



Universal preschool and cognitive skills – the role of school starting age as a moderating factor ^{☆,☆☆,★,★★}



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ABSTRACT

Previous empirical evidence is mixed regarding the impact of universal preschool on cognitive skills. We show that preschool enrollment can impact test scores positively if it does not lead to earlier school enrollment. We examine rich student data and use different enrollment cutoff dates in Hungary to separate the beneficial direct effect of earlier preschool enrollment from a negative indirect effect that may occur through earlier school enrollment. We find significant direct impacts: 6th-grade reading (math) test scores increase by 9.0 (6.3) percent of a standard deviation for children who enroll in preschool a year earlier. This impact persists through 10th grade and is larger among disadvantaged children. The findings support the importance of universal preschool for improving cognitive skills and equity. They highlight a key consideration for policy evaluation and design and help reconcile ambiguities in the previous empirical evidence.

1. Introduction

Current policy debates about universal preschool involve proposals on historic investments in Early Childhood Education (ECE). In the United States, the American Families Plan aimed to provide free preschool to all 3–4-year-olds at a cost of about \$200 billion (White House, 2021). The European Union (EU) set the Barcelona Target to provide 90% of children from age 3 to school age with high-quality, affordable preschool, which has been achieved by several countries, including Denmark, France, Sweden, and Belgium (European Commission, 2018). The United Nations Sustainable Development Goals (United Nations, 2015) aim for at least one year of quality pre-primary education for every child in the world by 2030. Proponents argue that the benefits include increased maternal labor market participation, positive impacts on children's human capital and later labor market success,

and a decrease in inequality. However, since preschool expansion (increasing the number of available preschool seats) is costly, it is important to assess potential benefits accurately and to consider the factors that may constrain the beneficial impact.

1.1. The effect of preschool on cognitive skills

Previous studies provide empirical evidence that *targeted* preschool programs can have a significant positive impact on cognitive skills measured by grade point average (Heckman et al., 2010), noncognitive skills, such as social skills and attitudes towards learning (Zhai et al., 2014), and on individual success in the longer run, measured by college attendance or income (García et al., 2020; Gray-Lobe et al., 2021; McCoy et al., 2017). These targeted programs (such as the Perry

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Preschool Project) focus on disadvantaged children and offer high quality care, which means they would be prohibitively expensive to extend to the entire population.

The empirical evidence on the effect of *universal* preschool on cognitive skills, most often measured by test scores (referred to as the “preschool effect” throughout this study) is ambiguous, with several studies pointing to small or insignificant impacts (Apps et al., 2013; Bernal & Keane, 2011; Gray-Lobe et al., 2021; Kuehnle & Oberfichtner, 2017, 2020). Institutional quality (physical resources, staff to child ratio, curriculum, and pedagogies) and maintaining childcare capacities for 0 to 3-year-olds have been discussed as important determinants (Andrew et al., 2019; Duncan, 2003; Peisner-Feinberg et al., 2001). In this study, we highlight school starting age - the age at which children enroll into primary school - as a further key factor to be taken into account. We assess whether preschool expansion has a positive impact on cognitive skills if it does not lead to earlier enrollment into primary school.

Preschool enrollment can impact cognitive skills through multiple channels. There may be a direct positive impact, as children are exposed to socialization, rules, and knowledge earlier, during a period shown to be critical in brain development (Brown & Jernigan, 2012). However, earlier enrollment into preschool may also lead to earlier enrollment into primary school, which has a potentially negative effect. Previous studies have shown that earlier school enrollment harms school outcomes and leads to lower test scores. These generally rely on within-population variation in school starting age due to enrollment cutoff dates to estimate the impact (Bedard & Dhuey, 2006; Black et al., 2010; Elder & Lubotsky, 2009; McEwan & Shapiro, 2008; Puhani & Weber, 2008). They highlight that children enrolled in school earlier are younger at the date of the tests than those who start school later (*age effect*); they are also less mature at school starting and may be less able to cope with school pressures (*maturity effect*); and they are less mature compared to their peers (*relative maturity effect*). The school starting age channel may therefore lead to a potential negative indirect impact of earlier preschool enrollment on cognitive skills.

1.2. The role of school starting age in previous estimates of the preschool effect

A correlation between preschool and school starting ages may arise because preschool improves school readiness and facilitates earlier school entry (Cornelissen et al., 2018), or due to capacity constraints that incent preschools to push children towards earlier school enrollment to free up seats. In line with the latter, this correlation is higher in countries with low preschool availability (Fig. 1 in the Online Appendix). Moreover, the correlation is strong in many OECD countries (Table A5), suggesting that in most settings, the indirect school starting age channel plays some role.

The overall effect of preschool depends on the relative magnitudes of the direct positive impact of preschool enrollment and the indirect negative impact through the school starting age channel. Noting this can help reconcile some of the ambiguities in the existing empirical evidence. Previous causal estimates rely on two main methodologies: they use either within-population variation in preschool starting age (due to eligibility cutoffs) or preschool capacity expansions for the identification of the preschool effect. The estimates of these two approaches are likely to differ because the negative school starting age effect is less likely to play a role in the latter case.

In eligibility cutoff-based studies, the estimated preschool effect depends on the correlation between preschool and school starting age in the population. Since this correlation is strong in most countries, estimates relying on this methodology are likely to include a negative school starting age effect that dampens the positive direct impact of preschool. Accordingly, many such studies find small or insignificant effects (Apps et al., 2013; Bernal & Keane, 2011; Kuehnle & Oberfichtner, 2017, 2020). These estimates pertain to the overall impact

of preschool enrollment, without separating out the indirect impact through school starting age. Notably, when cognitive skills are measured prior to school enrollment - when school starting age has not yet had any influence - the evidence points to a significant, though modest, positive preschool starting age effect (Cornelissen et al., 2018; Magnuson et al., 2007).

Studies examining preschool expansions generally refer to a situation where more children are able to enroll into preschool (or all children enroll earlier) without any subsequent change in the timing of their enrollment into primary school. Parents who now have access to (affordable) preschool do not have an incentive to enroll their children in school earlier. The pressure also eases on preschool institutions to free up seats by pushing children into primary school. Consequently, preschool expansion leads to a positive direct impact without a negative indirect school enrolment age impact, or possibly even a positive indirect impact (if the expansion leads to an increase in the average age of enrollment into school). In line with this, these studies find more substantial positive effects of preschool on cognitive outcomes (see Berlinski et al., 2008; Felfe et al., 2015; Havnes & Mogstad, 2011, 2015). An exception is Blanden et al. (2016) who examined an expansion in England in the early 2000s and found no large effects, however, this was due to the fact rather than a growth in new enrollment, children switched from existing private preschools to public institutions. These studies also estimate the overall impact of preschool without separating out the indirect school starting age impact, however, in their case this is likely to be zero or positive. This can explain their findings of higher estimates of the overall preschool effect.

