Learning inequality during the COVID-19 pandemic

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Abstract

Suspension of face-to-face instruction in schools during the COVID-19 pandemic has led to concerns about consequences for student learning. So far, data to study this question have been limited. Here we evaluate the effect of school closures on primary school performance using exceptionally rich data from the Netherlands (n≈350,000). The Netherlands represents a best-case scenario with a relatively short lockdown (8 weeks) and high degree of technological preparedness. We use the fact that national exams took place before and after lockdown, and compare progress during this period to the same period in the three previous years using a difference-in-differences design. Our results reveal a learning loss of about 3 percentile points or 0.08 standard deviations. These results remain robust when balancing on the estimated propensity of treatment and using maximum entropy weights, or with fixed-effects specifications that compare students within the same school and family. Losses are up to 55% larger among students from less-educated homes. Investigating mechanisms, we find that most of the effect reflects the cumulative impact of knowledge learned rather than transitory influences on the day of testing. The average learning loss is equivalent to a fifth of a school year, nearly exactly the same period that schools remained closed. These results imply that students made little or no progress whilst learning from home, and suggest much larger losses in countries less prepared for remote learning.

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1 Introduction

The COVID-19 pandemic is transforming society in profound ways, often exacerbating social and economic inequalities in its wake. In an effort to curb its spread, governments around the world have moved to suspend face-to-face teaching in schools, affecting some 95% of the world's student population—the largest disruption to education in history (*1*). The UN Convention on the Rights of the Child states that governments should provide primary education for all on the basis of equal opportunity (*2*). To weigh the costs of school closures against public health benefits (*3–5*), it is crucial to know whether students are learning less in lockdown, and whether disadvantaged students do so disproportionately.

Whereas previous research examined the impact of summer recess on learning, or disruptions from events such as extreme weather or teacher strikes (*6–11*), COVID-19 presents a unique challenge that makes it unclear how to apply past lessons. Concurrent effects on the economy make parents less equipped to provide support, as they struggle with economic uncertainty or demands of working from home (*12*, *13*). The health and mortality risk of the pandemic incurs further psychological costs, as does the toll of social isolation (*14*, *15*). Family violence is projected to rise, putting already vulnerable students at increased risk (*16*, *17*). At the same time, the scope of the pandemic may compel governments and schools to respond more forcefully than during other disruptive events.

Data on learning loss during lockdown have been limited. Unlike societal sectors like the economy or the healthcare system, school systems do usually not post data at high-frequency intervals. Schools and teachers have been struggling to adopt online-based solutions for instruction, let alone for assessment and accountability (*10*, *11*). Early data from online learning platforms suggest a drop in coursework completed (*18*) and an increased dispersion of test scores (*19*). There is also evidence that families' response to home schooling differs widely (*12*, *20–24*). More recently, data have emerged from students returning to school (*25*). Yet, no study to date has

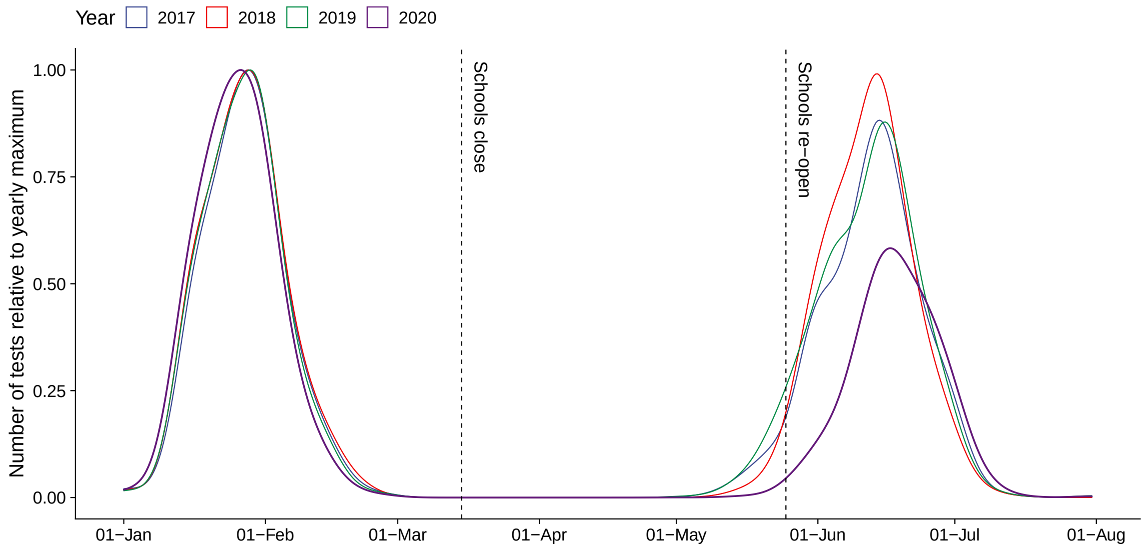

Figure 1. Distribution of testing dates 2017–2020 and timeline of 2020 school closures. Density curves show the distribution of testing dates for national standardized assessments in 2020 and the three comparison years 2017–2019. Vertical lines show the beginning and end of nationwide school closures in 2020. Schools closed nationally on March 16 and re-opened on May 11, after 8 weeks of remote learning. Our difference-in-differences design compares learning progress between the two testing dates in 2020 to that in the three previous years.

quantified the learning loss from COVID-19 using a representative sample, externally validated tests, and techniques that allow for rigorous causal inference.

2 Study setting

Here we present evidence on the pandemic's effect on student progress in the Netherlands, drawing on a unique dataset containing individual-level test scores and demographics from 15% of all schools nationwide (n≈350,000). Hypotheses and analysis protocols for this study were pre-registered (*26*). Our main interest is whether learning stalled during lockdown, and whether students from less-educated homes were disproportionately affected. In addition, we examine differences by sex, school grade, subject, and prior performance.

The Netherlands has several features that makes it attractive as a testing ground for learning loss during the pandemic. While close to the OECD average in terms of school spending and educational performance (*27*), the country leads the world in broadband penetration (*28*, *29*). National and local governments also took swift action to ensure that students had access to appropriate technology (*30*, *31*). School closures were short in comparative perspective (Appendix Figure A1), and the first wave of the pandemic was relatively mild (*32*). The Netherlands therefore presents a best-case scenario, providing a lower bound on learning loss elsewhere in Europe and the world. Despite these favorable conditions, survey evidence from lockdown indicates high levels of dissatisfaction with remote learning (*33*), and considerable disparities in help with schoolwork and learning resources (*20*).

Key to our study design is the fact that national assessments take place twice a year in the Netherlands (*34*): halfway into the school year in January–February and at the end of the school year in May–June. In 2020, these testing dates occurred just before and after the nationwide school closures that lasted 8 weeks starting March 16 (Figure 1). Access to data from 3 years prior to the pandemic lets us create a natural benchmark against which to assess learning loss. We do so using a difference-in-differences design (*35*), and address loss to follow-up using various techniques: regression adjustment, re-balancing on the propensity score and maximum entropy weights, and comparison within schools and families.

3 Results

We assess standardized tests in Maths & Arithmetics, Spelling, and Reading Comprehension for students aged 7–11 (grades 4–7), and a composite score of all three subjects. Results are transformed into percentiles using a common norm for all years. Figure 2 shows the difference between students' percentile placement between the first and second test in each of the years 2017–2020. This difference is centered closely around zero in the years leading up to the pandemic, but drops in 2020 when students fall short of performance in previous years. To adjust for potential confounders and distinguish between subgroups, we go on to estimate a difference-in-differences model

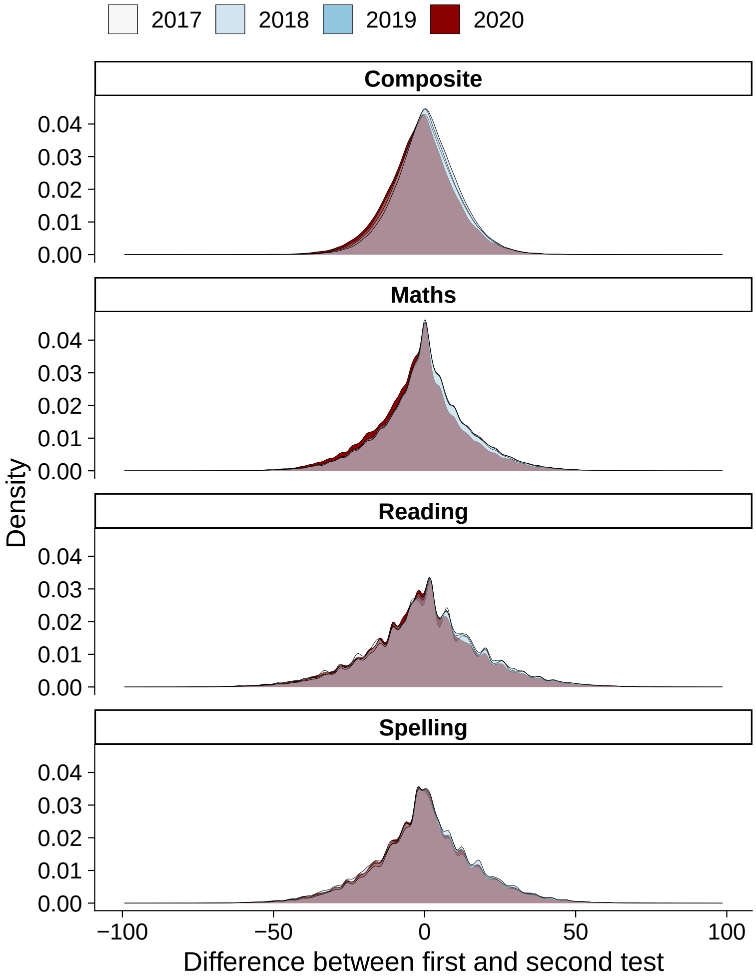

Figure 2. Difference in test scores 2017–2020. Density curves show the difference between students' percentile placement between the first and second test in each of the years 2017–2020, with 2020 marked in red. The dark red area reflects learning loss in the treatment year. Note that this graph does not adjust for confounding due to trends, testing date, or sample composition, which we address in subsequent analyses using a variety of techniques.

