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ABSTRACT

Parental and School Responses to Student Performance: Evidence from School Entry Rules*

We examine whether parental and school investments reinforce or compensate for student performance. Our analysis exploits school-starting-age rules in 34 countries, capturing achievement variation that arises because younger children typically underperform their older peers. Parents respond to lower performance by providing additional homework help, while schools allocate weaker students to smaller classes and offer more remedial tutoring. Notably, parents provide more support to low-performing children in nearly all countries studied. Compensatory investments increase over grade levels, suggesting parents and schools respond as information about achievement is revealed. Moreover, our evidence suggests that parental and school investments are substitutes.

JEL Classification: I21, I28, J24

Keywords: human capital investment, parental inputs, school inputs, student performance, school starting age

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1 Introduction

Parental and school investments are crucial for children's human capital formation.[1] Whether the allocation of parental and school resources reinforces or compensates for student performance thus has important consequences for the distribution of skills in both the present and future generations. Such responses also affect the efficiency of education policies aiming to reduce performance gaps and can help explain why interventions have differential effects across contexts and family background (Todd and Wolpin, 2003).

Using data from education systems around the world, this paper studies how parental and school investments adjust to student performance. We also examine how parents' and schools' responses vary over grade levels and whether they interact with one another. Our empirical approach leverages exogenous variation in achievement due to students' expected relative age – i.e., their date of birth relative to the legal school entry cutoff date – and, thus, exploits the well-established performance gaps for children who start school at different ages (e.g., Fredriksson and Öckert, 2005; Bedard and Dhuey, 2006; McEwan and Shapiro, 2008; Elder and Lubotsky, 2009).

It is theoretically ambiguous how parents and schools choose to allocate resources across low- and high-performing students. The optimal allocation depends on their objectives, the properties of the human capital production function, as well as the budget and time constraints they face. Decreasing returns to child quality typically cause families to invest more in weaker children (e.g., Becker, 1981).[2] Additionally, policymakers may harbor redistributive goals for schools (Chambers, 1978; Thomas, 1980). The objectives of policymakers and parents must, however, be weighed against the properties of the human capital production function, which may provide incentives to allocate more resources to high-performing students (Becker and Tomes, 1976; Cunha and Heckman, 2007). In addition, decision makers may be constrained by the availability of resources, which can restrict their possibilities to invest optimally.

In the absence of clear theoretical predictions, the responsiveness of parental and

[1] For evidence on the importance of school resources, see, e.g., Krueger (1999), Angrist and Lavy (1999), Fredriksson et al. (2013), and Jackson et al. (2016). For evidence on the importance of family inputs, see, e.g., Björklund et al. (2006), Holmlund et al. (2011), Grönqvist et al. (2017), and Hanushek et al. (2021).

[2] Parents may also be averse to income inequality across children, which can reinforce compensatory behavior in terms of transfers (Becker and Tomes, 1976) or human capital investments (Behrman et al., 1982).

school inputs to student performance remains an empirical question. Our study provides international evidence on such responses using data from the Progress in International Reading Literacy Study (PIRLS) and the Early Childhood Longitudinal Study (ECLS). We construct measures of parental inputs (e.g., help with homework and skills practice) and school resources (e.g., class size and remedial tutoring) and document responses to performance variation across 34 countries. Additionally, we shed light on the dynamics of parental and school investments over grade levels in the US. Our analysis establishes five main facts.

First, parents compensate for academic disadvantage. Low-performing students receive significantly more help with homework than other students. Parents also spend more time practicing literacy and numeracy skills with children who perform worse in school. These parental responses are not driven by the behavior of teachers: we find no evidence that low-performing students receive more homework than high-performing students.

Second, parents' responses to student performance are consistent across contexts. Parental investments are compensatory in all 34 countries we study, and in three quarters of the countries, the response is statistically significant.[3] Moreover, we find parental adjustment across the entire socioeconomic background distribution, although the response is larger in absolute value among high-educated parents.[4]

Third, schools allocate more resources to weaker students. Low-performing children are more likely to be placed in smaller classes and are more frequently given remedial tutoring.

Fourth, there is little parental and school compensation at kindergarten entry, but such behavior emerges in subsequent grade levels. This result may be a bit surprising because the disadvantage of relatively younger children should be apparent at school start. We conjecture that parents are uninformed about the strength of the relationship between relative age and performance and that they update their priors over time.[5]

Fifth, parental responses are stronger in contexts where relative age has a larger impact on student performance. This suggests that parents compensate more when

[3] This differs from Celhay and Gallegos (2023), who find higher parental investments for relatively older children in Chile.

[4] This contrasts with Berniell and Estrada (2020), who find no compensation among low-educated parents.

[5] See Dizon-Ross (2019) for the importance (of the lack) of information for parental behavior.

schools are less successful in reducing the relative age gradient and that parental and school inputs are substitutes.

Our study is closely related to two recent papers that investigate parental responses to variation in relative age in Spain (Berniell and Estrada, 2020) and Chile (Celhay and Gallegos, 2023).[6] The focus of our paper is different. Although we show some reduced-form estimates, our primary interest is not in the effects of relative age per se. Rather, we use the stipulated school entry rules as a source of exogenous variation in student performance. This approach allows us to study how schools and parents respond to the same change in achievement across contexts; however, it requires that relative age does not directly impact parents' and schools' investment behavior, holding child performance constant. While recognizing that this may be a strong assumption, we show that parental investments do not change when we control for absolute age.[7] Moreover, we show that parents and schools do not respond to relative age at kindergarten entry – a result that is difficult to reconcile with relative age having a direct impact on behavior.

Our paper also relates to the literature on parental responses to school inputs, such as school quality (Cullen et al., 2006; Pop-Eleches and Urquiola, 2013), school grants (Das et al., 2013), class size (Fredriksson et al., 2016), and teacher qualifications (Chang et al., 2022). The existing evidence is mixed: some studies find that public and private inputs are substitutes, while others find that they are complements. In contrast to these papers, we study the responses to student performance directly. The estimates are thus informative about the possible adjustments of parents (and schools) to interventions with a given impact on student achievement. In addition, we provide evidence on how parental and school responses interact with one another.

To our knowledge, this is one of the first papers to directly estimate how school inputs adjust to arguably exogenous variation in student performance.[8] In general, parental and school responses may explain why the effects of various educational

[6] Our paper also relates to Landersø et al. (2020) and Karbownik and Özek (2023). They study family spillovers of relative age (on, e.g., sibling's school performance and mother's employment), providing indirect evidence of parents' and schools' investment behavior.

[7] We base this analysis on data from the US and Canada, which have within-country variation in the school entry rules, enabling us to hold quarter of birth constant.

[8] Figlio and Özek (2024) show that students scoring below proficiency cutoffs on standardized tests are placed into remedial classes and receive more educational resources. This complements earlier evidence on the effects of relative age on grade retention (Bedard and Dhuey, 2006; Dhuey et al., 2019) and the likelihood of receiving special education (Dhuey and Lipscomb, 2010; Shapiro, 2023).

interventions vary in the distribution of socioeconomic background, as well as over time – i.e., whether the effects persist or fade out.[9] Krueger and Whitmore (2001) and Bailey et al. (2020), for instance, argue that school responses are one reason for the fade-out of effects. We provide direct evidence on such responses.

The paper unfolds as follows. In the next section, we briefly discuss some additional literature that is related to our study. Section 3 includes a description of the data, some institutional details, and our empirical strategy. Section 4 presents the main results, and Section 5 contains some robustness checks. Finally, Section 6 concludes.

2 Related literature

There is an extensive literature on how parental investments adjust to children's genetic endowments, health at birth, exposure to early health interventions, and academic achievement.[10] The evidence is, however, far from conclusive. Some studies find that parents reinforce differences in endowment and skills, while others find support for compensatory responses. These discrepancies can in part be explained by the fact that researchers have used different identification strategies to estimate parental responses.[11] Most papers attempt to account for differences in parental inputs across families by exploiting within-sibling pair variation in birth weight or abilities. However, parental responses to child performance within families likely differ from the average behavior in the population (both between and within families), since parents

[9] Several papers have examined whether the effects of educational interventions persist or fade out. Lee and Loeb (1995), Currie and Thomas (2000), and Johnson and Jackson (2019) examine preschool programs; Krueger and Whitmore (2001) and Fredriksson et al. (2013) investigate class size reductions; and Jacob et al. (2010) and Chetty et al. (2014a) examine teacher quality.

[10] For evidence on parental responses to the child's genetic endowment, see, e.g., Breinholt and Conley (2023), Houmark et al. (2024), and Muslimova et al. (2020). For evidence on responses to health at birth, see, e.g., Royer (2009), Datar et al. (2010), Almond and Currie (2011), Del Bono et al. (2012), Hsin (2012), Lynch and Brooks (2013), Rosales-Rueda (2014), Yi et al. (2015), Grätz and Torche (2016), Abufhele et al. (2017), Leight (2017), and Bharadwaj et al. (2018). For evidence on exposure to early health interventions, see, e.g., Adhvaryu and Nyshadham (2016) and Bharadwaj et al. (2018). For evidence on responses to academic achievement, see, e.g., Yurk Quadlin (2015), Grätz and Torche (2016), Frijters et al. (2013), Nicoletti and Tonei (2020), and Fan and Porter (2020).

[11] The studies differ not only in the identifying strategies applied, but also in the treatments studied (polygenic scores, birth weight, health shocks, health interventions or skills), the measures of parental investments used (breastfeeding, parent-child interactions or educational expenditures) and the institutional context (time and space).

may be reluctant to invest unequally in their children. The generalizability of sibling comparisons is further complicated by possible spillover effects when redirecting scarce parental resources across siblings and through sibling interactions.

There is little credible evidence on how schools distribute resources in response to variation in student performance. There is descriptive work on the allocation of school resources across students in various school systems.[12] However, it is not clear whether the observed – typically compensatory – resource allocation is driven by egalitarian ambitions of policymakers or some other characteristic correlated with student background – e.g., the density of the student population in more remote areas, where disadvantaged students tend to live. The resources available to students from different backgrounds may also be the result of parents' residential sorting or school choice, rather than an effect of compensatory resource allocation across schools.[13]

3 Data, institutions, and empirical strategy

We use two data sources to investigate parental and school responses to student performance. Our primary data source is the Progress in International Reading Literacy Study (PIRLS), which we supplement with the Early Childhood Longitudinal Studies – Kindergarten Cohort (ECLS-K) for the US.[14] The datasets provide comprehensive information on student performance and parental and school inputs, enabling us to study the allocation of inputs in different countries and over grade levels in the US.

PIRLS is an international assessment of reading achievement for representative samples of fourth graders.[15] The International Association for the Evaluation of Educational Achievement (IEA) has administered the test every fifth year since 2001. In addition to the reading assessment, the data include information on parental and school inputs gathered from questionnaires sent to schools, teachers, students, and parents. Due to data constraints in several years, our analysis focuses on the PIRLS

[12] See, e.g., West and Wößmann (2006), Rubenstein et al. (2007), Cohen-Zada et al. (2013), Knight (2019), OECD (2019), Baker et al. (2022), and Lee et al. (2022).

[13] For evidence on residential sorting, see, e.g., Black (1999), Kane et al. (2006), Fack and Grenet (2010), Schwartz et al. (2014), and Zheng (2022). For evidence on school choice, see, e.g., Figlio and Lucas (2004), Rothstein (2006), Hastings and Weinstein (2008), Burgess et al. (2015), Abdulkadiroğlu et al. (2020), and Beuermann et al. (2023).

[14] Data from the PIRLS assessments and ECLS-K studies are publicly available through the IEA Data Repository and the NCES Data Products.

[15] Several countries test students in other grade levels, primarily fifth grade.

waves in 2006 and 2011. The majority of children in the dataset were born in 1995–96 or 2000–01.

ECLS-K:1999 and ECLS-K:2011 are longitudinal studies conducted by the National Center for Education Statistics (NCES) in the US. The studies follow children who entered kindergarten in the 1998/99 and 2010/11 school years, with the majority born in 1992–93 and 2004–05. Throughout the children's elementary school years, the studies collect a wide range of data through child assessments, parent interviews, and teacher and school administrator questionnaires. These data include information on children's reading and math proficiency, as well as their home, school, and classroom environments.

3.1 Key variables

We use the PIRLS and ECLS-K data to construct four key variables: (1) expected relative age, (2) reading test scores, (3) parental inputs, and (4) school inputs. We provide the most relevant details concerning the construction of these variables below and offer comprehensive explanations in the data appendix.[16]

Expected relative age We measure expected relative age as the difference between the test date and the student's date of birth, had the student adhered to the legal school starting age (SSA) rule in their country or region. We use the modal test date so that the variation in expected relative age stems from students' birthdate relative to the stipulated cutoff date for entering primary school.[17] We rank the resulting variable such that values of zero and one correspond to the youngest and oldest students in a school cohort, respectively.

To obtain reliable measures of students' expected relative age, we exploit information about the SSA rules in each country or region. Due to data restrictions in the ECLS-K public-use files, we use a data-driven approach to determine the relevant cutoff date for US students (see the data appendix for details). For all other countries, we obtain information on SSA rules from the PIRLS curriculum survey. We restrict our sample to countries with unambiguous cutoff dates that can be confirmed in the

[16] In the main appendix, we also provide descriptive statistics for our estimation samples; see Tables A.1, A.2, and A.3.

[17] The test date does not vary substantially across schools or students in the same country.

empirical distribution of birthdates. Figure A.1 in the appendix shows a map of the 34 countries that remain in our sample after this restriction.

Reading scores We use the PIRLS and ECLS-K reading assessments as measures of student performance, standardizing the item response theory (IRT) scale scores by country, wave, and grade level.[18] We focus on reading proficiency due to data availability. Students' relative age impacts performance similarly across academic subjects, implying that reading scores can be considered a general performance measure.[19] Parents and schools do not receive the results of the PIRLS and ECLS-K assessments and thus cannot react directly to students' scores. However, our US data reveal that these scores are highly correlated with other performance measures that should be more salient to parents and schools, such as teacher evaluations of children's literacy skills (see Figures B.4 and B.5 in the data appendix).

Parental inputs We obtain information on parental inputs from the home survey (PIRLS) and parent interviews (ECLS-K), which are typically completed by the child's mother or female guardian. Our main measure of parental investment is the frequency with which parents help their children with schoolwork or homework[20] – arguably one of the most crucial school-related activities in which parents can directly participate. We standardize parents' responses by country, wave, and grade level. When possible, we also create standardized measures of the frequency with which parents practice basic skills with their children.

School inputs We obtain information on school inputs from teacher questionnaires. Class size is our main measure of school inputs – it is one of the most crucial school inputs, and we observe it in all countries in our data. For the US, we have richer data on school inputs because teachers complete child-level questionnaires in addition to

[18] The PIRLS data includes five plausible values for individual reading scores. We use the first value for our main analysis, and as a robustness check, verify that the results are similar using the other four values.

[19] Figure B.3 in the data appendix shows the relationship between relative age rank and standardized test scores for a subset of children who were assessed in multiple subjects. The estimated gradients for reading, math, and science scores are not statistically different from each other. We do not use math and science scores in the main analysis because they are missing for most of the sample.

[20] The exact phrasing of the question varies somewhat by data source and survey wave. See the data appendix for details.

providing information at the class level. When analyzing data from the US, we also use an indicator of whether the child receives remedial tutoring at school.

3.2 Empirical strategy

We study how parents and schools adjust their investments in children in response to variation in their academic performance. The naive OLS estimate of, e.g., parental investments on children's performance is biased due to, e.g., omitted variables or reverse causality. To overcome this bias, we implement a two-stage least squares (2SLS) approach, exploiting the well-known fact that children who are relatively old for their school cohort tend to outperform their younger peers (e.g., Fredriksson and Öckert, 2005; Bedard and Dhuey, 2006; McEwan and Shapiro, 2008; Elder and Lubotsky, 2009).

Specifically, our approach leverages performance variation stemming from children's date of birth relative to the school starting age rule in their country or region. We estimate first stage (reduced form) regressions of the following form:

$$H_{ij} = \alpha_j + \gamma A_{ij} + \epsilon_{ij}, \tag{1}$$

where i indexes the child and j the school. H_{ij} is the child's reading score (human capital) and A_{ij} denotes the child's expected relative age rank, had they complied with the school entry rule in their country or region. To increase the precision of the estimates, we also include school fixed effects, α_j.[21] In most countries, the school starting age rule is the same in the entire country. But some countries – for instance, the US and Canada – have varying school starting age rules across the country. In these countries, children born at the same point in time may have different expected relative age rank across regions. In more restricted models, which include country – instead of school – fixed effects, we can thus hold date of birth (or absolute age) constant, while still identifying the coefficient on A_{ij}.

