Personality and Individual Differences 183 (2021) 111121

Contents lists available at ScienceDirect

Personality and Individual Differences

journal homepage: www.elsevier.com/locate/paid

Intensive schooling and cognitive ability: A case of Polish educational reform

Maciej Karwowski [a,*], Bogusław Milerski [b]

[a] Institute of Psychology, University of Wrocław, Poland
[b] Department of Social Sciences, Christian Theological Academy in Warsaw, Poland

ARTICLE INFO

Keywords:
Intelligence
Schooling
Intensity of education
Educational reforms

ABSTRACT

Intelligence is the most critical predictor of school successes, yet the length and quality of education influence cognitive development as well. Using data collected during the recent educational reform introduced in Poland, this study examined whether schooling's intensity relates to changes in cognitive abilities. Due to the reformed structure and curricula, students who attended the last two grades of the reformed primary school (i.e., new grades 7–8) had to master in two years the curricula that were earlier realized in three years of middle schools. We examined changes in cognitive abilities in two cohorts: pre-reform middle schoolers (2nd grade, average age 14) and post-reform primary schoolers (7th grade, average age 13), measured at the beginning of 2nd/7th grade and the end of the school year. At the beginning of the school year, there was a slight yet significant difference in cognitive ability between cohorts ($d = 0.22$, or an equivalent of 3 IQ points) – the combined effect of age and prior school influences. We also observed a statistically significant increase in intelligence in both groups (average change in latent means $d = 0.14$/2 IQ points), with a slightly stronger effect among more intensively educated primary schoolers ($d = 0.20$/3 IQ points) than middle-school students ($d = 0.09$/1 IQ point). However, after separate analyses for verbal and nonverbal intelligence, together with additional robustness checks, we conclude that the effect of more intensive schooling on cognitive growth was not systematic and quite unstable. We discuss the consequences of these findings and future research directions.

1. Introduction

Education might be considered a particular form of the training of human cognitive abilities (Ritchie & Tucker-Drob, 2018; Snow, 1996). Yet, while cognitive ability serves as the primary (Laidra et al., 2007) and robust predictor of school successes (Deary et al., 2007), length and quality of schooling are well-established factors that build cognitive growth as well (Bergold & Steinmayr, 2019; Cahan & Cohen, 1989; Ceci, 1991). So indeed, the links between schooling and intelligence are bidirectional. What seems more controversial is what exactly is influenced by education, with empirical reasons to assume that specific abilities rather than *g* benefit from longer and better-quality schooling (Baker et al., 2015; Ritchie et al., 2015).

A similarly overlooked question is the one about the relationship between education intensity and changes in intelligence (Bergold et al., 2017). Intensity denotes the necessity to master particular curricula in a shorter time; hence, the same amount of content is being delivered in a shorter time. While this strategy seems efficient in gifted students who are accelerated (Steenbergen-Hu & Moon, 2011), it is largely unknown what the effect is for a more general sample of students who vary in their intellectual ability. On the one hand, there are reasons to expect that much more intensive schooling might result in the more dynamic growth of specific skills rather than *g* (Ritchie et al., 2015). On the other hand, even if such a change occurs, which is not apparent (Bergold et al., 2017), there is an open question of potential costs it is associated with. Such costs might, for example, include a decrease in students' motivation and academic self-concept (Jacobs et al., 2002; Scherrer & Preckel, 2019).

Quite ironically, most available data on the links between schooling and intelligence are related to lessening education intensity, i.e., due to different reforms in many countries, compulsory education is being extended rather than shortened (see Brinch & Galloway, 2012). Here, we explore the opposite example. We use the case of a Polish educational reform introduced in 2017. This reform disbanded three-year middle school and extended 6-year primary school to eight years. Consequently, compulsory education was shortened from nine (six of primary and

* Corresponding author at: Institute of Psychology, Dawida 1, St., 50-527 Wrocław, Poland.
E-mail address: maciej.karwowski@uwr.edu.pl (M. Karwowski).

https://doi.org/10.1016/j.paid.2021.111121
Received 9 March 2021; Received in revised form 1 July 2021; Accepted 5 July 2021
Available online 15 July 2021

three of middle school before the reform) to eight (primary school only) years. More interestingly, however, just after introducing the reform in schools (September 2017), there were two cohorts in the system that, on average, differed by one year and attended different school streams. Students of the new 7th class of primary school and students of the 2nd class of middle schools were taught similar curricula. However, students from primary schools were expected (and pushed) to master in two years (7–8 grade) the material that was earlier covered during the entire middle school, so in three years. That created the widely criticized overload (Karwowski & Milerski, 2021), yet it could also have resulted in specific, intensive educational training. Given that students from pre-reformed middle schools and post-reform, extended primary schools were often located in the same, so-called school complexes, so not only placed in the same cities, but also in the same buildings and taught by the same teachers (while, formally being in two different types of schools), this creates a unique quasi-experimental opportunity.

