Delayed school entry increases internal locus of control

Dániel Horn[a,b], Hubert János Kiss[a,b,*], Ágnes Szabó-Morvai[a,c,d]

[a] *HUN-REN KRTK Institute of Economics, 1097 Budapest, Tóth Kálmán u. 4.*
[b] *Corvinus University, 1093 Budapest, Fővám tér 9., Hungary*
[c] *University of Debrecen, 4032 Debrecen, Egyetem tér 1, Hungary*
[d] *CERGE-EI Foundation Teaching Fellow*

Abstract

We study the impact of delayed school entry on the locus of control (LoC) among Hungarian students, using statutory cutoff dates for school enrollment as a plausibly exogenous variation. Our findings indicate a causal relationship between delayed school entry and an increase in internal LoC, with a policy effect of approximately one-tenth of a standard deviation for 8th-grade students, which corresponds to a one-third standard deviation effect for complier students. The policy implications of these findings are significant, providing evidence that delaying school start could serve as an effective intervention to enhance LoC among students, which is positively associated with many later life outcomes.

Keywords: causal relationship, locus of control, redshirting, school starting age
JEL: D91, I21, I26, I28, I29

---

*Corresponding author: Hubert János Kiss (kiss.hubert.janos@krtk.hun-ren.hu)

[1] Financial support from the NKFIH-K143415 (Dániel Horn), the Hungarian Academy of Sciences Momentum Grant No. LP2021-2 (Hubert János Kiss), the NKFIH-FK131422 (Ágnes Szabó-Morvai), the Hungarian Academy of Sciences Momentum Grant No. LP2018-2/2018 (Ágnes Szabó-Morvai), and the University of Debrecen Program for Scientific Publication (Ágnes Szabó-Morvai) is gratefully acknowledged.

1. Introduction

Having an internal locus of control (LoC), the belief that one's actions are causally related to life outcomes, has been shown to positively correlate with many relevant life outcomes, including educational attainment (Coleman and DeLeire, 2003; Szabó-Morvai and Kiss, 2023), labor market outcomes (Cobb-Clark, 2015), financial decisions (Cobb-Clark et al., 2016) and healthy behaviors (Cobb-Clark et al., 2014). Consequently, understanding how to foster internal control tendencies is of significant interest. However, LoC is considered a stable (though not time-invariant) trait, with only significant life events, such as chronic health issues, capable of altering it (Cobb-Clark and Schurer, 2013; Elkins et al., 2017). It is less known whether certain educational interventions are influential enough to alter an individual's LoC.

Postponing the school starting age (SSA) of children has been argued to significantly influence short- and mid-term outcomes, including test scores (Bedard and Dhuey, 2006; Cornelissen and Dustmann, 2019; Cook and Kang, 2020; Balestra et al., 2020; Szabó-Morvai et al., 2023), grades (Attar and Cohen-Zada, 2018), track choice (Fredriksson and Öckert, 2014; Schneeweis and Zweimüller, 2014), health (Balestra et al., 2020), and non-cognitive skills (Cornelissen and Dustmann, 2019). However, it appears to have little to no effect on longer-term labor market outcomes (Black et al., 2011; Fredriksson and Öckert, 2014; Balestra et al., 2020).[2]

This study tests whether postponing school starting by one year influences LoC. We exploit the statutory cutoff dates for school enrollment in Hungary as a plausibly exogenous variation in school starting age to investigate its long-term effects on LoC. To date, there has been only scant evidence on the causal relationship between a life event or a policy intervention and a change in LoC. Gottschalk (2005) analyzes a wage subsidy intervention targeted at welfare recipients, depending on their employment. This incentive introduced an exogenous variation in employment status, leading to a notable shift in participants' LoC toward more internal tendencies, an effect that persisted up to three years post-program. Similarly, Preuss and Hennecke (2018) reveal a causal relationship between unemployment and LoC, with a change amounting to 30% of a standard deviation.[3] Furthermore, by exploiting cohort-specific eligibility age for pension, Clark and Zhu (2023) show that retirement increases internal LoC by 0.57 of a standard deviation.

[2] The potential reason for this latter null effect is the 'age effect'; i.e. redshirted students (those who delay enrollment by one year) are a year older than their non-delayed peers when tested at the same time (Black et al., 2011).

[3] Interestingly, the authors argue that this effect is not long-lasting, as it reflects the current state (unemployment) rather than the underlying trait (LoC).

To our knowledge, no studies have investigated the effects of an educational intervention on LoC. We contribute to the literature by establishing a causal relationship between delaying school start by one year and an increase in internal LoC. We document an intent-to-treat effect of approximately 0.1 standard deviation from a later school start on LoC in 8th grade, which corresponds to a one-third standard deviation effect for complier students. This impact is significantly more long-lasting than those documented in existing literature.