The reduced-form relationship between, say, parental investments, P_{ij}, and A_{ij} is

[21] The inclusion of school fixed effects has little impact on the point estimates (see, e.g., Table 1), but matters for precision when estimating class size effects. The fact that school fixed effects do not alter the point estimates suggests that parents do not try to compensate for children's low performance by enrolling them in schools with better-qualified teachers, smaller class sizes, or higher-achieving peers. Consistent with this, we find no relative age gradient in average school-level characteristics (see Table A.4).

given by

$$P_{ij} = \delta_j^p + \pi^p A_{ij} + \eta_{ij}^p \tag{2}$$

The instrumental variables (IV) estimate of the response of parental investments to performance variation is thus obtained from equations (1) and (2) as: $\beta^p = \pi^p/\gamma$. Analogously, the IV estimate of the response of school resources, S_{ij}, to performance is given by $\beta^s = \pi^s/\gamma$, where π^s is the coefficient in the reduced-form relationship between school resources and the expected relative age rank, and γ comes from equation (1). In many specifications, we pool the data for all countries and estimate the average responses to changes in expected relative age.

3.3 Validity of the empirical strategy

The validity of our instrumental variable approach relies on the standard identifying assumptions: relevance and exclusion. Regarding relevance, panel (a) of Figure 1 provides a graphical depiction of the first-stage regression, revealing a strong relationship between expected relative age and standardized reading score (t-ratio = 33.64). On average, the oldest children in a grade score almost a quarter of a standard deviation higher than their youngest peers.

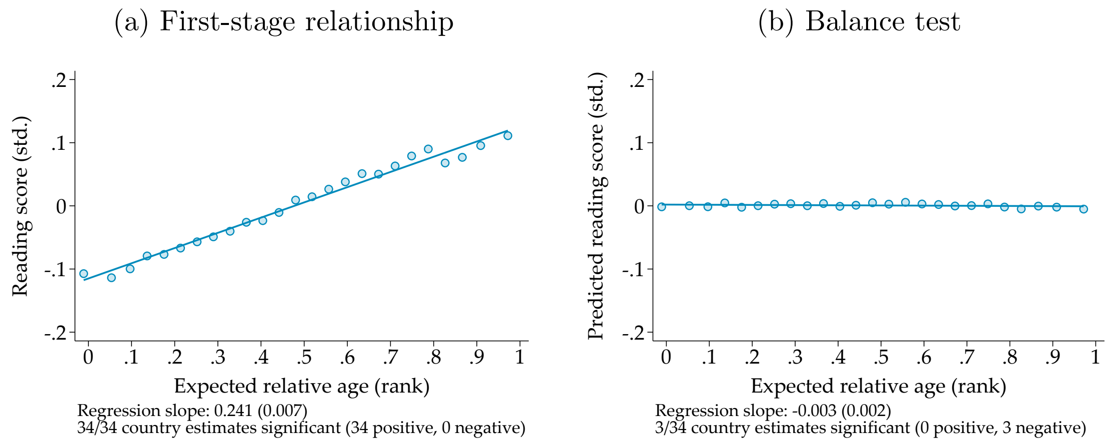
The exclusion restriction requires: (1) that relative age is as good as randomly assigned, and (2) that parents and schools respond to performance rather than to children's expected relative age per se. Since we exploit the relative age variation within – rather than across – school cohorts, the main threat against the first part of the exclusion restriction is that children born just after the school entry cutoff date differ from those born later.[22] Panel (b) of Figure 1 shows a balance test for predicted reading score, indicating that children's expected relative age is unrelated to pre-determined background variables (gender, foreign background, and parental education). This result suggests that children born in different parts of the school year are comparable, thus supporting the first part of the exclusion restriction.[23]

The second part of the exclusion restriction – that parents and schools respond to

[22]Given that we use a linear specification, any differences in the composition of children born in different parts of the year (e.g., Buckles and Hungerman, 2013) likely cancel out.

[23]When running country-specific regressions, three nations fail the balance test: Croatia, Hong Kong, and Singapore. In the appendix, we show that our main estimates are insensitive to dropping these three nations (see Table A.5).

Figure 1: Validity of the empirical strategy



Notes: Panel (a) shows the first-stage relationship from equation (1), and Panel (b) shows the relationship between the predicted reading score and the expected relative age rank. The prediction uses gender, foreign background, and parents' highest level of education as predictor variables. In both panels, we have used pooled international data ($N = 287,675$) and residualized on school fixed effects before binning and plotting.

performance rather than directly to children's expected relative age – may be more of a concern. In the sequel, we shed light on the validity of this assumption by showing that our estimates of parental responses are largely unaffected by controlling for absolute age and that parents do not respond to relative age at school start. If parents respond to relative age per se, we would expect to see such a relationship also at kindergarten entry.

4 Results

4.1 Parental responses

We begin by documenting that parents make compensatory investments in their children's learning.[24] Figure 2a depicts the reduced-form relationship between a child's expected age rank and the amount of homework help that parents provide. On average, the youngest children in a grade receive 0.14 standard deviations more homework help than the oldest children. Scaling this number by the corresponding gap in read-

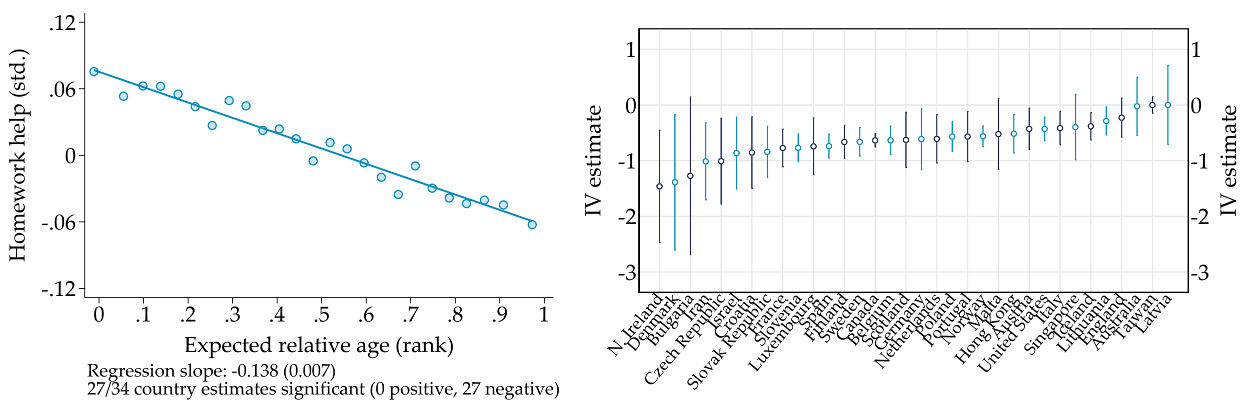
[24]In the appendix, we present a simple theory of parental and school behavior that, among other things, makes precise under what conditions we observe compensatory behavior.

ing performance yields an implied IV estimate of $\hat{\beta}^p = -0.14/0.24 = -0.58$; see Table 1 below. Thus, when children's reading performance decreases by one standard deviation, parents increase their homework help by 0.58 standard deviations. This amounts to about 1.19 additional days per week on a base of 2.77 days. The observed response does not arise because teachers support weaker students by assigning them extra homework; Table A.6 shows that the youngest and oldest children in a grade receive similar workloads.

Figure 2: Parental help with homework

(a) Reduced form

(b) Across countries (IV estimates)



Notes: Panel (a) illustrates the reduced-form relationship between parental help and expected relative age using pooled data from PIRLS 2006, PIRLS 2011, ECLS-K:1999 (grade 5), and ECLS-K:2011 (grade 5). We have residualized on school fixed effects before binning and plotting. Panel (b) plots country-specific estimates from instrumental variables specifications in which we use expected relative age as an instrument for standardized reading performance. All regressions include school fixed effects.

Panel (a) of Table 1 compares the OLS estimate of the relationship between parental responses and the reading proficiency of the child (column 1) with our main IV estimate (column 2). The OLS estimate is smaller in absolute value than the corresponding IV estimate. Parental compensation is thus stronger than revealed by the descriptive relationship possibly because highly skilled parents (in the unobserved sense) tend to invest more in their children and have children who do better in school. Furthermore, column (3) shows that the baseline IV estimate is invariant to the use of country fixed effects rather than school fixed effects. This is reassuring because one concern could be that we are removing an important dimension of parental investment behavior by including school fixed effects.

Table 1: Parental responses to variation in children's reading proficiency

	Panel (a): Full sample			Panel (b): US and Canada		
	(1)	(2)	(3)	(4)	(5)	(6)
Reading score (std.)	–0.238	–0.579	–0.547	–0.567	–0.529	–0.686
	(0.004)	(0.029)	(0.028)	(0.055)	(0.052)	(0.147)
Estimation type	OLS	IV	IV	IV	IV	IV
Fixed effects:						
School	✓	✓	–	✓	–	–
Country	–	–	✓	–	✓	✓
Database	–	–	✓	–	✓	✓
Quarter×Year of Birth	–	–	–	–	–	✓

Notes: We estimate all regressions using pooled data from PIRLS 2006, PIRLS 2011, ECLS-K:1999 (grade 5), and ECLS-K:2011 (grade 5). The number of observations is 251,596 for the full sample and 48,918 for the US and Canada. Standard errors are shown in parentheses and clustered by expected date of birth.

Not only is parental compensation strong on average, compensation is also pervasive across countries as well as across the distribution of family background. Figure 2b presents country-specific IV estimates for parents' help with homework.[25] There is significant compensatory behavior in three quarters of the countries in our data (p-value < 0.05), and there is no country where parents provide less homework help when their children's reading performance declines. Furthermore, Table 2 shows that parents respond similarly to changes in performance for students with different characteristics. Columns (1)–(4) reveal no significant differences by foreign background or gender of the child, although the estimates suggest slightly stronger responses among natives.

We find some evidence of heterogeneous compensatory behavior by students' socioeconomic background. Highly educated parents respond more strongly to their children's academic performance than less-educated parents (columns 5–6), although

[25]For the sake of comparability, we opt to present IV estimates rather than reduced forms. This is motivated by substantial variability in first-stage estimates across countries, e.g., due to different grade retention policies or differential enforcement of school entry rules. See Table A.7 for additional country-specific estimates.

the difference is notably smaller compared to findings in other studies (e.g., Berniell and Estrada, 2020; Fredriksson et al., 2016). However, since low-educated parents are much more likely to have children who struggle in school (see Figure A.2), parental investments are concentrated at the lower end of the performance distribution.

Table 2: Heterogeneity in parental help with homework

	Gender		Foreign background		Parental education	
	Girl	Boy	Native	Foreign	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Reading score (std.)	–0.618	–0.560	–0.602	–0.453	–0.704	–0.531
	(0.041)	(0.048)	(0.029)	(0.089)	(0.053)	(0.034)

Notes: All regressions include school fixed effects and are estimated using pooled data from PIRLS 2006, PIRLS 2011, ECLS-K:1999 (grade 5), and ECLS-K:2011 (grade 5). Parents are considered highly educated if they have a university degree. Students are considered to have a foreign background if they are exposed to a foreign language at home. The regression samples consist of 125,441 girls and 125,695 boys; 202,238 and 35,840 children with native and foreign backgrounds; and 75,238 and 164,055 children with high- and low-educated parents. Standard errors are shown in parentheses and clustered by expected date of birth.

The interpretation of the IV estimates relies on the assumption that parents react to changes in their children's performance rather than directly to their age. If this assumption holds, our IV approach should result in similar estimates when controlling for children's (expected) age. Estimating such a model poses an empirical challenge because it requires independent variation in relative age for given absolute age. Most countries in our data have a universal school entry rule, however, and children born on the same date thus have identical relative ages. Furthermore, in the few countries where SSA rules vary across regions, most cutoff dates are only a few months apart. Absolute and relative age are therefore highly correlated, which makes it is hard to disentangle the two effects.

With the aforementioned caveat in mind, panel (b) of Table 1 examines the plausibility of our identifying assumption by exploiting within-country variation in school entry rules in the US and Canada.[26] Intuitively, we compare parental responses for children who are born at the same point in time but have different relative age ranks

[26]In principle, there is variation in school entry age rules in Australia as well. However, 50 percent of parents do not complete the questionnaires, rendering these data less useful.

as a consequence of the SSA rule in their region.[27] To accomplish this, we replace the school fixed effects in our main model with country fixed effects, such that our identifying variation stems from differences in SSA rules across regions. Additionally, we control for absolute age by including fixed effects for children's expected date of birth (quarter-by-year).[28]

Column (4) of Table 1 shows that the IV estimate for the US and Canada is basically identical to the estimate for the full sample (c.f. column 2). The IV estimate does not change substantively when using country rather than school fixed effects (columns 4 vs. 5) or when adding controls for absolute age (column 5 vs. 6). We thus view the evidence in Table 1 as supporting the exclusion restriction. In section 4.3, we provide additional evidence on this point when studying the dynamics of parental responses over grade levels in US.

4.2 School responses

This section turns to the question of whether and how schools respond to variation in student performance. Conceptually, we think of schools as operating under a fixed budget constraint, and thus we examine whether there is compensatory or reinforcing resource allocation across students within schools.[29] To achieve this conceptual benchmark, we include school fixed effects. Note, however, that the point estimates are not affected by the inclusion of school fixed effects – it only makes the estimates more precise.

Figure 3 shows that schools make compensatory investments in children. Panel (a) reveals that class size increases with children's expected relative age rank. The implied IV estimate equals $\hat{\beta}^s = 0.05/0.24 = 0.22$, indicating that schools reduce class size by 0.22 students when achievement declines by one SD. This corresponds to about 12 percent of the within-school standard deviation in class size.[30]

[27] Since the school years starts in about the same time in all regions, children should have the same amount of schooling at the test date, even though the school starting age regulations differ.

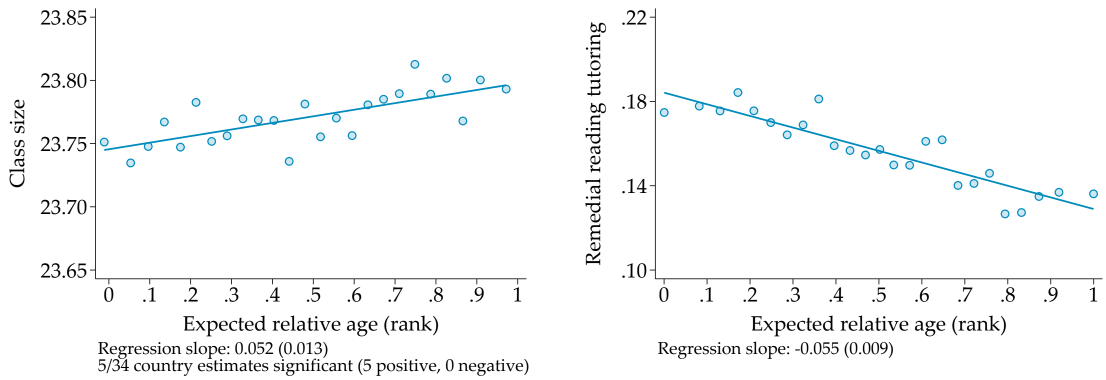
[28] We use expected date of birth rather than actual date of birth so that the results are not driven by children who repeated a grade, skipped a grade, or did not comply with the SSA rule at the time of primary school entry.

[29] See the theory section in the appendix.

[30] In many countries in our data, there is little to no variation in class size due to the sampling design in PIRLS. This limits our ability to compare estimates across countries. Note that the estimate for the US, where there is significant within school variation, is much larger. Class size increases one-for-one with pupil achievement in the US; see Figure 4.

Figure 3: School responses – reduced forms

(a) Class size (b) Remedial tutoring (US only)



Notes: Panel (a) depicts the reduced-form relationship between class size and expected relative age rank using pooled international data from PIRLS 2006, PIRLS 2011, ECLS-K:1999 (grade 5), and ECLS-K:2011 (grade 5). Panel (b) depicts the reduced-form relationship between remedial tutoring and expected relative age rank using US-based data from the fifth-grade wave of ECLS-K:1999 and ECLS-K:2011. In both panels, we have residualized on school fixed effects before binning and plotting.

Panel (b) of Figure 3 illustrates the reduced-form estimate for individual tutoring. At this stage of the analysis, we restrict attention to the US because information on this outcome does not exist in the international data. The figure shows that schools assign more tutoring to children who are young for their grade. The implied IV estimate indicates that a reduction in student performance by one SD increases the probability that children receive remedial tutoring by 24 percentage points (from a mean of 16 percentage points).