1.3. Present study

In this study we rely on within-population variation to estimate the overall impact of preschool starting age, similarly to previous studies in the literature (Apps et al., 2013; Bernal & Keane, 2011; Kuehnle & Oberfichtner, 2017, 2020). However, we separate out the role of school starting age, and also estimate the direct impact of preschool without the indirect school starting age channel.

We use a rich, representative student dataset from Hungary that includes full cohorts of students for ten years (2008–2017) and measure the effect of preschool starting age on mathematics and reading test scores. Our dataset includes test scores and detailed student background information for three grades of students (6th, 8th, and 10th). The initial dataset contains over 2.5 million student level observations, which - after dropping observations with missing values - results in estimation samples of around 650 thousand students per grade.

We first replicate previous estimates of the overall preschool effect, relying on a preschool enrollment cutoff. We next document how school starting age is correlated positively with preschool starting age in Hungary, similarly to many countries. We then test whether the estimated effect of preschool enrollment includes a negative impact occurring through school starting age that dampens the beneficial direct impact. To do so, we utilize a particular aspect of the Hungarian institutional system: different eligibility cutoff dates for preschool and school enrollment. This allows us to separate the indirect effect of school starting age from the direct effect of preschool starting age. We rely on instrumental variables regressions, where expected preschool and school starting age variables serve as instruments (similarly to Black et al., 2010). Under certain conditions, i.e. (1) the instruments are correlated with the endogenous explanatory variable (relevance), (2) the instruments do not have any direct effect on the outcome variable (exclusion restriction), and (3) the instruments are uncorrelated with the error term (independence), we can interpret the findings in a causal way.

We provide an estimate of what the impact of preschool starting age would be in a setting where school starting age is not affected by preschool starting age. In order to also provide estimates that are comparable to those examining preschool programs targeted at disadvantaged children (Carneiro & Ginja, 2014; Currie & Thomas, 1995; Garces et al.,

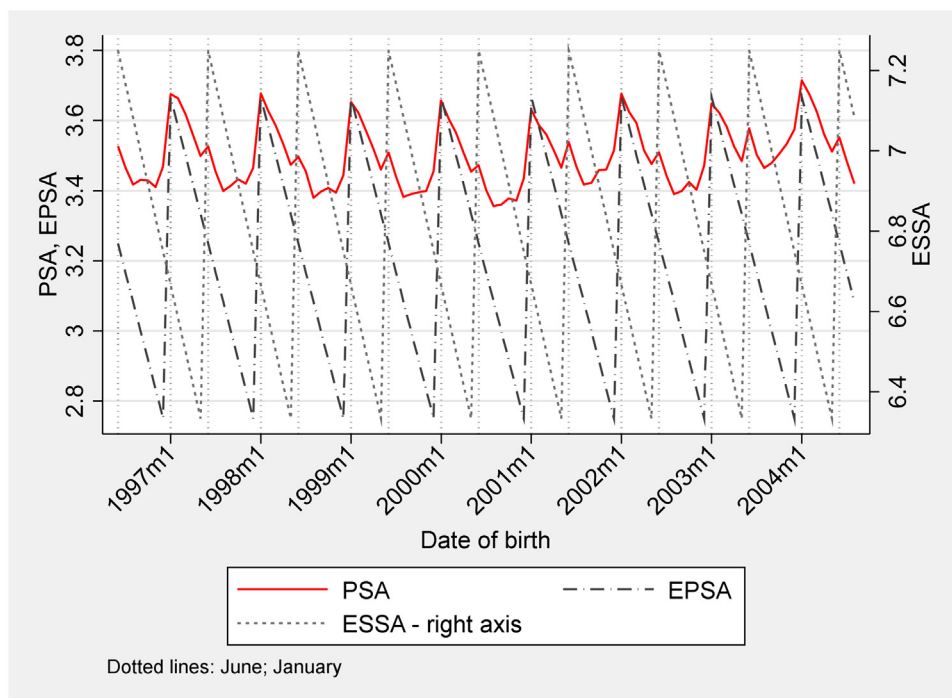


Fig. 1. Average Preschool Starting Age (PSA) and the instruments, EPSA and ESSA (by birth year and month)
 Data source: NABC database. Birth years where only a part of a cohort is covered by the database are excluded.

2002; Moon et al., 2010; Pinto et al., 2010), we further examine the impact of preschool among the subgroup of children with lower-educated mothers. This allows us to assess the impact of a non-targeted, universal program on disadvantaged children in particular, which is important from an equity perspective.

The research questions are as follows:

- What is the overall impact of preschool starting age on later test scores in a setting where preschool and school enrollment ages are correlated?
- Is the estimated direct impact of preschool starting age positive once the indirect impact through school starting age is controlled for?
- Is the impact of universal preschool stronger among children with disadvantaged backgrounds?

2. Context and method

2.1. Institutional framework

In Hungary, state-subsidized nursery school (*bölcsőde*) is available from age 5 months to 3 years. This covers around 13 percent of children, and provides childcare, but no pedagogical content. State-subsidized preschool (*óvoda*) is available for children aged 3-6, with a coverage rate of approximately 90 percent. Children are eligible to enroll around their third birthday, dependent on the birthdate-based eligibility cutoff and the availability of seats. The enrollment period starts on September 1st. In most places, children who turn 3 in the given calendar year are admitted. Practically, this means that there is a drop in preschool enrollment probabilities after birth dates on January 1st and many children born after this date are enrolled in preschool next September, in compliance with the enrollment regulations. Enrollment in preschool is compulsory from the age of 5, however, most children enroll at the age of 3 or 4. Preschools offer pedagogical programs, with a focus on the improvement of social competencies, visual and musical arts, drama, basic mathematics, and physical education.

At the age of 6 or 7, children enroll in Grade 1 of primary school, without spending any time in Reception Classes. According to the regulations, children born between January 1st and May 31st are required to enroll in primary school in September of the same year of their sixth birthday. Those born between June 1st and December 31st are required

to enroll in primary school in the September of the year following their sixth birthday. However, redshirting (delaying enrollment by one year) is possible. For those born between June and December, this requires a parental request for postponing school enrollment to be submitted and approved by the child’s preschool and the local government Developmental Advisory Board. This process includes a standardized evaluation process conducted by developmental experts, which is free of charge, but imposes time and travel costs on parents. Compared to the June–December group, children born between January and May face a smaller administrative barrier to redshirting, as the parental request only requires the approval of their preschool.

As a result of these primary school enrollment regulations, there are jumps in primary school enrollment probabilities on January 1st and June 1st. We utilize the variation around these two dates, as well as the preschool enrollment cutoff date of January 1st, to estimate and compare the direct impact of preschool starting age and its overall impact including the indirect impact through the school starting channel.