The remainder of the paper is structured as follows. Section 2 presents the data. The empirical strategy is explained in Section 3. Section 4 contains the main results, while Section 5 concludes.

2. Data

We utilize the Hungarian Life Course Survey (HLCS), a detailed longitudinal dataset from the TÁRKI Social Research Institute. This dataset includes a representative sample of 10,022 adolescents who completed the Hungarian National Assessment of Basic Competences (NABC) in May 2006, a national low-stakes assessment in reading and math in the 8th grade.[4]

The first wave of the HLCS was conducted between October and December 2006, after students had enrolled in upper secondary education in 9th grade. In total, there were six waves of the HLCS, running until 2012. In this study, we use only the first wave (9th grade) of the survey. The HLCS oversampled the socially disadvantaged population, but corrective probability weights are applied throughout the study to ensure the results are representative of the total population.

In 2006, this survey included a section on LoC using a short version of the Rotter scale (Rotter, 1966), similar to the approach used by the National Longitudinal Surveys of Youth (NLSY) in the United States. Participants chose from pairs of statements that best reflected their views on life.[5] Following Mendolia and Walker (2014), we applied factor analysis to construct our LoC metric, standardizing it to have a mean of zero and a variance of one, with higher scores indicating a stronger internal LoC.

The main advantage of the HLCS data is its detailed account of students' parental backgrounds and educational careers. Therefore, it is possible to control for various individual characteristics to account for potential confounders and unwanted mechanisms (see the list of main variables in Table 1 below). Math and reading test scores, measured in the NABC, are also included in the HLCS database. These test scores are z-standardized for the total

[4]A noteworthy paper using the HLCS database is Kertesi and Kézdi (2011). For details of the Hungarian assessment system and the NABC database, see Balázsi and Ostorics (2020).

[5]See Appendix, SectionA1.1, for the statements.

population.

The variable of prime interest is school starting age (SSA), which we measure in two ways. First, in the HLCS survey, parents were asked at what age their child had enrolled in primary education, with options being age 6, age 7, or age 8 or later. However, it is unclear how parents interpreted 'age'. For instance, a parent might report that their child was 7 at school start if the child was born in October, even though the child was actually only 6 years and 11 months old. This ambiguity is illustrated in the left panel of Figure 1, where the average school starting age does not differ between students born in the fall, despite October-born children, on average, being around a month older than those born in November. To address this, we also calculate another measure using the birth year and month of the students. By subtracting the years spent in education from their actual age in the 9th grade, we obtain a more accurate measure. However, this calculated measure is only available for the 8,573 students (out of the total 9,973) who have not repeated any classes throughout their educational career. This measure appears to show a more intuitive relationship with the birth month of the students, as seen in the right panel of Figure 1. The top of Table 1 shows the descriptive statistics of the outcome variable and the main variables of interest.

To control for potential confounders, we include a variety of variables: sex, type of upper secondary school, previous grade retention, special education needs, expulsion history, various family background characteristics (such as household size, parental financial distress in 2006, parental age, dummies for mother's and father's education levels, whether the student was disadvantaged in 2006, parental divorce, and whether the student lives with parents), birth weight and some caregiving proxies (such as whether parents read tales to the child, and the HOME (Home Observation for Measurement of the Environment) cognitive and emotional scales, NLSY (2004)). Additionally, we consider the duration of pre-primary education attended by the child. Even though our causal identification relies on the exogenous variation from whether a student was born before or after the school enrollment cutoff date, accounting for these variables allows us to clearly exclude the possibility that school starting age is related with LoC through these factors, e.g. students from more favorable families starting school later. The bottom part of Table 1 presents the descriptive statistics of these main control variables used in the estimations.

Table 1: Descriptive statistics

	N	Mean	SD	Min	Max
Main					
LoC	9898	0.00	1.00	-2.9	1
Math score	9973	-0.06	1.02	-3.2	3.1
Reading score	9973	-0.10	1.03	-3.8	2.9
SSA Survey	9964	6.69	0.49	6	9
SSA Calculated	8573	6.89	0.41	5.7	9.6
Treated (Born after June 1)	9677	0.59	0.49	0	1
Controls					
Female	9973	0.48	0.50	0	1
School type: vocational	9838	0.41	0.49	0	1
School type: academic	9838	0.36	0.48	0	1
School type: other	9838	0.01	0.12	0	1
Grade retention in grades 1-4	9973	0.05	0.22	0	1
Grade retention in grades 5-8	9973	0.04	0.20	0	1
Special education needs	9973	0.06	0.23	0	1
Expelled from school	9973	0.01	0.12	0	1
Household size	9973	4.24	1.34	2	15
Financial distress (2006)	9973	0.31	0.46	0	1
Birthweight	9973	3234.28	554.46	750	5700
Age of female caretaker	9973	41.08	6.32	9	82
How often read tales	9973	17.36	8.81	0	25
HOME cognitive scale	9822	83.43	25.81	0	130
HOME emotional scale	9690	98.79	22.23	10	140
Childcare enrollment (years)	9973	3.29	0.61	.5	3.5
Mother: less than high school	9973	0.49	0.50	0	1
Mother: high school	9973	0.32	0.47	0	1
Father: less than high school	9973	0.69	0.46	0	1
Father: high school	9973	0.19	0.39	0	1
Social disadvantage (2006)	9973	0.62	0.49	0	1
Parents divorced	9973	0.22	0.42	0	1
Lives with father	9973	0.79	0.41	0	1
Lives with mother	9973	0.97	0.17	0	1

