



Influence of Within-Class Age Differences on Adolescents' Eating Behaviors

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Influence of Within-Class Age Differences on Adolescents’ Eating Behaviors

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Abstract

This study examines within-class age differences as a novel determinant of adolescents’ dietary behaviors, isolating it from confounders such as absolute age, season of birth, and country-specific school entry rules. Using a multi-country dataset of over 600,000 European students, we find that younger students within a class exhibit poorer dietary habits. Since confounders are controlled for, these effects are likely driven by peer influence. The findings are robust across various model specifications, with minimal variation across gender, socio-economic status, and family composition, highlighting the broad relevance of relative age effects on adolescent diet. However, the influence of relative age is significantly attenuated in countries with universal school meal programs.

Keywords: Diet, Adolescence, Relative age effects.

JEL Codes: I12, I18, I24.

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1 Introduction

The global rise in unbalanced diets is intensifying the phenomenon known as “globesity,” which is particularly concerning among adolescents.¹ Over the last 50 years, their obesity prevalence has increased exponentially worldwide, to the point where the number of overweight adolescents now exceeds that of underweight adolescents ([Abarca-Gómez et al., 2017](#)). The COVID-19 pandemic, with its accompanying increase in sedentary behaviors, has further exacerbated this trend ([Nour & Altıntaş, 2023](#)).

An unbalanced diet has far-reaching repercussions beyond body-weight issues. Adequate fruit and vegetable consumption is linked to lower rates of depression, enhanced well-being ([Akbaraly et al., 2009](#); [Jacka et al., 2011](#); [McMartin et al., 2013](#); [Cobb-Clark et al., 2014](#); [Mujcic & J. Oswald, 2016](#)), and reduced risks of diseases, particularly those affecting the digestive system, such as oropharyngeal cancer (mouth and throat), esophageal cancer (i.e., esophagus, a tubular organ in the digestive system that connects the throat to the stomach), and colorectal cancer (colon and rectum), as well as lung and stomach cancers ([Soerjomataram et al., 2010](#); [Wallace et al., 2020](#)). Furthermore, a nutritious diet positively impacts cognitive abilities, potentially leading to long-term benefits for lifetime income ([Au et al., 2016](#); [Frisvold, 2015](#); [Lundborg et al., 2022](#)). Identifying the factors shaping adolescents’ dietary habits is therefore a critical research priority.

Eating behaviors begin to form early in life, likely during the preschool years, when eating routines and food preferences start to evolve. Additionally, there are key developmental periods, often referred to as “windows of change” in the literature—such as middle childhood, adolescence, and early adulthood—when individuals may be particularly receptive to dietary change. During adolescence, for instance, key influences on dietary behaviors shift toward external factors, notably the school context, where peers’ behaviors and institutional offerings play a significant role ([Chong, 2022](#)). In fact, many influential determinants of adolescents’ dietary behaviors are found in the school environment. A growing body of literature highlights the pivotal role of school peers’ dietary and weight management behaviors ([Yakusheva et al., 2011](#); [Fortin & Yazbeck, 2015](#); [Gwozdz et al., 2019](#)), showing positive effects of peers’ healthy behaviors and detrimental effects of unhealthy ones. Another strand of literature demonstrates that providing nutritious meals in schools positively influences students’ dietary habits, yielding long-term benefits ([Au et al., 2016](#); [Frisvold, 2015](#); [Lundborg et al., 2022](#)).

¹Term used by the [WHO](#).

In this study, we investigate a novel school-based determinant of adolescents’ dietary behaviors: the within-class age difference, referred to as relative age. The relative age phenomenon is geographically widespread and of broad interest due to its roots in two universal features of modern educational systems. First, the cutoff date for school enrollment, which determines the oldest and youngest students in a cohort. Second, the 12-month cohort grouping, which allows for an age difference of up to twelve months among classmates – excluding cases of retention, grade skipping, redshirting (delaying school entry by one year), and greenshirting (entering school a year early), where applicable. As a result, a significant portion of the global population may be affected by negative relative age effects. For illustrative reasons, one could think of this example: if we divide a class in two groups around the median age, the treatment group (i.e., 50% of students who are relatively younger) experience worse outcomes than the control group (i.e., 50% of students who are relatively older) because of this age-grouping system, with potential repercussions throughout their education and beyond.² Thus, relative age is a topic of great general interest.

In this paper, we first examine the influence of relative age on objective and perceived overweight in adolescents. To gain a more detailed insight into adolescents’ eating habits and weight management behaviors, we then examine how relative age affects the probability of dieting, the frequency of consumption of soft drinks, sweets, vegetables, and fruits, and the likelihood of eating breakfast regularly. Using data from the “Health Behavior in School-Aged Children (HBSC)” survey, we analyze a representative sample of European students aged 10 to 17 from 32 countries with varying education systems and health-related dietary parameters. To identify the impact of a student’s relative age on eating behaviors, we estimate relative age effects using a 2SLS approach, which addresses the potential endogeneity of relative age by exploiting variation in school enrollment cutoff dates.

The results show that relatively young students are more likely objectively and subjectively overweight. Moreover, they are more likely on a diet. Furthermore, we find that relatively young students consume more candies and soft drinks, and less fruit and vegetables. Finally, they are more likely to skip breakfast on weekdays, but not on weekends. We ensure the robustness of our main findings through several

²This is a Solomonic fashion to split students based on their relative age, and it is meant for illustrative purposes. In reality, one should think of relative age as being a continuous treatment, which becomes stronger the wider becomes the age difference of student i with respect to the reference student (e.g., the oldest student in class or the hypothetical average age in class).

checks, including the use of an alternative measure of relative age, adjustments to model specifications (e.g., the usage of school fixed-effect), and a leave-one-out analysis to confirm that results remain consistent across different subsamples of countries.

While we cannot pinpoint the mechanisms that lead to these results, we should stress that they are net of absolute age effects,³ and other confounders, such as season-of-birth effects. Therefore, these findings likely reflect peer effects: two students of the same absolute age, born in the same season and starting school at the same time, exhibit different dietary behaviors depending on how their age compares to their classmates’.

We also examine heterogeneity in relative age effects across gender, socio-economic status, absolute age, regional dietary patterns, and school meal provision. While the direction of effects is generally consistent, some differences in magnitude emerge. In particular, we find that relative age effects are substantially weaker in countries with universal school meal programs, suggesting that institutional food provision may mitigate peer-related disparities in dietary behavior.

With the present study, we add to the growing body of evidence showing that relative age has far-reaching effects on individuals’ well-being. Evidence suggests that relatively young students tend to have lower life satisfaction and worse physical and mental health compared to their older peers, with these gaps persisting over time (Fumarco et al., 2020; Black et al., 2011; Dee & Sievertsen, 2018; Arnold & Depew, 2018; O’Neil et al., 2014). Younger students are also more likely to be (mis)diagnosed with attention deficit and hyperactivity disorder (Layton et al., 2018; Schwandt & Wuppermann, 2016; Furzer et al., 2022; Dee & Sievertsen, 2018; Elder & Lubotsky, 2009; Evans et al., 2010; Balestra et al., 2020). This pattern may reflect not only developmental differences but also the social dynamics of school environments, where younger children may be perceived as less attentive or more impulsive simply due to age-related immaturity. Such diagnostic labeling can carry psychological consequences—through stigma, lowered self-esteem, or altered peer and teacher expectations—which in turn may impact mental health and coping behaviors, including eating habits. These processes may form part of a broader, self-reinforcing cycle in which early age-related disadvantages contribute to

³While the relative-age effect is the effect of age differences between classmates, there are three main absolute age effects in the literature. Age-at-school start effect, that is, the effect of the age at which students start school. Age-at-outcome effect, that is, the effect of the age at which the outcome was measured (or the survey was conducted). Time-in-school effect, that is, the effect of the time spent in school. All these different but related age effects cannot be typically disentangled. More details are discussed in Section 2.5.

accumulating risks over time.⁴ Finally, previous studies find evidence that relatively young students are less likely to participate in sport activities (Smith et al., 2018; Dixon et al., 2020; Fumarco & Schultze, 2020) and to be more likely overweight (Anderson et al., 2011; Fumarco et al., 2020; Carpenter & Churchill, 2024).

Building on economic and clinical evidence, we contribute to the literature by examining whether relative age influences eating behaviors—an important but understudied aspect of well-being. Younger students may experience greater social pressures, heightened stress, and lower self-esteem due to their position in the age distribution, which could shape their dietary habits. Peer effects also play a crucial role in individual decision-making, particularly during adolescence when social influence is strong. Older peers, who often have greater autonomy over food choices, may establish behavioral norms that younger students adopt to gain social acceptance, sometimes amplifying or overindulging in these habits.⁵ Finally, differences in maturity levels may contribute to variations in self-regulation and coping mechanisms related to food consumption (Post & Kemper, 1993; Gay et al., 2018).

This paper proceeds as follows: Section 2 illustrates data and variables. Section 3 describes methods and results, while Section 4 concludes the paper.

2 Data and Variables

This analysis draws from survey data from the “Health Behaviour in School-Aged Children (HBSC),” all the five publicly available waves: 2001/2, 2005/6, 2009/10, 2013/14, and 2017/18. The following subsections discuss data and methods.

2.1 Data

The HBSC is a multi-country survey that focuses on adolescents’ health and well-being, and is administered in schools every four years. It targets students aged 10 to 17. From the five waves, we exclude observations from countries for which we could not retrieve information on the cutoff date, for which the cutoff does not fall on the first day of the

⁴There are additional effects beyond the scope of this study, such as on risky behaviors, unplanned births, sexually transmitted diseases (Johansen, 2021), juvenile crime (Landersø et al., 2017), and mothers’ labour market outcomes (Landersø et al., 2020), to name a few examples.

⁵A systematic literature review on peer effects in weight-related behaviors among young people is provided by Müller et al. (2024).

month,⁶ and countries that adopt multiple cutoffs—because we do not have information on the school’s region or state. The final sample consists of more than 600,000 students from 32 highly diverse countries.⁷ The primary sampling unit is the class. Table A.1 in the Appendix includes the number of observations per country per wave and reports the country-specific cutoff date.

2.2 Outcome variables

We investigate nine outcome variables. *(i)* Objective overweight, a dummy variable which equals one if the student is overweight (the underlying information was not collected in the 2006 wave). This variable is based on a standardized anthropometric measure (i.e., body mass index measured as kg/m^2) that accounts for students’ age and gender (Vidmar et al., 2013). *(ii)* Subjective overweight, a dummy variable which equals one if the student thinks to be at least a bit too fat.⁸ *(iii)* On a diet, a dummy variable which equals one if the student is on a diet or is doing something else.⁹ *(iv)* Vegetables, a dummy variable which equals one if the student consumes vegetables at least 5 days a week. *(v)* Fruits, a dummy variable which equals one if the student consumes fruits at least 5 days a week. *(vi)* Sweets, a dummy variable which equals one if the student consumes sweets at least 5 days a week. *(vii)* Soft drinks, a dummy variable which equals one if the student consumes soft drinks at least 5 days a week. *(viii)* Breakfast on schooldays, a dummy variable that equals one if the student gets breakfast on all five school days. *(ix)* Breakfast on weekends, a dummy variable which equals one if the student gets breakfast on both weekends.¹⁰

Table 1 reports the number of observations and descriptive statistics for these outcomes, relative age, the instruments, and control variables.