2.2. Data

Our analysis utilizes the Hungarian National Assessment of Basic Competencies (NABC) data (see Sinka, 2010 for details). The NABC is administered as a pen-and-paper test. It assesses non-curriculum-based domains of mathematical and reading literacy. The content framework of the NABC was first published in 2006, and later revised in 2014 (Balázi et al., 2006, 2014). Tests are administered annually in May, on the same day and at the same time for all grades. Tests are then collected and sent to the Education Authority. Background questionnaires are sent home with the children, collected by the schools a couple of days later, and sent to the Education Authority. After the data is recorded, coded, and cleaned, test scores are calculated from the test items using item response theory (IRT). The NABC covers all students in grades 6, 8 (lower secondary education), and 10 (upper secondary).

The dataset consists of a rich and comprehensive standard-based assessment of mathematics and reading skills that follows the model of the Programme for International Student Assessment (PISA). The dataset used in the current study covers the time period from 2008 to 2017. Due to missing data for some key variables, the regressions in our analysis were run on an 87.5 percent sample of the original data. 11.9 percent

of the original sample was dropped due to missing birthdate, school-, or preschool starting age information, and a further 0.6 percent of the sample was dropped due to errors in key variables (see Table 4 in the Online Appendix). Our initial sample covers over 2.5 million student year-level observations. The analytical sample exceeds 631,067 in each estimation for the separate grades (6th, 8th, and 10th).

We focus on standardized literacy and mathematics test scores as our main outcome variables. The scores are standardized to have a mean of 0 and a standard deviation of 1 in each year and each grade. The NABC dataset includes several further measures of student outcomes: self-reported plans to continue studies in higher education, class marks given by the teacher in the last term, and the student’s grade point average (GPA). At the time of the tests, the students received questionnaires which were taken home and completed by parents. Based on these questionnaires, we observe a variety of individual and parental background characteristics at the time of testing. Matching the data to regional characteristics, we also observed the scarcity of preschool seats, using the available seats per population of 3–5-year-olds in the given settlement. Table A1 in the Appendix presents the descriptive statistics of all covariates included in our regressions as controls, as well as the main independent and dependent variables.

2.3. Analysis

Our goal is to measure the effect of preschool starting age (PSA) on mathematics and reading test scores in grades 6, 8 and 10, with and without the impact realized through school starting age. Our identification of the impact of PSA relies on individual level variation among the students in our sample. However, children’s family background, living conditions, as well as their cognitive and noncognitive traits correlate strongly with the timing of their preschool enrollment. At the same time, these factors are also likely to have a strong influence on test scores measured in school, so our estimates of the impact of PSA may be biased.

To overcome these endogeneity issues, we utilize exogenous variation in enrollment probabilities that are determined by the institutional setup (see section 2.1 detailing the institutional framework). The exogenous variation originates from the variation in birth dates and the enrollment cutoff date of January 1st. The children in the sample are expected to enroll in preschool in September of the calendar year when they turn 3 years old. We use a two-stage-least-squares (2SLS) model, where we instrument actual preschool starting age (PSA) with the expected preschool starting age (EPSA), similar to Black et al. (2010). We calculate EPSA as $3 + (9 - \text{month of birth})/12$, which was in line with preschool enrollment date of September 1st, and the cutoff date of January 1st. We allow for heterogeneous treatment effects and measure local average treatment effects (LATE).

It should be noted that EPSA depends only on birth month but not the choices of the family. Compliance with the enrollment regulations (reported in Table A2 in the Appendix) is strong in the first half of the year. It shrinks somewhat in the later months, which is a consequence of preschool scarcity in some municipalities, where older children have priority of enrollment in September. The average PSA decreases until September and jumps at January 1st. There is a smaller jump on June 1st, which is related to the school enrollment rules, as discussed in detail later in this section. Consequently, and as shown more extensively in Appendix B, EPSA serves as a suitable instrument.

In our first specification we mimic earlier findings in the literature using a similar methodology, and run a 2SLS regression of the following form, where any impact realized through school starting age is included in the estimated PSA effect:

$$PSA_{iys} = \alpha_1 EPSA_{iys} + \gamma'_1 X_{iys} + \mu_{1y} + \theta_{1s} + \varepsilon_{1iys} \quad (1)$$

where $EPSA_{iys}$ denotes the expected preschool starting age for child i in year y and school s and X_{iys} is a vector of individual and family characteristics (see Table A1 in the Appendix for the full list of variables included). We also include year fixed effects, μ_{1y} , and school fixed effects,

θ_{1s} , to control for time and school-specific unobserved heterogeneity. The second stage of the 2SLS is the following:

$$Y_{iys} = \gamma_1 \widehat{PSA}_{iys} + \gamma'_3 X_{iys} + \mu_{3y} + \theta_{3s} + \xi_{iys} \quad (2)$$

where Y_{iys} denotes the outcome variables of child i : math and reading NABC test scores. As further checks, we also estimate impacts on mathematics class marks based on administrative information and last term GPA (alternative cognitive outcomes), and self-reported higher education aspirations (noncognitive outcome). We estimate robust standard errors clustered on the municipality level.

Instrumenting PSA with EPSA should control for endogeneity bias due to individual background characteristics, and give us a causal estimate of the overall impact of PSA. If PSA and school starting age (SSA) are correlated, the overall impact includes the negative indirect impact due to SSA. In our Hungarian individual level dataset, PSA is strongly correlated with SSA (corr.=0.35), as it is in many OECD countries (Table A5). This is indicative of capacity constraints, which push children who enroll earlier into preschool to enroll earlier into primary school as well. We next estimate what the expected impact of PSA would be in a context where no such capacity constraints are present, i.e., without an indirect impact through SSA.

Similarly to the EPSA variable, we define a variable indicating expected school starting age (ESSA) as:

$$ESSA = \begin{cases} 6 + \frac{(9-mob)}{12} & \text{if } mob < 6 \\ 7 + \frac{(9-mob)}{12} & \text{if } mob \geq 6 \end{cases} \quad (3)$$

where mob is month of birth.

Figs. 1 and 2 depict PSA and SSA, as well as their expected values, EPSA and ESSA, over birth date year and month for the children in our sample. We can see that both PSA and SSA jump around the cutoff dates of January 1st and June 1st. For PSA, there is a large jump at January 1st, in line with the eligibility cutoff, and a smaller jump at June 1st. For SSA, the jump is larger at June 1st. We can also see that PSA is correlated not only with EPSA, but also with ESSA.

