Munich Personal RePEc Archive

Relative Age Effect on European Adolescents' Social Network

Fumarco, Luca and Baert, Stijn

STATEC Research, Ghent University

2018

Online at https://mpra.ub.uni-muenchen.de/89966/
MPRA Paper No. 89966, posted 25 Nov 2018 09:27 UTC

Relative Age Effect on European Adolescents' Social Network

Luca Fumarco,[1] Stijn Baert[2]

[1] (Corresponding author) STATEC Research – National Institute of Statistics and Economic Studies, Rue Erasme 13, L-1468, Luxembourg, Grand-Duchy of Luxembourg. Email address: luca.fumarco@statec.etat.lu.

[2] Ghent University, Sint-Pietersnieuwstraat 25, B-9000, Ghent, Belgium. Email address: Stijn.Baert@UGent.be.

Abstract. We contribute to the literature on relative age effects on pupils' (non-cognitive) skills formation by studying students' social network. We investigate data on European adolescents from the Health Behaviour in School Aged Children survey and use an instrumental variables approach to account for endogeneity of relative age while controlling for confounders, namely absolute age, season-of-birth, and family socio-economic status. We find robust evidence that suggests the existence of a substitution effect: the youngest students within a class e-communicate more frequently than relatively older classmates but have fewer friends and meet with them less frequently.

Keywords: Relative age, adolescents, education, Europe, social network

JEL-classification: I21

1 Introduction

The policy of grouping students born up to one year apart in the same class, based on an arbitrary cutoff date, is widespread in OECD countries but costly in terms of human capital formation. Indeed, scholars have shown that the youngest pupils in a class are more likely to suffer from grade retention, to be assigned to remedial classes, to receive lower grades, to be retained, and to skip lessons (Peña, 2016; Liu & Li, 2016; Navarro et al., 2015; Ponzo & Scoppa, 2014; Bernardi, 2014; Sprietsma, 2010; Elder & Lubotsky, 2009; Bedard & Dhuey, 2006). These performance gaps are also known as relative age effects (RAEs; Allen & Barnsley, 1993). Recent literature shows RAEs on non-cognitive abilities and well-being: relatively young pupils are, compared with their older classmates, (i) less likely to be leaders, (ii) more likely to be diagnosed with a learning disability, attention-deficit, and/or hyperactivity disorder, (iii) more likely to be bullied, (iv) less likely to adapt to change, (v) more likely to suffer from low self-esteem, and (vi) more likely to encounter difficulties with peer acceptance (Balestra et al., 2017; Schwandt & Wuppermann, 2016; Patalay et al., 2015; Mühlenweg et al., 2011; Mühlenweg, 2010; Dhuey & Lipscomb, 2010; Elder & Lubotsky, 2009; Dhuey & Lipscomb, 2008; Lien et al., 2005; Thompson et al., 2004). This literature is expanding but thus far has neglected to investigate one important aspect of whether relative age directly affects adolescents' social network strength.[1] Our study aims to fill this gap.

From a theory point of view, there is support for RAEs on adolescents' social network. Many of the RAEs previously studied are expected to be reflected in a higher likelihood of suffering from a weak social network. In particular, first, a weak social network may be the consequence of difficulties in peer acceptance (Patalay et al., 2015; Lien et al., 2005). Second, a weak social network may be a direct manifestation of relational bullying; that is, students may intentionally exclude other students from social interactions (Wang et al., 2011).

1 In this study we use the broad definition of 'adolescent', which includes students between nine and nineteen years of age (source: https://www.psychologytoday.com/basics/adolescence ; March 27, 2018).

Moreover, this and additional forms of bullying, such as physical, verbal, and cyber, may lead to anxiety and thus to avoidance of social contacts (Lereya et al., 2015). Third, behavioural differences may be wrongly attributed to behavioural disabilities in lieu of maturity gaps (Balestra et al., 2017; Schwandt & Wuppermann, 2016); in turn, a disability diagnosis may lead to stigmatization and thus—again—to the avoidance of social contacts (Moses, 2010). Fourth, lower performance may lead to low self-esteem, and thus to limiting communication with peers (Liu & Li, 2016; Thompson et al., 2004). Fifth, these adverse situations may cause depression, which is another factor commonly associated with loneliness and thus fewer social contacts (Matthews et al., 2016). These phenomena are known to be more of a struggle for relatively young students (Liu & Li, 2016; Patalay et al., 2015; Schwandt & Wuppermann, 2016). Therefore, based on this background, it is natural to suspect that, ceteris paribus, relatively young students have a weaker social network.

RAEs on social networks may have a large societal impact and great relevance from policy makers' points of view for at least two reasons. First, social networks, and thereby the development of social skills, are massively associated with well-being and non-cognitive skills. This association is related to the fact that interactions with friends help with socialization as well as communication skills, enhance learning opportunities and foster the formation of social capital and civic engagement (Deming, 2015; Lenzi et al., 2015; Ellison et al., 2014; Peter et al., 2005; Kraut et al., 2002; McKenna & Bargh, 1999).[2] Second, evidence of RAEs on social networks could be dealt with more easily than RAEs on standard educational outcomes. The detection of the latter RAEs is hardly followed by tangible policy interventions because they would entail profound reforms of the educational system that would require significant resources (e.g. the formation of classes with students born at most

[2] In addition, scholars have provided indirect evidence of positive returns to one's social network on the labor market. In particular, a strong social network develops social integration and social skills, which are associated with lower coordination costs with co-workers (Deming, 2015; McCann et al., 2012; Cunha & Heckman, 2010) and better employment outcomes (Borghans et al., 2014; Lindqvist & Vestamn, 2011).

nine months apart to reduce biological differences). In contrast, the detection of RAEs on social networks could call for less-radical interventions, such as a greater involvement of children in after-school activities, characterized by different age grouping rules or when age grouping matters less.

To investigate RAEs on adolescents' social network, we use international survey data from the Health Behaviour in School Aged Children (HBSC) conducted in European and North American countries. While these data have thus far been neglected in the economic field, they are broadly used in the medical, sociological, and psychological literature.[3] Moreover, they comprise various information on three proxies for social network for hundreds of thousands adolescents: (i) frequency of e-communications with friends (i.e. talking over the phone, SMS, or the Internet, including online social networks), which is the focus of our analyses; (ii) quantity of friends; (iii) frequency of meetings with friends that take place after the end of the school day (henceforth, after school).

The use of these data allow us to circumvent two methodological concerns with respect to most studies on RAEs. First, studies on RAEs often investigate people from just one country.[4] This does not allow researchers to separate RAEs (i.e. effects of maturity differences between classmates) from season of birth confounders (i.e. unobservable characteristics of the season of birth that directly affect students' skills).[5] This issue is discussed in much greater detail in Sub-subsection 2.3.3. Second, the geographic limitation also leads to a lack of representativeness; in particular, results are generalizable only to countries with similar educational settings. Because the HBSC data are collected from dozens of countries with different educational settings, including cutoff dates, we can disentangle

[3] See the updated list of published articles that use HBSC data: www.hbsc.org/publications/journal (March 27, 2018).

[4] Some exceptions are Mühlenweg et al. (2011), Sprietsma (2010), Mühlenweg (2010), and Bedard and Dhuey (2006).

[5] Examples of such confounders are investigated in Fan et al. (2017), Quesada and Nolasco (2017), Rietveld and Webbink (2016), Ramírez and Cáceres-Delpiano (2014), Buckles and Hungerman (2013), Currie & Schwandt (2013), Lokshin and Radyakin (2012), Bound and Jaeger (2001), and Musch and Hay (1999).

season of birth confounders from RAEs and obtain greater representativeness.

Studies on RAEs must often deal with a third problem: the presence of heterogeneous ages within classes. This problem may be caused by redshirting (i.e. late school entry), early school entry or grade retention, and could cause the estimates of RAEs to be biased (Peña, 2016). For example, retained students enjoy maturity advantages compared with their classmates. They might be more than one year older than their classmates, which facilitates networking; however, they also face stigma due to the retention and to the loss of direct contact with their former classmates, posing obstacles to networking. There are two main ways to deal with this problem: use instrumental variable techniques or focus on the discontinuity around the cutoff date (Bahrs & Schumann, 2016; Matta et al., 2016; Ponzo & Scoppa, 2014; Mühlenweg, 2010; Bedard & Dhuey, 2006). Both methods face the criticism that the instrument, or 'running variable', does not fulfil the monotonicity assumption (i.e. the instrument has to monotonically affect the instrumented variable; Barua & Lang, 2016). Moreover, the latter method requires a large range of values of the running variable not present in our dataset and focuses only on the students around the cutoff date. Based on these criticisms, we opt for the instrumental variable technique and implement a transformation of the instrumental variable that partially mitigates the issue of the monotonicity assumption.

The remainder of the paper proceeds as follows. In Section 2 we discuss our data and the main descriptive statistics. In Section 3 we proceed with the main analysis, robustness checks, and investigations of additional outcomes and heterogeneous treatment effects. In Section 4 we conclude and provide the reader with directions for future research.

2 Data and Descriptive Statistics

2.1 Survey Background and Data Set

The HBSC survey has been administrated every four years since 1985/6 to adolescents between 10.5 and 16.5 years of age, in several European and North American countries. The data are obtained by means of standardized questionnaires administered by teachers to nationally representative samples of adolescents.

We investigate the three most recent publicly available waves of the HBSC survey, i.e. 2001/2, 2005/6, and 2009/10.[6,7] In total, these three waves contain information on more than 581,838 students from Europe and North America. Table A.1 in Appendix A provides a complete list of the countries in the survey and the quantity of observations per country. As the table suggests, for each wave we exclude observations from a few countries from our analyses for five reasons: (i) in some countries, the cutoff date varies between regions, which are anonymized in the data set; (ii) information on cutoff dates could not be retrieved; (iii) information on students' birthdate is not disclosed; (iv) the question on e-communication was omitted from the survey—questions on quantity of friends as well as meetings with friends are present in all of the considered countries within these three waves; (v) since the data include month of birth but not day of birth, countries in which the cutoff is not the first day of the month cannot be investigated because we cannot tell whether a student was born before or after that day. Therefore, we are left with a sample of 423,575 observations. More details on what countries and waves per country were eliminated are discussed in Appendix A.

