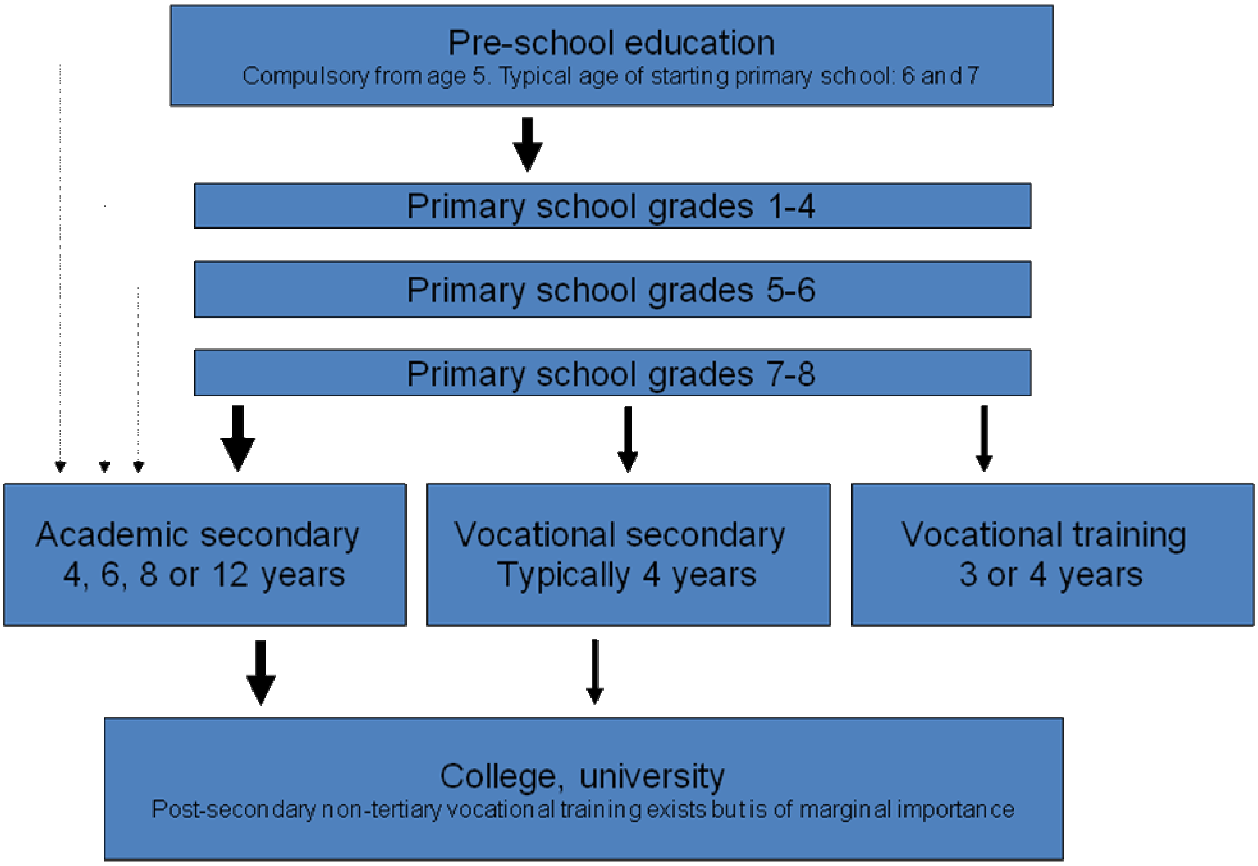
Appendix

A The Hungarian education system

Figure A1 The Hungarian education system



Notes: A secondary school leaving exam is required for those applying to higher education. Vocational training schools do not prepare their students for the secondary school leaving exam, but their graduates can participate in preparatory courses voluntarily.