Table 3 summarizes the main results for school responses. Columns (2) and (4) present IV estimates of the relationship between school responses and the reading proficiency of the child. For comparison, columns (1) and (3) show the corresponding OLS estimates. The OLS estimates are uniformly smaller in absolute value compared to the IV estimates. School compensation is thus stronger than revealed by the descriptive relationship possibly because highly skilled parents (in the unobserved sense) can circumvent, e.g., class size allocations, at least to some extent.

Table 3: School responses to variation in child reading proficiency

	Class size (all countries)		Remedial tutoring (US only)	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Reading score (std.)	0.134	0.218	–0.140	–0.237
	(0.010)	(0.053)	(0.004)	(0.036)

Notes: The US data come from the fifth-grade waves of ECLS-K:1999 and ECLS-K:2011. Sample size is 271,690 for class size and 20,683 for remedial tutoring. Average class size is 23.772 with a standard deviation of 6.098. The share of children who receive remedial tutoring is 15.655%. All regressions include school fixed effects. Standard errors are shown in parentheses and clustered by expected date of birth.

4.3 Responses over grade levels

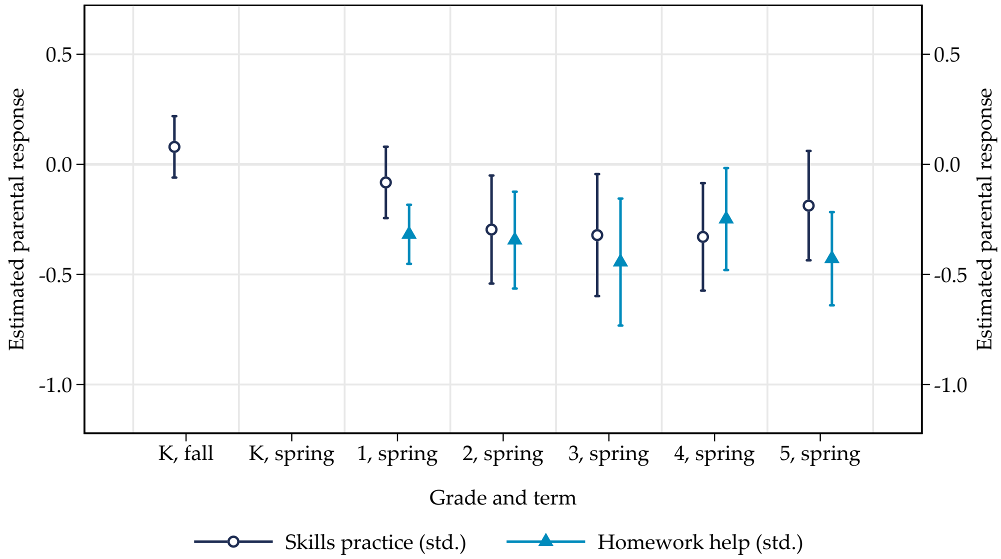
An interesting question is what information parents and schools respond to. In principle, the average performance variation generated by relative age ranks is predictable. Thus, one might expect parents and schools to act preemptively to counter the predicted disadvantage of the youngest children in the class. In this section, we shed light on this question by using longitudinal data from the US to examine parental and school responses over grade levels.

Figure 4 shows parental and school responses over grades. The top panel pertains to help with homework; in addition, we show how parents respond in terms of practicing basic skills (reading, writing, and numeracy) with their children – an outcome we observe from the start of kindergarten. The second panel pertains to school responses (class size and remedial tutoring – note that we have inverted the scale for class size to improve readability).

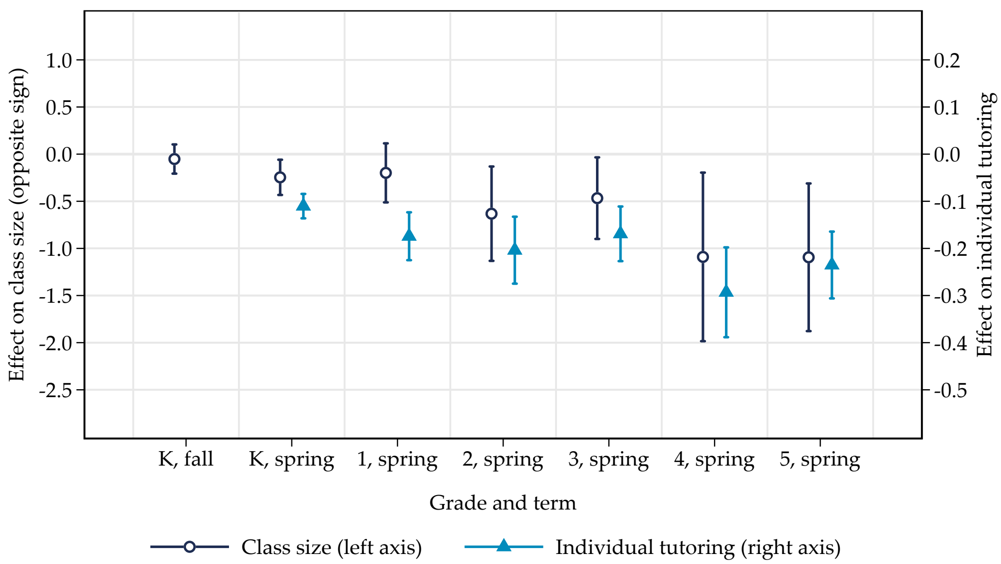
Panel (a) shows that parents do not respond at kindergarten entry: the estimate is not statistically significant and has an unexpected sign. After kindergarten, however, there is evidence of compensatory behavior. For instance, in the spring of second grade, parents respond to a one SD reduction in reading achievement by increasing basic skills practice, as well as homework help, by 0.3 of a standard deviation. Whereas parental responses are fairly stable throughout the remainder of primary school, panel (b) of Figure 4 demonstrates that the compensatory responses of schools grow over grades – from zero at kindergarten entry for class size to being

Figure 4: Parental and school responses over grade levels in the US

(a) Parental responses



(b) School responses



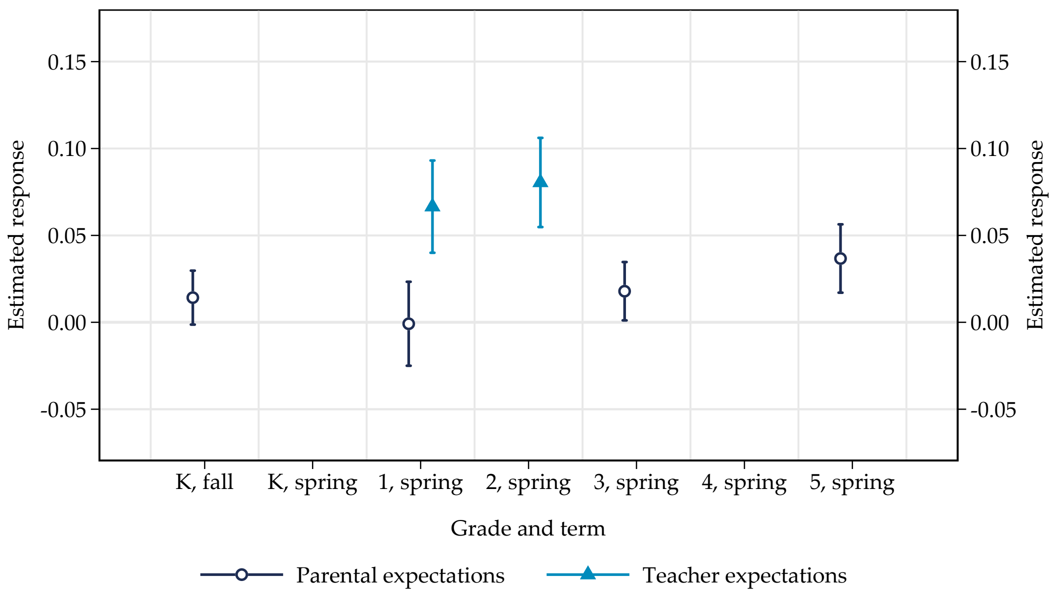
Notes: This figure shows the IV estimates over grade levels using pooled data from ECLS-K:1999 and ECLS-K:2011. All regressions include fixed effects for children's base-year school. The bars indicate 95% confidence intervals when standard errors are clustered by expected date of birth. To enhance readability, we have reversed the sign of the class size estimate.

quite substantive by the end of primary school. In fifth grade, a one SD reduction in performance leads to a 1.3 student reduction in class size and a 0.25 increase in the probability of receiving remedial tutoring.

What causes the variation over grades? One contributory factor may be that parents (and schools) do not have precise information about reading achievement in kindergarten. Indeed, previous research has shown that parents are partially uninformed about the progress of their children and that they change their investment behavior when new information arrives; see Dizon-Ross (2019).

Related to availability of information on performance, Figure 5 shows that the impact of relative age on parents' expectations regarding college education grows over grades.[31] This is consistent with parents updating their information on child performance over time in primary school.

Figure 5: Parental and teacher expectations over grade levels in the US



Notes: This figure shows the reduced-form estimates for parental and teacher expectations regarding college education over grade levels in the US. All regressions include fixed effects for children's base-year school. The bars correspond to 95% confidence intervals when standard errors are clustered by expected date of birth. Data for parental expectations come from ECLS-K:1999 and ECLS-K:2011. Data for teacher expectations come from ECLS-K:2011.

[31] In the appendix, we show that expected relative age has a positive impact on parents' expectations in fifth grade across the countries in PIRLS; see Figure A.3.

For completeness, Figure 5 also shows the reduced-form relationship between children's relative age and teacher expectations. Data is available only for first and second graders, making it difficult to assess how expectations evolve as students age. Interpreted literally, however, it seems that teachers are better informed than parents about the impact of relative age on performance.

Finally, let us return to the issue of the validity of the exclusion restriction. If parents and schools react directly to relative age, we would expect compensatory responses around the time of kindergarten entry. As documented in this section, however, parents do not make compensatory investments at kindergarten entry. It is thus difficult to reconcile the view that parents respond directly to age with the result that there is no response at kindergarten entry.

4.4 Parental responses in different contexts

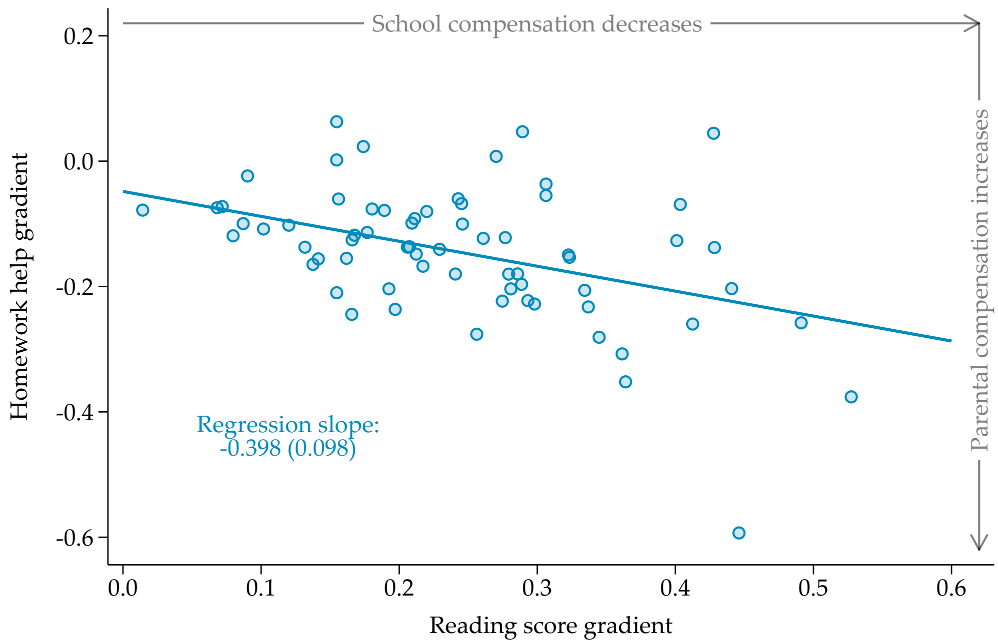
Intuitively, we might expect that parents are more responsive to student achievement in contexts where schools are less compensatory. This intuition is based on the assumption that public and private investments in children's learning are substitutes, which is what previous research suggests; see, for example, Fredriksson et al. (2016), Pop-Eleches and Urquiola (2013), and Cullen et al. (2006).

The challenge in testing this prediction is that we lack a comprehensive measurement of how compensatory schools are. To address this issue, we implement an approach akin to that of Chetty et al. (2014b) and Rothstein (2019). We use regional differences in the relationship between children's reading score and relative age rank to measure how compensatory schools are in a particular region. This approach relies on the premise that schools' compensatory investments reduce the performance gap between younger and older students, and thus, the reading gradient should be lower (higher) in regions where schools are more (less) compensatory.

We implement our method in three steps. First, we divide each country in our data into groups of urban and non-urban schools, hereafter referred to as regions.[32] Next, we estimate region-specific regressions for school compensation (measured by the reading gradient in equation 1) and parental compensation (measured by the homework help gradient in equation 2). All regressions include school fixed effects to

[32]We create regions using an indicator of school urbanicity because the data from most countries does not contain identifiers for administrative divisions, such as municipalities or provinces.

Figure 6: Parental responses in different contexts



Notes: Each dot shows the reduced-form estimates for one region, defined as a combination of country x school urbanicity. There are 67 observations – two per country, except in the case of Singapore, where there are no rural schools. When regressing the regional homework help gradient on the regional reading gradient, each observation is weighted by the inverse standard error of the reading score gradient in that region.

account for parental sorting across schools and urban areas. Finally, we correlate the resulting estimates, weighting by the inverse standard error of the reading gradient.

Figure 6 depicts the correlation between parental and school compensation across regions. Each point plots the reduced-form estimates for one regional subdivision.[33] The degree of school compensation decreases as one moves outward along the horizontal axis, whereas the degree of parental compensation increases as one moves down the vertical axis. The observed negative relationship, with an estimated slope of –0.40, thus indicates that parents compensate more in regions where schools com-

[33]We present robustness checks for this analysis in Figure A.4. Panel (a) shows that we obtain similar estimates if we drop the three nations that fail the balance test. Another concern is that the observed relationship between the reduced-form estimates may partly reflect differential compliance with school starting age rules across regions. Therefore, we have also estimated IV specifications in which we instrument children's actual age rank with their expected age rank; see panel (b). We find an estimate of –0.32, which confirms the negative relationship between parental and school compensation.

pensate less. This finding aligns with our prediction and suggests that parental and school investments are substitutes rather than complements.

5 Robustness checks

Figure A.5 provides several sensitivity checks of our main results. For instance, it shows that it does not matter which of the five plausible values we use for the reading score, or whether we give countries equal weight in the estimations: these changes have a negligible effect on our estimates and do not affect our main conclusions.

Figure A.6 provides additional sensitivity analyses to support our argument that children's reading scores can be thought of as a general performance measure. These sensitivity analyses are possible because children in the ECLS-K studies were assessed in mathematics and science in addition to reading. Moreover, a subset of countries that participated in PIRLS 2011 also participated in TIMMS 2011, an international mathematics and science assessment; thus, we have scores for all three test domains for a subsample of our data. The figure shows that the main IV estimates for parental help are not sensitive to using the mathematics or science assessments as the first-stage performance measure instead the reading test.

In a similar vein, Figure A.7 provides evidence that our results on parental responses are not driven by our focus on help with reading homework rather than parental help with other important subjects, such as mathematics. The analysis in the figure is based on the subsample of data in which parents provide answers about the extent to which they help with homework or skills practice in a specific subject (e.g., help with reading vs. math homework or practicing reading vs. math skills). Using data from the US, the left side of the figure shows that the IV estimates for help with math homework are similar – albeit slightly larger in absolute magnitude – than those for help with reading homework. The right side of the figure uses international data and also reveals somewhat stronger – though not statistically different – compensatory responses for skills practice in math compared to skills practice in reading.

As a next robustness check, Table A.8 shows that our findings hold when using alternative measures of parental responses. To facilitate comparison across definitions, the first column of panel (a) replicates our main point estimate from Table 1. In this specification, our outcome variable is the extent to which parents help their children with homework, and we have standardized the survey answers by country and database. The remaining columns of the panel show that we still find significant compensatory responses if we instead normalize the survey responses to a 0–1 scale (second column), convert the possible survey responses to number of days per week (third column), or create a binary indicator for helping children every day or almost every day (fourth column).

Finally, panels (b) through (e) of Table A.8 show that our findings hold when studying other dimensions of parental time investment besides help with homework. We find significant compensatory responses for the frequency of skills practice (panel b), homework monitoring (panel c), discussion about schoolwork (panel d), and discussion about reading (panel e). However, the magnitude of the response is noticeably stronger for activities that have the most direct input into children's schoolwork, such as the frequency of homework help and skills practice.