Here, we examine whether and—if yes—then how the situation created by the reform influenced the growth of cognitive abilities in adolescents. We analyze longitudinal changes in cognitive ability across these two cohorts at the beginning and end of their school year. We put special attention to changes in overall cognitive ability and more specific factors composed of verbal and nonverbal tasks.

2. The present study

The present study benefited from a unique natural experiment created by Poland's educational reform in 2017. We compared the patterns of cognitive ability changes in post-reform seventh-graders of primary schools and one-year older students from middle schools who started their 2nd grade in middle school at the same time (September 2017). We followed them for a school year to examine the changes in cognitive ability in both groups.

3. Method

3.1. Participants and procedure

Two cohorts of adolescents (N = 1961 overall) from two types of schools: 7th grades of primary schools (50%) and 2nd grades of middle schools (50%) participated in this study. On average, primary school students during T1 were 13.38 years old (SD = 0.44) and middle schoolers were a year older (M = 14.39, SD = 0.44). The proportion of male and female students was the same across cohorts (53% female students in both groups). Participants attended 90 schools: 48 primary and 42 middle schools, located in medium and large cities across Poland. More than half of all schools (n = 48) belonged to the so-called "school complexes," so primary and middle schools not only were located in the same localizations but also in the same building and taught by mostly the same teachers. There were 24 such complexes containing primary and middle school in the sample (N = 1037 students). In remaining schools (n = 42), primary and middle schools were located in the same localizations (towns or cities), yet not within the same school complexes.

The study was longitudinal, with the first wave was conducted between October and November 2017 (the school year in Poland starts in September and ends in the last week of June) and the second wave in May–June 2018. Participants, their parents, and school principals, provided informed written consent to participate.

3.2. Measure

In T1 and T2, participants solved an intelligence test that consisted of 26 tasks divided into blocks of analogies (3 tasks), reasoning (7 tasks), matrices (10 tasks), and mental rotations (6 tasks). The majority of tasks were adapted from the International Cognitive Ability Resource (ICAR) (Dworak et al., 2021; Revelle et al., 2020). The tests were solved during regular classes, with no time restrictions; typically, it took participants about 35–40 min to solve them. Examples of the tasks and their descriptive statistics are present in SOM Tables ST1-ST2. As explained below, we modeled intelligence as a latent variable, yet for the descriptive purposes, we notice that the scale was reliable – Cronbach's a estimated on a matrix of tetrachoric correlations (due to dichotomous items) was $\alpha = 0.88$ in T1 and $\alpha = 0.91$ in T2.

3.3. Data analysis strategy and missing values

We conducted measurement invariance tests (both between cohorts and longitudinal) to ensure that the latent means could be meaningfully compared. We modeled cognitive ability as a general factor loaded by four parcels of averaged tasks within reasoning, analogies, matrices, and rotations blocks. The level of missing values was moderate as it ranged between 11.7% for intelligence items in T1 and 16% in T2. The Missing-Completely-At-Random Test, MCAR (Little, 1988) was not significant, χ^2 (df = 46) = 56.13, p = .15, so all analyses were conducted with full information maximum likelihood estimator in lavaan (Rosseel, 2012). Then, we proceeded with a mixed analysis of variance to test the main effect of time (within-person factor), cohort (between-person factor), and their interaction. This analysis was supplemented by multilevel modeling conducted on a subsample of students who attended the same school complexes. Multilevel models controlled for clustering students within school complexes (see Table 3 for details). For comparative purposes, we have also proceeded with item-level CFA on two sub-factors, namely: verbal reasoning (10 items: analogies and reasoning) that resembled verbal intelligence, and matrices (10 items), strongly g-loaded (Carpenter et al., 1990) that resembled non-verbal intelligence (see SOM Tables ST3–ST6 for model fit and measurement invariance details). The dataset and R scripts are available on the Open Science Framework archive (OSF): https://osf.io/hm3df/

4. Results

Before any comparisons were made, we examined whether our two samples were comparable in terms of socioeconomic status and examined the measurement invariance of the central construct of interest. As illustrated in Table 1, Bayesian analyses attested that cohorts were fully equivalent (all Bayes Factors, $BF_{01} > 500$, indicating extremely strong support for null hypothesis) in terms of important socioeconomic characteristics of their families.