(see Section 5: Materials and Methods). In our baseline specification, we adjust for the time elapsed between testing dates and a linear trend in year, and cluster standard errors at the school level.

Baseline specification Figure 3 shows our baseline estimate of learning loss in 2020 compared to the three previous years, using a composite score of students' performance in Maths & Arithmetics, Spelling, and Reading Comprehension. Students lost on average 3.13 percentile points in the national distribution, equivalent to 0.08 standard deviations (SD) (*36*). Losses are not distributed equally but concentrated

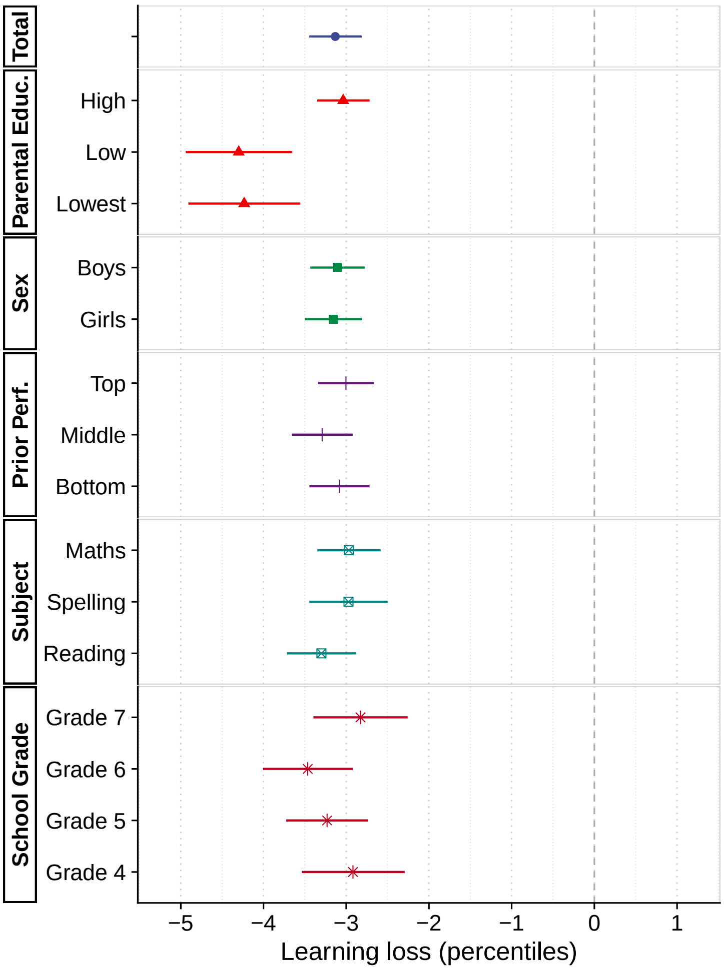

Figure 3. Estimates of learning loss for the whole sample and by subgroup and test. The graph shows estimates of learning loss from a difference-in-differences specification that compares learning progress between the two testing dates in 2020 to that in the three previous years. Statistical controls include time elapsed between testing dates and a linear trend in year. Point estimates with 95% confidence intervals, robust standard errors accounting for clustering at the school level. One percentile point corresponds to approximately 2.5% of a standard deviation. Where not otherwise noted, effects refer to a composite score of Maths, Spelling, and Reading. Regression tables can be found in Appendix, Section 2.1.

among students from less-educated homes. Those in the two lowest categories of parental education—together accounting for 8% of the population (Appendix Figure A2)—suffered losses 40% larger than the average student ($\hat{\beta}$ by high/low/lowest parental education: 3.04, 4.30, 4.24). In contrast, we find little evidence that the effect differs by sex, school grade, subject, or prior performance. In Appendix Figures A3–A4, we document considerable variation by school, with some schools seeing a learning slide of 10 percentile points or more, and others recording no losses.

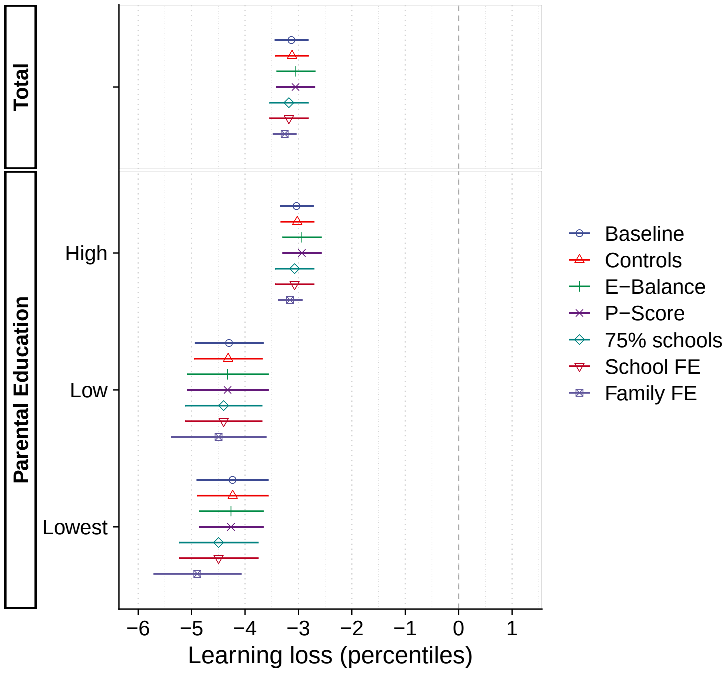

Figure 4. Robustness to specification. The graph shows estimates of learning loss for the whole sample and separately by parental education, using a variety of bias adjustments. Point estimates with 95% confidence intervals, robust standard errors accounting for clustering at the school level. For details, see Section 5: Materials and Methods, and Appendix Section 2.

Placebo analysis and year exclusions In Appendix, Section 2.3–2.4, we examine the robustness of these results in several ways. To confirm that our baseline specification is not prone to false positives, we perform a placebo analysis assigning treatment status to each of the three comparison years (Appendix Figure A5). In each case, the 95% confidence interval of our main effect spans zero. We also re-estimate our main specification dropping comparison years one at a time (Appendix Figure A6). These results are estimated with less precision but otherwise align well with those of our main analysis.

Additional specifications and robustness tests In Figure 4, we report a series of additional specifications addressing the fact that only a subset of students returned after lockdown (see Figure 1). Our difference-in-differences design discards with those students who did not do so, which might lead to bias if their performance differs from those we observe. First, we adjust for the individual covariates used to examine heterogeneity in the main analysis. We also balance treatment and control groups on a wider set of covariates using maximum entropy weights (*37*) and the estimated propensity of treatment (*38*). Moreover, we restrict analysis to schools where at least 75% of students returned after lockdown. Finally, we adjust for unobserved heterogeneity using school and family fixed effects (*39*). As Figure 4 shows, social inequalities grow somewhat when adjusting for selection at the school and family level. The largest gap in effect sizes between educational backgrounds is found in our within-family analysis, estimated at 55% ($\hat{\beta}$ by high/low/lowest parental education: 3.16, 4.50, 4.90).

Knowledge learned vs. transitory influences Do these results actually reflect a decrease in knowledge learned, or more transient "day of exam" effects? Social distancing measures may alter factors such as seating arrangements or indoor climate, that in turn can influence student performance (*40–42*). Following school re-openings, tests were taken in-person under normal conditions and with minimal social distancing. Still, students may have been under stress or simply unaccustomed to the school environment after several weeks at home. Similarly, if remote teaching covered the requisite material but put less emphasis on test-taking skills, results may have declined while knowledge remained stable. We address this by inspecting performance on tasks that are independent of the curriculum (see Section 5: Materials and Methods). These results, in Figure 5, show that treatment effects shrink by nearly two thirds when doing so (main effect $\hat{\beta}$ –1.19 vs. –3.13), which implies that differences in knowledge learned account for the majority of the drop in performance. In years prior to the pandemic, we observe no such difference in students' performance on the two types of test (Appendix Figure A10).