Notes: Sample weights are used for the calculation of the mean and the standard deviation.
SSA Survey - School Starting Age as reported in the survey;
SSA Calc - School Starting Age calculated from actual age and grade
(only for those with no grade retention).

3. Empirical strategy

In Hungary, children are required to enroll in primary education at the age of six or seven.[6] For the children in our data, those born before June 1st should enroll in primary school in September of the same year they turn six.[7] Those born between June 1st and December 31st are to enroll in primary school in September of the following year, after they have turned six.

However, redshirting is possible. Specifically, students (or their parents) can choose to postpone the school start by one year. For those born between June and December, this requires submitting a parental request to delay school enrollment and obtaining approval from the child's preschool and the local government Developmental Advisory Board. This process involves a standardized evaluation by developmental experts, which is free of charge but incurs time and travel costs for parents. Conversely, children born between January and May face a lower administrative hurdle for redshirting, as the parental request only requires approval from their preschool (for details, see Szabó-Morvai et al., 2023).

Figure 1 illustrates the correlation between birth month and the two school starting age (SSA) measures. While each measure has its own potential issues, both clearly indicate a significant increase in the probability of a delayed school start between students born in May and June.

Similar to various previous studies (e.g. Schneeweis and Zweimüller, 2014; Attar and Cohen-Zada, 2018, and many more), we use birth months as exogenous variation to identify effects on later outcomes. We particularly focus on the difference between those born before and after June 1st, as the policy effect should be more pronounced there. However, this identification strategy does not allow us to disentangle the effect of school starting age from the age at testing, as these are perfectly correlated – excluding grade repeaters. To separate these effects, we would need two sources of exogenous variation, which are not available to us.[8]

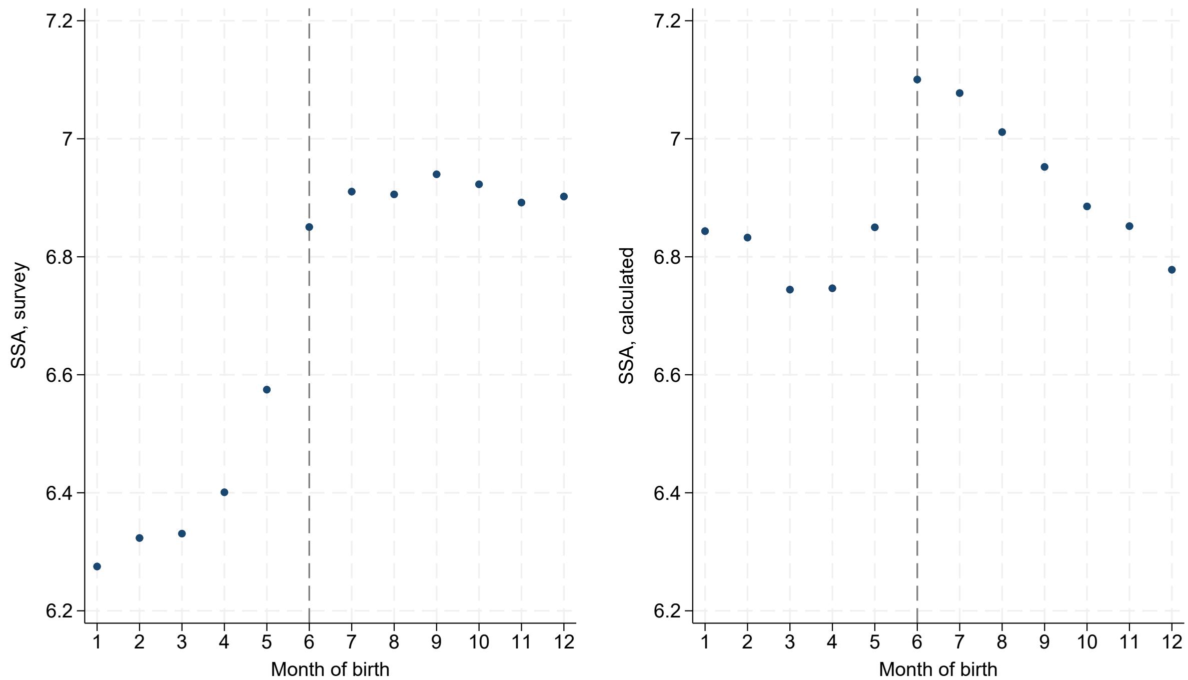
However, unlike standardized testing (a proxy for cognitive abilities), personality traits (including locus of control) might be considered less susceptible to the effects of aging. For instance, Elkins et al. (2017) investigate the stability of personality traits over time. Their analysis of various traits, including LoC, during adolescence concludes that *"most*

[6] However, in special cases such as when the child has special education needs, school enrollment can be further delayed.