In the second specification, we directly account for the SSA channel by including SSA as an explanatory variable in the regressions. We use EPSA and ESSA as instruments for PSA and SSA, since SSA is likely to be endogenous for the same reasons as PSA. The two instruments are correlated (the correlation is 0.46), but it is not uncommon to use correlated instrumental variables as long as they are not linear combinations of each other (J. D. Angrist & Krueger, 1991; see Belloni et al., 2014; Burgess et al., 2016; Mogstad et al., 2019). Using the two instruments, we estimate a 2SLS model. The first-stage equations are:

$$PSA_{iys} = \delta_{11} EPSA_{iys} + \delta_{12} ESSA_{iys} + \gamma'_1 X_{iys} + \mu_{1y} + \theta_{1s} + \varepsilon_{1iys} \quad (4)$$

$$SSA_{iys} = \delta_{21} EPSA_{iys} + \delta_{22} ESSA_{iys} + \gamma'_2 X_{iys} + \mu_{2y} + \theta_{2s} + \varepsilon_{2iys} \quad (5)$$

The second-stage structural equation is the following:

$$Y_{iys} = \beta_1 \widehat{PSA}_{iys} + \beta_2 \widehat{SSA}_{iys} + \gamma'_3 X_{iys} + \mu_{3y} + \theta_{3s} + \xi_{iys} \quad (6)$$

The parameter of interest is β_1 , which shows the direct effect of preschool starting age on the outcome variable, without the impact that is realized through SSA. β_2 reflects the impact of school starting age on student outcomes.

3. Results

3.1. The impact of preschool starting age

The descriptive statistics of the dependent and control variables are shown in Table A1 in the Appendix for grades 6, 8 and 10 respectively. The average preschool starting age is 3.5, and the average school starting age is 7.04. In Fig. 1 and 2, we can see that both enrollment timing variables are correlated with the two instruments, which is also demonstrated by the strong first stage statistics reported in Table B1 in the

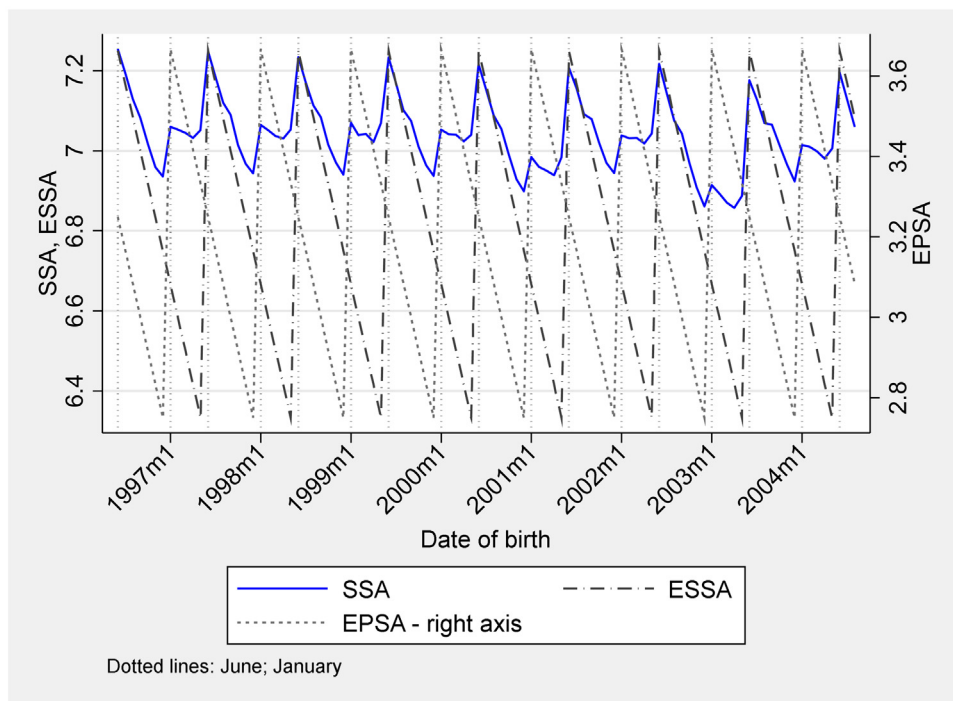


Fig. 2. Average School Starting Age (SSA) and the instruments, EPSA and ESSA (by birth year and month)
 Data source: NABC database. Birth years where only a part of a cohort is covered by the database are excluded.

Table 1
 The effect of preschool starting age on test scores – Grade 6

	Math test scores		Reading test scores	
	(1)	(2)	(1)	(2)
Preschool Starting Age (PSA)	-0.017 (0.019)	-0.063*** (0.015)	-0.038* (0.018)	-0.090*** (0.014)
School Starting Age (SSA)		0.203*** (0.017)		0.226*** (0.015)
IV	EPSA	EPSA, ESSA	EPSA	EPSA, ESSA
SSA included as an endogenous variable		Yes		Yes
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes
Number of clusters	1602	1602	1602	1602
Number of observations	645277	645277	645441	645441
Adjusted R2	0.171	0.153	0.203	0.185
AIC	1555602.467	1569468.302	1525156.158	1539568.930

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Notes: Robust standard errors clustered on the municipality level are in parentheses. Test scores are annually standardized to mean 0 and standard deviation 1. The individual covariates include year and school fixed effects, gender, special education needs, entitlement to a cheaper or free meal indicating poor financial circumstances of the family, whether a student is living with their own family, the mother's and father's level of education, employment status, and age, whether the student's family has a car, a bathroom with public utilities, an internet connection at home, the number of books at home, whether the student has their own desk, and whether the family helps the student with their homework. For the full set of results, see the Online Appendix.

Appendix. Our main results shown in Table 1 present regressions with math and reading test scores as the outcome variables. In the first column, we report the parameter estimates of the overall impact of PSA, which includes any impact realized through SSA (Eq. 2). In the second column, SSA enters the regression in a similar fashion as PSA, as an endogenous explanatory variable, and both are instrumented (Eq. 6). The table shows the point estimates for the coefficients of PSA and SSA.

The parameter estimates of PSA are close to zero both for math (Column 1) and reading (Column 3), and weakly significant for reading. These estimates are in line with the weak effects measured in the previous literature on the impact of universal preschool programs (e.g., Bernal & Keane, 2011; Kuehnle & Oberfichtner, 2020). We hypothesize that the estimated overall effect of starting preschool earlier includes a negative impact due to starting primary school earlier, which counteracts any positive direct impacts of a lower PSA. We expect that once the SSA is controlled for, PSA will have a more negative coefficient, i.e., a lower PSA leads to an improvement in test scores.

Using both EPSA and ESSA as instruments and treating PSA and SSA as endogenous variables produces large and significant negative estimates, as reported in Columns 2 and 4. These estimates of the PSA effect include only the direct impact, excluding the indirect impact that may occur due to the correlation between PSA and SSA. These point estimates suggest that in the same year and the same school, holding various demographic variables constant and holding SSA fixed, enrolling in preschool one year earlier increases reading (math) test scores significantly, by 9.0 percent (6.3 percent) of a standard deviation in 6th grade. The effects are significant at $p < .001$. This effect size is twice as large as the effect of a child's mother having vocational school as their highest level of education instead of elementary school (see Table 1 column 3 of the Online Appendix: the difference of the coefficients of „Mother's education: elementary” and „Vocational” is 0.029). The coefficient estimates of school starting age are in line with the previous literature, pointing to a large significant positive impact of later school enrollment.

