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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.

## 1. Introduction

The global rise in unbalanced diets is intensifying the phenomenon known as "globesity",[1] which is particularly concerning among adolescents. 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 & Altintaş, 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; Cobb-Clark et al., 2014; Jacka et al., 2011; McMartin et al., 2013; Mujcic & Oswald, 2016), and reduced risks of diseases, particularly those affecting the digestive system, such as different types of cancers that affect the digestive system, as well as lung and stomach cancer (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 (Fortin & Yazbeck, 2015; Gwozdz et al., 2019; Yakusheva et al., 2011), 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).

In this study, we investigate a novel school-based determinant of adolescents' dietary behaviors: the within-class age difference, referred

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[1] Term used by the WHO.

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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 large share of the global population may experience relative age differences, with potential repercussions on their educational and health trajectories well beyond the school years. This widespread and enduring relevance makes relative age an important subject of research and policy discussion.

Yet, despite its broad relevance, the direction of relative age effects on adolescents' eating behaviors is not clear *a priori*. Being relatively younger within a class may involve differences in maturity, self-regulation, or exposure to age-related pressures, which could contribute to less healthy dietary patterns; however, compensatory behaviors or closer parental monitoring may plausibly work in the opposite direction. Because both positive and negative effects are theoretically possible, the sign of the relationship is ultimately an empirical question.[2]

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 fruit, 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 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. In addition, it is important to stress that these findings are net of absolute age effects,[3] and other confounders, such as season-of-birth effects. Overall, these findings likely reflect the position of children within the age distribution of their relevant peer group: 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'.

While we do not directly isolate mechanisms, heterogeneity by school-meal regimes and by region suggests the effect might depend on institutional context and routine structures. We 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. For example, we find that relative age effects might be 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 (Arnold & Depew, 2018; Black et al., 2011; Dee & Sievertsen, 2018; Fumarco et al., 2020; O'Neil et al., 2014). Younger students are also more likely to be (mis)diagnosed with attention deficit and hyperactivity disorder (Balestra et al., 2020; Dee & Sievertsen, 2018; Elder & Lubotsky, 2009; Evans et al., 2010; Furzer et al., 2022; Layton et al., 2018; Schwandt & Wuppermann, 2016). 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. Therefore, one might expect relatively younger students to exhibit less healthy eating behaviors, as these psychological and social pressures could translate into adverse coping patterns. These processes may form part of a broader, self-reinforcing cycle in which early age-related disadvantages contribute to accumulating risks over time.[4] Finally, previous studies find evidence that relatively young students are less likely to participate in sport activities (Dixon et al., 2020; Fumarco & Schultze, 2020; Smith et al., 2018) and to be more likely overweight (Anderson et al., 2011; Carpenter & Churchill, 2024; Fumarco et al., 2020).

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 (Gay et al., 2018; Post & Kemper, 1993).

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] This ambiguity is consistent with prior evidence documenting mixed effects on educational and health outcomes (e.g., Cascio & Schanzenbach, 2016; Elder & Lubotsky, 2009; Peña, 2017; Peña & Duckworth, 2018; Sontheim, 2025), including studies that isolate relative-age effects and others that combine relative- and starting-age variation.

[3] While 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.

[4] There 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.

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

### 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 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 $\mathrm{kg/m^2}$) 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)* Fruit, a dummy variable which equals one if the student consumes fruit 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.

**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 | |
| Fruit | 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 Eq. (1).

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

For regular students, this measure ranges between zero (i.e., when $i$ is the oldest regular student in class, we have that $i = j$) 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, and the range is from −1 to 0 in order inform the reader on how much younger student $i$ is compared to reference student $j$.

The reference student $j$ is a regular student 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 idea behind this measure for relative age is based on two cornerstones. First, it allows us to exploit within-class age variation; as such, and differently from studies exploiting cohort-level variation in an RDD setting, this measure benefits from additional variation, because the focal student $j$ varies between classes. Second, the usage of regular student $j$ as focus student is a reflection of the norms within this literature (e.g., in RDD settings, the reference date is the cutoff date, which corresponds to the date of birth of the hypothetical regular student in class).

However, due to non-random grade skipping, greenshirting, retention, and redshirting, student $i$'s relative age might be endogenous and go beyond the −1 and 0 range; in this case, student $i$ would be a non-regular student. For example, a student who was born on the month with the cutoff and who was retained will have a RA equal to 1, meaning that the student is 1 year older than the oldest regular student $j$ in class $c$.