6 Conclusions

This paper provides the first international evidence on whether parents and schools invest in children's learning in ways that reinforce or compensate for academic performance. Using data on fourth and fifth graders in 34 countries, we exploit age-based school entry rules and show that children who are relatively young for their school cohort underperform relative to their older peers. Both parents and schools respond to children's poorer academic performance in a compensatory way. Parents invest additional time helping with schoolwork and practicing basic skills with their children, while schools place children in smaller classes and provide remedial tutoring to a greater extent.

To our knowledge, we are the first to show that compensatory parental investments are a pervasive phenomenon, with significant effects in a wide variety of education systems around the world. We do not find differential responses by children's gender or foreign background, although high-educated parents compensate to a slightly larger extent than low-educated parents. Additionally, we provide suggestive evidence that parents' compensatory responses are stronger in less compensatory educational contexts. This aligns with previous studies finding that parental and school investments are substitutes.

We also shed some light on the mechanisms, using longitudinal data from the US to examine when compensatory responses emerge and whether they change across grade levels. We find that parental responses are fairly stable from first through fifth grade. Interestingly, however, parents do not compensate for worse academic performance around the time of kindergarten entry. One explanation may be that parents underestimate the magnitude or persistence of relative age effects; in that case, better information could lead them to provide compensatory support at young ages – a time when such investments may have particularly beneficial effects.

Finally, schools do not appear to compensate for achievement disadvantages at kindergarten entry. School compensation then grows over grade levels, particularly for class size. These dynamics provide insight into why the effects of educational interventions may fade out over grade levels.

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A. Main appendix

A.1 Theoretical framework

We present a stylized theoretical framework that outlines the trade-offs facing parents and schools when investing in children's human capital.

A.1.1 Human capital production function

The human capital of the child is given by

$$H_i = g(\theta, a)h(R)$$

where

$$R = [\mu p^{\phi} + (1-\mu)s^{\phi}]^{1/\phi}$$

and $h_R > 0$, $h_{RR} < 0$. Here p denotes parental investment, s school investment, θ the child's innate ability, and a the child's relative age. The parameter $\phi \leq 1$ governs the elasticity of substitution. The relative age of the child matters because children are assumed to be graded on a scale (at least partially). Children who are relatively older at the evaluation point obtain higher grades on average. They are thus able to progress in the education system to a greater extent, and, ultimately, obtain higher educational attainment. The variation in the age rank is treated as idiosyncratic by the parents, conditional on age – i.e., we suppress the dependence on age per se.

A.1.2 Parental behavior

Parents have preferences defined over their own consumption (c^p) and their children's long-run consumption opportunities (i.e., income – $y = wH$):

$$U_i^p = U(c^p, y) = \ln c^p + \frac{y^{1-\gamma} - 1}{1-\gamma}$$

The budget constraint is given by $c^p = y^p - w^p p$. Parents thus supply a fixed amount of labor, they do not leave bequests, and the price of investing in p is w^p. Parents maximize U_i^p subject to the budget constraint while taking school resources as given.

Optimal investments are given by

$$\frac{w}{w^p}\frac{\partial H}{\partial p} = MRS(c^p, y)$$

where MRS denotes the marginal rate of substitution between the parents' own consumption and the child's consumption opportunities. Without loss of generality, we assume $w = w^p$. Furthermore, $h(R) = R^\rho$, $\rho \leq 1$. We can then write the first order condition as

$$\psi(p, s, a) \equiv \rho\kappa(p, s)\frac{H(a, p, s)}{p} - MRS(y^p - wp, wH(a, p, s)) = 0$$

where

$$\kappa = \frac{\mu p^\phi}{\mu p^\phi + (1-\mu)s^\phi},\ \kappa \in [0, 1]$$

Notice that favorable news about the child's human capital increases the return to investing in p, given the structure we have imposed on the education production function. Thus, $(\partial^2 H/\partial p \partial a) > 0$. When the parent perceives that the child has become richer, the MRS between their own consumption and the child's consumption increases, however. This mechanism pulls parental investments in the opposite direction.

By the implicit function theorem, we have

$$\frac{\partial p}{\partial x} = -\frac{\psi_x}{\psi_p}$$

where $x = \{s, a\}$. Since $\psi_p < 0$, the sign of this derivative is the same as the sign of ψ_x. We have

$$\frac{\partial p}{\partial a} = -\frac{\gamma - 1}{1 - \phi(1-\kappa) + (\gamma-1)\rho\kappa + \frac{wp}{c^p}}\frac{p}{H} \Rightarrow \mathsf{sign}\{\frac{\partial p}{\partial a}\} = -\mathsf{sign}\{\gamma - 1\}$$

Figure 2 in the main text presents evidence that $(\partial p/\partial a) < 0$. In terms of the model, this means $\gamma > 1$. The marginal utility of the child's income potential thus has to fall sufficiently for parents to reduce investments in their children. When $\gamma < 1$, the efficiency motive for investing in children is stronger than decreasing marginal utility.

$$\frac{\partial p}{\partial s}\frac{s}{p} = -\frac{(1-\kappa)[\phi + (\gamma-1)\rho]}{1 - \phi(1-\kappa) + (\gamma-1)\rho\kappa + \frac{wp}{c^p}} \Rightarrow \mathsf{sign}\{\frac{\partial p}{\partial s}\} = -\mathsf{sign}\{\phi + (\gamma-1)\rho\}$$

Given $\gamma > 1$, this expression is unambiguously negative if $\phi > 0$, i.e., if private and public investments are substitutes. The evidence in Fredriksson et al. (2016) is consistent with $\phi > 0$.

A.1.3 School behavior

Schools care about aggregate human capital as well as the distribution across the children in the school. We take the school objective function to be

$$U^s = (\sum H_i^\lambda)^{1/\lambda}$$

where the CES parameter λ indexes how much the school cares about efficiency relative to inequality. If $\lambda = 1$, the school only cares about aggregate human capital; if $\lambda \to -\infty$, it cares only about equity.[1]

The school operates under a given budget constraint and thus only decides how to allocate resources across the student population: $\sum s_i = \bar{s}$. The optimal allocation of resources across students is governed by

$$\frac{s_i}{s_j}\frac{1-\kappa_j}{1-\kappa_i} = \left(\frac{H_i}{H_j}\right)^\lambda$$

where κ is defined in the equation in section A.1.2.

Suppose there is an innovation in a for child i. Whether this causes a reallocation of resources away from the child, depends on the relative strength of efficiency and equity concerns in the school objective function – i.e., on the parameter λ. An innovation in a_i has two effects. First, it implies that it is more efficient to invest in child i because of the complementarity between a and R in the human capital production function. Second, it creates an incentive for schools to reallocate resources to other children, since the school values equality. When $\lambda = 0$, these two motives exactly balance, and the allocation of resources does not change in response to a change in performance. Thus

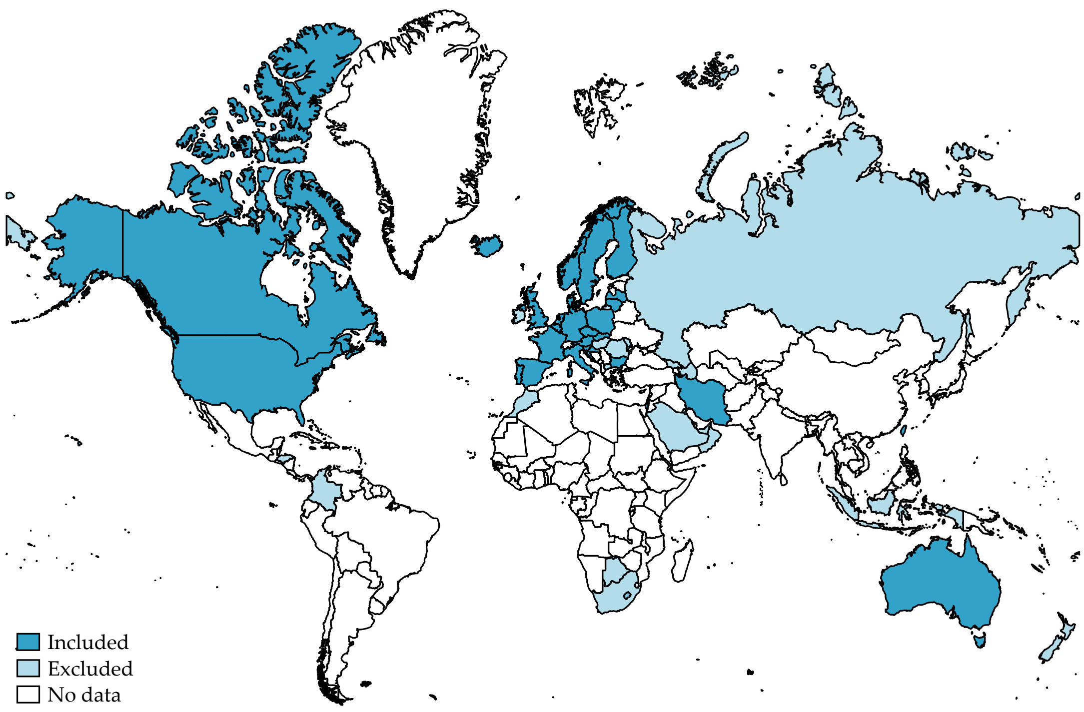
$$\mathsf{sign}\left\{\frac{\partial s}{\partial a}\right\} = \mathsf{sign}\left\{\lambda\right\}$$

[1]We do not explicitly model where these preferences come from, but we think of them as being generated from an initial state where parents agree on the objective function under the veil of ignorance.

Schools must care sufficiently about equality for there to be a compensatory reduction in resources in response to an innovation in the age rank.

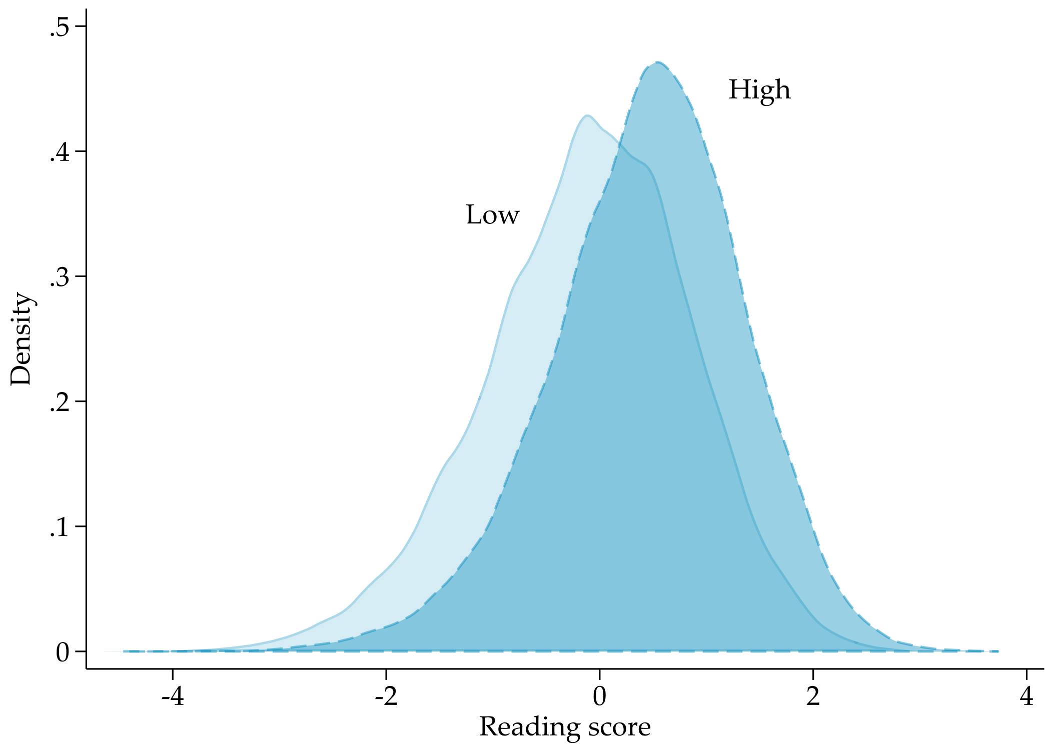
A.2 Figures

Figure A.1: World map illustrating coverage of our data



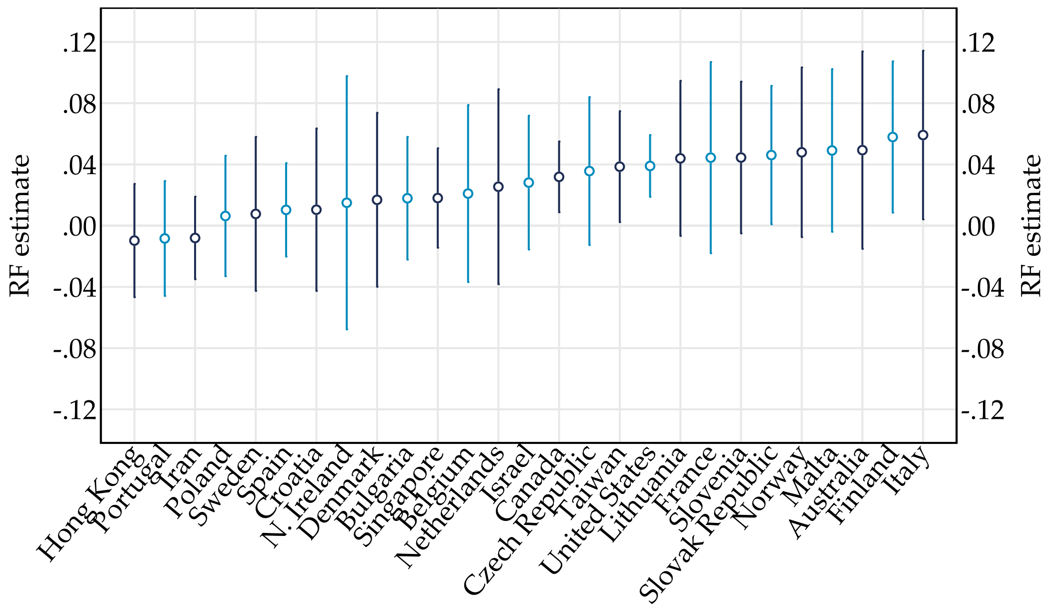
Notes: This figure shows the countries that we include in our estimation sample (darker shading) or exclude due to unclear school starting age rules (lighter shading). Non-shaded countries did not participate in PIRLS 2006 or PIRLS 2011.

Figure A.2: Reading score distribution by parental education



Notes: This figure shows the distribution of standardized reading scores for children with low-educated parents (lighter shading) and high-educated parents (darker shading). We define a child as having highly educated parents if either the mother or father holds a college degree. On average, children with high-educated parents score 0.56 standard deviations higher than children with low-educated parents.

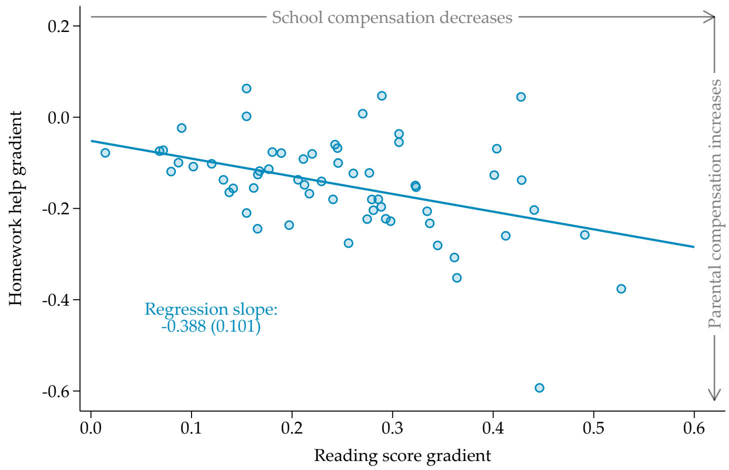
Figure A.3: Parents' educational expectations across countries



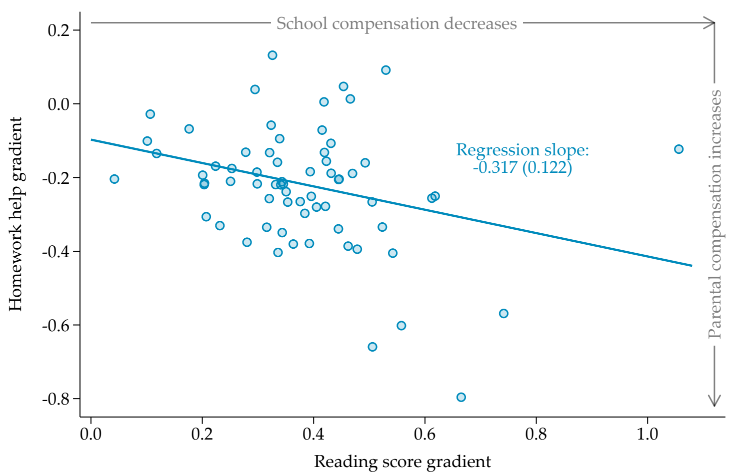
Notes: This figure shows the reduced-form relationship between children's expected relative age rank and parents' educational expectations. The outcome variable is an indicator equal to one if the parent expects that their child will complete a college degree. Data comes from PIRLS 2011 and the fifth-grade waves of ECLS-K:1999 and 2011. All regressions include school fixed effects. Standard errors are clustered by expected date of birth.