⁶HBSC data do not include information on the day of birth, so it is not possible to tell whether a student is born before or after the cutoff date, when this falls in the middle of the month. In countries where the cutoff is the first day of the month, we know that who is born whenever in that month is born after the cutoff date.

⁷More correctly, it is 30 countries. In two countries, the HBSC survey is conducted independently in different regions: in Belgium, there are two surveys for Flanders and Wallonia, while in Denmark there are two surveys for the mainland and for Greenland.

⁸This is the wording used in the survey answer.

⁹This is the wording used in the survey question. “Something else” refers to those situations where a person restricts its diet, without being on a diet under a dietitian supervision. For example, to limit the daily intake of sweets, soft drinks, or to do portion control and avoid second servings.

¹⁰Similar questions on dinner and lunch were asked only for the year 2001; thus, due to lower comparability and lack of statistical power, we have excluded analyses on these outcomes.

Table 1: Descriptive statistics.

Variable	Obs.	Mean	SD
Objective overweight	402,628	0.139	
Subjective overweight	616,973	0.319	
On a diet	489,984	0.441	
Vegetables	611,230	0.525	
Fruits	612,554	0.387	
Sweets	611,326	0.385	
Soft drinks	611,637	0.298	
Breakfast school	585,129	0.636	
Breakfast weekend	591,378	0.934	
RA	597,327	-0.306	0.454
AA	616,973	0	1.646
Female	616,973	0.508	
Lives with both parents	596,387	0.760	
SES: Low	616,973	0.367	
SES: Medium	616,973	0.229	
SES: High	616,973	0.403	
ERA	616,973	5.529	3.373

Note: RA is relative age, AA is absolute age and is centered around the mean. ERA is expected relative age. SES stands for socio-economic status. Analyses additionally include vectors for wave, country, and season of birth fixed-effects.

2.3 Relative age

Relative age, RA_{ic} , is measured as the difference between student i 's age in class c , AGE_{ic} , and the age of the oldest regular student j in class c , AGE_{jc} , as in Equation [1].

$$RA_{ic} = AGE_{ic} - \max(AGE_{jc} | j \in R_c) \quad (1)$$

For regular students, this measure ranges between zero (i.e., when i is the oldest regular student in class $i = j$) and -1 (i.e., when i is the youngest regular student in class, there is almost one year difference between her and the oldest regular student in the class,

student j).¹¹

Thus, an increase in relative age means that student i is relatively older. We refer to a regular student as one who has progressed through the school system without grade retention and who enrolled at the age prescribed by national regulations, based on the country’s official enrollment cutoff date. The core idea here is to exploit exogenous variation in age due to school entry cutoffs. This variation is “as good as random” among regular students who start school on time and progress through grades without delay or acceleration, in order to ensure that we are comparing the focal individual to peers who, like them, followed the standard schooling trajectory. This allows us to isolate the causal impact of being younger or older within the normal range, without contamination from idiosyncratic educational paths. However, due to non-random grade skipping, greenshirting, retention, and redshirting, relative age might still be endogenous and go beyond this range for non-regular students. For example, for a student who was born on the month with the cutoff and who was retained, $RA = 1$, meaning the student is 1 year older than the oldest regular student in class c . To allay any residual endogeneity concerns, we instrument relative age with expected relative age. By doing so, we rely on variation in age that is strictly related to the official school entry cutoff and birthdate, which is independent of the non-random factors affecting grade retention, skipping, or early school entry. This instrumental variable approach allows us to isolate the exogenous variation in relative age that is not contaminated by these endogeneity concerns, ensuring that the estimates of the effect of relative age on educational outcomes are not biased.

Note that this process requires classroom-specific identifiers, in order to identify students who are relatively older and younger than their classroom peers; thus we exclude observations without a classroom identifier in the HSBC data. Moreover, as discussed in [Fumarco & Baert \(2019\)](#), we exclude classes in the top 5% of the class size distribution.¹² Overall, the class size ranges from 8 to 32 students.

We follow previous literature ([Fumarco & Baert, 2019](#)), and combine information on country cutoff date with information on the country-specific modal year of birth for students born in the second academic quarter—because these students are less likely to

¹¹It is *almost* one year, because exactly one year would mean that student i was born on the same day, but in the next academic year.

¹²We trim the top 5% of the class size distribution in order to reduce any concerns about wrongful coding of the class identifier, e.g., different classes from different grades from the same school were incorrectly assigned the same class identifiers, and so the class size is inflated and the age range is too large.

be redshirted and greenshirted—to identify regular students.¹³

There is one additional remark. On one hand, relative age variation at class level improves the estimate of peer effects; on the other hand, it does not allow us to control for class fixed-effects. However, we conducted additional analyses with fixed-effects at school level, instead of country and wave,¹⁴ and the results are similar.

Table 1 shows the descriptive statistics. While the mean value of this variable should be about 0.5, it is 0.3, which reflects the fact that non-regular older students are more frequent than non-regular younger students.¹⁵

2.4 Expected relative age

The instrument for relative age is expected relative age ERA_{iCOU} , that is, the month of birth of student i within the academic year (henceforth, academic month of birth) of country COU . Academic month of birth is a proxy for the relative age that student i would have had, had she been a regular student. This variable ranges between 0 and 11.¹⁶ Zero is the reference month, corresponding to the hypothetically oldest student in a class, while eleven corresponds to the academic month of the hypothetically youngest student in a class.

We disaggregate this variable into individual month dummies—similar to what is suggested in Angrist & Pischke (2008), so that the instrument of observed relative age is actually a vector of dummies. A similar disaggregation is used in other studies as well, with the most prominent being Angrist & Krueger (1991).

In order to assess the exogeneity of this set of instruments, we proceed in four ways. First, this disaggregation of expected relative age allows us to conduct the overidentification test. This test checks whether the extra instruments—beyond the minimum needed for identification—are uncorrelated with the error term. For all the analyses, the

¹³There are potentially other ways to estimate who is a regular student, and one of these alternative methods would rely on age at school entry. However, the utilization of this piece of information would require the HBSC survey to, in turn, provide information on: (i) exact date of birth, and (ii) year of school entry, or (iii) directly information on age at school entry—which otherwise could be estimated based on (i) and (ii) in combination with institutional mandatory age at school entry. However, HBSC does not provide information on (i) and (ii); thus, we cannot follow this route.

¹⁴This is because school identifiers are wave-specific.

¹⁵If we look at extreme values, we observe that the 1st percentile of the relative age distribution is -1.3 (i.e., about 1 year and four months younger than the oldest regular student in class) and the 99th percentile is 1.25; this suggests that these very young or old students are rare outliers.

¹⁶Figure A.2, Example 1, illustrates how observed relative age varies from expected relative age, for the case of a retained student. Similar examples can be made for redshirting, greenshirting, grade skipping.

tests yield reassuring results, suggesting the instruments are valid. Results from these tests are included in the Appendix and discussed in greater detail, along with the main results, in Section 3.2.

Second, we test whether expected relative age is uncorrelated with observable demographic characteristics; with that goal in mind, we conduct joint orthogonality tests on the instruments. For these tests, we regress students' observable demographic characteristics (i.e., students' gender, whether they have both parents at home, and low, medium, or high family socio-economic status) on this set of instruments, and fixed-effects for country, wave and calendar month of birth. More details on these control variables are discussed in the Section 2.5. Then, we test two things: (a) whether individual coefficients for each instrument are equal to zero, and (b) whether the coefficients of these instruments considered together are equal to zero. The results are reported in Table A.2 in the Appendix. They are reassuring because of two reasons: (a) individual coefficients are consistently non significant, and (b) F-tests on the joint orthogonality of the coefficients do not reject the null hypothesis at the standard f-test p-value threshold of 0.05.

Third, to assess the possible impact of the correlation between the instruments and both family socio-economic status and family composition, we conduct a robustness check without these control variable, but the results are unchanged compared to the main ones, as discussed in Section 3.3.

Fourth, the picture provided in all the previous tests is reassuring; in particular, in Europe, on average, expected relative age is as good as random, and uncorrelated to both observable and unobservable characteristics that might bias the estimates. However, someone may argue that, at least in the vicinity of the cutoff date, there might be some manipulation of date of birth. Since we are not conducting RDD analyses, this possible threat would have a marginal impact; however, it is worth looking into it. Therefore, we center expected relative age, so that it goes from -6 to 5, that is, from six months before the month that starts with the cutoff date to five months after, and visually inspect the histogram for a possible discontinuity around 0, see Figure A.1 in the Appendix. Moreover, we conduct the test for manipulation around the cutoff.¹⁷ We do not find evidence of any discontinuity; however, we notice that month of birth distribution is not uniform, is this a problem? No, birth dates are not uniformly distributed across months in European countries, thus this figure reflects the general non-uniform

¹⁷We use the Stata command `rddensity` to conduct the robust bias-corrected test from Cattaneo et al. (2020).

distribution of births. It would be problematic if that distribution was caused by general birth date manipulation (i.e., not only around the cutoff date) and was correlated to, for example, family socio-economic status. However, the above results mostly ruled out this possibility.

Results obtained while using the discrete version of expected relative age return equivalent results, but they do not allow to conduct the overidentification test.

2.5 Control variables

The main control variable is absolute age.¹⁸ For sake of the analyses and coefficients interpretation, absolute age was centered around the sample mean. While the relative-age effect is the effect of age differences between classmates, absolute age controls for two related concepts that cannot be typically disentangled. First, Age-at-outcome effect, that is, the effect of the age at which the outcome was measured (or the survey was conducted), and, second, Time-in-school effect, that is, the effect of the time spent in school—which is typically a linear transformation of the former. In addition to absolute age, our empirical model includes several additional sociodemographic controls such as student gender, family structure (dummy variable equal to one if the student lives with both parents), and socio-economic status (SES). These factors are consistently associated with adolescents’ dietary patterns and weight-related behaviors (e.g. [Caine-Bish & Scheule, 2009](#); [Cooke & Wardle, 2005](#); [Elfhag & Rasmussen, 2008](#); [Rolls et al., 1991](#); [Shisslak et al., 1998](#)).¹⁹

Our analyses additionally control for unobservable “season-of-birth effects.” [Bound & Jaeger \(2000\)](#) explain that winter-born babies might be more likely to develop health

¹⁸Note that also this variable could be endogenous for similar reasons as relative age. Thus, we conduct additional analyses where we instrument absolute age with expected absolute age, EAA_i , that is, the absolute age that student i would have if she was a regular student. These analyses return identical results.