According to usually applied criteria, measurement invariance was established in both between-cohorts comparisons (multi-group CFA) and longitudinally (Chen, 2007) (see Table 2). The only exception was longitudinal metric invariance that resulted in a somehow mediocre fit. When we allowed two sets of loadings to vary freely (analogies and mental rotations), the partial metric model fit was very good.

In T1, the difference between cohorts in latent means of intelligence was small, yet significant $d = 0.22$ (95% CI: 0.08, 0.35, p = .002), an equivalent of 3 IQ points: a difference that could be attributed to the effect of age and schooling, as both cohorts not only, on average, differed by one year of age, but also differed by one year of education. Importantly, the difference was not significant in T2 ($d = 0.07$, 95% CI: −0.05, 0.18, p = .23), so the distance between cohorts diminished. Overall, when we compared latent means (T1 latent mean across groups was fixed to 0 for longitudinal comparisons), the estimated growth between T1 and T2 was $d = 0.14$ (95% CI: 0.08, 0.20, $p < .001$): an equivalent of 2 IQ point increase in seven months. The interaction *Cohort* × *Group*, however, did not reach significance: $F(1, 1959) = 3.14$, p = .076 (Fig. 1). While the growth indeed seemed to be slightly more pronounced (and statistically significant) among primary school students ($d = 0.20$, 95% CI: 0.11, 0.29, $p < .01$) than middle school students ($d = 0.09$, 95% CI: −0.001, 0.17, p = .053), the difference between slopes was not statistically significant and confidence intervals around point estimates clearly overlapped.

To examine the robustness of our findings, we also repeated this

Table 1
Samples comparison and Bayesian results of Bayesian analyses.

Family characteristics	Primary school	Middle school	Bayes factor 01
Mother's education			
Primary	4.9%	5.0%	592.3
Vocational	20.2%	26.6%	
Technical high school	21.7%	21.1%	
General high school	10.8%	11.1%	
Post-diploma	7.6%	8.6%	
B.A. or equivalent	7.2%	6.4%	
M.A. or equivalent	27.4%	21.3%	
Ph.D. or equivalent	0.3%	0.0%	
Father's education			
Primary	5.9%	6.6%	1731
Vocational	37.6%	39.8%	
Technical high school	22.2%	25.4%	
General high school	7.9%	7.5%	
Post-diploma	5.5%	5.2%	
B.A. or equivalent	6.4%	6.5%	
M.A. or equivalent	13.8%	8.2%	
Ph.D. or equivalent	0.7%	0.7%	
Mother's professional status			
Works full-time	72.3%	71.6%	21,985
Works part-time	2.6%	3.4%	
Unemployed, actively searches	1.2%	1.7%	
Unemployed, doesn't search	2.9%	3.4%	
Retired	2.2%	1.7%	
Works in the household	18.4%	18.2%	
Dead	0.4%	0.1%	
Father's professional status			
Works full-time	87.8%	86.1%	3398
Works part-time	4.5%	3.5%	
Unemployed, actively searches	0.3%	0.4%	
Unemployed, doesn't search	1.2%	1.9%	
Retired	3.7%	5.3%	
Works in the household	0.7%	0.7%	
Dead	1.8%	2.2%	
The family self-assessed material situation			10,334
Some savings for the future	26.3%	29.6%	
No savings but enough for normal functioning	27.4%	25.4%	
When living sparingly enough money	33.2%	30.9%	
Live very sparingly	6.3%	6.4%	
Money enough only for the primary needs	6.8%	7.8%	

Note. Data on the family situation were obtained from participants' parents.

analysis only on students who were matched within school complexes (24 complexes, overall $N = 1037$, primary students $n = 508$, middle-school students $n = 529$). In this restricted and matched sample, not only we observed the main effect of Time, $F(1, 1035) = 32.75$, $p < .001$, $\eta_p^2 = 0.03$, but also a significant *Time* × *Cohort* interaction, $F(1, 1035) = 5.90$, $p = .015$, $\eta_p^2 = 0.01$. The main effect of cohort did not reach significance, $F(1, 1035) = 1.80$, $p = .18$. The difference between cohorts in T1 was significant ($d = 0.14$, $p = .015$, an equivalent of 2 IQ points), and disappeared in T2 ($d = -0.02$, $p = .79$). The overall increase was significant, $d = 0.19$, $p < .001$, equivalent of about 3 IQ points, being significantly stronger in primary school students, $d = 0.27$/4 IQ points, $p < .001$ than middle school students, $d = 0.11$/2 IQ points, $p = .02$. While this result does indeed suggest a more profound growth in cognitive ability among more intensively schooled students, it did not survive the control for nesting participants within the same school complexes (see Table 3). As illustrated in Table 3, the increase in primary school students ($d = 0.27$, 95% *CI*: 0.18, 0.36) was comparable with this observed among middle-school students ($d = 0.20$, 95% *CI*: 0.11, 0.29), with a non-significant interaction effect ($p = .28$).