[7] This cutoff date was changed to September 1st in 2013, but it was June 1st for the cohort in our data.

[8] A notable study that achieves this separation is Black et al. (2011), who utilized variation in school starting age from school entry cutoff dates and variation in testing age from test dates in the military IQ tests in Norway. Their findings highlight the significance of the age at the test.

Figure 1: Average school starting age (SSA) by month of birth



Note: The left panel shows SSA as reported by parents in the HLCS survey. The right panel shows SSA as calculated by subtracting the time spent in education from individual age (excluding grade repeaters).

individuals do not change their scores in a statistically reliable way during adolescence and young adulthood, or changes occur in equal proportions in opposite directions" (Elkins et al., 2017, p.37).

We define our treatment (T) as being born after June 1st. Our baseline model is a reduced form estimate, as shown in equation 1, where we regress LoC on this treatment. We include all control variables listed in Table 1 and incorporate lower secondary school fixed effects to account for unobserved differences related to school choice in primary education, as well as differences in teacher or peer quality during those eight years.

$$LoC_i = T_i + \sum_{n=1}^{k} \beta_n X_i + \eta_s + \varepsilon_i \tag{1}$$

where i represents the student and s denotes the school in 8th-grade, X is the vector of k individual control variables, η_s captures the school fixed effects, and ε_i is the idiosyncratic error term.

We will test the robustness using alternative sample specifications, focusing on students born around the cut-off, and examining whether excluding various controls alters the results.

In addition, we will compare our findings with previous results on standardized test scores to determine whether the effect of delaying school start on LoC is mediated by cognitive skills.

In a second set of regressions (see equation 2), we replace the T treatment dummy we include month dummies (λ_m) and present the results in Figure 2.

$$LoC_i = \sum_{n=1}^{k} \beta_n X_i + \eta_s + \lambda_m + \varepsilon_i \tag{2}$$

To assess the estimated size of the causal effect on the treated, we estimate the local average treatment effects using a 2SLS instrumental regression. Equation (3) shows the first stage where the dependent variable is the (potentially endogenous) school starting age (SSA). On the right-hand side, we include month-of-birth dummies as instrumental variables and the control variables.

$$SSA_i = \alpha_0 + \sum_{m=1}^{11} \lambda_m + \sum_{n=1}^{k} \gamma_n X_i + \eta_s + u_i \tag{3}$$

Equation (4) shows the second stage, where the dependent variable is LoC, and the right-hand side includes the instrumented SSA.

$$LoC_{is} = \delta_0 + \delta_1 \widehat{SSA}_i + \sum_{n=1}^{k} \beta_n X_i + \eta_s + \varepsilon_i \tag{4}$$

Lastly, we examine whether there are any heterogeneities within this effect.

4. Results

Columns (1) to (3) of Table 2 present the baseline regression results which can be interpreted as a reduced form estimate. Here, we compare the average LoC scores of students born after June 1st (treated) to those born before this date (untreated). In the total sample, treated students exhibit LoC scores that are approximately 0.07 standard deviations higher than their untreated peers. This result is significant at the 5% level. When we narrow our focus to the 3 months immediately before and after the June 1st cutoff (from March to August), the effect size increases to 0.1 standard deviations, with the level of significance remaining at 5%. By further narrowing the sample to 2 months on either side of the cutoff, the effect size increases to 0.145 standard deviations and becomes only marginally significant (at the 10% level) as the sample size decreases substantially. We will use the March-August sample for subsequent estimations, as it represents a narrow band around the cutoff, yet the sample size remains large enough to yield statistically meaningful results.

Table 2: Reduced form estimates

	1) Months included			2) Testing controls			3) With test scores		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	LoC	LoC	LoC	LoC	LoC	LoC	Math score	Reading score	LoC
Treated (Born after June 1)	0.072**	0.108**	0.148*	0.118*	0.074**	0.086**	0.094*	0.086	0.098**
	(0.025)	(0.035)	(0.056)	(0.050)	(0.022)	(0.022)	(0.037)	(0.051)	(0.035)
Math score									0.082**
									(0.024)
Reading score									0.027
									(0.031)
Observations	9128	4607	3018	4607	4607	4607	4607	4607	4607
Months included	All	Mar-Aug	Apr-Jul	Mar-Aug	Mar-Aug	Mar-Aug	Mar-Aug	Mar-Aug	Mar-Aug
School FE	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Indiv. controls	Yes	Yes	Yes	No	Yes	No	Yes	Yes	Yes

Standard errors are shown in parentheses.