¹⁹[Landersø et al. \(2020\)](#) show that relative age affects marriage stability in Denmark, which suggests that controlling for a student’s family structure might induce a bad control problem. Similarly, SES may be endogenous: relative age might influence family’s SES ([Landersø et al., 2020](#)), and conversely, SES may affect relative age through (more or less consciously) parental timing of childbirth, as found in the U.S. ([Clarke et al., 2019](#); [Buckles & Hungerman, 2013](#); [Dhuey & Lipscomb, 2010](#)), Australia ([Gans & Leigh, 2009](#)), and China ([Huang et al., 2020](#)), although such patterns depend on local characteristics, such as social norms and tax incentives ([Dickert-Conlin & Elder, 2010](#)). In our case, three factors mitigate these concerns: (i) country fixed-effects account for institutional and cultural differences; (ii) our balance test in Table [A.2](#) shows no relationship between relative age and family status or SES in Europe; and (iii) robustness checks omitting SES and family structure controls yield results consistent with the main findings.

issues, such as mental disabilities and multiple sclerosis, while spring-born babies might be more likely to become shy. The variable for season of birth is proxied by the month of birth within the calendar year (henceforth, calendar month) and ranges between 0 (January, the reference month) and 11 (December). There are a number of additional studies on the effect of season of birth (e.g. [Currie & Schwandt \(2013\)](#); [Buckles & Hungerman \(2013\)](#)).

Finally, the analyses account for wave and country fixed-effects. Among other unobservable characteristics, country fixed-effects allow us to control for countrywide expected average age-at-school start effects. This is the country-wise effect on the outcome of the expected average age at which students start school; thus, there is no individual variation, with which we would have been able to capture individual expected age-at-school start effects.²⁰ Finally, country fixed-effects allow us to account for local circumstances, such as gender norms, availability of kindergartens, and features of the educational system, which might play a role in individual countries.

3 Methods and Results

3.1 Methods

In order to identify the relative age effects on food choices and dietary habits, we employ a two-stage least square approach.²¹ The second stage is illustrated by Equation [2]:

$$Y_i = \beta_0 + \beta_1 \widehat{RA}_i + \beta_2 AA_i + \mathbf{X}_i \zeta + \mathbf{FE}_i \delta + \mu_i \quad (2)$$

Index i represents the individual student. Y_i is one of the outcome variables. \widehat{RA}_i is predicted relative age and AA_i is absolute age. β_1 represents the coefficient of interest. \mathbf{X}_i is a vector of demographic control variables, that is, gender, family SES, and the

²⁰For example, in Italy, all students who turn 6 by January of year t , should start school in that year. Thus, the average expected age at school start in Italy is 6.5, with students born in January starting school at about 7 years of age and students born in December starting school at about 6 years of age.

²¹A similar approach has been employed by [Bedard & Dhuey \(2006\)](#) and [Page et al. \(2019\)](#) for studying the long-term effect of relative age at school. An alternative approach to identification would be to employ a fuzzy regression discontinuity design (fuzzy-RDD), as is common in much of the existing literature. We do not use this method for two key reasons. First, the HBSC dataset lacks information on age at school entry, on exact day of birth or on the precise date of survey participation, making it impossible to pursue this route. Second, an advantage of our dataset is that it allows for the computation of relative age at the class-school level, which enables the isolation of relative age from the three absolute age effects—a distinction that a fuzzy-RDD cannot achieve.

presence of both parents at home. \mathbf{FE}_i is a vector of fixed-effects, that is, wave, country, and calendar month of birth—which proxies season of birth.

The first stage for relative age is illustrated in Equation [3]:

$$Endogenous_i = \gamma_0 + \mathbf{ERA}_i\gamma + \zeta AA_i + \mathbf{X}_i\iota + \mathbf{FE}_i\phi + \nu_i \quad (3)$$

Where *Endogenous* is RA. \mathbf{ERA}_i is the vector of academic months of birth, that is, expected relative age. Thus, ERA is separated into dummies as suggested in Angrist and Pischke [Angrist & Pischke \(2008\)](#).

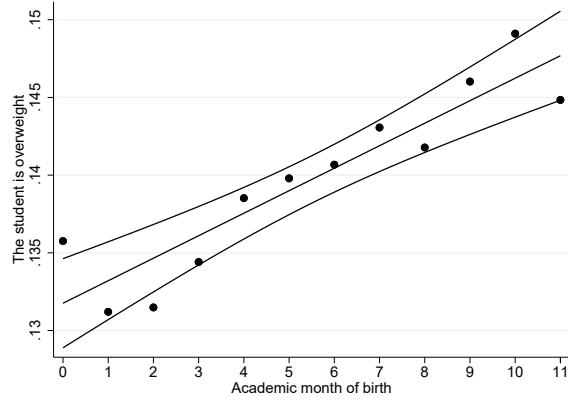
It is important to note that, due to the variation in cutoff dates, the correlation between the instruments (i.e., the dummies for expected relative age, represented by the academic months of birth) and the dummies for calendar month of birth is low. Table [A.3](#) in the Appendix shows that the variance inflation factors for both ERA and season-of-birth from the first stage are low: all of them are below 4, and the mean is 3.62; this is well below the 10-threshold, beyond which multicollinearity could be a problem. Figure [A.2](#) in the Appendix, Example 2, illustrates why academic month of birth—which depends from the country cutoff date—differs from calendar month of birth—which starts with January all over the world.

Table [A.4](#) in the Appendix reports first-stage results. This table shows that academic months of birth (i.e., the exogenous instrument that represents a proxy for expected relative age) has a negative and increasing effect on observed relative age (i.e., the endogenous independent variable of interest); in other words, later academic months of birth are associated with lower relative age. Moreover, single coefficients are mostly highly statistically significant. Finally, we shall observe that ancillary tests discussed in greater detail in the Section [3.2](#) provide evidence that these instruments are not weak. This is an important strength of our setting, since the typical issue with using a set of dummies as instruments is its weak correlation with the endogenous variable; this is a common problem in the so-called “many-weak-instruments” literature ([Angrist & Frandsen, 2022](#)).

3.2 Results

Basic descriptive statistics confirm our expectations about the relationship between students’ relative age and dietary behaviors. As main example, Figure [1](#) shows a positive correlation between the academic month of birth (i.e., expected relative age) and students’ probability of being objectively overweight, which implies a negative correlation

Figure 1: Mean values of objective overweight status per academic month of birth.



Note: Student i 's probability of being overweight on the y-axis, and academic month of birth on the x-axis. The latter represents the expected relative age, that is, the expected age difference of student i with respect to the hypothetically oldest student in class, who is born in the month that starts with the cutoff date that determines school cohorts. Academic month of birth 0 is the month that starts with the cutoff. The graph reports 90% confidence intervals and is based on regular students only (i.e., students who have not been redshirted, greenshirted, retained, or skipped the grade).

between relative age and overweight.

Table 2 shows the impact of relative age on the nine outcomes under consideration. A one-year increase in relative age (i.e., the theoretical maximum age difference between students in the same school class) decreases the likelihood of being objectively overweight by 2%, representing a 14.4% reduction relative to the sample mean, and reduces the likelihood of being subjectively overweight by 2.1%, a 6.6% decrease relative to the sample mean.²² In addition, the probability of being on a diet decreases by 2.2%, an effect size comparable to the difference between students from low and high socio-economic backgrounds. Relative age also appears to influence dietary habits. A one-year increase in relative age increases the likelihood of consuming vegetables at least five days a week by 2.4% and fruit by 2.2%, each corresponding to an increase of around 5% relative to their respective sample means. In contrast, it reduces the likelihood of regular candies and soft drinks consumption by 1.5% and 2.3%, respectively. Finally, a one-year increase in relative age increases the probability of having breakfast each day of the school week by 2.4%. This effect corresponds roughly to the difference between students with a low and medium socio-economic family background, or half the difference between those from low and high socio-economic backgrounds. However,

²²To obtain the effect of a one-month increase, one needs to divide the estimated effect by 12.

relative age does not seem to affect breakfast habits on weekends.

The full parameter estimates and test statistics on the validity of the excluded instruments are presented in Table A.5, Table A.6, and Table A.7 in the Appendix. These tests reject the null hypothesis that the instruments are not correlated with the endogenous variable and that they are only weakly correlated, respectively. In particular, the F-statistics exceed the critical values outlined by Stock & Yogo (2005), confirming that the instruments are not weak. Furthermore, the overidentification tests fail to reject the null hypothesis that the instruments are uncorrelated with the second-stage error term, suggesting the instruments are valid.

Table 2: Two-stage least square estimates of relative age effects on all nine outcomes.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.020*** (0.005)	-0.021*** (0.006)	-0.022*** (0.007)	0.024*** (0.006)	0.022*** (0.006)
N	374,064	577,691	457,398	572,881	574,055
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.015** (0.006)	-0.023*** (0.006)	0.024*** (0.006)	0.004 (0.003)	
N	572,962	573,242	548,696	554,528	

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes are regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

To understand the sign and the magnitude of the bias due to the endogeneity of relative age, the Appendix reports the results from the OLS, without instrumenting the variable of interest, see Table A.8. We note that the magnitudes of the effects from the OLS are much smaller and, for frequency of consumption of vegetables, soft drinks, and breakfasts, the sign of these estimates reverses as well.

We should note that, although our analyses benefit from cutoff variation, many

countries have January 1st as cutoff, which might reduce our ability to disentangle expected relative age from season of birth effect. However, there are two important aspects with respect to the above point: (a) there is little collinearity in the first stage with set of dummies for academic and calendar month of birth, as shown in Table A.3, and, (b) there is greater cutoff variation across students than across countries. In other words, there are relatively more students from countries with a cutoff other than January 1st: roughly 40% of students in our analyses attend a school under a different cutoff date. These two aspects mitigate potential concerns on our ability to properly disentangle expected relative age from season of birth in the first stage.

3.3 Robustness checks

In this section, we conduct six sets of robustness checks to address potential threats to our identification strategy and validate our results.