For nonverbal tasks, we observed a small, yet significant difference between cohorts in T1 ($d = 0.13$/2 IQ points, $SE = 0.04$, $p = .001$) and a similarly small growth from T1 to T2 ($d = 0.11$/1.7 IQ point, $SE = 0.02$, $p < .001$). Importantly, the *Time* × *Cohort* interaction was significant as well: $F(1, 1959) = 10.21$, $p = .001$, $\omega^2 = 0.001$. Post hoc comparisons with Holm corrections demonstrated that the effect of growth was stronger among primary schoolers ($d = 0.16$/2.4 IQ points, $SE = 0.02$, $p < .001$) than middle schoolers ($d = 0.06$/0.9 IQ point, $SE = 0.02$, $p = .029$). The cohorts did not differ in T2 ($d = 0.03$, $SE = 0.04$, $p = .779$) (see Fig. 2, left panel).

Table 2
Measurement of invariance in the multi-group CFA (comparisons between cohorts) and in a longitudinal manner – separately for cohorts and overall.

Model	CFI/TLI	RMSEA (90% CI)/SRMR	DCFI	DRMSEA
MG CFA				
Time 1				
Overall model	0.985/ 0.954	0.051 (0.025, 0.082)/ 0.017	–	–
Configural	0.989/ 0.966	0.044 (0.013, 0.077)/ 0.016	–	–
Metric	0.982/ 0.970	0.042 (0.018, 0.067)/ 0.026	0.007	0.002
Scalar	0.978/ 0.974	0.039 (0.018, 0.060)/ 0.028	0.004	0.003
Time 2				
Overall model	0.970/ 0.911	0.088 (0.061, 0.119)/ 0.027	–	–
Configural	0.973/ 0.918	0.085 (0.057, 0.116)/ 0.027	–	–
Metric	0.973/ 0.953	0.064 (0.042, 0.088)/ 0.029	0.000	0.021
Scalar	0.969/ 0.962	0.058 (0.039, 0.078)/ 0.033	0.004	0.006
Longitudinal				
Overall				
Configural	0.983/ 0.969	0.033 (0.022, 0.044)/ 0.021	–	–
Metric	0.965/ 0.948	0.042 (0.033, 0.052)/ 0.044	0.018	−0.009
Partial metric[a]	0.980/ 0.968	0.033 (0.024, 0.044)/ 0.027	0.003	0.000
Scalar[b]	0.979/ 0.970	0.032 (0.023, 0.032)/ 0.028	0.001	0.001
Primary school				
Configural	0.981/ 0.965	0.033 (0.016, 0.049)/ 0.025	–	–
Metric	0.937/ 0.907	0.054 (0.041, 0.067)/ 0.061	0.044	−0.021
Partial metric[a]	0.965/ 0.942	0.042 (0.028, 0.057)/ 0.040	0.016	−0.009
Scalar[b]	0.963/ 0.949	0.040 (0.026, 0.053)/ 0.041	0.002	0.002
Middle school				
Configural	0.989/ 0.980	0.028 (0.008, 0.045)/ 0.022	–	–
Metric	0.982/ 0.974	0.032 (0.017, 0.047)/ 0.035	0.007	−0.004
Partial metric[a]	0.991/ 0.985	0.024 (0.000, 0.041)/ 0.023	−0.002	0.004
Scalar[b]	0.981/ 0.973	0.032 (0.017, 0.046)/ 0.028	0.010	0.000

[a] Comparisons to configural model.
[b] The constraints relaxed in the partial metric model were restrained.