Notes: All controls as listed in Table 1 and including lower-secondary school fixed effects.

Robust standard errors, clustered at the birth month level.

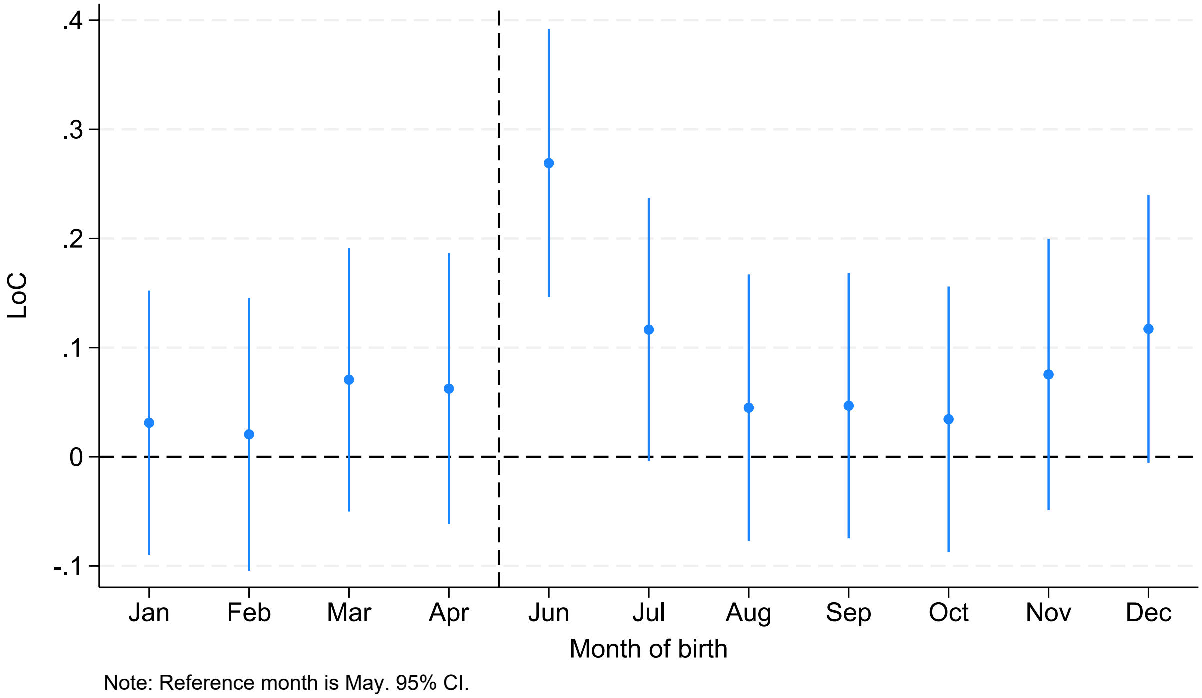
The specifications are varied in the groups by: 1) the number of months included in the sample 2) the control variables included in the regressions, and 3) testing for test scores as possible confounders.

The full regression table for specifications 1 to 3 is reported in Table A.5 in the Appendix.

The full regression tables of specifications 4 to 9 are available from the authors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2: Coefficients of the months dummies from equation 2 - 9th grade



Columns (4) to (6) of Table 2 evaluate the importance of control variables in our analysis. The removal of all individual-level controls (Col (4)) listed in Table 1 does not significantly affect the point estimates. However, removing school fixed effects (Col (5)) reduces the size of the coefficient. This suggests that different primary schools may have varied practices regarding redshirting or that they may differ in their ability to address initial differences among students starting school, such as fine motor skills, endurance, or discipline. Even without school-fixed effects and individual controls (Col (6)), we observe a significant and sizable difference (0.084 s.d.) in LoC scores between treated and untreated students.

Previous research has primarily focused on the impact of delayed school start on test scores. Using a larger dataset from Hungary for later-born cohorts of students and employing a similar but distinct estimation procedure, Szabó-Morvai et al. (2017) found that a one-year delay in the start of school increases test scores in 8th grade by 0.15 s.d. in math and 0.18 s.d. in reading. Our estimates in Col (7) and (8) of Table 2 are comparable to this as we estimate a 0.094 and a 0.086 s.d. increase in math and reading test scores. This result raises the question of whether the higher LoC of students born in June is merely a consequence of higher test score results of the redshirted students. To test this, in column (9) we control for the 8th grade test scores, but even after accounting for these test scores, the differences between the LoC of treated and untreated students persist.