First, we examine the robustness of our results using alternative measures of relative age that capture different dimensions of within-class age variation. We begin by calculating the distance between student i 's age and the class average age. While our main specification captures how far behind a student is from the maximum age in the class, potentially reflecting perceived maturity gaps, this alternative measure indicates whether a student is younger or older than the average peer. This distinction reflects a different social reference point, as comparisons to average peers may reflect more salient group norms than comparisons to the oldest student. Table A.9 in the Appendix presents analyses using this alternative specification, showing equivalent results. In addition, in Table A.10, we employ a gender-specific relative age measure, comparing the age of student i with the age of the oldest same-sex student in the class. This accounts for the possibility that body-related comparisons and dietary behaviors are more influenced by same-sex peers. By doing so, our results hardly change. That our findings remain robust across both alternative specifications strengthens the interpretation that relative age effects are not dependent on a specific definition but rather reflect a broader peer-based mechanism influencing adolescents' dietary choices.²³

Second, we conduct three additional robustness checks concerning the model specification. The first involves replacing country fixed-effects with school fixed-effects. The

²³Furthermore, we show in Table A.11 in the Appendix that our results are maintained even if the calculation of relative age is not restricted to regular students, but also takes irregular students into account.

results, reported in Table A.12 in the Appendix, remain virtually unchanged in terms of statistical significance and magnitude, except for the analysis on subjective overweight, where the effect of relative age is no longer statistically significant—although the effect on being on a diet remains significant. The second check excludes control variables for both the presence of parents at home and the SES of the family, respectively, given recent evidence suggesting that relative age may influence these factors (Landersø et al., 2020), potentially introducing endogeneity. The results are reported in Table A.13 and Table A.14 in the Appendix and show parameter estimates that are nearly identical to the original analyses in both cases. In the third check, we extend our set of control variables to include class-specific characteristics such as the class size, the share of female students, and the share of students from high-SES families.²⁴ The results reported in Table A.15 are identical to those in the main analyses.

Third, assuming that assignment to classes is as good as random, we can create an instrument with class specific expected relative age. We create the deviation of student i 's expected relative age from the rest of the class average expected relative age. This instrument still allows us to control for entire battery of control variables, including season of birth fixed-effects. There are two caveats. First, this instrument does not allow us to conduct the overidentification test, which is the main strength of our current instrument. Second, we cannot verify in which countries student assignment to classes is not random; however, these analyses are still conducted with country fixed-effects which should partially capture this heterogeneity in institutional settings. Results are reported in Table A.16 and show substantially identical results, except for the analysis on subjective overweight, where the relative age effect is no longer statistically significant—although still negative.

Fourth, we perform a leave-one-out analysis, estimating our model while excluding one country at a time. Figures A.3, A.4, and A.5 present the estimates of the relative age effect coefficient corresponding to each excluded country. The results demonstrate robustness, as omitting any single country does not affect the magnitude or statistical significance of the estimates relative to the baseline specification.

Fifth, we conduct analyses with school level standard errors, in lieu of class standard errors—which is the level where relative age is measured at. Results are reported in Table A.17 and they are substantially identical, in terms of statistical significance, to

²⁴We cannot include class fixed-effects because that would induce collinearity with absolute age—which would have to be excluded—and alter the interpretation of the relative age coefficient.

those from the main analyses.²⁵

Sixth, we conduct analyses where we additionally instrument absolute age with expected absolute age, EAA_i , that is, the absolute age that student i would have if she was a regular student. More concretely, it is computed as the expected absolute age of students who are surveyed in the same wave and in the same country, attend the same classroom, and were born in the same quarter, similar to [Peña & Duckworth \(2018\)](#). These results are reported in the Appendix, Table [A.18](#), which are identical to the main results.

3.4 Heterogeneity

Having established the robustness of our results, we now extend our analysis to examine heterogeneity across several individual and institutional dimensions. Specifically, we explore heterogeneity by (i) socio-economic status, (ii) absolute age, (iii) gender, (iv) school meal provision, and (v) regional differences in diets to shed light on the underlying mechanisms and contextual factors that may mediate or amplify relative age effects on adolescents' dietary behaviors.

Socio-economic status. Prior research has identified socio-economic status as a critical determinant of eating behavior, with lower SES often associated with less healthy dietary patterns ([Desbouys et al., 2020](#)). To examine whether relative age effects differ by SES, we replicate our main analyses separately for adolescents from low, medium, and high SES backgrounds. Table [A.19](#) reveals that the direction and magnitude of relative age effects are broadly consistent across SES groups. The only two exceptions are the consumption of vegetables where we observe no significant relative age effect among high SES students and soft drink consumption where we find no effect among low SES students.

Absolute age. Another individual-level characteristic that might affect the relevance of relative age is a student's absolute age. Previous studies have shown that younger children might be more vulnerable to relative age disadvantages compared to children with a higher absolute age. In contrast, relative age differences may become more

²⁵Had we had more countries, we could have conducted analyses with standard errors at this level. The recommended minimum range for reliable inference is at least 42 clusters ([Angrist & Pischke, 2008](#)), as the usage of cluster-robust standard errors assumes a large number of clusters ([Cameron et al., 2008](#)), while we have 32 regions—with Belgium and Denmark split in two regions each.

salient as children grow older and gain greater autonomy over their behaviors, including dietary choices and eating routines. To investigate heterogeneity with respect to the age, we include an interaction term of relative age and absolute age of a student in our main specification.²⁶ As shown in Table A.20, the influence of the relative age does not depend on absolute age.

Gender. Next, we explore gender-based heterogeneity in relative age effects. As shown in Table A.21, weight-related outcomes (subjective and objective overweight and dieting) are more strongly associated with relative age among male students. The effect on objective overweight is statistically significant for males but not for females, indicating a gender-specific sensitivity to relative age in terms of physical weight status. In contrast, the associations with fruit and vegetable consumption are largely similar across genders, suggesting that relative age does not differentially shape healthy eating habits in this domain. The effect on sweet consumption is modestly stronger for females, while the negative association between relative age and soft drink consumption is only statistically significant for males. Additionally, the positive effect of relative age on school-day breakfast consumption is also stronger for males, while the effect on weekend breakfast consumption is negligible for both genders. Taken together, these patterns suggest that male adolescents may be more vulnerable to relative age effects, particularly in relation to weight perception and weekday dietary routines.

Finally, as girls and boys reach puberty at different ages, we interact relative and absolute age in the male and female subsample, respectively. Again, we find no evidence that the influence of relative age changes with the absolute age of the students.

We conduct two additional analyses, with results reported in Tables A.23 and A.24 in the Appendix.²⁷ In these analyses, we focus on objective and perceived overweight. First, we investigate relative age effects on being both objectively and subjectively overweight, with interaction with absolute age, and by gender. The outcome variable is an indicator equal to 1 when the student is both objectively and subjectively overweight, and 0 otherwise. This outcome captures the extent to which overweight status is both “real” and internalized. The results, shown in Table A.23 confirms a strong negative effect of relative age on this joint condition: relatively younger students are significantly more likely to be both objectively and subjectively overweight. This effect is particu-

²⁶In this case, we do not proceed in conducting analyses on different subsamples, because the variable is continuous and subgrouping would be arbitrary.

²⁷We thank an anonymous referee for suggesting these two analyses.

larly pronounced among boys, and the interaction with absolute age suggests that this pattern becomes more marked as students get older. These findings support the idea that relative age can influence not only objective outcomes but also the internalization of those outcomes in terms of self-perception.

Second, we investigate relative age effects on overweight misperception, with interaction with absolute age, and by gender. The outcome variable defines a mismatch between objective and subjective overweight status. Results are presented in Table A.24. Here, we find no statistically significant relationship between relative age and the likelihood of misperceiving one’s weight status, either in the overall sample or by gender. This suggests that while relative age influences both objective and perceived overweight status when aligned, it does not appear to systematically drive misperception of overweight status.

School meal provision. Beyond individual factors, institutional differences between countries such as school meal provision may also moderate the relationship between relative age and eating behavior. Since our dataset covers students from various countries, it allows us to investigate such factors. To explore the role of school meal provision, we use data from Guio (2023) to classify countries into two groups: (i) those that provide free school meals to all students, at least at some ages (e.g., the Baltic countries, Finland, or Sweden), and (ii) those where meals are only available to specific groups (e.g., low-income or refugee students) or not provided at all (e.g., Denmark and the Netherlands).²⁸ The results, reported in Table 3, show that relative age has hardly any significant effect on the eating regime of students in countries that generally provide meals at school for all students. Notably, even in these countries, relative age has a positive and significant effect on the regularity with which children eat breakfast on non-school days, i.e, on weekends.

Regional diets. To further explore institutional and cultural moderators of relative age effects, we analyze heterogeneity between Southern and non-Southern European countries, using the United Nations geoscheme classification. Southern European countries, particularly those around the Mediterranean and Atlantic South are characterized by dietary traditions emphasizing a higher intake of fruits and vegetables as well as moderate consumption of fish, nuts, and olive oil, which provide healthy fats (Turati

²⁸The information provided by Guio (2023) contains information on school meal provision of 23 countries out of the 32 countries in our main analysis, representing 75% of our observations.

Table 3: Two-stage least square estimates of relative age effects on all nine outcomes, by universal school meals provision.

		Outcome			
Not universal					
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.024*** (0.007)	-0.022** (0.009)	-0.032*** (0.011)	0.023** (0.010)	0.020** (0.010)
N	228,254	329,408	249,209	327,022	327,614
		Sweets	Soft drinks	Breakfast school	Breakfast weekend
Relative age	-0.013 (0.010)	-0.027*** (0.009)	0.032*** (0.011)	0.008* (0.005)	
N	327,057	327,225	305,416	311,556	
Universal					
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.012 (0.020)	-0.020 (0.022)	-0.051* (0.029)	-0.006 (0.024)	-0.009 (0.021)
N	69,239	102,387	86,590	101,678	101,881
		Sweets	Soft drinks	Breakfast school	Breakfast weekend
Relative age	-0.004 (0.022)	-0.001 (0.017)	0.025 (0.023)	0.022** (0.011)	
N	101,684	101,745	101,157	100,560	

Note: Second stage estimates from the 2SLS. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

et al., 2015; Grosso & Galvano, 2016). At the same time, the intake of red meat and sugary foods tends to be lower compared to Central and Northern European countries. These regional dietary norms may influence adolescents' baseline eating behaviors and, consequently, how strongly these behaviors respond to relative age differences.

Table A.22 presents results across both regional groups. We observe consistent negative relative age effects on both objective and subjective overweight across both regions. However, when focusing on dieting, the effect is notably stronger in Southern

countries. Relative age also correlates with improved eating habits in both regions, such as reduced consumption of sweets and soft drinks, and increased intake of vegetables, with these effects generally more pronounced in the South. In addition, we observe in both regions positive relative effects concerning fruit consumption, which is only statistically significant among non-Southern countries. Lastly, the positive influence of relative age on structured eating routines (i.e., breakfast on weekdays) appears to be more pronounced among the Southern countries.

Taken together, these findings suggest that while relative age effects are pervasive, they may be amplified by regional dietary cultures and norms, particularly in Southern Europe.

4 Discussion

This study shows that, overall, relative age affects dietary behaviors, which could partly explain why previous studies identify a higher risk of suffering from body-weight issues among relatively young students ([Anderson et al., 2011](#); [Fumarco et al., 2020](#)). In sum, relatively young students have a less balanced diet. Moreover, relatively old students are more likely to eat breakfast daily. Finally, relatively young students exhibit a higher likelihood of dieting.