Robustness check conducted on a smaller subsample of students matched within school complexes did confirm a main effect of Time, $F(1, 1018) = 27.66$, $p < .001$, $\eta_p^2 = 0.026$, but not *Time* × *Cohort* interaction, $F(1, 1018) = 1.87$, $p = .17$, nor the main effect of the cohort, $F(1, 1018) = 1.82$, $p = .18$. The overall increase between T1 and T2 was significant yet small, $d = 0.12$/1.8 IQ points, $p < .001$, being slightly, yet not-significantly stronger among primary school students, $d = 0.15$/2.3 IQ points, $p < .001$ than middle school students, $d = 0.09$/1.3 IQ points, $p = .005$. The same conclusion stems from our second robustness check – the multilevel model (see Table 3). *Time* × *Cohort* interaction was not significant ($p = .17$) and the increase among more intensively taught primary school students ($d = 0.15$, 95% CI: 0.08, 0.21) was comparable to this observed among middle-school students ($d = 0.09$, 95% *CI*: 0.03,

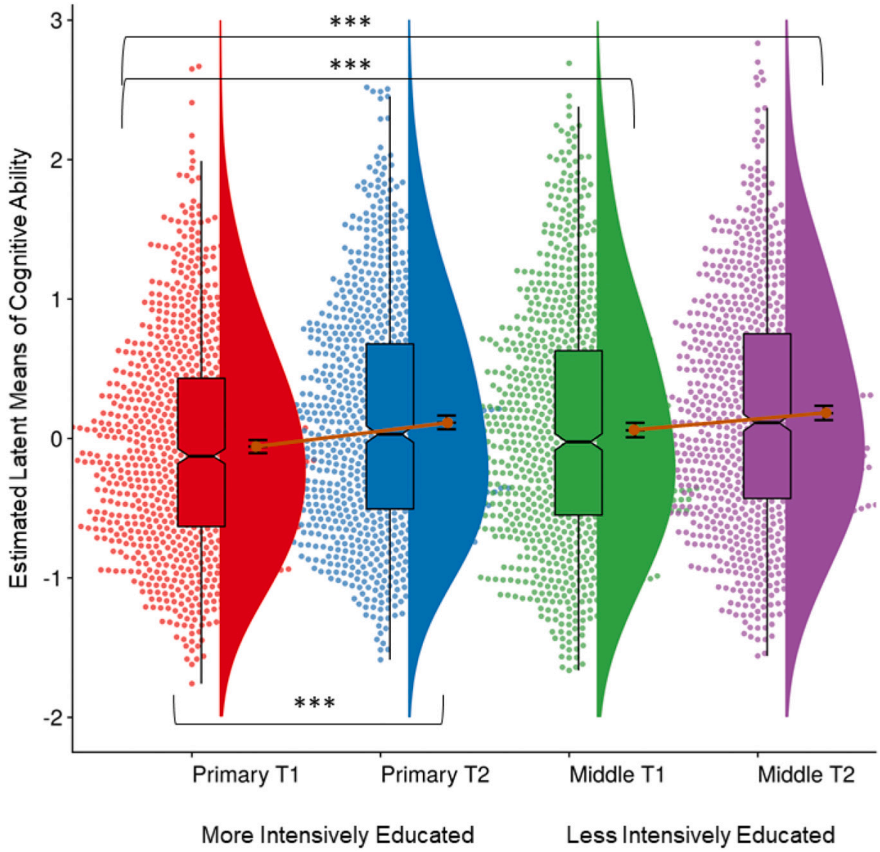

Fig. 1. Differences between cohorts in latent means (latent means for T1 were fixed to 0). Means and 95% confidence intervals are presented in the middle of violin plots.

Table 3
Multilevel analyses results – controlling for grouping schools and students into school complexes.

Predictor	Intelligence					Nonverbal intelligence					Verbal intelligence				
	Est.	SE	95% CI LB	95% CI UB	*p*	Est.	SE	95% CI LB	95% CI UB	*p*	Est.	SE	95% CI LB	95% CI UB	*p*
Fixed effects															
Intercept	0.24	0.07	0.10	0.39	.002	0.16	0.05	0.05	0.27	.006	0.43	0.07	0.28	0.57	<.001
Time (T2 − T1)	0.20	0.05	0.11	0.29	<.001	0.09	0.03	0.03	0.15	.005	0.44	0.03	0.38	0.50	<.001
Cohort	0.04	0.06	−0.07	0.15	.47	0.03	0.05	−0.07	0.13	.54	0.08	0.05	−0.02	0.17	.13
Time * cohort	−0.07	0.06	−0.20	0.06	.28	−0.06	0.04	−0.15	0.03	.17	0.03	0.04	−0.06	0.11	.55
Random effects															
Residual	0.55	0.02	0.50	0.60	<.001	0.25	0.01	0.23	0.27	<.001	0.23	0.01	0.21	0.25	<.001
Complex	0.08	0.03	0.04	0.16	.004	0.04	0.02	0.02	0.08	.008	0.10	0.03	0.05	0.18	.003
Participant	0.24	0.03	0.19	0.29	<.001	0.34	0.02	0.30	0.39	<.001	0.34	0.02	0.30	0.38	<.001
Changes (in Cohen's *d*s)															
T2 − T1 (overall)	0.23	0.03	0.17	0.30	<.001	0.12	0.02	0.07	0.16	<.001	0.43	0.02	0.38	0.47	<.001
T2 − T1 (primary)	0.27	0.05	0.18	0.36	<.001	0.15	0.03	0.08	0.21	<.001	0.41	0.03	0.35	0.47	<.001
T2 − T1 (middle)	0.20	0.05	0.11	0.29	<.001	0.09	0.03	0.03	0.15	.001	0.44	0.03	0.38	0.50	<.001