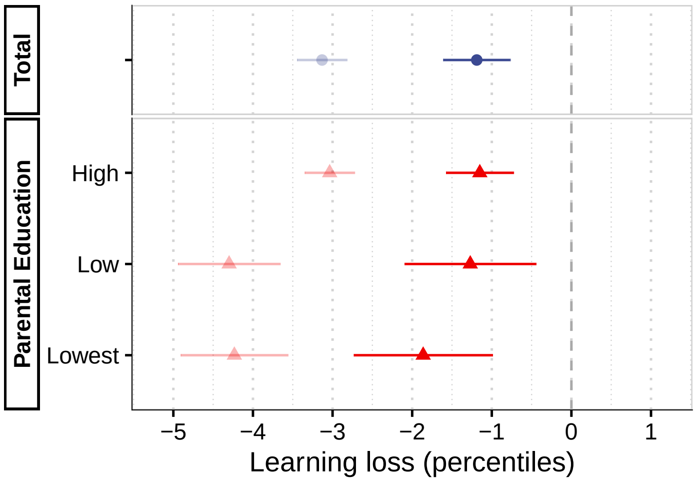

Figure 5. Knowledge learned vs. transitory influences. The graph compares estimates for the composite achievement score in our main analysis (light color) with test not designed to assess curricular content (dark color). Both sets of estimates refer to our baseline specification reported in Figure 3. Point estimates with 95% confidence intervals, robust standard errors accounting for clustering at the school level. For details, see Section 5: Materials and Methods, and Appendix Section 2.9.

4 Discussion

During the pandemic-induced lockdown in 2020, schools in many countries were forced to close for extended periods. It is of great policy interest to know whether students are able to have their educational needs met in these circumstances, and identify groups at special risk. In this study, we have addressed this question with uniquely rich data on primary school students in the Netherlands. There is clear evidence that students are learning less during lockdown than in a typical year. These losses are evident in all grades 4 through 7 and across three subject areas: Maths, Spelling, and Reading. The size of these effects is on the order of 3 percentile points or 0.08 *SD*, but students from disadvantaged homes are disproportionately affected. In the most low-resourced households, the size of the learning slide is up to 55% larger than in the general population.

Are these losses large or small? To answer this, we turn to projections made early in the pandemic (*11*, *24*, *29*, *43–45*). The most credible estimates are from internationally recognized bodies like the World Bank (*43*) or the European Commission

(*29*). Helpfully, these projections span a range of scenarios that let us position the Netherlands as a "best case" based on its resilience in the first wave of the pandemic. The World Bank's "optimistic" scenario—schools closed for 3 months and remote learning operating at 60% efficiency—projects a 0.06 *SD* loss in standardized test scores (*43*). This is less than our 0.08 *SD*, despite the fact that Dutch schools only closed for 8 weeks. The European Commission estimates a lower bound of learning loss of 0.008 *SD* per week (*29*). Multiplied by 8 weeks this translates to 0.064 *SD*, again on the same order of magnitude as our findings if marginally smaller.

Another way to quantify these effects is as a proportion of gains made in a normal year. Here, the World Bank (*43*) projects a best-case scenario of 0.3 years of quality-adjusted schooling lost. In the US, Kuhfeld et al. (*11*) estimate that students may lose a third of a school year in reading and more in maths.[1] Both scenarios assume that schools remain closed for 3 months, not re-opening until after summer. Typical estimates of yearly progress for our age span range from 0.30–0.40 *SD* (*46*, *47*). Under the World Bank assumption of 0.40 *SD* yearly progress (*43*), our estimated loss of 0.08 *SD* translates into 20% of a school year (33% if extrapolating to a 3-month lockdown). This length coincides with the period that schools actually remained closed, implying that students made little or no progress during this time. If we allow for the fact that a third of our treatment effect may reflect transitory testing fluctuations (Figure 5), students nevertheless lost two thirds of expected progress whilst learning from home.

Taken together, our estimates suggest that existing projections of learning loss are, if anything, too conservative. This is alarming in light of the much larger losses projected in countries less prepared for the pandemic. At the same time, our results may underestimate costs even in the context that we study. Schools remained at reduced capacity following re-openings. Dynamic models demonstrate how small initial losses can accumulate into large disadvantages with time (*44*, *48*). Test scores do not consider children's psycho-social development (*49*, *50*), neither societal costs

[1]Contrary to expectations from previous literature (*26*), we did not find losses to be larger in maths. It should be stressed that, the Netherlands, while close to the OECD average in reading performance, places among Europe's top performers in maths (*27*).

due to productivity decline or heightened pressure among parents (*51–53*). Overall, our results highlight the importance of social investment strategies to "build back better" and enhance resilience and equity. Further research is needed to assess the success of such initiatives, and address the long-term fallout of the pandemic for student learning and well-being.

5 Materials and Methods

We access data through partnership with a digital platform that supplies teachers and principals with tools to track student performance. The data have been analyzed under stringent data security protocols, and in a fully anonymized way not traceable to any individual student or school. The sample covers 15% of all primary schools (n≈350,000) and is broadly representative of the national student body in the Netherlands (Appendix Figure A2).

Test scores Nationally standardized tests are taken across three main subjects: Maths & Arithmetics, Spelling, and Reading Comprehension. Students across the Netherlands take the same exam within a given year. These tests are administered in school, and each of them lasts up to 60 minutes. Test results are transformed to percentile scores, but the norm for transformation is the same across years so absolute changes in performance over time are preserved. We rely on translation keys provided by the test producer to assign percentile scores. However, as these keys are actually based on smaller samples than that at our disposal, we further re-norm the distribution to ensure that it is uniform within our sample.

Our main outcome is a composite score that takes the average of all non-missing values in the three areas (Maths, Spelling, and Reading). In sensitivity analyses in Appendix Table A6, we require a student to have a valid score on all three subjects. We also display separate results for the three sub-tests in Figure 3. The test in Maths & Arithmetics contains both abstract problems and contextual problems that describe a concrete task. The test in Reading Comprehension assesses the student's ability to understand written texts, including both factual and literary content. The test in Spelling asks students to write down a series of words, demonstrating that they have mastered the spelling rules.

As an alternative outcome we also assess students' performance on shorter quizzes known as "3-minute tests" (*drieminutentoets*) in Figure 5. The purpose of these tests is somewhat different as they are not designed to elicit curricular content. Instead, they aim to establish how well students can process unfamiliar information and how this skill develops over the years. In a typical task, students are presented with an infographic (e.g., a map), and asked to answer a set of simpler questions. As this part of the assessment does not test for the retention of curricular content, we would expect it to be less affected by school closures, which is indeed what we find.

Parental education We classify parental education according to the official definition designed by Statistics Netherlands (CBS), also used by the Dutch Ministry of Education to allocate resources to schools. The variable codes as *high* those households where at least one parent has a degree above lower secondary education; *low* those where both parents have a degree above primary education but neither has one above lower secondary education; and *lowest* those where at least one parent has no degree above primary education and neither has a degree above lower secondary education. These groups make up, respectively, 92%, 4%, and 4% of the student body and our sample (Appendix Figure A2).

Other covariates Sex is a binary variable distinguishing boys and girls. Prior performance is constructed from all test results in the previous year. We create a composite score similar to our main outcome variable, and split this in to tertiles of top, middle, and bottom performance. School grade is the year the student belongs in. School starts at age 4 in the Netherlands but the first three grades are less intensive and more akin to kindergarten. The last grade of comprehensive school is grade 8, but this grade is shorter and does not feature much additional didactic material. In matched analyses using reweighting on the propensity of treatment and maximum entropy weights, we also include a set of school characteristics described in Appendix, Section 1.3: school-level socioeconomic disadvantage, proportion of non-Western immigrants in the school's neighborhood, and school denomination.

Difference-in-differences analysis We analyze the rate of progress in 2020 to that in previous years using a difference-in-differences design. This first involves taking the difference in educational achievement pre-lockdown (measured using the middle-of-year test) compared to that post-lockdown (measured using the end-of-year test): $\Delta y_i^{2020} = y_i^{\text{2020-post}} - y_i^{\text{2020-pre}}$, where y_i is some achievement measure for student i and the superscript 2020 denotes the treatment year. We then calculate the same difference in the three years prior to the pandemic, $\Delta y_i^{2017-2019}$. These differences can then be compared in a regression specification:

$$\Delta y_i = \beta_0 + \mathbf{X}_i'\gamma + \beta_1 T_i + \epsilon_i, \tag{1}$$

where Δy_i reflects the difference between end-of-year achievement and middle-of-year achievement, $\mathbf{X}_i'\gamma$ is a set of individual control variables, and T_i is an indicator variable for the year 2020. The coefficient $\hat{\beta}_1$ reflects overall learning loss due to the pandemic. From here, we estimate a model including one of our various interest variables and its interaction with the treatment variable to capture possible heterogeneity in the estimated learning loss:

$$\Delta y_i = \beta_0 + \mathbf{X}_i'\gamma + \beta_1 T_i + \beta_2 A_i + \beta_3 T_i A_i + \epsilon_i, \tag{2}$$

where A_i is a categorical variable reflecting some student characteristic. We estimate Equation (2) in turn including parental education, student sex, and prior performance in A_i. In addition, we estimate Equation (1) separately by grade and subject. In running text, we use $\hat{\beta}$ as a shorthand for the estimated treatment effect in a given group. In our baseline specification in Figure 3, the vector $\mathbf{X}_i$ includes terms for the time elapsed between testing dates and a linear trend in year. Throughout our analyses, we adjust confidence intervals for clustering on schools using robust standard errors.