The first contribution of this study is to the literature on school determinants of dietary patterns among adolescents. Previous studies have focused on the effects of school peers' dietary and weight management behaviors ([Yakusheva et al., 2011](#); [Fortin & Yazbeck, 2015](#); [Gwozdz et al., 2019](#)), as well as on schools meals provision ([Au et al., 2016](#); [Frisvold, 2015](#); [Lundborg et al., 2022](#)). This paper also shows that relative age is an important determinant of dietary behaviors, although we cannot investigate mechanisms behind this effect.

This study has a twofold contribution to the literature on relative age effects as well. First, previous studies provide evidence that relatively young students tend to perform worse in school ([Bedard & Dhuey, 2006](#); [Elder & Lubotsky, 2009](#); [Peña, 2017](#); [Sprietsma, 2010](#)) and exhibit lower non-cognitive abilities and poorer well-being ([Dhuey & Lipscomb, 2008, 2010](#); [Fumarco & Baert, 2019](#); [Fumarco et al., 2020](#); [Fumarco & Schultze, 2020](#); [A. M. Mühlenweg, 2010](#); [A. Mühlenweg et al., 2012](#); [Patalay et al., 2015](#); [Schwandt & Wuppermann, 2016](#); [Thompson et al., 2004](#)); relative age differences

in dietary behaviors might explain part of these effects. Moreover, previous studies show that regular intakes of breakfast, fruits, and vegetables are associated with higher levels of school performance (Kim et al., 2016), while the consumption of soft drinks is linked to lower school performance. Additional studies document a positive relationship between diet quality and academic performance, while others find that unhealthy eating habits are accompanied by lower levels of well-being among adolescents (Florence et al., 2008; Puloka et al., 2017). Relatively younger students already underperform at school (Bedard & Dhuey, 2006); poorer diets, combined with less frequent breakfasts on school days, may create a vicious cycle that further exacerbates their performance gaps.

Second, while most studies on relative age effects offer high internal validity, concerns about external validity often remain, as these studies typically rely on administrative data from a single country with unique institutional or cultural features. A notable exception is the seminal study by Bedard & Dhuey (2006). By contrast, our analysis is based on a multi-country sample, which strengthens the external validity of our findings. This broader context allows us to observe consistent patterns across diverse educational systems, cultures, and dietary norms. Our heterogeneity analyses suggest that the main results are broadly robust across country groups (Southern vs. non-Southern countries), gender, and SES, even though the magnitude of some effects (e.g., on specific foods or drinks) may vary slightly. This contributes to the growing literature in economics emphasizing the importance of external validity and generalizability of empirical results (Alubaydli & List, 2015; List, 2020; Bo & Galiani, 2021; List, 2022). Furthermore, our leave-one-out robustness checks indicate that no single country is driving the overall effects.

Although not a primary aim of this study, our findings may still contribute to the public debate on school start times. This debate reached the apex in a famous 2013-Tweet by Arne Duncan, then US Secretary of State for Education, that read: ‘**Let teens sleep, start school later.**’ We find that there is a relative age effect on the frequency of breakfast on school days, but not on weekend days. While there might be several causes to this result, one of them could be a compensatory behavior during school days. In particular, relatively younger students might ask their parents to sleep longer, and skip breakfast, at least on one schoolday per week, when they have to wake up early.²⁹

²⁹Note that the students we observe are from middle and high-school; they could reasonably obtain permission from their parents to skip breakfast at least on one schoolday per week. One may argue that an alternative explanation is that these students are “forced” to skip breakfast by their older

Future studies should delve more into this result and underlying mechanisms.

Additional analyses show that the results might change by gender, but only in terms of magnitude, which may reflect gendered differences in dietary habits or health awareness. Overall, the results highlight potential gender-based heterogeneity in how relative age shapes health-related behaviors.

There is one limitation to this study. We cannot rigorously answer the question of why relative age affects dietary behaviors. Knowing the mechanisms would have important policy implications. Thus, an important next step in the literature should be that of gaining empirically driven insights on the mechanisms.

For now, based on the existing literature, we can only envisage some mechanisms. For example, the discomfort coming from lagging behind in school (Bedard & Dhuey, 2006), from having few friends (Fumarco & Baert, 2019), from being stigmatized for being (mis)diagnosed with attention-deficit and hyperactivity disorder (Layton et al., 2018; Furzer et al., 2022), and from facing bullying (Ballatore et al., 2020), might mediate the effect of relative age on students' life-satisfaction and general mental health, which in turn might affect relatively young students' dietary behaviors.

The observed absence of relative age effects in countries with universal school meal provision suggests a clear policy direction. To address the disadvantages faced by relatively younger students, policymakers should prioritize expanding food programs within schools. This includes not only introducing meals in countries where they are currently absent but also enhancing existing programs by broadening eligibility or adding options like breakfast. In most European countries, breakfast is not typically provided in schools in the same way as in the U.S., where free or subsidized school breakfast programs are more common, but one promising approach could be targeted nutritional programs that ensure younger students receive dietary support. Breakfast programs, which have been shown to improve both dietary habits and academic outcomes, could be further tailored to encourage participation among younger students. Additionally, awareness campaigns aimed at teachers, parents, and school administrators could help reduce the unintended consequences of relative age effects, such as social isolation or unhealthy dietary coping mechanisms. Schools might also implement peer mentoring or structured social integration activities to counteract negative peer dynamics that

classmates, as a form of bullying (e.g., older classmates steal their breakfast). This would be true if these students had breakfast at school and not at home. However, the survey does not indicate whether this is the case, and it does not appear to be a frequent occurrence in most of Europe—unlike in the United States, where studies on the impact of free school breakfasts on performance have been conducted (see, e.g., Frisvold (2015); Leos-Urbel et al. (2013)).

disproportionately affect younger students. Further research into age-adjusted curriculum pacing or flexible grouping strategies within classrooms could help create a more supportive environment, reducing the need for maladaptive behaviors like excessive dieting. Recognizing that students' relative age—determined by their month of school entry—can shape dietary behaviors opens new avenues for education and public health policies to address age-related disparities and promote healthier outcomes for all.

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Appendix

Table A.1: Quantity of observations per country/territory, per wave.

Country	Wave						Cutoff month
	2001/2 N	2005/6 N	2009/10 N	2014/15 N	2017/18 N	All waves N	
Austria	4,150	4,757	4,679	3,313	3,794	20,693	Sep
Belgium, Flanders	1,345	3,113	3,029	3,230	3,013	13,730	Jan
Belgium, Wallonia	3,026	3,589	3,080	4,845	4,146	18,686	Jan
Bulgaria	0	4,826	0	4,639	3,199	12,664	Jan
Croatia	4,270	4,680	6,058	5,507	4,687	25,202	Apr
Czech Republic	5,006	0	4,324	5,041	11,265	25,636	Sep
Denmark	4,468	5,319	3,921	3,784	3,112	20,604	Jan
Estonia	3,279	4,202	4,131	4,001	4,592	20,205	Oct
Finland	5,143	5,143	6,496	5,810	0	22,592	Jan
France	7,393	5,736	5,457	5,168	8,599	32,353	Jan
Greece	0	0	4,808	4,078	3,807	12,693	Jan
Greenland	0	0	198	141	556	895	Jan
Hungary	3,985	3,450	4,569	3,737	3,456	19,197	Jul
Iceland	0	8,480	8,747	9,160	3,643	30,030	Jan
Ireland	1,951	3,730	1,859	3,366	3,120	14,026	Jan
Italy	4,313	3,867	4,734	3,906	4,025	20,845	Jan
Latvia	3,225	4,096	4,053	4,924	3,946	20,244	Jan
Lithuania	5,586	5,575	5,221	0	1,507	17,889	Jan
Luxembourg	0	2,889	2,968	2,192	2,315	10,364	Sep
Malta	1,853	0	0	2,227	1,936	6,016	Jan
Netherlands	3,778	3,796	4,076	3,862	4,206	19,718	Oct
North Macedonia	3,593	4,749	3,434	4,096	4,072	19,944	Jan
Norway	4,943	0	4,050	3,144	2,891	15,028	Jan
Poland	6,245	5,475	4,190	4,068	4,953	24,931	Jul
Slovakia	0	0	4,468	4,997	0	9,465	Sep
Slovenia	3,894	5,070	5,322	4,795	5,574	24,655	Jan
Spain	5,418	7,738	3,861	3,442	2,938	23,397	Jan
Sweden	3,778	4,332	6,627	7,471	4,076	26,284	Jan
Ukraine	3,943	4,859	5,345	3,095	5,263	22,505	Jan
England	3,822	4,697	3,437	5,261	3,084	20,301	Sep
Scotland	4,381	6,130	6,668	5,672	4,799	27,650	Mar
Wales	3,771	4,384	5,326	5,050	0	18,531	Sep
Total	109,227	128,406	140,087	139,060	122,629	639,409	

Note: Flanders and Wallonia as well as Denmark mainland and Greenland hold separate surveys within Belgium and Denmark, respectively.

Table A.2: Conditional correlation between expected relative age and main control variables.

Variables	Female (1)	Lives with both parents (2)	SES: Low (3)	SES: Medium (4)	SES: High (5)
ERA 1	0.000 (0.004)	-0.003 (0.004)	-0.001 (0.004)	0.001 (0.004)	0.000 (0.004)
ERA 2	0.005 (0.004)	0.001 (0.003)	-0.000 (0.004)	-0.001 (0.003)	0.001 (0.004)
ERA 3	0.002 (0.004)	-0.002 (0.003)	-0.005 (0.004)	0.002 (0.003)	0.004 (0.004)
ERA 4	0.004 (0.004)	-0.001 (0.003)	-0.001 (0.004)	-0.002 (0.003)	0.003 (0.004)
ERA 5	0.001 (0.004)	-0.001 (0.004)	0.002 (0.004)	-0.000 (0.004)	-0.002 (0.004)
ERA 6	0.004 (0.004)	0.005 (0.003)	0.000 (0.004)	-0.003 (0.003)	0.002 (0.004)
ERA 7	0.010 (0.004)	0.002 (0.004)	0.003 (0.004)	-0.001 (0.003)	-0.002 (0.004)
ERA 8	0.006 (0.004)	0.001 (0.003)	-0.000 (0.004)	0.002 (0.003)	-0.001 (0.004)
ERA 9	0.004 (0.004)	0.001 (0.003)	-0.002 (0.004)	0.003 (0.003)	-0.002 (0.004)
ERA 10	0.003 (0.004)	-0.005 (0.004)	0.003 (0.004)	0.001 (0.003)	-0.004 (0.004)
ERA 11	0.003 (0.004)	-0.004 (0.004)	0.004 (0.004)	-0.000 (0.004)	-0.004 (0.004)
N	616,973	596,387	616,973	616,973	616,973
F-test: p-value	0.371	0.064	0.362	0.791	0.339

Note: ERAs are dummies for academic month of birth, that is, expected relative age. SES is socio-economic status. All these regressions include controls for country, wave, and season of birth fixed-effects. Clustered standard errors at the class level in parentheses. The p-value of the F-test refers to the joint significance of the instruments. ***, **, * indicate significance at 1%, 5% and 10%, respectively. P-values on estimated effects of individual dummies are Bonferroni adjusted (i.e., p-value x 11).