We also exclude observations on students from classes that cannot be properly identified. Observations on students who are not assigned a class identifier must be dropped because this is a crucial piece of information for our study since we focus on maturity gaps between classmates. In some other cases, the class identifier seems to be assigned to different classes in the same school as, for instance, some classes are larger than 100 students with ages that vary between 10.5 and 16.5. Therefore, we exclude classes in the top 5% of the class size distribution (i.e. classes larger than 31 students). Finally, we exclude observations on students

[6] There are also five previous waves: 1983/84, 1985/86, 1989/90, 1993/94, and 1997/98. But the question on the frequency of e-communication with friends was asked only from wave 2001/2 forward. In addition, there is one more recent wave, 2013/14 currently available only to researchers within the HBSC research network.
[7] The description of the survey methodology is provided in Currie et al. (2009).

from classes that include only students born in one single academic quarter because we cannot identify younger and older students. This identification strategy is described in detail in Section 1 of the Online Appendix. Eventually, we end up with a sample composed of 389,313 observations.

2.2 Outcome Variables

The main outcome of interest is the average number of days per week in which the adolescent has e-communicated with friends (i.e. talking over the phone, SMS, or the Internet, including online social networks) in the six months before the survey. This is a suitable proxy for the strength of one's social network. Indeed, in general, several studies have shown that the frequency of e-communication provides an accurate representation of the extent of real-life interactions and of self-reported friendships, as measured by means of traditional socio-metric methods (Wuchty & Uzzi, 2011; Eagle et al., 2009). In other words, the intensity of e-communication has been shown to visualize an underlying social structure and, as a consequence, to capture the strength of social interactions in general (Yang et al., 2016; Wuchty & Uzzi, 2011). Finally, over the last two decades, e-communication has become increasingly important in directly maintaining one's social life (Valkenburg & Peter, 2011).

The e-communication variable is categorical. It ranges from 1 to 5: 1 equals rare or no e-communication; 2 equals one or two days a week; 3 equals three or four days a week; 4 equals five or six days a week; and 5 equals every day. Table 1 provides the number of observations per category of the e-communication variable pooled through the three waves. We observe a skewed distribution of frequencies: 38.0 percent of the adolescents' e-communicate every day and 12.8 percent of the adolescents e-communicate five to six days per week while the lowest three frequencies each contain approximately 16 percent to 16.5 percent of adolescents. We can observe information on e-communication for 382,173 students out of the 389,313 included in the sample because of 7,135 (i.e. 1.7%) missing values for this variable.

Table 1. E-communication by levels.

E-communication	N	%
Rarely or never	61,067	15.98
1 or 2 days a week	63,042	16.50
3 or 4 days a week	64,094	16.77
5 or 6 days a week	48,739	12.75
Every day	145,236	38.00
Total	382,178	100.00
Missing	7,135	

Table A.2 in Appendix A tabulates the frequencies of e-communication by wave. This table shows that the distribution becomes more skewed towards higher frequencies of e-communication from 2001/2 to 2009/10, as one may expect given the explosion of social media use. During the 2001/2 wave, 27.1 percent of the adolescents e-communicate each day; during the 2005/6 wave this figure becomes 40.2 percent; and during the 2009/10 wave it rises to 44.6 percent. This time evolution is controlled for in our regression analyses by means of wave fixed effects.

Although the focus of this paper is e-communication, we explore two additional outcomes in order to gain broader insights on the interpretation of RAEs on students' social network, namely number of friends and number of days per week in which students meets with friends after school. Both additional outcomes are categorical variables. The former variable can take on four values: 0 equals no friend; 1 and 2 equal the corresponding amount of friends; 3 stands for three or more friends. Differently, the second additional outcome can take on values from 0 to 6 for the corresponding amount of schooldays. Table 2 provides the number of observations per category pooled through the three waves.

Table 2. Quantity of friends and of meetings with them after school.

Quantity of	N	%	Quantity of	N	%

friends			meetings		
0	10,594	2.72	0	40,622	10.77
1	11,769	3.02	1	50,555	13.4
2	22,217	5.71	2	67,098	17.79
3 or more	344,733	88.55	3	73,360	19.45
			4	39,628	10.51
			5	92,172	24.44
			6	13,760	3.65
Total	389,313	100.00		377,195	100
Missing	0			12,118	

We observe a skewed distribution of quantity of friends: 88.55 percent of the adolescents' declare to have at least three friends, meaning only approximately10 percent of adolescents declare to have fewer or no friends. The quantity of meetings with friends after school appear to be bimodal: approximately 37 percent of adolescents meet friends 2-3 days a week after school, and 25 percent of students meet with friends 5 days a week. Note also that a very low percentage of adolescents declare to meet with friends 6 days a week, which appears to be due to country-specific school attendance rules—only Austria, France, Italy, and Ukraine have a large number of students with six meetings a week, suggesting that this is the number of schooldays per week.

To allow the comparability of the main results on e-communication with those on quantity of friends and meetings with them, we transform these variables into a z-score, as Mühlenweg et al. (2011) performed. Notice that due to this transformation, the estimates are expressed in terms of standard deviations.

2.3 Explanatory and Control Variables

2.3.1 Explanatory Variable of Interest: Relative Age

Our explanatory variable of interest to proxy, relative age, is constructed as in Fumarco and Baert (2018). This variable is the difference in age (in months) between the oldest regular student in a class and student *i*; by 'regular student' we mean that she has not been retained and has started school when she was supposed to, based on her age and on the country cutoff date. For regular students, this measure ranges between 0 and 12, meaning the student *i* is the oldest or youngest regular student in class, respectively. More detail on how we identify students who are regular and those who are not is provided in Section 1 of the Online Appendix.

Previous studies consider the role of relative age as a mechanism that leads to age at school entry (ASEs) (Ponzo & Scoppa, 2014; Mühlenweg et al., 2011; Mühlenweg & Puhani, 2010; Mühlenweg, 2010; Dhuey & Lipscomb, 2010; Elder & Lubotsky, 2009; Bedard & Dhuey, 2006). In these studies, expected age at school entry is used as an instrument for age at entry. Their goal is to investigate the effect of age at entry on a specific outcome, such as educational performance. By operationalizing relative age as we do, we explicitly focus on the age-grouping system instead of age at school entry. In so doing, we are closer to the original literature on RAEs (Allen & Barnsley, 1993). This operationalization helps to reduce the correlation between age and relative age as well.[8]

Table 3. Pairwise correlations and descriptive statistics.

	Pairwise correlations									
Variables	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
1 E-communication	1.000									
2 Quantity of friends	**0.134**	1.000								
3 Quantity of meetings	**0.219**	**0.138**	1.000							
4 Relative age	0.000	**0.014**	**-0.021**	1.000						
5 Season-of-birth	**-0.009**	**-0.003**	0.000	**0.202**	1.000					
6 Absolute age	**0.269**	**0.006**	**0.025**	**-0.185**	**-0.043**	1.000				
7 Gender	**0.143**	**0.010**	**-0.071**	**0.036**	**0.003**	-0.002	1.000			
8 Father at home	**-0.040**	**0.041**	**-0.036**	**0.028**	0.000	**-0.030**	**-0.017**	1.000		
9 Mother at home	-0.001	**0.078**	**-0.015**	**0.031**	-0.001	**-0.021**	**0.031**	**0.204**	1.000	
10 SES	**0.162**	**0.124**	**-0.022**	**0.037**	-0.002	**-0.019**	**-0.044**	**0.094**	**0.042**	1.000
Statistics										
Mean	3.403	2.801	2.934	3.772	3.358	13.556	0.507	0.790	0.947	1.167
Standard deviation	1.513	0.619	1.758	5.506	5.471	1.652	0.500	0.407	0.225	0.754
Minimum	1	0	0	-62	0	9.833	0	0	0	0
Maximum	5	3	6	69	11	17	1	1	1	2

[8] Moreover, although irrelevant to this study, this operationalization of relative age could be useful when using panel data sets. In fact, while the date of birth and school entry age are fixed characteristics in time—and thus they would be cancelled out by means of the so called 'within transformation'—the age difference between the oldest regular student in class and student *i* may change in time because the oldest regular student—and thus her age—may change for several reasons. For instance, students may change class when they pass from primary to middle school and then to high-school. As another example, consider the case of students in Italian technical high schools: in the third year, students are re-grouped in different classes based on the specialization they have chosen (e.g. electronic, mechanic, hydraulic). In these cases, with enough variation across time in the difference between the age of the oldest regular student in a class and student *i*, the within transformation would not remove relative age so that its effect could be estimated.

N	382,178	389,313	377,195	372,459	389,313	389,308	389,313	381,133	386.266	389,313
Missing	7,135	0	12,118	16,854	0	3,047	0	8,180	3,047	0

Note: 'SES' stands for socio-economic status. Correlations in bold are statistically significant at least at the 10% level.

Table 3 shows that the relative age variable is right skewed. In fact, its mean is 3.772, or approximately a difference of 3 months and 24 days, which suggests the possible presence of some non-regular students. In the case of non-regular students, this variable can be negative (e.g. for retained or redshirted students who are older than expected) or larger than 12 (e.g. for students who skipped a grade or entered school earlier). Moreover, the maximum and minimum values of this variable suggest that there might be measurement errors due to the wrong assignment of the class identifier, despite our precautions of dropping classes larger than 31 students. However, there are not so many non-regular students: Table O.1 in the Online Appendix shows that 10 percent of students in the sample are older than expected—we could call them 'Older students', while only 4 percent of students are younger than expected—we could call them 'Younger students'.

In addition, although statistically significant, the correlations between relative age and control variables, namely family socio-economic status (SES), parents' presence at home, and absolute age are lower than 0.3, which qualifies as a negligible correlation within the behavioral sciences (Hinkle et al., 2003).

The correlations matrix in Table 3 does not provide any particular insights on the relationship between relative age and social network. There is no correlation with e-communication, a positive and statistically significant correlation with number of friends while the correlation is negative and also statistically significant with number of meetings with friends in the afternoon. However, these correlations do not account for various confounders controlled for in the econometric analyses.

Besides measurement error, there are two additional reasons why there might be heterogeneous ages within classes and thus possible endogeneity bias in the RAEs estimates.

First, due to children's unobservable characteristics and relative age, parents might delay or expedite their children's school entry; for the same reasons, underperforming children might be retained. Second, parents' unobservable characteristics might drive them not to strictly follow the school entry rules and choose to delay or expedite school entry. To address this concern, we use a 2SLS as a robustness check, in which we instrument relative age with expected relative age—see Sub-subsection 2.3.4 for a discussion of this instrumental variable.

2.3.2 Demographic Control Variables

The analyses account for a set of socio-economic variables. Absolute age is included to disentangle it from the effect of relative age (e.g. younger students in a class might e-communicate less simply because their parents believe they are too young to use a computer or a smartphone, not because they are young compared with their classmates). Our analyses also includes absolute age squared to capture non-linear effects on social network. Gender is included because past studies find evidence that women tend to use social networks differently from men (Muscanell & Guadagno, 2012). Further, we control for family characteristics, such as whether the student lives with her mother, whether she lives with her father, and the family's socio-economic status. The SES variable is constructed based on the HBSC guidelines (Currie et al., 2008). The HBSC survey includes four questions: whether the respondent's family owns zero, one, or more than one car; whether the respondent sleeps in her own bedroom; whether the respondent has travelled for holidays in the last twelve months never, once, or more often; and whether the respondent owns zero, one, or more than one computer. For each student the numeric answers to these questions are summed and divided into three status levels of family SES following Currie et al. (2008): low, medium, and high. Family economic status and parents' presence in the household may further capture a specific season of birth confounders as well.