Figure A.4: Robustness checks for parental responses in different contexts

(a) RF estimates for balanced countries

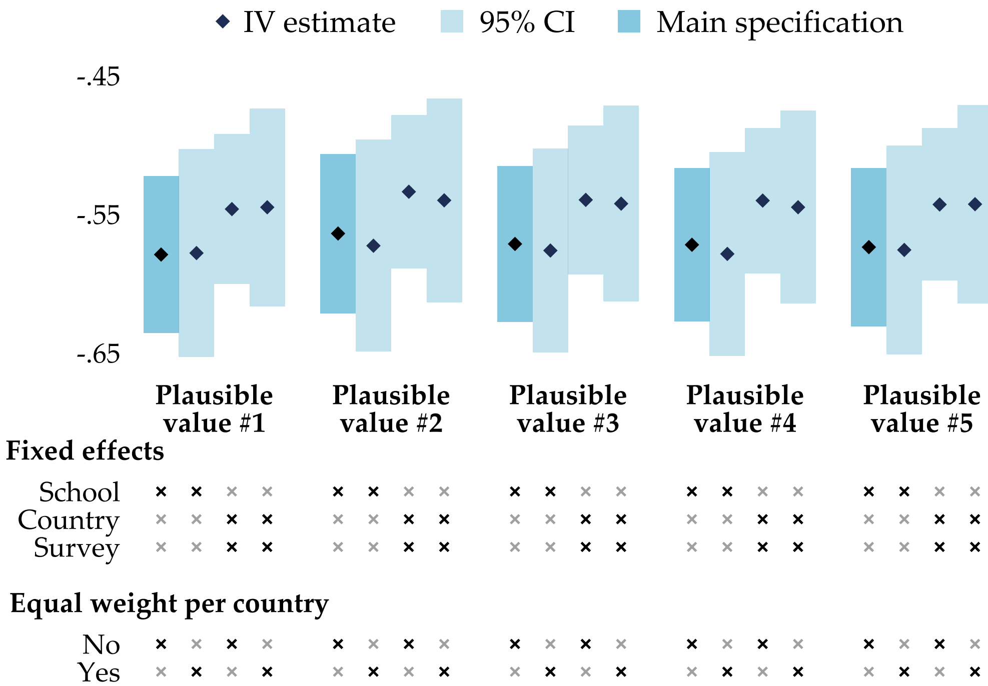


(b) IV estimates for all countries



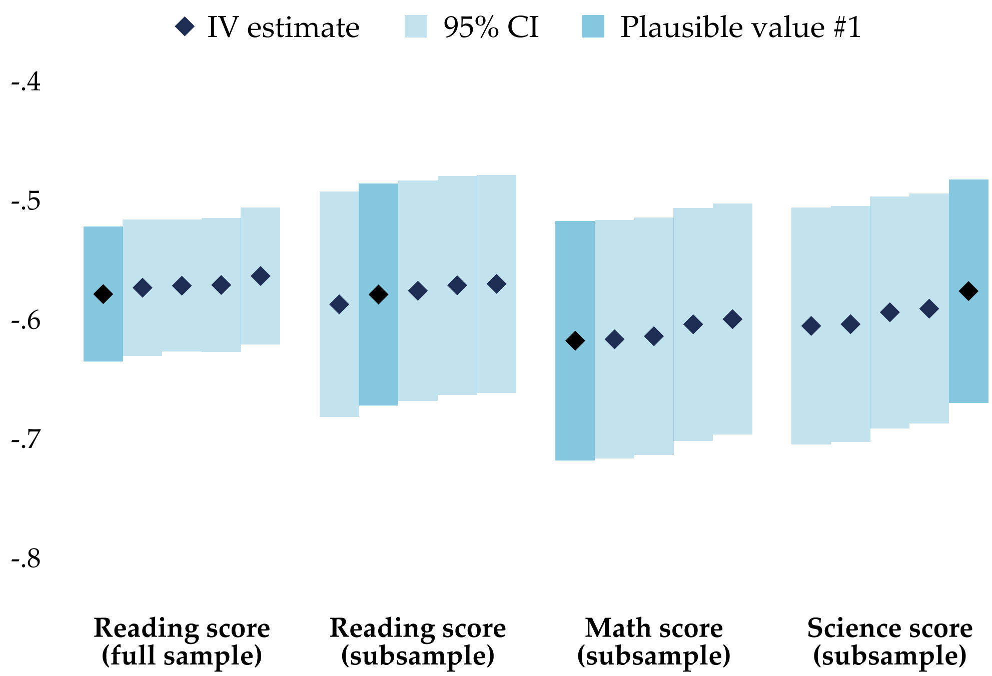
Notes: Panel (a) repeats the analysis shown in Figure 6 after dropping the three nations that fail the balance test. The reported slope comes from a regression of the regional homework help gradient on the regional reading gradient. There are 62 observations (two per country). Each observation is weighted by the inverse standard error of the reading score gradient. Panel (b) repeats the analysis shown in Figure 6 using an IV specification in which children's expected age rank is an instrument for their actual age rank. Each dot shows the IV estimates for one region, defined as a combination of country x school urbanicity. The reported slope comes from a regression of the regional homework help gradient on the regional reading gradient. There are 67 observations. Each observation is weighted by the inverse standard error of the reading score gradient.

Figure A.5: Robustness checks for parental homework help (std.) using different specifications and plausible values



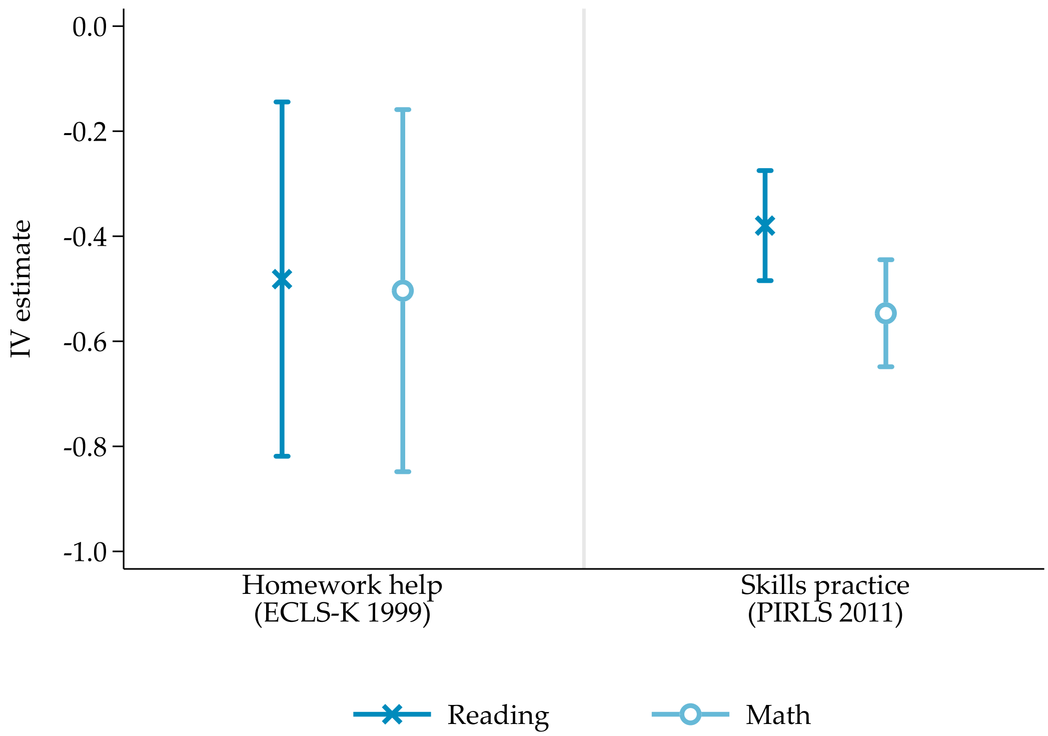
Notes: The figure plots the IV estimates and corresponding 95% confidence intervals from various regression specifications in which we use students' expected age rank as an instrument for one of the five plausible values of the PIRLS reading score. In the ECLS-K data, there is only one value for the reading score, and thus, this value is used for children in the US across all specifications. Standard errors are clustered by expected date of birth.

Figure A.6: Robustness checks for parental homework help (std.) using different test domains in the first stage



Notes: The figure plots the IV estimates and corresponding 95% confidence intervals from regressions in which we use students' expected age rank as an instrument for one of the five plausible values from the various subject tests in PIRLS, TIMMS, and ECLS-K. In the ECLS-K data, there is only one value for each subject, and thus, this value is used for children in the US across all specifications. The "full sample" refers to our main estimation sample. The "subsample" refers to the sample of students tested in all three subjects. Data from PIRLS 2006 is excluded from the subsample because TIMMS was not administered that year. Only a subset of data from PIRLS 2011 is included because not all countries participated in both TIMMS and PIRLS in 2011.

Figure A.7: Subject-specific parental responses (std.)



Notes: The figure plots the IV estimates and corresponding 95% confidence intervals from regressions in which we use students' expected age rank as an instrument for their reading score (darker bars) or math score (lighter bars). In the left panel, the outcome is the frequency of homework help in reading, language arts, or spelling (darker shading) or homework help in math (lighter shading). In the right panel, the outcome is either the frequency of reading practice (darker shading) or practice with math skills (lighter shading). We cannot use data from PIRLS 2006 or ECLS-K:2011 because there were no subject-specific questions about homework help and skills practice.

A.3 Tables

Table A.1: Number of observations and survey response rates by country

		Survey response rate	
Country	Obs.	Parents	Teachers
All	287,721	0.875	0.944
Australia	5,059	0.536	0.681
Austria	9,737	0.941	0.988
Belgium	12,754	0.916	0.964
Bulgaria	9,111	0.964	0.979
Canada	35,733	0.838	0.923
Croatia	4,587	0.986	0.972
Czech Republic	4,553	0.967	0.996
Denmark	8,595	0.933	0.979
England	4,033	0.460	0.930
Finland	4,640	0.948	0.962
France	8,839	0.914	0.977
Germany	7,899	0.855	0.944
Hong Kong	8,585	0.943	0.986
Iceland	5,052	0.730	0.874
Iran	5,755	0.975	1.000
Israel	4,186	0.777	0.870
Italy	7,769	0.931	0.979
Latvia	4,160	0.935	0.977
Lithuania	9,361	0.954	0.995
Luxembourg	5,101	0.915	0.992
Malta	3,596	0.904	0.973
Netherlands	8,151	0.617	0.914
Northern Ireland	3,586	0.583	0.834
Norway	8,834	0.906	0.831
Poland	9,859	0.970	0.956
Portugal	4,085	0.946	0.954
Scotland	3,775	0.514	0.898
Singapore	6,367	0.970	0.979
Slovak Republic	10,992	0.966	0.984
Slovenia	9,848	0.948	0.992
Spain	12,631	0.824	0.952
Sweden	9,013	0.891	0.857
Taiwan	8,882	0.970	0.989
United States	22,593	0.841	0.935

Notes: The parental response rate is measured for the homework help question. The teacher response rate is measured for the class size question. For the US data, we report the number of observations and the response rates in the subsample of children who completed the fifth grade reading assessment.

Table A.2: Descriptive statistics for the different samples

	International	*United States data by grade level*						
	Grade 4–5†	K, fall	K, spring	Grade 1	Grade 2‡	Grade 3	Grade 4‡	Grade 5
		Panel (a): Parental and school responses						
Homework help (days/week)	2.77	n.a.	n.a.	3.46	3.44	3.30	2.78	2.32
	(2.05)	n.a.	n.a.	(1.89)	(1.83)	(1.96)	(1.80)	(1.78)
Skills practice (days/week)	2.52	5.79	n.a.	5.42	4.87	5.18	3.76	3.27
	(1.90)	(1.70)	n.a.	(1.88)	(1.99)	(2.09)	(2.36)	(2.42)
Class size	23.77	20.30	20.35	20.93	21.47	21.69	22.90	23.04
	(6.10)	(4.46)	(4.51)	(4.30)	(4.36)	(4.48)	(5.47)	(5.85)
Remedial tutoring	n.a.	n.a.	0.09	0.15	0.20	0.19	0.26	0.16
		Panel (b): Children's background characteristics						
Expected age at test	10.21	5.53	6.03	7.08	8.04	9.03	10.04	11.00
	(0.53)	(0.32)	(0.32)	(0.32)	(0.31)	(0.32)	(0.31)	(0.32)
Actual age at test	10.29	5.59	6.09	7.14	8.10	9.09	10.10	11.06
	(0.60)	(0.37)	(0.37)	(0.36)	(0.37)	(0.36)	(0.37)	(0.36)
Girl	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49
Foreign	0.16	0.13	0.15	0.17	0.22	0.18	0.23	0.19
High-educated parents	0.32	0.28	0.28	0.29	0.40	0.29	0.40	0.30

Notes: We convert the categorical responses for homework help and skills practice to days-per-week measures by using the midpoint of each category. See the data appendix for additional details on our variable definitions. Table A.3 shows the number of observations in each cell. †In the international sample, just under 90% of children are in fourth grade at the time of assessment. Several countries administer the PIRLS test in third or fifth grade, and we use fifth-grade data from the US due to data availability. ‡There is no data from ECLS-K:1999 in second or fourth grade.

Table A.3: Number of observations in the different samples

	International	*United States data by grade level*						
	Grade 4–5†	K, fall	K, spring	Grade 1	Grade 2‡	Grade 3	Grade 4‡	Grade 5
		Panel (a): Parental and school responses						
Homework help	251,596	n.a.	n.a.	26,358	11,348	11,930	9,658	18,965
Skills practice	133,776	12,904	n.a.	14,602	11,341	12,291	9,734	8,980
Class size	271,690	30,479	31,768	27,701	12,411	23,217	10,935	21,090
Remedial tutoring	n.a.	n.a.	33,747	27,837	12,463	23,067	10,872	20,693
		Panel (b): Children's background characteristics						
Expected age at test	287,721	33,281	36,112	31,443	13,833	27,141	12,071	22,690
Actual age at test	287,721	33,281	36,112	31,443	13,833	27,141	12,071	22,690
Girl	287,721	33,248	36,082	31,422	13,815	27,125	12,057	22,678
Foreign	246,136	31,576	34,221	30,015	13,306	25,986	11,670	21,814
High-educated parents	246,507	31,403	34,112	29,926	13,135	25,925	11,537	21,756

Notes: Each cell of the table reports the number of observations used to compute the descriptive statistics in the corresponding cell in Table A.2. †In the international sample, just under 90% of children are in fourth grade at the time of assessment. Several countries administer the PIRLS test in third or fifth grade, and we use fifth-grade data from the US due to data availability. ‡There is no data from ECLS-K:1999 in second or fourth grade.

Table A.4: Average school characteristics

Variable	Estimate	Std. Error	Average	Obs.
School enrollment (total)	1.029	2.342	514.661	267,225
Urban school	–0.008	0.006	0.356	269,545
Share low SES students in school	–0.001	0.002	0.241	259,914
Class size	0.034	0.081	23.779	278,321
Teacher's years of experience	0.025	0.089	17.290	278,693
Age when tested	0.031	0.004	10.294	287,721
Girl	0.001	0.001	0.493	287,721
Foreign background	–0.001	0.003	0.166	286,562
High-educated parent(s)	0.001	0.001	0.318	286,586
Parent(s) in a professional job	0.003	0.002	0.412	264,027
Reading score (std.)	0.024	0.004	0.001	287,721

Notes: The regressions are estimated using pooled international data from PIRLS 2006 and 2011 and the fifth-grade waves of ECLS-K:1999 and 2011. All regressions include fixed effects for database and country. Standard errors are clustered by expected date of birth.

Table A.5: Robustness check – dropping countries that fail the balance test

	Homework help		Class size	
	All countries (1)	Balanced countries (2)	All countries (3)	Balanced countries (4)
IV estimate	–0.579	–0.580	0.218	0.260
	(0.029)	(0.030)	(0.053)	(0.054)
Mean	0.009	0.010	23.772	23.153
Observations	251,596	232,799	271,690	252,534

Notes: Columns (1) and (3) report our main IV estimates. Columns (2) and (4) report the corresponding IV estimates after dropping the three nations (Croatia, Hong Kong, and Singapore) that fail the balance test in Figure 1b. All regressions include school fixed effects. Standard errors are shown in parentheses and clustered by expected date of birth.