Propensity score and entropy weighting Moreover, we match treatment and control groups on a wider range of individual- and school-level characteristics using reweighting on the propensity of treatment (*38*) and maximum entropy balancing (*37*). In both cases, we include the following covariates: parental education, student sex, prior performance, school-level socioeconomic disadvantage, proportion immigrant background, and school denomination. Propensity of treatment weights involve first estimating the probability of treatment using a binary response (logit) model and then reweighting observations so that they are balanced on this propensity across comparison and treatment groups. The entropy balancing procedure instead uses maximum entropy weights that are calibrated to directly balance comparison and treatment groups non-parametrically on the observed covariates.

School and family fixed effects We perform within-school and within-family analyses using fixed-effects specifications (*39*). The within-school design discards all variation between schools by introducing a separate intercept for each school. By doing so, it eliminates all unobserved heterogeneity across schools which might have biased our results if, for example, schools that perform worse in all years are over-represented during the treatment year. The same logic applies to the within-family design, which discards all variation between families by introducing a separate intercept for each group of siblings identified in our data. This step reduces the size of our sample by approximately 60%, as not every student has a sibling attending a sampled school within the years that we are able to observe. The benefit is that it allows us to remove any unobserved confounding at the family level.

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Appendix

1 Background

1.1 Study context

Figure A1 provides a timeline of school closures in the OECD. In the Netherlands, schools closed on March 16 and reopened on May 11. While students initially attended classes every other day, since June 8 in-person instruction is back to normal activity. Dutch school closures were short compared to many other countries and, in general, the country pursued a light lockdown in the first wave of the pandemic (*33*, *54*). The country leads the world in technology adoption (*28*, *55*): in 2019, more than 90% of households enjoyed broadband access even among the poorest quartile (*29*). Adding to this advantage, the policy response has been swift: already in March 2020, the Ministry of Education devoted 2.5 million euros to online learning devices for students in need (*30*), and this scheme was extended with another 3.8 million in June (*31*).

1.2 Representativeness

Our sample contains schools that have opted in to the digital platform service that we partner with to analyze learning loss. Although covering a subset of schools, the sample is large (n≈350,000) and broadly representative of the national student population. Figure A2 compares the distribution of key school characteristics in the sample and the population as a whole. As this figure shows, there is some over-representation of mid-sized schools (101–200 students). Crucially, however, the relative representation of public as opposed to private schools is identical to that in the student population, as is the socioeconomic composition of the sample as represented by the parental education variable that we use in our main analysis.

1.3 School variables

Test scores and individual covariates are described in the main manuscript. In addition, we use school-level characteristics to balance observations using maximum entropy and propensity of treatment weights (Section 2.5 below and Figure 4 in the main manuscript). We also use these measures in Section 2.2, where we examine school-level treatment effects.

Socioeconomic disadvantage This score ranges from 20 to 40, and is a composite based on the parental education variable used in our main analyses, with weights assigned according to the norm used by the Central Bureau of Statistics: 0.3 for "low education" (both parents have a degree above primary education but neither has one above lower secondary education) and 1.2 for "lowest education" (at least one parent has no degree above primary education and neither has a degree above lower secondary).

Proportion immigrant background The proportion of non-Western inhabitants is based on the neighborhood in which a school is located, which closely correlates with a school's student composition as most students attend a school close to their home. A person is defined as having a non-Western background if they or at least one of their parents were born in Turkey or countries in Africa, Latin America and Asia, except former Dutch colonies and Japan. This variable is not available at an individual level in our data.

School denomination In our matched analyses, we include school denomination as an additional control variable. Here we distinguish three categories: public schools, Christian schools (including Protestant, Catholic, and Reformist denominations), and other denominations (including *inter alia* Islamic and Waldorf schools).

2 Additional results

2.1 Regression tables

Table A1 displays the main effect reported in main manuscript Figure 2, as well as the separate results by subject domain. Table A2 shows results by parental education for the composite score as reported in main text Figure 2, and results for separate subjects. Table A3 does the same for student sex and Table A4 does so for prior performance. In Table A5, we report additional regression results simultaneously controlling for individual-level covariates: parental education, student sex, prior performance. In Table A6 we restrict the sample to only those students with a valid score in all three subjects.

2.2 School variability

Figure A3–A4 report estimates from a mixed-effects model that lets the learning loss differ between schools (*56*). The results reveal considerable variation, with some schools seeing a learning slide of 10 percentile points or more, and others recording no losses or even small gains. In both cases, we plot the predicted school-level treatment effects against school-level socioeconomic disadvantage and the share of non-Western immigrants in the school neighborhood. Losses are larger in schools with a high proportion of students from less-educated homes and of immigrant background, and this holds further when adjusting for individual-level covariates.

2.3 Placebo analysis

In Figure A5 we perform a placebo analysis on non-treated years. We do so by keeping the specification identical to our main analysis but excluding the actual treatment year and, in turn, assigning treatment status to each of the three comparison years. Doing so reveals few significant effects, and those that are so by chance are mostly in the opposite direction of the results reported in the main manuscript. Our identification strategy thus appears robust to false positives, and if anything, is likely to underestimate the treatment effect somewhat given the small bias towards a positive treatment effect in two of three control years. Crucially, however, the pooled effect is not significantly different from zero in any year.

2.4 Year exclusions

We re-estimate our main specification dropping comparison years one at a time to confirm that our results are not driven by any one comparison year. Figure A6 reports the results from these analyses. Although the estimates are less precisely estimated, especially in the last analysis dropping the year immediately preceding the treatment, the qualitative results remain unchanged. Specifically, the difference in effect size between students from high- and low-educated homes remains similar and is significant at the 0.1% level throughout these analyses.

2.5 Covariate balancing

To further address loss to follow-up we implement re-weighting on the propensity of treatment and maximum entropy balancing. Figure A7 shows that both methods achieve a sample that is balanced on the desired characteristics. The weighted regressions use either of these sets of unit weights to rebalance treatment and control groups and achieve comparability. Figure A8 displays our main results using both sets of weights. The results correspond closely across both weighting schemes and are also not appreciably different from our main specification as reported in Figure 2 of the main manuscript.

2.6 Near-complete schools

Moreover, we restrict the sample to only schools where at least 75% of students were tested in the treatment year. Table A7 reports the main treatment effect using this restriction, which remains significant at the 0.1% level and near identical in magnitude to that of our main analysis (–3.18 in Table A7 vs –3.13 in Table A1 and main manuscript Figure 2). Table A8 further displays differential treatment effects by parental education. The estimated treatment at low levels of education is, if anything, slightly larger than estimates reported in our main analysis.

2.7 School fixed effects

Table A9 shows results adding school fixed effects, while Table A10 does so for the interaction by parental education. Again, the estimated treatment effect is significant at the 0.1% level and remains similar in magnitude to our estimates reported in the main text: –3.17 for the whole student body pooled (Table A9) with an added penalty of –1.30 and –1.33 for the groups from less-educated homes, over the baseline of –3.07 for those with at least one higher educated parent (Table A10).

2.8 Family fixed effects

Figure A9 and Tables A11–A12 report results using family fixed effects. This restricts the sample to treated students where either they or a sibling is observed in a comparison year. We sacrifice roughly 60% of our original sample size, which makes the results somewhat less stable. Nevertheless they remain qualitatively similar, and socioeconomic differences in this analysis are somewhat larger: the added added penalty is –1.34 and –1.74 for those from less-educated homes, over the over the baseline of –3.16 for those with at least one higher educated parent (Table A12).

2.9 3-minute tests

We also assess students' performance on short 3-minute tests not designed to test curricular content. If our main estimates of learning loss reflect the cumulative impact of knowledge learned, we would expect these effects to be small or zero. In contrast, if our estimates of learning loss mainly reflect "day of exam" effects due to stress exposure, testing conditions, or familiarity with the school setting, we would expect similarly large losses on both kinds of test. Figure A10, top panel, reveals that the treatment effect on this outcome is on average 62% smaller than for our main outcome. Figure A10, bottom panel, shows that this is not the case in a non-treatment year, where estimated null effects on the both tests are instead near identical.