To allay endogeneity concerns, we instrument relative age with expected relative age. To do so, we rely on exogenous variation coming from the interaction between school-entry cutoff dates and student $i$'s month of birth, 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 factors causing endogeneity, ensuring that the estimates of relative age effects are not biased. This instrument is discussed in greater detail the next section.

As a further note, this relative age measure requires class-specific identifiers, $c$, which allow the comparison of student $i$ to the focal classmate $j$. Thus, we exclude observations without a classroom identifier in the HSBC data. Moreover, as discussed by Fumarco and Baert (2019),

[6] HBSC data do not include information on the day of birth, so it is not possible to tell whether a student is born before of 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.

[7] More 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.

[8] This is the wording used in the survey answer.

[9] This 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.

[10] Similar 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.

[11] It 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.

we exclude classes in the top 5% of the class size distribution.[12] Overall, the class size ranges from 8 to 32 students.

To identify focal student *j*, we need to identify regular students in each class. In order to do that, we follow a two-step procedure based on previous literature (Fumarco & Baert, 2019). First, for every class, we combine information on country cutoff date with information on the class-specific modal year of birth for students born in the second academic quarter–because these students are less likely to be redshirted or greenshirted. Second, students born in this quarter, in the quarter before, and in the following two quarters are 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. Moreover, we conduct additional analyses with different specifications of relative age, namely as the difference from regular classmates' average age, as gender-specific relative age (i.e., in each class, there are two focal students, based on student *i*'s gender), the difference from the oldest classmate–regardless of being a regular one. The results are reported in the Appendix and are discussed later in this manuscript.

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

Our instrumental variable leverages the mapping from calendar birth months to academic months induced by each country's cutoff, that is, the interaction of birth month with cutoff rule. More concretely, 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. Thus, this instrument counts the number of months from the month with the cutoff date (i.e., the month of birth of the hypothetically oldest student in class), or, said it otherwise, it informs the reader on how late in the academic year a student was born. This instrument reflects similar instruments in fuzzy-RDD settings.

We disaggregate this variable into individual month dummies—similar to what is suggested in Angrist and 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 and Krueger (1991).

The identification strategy relies on the assumption that academic month of birth influences dietary outcomes only through actual within-class relative age, conditional on a set of control variables. This identification strategy would not hold if this instrument was systematically correlated with some students' characteristics, where the main suspect being student's household socio-economic status. For example, high socio-economic status families could target specific periods of the year for giving birth or could manipulate the birthdate at the margin, next to the cutoff–for example through the cesarean section.

In order to gauge the validity of this set of instruments, we proceed in four ways. First, the 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 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. Because the set of instruments is composed of academic month-of-birth dummies, they all arise from the same country-specific enrollment-cutoff date. Thus, we invite caution in interpreting this test in isolation. Rather, we report the J statistic as part of a set of tests, which ultimately support the exclusion restriction. Below we discuss the additional tests.

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 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 and are reassuring because of two insights: (a) individual coefficients are consistently non significant, and (b) F-tests on the join orthogonality of the coefficients do not reject the null hypothesis at the standard $p$-value threshold of 0.05.

Third, the picture provided in the previous tests is reassuring; in particular, in Europe, on average, expected relative age seems to be uncorrelated to both observable and unobservable characteristics that might bias the estimates. However, as we argue above, there might be some birth date manipulation in the vicinity of the cutoff date (e.g., with cesarean section). 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.[18]

Fourth, to assess the impact of a possible the correlation between the instruments and both family socio-economic status and family

[12] We 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.

[13] There are potentially other ways to identify 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. Furthermore, the survey does not contain any direct information about students' grades, which would allow us to identify (iii), in combination with (i) and mandatory age at school entry.

[14] This is because school identifiers are wave-specific.

[15] If 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.

[16] Figure 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.

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

[18] This figure reflects the general non-uniform dates of birth distribution in Europe, and it is not a problem in this case. Non-uniformity would be problematic if: (a) non-uniformity was caused by birth date manipulation and (b) it was correlated to, for example, family socio-economic status. However, the above results are reassuring with this respect.

composition, we conduct a robustness checks without these control variables, but the results are unchanged compared to the main ones, as discussed in Section 3.3.

In conclusion, taking together results from these four groups of tests, we are cautiously optimistic that the set of instruments represented by dummies for academic month of birth is valid.