Table 3: Instrumental variable estimates (2nd stage - LATE)

	(1) LoC	(2) LoC	(3) LoC	(4) LoC
SSA Survey	0.118**	0.209**		
	(0.042)	(0.074)		
SSA Calculated			0.329*	0.376**
			(0.160)	(0.126)
Observations	8758	4078	7788	3610
Months included	All	Mar-Aug	All	Mar-Aug
School FE	Yes	Yes	Yes	Yes
Indiv. controls	Yes	Yes	Yes	Yes

Standard errors are in parentheses.

Notes: All controls as listed in Table 1, and with lower-secondary school fixed effects included. Robust standard errors, clustered at the birth month level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As a robustness test for our simplified treatment measure, we examine conditional LoC differences between students born in different months. Figure 2 displays the month coefficients from equation 2, with students born in May as the reference group. Students born in June exhibit significantly higher LoC scores compared to those born in May, and the difference between students born in July and May remains sizable and significant. If our assumption that the month of birth is exogenous to students' internal control tendencies holds true, this difference can be explained by the fact that, even with the possibility of redshirting, the average school starting age of students born in June and July is about three months higher than that of students born in May.

Table 3 presents the second stage of the instrumental variable estimations. Under this assumption, the Local Average Treatment Effect (LATE) estimates indicate that complier students benefit from a delayed school start, with an increase in locus of control (LoC) ranging from 0.2 to 0.36 standard deviations. This effect is substantial and statistically significant. In Columns (1) and (2), the SSA reported in the survey is used, while in Columns (3) and (4) the calculated SSA is included. The estimates are presented for the entire sample (columns 1 and 3) and the sample restricted to students born between March and August (columns 2 and 4). Our preferred estimate is in Column (4), which indicates that delaying school start by one year increases the internal LoC of complier students by 0.369 standard deviations.

We report heterogeneity tests in Tables A.6 and A.7, examining the student's gender and the mother's highest level of education. These estimates reveal no significant effects.

5. Discussion

In this article, we examine the effect of delayed school starting on locus of control. We find that delaying school start by one year increases internal LoC of complier students by approximately one-third of a standard deviation. This effect size is comparable to the LoC difference between regular students (0.03) and students with special education needs (-0.31), and is larger than the gap between students with low-educated (-0.1) and those with high-educated mothers (0.13).

The intuitive explanation behind our main finding might be as follows: Students who start school later and are therefore older and more mature in the classroom may have better fine motor skills, stronger endurance, and higher levels of discipline. These attributes could make them more successful in school (e.g., achieving higher test scores), leading to a stronger sense of control over their achievements, and consequently, higher scores on the internal locus of control scale.

Compared to other studies on factors that causally change LoC, our study stands out for documenting the longest significant impact, with a causal change observed 8 years after the intervention.[9] Previously, only early-childhood interventions have been argued to have such long-lasting effects, typically not on test scores but on non-cognitive outcomes, income or criminal activity (e.g. Deming, 2009; Carneiro and Heckman, 2003).

Our findings have policy relevance, particularly given that the prevalence of redshirting ranges from 5 to 10% in many countries (OECD, 2021). As previously discussed, redshirting has been associated with improved academic achievement, prompting suggestions for its adoption as a policy measure to address gender disparities in educational outcomes (Fortin et al., 2015; Reeves, 2022). Our results suggest that a mechanism through which redshirting may exert its long-lasting effects is by enhancing internal control tendencies.

6. Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author(s) used ChatGPT in order to improve the English and the readability of the study. After using this tool/service, the author(s) reviewed and edited the content as needed and take(s) full responsibility for the content of the publication.

[9] Gottschalk (2005) report a causal change in LoC after 3 years, Preuss and Hennecke (2018) do not document long-term LoC changes, while Clark and Zhu (2023) does not have a well-defined horizon.

References

Attar, I. and Cohen-Zada, D. (2018). The effect of school entrance age on educational outcomes: Evidence using multiple cutoff dates and exact date of birth. *Journal of Economic Behavior & Organization*, 153:38–57.

Balázsi, I. and Ostorics, L. (2020). *The Hungarian Educational Assessment System*, pages 157–169. Springer International Publishing, Cham.

Balestra, S., Eugster, B., and Liebert, H. (2020). Summer-born struggle: The effect of school starting age on health, education, and work. *Health economics*, 29(5):591–607.

Bedard, K. and Dhuey, E. (2006). The persistence of early childhood maturity: International evidence of long-run age effects. *The Quarterly Journal of Economics*, 121(4):1437–1472.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2011). Too young to leave the nest? the effects of school starting age. *The review of economics and statistics*, 93(2):455–467.

Carneiro, P. M. and Heckman, J. J. (2003). *Human capital policy*, pages 77–240. Cambridge, MA: MIT Press.

Clark, A. E. and Zhu, R. (2023). Taking Back Control? Quasi-Experimental Evidence on the Impact of Retirement on Locus of Control. *The Economic Journal*, page uead118.