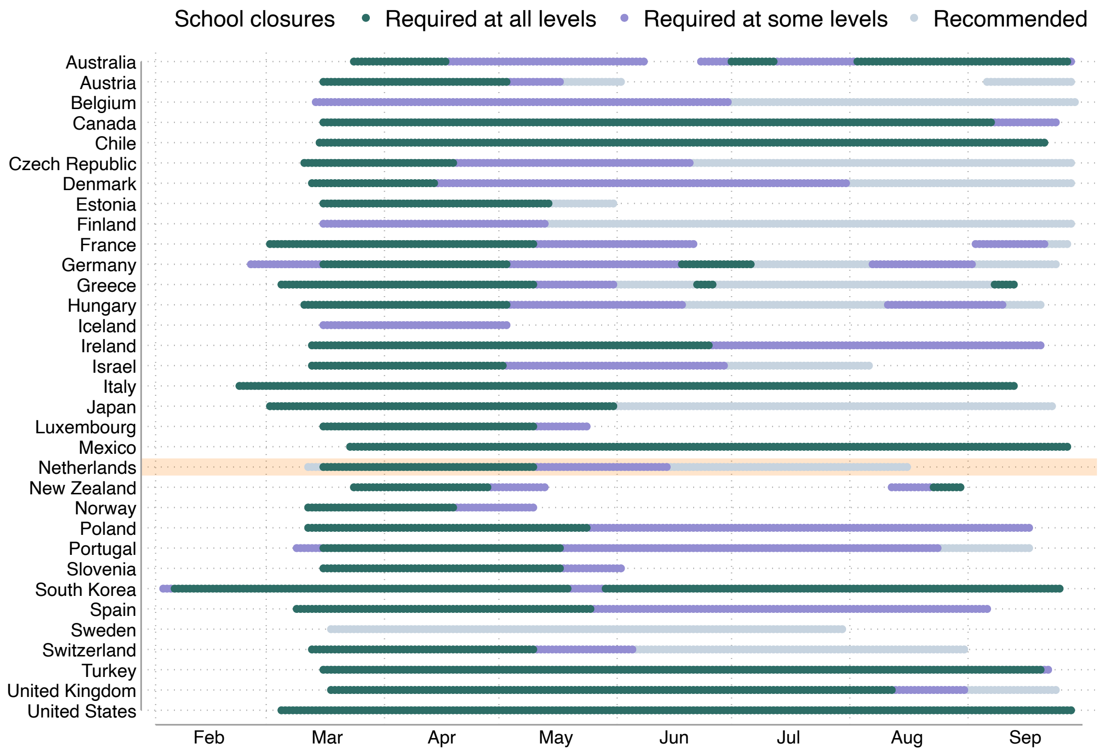


Figure A1. School closures in the OECD. The graph shows the onset and duration of school closures in 33 OECD countries, with the Netherlands marked in orange. Includes all OECD countries for which data could be located. Source: Oxford COVID-19 Government Response Tracker (`https://covidtracker.bsg.ox.ac.uk/`).

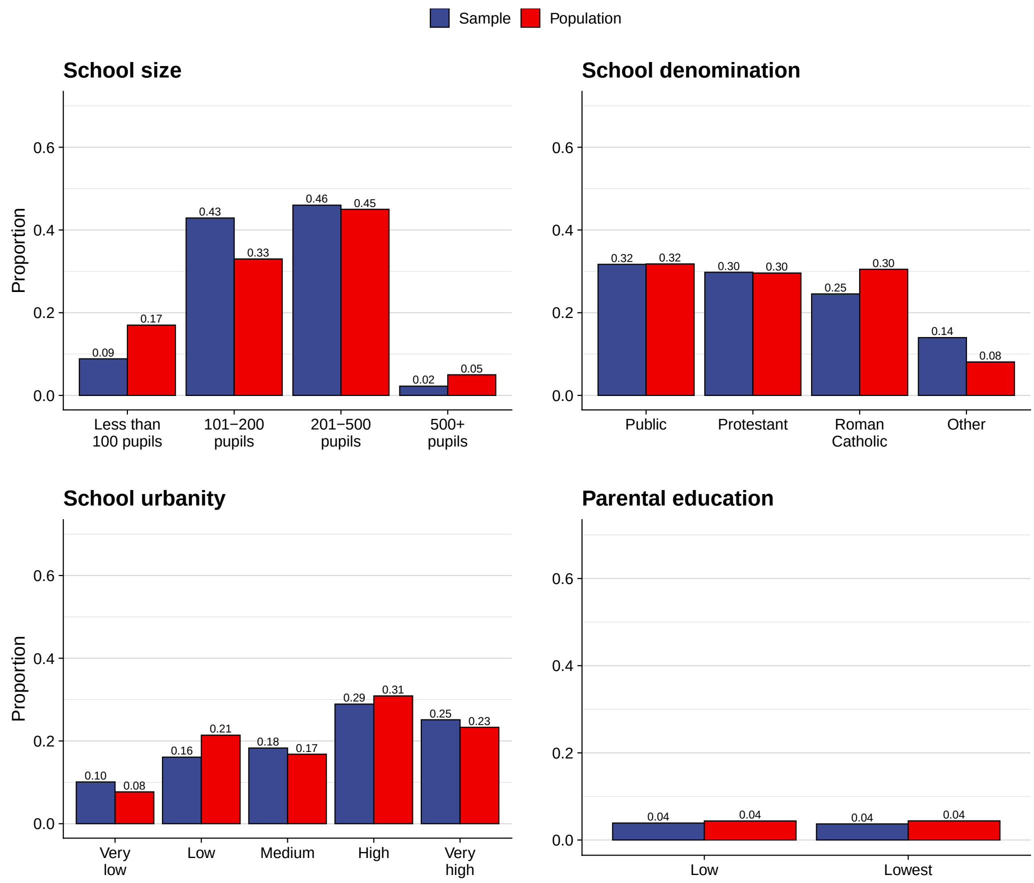
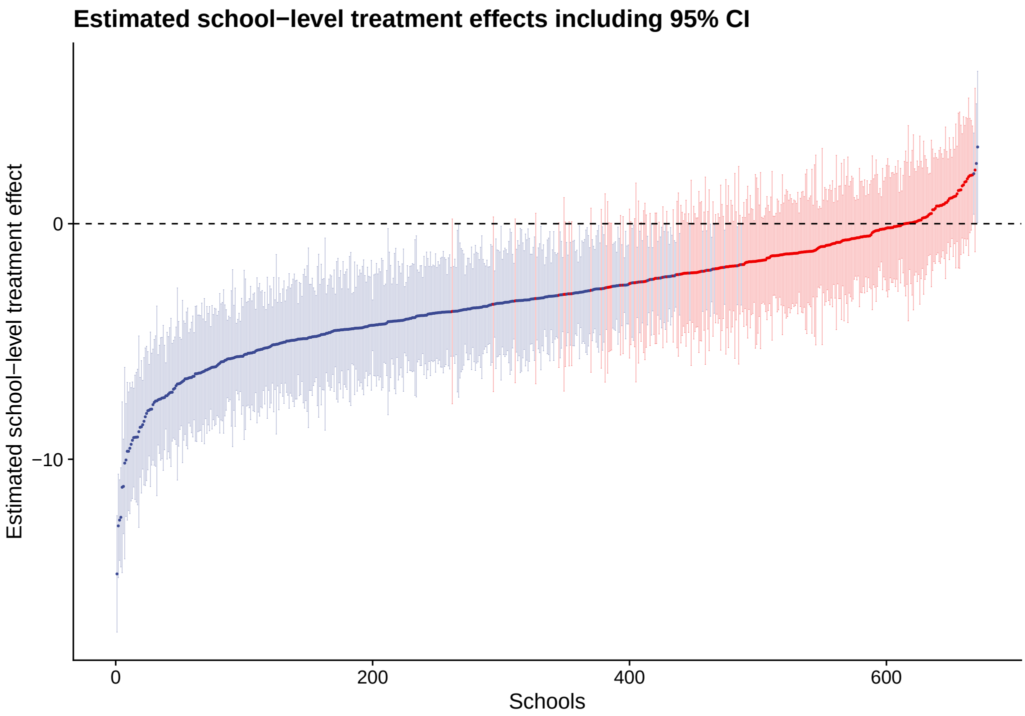
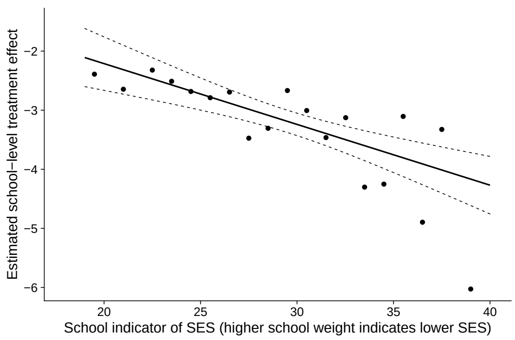


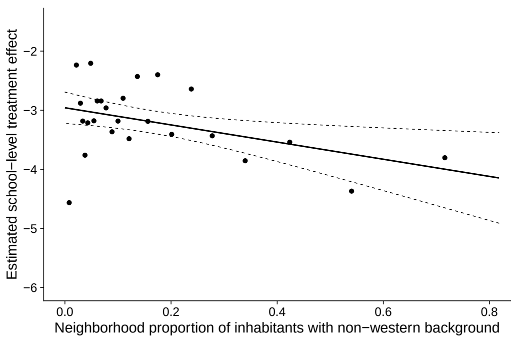
Figure A2. Representativity of the sample. The graph compares the distribution of school characteristics in our sample, shown in blue, with that of the universe of primary schools in the Netherlands, shown in red.