2.3.3 Calendar Month of Birth

Our empirical analyses account for students' month of birth based on the calendar year; that is, calendar month of birth. This variable corresponds to the position of the month within the calendar year and ranges between zero and eleven, in which zero is January and eleven is December. In the regression analyses, this variable is disaggregated into dummies to capture non-linear effects of unobservable characteristics of birth timing unrelated to maturity differences and usually referred to as season-of-birth effects. More concretely, season of birth confounders are country-specific climatic, environmental, sociocultural, and biological characteristics that may cause performance gaps between students born in different calendar months, ceteris paribus. For example, in the United States and Spain, single mothers, teenage mothers, and mothers without a high-school degree tend disproportionally to give birth in winter months (Ramírez & Cáceres-Delpiano, 2014; Buckles & Hungerman, 2013). If a study on RAEs of those two countries failed to account for these confounders, the estimates of RAEs would be biased towards zero because the disadvantageous family background would counterbalance the positive effect of greater relative age than the classmates.

2.3.4 Instrumental Variable: Expected Relative Age

Robustness checks uses two-stage least square (2SLS) regressions. In these regressions, we use the month of birth within the academic year, which is established by the country-specific cutoff date as an instrument for relative age. Table B.1 in Appendix B shows the country-specific cutoff dates. This variable proxies the age difference in months between student *i* and the hypothetical oldest regular student in class (i.e. a regular student born in the first month of the academic year) if student *i* was a regular student. Therefore, this variable could be called *expected relative age*, ranging between 0—for students born in the month that starts with the cutoff date—and 11—for students born in the month immediately before the cutoff date. This same instrument is used in Fumarco and Baert (2018) while a similar instrument is used in

Datar (2006) in which the number of days between the child's birthday and the cutoff date is used as an instrument.

Notice that since our data set presents variation in cutoff dates, expected relative age does not overlap with the calendar month of birth. Consider Figure 1, which illustrates the case of two students born in March of year t (red boxes). One is born in Italy, in which the cutoff date is 1 January—the class incorporates students born from January to December of year t and corresponds to the calendar year. The other is born in Croatia, in which the admission date is 1 April—the class incorporates students born from April of year t to March of year t+1. Although born in the same month, both students have different expected relative age (i.e. the Italian student is expected to be among the oldest students, while the Croatian student is expected to be among the youngest students) because of different cutoff dates. The two students are in two different grades as well: the Italian student is in grade x while the Croatian student is in grade x-1. Because expected relative age does not overlap with the calendar month of birth, we can control for both variables.

Figure 1. Expected relative age and calendar month of birth; the example of Italian and Croatian students born in March.

Year	t												t+1		
MOB	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar
ERA, Italy	0	1	2	3	4	5	6	7	8	9	10	11	1	2	3
ERA, Croatia	9	10	11	0	1	2	3	4	5	6	7	8	9	10	11

Legend: Grade x; Grade x-1; Grade x+1; Italian student's MOB; Croatian student's MOB

Note: 'ERA' stands for expected relative age; 'MOB' stands for calendar month of birth.

A transformation of this instrument from a continuous variable to a set of indicator variables allows us to (partially) face an often-neglected criticism. As shown in previous studies (Sprietsma, 2010; Bedard & Dhuey, 2006), students born in the first few months and in the last few months of the academic year have the highest chances of being non-regular students, which could cause the infringement of the monotonicity assumption on which 2SLS regressions rely (Barua & Lang, 2016). To limit the relevance of this issue, we follow the suggestion in Angrist and Pischke (2008) on non-linear first stages and disaggregate expected relative age into a set of dummy variables—one per academic month of birth.[9] With this approach, only those dummies that equal one for students born in proximity of the cutoff date might be infringing the monotonicity assumption because these are the students who most likely are not in the right class. Moreover, this disaggregation allows us to conduct the test for the over-identifying restrictions, which could not be conducted in previous studies. Finally, utilization of dummy instrumental variables—in place of a unique discrete variable—increases the fit of the first-stage regression and thus the efficiency of the estimate of the instrumented variable.

2.4 Educational Settings and Cutoff Dates

The interpretation of the results from our later robustness checks and analyses at the country level uses information on country-specific educational settings. At least four characteristics are thought to affect the magnitude of the RAEs (Sprietsma, 2010; Bedard & Dhuey, 2006): (i) ability grouping; (ii) the absolute age of first formal tracking; (iii) the possibility of grade retention and of anticipating or postponing school entry; and (iv) the absolute age at school entry.

Why are these characteristics important? Ability grouping provides more chances to develop skills for those who are perceived as more skilled, which might be the case for children born in a month early in the academic year (Fredriksson & Öckert, 2014; Mühlenweg & Puhani, 2010). As Cunha and Heckman (2007) assert: ‘skills beget skills and abilities beget abilities’ (p. 35). While these students are put into high-ability groups and can improve their

[9] See Angrist and Pischke (2008), pp. 100-103: ‘…many credible instruments can be thought of as defining categories, such as quarter of birth’; and, ‘… any 2SLS estimator using a set of dummy instruments can be understood as a linear combination of all the Wald estimators generated by these instruments one at a time’.

leadership and communication skills, which might help them with social networking, those students who are perceived as less skilled are put into low-ability groups and could suffer from a loss of self-esteem (Hart & Moro, 2017) and thus from reduced networking opportunities. The possibility to be retained or to anticipate or to postpone school entry may change the extent of the maturity differences within a class and cause additional mental difficulties. Finally, absolute age at school entry and of formal tracking are complementary to the above characteristics and acquire greater importance at younger ages. The anticipated school entry determines initial, larger maturity differences more easily mistaken for skill differences, leading to different chances to improve skills or to different chances to be retained. Similarly, tracking into different educational paths is more likely to reflect differences in maturity when it occurs early (Hart & Moro, 2017; Mühlenweg & Puhani, 2010). These different educational paths are characterized by different chances to improve skills. Therefore, in addition to cutoff dates, Table B.1 in Appendix B reports the educational settings per country.

Regarding the retrieval of information on educational settings, the Eurydice website represents the main source of information for multiple countries[10] but additional sources are used. The complete list of sources is reported in Table O.2 in the Online Appendix.

3 Results

3.1 Main Results

The main analyses are conducted with an ordinary least square (OLS) regression model. We choose a linear model because of the greater flexibility compared with non-linear counterparts.

We regress the z-score of e-communication, *E-com*, on an increasing number of variables. First, we regress this outcome variable on relative age and we control for school

[10] See https://eacea.ec.europa.eu/national-policies/eurydice/home_en (July 20, 2018).

and wave fixed effects. In a second step, we insert control variables on demographic characteristics: absolute age and age square, a dummy on students' gender, two dummies for having father and mother at home and two dummies for SES. The references are, respectively: male student, no father at home, no mother at home, and low SES. In a third step, we include a set of dummies for calendar month of birth: the reference is January. In each analysis, we compute robust standard errors clustered on class.[11] The estimates for RAEs so obtained should be interpreted as aggregate effects of initial maturity differences that have evolved over time; that is, the effects of those characteristics that vary by relative age (e.g. acceptance by peers, relational bullying, and low self-esteem) and that influence *E-com*. The aim of this study is not to disentangle the different channels through which relative age affects social networks. See Table 4 for the results.

Table 4. Relative age on standardized e-communication.

Variables	E-com	E-com	E-com
	(1)	(2)	(3)
Relative age	-0.001**	0.007***	0.007***
	(0.000)	(0.000)	(0.000)
Absolute age		0.849***	0.848***
		(0.027)	(0.027)
Absolute age square		-0.025***	-0.025***
		(0.001)	(0.001)
Gender		0.297***	0.297***
		(0.003)	(0.003)
Father at home		-0.067***	-0.067***
		(0.004)	(0.004)
Mother at home		0.047***	0.047***
		(0.008)	(0.008)
Medium SES		0.230***	0.230***
		(0.005)	(0.005)
High SES		0.382***	0.382***
		(0.005)	(0.005)
Fixed effects			
School	X	X	X
Wave	X	X	X
Season-of-birth			X
N	365,603	357,128	357,128
Adj. R-squared	0.099	0.190	0.190

Note: 'E-com' stands for E-communication, which is

[11] Some readers may see the need to use survey weights as well, but it does not make a difference to our analysis. We conducted a robustness check in which we account for survey weights. This analysis returns equivalent results.

transformed into a z-score. 'SES' stands for socio-economic status. Standard errors clustered on class are in parenthesis. *** p < 0.01, ** p < 0.05, * p < 0.1.

Although column (1) suggests the presence of negative RAEs on e-communication**,** when we add control variables in columns (2) and (3), we observe evidence of the opposite: positive RAEs on e-communication. This result does not align with our initial expectations that relatively young students have weaker social networks. For the most extended model, we observe that a one-month increase in relative age increases e-communication by 0.007 standard deviations. This implies that a twelve-month increase in relative age (i.e. the theoretical maximum age gap between regular students) yields an increase in e-communication by 0.084 standard deviations.

The effect of absolute age is interesting as well. Ceteris paribus, a one-year increase in absolute age increases e-communication by 0.849 standard deviations. However, the relationship between absolute age and e-communication is concave since absolute age square has a negative effect. It is legitimate to expect that with the increase in absolute age comes an increased access to e-communication devices, but that beyond a certain absolute age the access to these devices does not increase sensibly.

Additionally, we find that female students tend to e-communicate 0.297 standard deviations more than male classmates. The presence of the father at home reduces e-communication by 0.067 standard deviations while the presence of mothers increases e-communication by 0.047 standard deviations. Finally, it appears that an increase in household socio-economic status increases e-communication. This result is not surprising since this set of variables incorporates the household ability to buy e-communication devices.[12]

[12] One might wonder whether it is necessary to include season of birth controls. Since the estimates in columns (2) and (3) are obtained from two nested models, we can test the difference between them with a likelihood ratio test. The result of this test provides evidence that these two models are statistically significantly different at the 10% level; however, results in columns (2) and (3) do not seem to differ. Alternatively, it is possible to conduct a Wald test on the difference between the sum of the RA coefficients (i.e. the RAEs) from Model (2) and the same

Overall, these results on RAEs unexpectedly move in the opposite direction. Yet, they might be misleading for two reasons. First, estimated RAEs might be biased owing to the presence of heterogeneous ages within groups, which is addressed in Subsection 3.2, in which we conduct a robustness check with a 2SLS regression framework. Second, a study on e-communication might not provide the full picture of RAEs of social networks because some mechanisms might be left unexplored. Quite possibly, relatively young students increase e-communication in the attempt to compensate for the lack of friends and face-to-face social interactions. In Subsection 3.3, we explore this issue by conducting analyses on two additional outcomes: quantity of friends and of meetings with them after school.