Table A.6: Amount of homework assigned by the teacher

	Assigned any reading HW		Frequency (days/week)		Expected time (min/assignment)	
	(1)	(2)	(3)	(4)	(5)	(6)
Expected age rank	0.002	0.002	–0.007	0.006	–0.061	0.008
	(0.001)	(0.001)	(0.011)	(0.005)	(0.120)	(0.045)
Mean	0.958		2.943		22.273	
Observations	268,768		258,300		271,452	
Fixed effects:						
Country	✓	–	✓	–	✓	–
Database	✓	–	✓	–	✓	–
School	–	✓	–	✓	–	✓

Notes: All regressions control for grade level and are estimated using pooled international data from PIRLS and ECLS-K. Standard errors are shown in parentheses and clustered by expected date of birth. The data come from class-level teacher questionnaires. Questions vary slightly across surveys. The sample size drops in columns (3)–(4) because the teacher survey in ECLS-K:1999 did not include a question on assigned days of homework per week.

Table A.7: Country-level estimates for parental help with homework

Country	Obs.	First stage	Reduced form	IV specification
All	251,596	0.239 (0.008)	–0.138 (0.007)	–0.579 (0.029)
Australia	2,711	0.251 (0.063)	–0.006 (0.067)	–0.023 (0.267)
Austria	9,163	0.125 (0.025)	–0.053 (0.025)	–0.429 (0.180)
Belgium	11,687	0.227 (0.029)	–0.144 (0.023)	–0.635 (0.130)
Bulgaria	8,785	0.065 (0.025)	–0.083 (0.034)	–1.272 (0.721)
Canada	29,954	0.287 (0.017)	–0.182 (0.017)	–0.635 (0.061)
Croatia	4,521	0.187 (0.049)	–0.160 (0.052)	–0.854 (0.326)
Czech Republic	4,405	0.163 (0.051)	–0.164 (0.060)	–1.009 (0.393)
Denmark	8,016	0.071 (0.031)	–0.098 (0.037)	–1.388 (0.620)
England	1,853	0.311 (0.051)	–0.071 (0.045)	–0.227 (0.159)
Finland	4,400	0.356 (0.053)	–0.236 (0.054)	–0.664 (0.152)
France	8,081	0.267 (0.033)	–0.206 (0.040)	–0.772 (0.172)
Germany	6,756	0.169 (0.032)	–0.104 (0.046)	–0.612 (0.250)
Hong Kong	8,099	0.223 (0.027)	–0.115 (0.038)	–0.515 (0.177)
Iceland	3,686	0.430 (0.051)	–0.164 (0.064)	–0.382 (0.119)
Iran	5,613	0.165 (0.037)	–0.167 (0.045)	–1.012 (0.352)
Israel	3,253	0.210 (0.050)	–0.182 (0.062)	–0.863 (0.329)
Italy	7,232	0.258 (0.039)	–0.107 (0.041)	–0.414 (0.153)
Latvia	3,889	0.186 (0.051)	0.001 (0.060)	0.003 (0.322)
Lithuania	8,926	0.237 (0.045)	–0.068 (0.037)	–0.287 (0.128)
Luxembourg	4,669	0.167 (0.047)	–0.124 (0.040)	–0.743 (0.232)
Malta	3,251	0.209 (0.058)	–0.109 (0.068)	–0.522 (0.324)
Netherlands	5,032	0.169 (0.041)	–0.103 (0.044)	–0.608 (0.221)
Northern Ireland	2,091	0.231 (0.071)	–0.337 (0.081)	–1.461 (0.513)
Norway	8,006	0.357 (0.033)	–0.201 (0.032)	–0.563 (0.095)
Poland	9,566	0.278 (0.026)	–0.158 (0.038)	–0.567 (0.136)
Portugal	3,864	0.270 (0.049)	–0.153 (0.054)	–0.566 (0.229)
Scotland	1,937	0.423 (0.091)	–0.265 (0.075)	–0.627 (0.226)
Singapore	6,177	0.152 (0.042)	–0.060 (0.045)	–0.397 (0.299)
Slovak Republic	10,619	0.165 (0.032)	–0.139 (0.031)	–0.841 (0.234)
Slovenia	9,336	0.249 (0.036)	–0.191 (0.030)	–0.769 (0.128)
Spain	10,410	0.349 (0.031)	–0.258 (0.038)	–0.739 (0.109)
Sweden	8,030	0.295 (0.037)	–0.195 (0.033)	–0.661 (0.130)
Taiwan	8,613	0.409 (0.040)	–0.000 (0.031)	–0.000 (0.075)
United States	18,965	0.235 (0.024)	–0.101 (0.026)	–0.428 (0.108)

Notes: The first-stage column presents estimates of γ from equation (1), and the reduced-form column presents estimates of π^p from equation (2). US estimates are based on data from the fifth-grade waves of ECLS-K:1999 and 2011. All other estimates are based on data from PIRLS 2006 and 2011. The regressions include school fixed effects. Standard errors are reported in parentheses and clustered by expected date of birth.

Table A.8: Different measures of parental inputs (IV estimates)

	Standardized by country	Normalized (0–1 scale)	Number of days/week	(Almost) every day
Panel (a): Frequency of help with homework				
Reading score (std.)	–0.579	–0.172	–1.037	–0.219
	(0.029)	(0.008)	(0.052)	(0.013)
Mean	0.009	0.698	2.774	0.432
Observations	251,596	251,596	251,596	251,596
Panel (b): Frequency of skills practice				
Reading score (std.)	–0.503	–0.147	–0.731	–0.105
	(0.041)	(0.012)	(0.073)	(0.018)
Mean	0.008	0.650	2.520	0.279
Observations	133,776	133,776	133,776	133,776
Panel (c): Frequency of homework monitoring				
Reading score (std.)	–0.373	–0.088	–0.544	–0.121
	(0.042)	(0.010)	(0.072)	(0.018)
Mean	0.007	0.876	4.007	0.746
Observations	134,344	134,344	134,344	134,344
Panel (d): Frequency of discussion about schoolwork				
Reading score (std.)	–0.126	–0.026	–0.198	–0.049
	(0.033)	(0.008)	(0.053)	(0.013)
Mean	0.013	0.810	3.439	0.599
Observations	232,861	232,861	232,861	232,861
Panel (e): Frequency of discussion about reading				
Reading score (std.)	–0.116	–0.031	–0.153	–0.027
	(0.032)	(0.009)	(0.056)	(0.014)
Mean	0.011	0.709	2.556	0.368
Observations	232,754	232,754	232,754	232,754

Notes: When possible, the robustness checks are estimated using pooled international data from multiple waves of PIRLS and ECLS-K. Due to data availability, panels (b) and (c) include data only from the 2011 studies. In column (1), parents' responses are standardized by database, country, and grade level. In column (2), the raw data is normalized to a 0–1 scale. In column (3), categorical responses are converted to days per week using the midpoint of the bin. In column (4), the outcome is a binary indicator equal to one if the parent reports doing the activity (almost) every day. The survey questions vary slightly across databases; see the data appendix for details.

B. Data appendix

Our study relies on public-use data from the International Association for the Evaluation of Educational Achievement (IEA) and the United States' National Center for Education Statistics (NCES).[1] In this data appendix, we provide a comprehensive description of the databases, our sample restrictions, and the variables used in our analysis.

B.1 Data sources

Data for all countries except the United States come from PIRLS, the Progress In International Reading Literacy Study. PIRLS is an internationally standardized reading assessment targeted at students who have completed four years of primary education and who are, on average, around 10 years old.[2] The IEA has administered the assessment every five years since 2001. Our analysis focuses on the 2006 and 2011 waves due to data limitations in the 2001, 2016, and 2021 waves.[3]

Although the US participated in PIRLS 2006 and 2011, the data available from the US are not suitable for our study. For example, we cannot analyze parental responses because the US opted not to administer the Home Questionnaire to the parents of participating students. To include the US in our analysis, we use another data source that contains comparable information on children and their parents, teachers, and schools.

The US data come from the Early Childhood Longitudinal Studies (ECLS) program, which is run by the NCES. The program includes two longitudinal studies, each of which tracked a nationally representative sample of students from kindergarten age through the end of primary school: ECLS-K:1999 (kindergarten cohort 1998/99) and ECLS-K:2011 (kindergarten cohort 2010/11).

[1] The data is publicly available through the IEA Data Repository and Early Childhood Longitudinal Studies (ECLS) Program – Data Products.

[2] In a few countries, students are assessed in their fifth year of schooling to ensure a minimum average age of 9.5 years old. This applies to students in England, Malta, New Zealand, and Trinidad and Tobego.

[3] In the 2001 wave, the Home Questionnaire did not ask about the extent to which parents help their children with homework or schoolwork. Thus, we cannot study our main parental response of interest using the data. Additionally, the public-use files for the 2016 and 2021 waves lack information on children's date of birth and the date of their reading assessment. This omission precludes us from deriving students' expected relative age, which is a crucial element of our identification strategy.

B.2 School-starting-age rules

Our empirical approach relies on school-starting-age (SSA) rules that stipulate the age at which children are old enough to begin formal schooling. These rules usually specify an exact cutoff date by which children must reach a certain age in order to enter primary school. In most countries, legislation on school-starting age is set at the national level, and the same cutoff date applies to all children in the country. There are, however, some countries where regulations are set at the local level, and the cutoff date can vary across provinces, states, or even school districts.

In this section, we discuss the four countries in our sample that have region-specific SSA rules and explain how we determine the relevant rule for children in those countries.[4] We list the national SSA rules for the other countries in our sample in Table B.1. All information was obtained from questionnaires completed by PIRLS National Research Coordinators. If the rule reported by the Coordinator was ambiguous or not verifiable in the data, we excluded the region from our sample. Table B.2 summarizes our exclusion criteria and the regions dropped under each criterion.

Australia Each Australian state and territory sets its own policy on the age at which children begin primary school. Table B.3 provides an overview of the rules. In the PIRLS data, we identify the state or territory where children attend school – and thus the relevant SSA rule – through the explicit stratification variable. The strata codes are provided in the PIRLS 2011 User Guide (see Supplement 4, Exhibit S4.1 in Foy and Drucker, eds, 2013).

Canada The thirteen provinces and territories in Canada manage their own school system and set their own SSA rules. Table B.4 lists the rules for the nine provinces that participated in PIRLS 2006 and/or 2011. Most participating provinces stipulate that children must enter primary school the calendar year they turn six, implying a cutoff date of January 1. Only three of the participating provinces deviate from this rule: Alberta, Nova Scotia, and Quebec. Thus, to determine the relevant SSA rule for each child, we need to identify which children reside in these three provinces. All

[4] In these cases, there is some measurement error in the rule that we assign to students because we cannot be certain where they lived when they entered primary school. For instance, some students in the PIRLS data may have moved between regions with different SSA rules by the time we observe them in fourth grade.

other children can be assigned a January cutoff.

In the 2006 wave, it is straightforward to identify where children attend school because each province was a benchmark participant. Consequently, the data were stored in separate province-level databases instead of a single country-level database. The filenames include a three-character abbreviation indicating which province the data come from.[5] We use these abbreviations to determine where children attended school at the time of the reading test and derive their expected date of birth according to the relevant SSA rule in that region. We exclude children in Alberta from our analysis because local school authorities are permitted to establish their own cutoff dates.

In 2011, only two of the three provinces that we need to identify – Alberta and Quebec – participated as benchmarking participants. As before, the data for benchmarking participants were stored in separate province-level files, enabling easy identification of the children who reside there. By contrast, the data from Nova Scotia were included with the data from the other non-benchmarking participants in a set of country-level files. The files lack an indicator variable specifying where the data was collected, so we rely on a data-driven approach to identify which schools – and thus which children – are located in Nova Scotia.

Three pieces of information guide our approach for identifying schools in Nova Scotia: (i) suggestive evidence from the benchmarking provinces that school identifiers are numbered sequentially by region; (ii) the fact that children in Nova Scotia should be born between October 1995 and September 1996, assuming full compliance with the SSA rule; and (iii) official statistics reporting that 203 schools and 4,388 children participated in Nova Scotia (see Tables III.2 and III.3 in Labrecque et al., eds, 2012). Guided by this information, we study the date-of-birth distribution at the school level and find a sequence of 203 school identifiers in which the observed distribution complies with an October rather than January cutoff. In line with the official statistics, we observe 4,388 children in these schools.

Germany All children in Germany are required to attend school from age six onwards. The sixteen federal states, known as Länder, have the authority to establish distinct cutoff dates by which children must turn six to enroll in school. Historically,

[5]The abbreviations for the benchmarking provinces are CAB (Alberta), CBC (British Columbia), CNS (Nova Scotia), COT (Ontario), and CQU (Quebec).

there was a uniform cutoff date of July 1 across the entire country. Between 2003 and 2011, half of the states gradually implemented later cutoff dates (Schwandt and Wuppermann, 2016). Due to the lack of regional identifiers in the PIRLS data, we cannot determine the relevant SSA cutoff date for children who entered first grade after the 2002/03 school year. We therefore restrict our analysis to the 2006 wave, as fourth graders in spring 2006 were the last cohort to face the uniform cutoff.

United States The age requirement to enter kindergarten varies across US states, and in a few instances, across local education authorities within a state. Ideally, our data would include information about children's place of residence when they entered kindergarten. This would allow us to assign their expected date of birth according to the region's SSA policy (see, e.g., Elder and Lubotsky, 2009). However, geographic identifiers are suppressed in the public-use versions of ECLS-K:1999 and ECLS-K:2011. We therefore use a data-driven approach to determine the applicable SSA rule for each child.

Our algorithm assigns children an SSA rule by analyzing the birth-date distribution in the school where they attended kindergarten. We compare the observed distribution to the expected distributions under various state-mandated cutoffs. Then, we select the cutoff that best aligns the observed distribution with the expected one. The procedure is detailed below.

1. We create an abbreviated set of state-level SSA rules that were in place when children in the sample entered kindergarten. The abbreviated set combines cutoff dates that are too close in time to distinguish using the observed birth-date distribution. For instance, cutoffs on September 30 and October 1 are grouped together; see Table B.5 for the complete list.

2. We assign all children to a base-year school, typically the one attended during the fall term of kindergarten. For kindergarteners who entered the study after the fall term, we use the spring-term school as the base-year school.

3. We select a subsample of first-time kindergarteners, excluding children whose parents reported that they did not comply with the school's SSA guidelines.

4. We iterate over all SSA rules in the abbreviated set and calculate the share of the subsample in each base-year school whose birth dates comply with the rule.

5. We assign children the SSA rule that maximizes the share of compliers in their base-year school. If multiple cutoff dates maximize the share of compliers, we select the cutoff date that is most common in the national distribution.[6]

The ECLS-K:1999 data also include information on SSA rules from the School Administrator Questionnaire.[7] In the first wave of the survey – i.e., the start of kindergarten – school administrators were asked, *"By what date did a child need to turn five to enter kindergarten for this school year, 1998–1999?"*. For consistency in variable definitions across studies, we do not use this data for our main analysis. We have, however, created a "reported cutoff" based on the survey responses and verified that our estimates are insensitive to the use of reported cutoffs rather than data-driven cutoffs (see Figure B.1).

B.3 Variable definitions

This section describes how we construct several key variables used in our analysis. All variables are derived from interviews and survey data. Thus, we also provide details on the questions posed to respondents, as well as the possible answers they could give. The ECLS-K interview and survey items are reproduced from official documentation provided by the NCES.[8] We have copied the PIRLS survey items from official documentation provided by the IEA.[9]

B.3.1 Date of birth

ECLS-K We observe children's exact date of birth in ECLS-K:1999, but in ECLS-K:2011, we observe only the month of birth, as well as a binned variable for birth year. We can, however, approximate the exact date of birth for children in ECLS-K:2011 using information on age in months at school start, age in months at the time

[6]For ECLS-K:1999, the tie-breaking order is as follows: September 1, December 1, October 1, January 1, October 15, June 1, August 1, September 15, and August 15. For ECLS-K:2011, the tie-breaking order is as follows: September 1, December 1, October 1, August 15, August 1, January 1, September 15, and October 15.

[7]The answers to these questions are suppressed in the public-use files from ECLS-K:2011.

[8]Source: National Center for Education Statistics. Early Childhood Longitudinal Studies Program (ECLS) — Instruments and Assessments.