B Enrolment patterns

Figure A2 Average actual school starting age versus average expected school starting age, disadvantaged children, grade eight

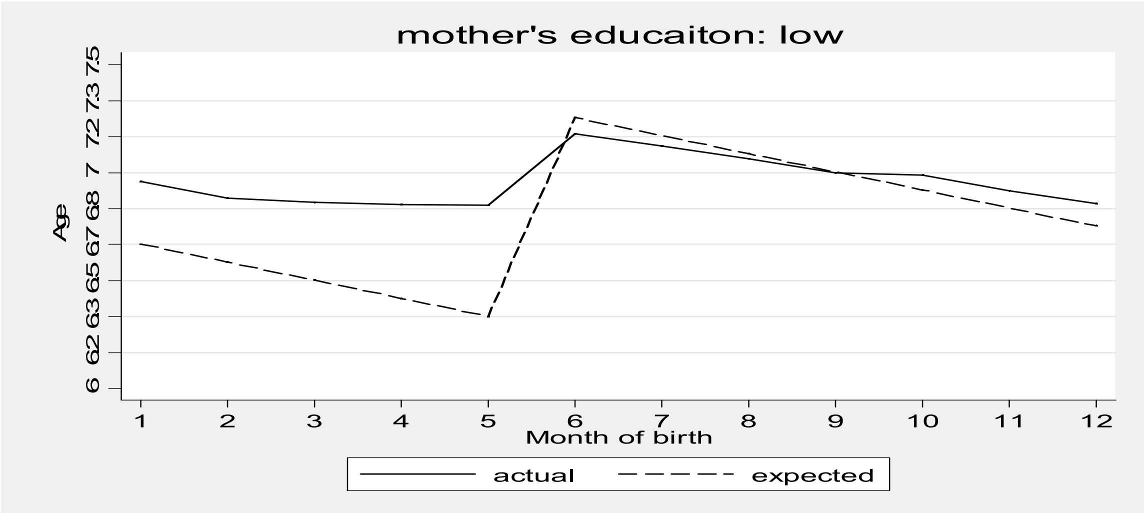


Figure A3 Average actual school starting age versus average expected school starting age, non-disadvantaged children, grade eight

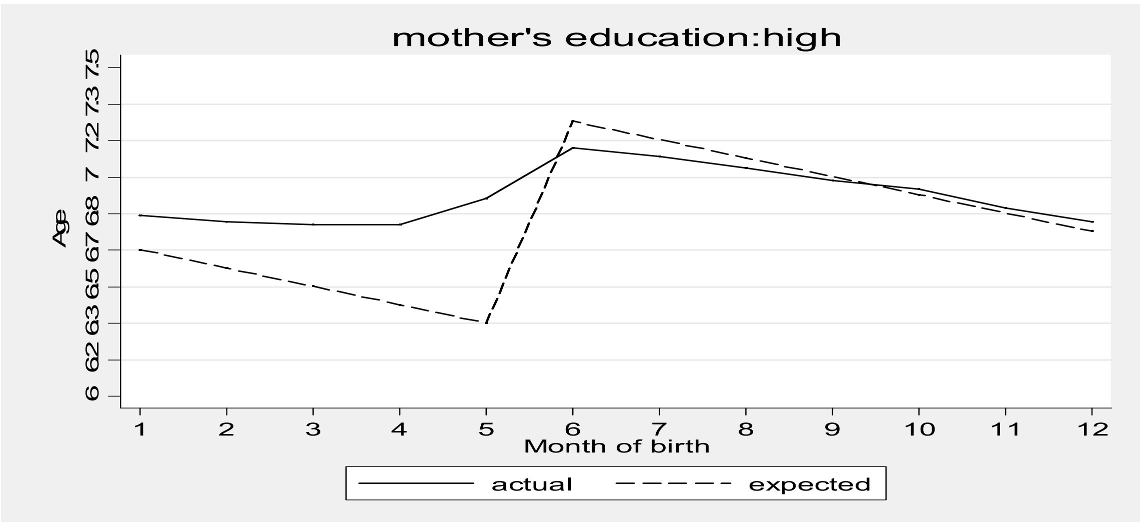


Table A1 Enrolment patterns (percent)