3.2 Robustness Check

The OLS results could be affected by sample selection bias caused by the presence of heterogeneous ages within-classes (see Section 1 and Sub-subsection 2.3.1). To address this concern, we re-conduct the benchmark analyses with a 2SLS regression model. As mentioned in Sub-subsection 2.3.4, we instrument the independent variable of interest with a set of dummies for academic month of birth; that is, expected relative age (ERA). And, the model specification is the same as in the main analyses.

Columns (1) to (3) of Table 5 report results from, respectively, the reduced form, the first and the second stage of the 2SLS analysis, whereas the results from the main analysis (i.e. Table 4, column (3)) are repeated in column (4) to facilitate the comparison. The estimates of the demographic control variables are omitted for brevity. The bottom of the table reports results from ancillary 2SLS tests.

Table 5. Relative age on standardized e-communication; instrumental variables approach.

Variables	E-com	Relative	E-com	E-com

sum from Model (3). This test leads to an equivalent result: the RAEs from Model (2) are statistically significantly different from those in Model (3) at the 10% level.

	age			
	Reduced form	First stage	Second stage	Results Table 4, Column (3)
	(1)	(2)	(3)	(4)
Relative age			0.004***	0.007***
			(0.001)	(0.000)
ERA 1	-0.004	0.087		
	(0.011)	(0.055)		
ERA 2	-0.004	0.738***		
	(0.010)	(0.051)		
ERA 3	-0.000	1.200***		
	(0.010)	(0.053)		
ERA 4	0.003	2.041***		
	(0.009)	(0.049)		
ERA 5	-0.001	2.598***		
	(0.010)	(0.054)		
ERA 6	0.011	3.049***		
	(0.009)	(0.053)		
ERA 7	0.014	3.602***		
	(0.010)	(0.059)		
ERA 8	0.017*	4.454***		
	(0.010)	(0.054)		
ERA 9	0.025**	4.663***		
	(0.010)	(0.063)		
ERA 10	0.005	4.488***		
	(0.010)	(0.064)		
ERA 11	-0.004	4.456***		
	(0.011)	(0.069)		
Demographic control variables	X	X	X	X
Fixed Effects				
School	X	X	X	X
Wave	X	X	X	X
Season-of-birth	X	X	X	X
N	357,128	357,128	357,128	357,128
Adj. R-squared	0.189	0.253	0.189	0.190
2SLS tests				
Endogeneity test, Hausman statistic (and p-value in brackets)		10.295 [0.001]		
Under-identification test, Lagrange-Multiplier statistic (and p-value in brackets)		5412.563 [0.000]		
Weak identification test, F statistic		1260.577		
Over-identification test of all instruments, Hansen J statistic (and p-value in brackets)		12.857 [0. 232]		

Note: 'E-com' stands for E-communication, which is transformed into a z-score; 'ERA' stands for expected relative age, with ERA 0 being the reference. Demographic control variables include: age and its square, dummy for gender, dummy for having mother and father at home, and dummies for medium and high-socio-economic status. The month of the academic year that starts with the cutoff date (i.e. Academic Month 0) is the reference. Standard errors clustered on class are in parenthesis. *** p < 0.01, ** p < 0.05, * p < 0.1.

Column (1) reports the results from the reduced form, which measures the impact of expected relative age on e-communication. These results suggest that academic month of birth does not have a clear impact on e-communication.

Column (2) reports the results from the first stage, in which the outcome variable is relative age and is regressed on demographic characteristics and dummies for expected relative age. These estimates are of straightforward interpretation: for students born towards the end of the academic year, the age difference with respect to the oldest regular students in class tends to be larger than for students born earlier (e.g. in the fourth month of the academic year). Returns to expected relative age appear to be non-linear for students born at the extremities of the academic year. These returns are statistically significant for all months, except for academic month of birth 1. The returns then increase gradually thereafter and seem to hit a plateau between academic month of birth 8 and 11. Therefore, the monotonicity assumption is more likely to be somewhat infringed by students born in the months close to the cutoff date.

Column (3) reports results from the second stage. We find confirmation of the direction of the estimated RAEs in our main analyses. The magnitude is reduced, however: A one-month increase in relative age increases e-communication by 0.004 standard deviations; or, equivalently, a twelve-month increase increases e-communication by 0.048 standard deviations (0.004×12). This result suggests that the initial estimates are somewhat downward biased due to selection of students.

Ancillary tests on the instrumental variables suggest that we are using proper instruments. The endogeneity test rejects the null hypothesis that relative age is exogenous. The tests for under-identification and for weak-identification reject the null hypotheses that the instruments are not correlated with the endogenous variable and that they are only weakly correlated (see critical values in Stock & Yogo, 2002), respectively. The over-identification test does not reject the null hypothesis that the instruments are uncorrelated with the second-stage error term.

3.3 Alternative Outcomes

In this section, we report and discuss the analyses for two alternative outcomes: quantity of friends and frequency of meetings with them after school. We report results for both outcomes and from both the 2SLS (i.e. reduced form, first and second stage) and the OLS regressions, in Table 6. Also in this case, the outcomes are standardized for comparability reasons.

Table 6. Relative age on standardized quantity of friends and of meeting with friends after school.

Variables	Friends				After school			
	Friends Reduced form (1)	Relative age First stage (2)	Friends Second stage (3)	Friends OLS (4)	After school Reduced form (5)	Relative age First stage (6)	After school Second stage (7)	After school OLS (8)
Relative age			-0.004***	0.001***			-0.003***	-0.003***
			(0.001)	(0.000)			(0.001)	(0.000)
ERA 1	-0.013	0.080			0.013	0.096*		
	(0.011)	(0.055)			(0.011)	(0.055)		
ERA 2	-0.008	0.723***			0.012	0.736***		
	(0.010)	(0.051)			(0.011)	(0.052)		
ERA 3	-0.012	1.186***			-0.013	1.205***		
	(0.010)	(0.053)			(0.011)	(0.053)		
ERA 4	-0.015	2.014***			0.006	2.040***		
	(0.010)	(0.050)			(0.010)	(0.050)		
ERA 5	-0.031***	2.578***			-0.011	2.596***		
	(0.010)	(0.054)			(0.011)	(0.055)		
ERA 6	-0.015	3.030***			-0.003	3.054***		
	(0.009)	(0.053)			(0.010)	(0.053)		
ERA 7	-0.017	3.576***			-0.000	3.615***		
	(0.011)	(0.058)			(0.011)	(0.059)		
ERA 8	-0.033***	4.443***			-0.003	4.415***		
	(0.010)	(0.054)			(0.010)	(0.055)		
ERA 9	-0.024**	4.656***			0.001	4.618***		
	(0.010)	(0.063)			(0.011)	(0.063)		
ERA 10	-0.014	4.487***			-0.020*	4.437***		
	(0.010)	(0.064)			(0.011)	(0.065)		
ERA 11	-0.030***	4.447***			-0.017	4.377***		
	(0.011)	(0.069)			(0.012)	(0.070)		
Demographic variables	X	X	X	X	X	X	X	X
Fixed Effects								
School	X	X	X	X	X	X	X	X
Wave	X	X	X	X	X	X	X	X
Season-of-birth	X	X	X	X	X	X	X	X
N	363,461	363,461	363,461	363,461	352,429	352,429	352,429	352,429
Adj. R-squared	0.0393	0.252	0.0393	0.0393	0.0436	0.253	0.0436	0.0439

2SLS tests		
Endogeneity test, Hausman statistic (and p-value in brackets)	22.939 [0.000]	0.064 [0.800]
Under-identification test, Lagrange-Multiplier statistic (and p-value in brackets)	5392.064 [0.000]	5201.931 [0.000]
Weak identification test, F statistic	1255.646	1203.530
Over-identification test of all instruments, Hansen J statistic (and p-value in brackets)	9.255 [0.5081]	17.153 [0.071]

Note: 'ERA' stands for expected relative age; 'Friends' stands for quantity of friends while 'After school' stands for frequency of meetings with friends after school, with both outcomes transformed into a z-score. Demographic control variables include: absolute age and its square, dummy for being female, dummy for having mother and father at home, and dummies for medium and high-socio-economic status. The month of the academic year that starts with the cutoff date (i.e. ERA 0) is the reference. Standard errors clustered on class are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

This table provides two interesting insights. First, the quantity of friends and meetings with them after school are negatively affected by relative age. An increase by one month in relative age reduces the quantity of friends by 0.004 standard deviations, which corresponds to 0.048 standard deviations for a one-year within-class age difference. The estimates are similar for quantity of meetings with friends after school: an increase by one month in relative age reduces the quantity of meetings by 0.003 standard deviations, which corresponds to 0.036 standard deviations for a one-year within-class age difference. Both results are highly statistically significant, and seem to persist in time, as they are obtained while controlling for absolute age. Second, when we do not account for endogeneity of relative age, the estimated RAEs on quantity of friends are positively biased, thus pointing to the opposite direction of what we expected.

Additionally, note that ancillary tests on the instrumental variables suggest two things. First, we are using proper instruments in the study on quantity of friends, whereas the analysis on quantity of meetings is not affected by endogeneity; plus, OLS and 2SLS provide the same results. Therefore, for this second alternative outcome, we rely on OLS results. There is at least one plausible reason why relative age is not endogenous when analyzing quantity of meetings after school: these meetings may happen within a context in which heterogeneous ages within group cannot happen (e.g. age grouping is strict in youth sports conducted after school).

How can we explain these results, in light of the estimated RAEs on e-communication? One plausible explanation relies on the existence of substitution effects: relatively young students have fewer friends and fewer face-to-face relationships, but they compensate for this lack of social interaction by increasing e-communication. As previous literature has found, relatively young students have a harder time being accepted by other students (Patalay et al., 2015; Lien et al., 2005) and are more often bullied (Mühlenweg, 2010), which explains why they might prefer e-communication to face-to-face contacts.[13] Furthermore, it is also possible that relatively young students have poorer face-to-face communication skills and thus spend more time e-communicating, vice-versa for relatively old students.

3.4 RAEs by Country

Policy makers could be more interested in how single countries fare than in average RAEs on social networks across European countries. Thus, in this subsection, we present country-level results. Moreover, these additional investigations could be interpreted as additional robustness checks and help us shed some light on the possible role of different educational settings in determining RAEs on social networks. The model specifications in these investigations differ from the previous ones with respect to one aspect: we do not control for season of birth because there is no variation in cutoff dates at the country level.