(a) School-level treatment effects

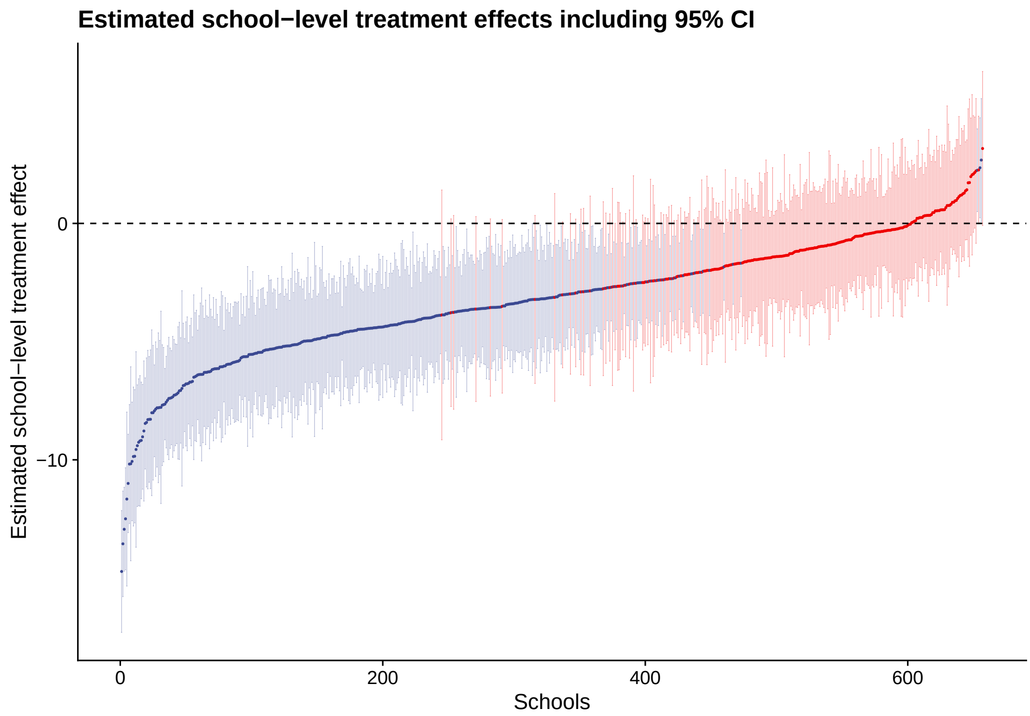


(b) School effect by SES

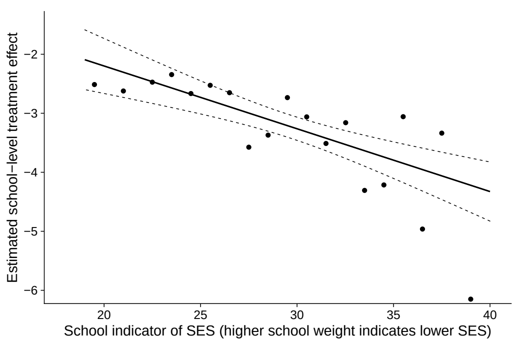


(c) School effect by proportion immigrants

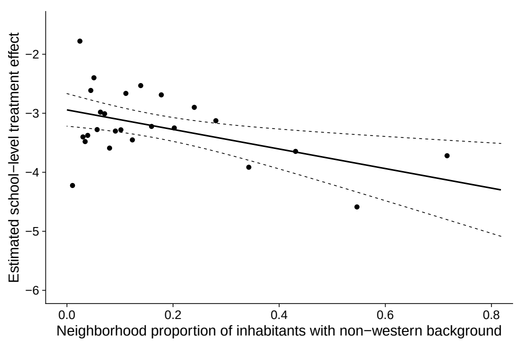
Figure A3. School-level effects. The top panel shows estimates of learning loss by school from a linear mixed model allowing learning loss to differ across schools. The bottom panels plot the predicted effects against school-level covariates: socioeconomic disadvantage and proportion non-Western immigrant background.



(a) School-level treatment effects

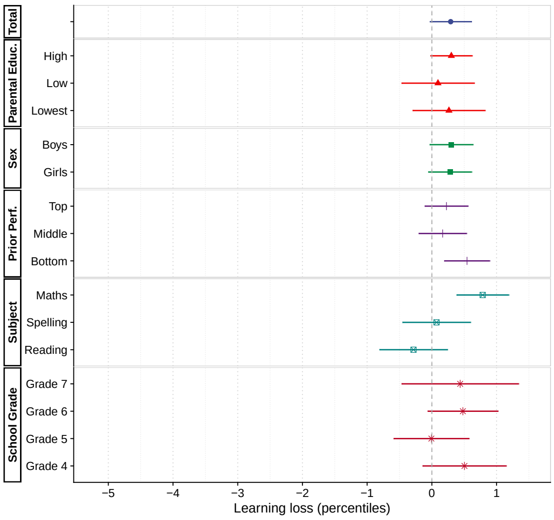


(b) School effect by SES

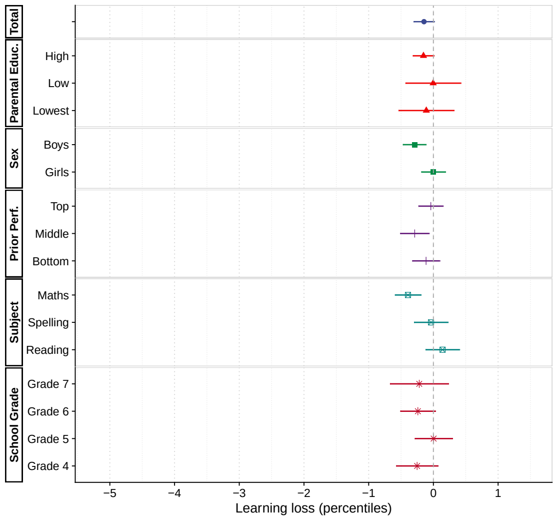


(c) School effect by proportion immigrants

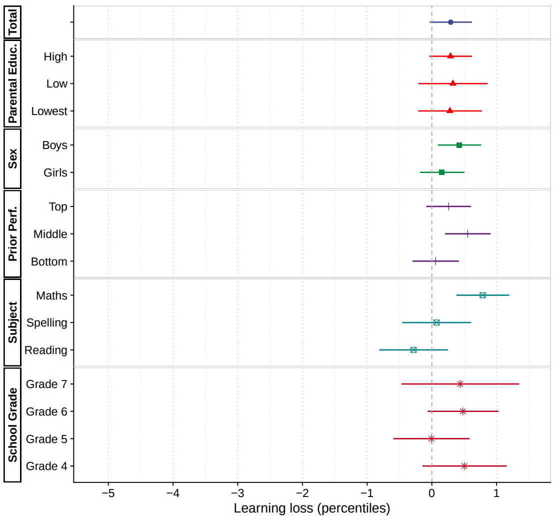
Figure A4. School-level effects with individual controls. The top panel shows estimates of learning loss by school, the bottom panels plot predicted effects against school-level covariates. These results are identical to those in Figure A3 except school-level effects are adjusted for individual-level covariates: parental education, student sex, and prior performance.