Table 2
The effect of preschool starting age on test scores – Grade 6, 8 and 10

	Math test scores			Reading test scores		
	6 th grade	8 th grade	10 th grade	6 th grade	8 th grade	10 th grade
Preschool Starting Age (PSA)	-0.063*** (0.015)	-0.049*** (0.013)	-0.047*** (0.010)	-0.090*** (0.014)	-0.075*** (0.014)	-0.056*** (0.010)
IV	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA
SSA included as an endogenous variable	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	1602	1602	1602	1602	1602	1602
Number of observations	645277	647704	631067	645441	647959	631323
Adjusted R ²	0.153	0.138	0.091	0.185	0.165	0.058
AIC	1569468.302	1561734.770	1371521.113	1539568.930	1538420.011	1349656.960

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Notes: Robust standard errors clustered on the municipality level are in parentheses. Test scores are annually standardized to mean 0 and standard deviation 1. The individual covariates include year and school fixed effects, gender, special education needs, entitlement to a cheaper or free meal indicating poor financial circumstances of the family, whether a student is living with their own family, the mother’s and father’s level of education, employment status, and age, whether the student’s family has a car, a bathroom with public utilities, an internet connection at home, the number of books at home, whether the student has their own desk, and whether the family helps the student with their homework. For the full set of results, see the Online Appendix.

Table 2 summarizes the preschool effects estimates that exclude the impact realized through school starting age (Eq. 6), for grades 6, 8, and 10. These suggest a fairly persistent impact. The effects diminish somewhat in higher grades but are still relatively large and significant. It is important to note that in Hungary, children sort into vocational or academic secondary schools after the 8th grade, so the between-school variation in test scores rises significantly in the 10th grade. Nevertheless, within schools, the effect of preschool starting age on test scores remains relatively high and significant. Starting universal preschool one year earlier increases reading (math) test scores in grade 10 by 5.6 (4.7) percent of a standard deviation.

We provide further results using additional student outcomes in Table A3 in the Appendix. These include self-reported plans to enter higher education, GPA in the last term, and math grade. The estimated impacts on math grades are nearly identical to those seen in the case of math test scores, which supports the robustness of our results. For the other two outcomes, the effects are somewhat weaker, though more pronounced for children of low-educated mothers.

3.2. Heterogeneity analysis

A large part of the previous literature focuses on childcare and preschool programs targeted at disadvantaged children (e.g., Currie & Thomas, 1995; Moon et al., 2010). Table 3 provides a basis for comparison, based on a proxy variable for disadvantage available in our dataset. It reports the regression results for the children of lower-educated mothers (without a maturity exam passed at the end of grade 12, similar to a General Educational Development Test), as well as for the children of higher-educated mothers (with a maturity exam). This allows us to assess how the impact of a universal program compares to those found previously in the case of targeted programs.

The point estimates are twice as large for the children of lower-educated mothers, reaching a 12.4 percent of a standard deviation effect in both domains in grade 6. By 10th grade, the effect size shrinks to 5.1 percent for both math and reading test scores. In the case of students with higher-educated mothers, preschool enrollment timing does not have a significant effect on math scores in the 6th and the 8th grades. In grade 10, the point estimate is 4.4 percent and significant at $p < .01$. The effect on reading test scores is between 5.6 and 6.9 percent and remains significant throughout all grades. Overall, these results suggest that, similarly to targeted programs, a universal preschool program can also improve equity by providing a particularly large benefit to disadvantaged children.

3.3. Robustness

Robustness test results for the main regression results (Eq. 6) are shown in Table A6 of the Appendix. First, we check whether our IV strategy results in a quasi-experimental research design. If the independence assumption for our IV holds, one would not need to use control variables at all. To check this, we excluded all exogenous control variables from the regression. According to Columns 1 and 4, this does not change the regression results. Some further tests for IV exogeneity are presented in Appendix B.

In our main specification, we used students born throughout the year in all months, but as a robustness check, we restricted our sample to those born close around the two cutoff dates (1st January and 1st June). Thus, in our second robustness specification, we included only those born in December, January, May, or June. The results are shown in Columns 2 and 5 in Table A6 in the Appendix. This specification results in similar, but slightly larger, point estimates compared to our main specification.

Finally, students with special education needs (SEN) may drive our results, so we excluded them from the analysis to determine whether this is the case. In Columns 3 and 6 in Table A6 in the Appendix, we present the results after dropping the SEN students. Again, this does not change our point estimates significantly.

4. Discussion

Our estimates of the effect of preschool starting age on cognitive outcomes observed in grades 6, 8, and 10 contribute to the literature by highlighting how school starting age can play a role in this relationship. If preschool and school starting ages are correlated, any positive direct impacts of preschool (an earlier preschool starting age) may be cancelled out by a negative impact due to an earlier school starting age. We demonstrate this using data from Hungary, where such a correlation exists. We show that, while the overall estimated impact of preschool starting age is small and insignificant, if we control for school starting age, earlier preschool enrollment has a positive impact on test scores in math and reading. Our main results indicate that starting preschool one year earlier has a significant positive effect on reading and math test scores measured in 6th grade, with magnitudes of 9.0 percent and 6.3 percent of a standard deviation, respectively. In line with the previous literature, we find substantively larger effects for children of lower-educated mothers. These results are in line with the long-run benefits found by studies focusing on targeted preschool programs for disadvantaged children, such as Head Start or the Perry Preschool Project

Table 3
The effect of preschool starting age on test scores – by mother’s level of education

Outcome variable	Full sample			Lower-educated mother ^(a)			Higher-educated mother ^(b)		
	6 th grade	8 th grade	10 th grade	6 th grade	8 th grade	10 th grade	6 th grade	8 th grade	10 th grade
Reading test score	-0.090*** (0.014)	-0.075*** (0.014)	-0.056*** (0.010)	-0.124*** (0.031)	-0.087*** (0.025)	-0.051* (0.022)	-0.069*** (0.017)	-0.068*** (0.016)	-0.056*** (0.011)
Math test score	-0.063*** (0.015)	-0.049*** (0.013)	-0.047*** (0.010)	-0.122*** (0.033)	-0.086*** (0.025)	-0.051** (0.019)	-0.030 (0.020)	-0.030 (0.015)	-0.044** (0.014)
IV	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA
SSA included as an endogenous variable	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	1602	1602	1602	1602	1602	1602	1602	1602	1602

p* < 0.05; *p* < 0.01; ****p* < 0.001.

Notes: Robust standard errors clustered on the municipality level are in parentheses. Test scores are annually standardized to mean 0 and standard deviation 1. The individual covariates include year and school fixed effects, gender, special education needs, entitlement to a cheaper or free meal indicating poor financial circumstances of the family, whether a student is living with their own family, the mother’s and father’s level of education, employment status, and age, whether the student’s family has a car, a bathroom with public utilities, an internet connection at home, the number of books at home, whether the student has their own desk, and whether the family helps the student with their homework.

(a) Lower-educated: mothers without maturity exam.