### 2.5. Control variables

The main control variable is absolute age.[19] 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).[20]

Our analyses additionally control for unobservable "season-of-birth effects". As discussed by Bound and Jaeger (2000) winter-born babies might be more likely to develop health 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. Buckles & Hungerman, 2013; Currie & Schwandt, 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.[21] 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.

[19] Note 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.

[20] As discussed by Landersø et al. (2020) 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. (Buckles & Hungerman, 2013; Clarke et al., 2019; 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.

[21] For 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.

## 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.[22] The second stage is illustrated by Eq. (2):

$$Y_i = \beta_0 + \beta_1 \widehat{RA_i} + \beta_2 AA_i + \mathbf{X_i}\boldsymbol{\zeta} + \mathbf{FE_i}\boldsymbol{\delta} + \mu_i \tag{2}$$

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.

$\beta_1$ represents the coefficient of interest. $\mathbf{X_i}$ is a vector of demographic control variables, that is, gender, family SES, and the 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 Eq. (3):

$$RA_i = \gamma_0 + \mathbf{ERA_i}\boldsymbol{\gamma} + \zeta AA_i + \mathbf{X_i}\boldsymbol{\iota} + \mathbf{FE_i}\boldsymbol{\phi} + \nu_i \tag{3}$$

where $RA_i$ is relative age. $\mathbf{ERA_i}$ 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 (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 on 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 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, Fig. 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 between relative age and overweight.

[22] A similar approach has been employed by Bedard and 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.

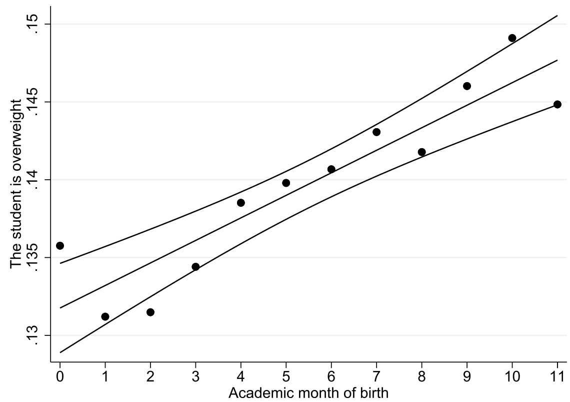


**Fig. 1.** Mean values of objective overweight status per academic month of birth.

*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. Thus, the larger the academic month of birth, the younger the student within their cohort. 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).

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. In addition, a one-year increase in relative age decreases 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, a one-year increase in relative age 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, relative age does not seem to affect breakfast habits on weekends. Notice that equivalent RA effects in months can be obtained by dividing the estimate by 12.

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-statistic exceed the critical values outlined by Stock and 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. Taken together – failure to reject null hypotheses from overidentification tests, orthogonality to observables, and no discontinuity at the cutoff – the evidence supports the exclusion restriction.

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.

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

| | Outcome | | | | |
|---|---|---|---|---|---|
| | Objective overweight | Subjective overweight | On a diet | Vegetables | Fruit |
| Relative age | −0.020*** | −0.021*** | −0.022*** | 0.024*** | 0.022*** |
| | (0.005) | (0.006) | (0.007) | (0.006) | (0.006) |
| N | 374,064 | 577,691 | 457,398 | 572,881 | 574,055 |
| | Sweets | Soft drinks | Breakfast school | Breakfast weekend | |
| Relative age | −0.015** | −0.023*** | 0.024*** | 0.004 | |
| | (0.006) | (0.006) | (0.006) | (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. RA coefficients are in years; thus, the implied per-month effect equals the coefficient divided by 12. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

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 great cutoff variation across students. In other words, there are relatively many 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]

[23] Furthermore, 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.

Second, we conduct three additional robustness checks concerning the model specification. The first involves replacing country fixed-effects with school fixed-effects. The 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 substantially 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.[25]

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 those from the main analyses.[26]

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 and Duckworth (2018). These results are reported in the Appendix, Table A.19, which are substantially 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.20 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 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. As shown in Table A.21, the influence of the relative age does not depend on absolute age.[27]

**Gender**. Next, we explore gender-based heterogeneity in relative age effects. As shown in Table A.22, 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.

[24] We 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.

[25] We used this different specification of expected relative age to replicate the analyses in Tables A.9, Table A.10, and A.11. The results are very similar, but they are not reported here for brevity.

[26] Had 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 subregions each. To circumvent this issue, we repeated the main analyses with wild boostrapped standard errors; these results lead to identical conclusion and are reported in Table A.18 in the Appendix.