(a) 2017 as treated

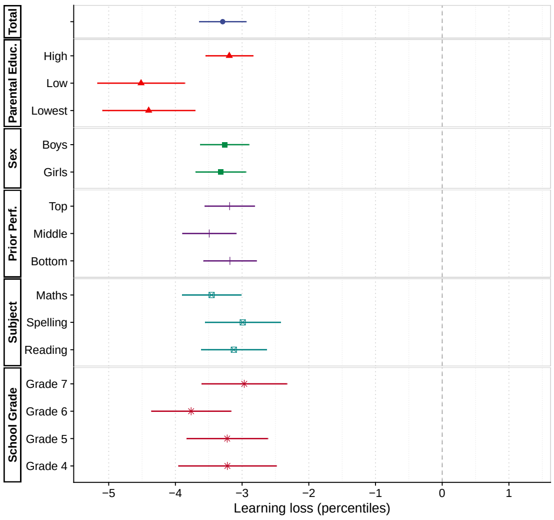


(b) 2018 as treated

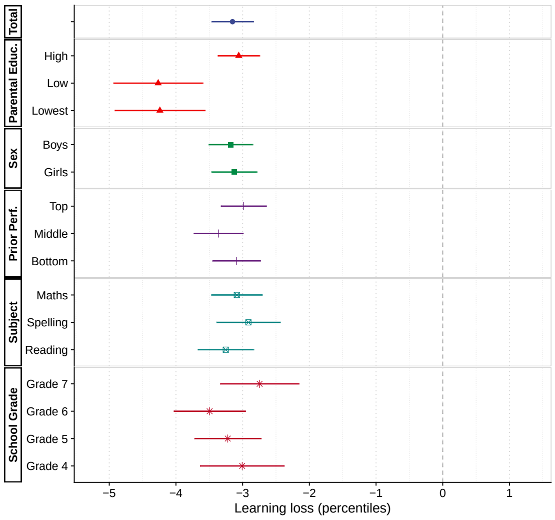


(c) 2019 as treated

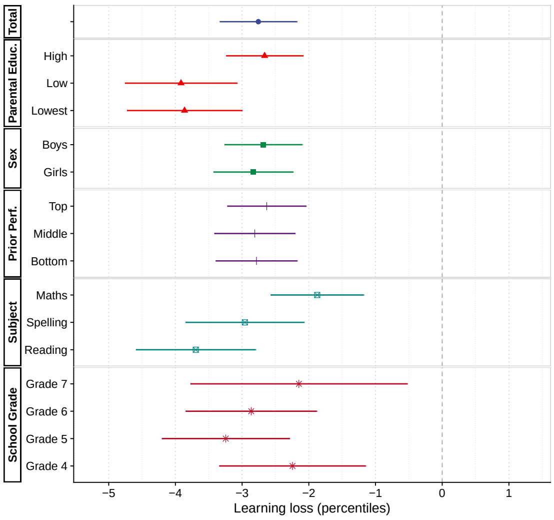
Figure A5. Placebo effects for non-treated years. The graphs show results using our main specification but excluding the actual treatment year and instead assigning treatment status to each comparison year.



(a) 2017 excluded



(b) 2018 excluded



(c) 2019 excluded

Figure A6. Robustness dropping comparison years. The graphs show results using our main specification but in turn excluding each comparison year from the analysis.

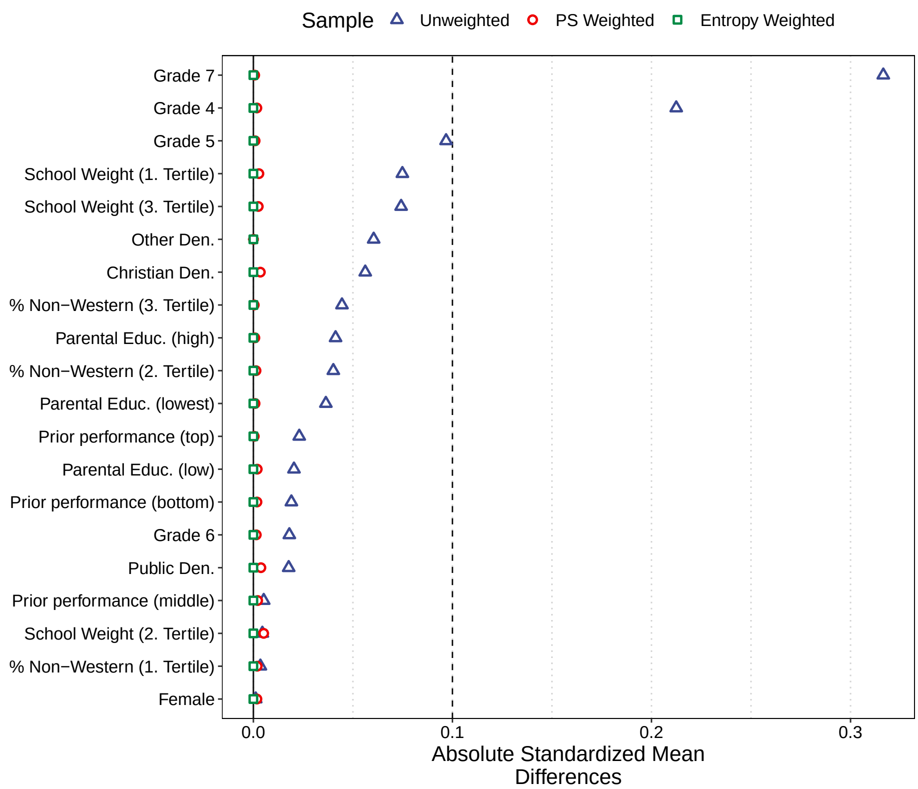


Figure A7. Balancing plot for weighted comparisons. The graph shows absolute standardized mean differences on balancing covariates between treatment and comparison years before adjustment and after reweighting on maximum entropy weights and the estimated propensity of treatment.

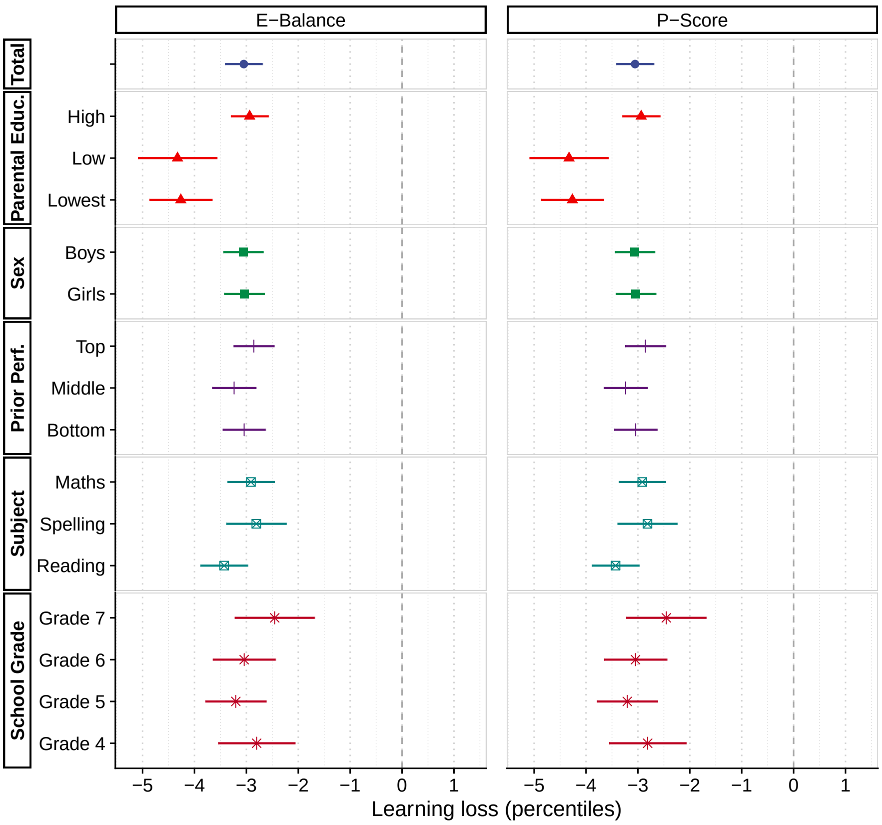


Figure A8. Entropy balanced and propensity-score weighted results. The graph shows results using our main specification while balancing treatment and control years on maximum entropy weights ("E-Balance," left) and the estimated propensity of treatment ("P-Score," right).