(b) Higher-educated: mothers with maturity exam.

(Carneiro & Ginja, 2014; Currie & Thomas, 1995; Garcés et al., 2002; Moon et al., 2010; Pinto et al., 2010).

The findings provide new evidence relevant to existing studies on universal preschool programs (Baker et al., 2019; Cornelissen et al., 2018; Currie, 2001; Datta Gupta & Simonsen, 2010; Kuehnle & Oberfichtner, 2020), which thus far provided inconclusive results (van Huizen & Plantenga, 2018). Our analysis offers an explanation as to why the previous studies found mixed results. Typically, studies that use exogenous variations in preschool enrollment when capacities were fixed (such as variations related to birth date) find very small or no effects. This can be explained by the fact that in such a context, preschool and school starting ages are correlated (see Table A5), and the estimated overall impact of preschool includes a negative impact due to an earlier school starting age. However, studies that exploit preschool expansion for identification – where earlier preschool enrollment does not lead to a decrease in school starting age - estimate large positive impacts. This is because the indirect school starting age impact is more likely to be zero or even positive, so the overall impact of preschool starting age is more positive.

These results hold for other education systems to the extent that school starting age is an important confounder in other countries as well. Two conditions determine the magnitude of the negative school starting age effect, and thereby, the overall effect of preschool starting age. First, whether school starting age is negatively correlated with test scores, of which a whole branch of the literature related to redshirting provides ample evidence for countries across the OECD (e.g., Bedard & Dhuey, 2006; Black et al., 2010; Elder & Lubotsky, 2009; McEwan & Shapiro, 2008; Puhani & Weber, 2008). Second, whether school starting age is correlated with preschool starting age. Table A5 in the Appendix shows that among OECD countries, this correlation ranges from 0.12 (Russian Federation) to 0.59 (Switzerland), with Hungary situated in the middle, with a correlation of 0.35. As a result, we suspect that the negative school starting age effect impacts the overall impact of preschool starting age in most countries.

4.1. Limitations

While interpreting the results of this study, it should be considered that the data and the context have some limitations. First, the estimated impact depends on the quality of preschool in Hungary, and therefore

the preschool effect may be larger or smaller in countries with higher or lower preschool quality. Second, we measure the effect of being enrolled in preschool one year sooner or later around three years of age. The effect may be different for children of younger ages. Third, we have measures in the data of some important outcomes such as math and reading test scores, but we do not observe some other important aspects, such as emotional and behavioral characteristics of the students. Therefore, we cannot infer from our data how these latter are affected by earlier preschool enrollment, despite the high importance of these other outcomes in shaping the careers and wellbeing of students. Finally, while our instrumental variables estimates provide causal evidence of the impact of preschool starting age with high internal validity, their external relevance may be limited in key ways. The estimates are derived from a country with an existing universal preschool system. Policy recommendations regarding preschool starting age may not be directly applicable to nations without such a system in place, as bigger policy issues need to be addressed before the question of optimal starting age becomes relevant.

4.2. Implications

Our results suggest that enrolling preschool one year earlier can have a significant and meaningful positive impact on the cognitive skills of children, as long as it does not lead to earlier primary school enrollment. Well-funded preschool expansions are therefore likely to have a positive impact on children’s cognitive skills since we do not expect them to lead to earlier enrollment into primary school. The details of implementation are key: expansion must involve true capacity increases and funding needs to be sufficient to enable the provision of high-quality services. Investments need to include funding for staffing, fair teacher compensation, curriculum requirements, credential requirements, continuous training, and child assessments. Universal preschool can also play a key role in increasing equity, since the beneficial cognitive impacts are largest among children with disadvantaged backgrounds.

Data availability

The authors do not have permission to share data.

CRedit authorship contribution statement

Ágnes Szabó-Morvai: Conceptualization, Methodology, Software, Formal analysis, Writing – original draft, Writing – review & editing, Supervision. **Daniel Horn:** Conceptualization, Funding acquisition, Data curation, Methodology, Formal analysis, Writing – original draft, Writing – review & editing. **Anna Lovász:** Conceptualization, Methodology, Writing – original draft, Writing – review & editing, Supervision. **Kristof De Witte:** Conceptualization, Methodology, Funding acquisition, Writing – review & editing.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:[10.1016/j.ecresq.2023.04.004](https://doi.org/10.1016/j.ecresq.2023.04.004).

Appendix A

Table A1
The NABC database – mean and standard deviation of the outcome and the control variables

	Grade 6		Grade 8		Grade 10	
	mean	sd	mean	sd	mean	sd
Average annual number of observations (2008-17)	89434	3935	88704	6533	88785	8852
Standardized math score	0	1	0	1	0	1
Standardized reading score	0	1	0	1	0	1
Plans to enter higher education	0.53	0.5	0.53	0.5	0.55	0.5
Student's GPA of last term	3.83	0.8	3.92	0.78	3.56	0.79
Mark of last term: mathematics (admin.)	3.26	1.17	3.29	1.15	2.97	1.14
Preschool starting age (PSA)	3.51	0.62	3.51	0.63	3.57	0.67
School starting age (SSA)	7.04	0.40	7.03	0.40	7.04	0.44
Years spent in preschool	3.53	0.66	3.52	0.67	3.47	0.68
Age at testing	12.77	0.50	14.75	0.48	16.82	0.58
Female	0.50	0.50	0.50	0.50	0.50	0.50
Special Education Needs	0.04	0.20	0.04	0.20	0.02	0.16
Entitled for cheap meal	0.23	0.42	0.19	0.40	0.14	0.35
Entitled for free meal	0.22	0.41	0.14	0.35	0.02	0.15
Student receives textbook free in school	0.45	0.50	0.39	0.49	0.32	0.47
Student lives in own family	0.92	0.27	0.86	0.35	0.85	0.36
Number of siblings living together	1.39	1.13	1.33	1.08	1.20	0.99
Mother's education: elementary	0.16	0.37	0.14	0.35	0.11	0.31
Mother's education: vocational	0.24	0.43	0.24	0.42	0.24	0.43
Mother's education: high school	0.28	0.45	0.27	0.44	0.27	0.45
Mother's education: university	0.23	0.42	0.20	0.40	0.22	0.41
Father's education: elementary	0.13	0.34	0.11	0.32	0.08	0.27
Father's education: vocational	0.37	0.48	0.36	0.48	0.37	0.48
Father's education: high school	0.21	0.40	0.20	0.40	0.20	0.40
Father's education: university	0.17	0.38	0.16	0.37	0.17	0.38
Age of student's mother/foster-mother	39.64	5.54	41.12	5.56	42.78	5.53
Age of student's father/foster-father	42.59	6.78	44.08	6.81	45.74	6.78
At least one car in the HH	0.66	0.47	0.63	0.48	0.65	0.48
At least one bathroom in the HH	0.87	0.33	0.82	0.38	0.82	0.39
At least 150 books in the HH	0.42	0.49	0.41	0.49	0.45	0.50
Internet connection in student's home	0.78	0.41	0.76	0.43	0.78	0.42
Student has own desk	0.83	0.37	0.79	0.41	0.78	0.41
Mother is employed	0.68	0.46	0.67	0.47	0.69	0.46
Father is employed	0.78	0.42	0.72	0.45	0.71	0.45
Family helps in HW at least once in a week	0.59	0.49	0.34	0.47	0.20	0.40
Large city	0.33	0.47	0.34	0.47	0.55	0.50
City	0.38	0.49	0.38	0.49	0.43	0.49
Small city	0.29	0.45	0.28	0.45	0.02	0.13
Region: Central Hungary	0.26	0.44	0.26	0.44	0.26	0.44
Region: Central Transdanubia	0.11	0.31	0.11	0.31	0.11	0.31
Region: Western Transdanubia	0.10	0.30	0.10	0.30	0.11	0.31
Region: Southern Transdanubia	0.09	0.29	0.09	0.29	0.09	0.29
Region: Northern Hungary	0.13	0.33	0.13	0.33	0.12	0.33
Region: Northern Great Plain	0.17	0.38	0.17	0.38	0.17	0.38
Region: Southern Great Plain	0.13	0.34	0.14	0.34	0.14	0.35
Scarcity of preschool places	1.22	0.40	1.19	0.39	1.14	0.37