[9]Source: International Association for the Evaluation of Educational Achievement, PIRLS 2006 Contextual Questionnaires and PIRLS 2011 Contextual Questionnaires.

of various assessments, and the date of these events. As a validation exercise, we implemented the same approach for the ECLS-K:1999 data and compared children's derived date of birth with their actual date of birth. Figure B.2 shows considerable overlap in the actual and derived distributions. The two dates are exact matches for 89% of the sample, and for the cases that do not match, the absolute difference is only 1.89 days on average. The results of this validation exercise suggest that there is minimal measurement error in the derived date of birth for children in ECLS-K:2011.

PIRLS We observe children's month and year of birth in both PIRLS waves, but only the exact day of birth for the 2011 wave. We impute the day of birth as the 15th of the month for all missing cases. This adjustment affected only 4% of observations in PIRLS 2011 (0.7% outside of Austria, where the day of birth is always missing).

B.3.2 Date of reading assessment

ECLS-K We observe the exact date of assessment in ECLS-K:1999. In ECLS-K:2011, we observe the month and year, along with a binned variable for the day of the month. We use the midpoint of the bin.

PIRLS We observe the exact testing date in PIRLS 2011, but only the month and year of the test in PIRLS 2006. Whenever we are missing the day of the month, we impute it as the 15th.

B.3.3 Expected relative age

We use three pieces of information to derive children's expected relative age: (i) the SSA rule in their country or region, (ii) their date of birth, and (iii) the date of reading assessment.

First, we calculate children's expected birthdate by adjusting their actual year of birth so that the resulting date complies with the SSA rule in their country or region. For example, consider a country that requires children to turn six before September 1, 2007 in order to enter primary school in the 2007/2008 school year. If all children comply with the SSA rule, and children cannot skip or repeat a grade level, then everyone in the grade would be born between September 1, 2000 and August 31, 2001. In practice, however, some children enter primary school early or late, and some may be held back or promoted. The expected birthdate for these non-compliers is found by setting the expected birth year to 2000 for everyone born from September through December and to 2001 for everyone born from January through August.

Second, we calculate the child's expected age as the difference between the modal test date in a country and the expected birthdate. We percentile rank children according to their expected age, such that values of zero and one correspond to children who are expected to be youngest and oldest for their grade, respectively.

Note that children's actual and expected birthdate – and thus their actual and expected age rank – are highly correlated ($\rho \approx 0.7$ in our pooled international data). The correlation is stronger in regions with stricter school entry rules and limited grade retention or advancement.

B.3.4 Student performance

We measure student performance using scores on standardized reading assessments that were administered as part of the ECLS-K and PIRLS studies. Children completed the assessments at school, but the results were solely for research purposes and did not affect their grades or academic progression. Because the results were never reported to participants of the study, schools and parents could not adjust their investments in children as a direct reaction to test performance.

We provide two pieces of evidence that the standardized reading score is a reasonable proxy for their overall school performance. First, in Figure B.3, we use ECLS-K data to show that the expected age gradient is similar for other performance measures that might be more salient to parents and schools, such as teacher evaluations of children's skills. Additionally, in Figures B.4 and B.5, we show that the expected age gradient is similar across subjects using data from a subsample of children who completed standardized tests in multiple subjects around the same time.[10]

ECLS-K In each wave, the data include an Item Response Theory (IRT) theta score for children who participated in the reading assessment. The scores are an

[10]The ECLS-K studies included standardized assessments for several subjects in every survey wave. Additionally, in 2011, some countries administered the Trends in International Mathematics and Science Study (TIMMS) to the same sample of children participating in PIRLS. We do not use the TIMMS data in our main analysis due to the lack of equivalent data for children in PIRLS 2006 and for some participating countries in PIRLS 2011.

estimate of children's underlying reading skills, determined by their performance on the specific set of test items that they were administered. The higher the value, the higher the child's estimated ability. Even though children answered different sets of questions depending on their demonstrated ability,[11] the scores are adjusted to reflect a child's latent ability and are unaffected by the difficulty level of the questions that they answered. For our main analysis, we use the IRT theta scores in reading, standardized by database and grade level. Due to an error in the original ECLS-K:1999 data release, we use the corrected set of theta values provided by the NCES.

PIRLS To minimize the burden of the test, a limited number of assessment items were administered to each child. PIRLS uses IRT and multiple imputation techniques to derive estimates of what student performance on the assessment as a whole would have been, had the student completed the entire test. Five plausible values for students' overall reading score are included in the data. In our main analysis, we standardize the first plausible value by database, country, and – when relevant – grade level.[12] We also present robustness checks showing that the estimates are not sensitive to using any of the other four plausible values instead (see Figure A.5 in the main appendix).

B.3.5 Homework help

Our primary variable of interest for parental time investment is the extent to which someone in the home helps children with homework or schoolwork.

ECLS-K Information about homework help was obtained through parent interviews conducted toward the end of each school year, typically between April and June. In ECLS-K:1999, the wording of the question varied across waves with respect to who was helping the child and what subject they were helping with. By contrast,

[11]Following standard IRT procedures, the assessment is carried out in two stages. First, children answer routing questions that span a range of difficulty levels. Second, children are selected to take a low-, middle-, or high-difficulty test depending on their demonstrated ability level in the routing stage.

[12]Most countries test children in a single grade level – typically fourth grade – but in the 2006 wave, Iceland and Norway tested children in both fourth and fifth grade.

the question did not have a subject-specific focus in ECLS-K:2011, and the phrasing remained unchanged across waves. Specifically, parents were asked:

> *During this school year, how often...*
>
> *...did you help {child} with his/her homework?* (ECLS-K:1999, grade 1)
>
> *...have you or any of the people we just mentioned helped {child} with his/her reading, language arts, or spelling homework?* (ECLS-K:1999, grade 3)
>
> *...did someone help {child} with his/her reading, language arts, or spelling homework?* (ECLS-K:1999, grade 5)
>
> *...did you or someone else help {child} with his/her homework?* (ECLS-K:2011, grades 1, 2, 4, and 5)

The possible responses were (1) never, (2) less than once a week, (3) one to two times a week, (4) three to four times a week, or (5) five or more times a week. We standardize the responses by database and survey wave.

PIRLS Information about homework help was obtained through the Home Questionnaire. The question changed slightly between 2006 and 2011, shifting focus from reading help to general help. Specifically, parents were asked:

> *How often do you or someone else in your home do the following things with your child?*
>
> *...Help my child with reading for school?* (PIRLS 2006)
>
> *...Help my child with his/her schoolwork?* (PIRLS 2011)

The possible responses were (1) every day or almost every day, (2) once or twice a week, (3) once or twice a month, or (4) never or almost never. We re-order the responses from least to most compensatory, then standardize by database, country, and – when relevant – grade level.

B.3.6 Skills practice

Our secondary variable of interest for parental time investment is the extent to which parents practice basic skills with their children.

ECLS-K Information about the frequency of skills practice was obtained through a parent interview. The wording of the question remained the same across studies and grade levels:

> *In a typical week, how often do you or any other family member do the following things with {child}:*
>
> *...Practice reading, writing or working with numbers?*

If parents asked for clarification, the interviewer explained that they should include time spent on homework, reading a calendar, or practicing in an exercise book or workbook. The possible responses were (1) not at all, (2) once or twice a week, (3) three to six times a week, or (4) every day. We standardize the responses by database and survey wave.

PIRLS The Home Questionnaire included a question on the frequency of skills practice in 2011, but not 2006. The wording of the question was:

> *How often do you or someone else in your home do the following things with your child:*
>
> *...Help my child practice his/her reading?*
>
> *...Help my child practice his/her math skills?*

The possible responses were (1) every day or almost every day, (2) once or twice a week, (3) once or twice a month, or (4) never or almost never. Parents answered separately for reading and math skills. We re-order the responses from least to most compensatory and take the average over the two subjects for consistency with ECLS-K, which combined practice with reading and numbers in the same question. We standardize the average by country.

B.3.7 Class size

Our primary variable of interest for school investment is the size of the class in which children are enrolled.

ECLS-K Teachers were asked about the number of students in their class who belong to a certain sex, ethnicity, and, in some waves, age level. Class size can therefore be calculated by summing up the reported numbers by sex, ethnicity, or age. The resulting sum might differ across characteristics. Such inconsistencies are, however, relatively infrequent, occurring for about 5% of observations.

To deal with inconsistent responses, we derive a composite variable for class size following guidelines from the ECLS-K User's Manual (see Section 7.4.3.2 in National Center for Education Statistics, 2004). When at least two of the sums by sex, ethnicity, and age level matched, we set the composite variable equal to the matching value. In cases where there were no matching sums, we set the composite variable equal to the sum over both sexes. If that data was missing, we used the sum over all age levels. If that data was also missing, we used the sum over all ethnicities.

In third to fifth grade, inconsistencies may also arise because reading, math, and science teachers completed separate questionnaires.[13] We use the data reported by the reading teacher whenever possible (ca. 99% of cases) and supplement it with data from the math or science teacher if data from the reading teacher is missing. Our focus on the reading teacher is motivated by the fact that our international data come from a survey of reading teachers. Additionally, we have data from the reading teacher for the full sample of children, whereas we have data from either the math or science teacher for random subsamples.

PIRLS In both 2006 and 2011, teachers were asked to fill in a number in response to the following question: *"How many students are in this class?"* After data collection, the variable was top-coded at 60 students. In a small number of cases, classes are linked to multiple teachers, and the enrollment count reported by the teachers differed. We take the average of the teachers' responses for these cases (ca. 0.28% of observations).

B.3.8 Remedial tutoring

Our secondary variable of interest for school investment is whether children receive individual tutoring at school.

ECLS-K We obtain information on tutoring from the child-level teacher questionnaire. In ECLS:K 1999, the wording of the question varied slightly across waves, but

[13] These may not be true inconsistencies if students are sorted into different classes for each subject. It is, however, common for the same group of students to take all of their classes together, even if they have multiple teachers.

it was unchanged across waves in ECLS-K:2011. The possible formulations were as follows:

> *Does this child receive instruction [and/or related services] in any of the following types of programs in your school [during the school day]:*
>
> *...Individual tutoring program in reading?* (ECLS-K:1999, all grades, excluding the bracketed text in kindergarten and first grade)
>
> *Does this child receive, or has he/she received during the school year, instruction in any of the following types of programs in your school:*
>
> *...Individual tutoring or remedial program in reading/language arts?* (ECLS-K:2011, all grades)

For our analysis, we create an indicator equal to one if the teacher responds "yes" to the question.

PIRLS We cannot study this outcome using PIRLS data because teachers do not respond to child-level questionnaires.

B.3.9 Educational expectations

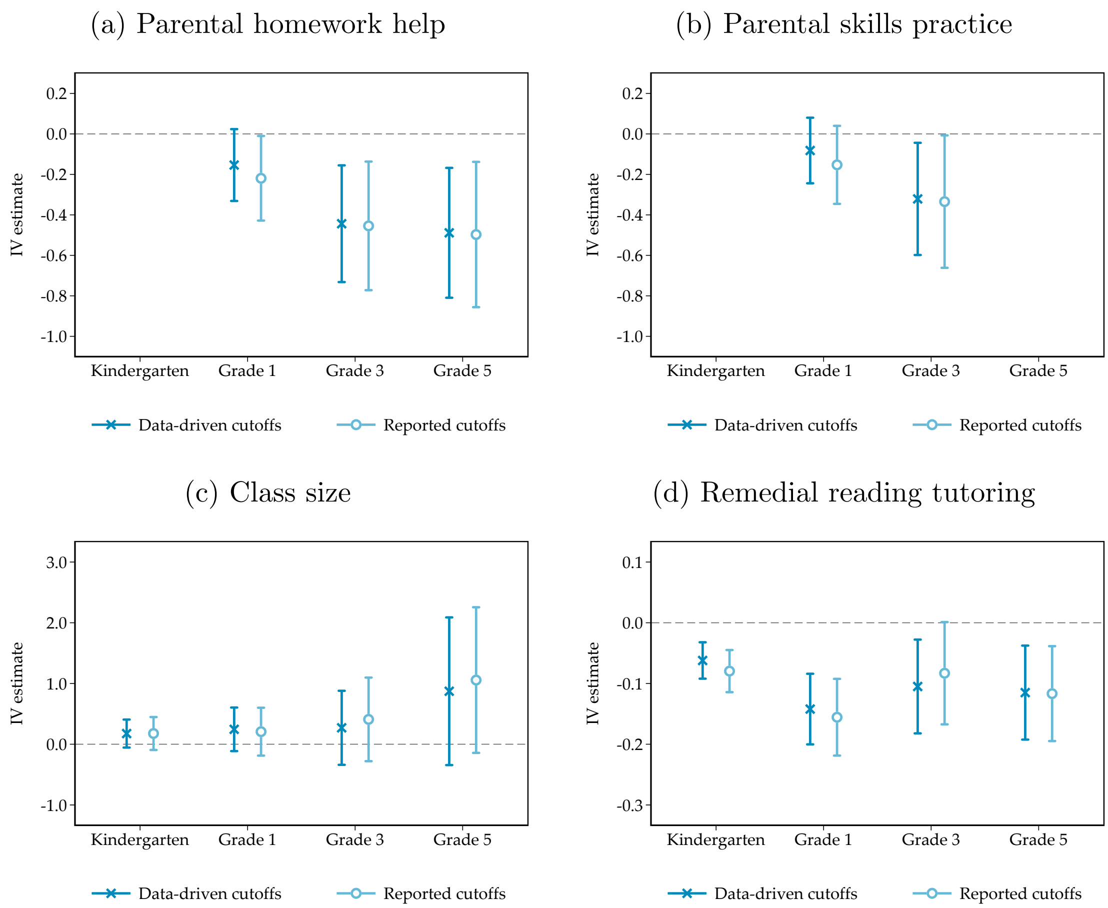
To assess whether parents and schools update their priors as information on student performance is revealed, we study teacher and parental expectations regarding children's future educational attainment. We are primarily interested in the dynamic effects over grade levels, and hence, our main analysis focuses on the ECLS-K panel data for this analysis. Nevertheless, for completeness, we also present results based on cross-sectional data from PIRLS 2011 in the main appendix.

ECLS-K The parent interview included the following question on educational expectations: *"How far in school do you expect (child) to go?"*. The question was asked in kindergarten, first grade (ECLS-K:1999 only), third grade, and fifth grade. For our analysis, we create an indicator equal to one if parents responded that they expect their child to finish a college degree or higher (i.e., at least four years of tertiary education). There is no data on teacher expectations in ECLS-K:1999. However, in the 2011 study, the teacher questionnaire included the following question for first and second graders: *"How far in school do you think this child will go"?* For consistency with our parental expectations variable, we create an indicator equal to one if the teacher responds that they expect the child to complete a four- or five-year college degree or higher.

PIRLS The Home Questionnaire did not include a question on parental expectations in the 2006 wave. In 2011, parents were asked: *"How far in his/her education do you expect your child to go?"* We create an indicator variable equal to one if parents responded that they expect their child to finish ISCED Level 5A or beyond (i.e., tertiary education). There is no child-level data on teacher expectations in either 2006 or 2011 because the teacher questionnaire collects information on class characteristics rather than individual students.