For brevity, Table 7 reports only the estimates from the second-stage of the 2SLS regressions of the RAEs on e-communication and on quantity of friends, as well as the OLS estimates of RAEs on quantity of meetings.[14] In addition, it reports country sample sizes. All estimates were obtained, including the entire battery of control variables.

Table 7. Country-specific RAEs on standardized e-communication, quantity of friends, and quantity of meetings with friends after school.

	E-com (2SLS)		Friends (2SLS)		After-school (OLS)	
	RAEs	N	RAEs	N	RAEs	N
Country	(1)	(2)	(3)			
Austria	0.013*	12,031	-0.001	12,298	-0.002	11,996
Belgium (Flemish)	0.009	8,282	0.004	8,727	-0.010***	8,224
Belgium (French)	0.002	11,034	-0.009	11,285	-0.006***	10,941
Bulgaria	0.002	4,719	-0.006	4,790	-0.002	4,723
Croatia	0.006*	14,458	0.005*	14,513	0.001	14,429

[13] We conduct analyses on measures of victimization and of acceptance by peers as well and find results consistence with the previous literature (Patalay et al., 2015; Mühlenweg, 2010; Lien et al., 2005). These analyses represent replications of previous ones, thus they are omitted from the text but can be provided upon request.
[14] Endogeneity tests for these analyses at the country level confirm the exogeneity of this variable for approximately 80 percent of the countries. It would be wrong to use the 2SLS with an exogenous independent variable of interest, as the first stage would not predict this exogenous variable.

Czech Republic	0.020***	8,837	0.005	8,919	0.000	8,865
Denmark	0.001	11,778	0.004	11,890	-0.002	11,728
England	-0.002	10,404	-0.005	10,760	-0.006*	7,365
Estonia	0.005	10,269	-0.007	10,312	-0.008***	10,244
Finland	0.003	16,538	-0.003	16,820	-0.008***	16,385
France	0.014***	18,202	0.007	18,534	-0.013***	18,006
Greece	-0.054*	8,453	-0.067*	8,512	0.033***	8,447
Greenland	-0.100	210	0.034	217	-0.026	211
Hungary	0.007	8,085	0.001	8,121	-0.002	7,821
Iceland	0.001	17,600	-0.005**	17,850	-0.002	17,546
Ireland	-0.000	10,553	0.006	11,058	-0.008***	10,508
Italy	0.007*	12,856	-0.012***	12,913	-0.009***	12,790
Latvia	0.004	11,424	-0.005	11,516	-0.007***	11,360
Lithuania	0.002	14,958	0.006	15,167	-0.003	14,988
Luxembourg	-0.004	5,668	0.005	5,850	-0.005**	5,577
Macedonia	0.019	11,114	-0.004	11,191	-0.003	11,033
Malta	0.029**	1,873	0.006	1,879	0.006	1,854
Netherlands	0.013*	9,835	-0.010	9,924	-0.004*	9,810
Norway	0.007*	4,763	0.003	4,782	-0.003	4,735
Poland	-0.005**	15,699	-0.006*	15,773	-0.006***	15,645
Scotland	-0.004	15,439	-0.009***	15,598	-0.007***	15,349
Slovakia	-0.005	4,026	-0.006	4,339	-0.001	4,048
Slovenia	0.007*	14,098	-0.004	14,175	-0.004	14,042
Spain	0.015**	9,300	-0.008	9,699	-0.002*	9,298
Sweden	0.002	13,287	-0.005	13,436	-0.011***	13,230
Switzerland	0.016**	14,726	-0.007	14,817	-0.002	14,678
Ukraine	0.003	14,346	-0.006	14,713	-0.006***	14,308
Wales	-0.001	12,263	-0.004	13,083	-0.004*	12,245
Pooled countries	0.004***	357,128	-0.003***	363,461	-0.003***	352,429

Note: 'E-com' stands for E-communication, 'Friends' stands for quantity of friends, 'After-school' stands for quantity of meeting with friends after school. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

The overall results on RAEs on social network seem to be confirmed for most countries. There are positive RAEs on e-communication for 24 out of 33 countries and negative RAEs on both quantity of friends and meetings with them after school for 21 and 29 countries, respectively.

These country-level results should be considered with a grain of salt for two reasons. First, sample sizes are strongly reduced; thus, it is not surprising to see that most estimates are not statistically significant. Second, unobservable characteristics related to season of birth might be biasing the results since they are not controlled for.

Against this background, we conduct a descriptive analysis at the macro-level. We compute pairwise correlations between the country-specific estimates of RAEs in Table 7 and educational settings. We find that in those countries in which ability grouping is possible, relative age tends to increase its positive impact on e-communication and its negative impact on the quantity of meetings after school, as suggested by the previous literature discussed in Subsection 2.4. However, relative age also tends to decrease its negative impact on quantity of friends, a result that might be due to the fact that similar students are more likely to become friends. These results are statistically significant at the 10% level but should be considered with some scepticism because of their descriptive nature.

4 Conclusions

A large cross-field literature shows that initial maturity gaps between students in the same class lead to gaps in cognitive skills, to the disadvantage of relatively young students. This quickly expanding literature indicates equivalent gaps in terms of students' non-cognitive skills and well-being. We contribute to this second strand of the literature as the first to investigate the effects of these maturity differences, the RAEs, on social network. While we focus on students' frequency of e-communication with friends, we explore quantity of friends and meetings with them after school as well.

We conduct this investigation using rich international survey data from the Health Behaviour in School Aged Children (HBSC) survey. These data are characterized by geographic variation that allows us to control for season of birth confounders and to obtain representative results, which is different from most of the previous studies conducted on individual countries.

Our approach to RAEs on social networks follows that of the original literature on the relative age (Allen & Barnsley, 1993), which focuses on the importance of age-grouping systems in determining effects on different outcomes. Therefore, we measure relative age as the age difference between student *i* and the oldest regular student in class (i.e. a student that was not retained and entered school when expected). We analyse the effect of relative age on social network with both an ordinary least square (OLS) and a two-stage least square (2SLS).

The latter approach allows us to account for endogeneity by instrumenting relative age with expected relative age—similarly to Datar (2006), which is proxied by academic month of birth. Moreover, by disaggregating this instrument into dummies, our analysis benefits from three advantages compared with alternative approaches: (i) we can conduct the over-identification test; (ii) we partially mitigate issues rising from not fulfilling the monotonicity assumption, which is often neglected; and (iii) we increase the fit of the first-stage regression and thus the efficiency of the estimate of the instrumented variable.

Contrary to what we initially expected, we find statistically significant evidence that relatively young students e-communicate more frequently than their relatively older peers. However, the analyses of two alternative outcomes, namely quantity of friends and meetings with them after school, lead to the expected results, that is, relatively young students have fewer friends and meet less frequently with them after school. These estimates are highly statistically significant and ancillary 2SLS tests confirm we solved the endogeneity problem with proper instruments in the analyses on e-communication and quantity of friends while the analyses on meeting with friends after school are not affected by endogeneity. The latter result should not be surprising since after-school meetings happen outside of the school system (e.g. youth sports activities with strict age grouping rules) and thus are less likely to be affected by the issue of heterogeneous ages within age-groups.

This combination of results draws an interesting picture. While relatively young students have fewer friends and fewer face-to-face relationships, they may compensate by increasing their e-communication. Whether this compensation is a good thing is still a matter of debate, as recent literature suggests that online social networks—through which a large part of e-communication occurs—decrease life-satisfaction and social trust (Sabatini & Sarracino, 2017). In addition, greater time spent e-communicating than in face-to-face communication could imply worse development of communication skills and of long-term social relationships, which in turn could negatively affect labour market outcomes.

We conduct further analyses to explore the possibility that RAEs vary by country since RAEs are characterized by different educational settings. Although country-level results tend to be non-statistically significant because of the strongly reduced sample size, the direction of the estimates is largely confirmed. Furthermore, country-level results partially confirm previous findings on the role of ability grouping in affecting the magnitude of RAEs (Fredriksson & Öckert, 2014; Mühlenweg & Puhani, 2010). We find suggestive evidence that for countries in which ability grouping is possible, relatively young students e-communicate more and meet with friends less frequently after school, but they have more friends, which suggests that similar students—in terms of relative age—are more likely to become friends.

Our study is characterized by four limitations. First, we use quantitative proxies for social network (i.e. frequency of e-communication and of meeting with friends after school, as well as quantity of friends) whereas proxies that reflect the actual quantity of time students e-communicate or spend face-to-face with friends could be more relevant. Such proxies are not present in the HBSC data, however. Second, following other recent studies on RAEs (Solli, 2017; Larsen & Solli, 2016), we focused on *aggregate* RAEs: We do not explore the role of single channels through which relative age affects social networks. Third, the disaggregation of the instrumental variable might help us to only partially mitigate the consequences of the infringement of the monotonicity assumption. Fourth, the correlation between relative and absolute age is limited, although not completely eliminated. These are two limitations in common with previous studies.

Scholars usually suggest that the reduction of RAEs on school performance passes through the revision of the age-grouping system (Pellizzari & Billari, 2012; Wattie et al., 2015), which could reduce the likelihood of systematic disadvantages for relatively young students and decrease possible long-run effects. However, these interventions are complicated; they demand a large amount of resources and there is still no evidence that they could work—although their reductive impact on RAEs seems intuitive. The reduction of RAEs on social network strength would instead require less dramatic interventions, for instance, parents could encourage their children to keep in touch with their peers in traditional manners, in particular through participation in after-school activities. Moreover, in countries in which the academic year does not correspond to the sports year, those students who are relatively young in school—and thus suffer from a disadvantage in terms of social network strength—could be encouraged to take part in sports activities in which they would enjoy a relative age advantage, which in turn could counteract adverse situations that lead to weak social networks. The existence of such a counteracting mechanism could be a topic for future studies.

Acknowledgements

HBSC is an international study carried out in collaboration with WHO/EURO. For the 2001/2, 2005/06, and 2009/10 survey waves, the International Coordinator was Candace Currie and the Data Bank Manager was Oddrun Samdal. A complete list of the HBSC study coordinators, databank managers, researchers, and participating countries is available on http://www.hbsc.org (March 27, 2018). We are grateful to the HBSC advisor for this study, Alessio Vieno; moreover, for their insightful feedback, we thank Philippe Van Kerm, Simon Amez, Ingeborg Foldøy Solli, Francesco Sarracino, and Simone Scarpa. Luca Fumarco gratefully acknowledges the support of the Observatoire de la Compétitivité, Ministère de l'Economie, DG Compétitivité, Luxembourg, and STATEC. Views and opinions expressed in this article are ours and do not reflect those of STATEC and funding partners.