	Full sample	Disadvantaged subsample	Non-disadvantaged subsample
	(1)	(2)	(3)
A. Grade 4, NABC (2006), primary school enrolment in 2002			
According to regulation: age of six	45.82	40.13	48.19
According to regulation: age of seven	33.74	33.25	33.51
Voluntary early enrolment	1.20	1.12	1.76
Voluntary delayed enrolment	19.25	25.51	16.53
B. Grade 8, NABC (2006), primary school enrolment in 1998			
According to regulation: age of six	51.13	47.83	51.61
According to regulation: age of seven	33.14	33.44	32.49
Voluntary early enrolment	1.60	1.31	2.27
Voluntary delayed enrolment	14.13	17.42	13.63
C. PIRLS (2001), primary school enrolment in 1997			
According to regulation: age of six	52.11	46.09	54.43
According to regulation: age of seven	31.38	29.22	31.43
Voluntary early enrolment	1.75	1.92	2.27
Voluntary delayed enrolment	14.76	22.77	11.88
D. TIMSS (2003), primary school enrolment in 1995			
According to regulation: age of six	52.82	45.35	55.36
According to regulation: age of seven	32.01	35.12	29.85
Voluntary early enrolment	1.84	1.16	2.81
Voluntary delayed enrolment	13.33	18.37	11.99

C Descriptive statistics

Table A2 Descriptive Statistics, NABC

	Grade 4		Grade 8	
	(1)	(2)	(3)	(4)
Mean test score: reading	137.87	149.83	443.94	562.88
	(23.34)	(20.09)	(87.33)	(90.41)
Mean test score: mathematics	127.32	187.69	436.94	561.62
	(45.12)	(54.04)	(83.66)	(96.92)
Mean test score: composite cognitive-academic skills	265.20	337.52	440.44	562.25
	(59.34)	(64.44)	(77.75)	(84.89)
Gender: Male	47.31	50.08	43.28	48.95
Gender: Female	52.69	49.92	56.71	51.04
Gender: Missing	0.00	0.00	0.01	0.01
Years attended kindergarten: Zero	0.61	0.14	0.67	0.30
Years attended kindergarten: At most one	6.56	1.59	6.65	2.10
Years attended kindergarten: Between one and two	16.20	4.75	14.75	7.09
Years attended kindergarten: More than two	76.04	93.30	76.98	90.00
Years attended kindergarten: Missing	0.59	0.22	0.95	0.51
Child lives with both parents: Yes	70.51	80.24	68.25	75.64
Child lives with both parents: No	27.40	19.38	30.76	24.13
Child lives with both parents: Missing	2.09	0.38	1.00	0.23
Number of siblings: Zero	6.83	14.53	6.34	13.01
Number of siblings: One	23.38	50.39	27.73	52.70
Number of siblings: Two	30.02	23.67	31.72	23.39
Number of siblings: Three	16.73	6.92	16.72	6.61
Number of siblings: More than three	21.39	3.75	16.23	3.63
Number of siblings: Missing	1.66	0.73	1.26	0.66
Father's education: At most eight years of primary school	47.41	0.89	37.33	0.76
Father's education: Vocational degree	40.28	16.09	48.24	16.77
Father's education: Secondary school degree	5.36	26.35	6.71	25.77
Father's education: Tertiary degree	1.18	54.82	1.14	54.56
Father's education: Missing	5.77	1.85	6.58	2.14
Presence of computer at home: Yes	45.88	95.30	58.32	96.97
Presence of computer at home: No	40.90	3.27	37.83	1.83
Presence of computer at home: Missing	13.22	1.43	3.85	1.20
Number of vacations in the past year: Zero	38.54	5.21	29.98	6.07
Number of vacations in the past year: One	25.26	19.71	26.73	20.78
Number of vacations in the past year: Two	15.46	26.62	20.25	28.20
Number of vacations in the past year: Three or more	17.71	46.96	21.21	43.48
Number of vacations in the past year: Missing	3.03	1.50	1.83	1.47
Number of books at home: Less than 50	43.41	0.68	34.62	0.59
Number of books at home: Around 50	23.11	1.77	24.51	1.54
Number of books at home: 51 – 150	18.16	9.94	22.71	9.40
Number of books at home: 151 – 300	5.93	16.33	8.77	15.41
Number of books at home: 301 – 600	2.77	24.71	4.05	22.99
Number of books at home: 601 – 1000	1.18	24.43	2.22	24.77
Number of books at home: More than 1000	0.73	21.49	1.13	24.57
Number of books at home: Missing	4.72	0.65	1.99	0.74
Child has books: Yes	85.27	99.35	86.59	98.78
Child has books: No	10.29	0.28	11.65	0.79
Child has books: Missing	4.44	0.37	1.76	0.43