Notes: 'Mark of last term' is given by the teacher and ranges from 1 to 5, where 1 is fail and 5 is the best mark. GPA is the average of term marks in all subjects.

Table A2
Compliance of preschool and school enrollment by the month of birth

	Preschool enrollment				B School enrollment			
	Early	On time	Late	Average preschool starting age	Early	On time	Late	Average school starting age
January	0.13	0.73	0.14	3.70	0.00	0.67	0.34	7.01
February	0.10	0.75	0.15	3.66	0.00	0.60	0.40	7.00
March	0.08	0.76	0.16	3.62	0.00	0.53	0.47	6.99
April	0.06	0.77	0.17	3.56	0.00	0.45	0.55	6.98
May	0.05	0.77	0.19	3.51	0.00	0.34	0.66	7.02
June	0.03	0.69	0.28	3.55	0.06	0.91	0.03	7.22
July	0.03	0.67	0.30	3.49	0.03	0.93	0.04	7.16
August	0.03	0.65	0.32	3.43	0.02	0.94	0.04	7.10
September	0.02	0.60	0.38	3.45	0.01	0.92	0.07	7.06
October	0.00	0.55	0.45	3.46	0.00	0.91	0.09	7.00
November	0.00	0.49	0.51	3.45	0.00	0.88	0.11	6.94
December	0.00	0.37	0.63	3.49	0.00	0.84	0.16	6.91

Notes: Preschool (school): Early starters: those children who enrolled before turning 2.75 (6). On time: children who enrolled between the age of 2.75 (6) and 3.75 (7). Late: children who enrolled after the age of 3.75 (7).

Table A3
Additional outcome variables – the effect of preschool starting age

Outcome variable	Full sample			Low educated mother			High educated mother		
	6 th grade	8 th grade	10 th grade	6 th grade	8 th grade	10 th grade	6 th grade	8 th grade	10 th grade
Plans to enter higher education	-0.018* (0.008)	-0.026*** (0.007)	-0.025*** (0.006)	-0.024 (0.014)	-0.040*** (0.012)	-0.045*** (0.013)	-0.014 (0.010)	-0.016 (0.008)	-0.013* (0.006)
Last term grade point average (GPA)	-0.005 (0.014)	-0.015 (0.011)	-0.015 (0.010)	-0.057* (0.024)	-0.055* (0.023)	-0.034 (0.022)	0.022 (0.016)	0.008 (0.014)	-0.002 (0.015)
Math grades (administrative)	-0.078*** (0.019)	-0.058*** (0.015)	-0.052** (0.016)	-0.166*** (0.042)	-0.107*** (0.029)	-0.083*** (0.025)	-0.029 (0.026)	-0.034 (0.018)	-0.036 (0.021)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Robust standard errors clustered on the municipality level are in parentheses. The individual covariates include year and school fixed effects, gender, special education needs, entitlement to a cheaper or free meal indicating poor financial circumstances of the family, whether a student is living with their own family, the mothers and fathers level of education, employment status, and age, whether the students family has a car, a bathroom with public utilities, an internet connection at home, the number of books at home, whether the student has their own desk, and whether the family helps the student with their homework. Grades given by the teacher range from 1, standing for the worst, to 5, indicating the best possible performance. "Higher education plans" is a dummy variable equal to 1 if the student plans to go on with their studies, and 0 otherwise.

Table A4
2SLS results for female and male subsamples

Subsample	Females			Males		
	6 th grade	8 th grade	10 th grade	6 th grade	8 th grade	10 th grade
Reading test scores	-0.098*** (0.017)	-0.087*** (0.017)	-0.063*** (0.012)	-0.096*** (0.023)	-0.080*** (0.024)	-0.062*** (0.017)
Math test score	-0.083*** (0.018)	-0.045* (0.018)	-0.052*** (0.013)	-0.049 (0.026)	-0.052* (0.022)	-0.046** (0.018)
IV	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA
SSA included as an endogenous variable	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Number of clusters	1602	1602	1602	1602	1602	1602

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The robust standard errors clustered on the municipality level are in parentheses. The test scores are annually standardized to mean 0 and standard deviation 1. The individual covariates include year and school fixed effects, gender, special education needs, entitlement to a cheaper or free meal indicating poor financial circumstances of the family, whether a student is living with their own family, the mothers and fathers level of education, employment status, and age, whether the students family has a car, a bathroom with public utilities, an internet connection at home, the number of books at home, whether the student has their own desk, and whether the family helps the student with their homework.