Table A.3: Variance inflation factor of expected relative age and season of birth, from first stage.

Variables	VIF
ERA	
1	3.39
2	3.30
3	3.37
4	3.15
5	3.85
6	3.08
7	3.75
8	3.08
9	3.27
10	3.15
11	3.43
Season of birth dummies	
1	3.35
2	3.39
3	3.39
4	3.21
5	3.85
6	3.07
7	3.74
8	3.06
9	3.29
10	3.16
11	3.46
Mean VIF	3.62

Note: ERAs are dummies for academic month of birth, that is, expected relative age. Season of birth dummies represent months within the calendar year. VIF's from in this table are obtained from the full first stage. Thus, the mean VIF reported here is not the mean VIF based on only the 22 variables in the table.

Table A.4: First stage results.

Variables	RA
ERA 1	0.001 (0.004)
ERA 2	-0.038*** (0.003)
ERA 3	-0.076*** (0.003)
ERA 4	-0.150*** (0.003)
ERA 5	-0.200*** (0.004)
ERA 6	-0.194*** (0.004)
ERA 7	-0.238*** (0.004)
ERA 8	-0.314*** (0.004)
ERA 9	-0.337*** (0.004)
ERA 10	-0.328*** (0.004)
ERA 11	-0.332*** (0.004)
N	577,691

Note: ERAs are dummies for academic month of birth, that is, expected relative age. RA is relative age. Clustered standard errors at the class level in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively. P-values on estimated effects of individual dummies are Bonferoni adjusted (i.e., p-value x 11).

Table A.5: Relative age effects on dietary outcomes.

Variables	Objective overweight (1)	Subjective overweight (2)	On a diet (3)
RA	-0.020*** (0.005)	-0.021*** (0.006)	-0.022*** (0.007)
AA	-0.001*** (0.000)	0.015*** (0.001)	0.024*** (0.001)
Female	-0.062*** (0.001)	0.121*** (0.001)	0.135*** (0.002)
Lives with both parents	-0.017*** (0.001)	-0.041*** (0.002)	-0.041*** (0.002)
SES: Medium	-0.012*** (0.002)	-0.014*** (0.002)	-0.011*** (0.002)
SES: High	-0.034*** (0.001)	-0.038*** (0.002)	-0.025*** (0.002)
Obs.	374,064	577,691	457,398
Adj. R2	0.019	0.037	0.042
<i>Ancillary tests</i>			
Lagrange Multiplier st.	5,674	7,422	5,742
[p-value]	[0.001]	[0.001]	[0.001]
F-statistics	639.2	983.9	824.2
J-statistics	10.570	6.536	5.438
[p-value]	[0.392]	[0.768]	[0.860]

Note: 2SLS estimates. RA is relative age, AA is absolute age and it is centered around the mean to accommodate a more meaningful interpretation. SES is socio-economic status. Control variables additionally include vectors for wave, country, season of birth fixed-effects. The Lagrange multiplier statistics is for the under identification test. The F-statistics is for the weak identification test. The J-statistics is for the over-identification test. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.6: Relative age effects on eating regime.

Variables	Vegetables (1)	Fruits (2)	Sweets (3)	Soft drinks (4)
RA	0.024*** (0.006)	0.022*** (0.006)	-0.015** (0.006)	-0.023*** (0.006)
AA	-0.010*** (0.001)	-0.029*** (0.000)	0.017*** (0.000)	0.018*** (0.001)
Female	0.081*** (0.001)	0.074*** (0.001)	0.017*** (0.001)	-0.082*** (0.001)
Lives with both parents	0.037*** (0.002)	0.035*** (0.002)	-0.003* (0.002)	-0.037*** (0.001)
SES: Medium	0.036*** (0.002)	0.039*** (0.002)	0.003** (0.002)	-0.009*** (0.002)
SES: High	0.085*** (0.002)	0.080*** (0.002)	0.004** (0.002)	-0.021*** (0.002)
Obs.	572,881	574,055	572,962	573,242
Adj. R2	0.059	0.058	0.089	0.085
<i>Ancillary tests</i>				
Lagrange Multiplier st.	7,423	7,423	7,427	7,433
[p-value]	[0.001]	[0.001]	[0.001]	[0.001]
F-statistics	1,206	1,207	1,208	1,209
J-statistics	15.10	7.264	11.850	9.066
[p-value]	[0.128]	[0.700]	[0.295]	[0.526]

Note: 2SLS estimates. RA is relative age, AA is absolute age and it is centered around the mean to accommodate a more meaningful interpretation. SES is socio-economic status. Control variables additionally include vectors for wave, country, season of birth fixed-effects. The Lagrange multiplier statistics is for the under identification test. The F-statistics is for the weak identification test. The J-statistics is for the over-identification test. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.7: Relative age effects on eating intensity.

Variables	Breakfast school (1)	Breakfast weekend (2)
RA	0.024*** (0.006)	0.004 (0.003)
AA	-0.034*** (0.001)	-0.013*** (0.000)
Female	-0.068*** (0.001)	0.008*** (0.001)
Lives with both parents	0.079*** (0.002)	0.020*** (0.001)
SES: Medium	0.023*** (0.002)	0.008*** (0.001)
SES: High	0.048*** (0.002)	0.019*** (0.001)
Obs.	548,696	554,528
Adj. R2	0.056	0.018
<i>Ancillary tests</i>		
Lagrange Multiplier st.	6,645	7,158
[p-value]	[0.001]	[0.001]
F-statistics	1,087	1,156
J-statistics	2.215	6.200
[p-value]	[0.994]	[0.798]

Note: 2SLS estimates. RA is relative age, AA is absolute age and it is centered around the mean to accommodate a more meaningful interpretation. SES is socio-economic status. Control variables additionally include vectors for wave, country, season of birth fixed-effects. The Lagrange multiplier statistics is for the under identification test. The F-statistics is for the weak identification test. The J-statistics is for the over-identification test. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.8: Ordinary least square estimates of relative age effects on all nine outcomes.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.005*** (0.001)	-0.011*** (0.002)	-0.013*** (0.002)	-0.004** (0.002)	0.010*** (0.002)
N	374,064	577,691	457,398	572,881	574,055
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.001 (0.002)	0.014*** (0.002)	-0.003* (0.002)	-0.003*** (0.001)	
N	572,962	573,242	548,696	554,528	

Note: OLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on relative and centered absolute age, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.9: Two-stage least square estimates of relative age effects on all nine outcomes, with observed relative age measured as the difference from the mean age among regular students in the class.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.021*** (0.006)	-0.022*** (0.006)	-0.024*** (0.007)	0.025*** (0.006)	0.023*** (0.006)
N	373,277	576,641	456,623	571,839	573,011

	Sweets	Soft drinks	Breakfast school	Breakfast weekend
Relative age	-0.016** (0.006)	-0.024*** (0.006)	0.027*** (0.007)	0.005 (0.003)
N	571,919	572,200	547,667	553,504

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. The instrument, expected relative age, is unchanged. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.10: Two-stage least square estimates of gender-specific observed relative age effects on all nine outcomes.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.021*** (0.006)	-0.022*** (0.006)	-0.023*** (0.007)	0.026*** (0.007)	0.023** (0.006)
N	370,990	573,241	454,110	568,484	569,647

	Sweets	Soft drinks	Breakfast school	Breakfast weekend
Relative age	-0.017** (0.007)	-0.026*** (0.006)	0.028*** (0.007)	0.006* (0.003)
N	568,563	568,840	544,366	550,215

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, wave, country, and season of birth fixed-effects. Relative age is now gender specific: so, it is computed as the difference between the age of student i and the age of the oldest regular student who is the same gender as student i . The instrument, expected relative age, is unchanged. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.11: Two-stage least square estimates of relative age effects on all nine outcomes, with observed relative age measured as the difference from the oldest student in the class, regardless of being regular.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.020*** (0.005)	-0.020*** (0.006)	-0.022*** (0.007)	0.027*** (0.006)	0.022*** (0.006)
N	387,192	596,387	471,897	591,404	592,617
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.015** (0.006)	-0.025*** (0.006)	0.027*** (0.006)	0.005* (0.003)	
N	591,477	591,777	566,423	572,629	

Note: Second stage estimates from the 2SLS. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age—with relative age being measured as the difference from the the mean age among regular students in the class, centered absolute age, gender, family’s socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. The instrument, expected relative age, is unchanged. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.12: Two-stage least square estimates of relative age effects on all nine outcomes, with school instead of country fixed-effects.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.017*** (0.006)	-0.005 (0.006)	-0.019*** (0.007)	0.022*** (0.006)	0.015** (0.006)
N	373,940	577,688	457,397	572,875	574,049
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.015** (0.006)	-0.024*** (0.006)	0.026*** (0.006)	0.005* (0.003)	
N	591,878	592,168	566,508	572,393	

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, both parents at home, school and season of birth fixed-effects (each school is surveyed in one wave only, so wave is omitted together with country). Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.13: Two-stage least square estimates of relative age effects on all nine outcomes, without both parents at home as control variable.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.020*** (0.005)	-0.021*** (0.006)	-0.023*** (0.007)	0.023*** (0.006)	0.022*** (0.006)
N	388,701	597,327	474,612	591,776	593,056
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.016*** (0.006)	-0.016*** (0.006)	0.018*** (0.006)	0.003 (0.003)	
N	572,956	573,236	548,687	554,525	

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.14: Two-stage least square estimates of relative age effects on all nine outcomes, without family SES.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.020*** (0.005)	-0.021*** (0.006)	-0.022*** (0.007)	0.024*** (0.006)	0.022*** (0.006)
N	374,064	577,691	457,398	572,881	574,055
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.015** (0.006)	-0.023*** (0.006)	0.025*** (0.006)	0.005 (0.003)	
N	572,962	573,242	548,696	554,528	

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.15: Two-stage least square estimates of relative age effects on all nine outcomes, with additional control variables at the class level.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.021*** (0.005)	-0.021*** (0.006)	-0.022*** (0.007)	0.024*** (0.006)	0.022** (0.006)
N	373,064	577,691	457,398	572,881	574,055

	Sweets	Soft drinks	Breakfast school	Breakfast weekend
Relative age	-0.015** (0.006)	-0.024*** (0.006)	0.025*** (0.006)	0.005 (0.003)
N	572,962	573,242	548,696	554,528

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, both parents at home, class size, the share of female students, and the share of students from high-SES families, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.16: Two-stage least square estimates of relative age effects on all nine outcomes, with class specific expected relative age, measured as deviation of student i 's expected relative age from the rest of the class average expected relative age.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.015** (0.006)	-0.006 (0.006)	-0.019*** (0.007)	0.023*** (0.006)	0.019*** (0.006)
N	374,064	577,691	457,398	572,881	574,055
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.015** (0.006)	-0.019*** (0.006)	0.017*** (0.006)	0.004 (0.003)	
N	572,962	573,242	548,696	554,528	

Note: 2SLS estimates. All outcomes are investigated with the same expanded model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.17: Two-stage least square estimates of relative age effects on all nine outcomes, with standard errors clustered at school level.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.020*** (0.006)	-0.021*** (0.006)	-0.022*** (0.007)	0.025*** (0.006)	0.022*** (0.006)
N	374,064	577,691	457,398	572,881	574,055
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.016** (0.006)	-0.024*** (0.006)	0.025*** (0.006)	0.005 (0.003)	
N	572,962	573,242	548,696	554,528	

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of school in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.18: Two-stage least square estimates of relative age effects on all nine outcomes, with instrumented absolute age as well.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.020*** (0.005)	-0.021*** (0.006)	-0.022*** (0.007)	0.024*** (0.006)	0.022*** (0.006)
N	374,064	577,691	457,398	572,881	574,055
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.015** (0.006)	-0.023*** (0.006)	0.024*** (0.006)	0.004 (0.003)	
N	572,962	573,242	548,696	554,528	

Note: 2SLS estimates. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, predicted and centered absolute age, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.19: Two-stage least square estimates of relative age effects on all nine outcomes, by SES.