[27] We replicate these analyses by estimating age-bin-specific effects of relative age (≤11, 12, 13, 14, 15, ≥16), merging ages 10 and 17 with adjacent bins due to small cell sizes. For each outcome, in a single 2SLS we interact relative age with age-bin dummies and instrument each interaction with the corresponding expected-relative-age instruments interacted with the same dummies, while including age-bin fixed effects and the full set of controls. We obtain similar insights, not reported here for sake of brevity. Overall, we do not observe a systematic pattern across age bins. The only exception are the results on "On a diet" outcome; we observe that from age 12 onward, the estimated effects are negative and of similar magnitude, whereas the reference-bin effect is not statistically significant. We note, however, that this pattern may reflect clinical practice discouraging dieting at very young ages rather than a genuine change in the relative-age effect (or its absence at earlier ages).

We conduct two additional analyses, with results reported in Tables A.24 and A.25 in the Appendix.[28] 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.24 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 particularly 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.25. 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).[29] 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 weekends.

However, we invite caution in the interpretation of these results. They might be driven by lack of power for the subsample of students from countries with universal school meal provision.[30]

**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 fruit and vegetables as well as moderate consumption of fish, nuts, and olive oil, which provide healthy fats (Grosso & Galvano, 2016; Turati et al., 2015). 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 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 | Fruit |
| Relative age | −0.024*** | −0.022** | −0.032*** | 0.023** | 0.020** |
| | (0.007) | (0.009) | (0.011) | (0.010) | (0.010) |
| N | 228,254 | 329,408 | 249,209 | 327,022 | 327,614 |
| | Sweets | Soft drinks | Breakfast school | Breakfast weekend | |
| Relative age | −0.013 | −0.027*** | 0.032*** | 0.008* | |
| | (0.010) | (0.009) | (0.011) | (0.005) | |
| N | 327,057 | 327,225 | 305,416 | 311,556 | |
| Universal | | | | | |
| | Objective overweight | Subjective overweight | On a diet | Vegetables | Fruit |
| Relative age | −0.012 | −0.020 | −0.051* | −0.006 | −0.009 |
| | (0.020) | (0.022) | (0.029) | (0.024) | (0.021) |
| N | 69,239 | 102,387 | 86,590 | 101,678 | 101,881 |
| | Sweets | Soft drinks | Breakfast school | Breakfast weekend | |
| Relative age | −0.004 | −0.001 | 0.025 | 0.022** | |
| | (0.022) | (0.017) | (0.023) | (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. RA coefficients are in years; thus, the implied per-month effect equals the coefficient divided by 12. Clustered standard errors at the level of class in parentheses. ***, **, * indicate significance at 1%, 5% and 10%, respectively.

Table A.23 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 (Fortin & Yazbeck, 2015; Gwozdz et al., 2019;

[28] We thank an anonymous referee for suggesting these two analyses.

[29] The 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.

[30] In a pooled 2SLS that includes the interaction between relative age and an indicator for universal school-meal provision, we test heterogeneity directly. The interaction term is not statistically significant across outcomes, and its standard errors imply large minimum detectable effects in the universal group, consistent with lower precision rather than absence of effects.

Yakusheva et al., 2011), 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; Mühlenweg, 2010; 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, fruit, 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 and 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; Bo & Galiani, 2021; List, 2020, 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.[31]

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 (Furzer et al., 2022; Layton et al., 2018), 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 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.

## Declaration of competing interest

All authors have participated in (a) conception and design, or analysis and interpretation of the data; (b) drafting the article or revising it critically for important intellectual content; and (c) approval of the final version.

This manuscript has not been submitted to, nor is under review at, another journal or other publishing venue.

The authors have no affiliation with any organization with a direct or indirect financial interest in the subject matter discussed in the manuscript

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[31] Note 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 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 (e.g. Frisvold, 2015; Leos-Urbel et al., 2013).

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### Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.econedurev.2025.102756.

### Data availability

The study uses data from the *Health Behaviour in School-aged Children (HBSC)* study. The HBSC Data Management Centre distributes the data in accordance with the HBSC data access policy. Information on how to request access to the HBSC data is available at: https://www.uib.no/en/hbscdata/113290/open-access. The replication package has been deposited at OpenICPSR under the title "Code for: Influence of within-class age differences on adolescents' eating behaviors."(https://doi.org/10.3886/E242800V1).

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