				Table A2 continued
Class size	19.96	24.15	20.93	25.41
Family plays music / sings together: Yes	58.88	64.08	49.76	43.24
Family plays music / sings together: No	31.72	33.76	46.52	54.59
Family plays music / sings together: Missing	9.40	2.16	3.72	2.17
Family goes to the cinema / theatre / concerts: Yes	30.43	76.07	37.51	72.54
Family goes to the cinema / theatre/ concerts: No	60.02	21.76	59.28	25.55
Family goes to the cinema / theatre/ concerts: Missing	9.55	2.17	3.20	1.91
Family goes to exhibitions / museums: Yes	18.75	64.53	18.47	55.29
Family goes to exhibitions / museums: No	70.36	33.00	77.82	42.34
Family goes to exhibitions / museums: Missing	10.89	2.47	3.71	2.37
Family discusses daily / almost daily what happens in school: Yes	68.50	88.44	54.72	72.04
Family discusses daily / almost daily what happens in school: No	25.27	10.22	42.83	26.58
Family discusses daily / almost daily what happens in school: Missing	6.23	1.34	2.45	1.38
Child attends extra-curricular activities: Yes	39.33	77.72	41.32	73.08
Child attends extra-curricular activities: No	52.21	20.84	55.34	25.48
Child attends extra-curricular activities: Missing	8.46	1.43	3.34	1.44
Child's reading habits: Currently reads something for enjoyment	28.89	60.52	18.85	47.47
Child's reading habits: Last time read something for enjoyment was last month	24.42	19.38	22.17	22.30
Child's reading habits: Last time read something for enjoyment was during this academic year	24.54	14.04	26.57	18.03
Child's reading habits: Used to read for enjoyment	10.67	3.47	19.45	8.47
Child's reading habits: Never read anything for enjoyment	9.15	1.80	11.77	2.84
Child's reading habits: Missing	2.34	0.79	1.20	0.88
Child has a desk: Yes	73.68	96.36	81.65	97.66
Child has a desk: No	20.95	3.15	16.79	1.87
Child has a desk: Missing	5.37	0.49	1.57	0.47
Mean Actual school starting age[a]	7.05 (0.38)	6.95 (0.35)	6.96 (0.37)	6.91 (0.35)
Mean expected school starting age[a]	6.80 (0.29)	6.80 (0.28)	6.80 (0.29)	6.80 (0.29)
Sample size	14973	16035	12332	17409

Notes Column (1) refers to the subsample of fourth graders with low-educated mothers, Column (2) refers to the subsample of fourth graders with high-educated mothers, Column (3) refers to the subsample of eighth graders with low-educated mothers and Column (4) refers to the subsample of eighth graders with high-educated mothers, whereby low and high education correspond to at most eight years of primary school and to tertiary degree respectively. Standard deviations are in parentheses for continuous variables.[a] School starting age measured in years.

Table A3 Descriptive Statistics for PIRLS and TIMSS

	Grade 4 (PIRLS)		Grade 8 (TIMSS)	
	(1)	(2)	(3)	(4)
Mean test score: reading	507.37	581.85		
	(54.41)	(49.96)		
Mean test score: mathematics			480.44	583.73
			(69.61)	(68.94)
Gender: Male	53.91	48.70	43.49	49.74
Gender: Female	45.95	51.19	56.51	50.26
Gender: Missing	0.14	0.11	0.00	0.00
Index of early home literary activities: High	49.11	71.49		
Index of early home literary activities: Medium	36.90	23.65		
Index of early home literary activities: Low	11.25	4.00		
Index of early home literary activities: Missing	2.74	0.86		
Number of people at home: Two or three	6.04	17.39	21.16	21.56
Number of people at home: Four	30.32	44.49	30.93	49.36
Number of people at home: Five	30.45	21.92	25.58	18.49
Number of people at home: More than five	25.38	11.12	18.14	8.67
Number of people at home: Missing	7.82	5.08	4.19	1.91
Father's education: At most primary school	39.09	0.97	40.93	0.89
Father's education: Vocational degree (Secondary school degree for TIMSS)	41.29	16.95	36.74	13.39
Father's education: Secondary school degree	7.54	23.87		
Father's education: Tertiary degree	1.23	54.75	0.93	58.93
Father's education: Missing	10.84	3.46	21.40	26.79
Presence of computer at home: Yes	29.36	79.05	44.65	92.73
Presence of computer at home: No	68.18	19.44	51.86	6.51
Presence of computer at home: Missing	2.47	1.51	3.49	0.77
Family has a car: Yes	42.66	83.80		
Family has a car: No	55.28	14.90		
Family has a car: Missing	2.06	1.30		
Number of books at home: Less than 100	70.78	24.84	79.77	15.82
Number of books at home: 100 or more	23.32	72.68	20.23	83.80
Number of books at home: Missing	5.90	2.48	0.00	0.38
Child has books: Yes	88.48	98.06		
Child has books: No	8.92	0.86		
Child has books: Missing	2.61	1.08		
Family has a VCR: Yes			47.87	88.01
Family has a VCR: No			51.63	11.73
Family has a VCR: Missing			0.70	0.26
Mean Actual school starting age[a]	7.00 (0.41)	6.89 (0.35)	6.98 (0.38)	6.88 (0.36)
Mean expected school starting age[a]	6.79 (0.28)	6.79 (0.29)	6.81 (0.29)	6.79 (0.29)
Sample size	729	926	430	784