	Outcome								
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits	Sweets	Soft drinks	Breakfast school	Breakfast weekend
Low SES									
Relative age	-0.023*** (0.007)	-0.019** (0.008)	-0.016* (0.009)	0.027*** (0.008)	0.014* (0.008)	-0.025*** (0.008)	-0.011 (0.008)	0.018** (0.009)	0.004 (0.004)
N	155,394	294,128	232,915	292,367	292,831	292,437	292,514	279,814	283,931
Middle SES									
Relative age	-0.039*** (0.011)	-0.027** (0.012)	-0.033** (0.014)	0.038*** (0.013)	0.024* (0.012)	-0.037*** (0.012)	-0.031*** (0.012)	0.031** (0.013)	0.004 (0.007)
N	86,494	133,305	105,506	132,353	132,594	132,380	132,444	126,347	127,940
High SES									
Relative age	-0.023*** (0.007)	-0.027*** (0.008)	-0.023** (0.009)	0.004 (0.008)	0.022*** (0.008)	-0.015* (0.008)	-0.022*** (0.008)	0.021*** (0.008)	0.004 (0.004)
N	155,394	234,579	181,301	232,983	233,387	233,003	233,112	224,266	227,145

Note: Second stage estimates from the 2SLS. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.20: Two-stage least square estimates of relative age effects on all nine outcomes, interaction with age.

	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.021*** (0.007)	-0.022*** (0.008)	-0.027*** (0.009)	0.021** (0.008)	0.022*** (0.008)
Relative age x	-0.004 (0.015)	-0.004 (0.017)	-0.018 (0.021)	-0.011 (0.018)	-0.001 (0.017)
Centered abs. age					
N	374,064	577,691	457,398	572,881	574,055
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.014* (0.008)	-0.021*** (0.008)	0.028*** (0.008)	0.004 (0.004)	
Relative age x	0.005 (0.017)	0.011 (0.016)	0.010 (0.018)	-0.001 (0.009)	
Centered abs. age					
N	572,962	573,242	548,696	554,528	

Note: Second stage estimates from the 2SLS. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age and its interaction with the former, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.21: Two-stage least square estimates of relative age effects on all nine outcomes, by gender and interacted with absolute age.

	Outcome								
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits	Sweets	Soft drinks	Breakfast school	Breakfast weekend
Female									
Relative age	-0.010 (0.007)	-0.019** (0.008)	-0.016* (0.009)	0.026*** (0.008)	0.013 (0.008)	-0.025*** (0.008)	-0.011 (0.008)	0.018** (0.009)	0.004 (0.004)
N	189,785	294,128	232,915	292,367	292,831	292,437	292,514	279,814	283,931
Female and interacted with absolute age									
Relative age	-0.011 (0.008)	-0.024** (0.011)	-0.028** (0.012)	0.024** (0.011)	0.012 (0.011)	-0.026** (0.011)	-0.006 (0.010)	0.024** (0.011)	0.001 (0.006)
Relative age x Centered abs. age	-0.004 (0.018)	-0.018 (0.024)	-0.042 (0.029)	-0.007 (0.025)	-0.006 (0.025)	-0.005 (0.025)	0.017 (0.023)	0.021 (0.026)	-0.010 (0.013)
N	189,785	294,128	232,915	292,367	292,831	292,437	292,514	279,814	283,931
Male									
Relative age	-0.030*** (0.009)	-0.023*** (0.008)	-0.029*** (0.010)	0.022** (0.009)	0.032*** (0.009)	-0.006 (0.009)	-0.040*** (0.009)	0.034*** (0.009)	0.005 (0.005)
N	184,279	283,563	224,483	280,514	281,224	280,525	280,728	268,882	270,597
Male and interacted with absolute age									
Relative age	-0.030*** (0.011)	-0.019* (0.011)	-0.026* (0.013)	0.018 (0.012)	0.033*** (0.012)	-0.003 (0.012)	-0.036*** (0.012)	0.033*** (0.012)	0.008 (0.006)
Relative age x Centered abs. age	0.000 (0.024)	0.012 (0.023)	0.010 (0.028)	-0.013 (0.025)	0.004 (0.024)	0.010 (0.025)	0.010 (0.024)	-0.004 (0.025)	0.009 (0.013)
N	184,279	283,563	224,483	280,514	281,224	280,525	280,728	268,882	270,597

Note: Second stage estimates from the 2SLS. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.22: Two-stage least square estimates of relative age effects on all nine outcomes, Southern European countries versus other European areas, based on UN definition.

Southern	Outcome				
	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.021** (0.008)	-0.017** (0.009)	-0.042*** (0.010)	0.032*** (0.009)	0.011 (0.009)
N	91,062	127,227	100,593	126,242	126,500
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.031*** (0.009)	-0.051*** (0.008)	0.040*** (0.009)	0.011** (0.005)	
N	126,251	126,301	115,890	116,107	
Non-Southern	Objective overweight	Subjective overweight	On a diet	Vegetables	Fruits
Relative age	-0.017*** (0.005)	-0.016*** (0.005)	-0.024*** (0.006)	0.018*** (0.005)	0.018*** (0.005)
N	283,002	450,464	356,805	446,639	447,555
	Sweets	Soft drinks	Breakfast school	Breakfast weekend	
Relative age	-0.011** (0.005)	-0.022*** (0.005)	0.023*** (0.005)	0.005* (0.003)	
N	446,711	446,941	432,806	438,421	

Note: 2SLS estimates. The analyses are conducted on two subsamples of students from Southern European countries versus other European areas, based on UN definition. All outcomes are investigated with the same model specification: the outcomes is regressed on predicted relative age, centered absolute age, gender, family's socio-economic status, wave, and country fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.23: Two-stage least square estimates of relative age effects on being both objectively and subjectively overweight, with interaction with absolute age, and by gender.

	Total sample		Subsample			
			Male	Male	Female	Female
Relative age	-0.010**	-0.019***	-0.017**	-0.030***	-0.004	-0.008
	(0.004)	(0.005)	(0.007)	(0.008)	(0.006)	(0.006)
Relative age x		-0.005***		-0.008***		-0.003**
Centered abs. age		(0.001)		(0.002)		(0.001)
N	374,064	374,064	184,279	184,279	189,785	189,785

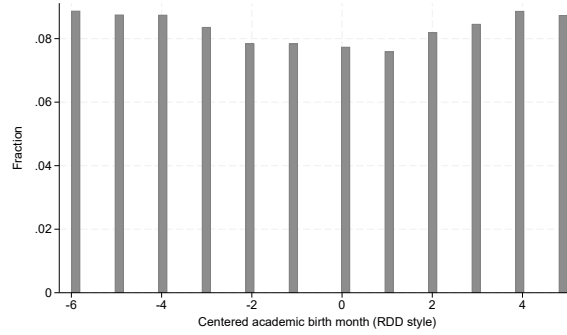
Note: 2SLS estimates. The outcome variable equals 1 when the student is both objectively and subjectively overweight. We use an expanded model specification: the outcomes is regressed on relative and centered absolute age, and their interaction, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.24: Two-stage least square estimates of relative age effects on overweight misperception, with interaction with absolute age, and by gender.

	Total sample		Subsample			
			Male	Male	Female	Female
Relative age	0.002	0.002	0.003	0.009	0.001	-0.001
	(0.007)	(0.008)	(0.010)	(0.012)	(0.010)	(0.011)
Relative age x		0.001		0.019		-0.009
Centered abs. age		(0.019)		(0.025)		(0.027)
N	374,064	374,064	184,279	184,279	189,785	189,785

Note: 2SLS estimates. The outcome variable equals 1 when the student is objectively overweight but not subjectively overweight, or vice-versa, while it is 0 when the student subjective perception corresponds to her objective overweight status. We use an expanded model specification: the outcomes is regressed on relative and centered absolute age, and their interaction, gender, family's socio-economic status, both parents at home, wave, country, and season of birth fixed-effects. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Figure A.1: Frequency of centered academic month of birth.



Note: Expected relative age is centered, so that it goes from -6 to 5, that is, from six months before the month that starts with the cutoff date to five months after. In absence of birth date manipulation around the cutoff date in month 0, there should be no discontinuity in the births frequency. Test for manipulation around the cutoff is carried out with the Stata command `rddensity`, which allows us conducting the robust bias-corrected test.