Cobb-Clark, D. A. (2015). Locus of control and the labor market. *IZA Journal of Labor Economics*, 4:1–19.

Cobb-Clark, D. A., Kassenboehmer, S. C., and Schurer, S. (2014). Healthy habits: The connection between diet, exercise, and locus of control. *Journal of Economic Behavior & Organization*, 98:1–28.

Cobb-Clark, D. A., Kassenboehmer, S. C., and Sinning, M. G. (2016). Locus of control and savings. *Journal of Banking & Finance*, 73:113–130.

Cobb-Clark, D. A. and Schurer, S. (2013). Two economists' musings on the stability of locus of control. *The Economic Journal*, 123(570):F358–F400.

Coleman, M. and DeLeire, T. (2003). An economic model of locus of control and the human capital investment decision. *Journal of Human Resources*, 38(3):701–721.

Cook, P. J. and Kang, S. (2020). Girls to the front: How redshirting and test-score gaps are affected by a change in the school-entry cut date. *Economics of Education Review*, 76:101968.

Cornelissen, T. and Dustmann, C. (2019). Early school exposure, test scores, and noncognitive outcomes. *American Economic Journal: Economic Policy*, 11(2):35–63.

Deming, D. (2009). Early childhood intervention and life-cycle skill development: Evidence from head start. *American Economic Journal: Applied Economics*, 1(3):111–134.

Elkins, R. K., Kassenboehmer, S. C., and Schurer, S. (2017). The stability of personality traits in adolescence and young adulthood. *Journal of Economic Psychology*, 60:37–52.

Fortin, N. M., Oreopoulos, P., and Phipps, S. (2015). Leaving boys behind: Gender disparities in high academic achievement. *Journal of Human Resources*, 50(3):549–579.

Fredriksson, P. and Öckert, B. (2014). Life-cycle effects of age at school start. *The Economic Journal*, 124(579):977–1004.

Gottschalk, P. (2005). Can work alter welfare recipients' beliefs? *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management*, 24(3):485–498.

Kertesi, G. and Kézdi, G. (2011). The roma/non-roma test score gap in hungary. *American Economic Review*, 101(3):519–525.

Mendolia, S. and Walker, I. (2014). The effect of personality traits on subject choice and performance in high school: Evidence from an english cohort. *Economics of Education Review*, 43:47–65.

NLSY (2004). Nlsy79 child and young adult data users guide.

OECD (2021). Education at a glance: Enrolment rate by age (edition 2020).

Preuss, M. and Hennecke, J. (2018). Biased by success and failure: How unemployment shapes locus of control. *Labour Economics*, 53:63–74.

Reeves, R. (2022). *Of boys and men: Why the modern male is struggling, why it matters, and what to do about it.* Brookings Institution Press.

Rotter, J. B. (1966). Generalized expectancies for internal versus external control of reinforcement. *Psychological monographs: General and applied*, 80(1):1.

Schneeweis, N. and Zweimüller, M. (2014). Early tracking and the misfortune of being young. *The Scandinavian Journal of Economics*, 116(2):394–428.

Szabó-Morvai, Á., Horn, D., Lovász, A., and De Witte, K. (2017). Human capital effects of kindergarten and school enrolment timing. Technical report, Budapest Working Papers on the Labour Market.

Szabó-Morvai, Á., Horn, D., Lovász, A., and De Witte, K. (2023). Universal preschool and cognitive skills–the role of school starting age as a moderating factor. *Early Childhood Research Quarterly*, 64:278–289.

Szabó-Morvai, Á. and Kiss, H. J. (2023). Locus of control, educational attainment, and college aspirations: the relative role of effort and expectations. *Education Economics*, pages 1–20.

A1. Supplementary tables and figures

A1.1. Locus of control test

Participants were presented with the following pairs of statements and were asked to select the one that best describes their judgment about their own lives, with statements in italics indicating external LoC:

- 1A - What happens to me is first of all my own doing.
- *1B - Sometimes I feel that I don't have enough control over the direction my life is taking.*
- 2A - When I make plans, I am almost certain that I can make them work.
- *2B - It is not always wise to plan too far ahead because many things turn out to be a matter of good or bad fortune anyhow.*
- 3A - In my case getting what I want has little or nothing to do with luck.
- *3B - Many times we might just as well decide what to do by flipping a coin.*
- 4A - Many times I feel that I have little influence over the things that happen to me.
- *4B - It is impossible for me to believe that chance or luck plays an important role in my life.*