Note. Level 2 (participant) n = 1037, Level 3 (school complex) $n = 24$.

0.15).

For verbal tasks, there was a significant main effect of time, $F(1,1954) = 630.35$, $p < .001$ with robust increase between waves, $d = 0.397$/6 IQ points, $SE = 0.016$, $p < .001$. The main effect of cohort was statistically significant – middle-schoolers slightly ($d = 0.07$/1 IQ point), yet significantly ($p = .03$) outperformed primary school students when the results were pooled across T1 and T2. This overall difference was not observed when separate comparisons (with Holm correction) were conducted in the T1 ($d = 0.07$, $p = .08$) and T2 ($d = 0.08$, $p = .08$). The *Time* × *Cohort* interaction was not significant, $F(1, 1954) = 0.05$, $p = .83$ and the increase observed between T1 and T2 was similar among primary school students ($d = 0.39$/6 IQ points, $p < .001$) and middle school students ($d = 0.40$/6 IQ points, $p < .001$).

In this case, the robustness check on students matched within school complexes also demonstrated the main effect of Time, $F(1, 1015) = 397.53$, $p < .001$, $\eta_p^2 = 0.28$, and the main effect of the Cohort, $F(1, 1015) = 6.68$, $p = .01$, $\eta_p^2 = 0.01$, but not *Time* × *Cohort* interaction, $F(1, 1015) = 0.37$, $p = .54$. The overall increase T1-T2 was robust, $d = 0.43$/6.5 IQ points, $p < .001$, being similar among primary school students, $d = 0.35$ / 5.3 IQ points, $p < .001$ and middle school students, $d = 0.48$/

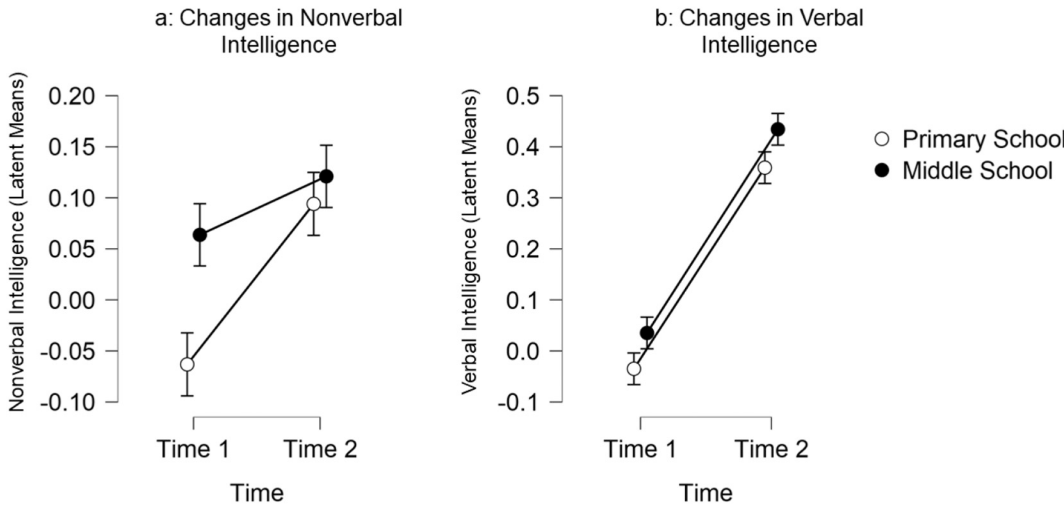

Fig. 2. Changes in nonverbal (left panel) and verbal (right panel) intelligence.