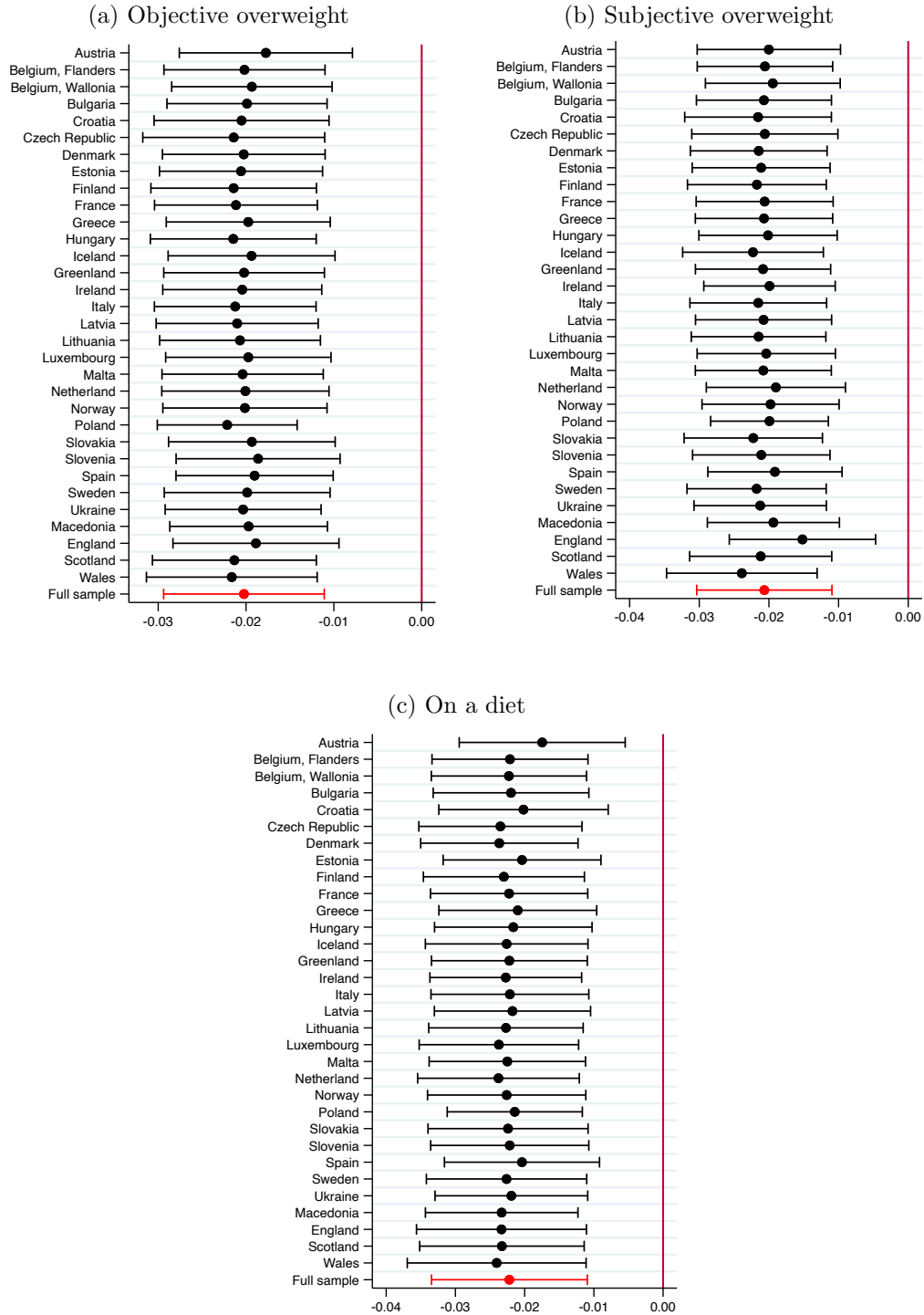
Figure A.2: Expected relative age relationship with season of birth and relative age; example with Luxemburgish and Scottish students.

		Calendar year t												Calendar year t+1		
Season of birth		Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar
Luxembourg	Academic year	x-1	x-1	x-1	x-1	x-1	x-1	x-1	x-1	x	x	x	x	x	x	x
	ERA	4	5	6	7	8	9	10	11	0	1	2	3	4	5	6
Scotland	Academic year	x-1	x-1	x	x	x	x	x	x	x	x	x	x	x	x	x+1
	ERA	10	11	0	1	2	3	4	5	6	7	8	9	10	11	0

Legend: Students' month of birth, Example 1 Students' month of birth, Example 2

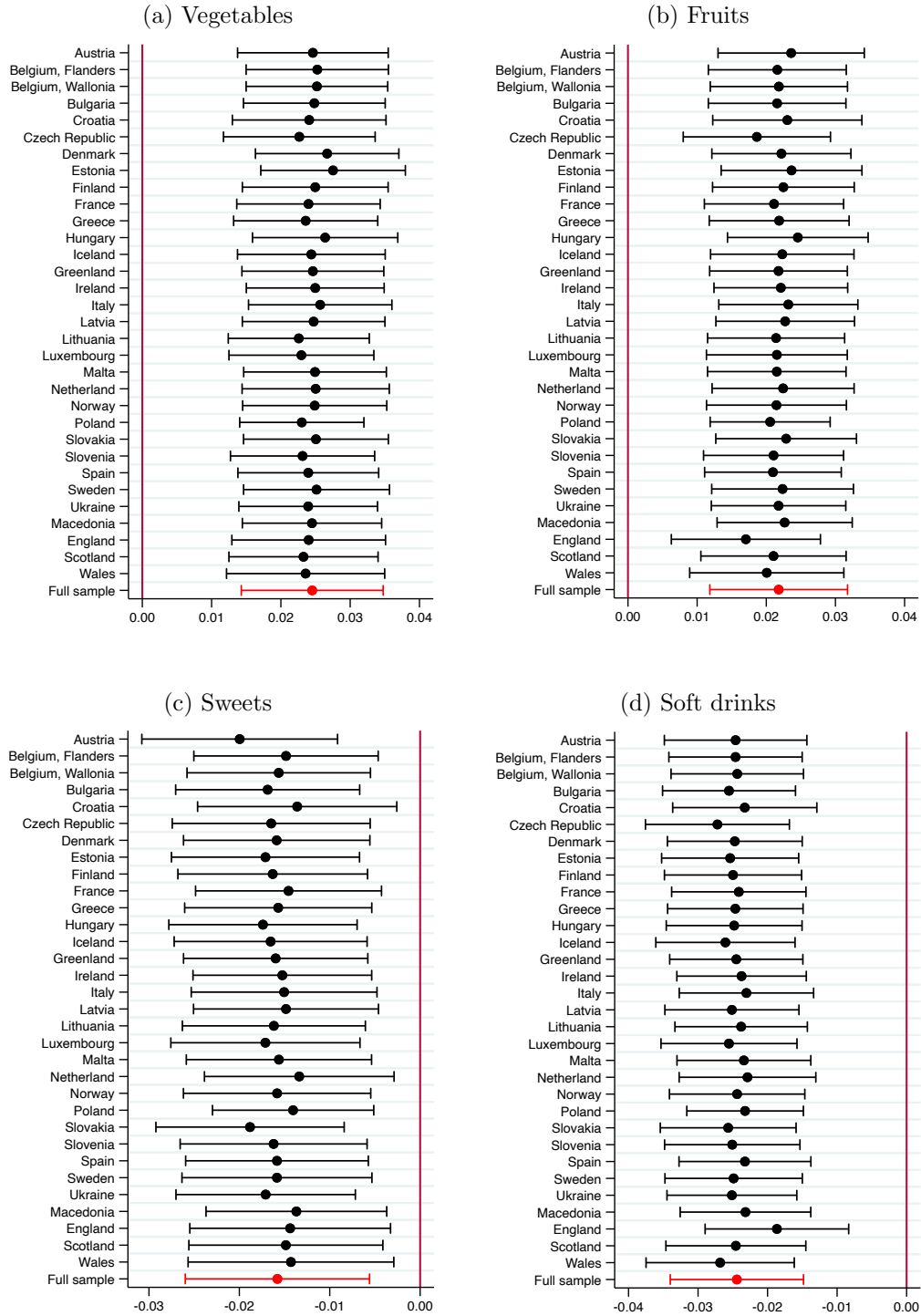
Note: This figure illustrates two examples. Cells with the dashed boarder illustrate Example 1; this example is about the difference between ERA and RA. The dashed-boarder cell under June shows that this student's ERA is 9, being born on the 9th month of academic year x-1, in Luxembourg. However, this student was retained and is placed in academic year x, where the oldest regular student is born four months later, in the dashed-boarder cell under October. Thus, this student is about four months older than the actual oldest regular student in their class. Cells with the thick boarder illustrate Example 2. Here, there are two children born in September of the same calendar year t, and thus in the same season of birth, but in countries with different cutoff dates: September 1st in Luxembourg and March 1st in Scotland. Thus, they have different Expected Relative Age (ERA).

Figure A.3: Leave-one-out, dietary outcomes.



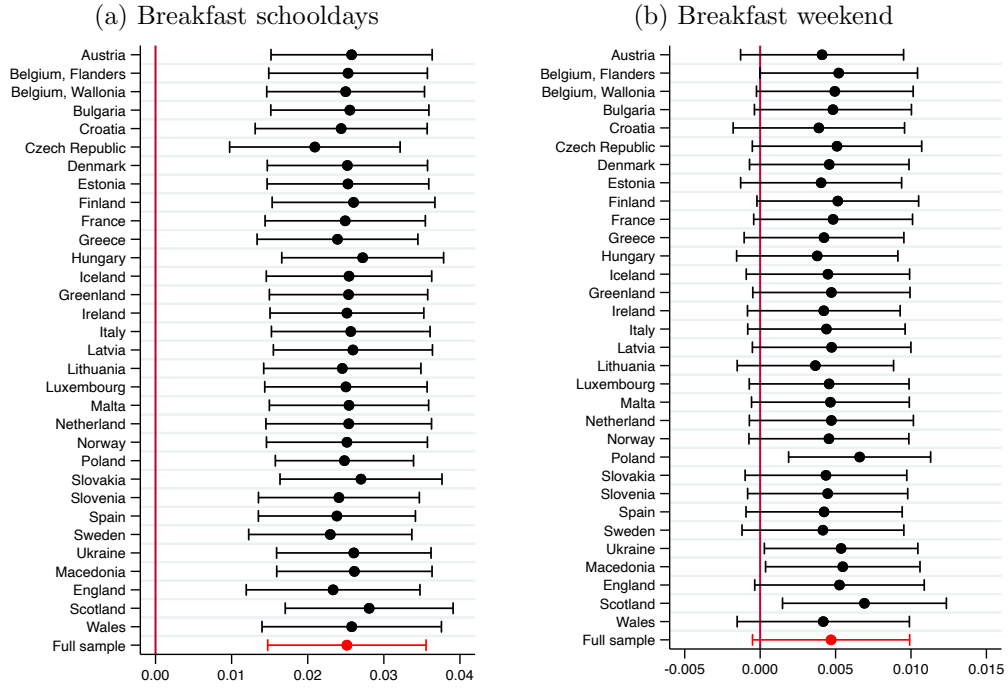
Note: Each point estimate represents the estimated coefficient of relative age from the baseline specification, re-estimated while excluding the indicated country from the sample. The *Full sample* estimate corresponds to the baseline specification including all countries. The confidence intervals are set at the 90% level of statistical significance.

Figure A.4: Leave-one-out, eating regime.



Note: Each point estimate represents the estimated coefficient of relative age from the baseline specification, re-estimated while excluding the indicated country from the sample. The *Full sample* estimate corresponds to the baseline specification including all countries. The confidence intervals are set at the 90% level of statistical significance.

Figure A.5: Leave-one-out, eating intensity.



Note: Each point estimate represents the estimated coefficient of relative age from the baseline specification, re-estimated while excluding the indicated country from the sample. The *Full sample* estimate corresponds to the baseline specification including all countries. The confidence intervals are set at the 90% level of statistical significance.