Table A.4: The locus of control pairs of statements - Rotter test

Question	N	%
a.) What happens to me is first of all my own doing. - Internal control	5,394	78.78%
Sometimes I feel that I don't have enough control over the direction my life is taking. - External control	1,453	21.22%
b.) When I make plans, I am almost certain that I can make them work. - Internal control	4,609	67.33%
It is not always wise to plan too far ahead because many things turn out to be a matter of good or bad fortune anyhow. - External control	2,236	32.67%
c.) In my case getting what I want has little or nothing to do with luck. - Internal control	5,443	79.61%
Many times we might just as well decide what to do by flipping a coin. - External control	1,394	20.39%
d.) Many times I feel that I have little influence over the things that happen to me. - External control	2,666	39.1%
It is impossible for me to believe that chance or luck plays an important role in my life. - Internal control	4,153	60.9%

A1.2. Additional tables and figures

Table A.5: Full reduced form estimates

	(1) LoC		(2) LoC		(3) LoC	
Treated (Born after June 1)	0.072**	(0.025)	0.108**	(0.035)	0.148*	(0.056)
Female	-0.050**	(0.022)	-0.011	(0.028)	-0.018	(0.018)
School type: vocational	0.157***	(0.045)	0.119	(0.068)	0.099	(0.068)
School type: academic	0.210***	(0.039)	0.076*	(0.037)	0.076	(0.035)
School type: other	-0.078	(0.184)	-0.288*	(0.128)	-0.136	(0.095)
Grade retention in grades 1-4	0.026	(0.077)	-0.112	(0.101)	-0.087	(0.181)
Grade retention in grades 5-8	0.001	(0.074)	0.041	(0.054)	0.073	(0.109)
Special education needs	-0.205***	(0.024)	-0.311***	(0.038)	-0.380**	(0.069)
Expelled from school	0.086	(0.168)	0.079	(0.205)	0.137	(0.200)
Household size	0.001	(0.013)	-0.001	(0.020)	0.007	(0.027)
Financial distress (2006)	-0.047	(0.028)	-0.042	(0.043)	-0.046	(0.083)
Birthweight	-0.000*	(0.000)	-0.000	(0.000)	0.000	(0.000)
Age of female caretaker	-0.003	(0.002)	0.000	(0.003)	-0.004	(0.004)
How often read tales	0.001	(0.002)	-0.001	(0.003)	0.001	(0.006)
HOME cognitive scale	0.004***	(0.001)	0.006***	(0.001)	0.005*	(0.002)
HOME emotional scale	0.003***	(0.001)	0.003*	(0.001)	0.002	(0.002)
Childcare enrollment (years)	-0.029	(0.026)	-0.049	(0.041)	-0.010	(0.018)
Mother: less than high school	0.013	(0.055)	0.014	(0.074)	0.031	(0.109)
Mother: high school	0.054	(0.046)	0.068	(0.093)	0.054	(0.132)
Father: less than high school	-0.027	(0.043)	-0.115	(0.058)	-0.048	(0.076)
Father: high school	-0.039	(0.049)	-0.163	(0.098)	-0.131	(0.128)
Social disadvantage (2006)	-0.051**	(0.022)	-0.074***	(0.016)	-0.057	(0.058)
Parents divorced	-0.012	(0.041)	-0.078	(0.041)	-0.076	(0.050)
Lives with father	0.002	(0.047)	-0.028	(0.068)	-0.024	(0.104)
Lives with mother	0.136	(0.104)	0.229	(0.115)	0.146	(0.256)
Constant	-0.510*	(0.255)	-0.661*	(0.312)	-0.683	(0.556)
Observations	9128		4607		3018	
Months included	All		March-Aug		April-July	
School FE	Yes		Yes		Yes	
Indiv. controls	Yes		Yes		Yes	

Standard errors in parentheses

Notes: With lower-secondary school fixed effects, coefficients are not reported.

Robust standard errors, clustered at the birth month level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Reduced form estimates - heterogeneity tests

	(1)	(2)
	LoC	LoC
Treated (Born after June 1)	0.100	0.131*
	(0.053)	(0.060)
T x Female	0.016	
	(0.051)	
T x Mother: low ed.		-0.047
		(0.104)
Observations	4607	4607
Months included	All	March-Aug
School FE	Yes	Yes
Indiv. controls	Yes	Yes

Standard errors are in parentheses.

Notes: All controls as listed in Table 1 and with lower-secondary school fixed effects included. Robust standard errors, clustered at the birth month level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Reduced form estimates - gender and mother's education subsamples

	(1)	(2)	(3)	(4)
	LoC	LoC	LoC	LoC
Treated (Born after June 1)	0.100	0.092	0.083	0.129
	(0.051)	(0.097)	(0.077)	(0.115)
Observations	2163	2444	2443	2164
Months included	All	All	All	All
School FE	Yes	Yes	Yes	Yes
Indiv. controls	Yes	Yes	Yes	Yes
Gender sample	Female	Male	All	All
Mother education	All	All	Low	Mid-High

Standard errors are in parentheses.

Notes: All controls as listed in Table 1 and with lower-secondary school fixed effects included. Robust standard errors, clustered at the birth month level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$