7.2 IQ points, $p < .001$. Middle school students slightly outperformed primary school students in both T1, $d = 0.11/1.7$ IQ points, $p = .028$ and T2, $d = 0.13/2$ IQ points, $p = .013$. Our second robustness check, so multilevel model (see Table 3) did not observe a significant interaction *Time* × *Cohort* ($p = .55$) and the increase in verbal intelligence among primary school students ($d = 0.41$, 95% *CI*: 0.38, 0.47) was almost the same as this observed among middle school students ($d = 0.44$, 95% *CI*: 0.38, 0.50).

5. Discussion

This study tested two cohorts of adolescents from different educational streams – pre-reform middle school and post-reform primary schools, to investigate whether more intensive education might be associated with more dynamic growth in cognitive abilities. We suspected that the necessity to master the more "squeezed" educational material might create a form of stimulation of cognitive abilities and result in more intensive changes over a school year (Bergold et al., 2017).

Our results were only partially consistent with these predictions. In the case of the overall latent factor of cognitive abilities, differences in slopes appeared inconclusive in the whole sample ($p = .076$), while being significant in the restricted, matched sample ($p = .015$), where we observed that primary schoolers' indeed tended to grow slightly faster than middle schoolers. However, a more robust comparison resulting from controlling the nested character of the sample (students nested within the same school complexes) does not support the conclusion that the growth was more intensive among primary school students. Given that these additional analyses were conducted on about half of the total sample, their power to identify small interaction effects is lower. Most importantly, however, given the inconsistencies observed, we see a little rationale for claiming that more intensive schooling resulted in the more dynamic growth of intelligence tests' scores.

In verbal tasks, more strongly related to typical school tasks, a similar and quite robust ($d \approx 0.40$, so an equivalent of six points on an IQ scale) ratio of growth was observed across both cohorts. When nonverbal tasks (matrices test) are considered, the *Time* × *Cohort* interaction turned out to be significant, with a steeper slope observed in primary school students, yet this finding did not survive robustness checks conducted on smaller and better-matched subsample and controlling for the clustering effects.

Consequently, these—somehow inconclusive—relationships we observed make strong claims about the potential effects of more-versus-less intensive education on cognitive growth premature. On the one hand, more intensive education was associated with the slightly more dynamic development of more *g*-loaded nonverbal tasks. Although we did not have specific predictions regarding which aspect of intelligence might benefit more from more intensive schooling, we consider this finding surprising as previous studies (Ritchie et al., 2015) have found the effect of education on specific rather than general cognitive skills. Indeed, the growth we observed in more "school-based" analogies and reasoning tasks was substantial ($d = 0.40$, so an equivalent of about six points on a typical IQ scale over seventh months), yet with the same pattern observed in both cohorts. The growth in matrices was much less spectacular overall ($d = 0.08$, so 1.2 IQ point), yet it was still more robust in the post-reform, "squeezed" cohort of primary school students.

5.1. Limitations and future directions

The findings of this study are not void of limitations. One of them is a relatively limited scope of the measurement of cognitive ability. The limited number of tasks did not allow for a more detailed measurement of all relevant aspects of cognitive functioning, for example, according to the Carroll-Horn-Cattell model (McGrew, 2009). Second, given that the same set of items was applied twice, there is a risk of the practice effect, although a 7-month period in-between makes it quite unlikely. Thirdly and finally, we acknowledge at least one possible alternative explanation of our findings: specificity of middle schools at the moment of testing. As these schools were "extinguished," we cannot rule out the possibility that the quality of education suffered there, and teachers were less motivated to teach since their schools were to be disbanded anyway (Karwowski & Milerski, 2021).

6. Conclusion

To conclude, this cross-sequential study does not provide conclusive support to the prediction that more intensive education might be beneficial for cognitive growth. While some detailed findings seem consistent with this expectation, more robust analyses raise doubts about whether this is the case.

CRediT authorship contribution statement

Maciej Karwowski: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Writing – original draft, Writing – review & editing. **Bogusław Milerski:** Methodology, Project administration, Writing – review & editing.

Acknowledgment

The authors report no conflict of interest. The study presented in this article was possible thanks to a grant from the National Science Centre, Poland (2016/23/B/HS6/03898).

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.paid.2021.111121.

References

Baker, D. P., Eslinger, P. J., Benavides, M., Peters, E., Dieckmann, N. F., & Leon, J. (2015). The cognitive impact of the education revolution: A possible cause of the Flynn Effect on population IQ. *Intelligence, 49*, 144–158. https://doi.org/10.1016/j.intell.2015.01.003.

Bergold, S., & Steinmayr, R. (2019). Non vitae sed scholae discimus? Schooling fosters intelligence. *Neuroforum, 25*(2), 121–128. https://doi.org/10.1515/nf-2018-0027.