References

Allen, J., Barnsley, R. (1993). Streams and tiers: the interaction of ability, maturity, and training in systems with age-dependent recursive selection. *Journal of Human Resources*, *28*(3), 649-659.

Angrist, J., Pischke, J. (2008). Mostly Harmless Econometrics. *Princeton, NJ: Princeton University*.

Attar, I., Cohen-Zada, D. (2017). The Effect of School Entrance Age on Educational Outcomes: Evidence using Multiple Cutoff Dates and Exact Date of Birth. *IZA Discussion Paper, 10568*.

Bahrs, M., Schumann, M. (2016). Unlucky to be young? The long-term effects of school starting age on smoking behaviour and health. HCHE Research Paper, 2016/13.

Balestra, S., Eugster, B., Liebert, H. (2017). The effect of school starting age on special needs incidence and child development into adolescence. *CESifo Working Paper, 6837*.

Barua, R., Lang, K. (2016). School entry, educational attainment, and quarter of birth: A cautionary tale of a local average treatment effect. *Journal of Human Capital, 10*, 347-376.

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

Bernardi, F. (2014). Compensatory advantage as a mechanism of educational inequality: a regression discontinuity based on month of birth. *Sociology of Education, 87*, 74-88.

Borghans, L., Ter Weel, B., Weinberg, B. A. (2014). People Skills and the Labor-Market Outcomes of Underrepresented Groups. *Industrial & Labour Relations Review, 67*, 287-334.

Bound, J., Jaeger, D. A. (2001). Do compulsory school attendance laws alone explain the association between quarter of birth and earnings? *Research in Labor Economics, 19*, 83-108.

Buckles, K. S., Hungerman, D. M. (2013) Season of birth and later outcomes: old questions, new answers. *Review of Economics and Statistics, 95*, 711-724.

Cunha, F., Heckman, J. J. (2007) The technology of skill formation. *American Economic Review, 97*(2), 31-47.

Cunha, F., Heckman, J. J. (2010). Investing in our young people. *NBER Working Paper, 16201*.

Currie, J., Schwandt, H. (2013). Within mother analysis of seasonal patterns in health at birth. *Proceedings of the National Academy of Sciences, 110*, 12265-12270.

Currie, C., Gabhainn, N. S., Godeau, E. (2009). The health behaviour in school-aged children: WHO Collaborative Cross-National (HBSC) study: origins, concept, history and development 1982-2008. *International Journal of Public Health, 54*, 131-139.

Currie, C., Nic Gabhainn, S., Godeau, E., Roberts, C., Smith, R., Currie, D., Picket, W., Richter, M., Morgan, A., Barnekow, V. B. (2008). Inequalities in young people's health – international report from the 2005/2006 Survey. *Copenhagen: WHO Regional Office for Europe.*

Datar, A. (2006). Does delaying kindergarten entrance give children a head start? *Economics of Education Review, 25*(1), 43–62.

Deming, D. J. (2015). The growing importance of social skills in the labor market. *NBER Working Paper, 21473*.

Dhuey, E., Lipscomb, S. (2008). What makes a leader? Relative age and high school leadership. *Economics of Education Review, 27*, 173-183.

Dhuey, E., Lipscomb, S. (2010). Disabled or young? Relative age and special education diagnoses in schools. *Economics of Education Review, 29*, 857-872.

Eagle, N., Pentland, A., Lazer, D. (2009). Inferring friendship network structure by using mobile phone data. *Proceedings of the National Academy of Sciences, 106*, 15274-15278.

Elder, T. E., Lubotsky, D. H. (2009). Kindergarten entrance age and children's achievement: Impacts of state policies, family background, and peers. *Journal of Human Resources, 44*, 641-683.

Ellison, N. B., Vitak, J., Gray, R., Lampe, C. (2014). Cultivating social resources on social network sites: Facebook relationship maintenance behaviors and their role in social capital processes. *Journal of Computer-Mediated Communication, 19*, 1-16.

Eurydice (2011). Grade Retention during Compulsory Education in Europe: Regulations and Statistics. *Brussels: Education, Audiovisual and Culture Executive Agency Publishing*.

Fan, E., Liu, J. T., Chen, Y. C. (2017). Is the quarter of birth endogenous? New evidence from Taiwan, the US, and Indonesia. *Oxford Bulletin of Economics and Statistics, 79*(6), 1-38.

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

Fumarco, L., Baert, S. (2018). Younger and dissatisfied? – Relative age and life-satisfaction in adolescence. Mimeo.

Hart, R. A., Moro, M. (2017). Date of birth and selective schooling. *IZA Discussion Paper, 10949*.

[dataset] Health Behaviour in School-Aged Children: World Health Organization Collaborative Cross-National Survey 2001/02. HBSC Data Management Center. Open Access ed.1.0. 2015. HBSC2001OAed1.0.

[dataset] Health Behaviour in School-Aged Children: World Health Organization Collaborative Cross-National Survey 2005/06. HBSC Data Management Center. Open Access ed.1.0. 2015. HBSC2006OAed1.0.

[dataset] Health Behaviour in School-Aged Children: World Health Organization Collaborative Cross-National Survey 2009/10. HBSC Data Management Center. Open

Access ed.1.0. 2015. HBSC2010OAed1.0.

Hoshen, M. B., Benis, A., Keyes, K. M., Zoëga, H. (2016). Stimulant use for ADHD and relative age in class among children in Israel. *Pharmacoepidemiology and Drug Safety, 25*, 652-660.

Hinkle, D. E., Wiersma, W., Jurs, S. G. (2003). Applied Statistics for the Behavioral Sciences. 5th ed. *Boston: Houghton Mifflin.*

Kraut, R., Kiesler, S., Boneva, B., Cummings, J., Helgeson, V. (2002). Internet paradox revisited. *Journal of Social Issues, 58*, 49-74.

Larsen, E. R., Solli, I. F. (2016). Born to run behind? Persisting birth month effects on earnings. *Labour Economics, 46*, 200-210.

Lenzi, M., Vieno, A., Altoè, G., Scacchi, L., Perkins, D. D., Zukauskiene, R., Santinello, M. (2015). Can Facebook informational use foster adolescent civic engagement? *American Journal of Community Psychology, 55*, 444-454.

Lereya, S. T., Copeland, W. E., Costello, E. J., Wolke, D. (2015). Adult mental health consequences of peer bullying and maltreatment in childhood: two cohorts in two countries. *Lancet Psychiatry, 2*, 524-531.

Lien, L., Tambs, K., Oppedal, B., Heyerdahl, S., Bjertness, E. (2005). Is relatively young age within a school year a risk factor for mental health problems and poor school performance? A population-based cross-sectional study of adolescents in Oslo, Norway. *BMC Public Health*, *5*(1), 1-8.

Lindqvist, E., Vestman, R. (2011). The labor market returns to cognitive and noncognitive ability: evidence from the Swedish enlistment. *American Economic Journal: Applied Economics, 3*, 101-128.

Liu, D., Li, X. (2016). Children born in July and August: a study on the age regulation in primary school and student's education access and development. *Journal of Chinese*

Sociology, 3, 1-19.

Lohmar, B., Eckhardt, T. (2013). The Education System in the Federal Republic of Germany 2012/2013. *Bonn: Secretariat of the Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany.*

Lokshin, M., Radyakin, S. (2012). Month of birth and children's health in India. *Journal of Human Resources, 47*, 174-203.

Matta, R., Ribas, R. P., Sampaio, B., Sampaio, G. R. (2016). The effect of age at school entry on college admission and earnings: a regression-discontinuity approach. *IZA Journal of Labor Economics, 5*, 1-25.

Matthews, T., Danese, A., Wertz, J., Odgers, C. L., Ambler, A., Moffit, T. E., Arseneault, L. (2016). Social isolation, loneliness and depression in young adulthood: a behavioural genetic analysis. *Social Psychiatry and Psychiatric Epidemiology, 51*, 339-348.

McCann, R. J., Shi, X., Siow, A., Wolthoff, R. (2014). Becker Meets Ricardo: Multisector Matching with Communication and Cognitive Skills. *IZA Discussion Paper, 6533*.

McKenna, K. Y. A., Bargh, J. A. (1999). Causes and consequences of social interaction on the internet. A conceptual framework. *Media Psychology, 1*, 249-269.

Moses, T. (2010). Being treated differently: stigma experiences with family, peers, and school staff among adolescents with mental health disorders. *Social Science & Medicine, 70*, 985-993.

Mühlenweg, A. M. (2010). Young and innocent: international evidence on age effects within grades on victimization in elementary school. *Economics Letters, 109*, 157-160.

Mühlenweg, A. M., Puhani, P. A. (2010). The evolution of school-entry age effect in a school tracking system. *Journal of Human Resources, 45*, 407-438.

Mühlenweg, A. M., Blomeyer, D., Laucht, M. (2011). Effects of Age at School Entry (ASE) on the Development of Non-Cognitive Skills: Evidence from Psychometric Data. *ZEW*

Discussion Paper, 11017.

Muscanell, N. L., Guadagno, R. E. (2012). Make new friends or keep the old: Gender and personality differences in social networking use. *Computers in Human Behavior, 28*, 107-112.

Musch, J., Hay, R. (1999). The relative age effect in soccer: cross-cultural evidence for a systematic distribution against children born late in the competition year. *Sociology of Sport Journal, 16*, 54-56.

Navarro, J.-J., García-Rubio, J., Olivares, P. R. (2015). The relative age effect and its influence on academic performance. *PLoS ONE, 10*.

Patalay, P., Belsky, J., Fonagy, P., Vostanis, P., Humprey, N., Deighton, J., Wolpert, M. (2015). The extent and specificity of relative age effects on mental health and functioning in early adolescence. *Journal of Adolescent Health, 57*, 475-481.

Pellizzari, M., Billari, F. C. (2012). The younger, the better? Age-related differences in academic performance at university. *Journal of Population Economics, 25*, 697-739.

Peña, A. P. (2016). Creating winners and losers: date of birth, relative age in school, and outcomes in childhood and adulthood. *Economics of Education Review, 56*(C), 152-176.

Peter, J., Valkenburg, P. M., Schouten, A. P. (2005). Developing a model of adolescent friendship formation on the internet. *CyberPsychology & Behavior, 8*, 423-430.

Ponzo, M., Scoppa, V. (2014). The long-lasting effects of school entry age: evidence from Italian students. *Journal of Policy Modeling, 36*, 578-599.

Quesada, J. A., Nolasco, A. (2017). Relationship between patients' month of birth and the prevalence of chronic diseases. *Medicina Clínica, 148*, 489-494.