Table A5
PSA and SSA correlations across OECD countries

Country	Mean preschool starting age (PSA) ^(a)	Mean school starting age (SSA) ^(b)	Within-country correlation of PSA and SSA	Country	Mean preschool starting age (PSA) ^(a)	Mean school starting age (SSA) ^(b)	Within country correlation of PSA and SSA
Russian Federation	2.70	6.64	0.12	New Zealand	2.70	5.04	0.32
Estonia	2.84	6.81	0.13	Belgium	2.60	5.84	0.35
Sweden	2.45	6.53	0.16	Hungary	3.31	6.62	0.35
Iceland	2.24	5.75	0.16	Portugal	3.34	5.96	0.36
Bulgaria	3.28	6.83	0.17	Spain	2.76	5.63	0.36
Finland	3.95	6.71	0.19	Spain (Regions)	2.77	5.65	0.36
Norway	2.37	5.78	0.19	United Kingdom	2.86	4.77	0.37
Denmark	3.17	6.42	0.20	Mexico	3.73	6.08	0.37
Montenegro	3.47	6.19	0.21	Greece	3.77	6.11	0.39
Latvia	3.09	6.70	0.21	Dominican Republic	3.94	5.68	0.39
Lithuania	3.52	6.58	0.23	Hong Kong	2.88	5.97	0.42
Poland	4.45	6.91	0.23	Chinese Taipei	4.07	6.82	0.43
Czech Republic	3.30	6.28	0.24	Australia	3.70	5.31	0.43
Slovenia	2.81	5.93	0.24	Luxembourg	3.64	6.07	0.43
Croatia	3.79	6.67	0.25	USA (Massachusetts)	3.35	5.73	0.43
Israel	2.96	6.05	0.26	Costa Rica	4.87	6.55	0.45
Uruguay	3.42	5.93	0.26	United States	3.48	5.64	0.46
Slovak Republic	3.40	6.31	0.26	Canada	3.77	5.25	0.46
Germany	3.22	6.13	0.28	Qatar	3.82	5.83	0.49
Austria	3.45	6.18	0.29	Thailand	3.87	6.55	0.49
Singapore	3.51	6.60	0.30	Macao	3.01	5.99	0.50
Tunisia	4.31	5.98	0.30	USA (North Carolina)	3.35	5.59	0.50
Turkey	5.37	6.86	0.30	B-S-J-G (China)	3.98	6.66	0.50
Peru	3.60	6.02	0.31	Ireland	3.01	4.70	0.51
France	2.80	5.75	0.31	Chile	4.24	5.96	0.53
Korea	4.29	6.91	0.31	United Arab Emirates	3.59	5.54	0.53
Italy	3.12	5.90	0.31	Colombia	4.37	5.83	0.57
Brazil	3.95	6.78	0.32	Switzerland	4.29	6.41	0.59

Note: authors' own calculations. Data source: PISA database 2015, available online: <https://www.oecd.org/pisa/data/>.

(a) ISCED 0

(b) ISCED 1

Table A6
Robustness tests

	Math test scores			Reading test scores		
	(1)	(2)	(3)	(4)	(5)	(6)
Preschool Starting Age (PSA)	-0.065*** (0.014)	-0.106** (0.037)	-0.062*** (0.015)	-0.091*** (0.014)	-0.139** (0.040)	-0.085*** (0.014)
IV	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA	EPSA, ESSA
SSA included as an endogenous variable	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Individual covariates	Y	Y	Y	Y	Y	Y
Month of birth	1-12	1, 5, 6, 12	1-12	1-12	1, 5, 6, 12	1-12
SEN students included	Y	Y	Y	Y	Y	Y
Number of clusters	1602	1602	1602	1602	1602	1602
Number of observations	809322	214485	618636	809483	214535	618773
Adjusted R2	-0.030	0.145	0.129	-0.031	0.177	0.155
AIC	2130348.915	520626.694	1504839.396	2125382.986	510842.816	1475854.247

* p < 0.05, ** p < 0.01, *** p < 0.001

Appendix B. The validity of the instrumental variables approach

As Angrist and Pischke (2009) explain, for an instrument to be valid, it needs to meet three essential conditions, and one additional condition if a local average treatment effect (LATE) is estimated. First, it should be relevant; that is, the first stage of the 2SLS should have explanatory power. In Table B1, the first stage F-statistics of the first stage equations (Equations 4 and 5) are presented along with the relevant coefficients and significance levels. The F-statistics are sufficiently large, and the instruments have a large and significant effect on the endogenous variables in each specification. Additional empirical tests indicate that EPSA and ESSA serve as strong instruments. In particular, the Kleibergen-Paap rk Wald F-statistic is high (429.66), as well as the Cragg-Donald Wald

F-statistic (4666.65). The under-identification test (Kleibergen-Paap rk LM statistic equal to 51.73) and the Stock-Yogo weak ID test (7.03) do not signal any issues.

Second, the independence of the instruments should hold, so that they are as good as random. Buckles and Hungerman (2012) showed that maternal characteristics, such as level of education, differ for children born at different months of the year, so we need to check whether this holds in our data. The main results without covariates (in Cols 1 and 4 of in Appendix A), were practically the same as those in our main specification. This is a strong indication for IV independence. Further, we checked directly whether in our sample, the month of birth has predictive power regarding the level of education of the mother or the father. We regressed various levels of education of the mother and the

Table B1
First-stage results (Equations 4 and 5)

	6 th grade		8 th grade		10 th grade	
	PSA	SSA	PSA	SSA	PSA	SSA
Expected Preschool Starting Age (EPSA)	0.221*** (0.011)	0.174*** (0.006)	0.245*** (0.011)	0.182*** (0.008)	0.279*** (0.012)	0.197*** (0.008)
Expected School Starting Age (ESSA)	-0.040*** (0.009)	0.244*** (0.011)	-0.032*** (0.008)	0.267*** (0.013)	-0.031*** (0.009)	0.312*** (0.016)
F-stat	231.32	234.37	385.73	748.30	661.50	970.66
Observations	664282	664282	685837	685837	684603	684603

* p < 0.05, ** p < 0.01, *** p < 0.001

Note: Robust standard errors clustered on the municipality level are in parentheses. The individual covariates include year and school fixed effects, gender, special education needs, entitlement to a cheaper or free meal indicating poor financial circumstances of the family, whether a student is living with their own family, the mothers and fathers level of education, employment status, and age, whether the students family has a car, a bathroom with public utilities, an internet connection at home, the number of books at home, whether the student has their own desk, and whether the family helps the student with their homework

father as a dependent variable on month of birth dummy variables. The point estimates are mostly insignificant and less than 0.01 (Table 5 in the Online Appendix.)

Third, the instrument should impact the outcome merely through the endogenous variable, and not through any other channel. There exists some evidence that children slightly differ in their health outcomes at birth by their season of birth (Bound & Jaeger, 1996). However, we argue that these slight differences are likely to diminish during the 12 years until grade 6. Additionally, we used Hungarian birth registry data containing all births in the cohorts included in our estimation sample, and we tested whether the month of birth has any predictive power for birth weight in our data. In Tables 6 and 7 in the Online Appendix, we present the results of our regressions. The probability of being low birth weight or very low birth weight (<2500 g or <1500 g), and the month of birth dummies are on the right-hand side. The point estimates are mostly zero and insignificant and are under 1.5 percent in all cases. Additionally, in Table 8 in the Online Appendix, we regress month of birth dummies on birth weight in each year using the same birth registry data. In most years and birth months, the coefficients are insignificant, and the point estimates are under 1 percent of the average birth weight.

Since we estimate a LATE model, we have an additional condition to evaluate: monotonicity. As Mogstad, Torgovitsky, and Walters (2019) showed in their study, with two instruments, the classical monotonicity condition can only be satisfied with severe restrictions on choice behavior. The partial monotonicity condition they propose is closer to our case, but still not entirely applicable. Instead, we used simple visual observation of Figure 1 and Figure 2, which demonstrate how the actual enrollment ages respond to the instruments.

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