Bergold, S., Wirthwein, L., Rost, D. H., & Steinmayr, R. (2017). What happens if the same curriculum is taught in five instead of six years? A quasi-experimental investigation of the effect of schooling on intelligence. *Cognitive Development, 44*, 98–109. https://doi.org/10.1016/j.cogdev.2017.08.012.

Brinch, C. N., & Galloway, T. A. (2012). Schooling in adolescence raises IQ scores. *Proceedings of the National Academy of Sciences, 109*(2), 425–430.

Cahan, S., & Cohen, N. (1989). Age versus schooling effects on intelligence development. *Child Development, 60*(5), 1239–1249. https://doi.org/10.2307/1130797.

Carpenter, P. A., Just, M. A., & Shell, P. (1990). What one intelligence test measures: A theoretical account of the processing in the Raven Progressive Matrices Test. *Psychological Review, 97*(3), 404–431. https://doi.org/10.1037/0033-295X.97.3.404.

Ceci, S. J. (1991). How much does schooling influence general intelligence and its cognitive components? A reassessment of the evidence. *Developmental Psychology, 27*(5), 703.

Chen, F. F. (2007). Sensitivity of goodness of fit indexes to lack of measurement invariance. *Structural Equation Modeling: A Multidisciplinary Journal, 14*(3), 464–504. https://doi.org/10.1080/10705510701301834.

Deary, I. J., Strand, S., Smith, P., & Fernandes, C. (2007). Intelligence and educational achievement. *Intelligence, 35*(1), 13–21.

Dworak, E. M., Revelle, W., Doebler, P., & Condon, D. M. (2021). Using the International Cognitive Ability Resource as an open source tool to explore individual differences in cognitive ability. *Personality and Individual Differences, 169*, Article 109906. https://doi.org/10.1016/j.paid.2020.109906.

Jacobs, J. E., Lanza, S., Osgood, D. W., Eccles, J. S., & Wigfield, A. (2002). Changes in children's self-competence and values: Gender and domain differences across grades one through twelve. *Child Development, 73*(2), 509–527. https://doi.org/10.1111/1467-8624.00421.

Karwowski, M., & Milerski, B. (2021). Who supports Polish educational reforms? Exploring actors' and observers' attitudes. *Education in Science, 11*(3), 120. https://doi.org/10.3390/educsci11030120.

Laidra, K., Pullmann, H., & Allik, J. (2007). Personality and intelligence as predictors of academic achievement: A cross-sectional study from elementary to secondary school. *Personality and Individual Differences, 42*(3), 441–451. https://doi.org/10.1016/j.paid.2006.08.001.

Little, R. J. A. (1988). A test of missing completely at random for multivariate data with missing values. *Journal of the American Statistical Association, 83*(404), 1198–1202. https://doi.org/10.1080/01621459.1988.10478722.

McGrew, K. S. (2009). CHC theory and the human cognitive abilities project: Standing on the shoulders of the giants of psychometric intelligence research. *Intelligence, 37*(1), 1–10. https://doi.org/10.1016/j.intell.2008.08.004.

Revelle, W., Dworak, E. M., & Condon, D. (2020). Cognitive ability in everyday life: The utility of open-source measures. *Current Directions in Psychological Science, 29*(4), 358–363. https://doi.org/10.1177/0963721420922178.

Ritchie, S. J., Bates, T. C., & Deary, I. J. (2015). Is education associated with improvements in general cognitive ability, or in specific skills? *Developmental Psychology, 51*(5), 573–582. https://doi.org/10.1037/a0038981.

Ritchie, S. J., & Tucker-Drob, E. M. (2018). How much does education improve intelligence? A meta-analysis. *Psychological Science, 29*(8), 1358–1369.

Rosseel, Y. (2012). Lavaan: An R package for structural equation modeling and more. Version 0.5–12 (BETA). *Journal of Statistical Software, 48*(2), 1–36.

Scherrer, V., & Preckel, F. (2019). Development of motivational variables and self-esteem during the school career: A meta-analysis of longitudinal studies. *Review of Educational Research, 89*(2), 211–258. https://doi.org/10.3102/0034654318819127.

Snow, R. E. (1996). Aptitude development and education. *Psychology, Public Policy, and Law, 2*(3–4), 536–560. https://doi.org/10.1037/1076-8971.2.3-4.536.

Steenbergen-Hu, S., & Moon, S. M. (2011). The effects of acceleration on high-ability learners: A meta-analysis. *The Gifted Child Quarterly, 55*(1), 39–53. https://doi.org/10.1177/0016986210383155.