Notes Column (1) refers to the subsample of fourth graders with low-educated mothers (PIRLS data), Column (2) refers to the subsample of fourth graders with high-educated mothers (PIRLS data), Column (3) refers to the subsample of eighth graders with low-educated mothers (TIMSS data) and Column (4) refers to the subsample of eighth graders with high-educated mothers (TIMSS data), whereby low and high education correspond to at most eight years of primary school and to tertiary degree respectively. Standard deviations are in parentheses for continuous variables.[a] School starting age measured in years.

D Statistical tests

Table A4 First-stage results, Specification 1

	Full sample	Disadvantaged subsample	Non-disadvantaged subsample	Discontinuity sample	Discontinuity sample, disadvantaged subsample	Discontinuity sample, non-disadvantaged subsample
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Grade 4, NABC, 2006			
A^E	0.24***	0.14***	0.27***	0.26***	0.21***	0.25***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	83425	14973	16035	26775	4879	5132
F-statistic[a]	3134.87	156.11	777.02	2260.73	278.11	353.61
Prob *F* > 0	0.000	0.000	0.000	0.000	0.000	0.000
			B. Grade 8, NABC, 2006			
A^E	0.39***	0.34***	0.36***	0.37***	0.36***	0.32***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	81236	12332	17409	27313	4126	5936
F-statistic[a]	9444.35	970.54	1677.97	4231.03	620.31	639.04
Prob *F* > 0	0.000	0.000	0.000	0.00	0.000	0.000
			C. PIRLS, 2001			
A^E	0.43***	0.25***	0.42***			
	(0.02)	(0.05)	(0.04)			
N	4452	729	926			
F-statistic[a]	557.16	22.77	132.86			
Prob *F* > 0	0.000	0.000	0.000			
			D. TIMSS, 2003			
A^E	0.46***	0.36***	0.43***			
	(0.02)	(0.06)	(0.04)			
N	3158	430	784			
F-statistic[a]	485.04	34.36	105.28			
Prob *F* > 0	0.000	0.000	0.000			

Notes A^E is expected school starting age. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level. Standard errors are in parentheses. [a] The *F*-statistic corresponds to a test of the null hypothesis that the instrument is zero.

Table A5 Chow test results, LATE estimates, Specification 1

	Dependent variable: composite cognitive-academic test score	Dependent variable: mathematics test score	Dependent variable: reading test score	Discontinuity sample
	(1)	(2)	(3)	(4)
		A. Grade 4, NABC, 2006		
F-statistic[a]	4608.58	4975.71	1101.75615	1706.20
Prob *F* > 0	0.000	0.000	0.000	0.000
N	31008	31008	31008	10011
		B. Grade 8, NABC, 2006		
F-statistic[a]	7803.45	6286.71	6621.24	2584.41
Prob *F* > 0	0.000	0.000	0.000	0.000
N	29741	29741	29741	10062
		C. PIRLS, 2001		
F-statistic[a]			288.59	
Prob *F* > 0			0.000	
N			1655	
		D. TIMSS, 2003		
F-statistic[a]		282.47		
Prob *F* > 0		0.000		
N		1214		

Notes [a] The *F*-statistic corresponds to a test of equality between coefficients of the disadvantaged and non-disadvantaged subsamples.