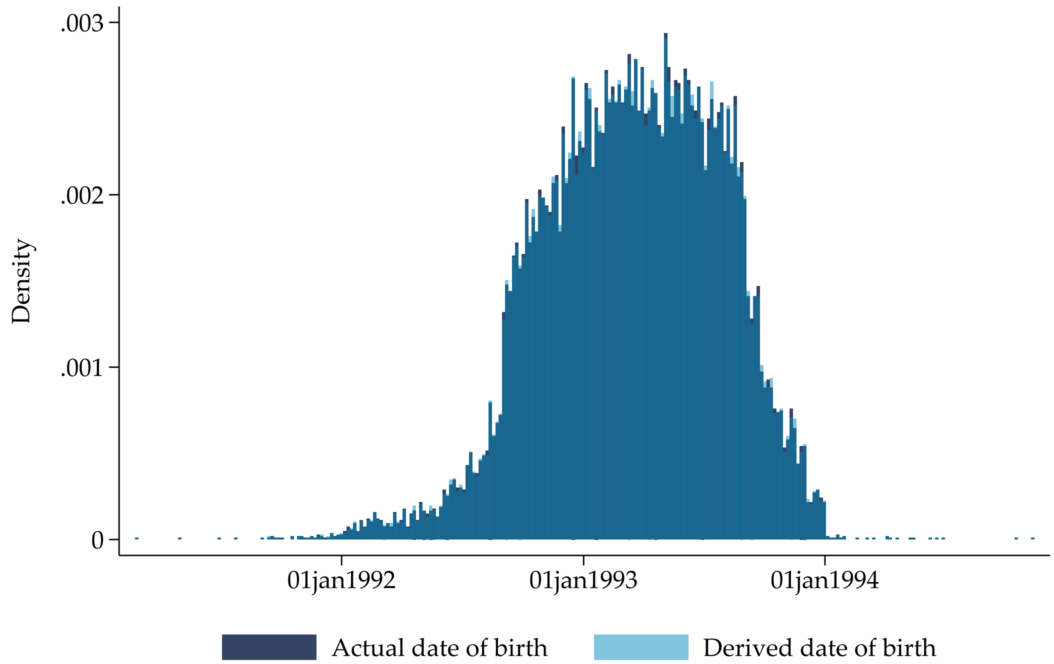
B.4 Supplementary figures and tables

Figure B.1: Robustness check – comparison of IV estimates for ECLS-K:1999 using data-driven and reported SSA cutoff dates



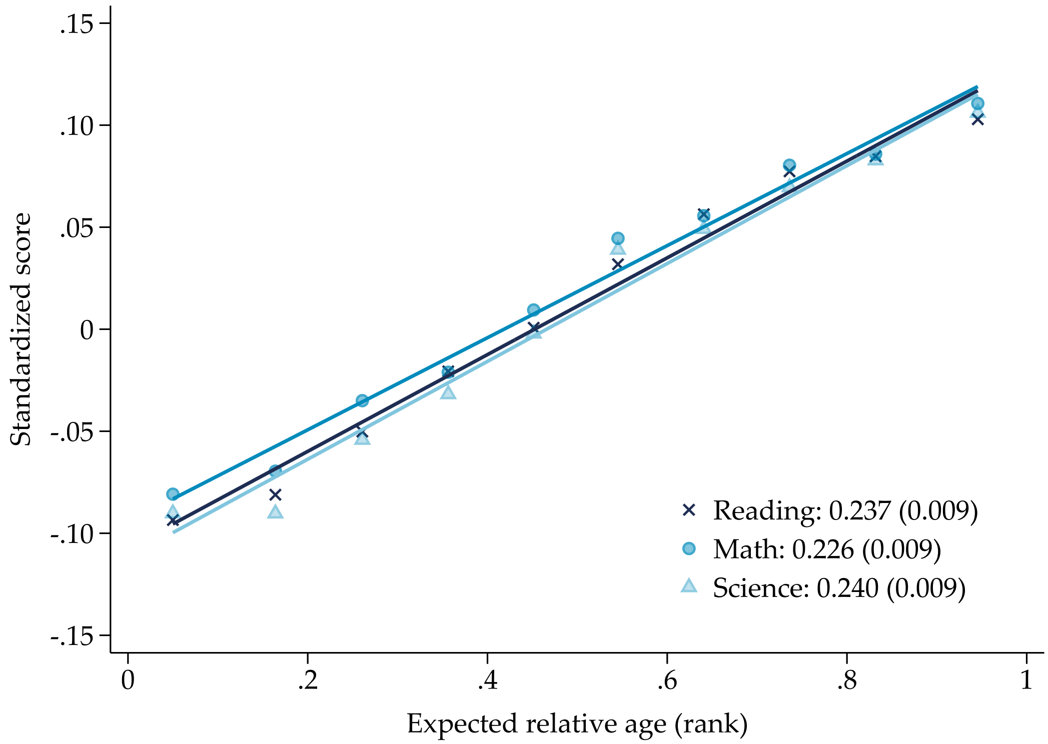
Notes: This figure uses data from ECLS-K:1999 to illustrate that our main IV estimates are unchanged when we calculate our instrument (children's expected relative age rank) using the SSA rules reported by school administrators instead of the SSA rules derived using our data-driven approach. The bars show 95% confidence intervals when standard errors are clustered by expected date of birth.

Figure B.2: Distribution of actual and derived date of birth in ECLS-K:1999



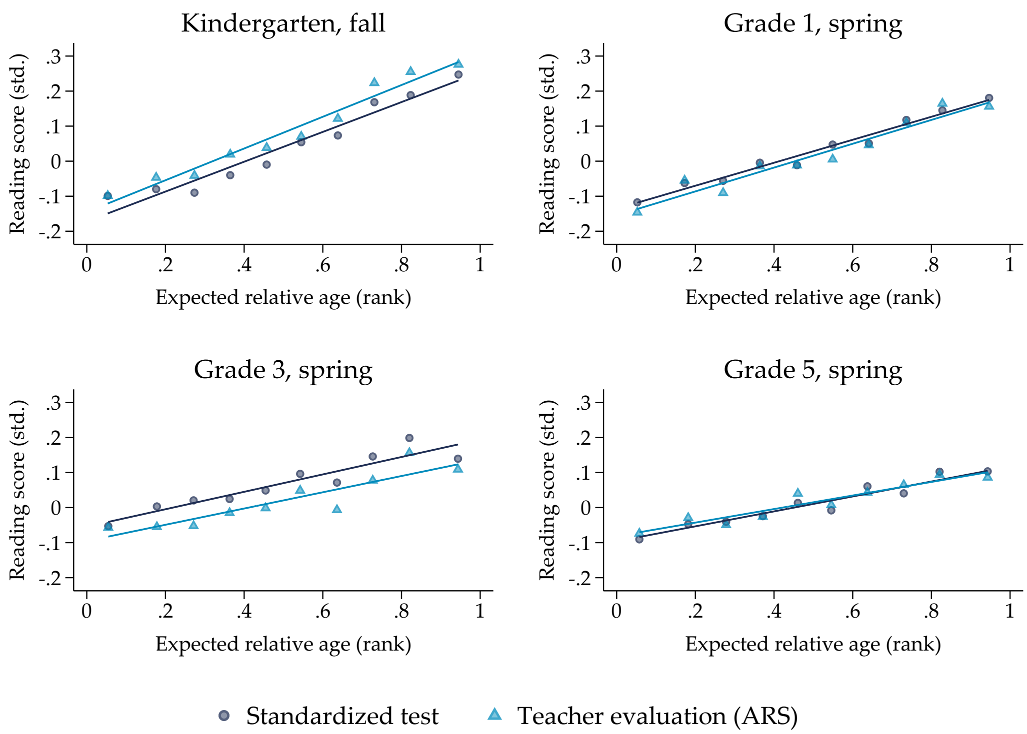
Notes: This figure uses data from ECLS-K:1999 to illustrate that the method we use to derive children's date of birth in the 2011 study produces a date-of-birth distribution (lighter shading) that is nearly identical to the distribution of children's actual date of birth (darker shading).

Figure B.3: Relationship between expected relative age rank and standardized test scores in reading, mathematics, and science



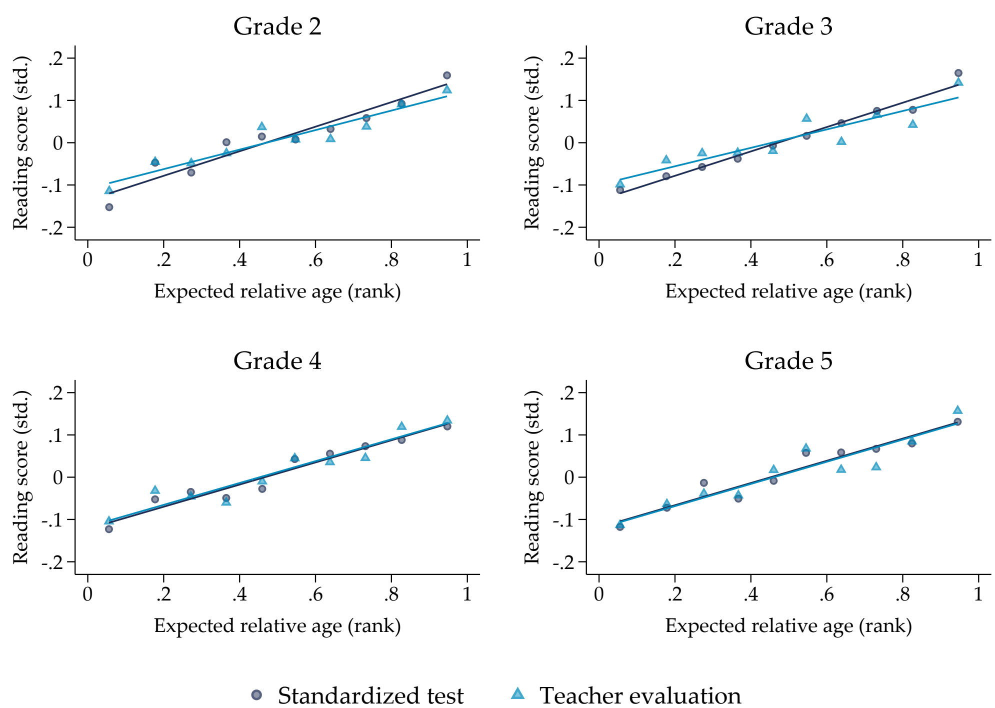
Notes: Each line depicts the first-stage relationship between children's expected relative age and their standardized test score in one of three different subjects: reading, math, and science. The point estimates for each subject are reported in the legend, with cluster-robust standard errors shown in parentheses. All estimates were obtained using data from the fifth-grade waves of ECLS-K:1999 and ECLS-K:2011, as well as the subsample of children who participated in both PIRLS 2011 and TIMMS 2011. The sample includes 115,264 children from 22 countries.

Figure B.4: Relationship between expected relative age rank and different forms of reading assessments in ECLS-K:1999



Notes: Data comes from ECLS-K:1999. Teachers were asked to evaluate multiple aspects of children's literacy skills – for instance, their ability to name letters of the alphabet or read simple books independently relative to similarly aged children. NCES used the teacher responses to compute an Academic Rating Score (ARS). We standardize the reported ARS score to have mean 0 and standard deviation 1.

Figure B.5: Relationship between expected relative age rank and different forms of reading assessments in ECLS-K:2011



Notes: Data comes from ECLS-K:2011. Teachers are asked to rate children's reading skills based on the curriculum standards for his/her grade level. The possible responses are: below grade level, about on grade level, and above grade level. We standardize the responses to have mean 0 and standard deviation 1.

Table B.1: School-starting-age (SSA) rules in the countries in our sample

Country	SSA rule	PIRLS Participant 2006	PIRLS Participant 2011
Australia	Varies by region	No	Yes†
Austria	Age 6 by September	Yes	Yes
Belgium	Age 6 by January	Yes	Yes
Bulgaria	Age 7 by January	Yes	Yes
Canada	Varies by region	Yes†	Yes†
Croatia	Age 6 by April	No	Yes
Czech Republic	Age 6 by September	No	Yes
Denmark	Age 7 by January	Yes	Yes
England	Age 5 by September	Yes	Yes‡
Finland	Age 7 by January	No	Yes
France	Age 6 by January	Yes	Yes
Germany	Varies by region	Yes	Yes‡
Hong Kong	Age 6 by January	Yes	Yes
Iceland	Age 6 by January	Yes	No
Iran	Age 6 by Mehr*	Yes‡	Yes
Israel	Age 6 by Tevet*	Yes‡	Yes
Italy	Age 6 by January	Yes	Yes
Latvia	Age 7 by January	Yes	No
Lithuania	Age 7 by January	Yes	Yes
Luxembourg	Age 6 by September	Yes	No
Malta	Age 5 by January	No	Yes
Netherlands	Age 6 by October	Yes	Yes
Northern Ireland	Age 4 by July	No	Yes
Norway	Age 6 by January	Yes	Yes
Poland	Age 7 by January	Yes	Yes
Portugal	Age 6 by January	No	Yes
Scotland	Age 5 by March	Yes	No
Singapore	Age 7 by January	No	Yes
Slovak Republic	Age 6 by September	Yes	Yes
Slovenia	Age 6 by January	Yes	Yes
Spain	Age 6 by January	Yes	Yes
Sweden	Age 7 by January	Yes	Yes
Taiwan	Age 6 by September	Yes	Yes
United States	Varies by region	Yes‡	Yes‡

Notes: The rules pertain to students' age of entry to primary school; e.g., "Age 6 by September" means that a child must be six years or older on August 31 to start school. We derive the information from questionnaires completed by the PIRLS National Research Coordinator in each country. For details, see question ACQ02 of the Curriculum Questionnaire in the PIRLS 2006 User Guide and Appendix C.1 in the PIRLS 2011 User Guide.

* Tevet begins sometime in December. Mehr typically begins on September 22 or 23.

† PIRLS data from certain regions is excluded from our analysis. See Table B.2.

‡ PIRLS data from the entire country is excluded from our analysis. See Table B.2.

Table B.2: Sample selection – Regions dropped from the PIRLS data

Reason for exclusion	Countries/regions
Reported SSA rule cannot be coded properly due to mid-month cutoff†	Iran (2006); Israel (2006)
Reported SSA rule is ambiguous, not uniform across local education authorities, and/or not verifiable in the observed date-of-birth distribution (i.e., first-stage relationship is weak or non-existent)	Australia—New South Wales; Azerbaijan; Botswana; Canada—Alberta; Colombia; Georgia; Honduras; Hungary; Indonesia; Ireland; Kuwait; Macedonia; Moldova; Morocco; New Zealand; Oman; Qatar; Romania; Russia; Saudia Arabia; South Africa; Trinidad and Tobago; United Arab Emirates
Home Questionnaire was not distributed to parents	England (2011); United States

Notes: †In PIRLS 2006, we do not observe children's exact date of birth – only the month and year. Thus, in countries where the date of birth stipulated by the SSA rule falls in the middle of the month, we cannot code children's expected date of birth correctly.

Table B.3: School starting age rules in Australia for the 2011 cohort

State/Territory	SSA rule	PIRLS Participant 2006	PIRLS Participant 2011
Australian Capital Territory	Age 6 by May	No	Yes
New South Wales	Age 6 by August	No	Yes
Northern Territory	Age 6 by July	No	Yes
Queensland	Age 6 by January*	No	Yes
South Australia	Age 6 by May	No	Yes
Tasmania	Age 6 by January	No	Yes
Victoria	Age 6 by May	No	Yes
Western Australia	Age 6 by July	No	Yes

Notes: The reported rules correspond to the age of entry to primary school (Year 1). Information on cutoff dates is derived from government documents provided by the Queensland Department of Education (Disclosure Log 340/5/2044). The symbol * indicates that the cutoff date changed after our study period. In the data, we identify states and territories through the explicit stratification variable, IDSTRATE. The codes are listed in Supplement 4, Exhibit S4.1 of the PIRLS 2011 User Guide.

Table B.4: School starting age rules in participating Canadian provinces

Province/Territory	SSA rule	PIRLS Participant 2006	PIRLS Participant 2011
Alberta	Varies locally	Yes†	Yes†
British Columbia	Age 6 by January	Yes†	Yes
Manitoba	Age 6 by January	No	Yes
New Brunswick	Age 6 by January	No	Yes
Newfoundland and Labrador	Age 6 by January	No	Yes
Nova Scotia	Age 6 by October	Yes†	Yes
Ontario	Age 6 by January	Yes†	Yes†
Quebec	Age 6 by October	Yes†	Yes†
Saskatchewan	Age 6 by January	No	Yes

Notes: Information is derived from the PIRLS 2006 and 2011 Curriculum Questionnaires. The symbol † indicates that the region was a benchmarking participant. Data for benchmarking participants is recorded in province-level databases. Abbreviations are used in the file names to indicate which province the data comes from: CAB (Alberta); CBC (British Columbia), CNS (Nova Scotia); COT (Ontario); and CQU (Quebec).

Table B.5: Abbreviated list of cutoffs for ECLS-K data-driven approach

Combined cutoff	Other dates included	Nr. of states
Panel (a): ECLS-K:1999		
June 1, 1993	—	1
August 1, 1993	—	1
August 15, 1993	—	1
September 1, 1993	August 31 & September 2, 1993	22
September 15, 1993	September 10, 1993	3
October 1, 1993	September 30, 1993	6
October 15, 1993	—	3
December 1, 1993	December 2, 1993	3
January 1, 1994	December 31, 1993	5

Combined cutoff	Other dates included	Nr. of states
Panel (b): ECLS-K:2011		
August 1, 2005	—	2
August 15, 2005	—	3
September 1, 2005	August 31 & September 2, 2005	23
September 15, 2005	September 10, 2005	3
October 1, 2005	September 30, 2005	6
October 15, 2005	—	2
December 1, 2005	December 2, 2005	3
January 1, 2006	December 31, 2005	4

Notes: The first column lists the cutoff dates that we iterate over when implementing our data-driven approach to identify the SSA rule for each school. We combine cutoffs that are too close in time to distinguish using our algorithm: for example, cutoffs on Aug. 31, Sep. 1, and Sep. 2 are combined into a Sep. 1 cutoff. The third column lists the number of states in which the rule applies. The number of states does not sum to 50 because some states do not have a uniform SSA rule. We obtained information on the legislated state cutoffs from Elder and Lubotsky (2009) and Education Commission of the States (2010).

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