Ramírez, A. A., Caceres-Delpiano, J. (2014). *Season of Birth and Mother and Child Characteristics: Evidence from Spain and Chile*. DE Working Paper, we1423.

Rietveld, C. A., Webbink, D. (2016). On the genetic bias of the quarter of birth instrument. *Economics and Human Biology, 21*, 137-146.

Sabatini, F., Sarracino, F. (2017). Online networks and subjective well-being. *Kyklos, 70*(3), 456-480.

Schwandt, H., Wuppermann, A. (2016). The youngest get the pill: ADHD misdiagnosis in Germany, its regional correlates and international comparison. *Labour Economics, 43*, 72-86.

Solli, I. F. (2017). Left behind by birth month. *Education Economics, 25*, 323-346.

Sprietsma, M. (2010). Effect of relative age in the first grade of primary school on long-term scholastic results: international comparative evidence using PISA 2003. *Education Economics, 18*, 1-32.

Stock, J. H., Yogo, M. (2002). Testing for weak instruments in linear IV regression. *Technical Working Paper, No* 284.

Thompson, A. H., Barnsley, R. H., Battle, J. (2004). The relative age effect and the development of self-esteem. *Educational Research, 46*, 313-320.

Valkenburg, P. M., Peter, J. (2011). Online communication among adolescents: an integrated model of its attraction, opportunities, and risks. *Journal of Adolescent Health, 48*, 121-127.

Wang, J., Iannotti, R. J., Nansel, T. R. (2011). Cyber bullying and traditional bullying: differential association with depression. *Journal of Adolescent Health, 48*, 415–417.

Wattie, N., Schorer, J., Baker, J. (2015). The relative age effect in sport: a developmental systems model. *Sports Medicine, 45*, 83-94.

Wuchty, S., Uzzi, B. (2011). Human Communication dynamics in digital footsteps: a study of the agreement between self-reported ties and email networks. *PLoS ONE, 6*.

Yang, S., Keller, F. B., Zheng, L. (2016). Social network analysis: methods and examples.

London: SAGE Publications.

Appendix A: Additional Basic Statistics

Table A.1. Number of observations by country and wave.

	Wave					
	2001/2		2005/6		2009/10	
Country	N	Data on cutoff	N	Data on cutoff	N	Data on cutoff
Armenia					2,833	
Austria	4,472	X	4,848	X	5,043	X
Belgium, Flanders	6,289	X	4,311	X	4,180	X
Belgium, Wallonia	4,323	X	4,476	X	4,012	X
Bulgaria			4,854	X		
Canada	4,361		5,930		15,919	
Croatia	4,397	X	4,968	X	6,262	X
Czech Republic	5,012	X	4,782		4,425	X
Denmark	4,672	X	5,741	X	4,330	X
England	6,081	X	4,783	X	3,524	X
Estonia	3,979	X	4,484	X	4,236	X
Finland	5,388	X	5,249	X	6,723	X
France	8,185	X	7,155	X	6,160	X
Germany	5,650		7,274		5,005	
Greece	3,807	X	3,715	X	4,944	X
Greenland	891	X	1,366	X	1,207	X
Hungary	4,164		3,532	X	4,864	X
Iceland			9,540	X	11,119	X
Ireland	2,875	X	4,894	X	4,965	X
Israel	5,661		5,686		4,135	
Italy	4,386	X	3,951	X	4,837	X
Latvia	3,481	X	4,245	X	4,284	X
Lithuania	5,645	X	5,632	X	5,338	X
Luxembourg			4,387	X	4,228	X
Macedonia	4,161	X	5,281	X	3,944	X
Malta	1,980	X	1,404	X		
Netherlands	4,268	X	4,278	X	4,591	X
Norway	5,023	X	4,711		4,342	
Poland	6,383	X	5,489	X	4,262	X
Portugal	2,940		3,919		4,036	
Romania			4,684		5,404	
Russia	8,037		8,231		5,174	
Scotland	4,404	X	6,190	X	6,771	X
Slovakia			3,882	X	5,344	X
Slovenia	3,956	X	5,130	X	5,436	X
Spain	5,827	X	8,891		5,040	X
Sweden	3,926	X	4,415	X	6,718	X
Switzerland	4,679	X	4,621	X	6,678	X
Turkey			5,639	X	5,664	X
Ukraine	4,090	X	5,069	X	5,890	X
Wales	3,887	X	4,409	X	5,454	X
United States	5,025		3,892		6,274	
N original by wave	162,305		205,938		213,595	
N with cutoff by wave	129,467		147,938		160,473	
N with class by wave	113,746		127,971		147,596	
Total N analysed	389,313					

Note: ‘N original’ is the quantity of observations in the original data set; ‘N with cutoff’ is the quantity of observations from countries for which we have information on the cutoff date per survey wave. ‘N with class’ is the quantity of observations per wave for which we can identify the class correctly. ‘N analysed’ is the total amount of observations that we analyse (it is the sum of ‘N with class’ over the three waves). Belgium and Denmark hold multiple surveys in each wave, for Flanders and Wallonia separately, and for mainland Denmark and Greenland separately.

'N original by wave' and 'N with cutoff by wave' differ because some countries and some waves per country could not be investigated. Data for Germany, Canada, and the United States are excluded because of multiple within-country cutoff dates and students' school and region, province as well as state are anonymized. See Bedard and Dhuey (2006) for an overview of state specific cutoff dates in the United States, Lohmar and Eckhardt (2013) for an overview of state-specific cutoff dates in Germany, and the material on the website of the Government of Prince Edward Island (www.gov.pe.ca/photos/original/ed_ageofentry.pdf; March 27, 2018) for an overview of Canadian provinces' cutoff dates. Data for Turkey, Russia, and Armenia are excluded because accurate cutoff dates could not be retrieved. While the recovery of such information may seem trivial, this task faces important barriers, namely language and organizational. This information is easily accessible when it is systematically discussed in English and well organized, such as on the Eurydice website managed by the European Commission or in scientific papers or national reports. On the contrary, when this information on educational settings is provided only on the websites of domestic institutes, such as a ministry of education portal, this task becomes nearly impossible for non-natives: either detailed information is available only in the local language or it is not discussed systematically. Sometimes this information can be retrieved by contacting the ministry of education directly (e.g. we contacted the Luxembourg Ministry of Education) but similar constraints may apply. Observations on students' birthdate are missing in the 2005/6 wave and in the 2001/2 wave for Czech Republic and Hungary, respectively. Questions on students' e-communication are missing in the 2005/6 wave for Spain and in the 2005/6 as well as 2009/10 waves for Norway. Because the data do not present information on students' day of birth, we cannot investigate countries with a cutoff in the middle of the month. For this reason, we exclude observations of students from Portugal and Romania, in which the cutoff is in mid-September. We additionally exclude students from Israel, where there is a moving cutoff date in December, which varies yearly and falls on the first day of the fourth month of the Jewish lunisolar calendar, called Tevet (Attar & Cohen-Zada, 2017; Hoshen et al., 2016). Finally, data for Slovakia from the 2005/6 wave and for Malta from the 2009/10 wave are not present in the data set, even though the survey was conducted there and then (Source: http://www.uib.no/en/hbscdata/94931/participating-countries-survey-years; March 27, 2018)

Table A.2. E-communication by levels and by survey waves.

	Wave					
	2001/2		2005/6		2009/10	
E-communication	N	%	N	%	N	%
Rarely or never	25,538	22.60	17,985	14.3	17,544	12.23
1 or 2 days a week	23,611	20.90	19,155	15.23	20,276	14.14
3 or 4 days a week	20,179	17.86	21,166	16.83	22,749	15.86
5 or 6 days a week	13,000	11.50	16,881	13.42	18,858	13.15
Every day	30,669	27.14	50,561	40.21	64,006	44.62
Total	112,997	100	125,748	100	143,433	100
Missing	749		2,223		4,163	

Appendix B: Educational Settings

Table B.1. Cutoff dates and educational settings by country.

Country	Educational setting						
	Cutoff date	Ability grouping	Age first tracking	Grade retention possible	Redshir-ting possible	Regular school entry age	Early entry possible
Austria	1 September	Y	10	Y	Y	6	Y
Belgium, Flanders	1 January	Y	14	Y	Y	6	Y
Belgium, Wallonia	1 January	Y	14	Y	Y	6	Y
Bulgaria	1 January	Y	14	Y	N	7	Y
Croatia	1 April	Y	15	Y	Y	6	Y
Czech Republic	1 September	Y	11	Y	Y	6	Y
Denmark	1 January	N	16	Y	Y	6	N
England	1 September	Y	16	N	N	5	N
Estonia	1 October	Y	16	Y	Y	7	N
Finland	1 January	N	16	Y	Y	7	N
France	1 January	Y	15	Y	N	6	N
Greece	1 January	N	14	Y	N	6	N
Greenland	1 January	missing	16	missing	missing	missing	missing
Hungary	1 July	Y	14	Y	Y	6	N
Iceland	1 January	N	16	Y	Y	6	N
Ireland	1 January	Y	15	Y	N	6	Y
Italy	1 January	Y	14	Y	N	6	Y
Latvia	1 January	Y	13	Y	Y	7	N
Lithuania	1 January	Y	11	Y	N	7	N
Luxembourg	1 September	Y	12	Y	Y	6	N
Macedonia	1 January	Y	14	missing	missing	6	N
Malta	1 January	Y	16	Y	N	5	N
Netherlands	1 October	Y	12	Y	N	6	N
Norway	1 January	N	16	N	N	6	N
Poland	1 September	N	15	Y	Y	7	Y
Scotland	1 March	Y	16	N	Y	5	N
Slovakia	1 September	Y	15	Y	Y	6	N
Slovenia	1 January	Y	15	Y	Y	6	N
Spain	1 January	N	15	Y	N	6	N
Sweden	1 January	Y	16	Y	Y	7	Y
Switzerland	1 July	Y	15	Y	Y	6	Y
Ukraine	1 January	missing	15	missing	Y	6	Y
Wales	1 September	Y	16	N	N	5	Y

Notice that, although redshirting is possible in most countries, it does not reflect an actual freedom of choice to postpone entry. The ultimate decision is based on either well-documented disability or on a concerted decision between school psychologists and teachers, for both kindergarten and school students (Eurydice, 2011).

Online Appendix

This appendix includes two sections. First, we illustrate the strategy we adopt to identify regular and non-regular students. Second, we list the references used to determine country-specific educational settings.

Identification of Regular, Younger, and Older Students

Two pieces of information are essential to identify which student is older or younger than the regular age range for a given class: (i) the identifier of the class to which a student belongs; and (ii) the cutoff date of the country in which the student is studying. Based on these two pieces of information, the identification proceeds through two steps.

First, for each class we find the reference year of birth: the mode year of birth of students born in the second academic quarter. Why the second? Students born in the first and fourth quarters are those who are more likely to end up in the 'wrong' classes because of redshirting, retention, early school entry, or skipped grade. Moreover, in European countries the combined number of students who are retained or redshirted—usually students born towards the end of the year—is much larger than the number of students who start earlier—usually born at the beginning of the academic year. Thus, we assume that it is more likely that the group of students born in the third quarter includes more students in the wrong class (i.e. in this case, older than expected) than the group of students born in the second quarter. Therefore, students born in the second quarter are more likely to be in the 'correct' class than students born in any other quarter, including the third quarter. If the mode year of birth is not unique, we choose the highest year of birth as a reference; again, the reason is that it is more likely that there are retained or redshirted students who are born in the year before the correct reference year.[15] In the case of countries with cutoff dates of 1 September and 1 October, we take as the reference year of birth the mode year of birth of students born from academic

[15] If there are two mode years of birth for the second academic quarter, we assume that it is more likely that the lowest mode year of birth corresponds to students who were born in that quarter and were either retained or redshirted.

month four to academic month six—refer to Figure O.1. In this way, we ensure that the period of three months that we are using to compute the mode year of birth falls within the same calendar year.

Second, for each student, we compare the mode year of birth found in the previous step with actual year of birth and combine this information with that on academic month of birth and on the cutoff date to identify which students are in the right age range; that is, *regular students* and which students are either *older* (i.e. they were either retained or redshirted) or *younger* (i.e. they entered school earlier) than expected. This second step is described below for each group of cutoff dates separately and illustrated in Figure O.1.

Cutoff: 1 January. A student is older if the real year of birth is lower than the mode year of birth (e.g. year t-1) and she is younger if the real year of birth is higher than the mode (e.g. year t+1).

Cutoffs: 1 March, 1 April, or 1 July. A student is older in two cases: first, if the actual year of birth is the same as the mode but the calendar month of birth comes before the academic month of birth that starts with the cutoff (e.g. February of year t, for countries with cutoff date 1 March); and second, if the actual year of birth is at least one year lower than the mode (e.g. year t-1). A student is younger in two cases: first, if the actual year of birth is one year higher than the mode but the calendar month of birth is the same as the academic month of birth that starts with the cutoff or later (e.g. May of year t+1, for countries with cutoff date 1 May); and second, if the actual year of birth is at least two years higher than the mode (e.g. year t+2).

Cutoffs: 1 September or 1 October. A student is older in two cases: first, if the actual year of birth is lower than the mode and the calendar month of birth comes before the academic month of birth that starts with the cutoff (e.g. July of year t, for countries with cutoff 1 September); second, if the actual year of birth is at least two years lower than the mode (e.g. year t-1). A student is younger in two cases: first, if the actual year of birth is the same as the mode but the calendar month of birth comes in the academic month of birth that starts with the cutoff or later (e.g. November of year t+1, for countries with cutoff 1 October); second, if the actual year of birth is at least one year higher than the mode (e.g. year t+2).

Table O.1 reports the main statistics on regular, younger, and older students. We cannot test whether these statistics are externally valid; however, we can compare them with those from other studies or reports. If we neglect the few students who entered school earlier in each country, for whom there is no available official statistics, and focus on students who were retained or redshirted (i.e. older students), we see that their proportions in the country-specific students' population reflect those from previous studies or reports (Bernardi, 2014; Eurydice, 2011; OECD, 2010).

There might be a drawback to this method. It is possible that in small classes there is no student born in the second academic quarter who is neglected. In our sample there are 2,316 such classes, accounting for a total of 16,849 students, who are thus neglected in our analyses.

Figure O.1. Identification of student type: regular, older, and younger.

		Student type																	
		Older			Regular												Younger		
	ERA	9	10	11	0	1	2	3	4	5	6	7	8	9	10	11	1	2	3
	Grade	x-1			x												x+1		
Cut-off																			
1 January																			
	Year	t-1			t												t+1		
	MOB	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar
1 March																			
	Year	t-1	t												t+1				
	MOB	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May
1 April																			
	Year	t												t+1					
	MOB	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun
1 July																			
	Year	t									t+1								
	MOB	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep
1 September																			
	Year	t							t+1										
	MOB	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov
1 October																			
	Year	t						t+1											
	MOB	Jun	Jul	Aug	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec

Legend :

Grade x

Academic quarter of birth used to find the reference year of birth within a class.

Note: 'ERA' stands for expected relative age; 'MOB' stands for calendar month of birth.

Table O.1. Student type by country.

Country	Student type						N
	Regular		Younger		Older		
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation	
Pooled countries	0.857	0.351	0.042	0.201	0.101	0.302	372,464
Austria	0.853	0.354	0.027	0.163	0.120	0.325	13,161
Belgium, Flanders	0.799	0.400	0.074	0.262	0.127	0.333	8,727
Belgium, Wallonia	0.718	0.450	0.075	0.263	0.207	0.405	11,311
Bulgaria	0.935	0.246	0.048	0.215	0.016	0.127	4,790
Croatia	0.918	0.274	0.023	0.149	0.059	0.235	14,987
Czech Republic	0.799	0.401	0.034	0.182	0.167	0.373	8,919
Denmark	0.895	0.306	0.022	0.148	0.082	0.275	13,675
England	0.984	0.124	0.005	0.070	0.011	0.102	10,765
Estonia	0.809	0.393	0.073	0.260	0.118	0.323	10,323
Finland	0.959	0.199	0.012	0.108	0.029	0.169	16,820
France	0.726	0.446	0.123	0.329	0.151	0.358	18,534
Greece	0.975	0.156	0.001	0.024	0.024	0.154	8,578
Greenland	0.372	0.484	0.281	0.451	0.346	0.477	231
Hungary	0.742	0.437	0.012	0.111	0.245	0.430	8,123
Iceland	0.988	0.108	0.006	0.077	0.006	0.075	17,955
Ireland	0.551	0.497	0.088	0.283	0.362	0.481	11,067
Italy	0.917	0.276	0.026	0.159	0.057	0.232	12,913
Latvia	0.867	0.340	0.023	0.151	0.110	0.313	11,612
Lithuania	0.825	0.380	0.092	0.289	0.083	0.276	16,461
Luxembourg	0.753	0.431	0.110	0.314	0.136	0.343	5,982
Macedonia	0.733	0.443	0.120	0.324	0.148	0.355	11,502
Malta	0.896	0.305	0.009	0.095	0.095	0.293	1,879
Netherlands	0.855	0.352	0.087	0.282	0.058	0.234	10,555
Norway	0.985	0.123	0.006	0.080	0.009	0.094	4,984
Poland	0.985	0.121	0.004	0.064	0.011	0.103	15,841
Scotland	0.942	0.235	0.005	0.072	0.053	0.225	16,930
Slovakia	0.837	0.369	0.052	0.221	0.111	0.314	4,550
Slovenia	0.916	0.277	0.048	0.214	0.036	0.186	14,207
Spain	0.730	0.444	0.081	0.273	0.189	0.391	9,846
Sweden	0.957	0.202	0.017	0.131	0.025	0.157	14,623
Switzerland	0.653	0.476	0.041	0.199	0.306	0.461	14,817
Ukraine	0.819	0.385	0.016	0.126	0.165	0.371	14,713
Wales	0.989	0.104	0.004	0.061	0.007	0.085	13,083

Additional Resources on Educational Settings

Table O.2. Sources concerning the educational settings by country.

Country	Source
Croatia	Sakic et al. (2013)
Estonia	Toomela et al. (2006)
Greenland	Statistics Greenland (2015)
Greenland	Rex et al. (2014)
Israel	Attar & Cohen-Zada (2017)
Israel	Hoshen (2016)
Luxembourg	Ministry of Education correspondence, private correspondence
Netherlands	Plug (2001)
Norway	Lien et al. (2005)
Norway	Solli (2017)
Scotland	Gamoran (2002)
Ukraine	Classbase: https://www.classbase.com/countries/Ukraine/Education-System (March 27, 2018)
Multiple countries	European Commission: https://webgate.ec.europa.eu/fpfis/mwikis/eurydice/index.php/Countries (March 27, 2018) European Commission: http://eacea.ec.europa.eu/education/eurydice/documents/thematic_reports/eu_press_release/126EN_HI.pdf (March 27, 2018) OECD: http://www.oecd.org/edu/bycountry (March 27, 2018) National Foundation for Educational Research: https://www.nfer.ac.uk/eurydice/compulsory-age-of-starting-school (March 27, 2018)

Note: 'Multiple countries' refers to the residual countries.

References Online Appendix

Attar, I., Cohen-Zada, D. (2017). The effect of school entrance age on educational outcomes: evidence using multiple cutoff dates and exact date of birth. I*ZA Discussion Paper, 10568.*

Bernardi, F. (2014). Compensatory advantage as a mechanism of educational inequality: a regression discontinuity based on month of birth. *Sociology of Education, 87*, 74-88.

Eurydice (2011). *Grade Retention during Compulsory Education in Europe: Regulations and Statistics*. Brussels: Education, Audiovisual and Culture Executive Agency Publishing.

Gamoran, A. (2002). Standards, Inequality & Ability Grouping in Schools. Mimeo.

Hoshen, M. B., Benis, A., Keyes, K. M., Zoëga, H. (2016). Stimulant use for ADHD and relative age in class among children in Israel. *Pharmacoepidemiology and Drug Safety, 25*, 652-660.

Lien, L., Tambs, K., Oppedal, B., Heyerdahl, S., Bjertness, E. (2005). Is relatively young age within a school year a risk factor for mental health problems and poor school performance? A population-based cross-sectional study of adolescents in Oslo, Norway. *BMC Public Health, 5*, 1-8.

OECD (2010). PISA 2009 Results: What Makes a School Successful?—Resources, Policies and Practices (Volume 4). Paris: OECD Publishing.

Plug, E. J. S. (2001). Season of Birth, Schooling and Earnings. *Journal of Economic Psychology, 22*, 641-660.

Rex, K. F., Larsen, N. H., Rex, H., Niclasen, B., Pedersen, M. L. (2014). A national study on weight classes among children in Greenland at school entry. *International Journal of Circumpolar Health, 73*, 1-6.

Sakic, M., Burusic, J., Babarovic, T. (2013). The relation between school entrance age and school achievement during primary schooling: evidence from Croatian primary schools. *British Journal of Education Psychology, 83*, 651-663.

Solli, I. F. (2017). Left behind by birth month. *Education Economics*, 25, 323-346.

Statistics Greenland (2015). Greenland in Figures – 2015. Nuuk: Statistics Greenland.

Toomela, A., Kikas, E., Mõttus, E. (2006). Ability grouping in schools: a study of academic achievements in five schools in Estonia. *Trames, 10*, 32-43.