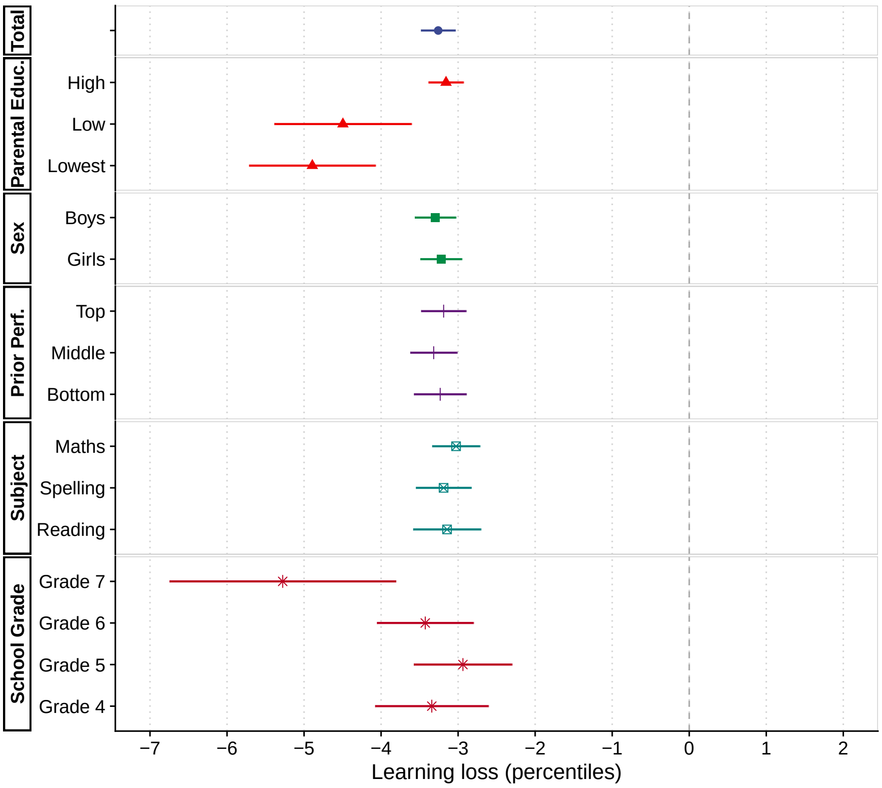
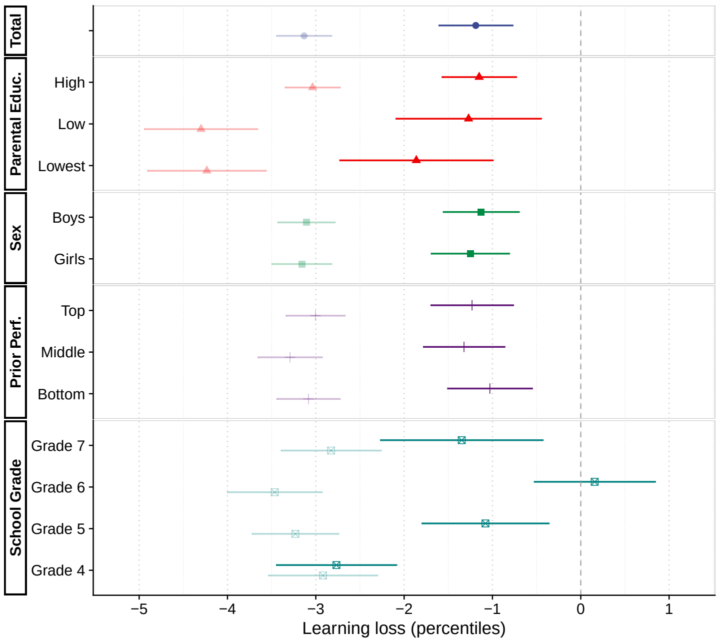
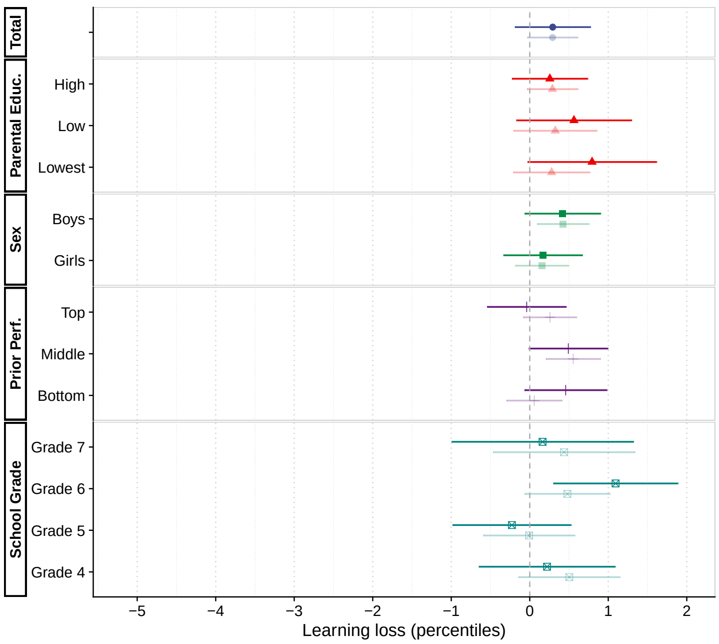


Figure A9. Family fixed effects. The graph shows results combining our difference-in-differences with family fixed effects. This analysis discards all variation between families by introducing a separate intercept for each sibling group, thus adjusting for any heterogeneity across families.



(a) Difference in treatment year



(b) Difference in placebo (2019)

Figure A10. Results for 3-minute tests. The graph contrasts results on 3-minute tests, in solid colors, with our composite achievement score, in transparent colors. The top panel shows estimated treatment effects for 2020, the bottom panel placebo results for 2019. The pooled treatment effect in 2020 is 62% smaller than that of our main analysis, arguably due to the fact that these tests do not assess curricular content.

Table A1. Main effects by subject.

	Composite	Maths	Reading	Spelling
Treatment	-3.13^{***}	-2.97^{***}	-3.30^{***}	-2.97^{***}
	(0.16)	(0.19)	(0.21)	(0.24)
Year (std.)	0.70^{***}	0.21^{**}	0.94^{***}	0.98^{***}
	(0.06)	(0.07)	(0.09)	(0.09)
Days between tests (std.)	0.48^{***}	0.54^{***}	0.20^{**}	0.64^{***}
	(0.05)	(0.05)	(0.07)	(0.09)
(Intercept)	0.69^{***}	0.56^{***}	1.11^{***}	0.60^{***}
	(0.06)	(0.07)	(0.08)	(0.11)
R^2	0.01	0.00	0.00	0.00
Adj. R^2	0.01	0.00	0.00	0.00
Num. obs.	358407	352656	272382	343151
RMSE	11.04	14.86	18.78	17.00
N Clusters	937	937	930	936

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A2. Results by parental education and subject.

	Composite	Maths	Reading	Spelling
Treatment	-3.04^{***}	-2.85^{***}	-3.24^{***}	-2.86^{***}
	(0.16)	(0.19)	(0.21)	(0.24)
Treat x Par. Educ. (low)	-1.26^{***}	-1.11^{**}	-1.15^{*}	-1.72^{***}
	(0.29)	(0.39)	(0.48)	(0.44)
Treat x Par. Educ. (lowest)	-1.20^{***}	-1.81^{***}	-0.41	-1.29^{*}
	(0.33)	(0.42)	(0.43)	(0.51)
Parental Educ. (low)	-0.26^{*}	-0.29	-0.58^{**}	0.11
	(0.12)	(0.16)	(0.21)	(0.20)
Parental Educ. (lowest)	0.21	0.41^{*}	-1.00^{***}	0.89^{**}
	(0.16)	(0.17)	(0.20)	(0.29)
Year (std.)	0.70^{***}	0.21^{**}	0.94^{***}	0.98^{***}
	(0.06)	(0.07)	(0.09)	(0.09)
Days between tests (std.)	0.48^{***}	0.54^{***}	0.20^{**}	0.64^{***}
	(0.05)	(0.05)	(0.07)	(0.09)
(Intercept)	0.70^{***}	0.55^{***}	1.18^{***}	0.56^{***}
	(0.06)	(0.07)	(0.08)	(0.12)
R^2	0.01	0.00	0.00	0.00
Adj. R^2	0.01	0.00	0.00	0.00
Num. obs.	358407	352656	272382	343151
RMSE	11.03	14.86	18.78	17.00
N Clusters	937	937	930	936

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A3. Results by student sex and subject.

	Composite	Maths	Reading	Spelling
Treatment	-3.11^{***}	-2.97^{***}	-3.28^{***}	-3.00^{***}
	(0.17)	(0.20)	(0.23)	(0.26)
Treat x Female	-0.05	-0.01	-0.04	0.05
	(0.12)	(0.15)	(0.19)	(0.18)
Female	0.26^{***}	0.66^{***}	-0.91^{***}	0.69^{***}
	(0.04)	(0.06)	(0.08)	(0.07)
Year (std.)	0.70^{***}	0.21^{**}	0.94^{***}	0.98^{***}
	(0.06)	(0.07)	(0.09)	(0.09)
Days between tests (std.)	0.48^{***}	0.54^{***}	0.20^{**}	0.64^{***}
	(0.05)	(0.05)	(0.07)	(0.09)
(Intercept)	0.56^{***}	0.23^{**}	1.56^{***}	0.26^{*}
	(0.06)	(0.07)	(0.09)	(0.12)
R^2	0.01	0.00	0.00	0.00
Adj. R^2	0.01	0.00	0.00	0.00
Num. obs.	358407	352656	272382	343151
RMSE	11.04	14.85	18.78	17.00
N Clusters	937	937	930	936

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A4. Results by prior performance and subject.

	Composite	Maths	Reading	Spelling
Treatment	-3.00^{***}	-3.11^{***}	-3.15^{***}	-2.46^{***}
	(0.17)	(0.21)	(0.24)	(0.27)
Treat x Prior Perf. (middle)	-0.29^{*}	-0.10	-0.22	-0.67^{***}
	(0.13)	(0.18)	(0.23)	(0.20)
Treat x Prior Perf. (bottom)	-0.08	0.62^{**}	-0.23	-0.90^{***}
	(0.16)	(0.21)	(0.25)	(0.24)
Prior Perf. (middle)	0.59^{***}	0.60^{***}	0.36^{***}	0.75^{***}
	(0.06)	(0.08)	(0.10)	(0.09)
Prior Perf. (bottom)	1.02^{***}	0.88^{***}	0.76^{***}	1.34^{***}
	(0.07)	(0.09)	(0.11)	(0.11)
Year (std.)	0.70^{***}	0.21^{**}	0.94^{***}	0.97^{***}
	(0.06)	(0.07)	(0.09)	(0.09)
Days between tests (std.)	0.48^{***}	0.54^{***}	0.20^{**}	0.64^{***}
	(0.05)	(0.05)	(0.07)	(0.09)
(Intercept)	0.18^{**}	0.08	0.76^{***}	-0.06
	(0.07)	(0.08)	(0.09)	(0.12)
R^2	0.01	0.01	0.00	0.00
Adj. R^2	0.01	0.01	0.00	0.00
Num. obs.	358407	352656	272382	343151
RMSE	11.03	14.85	18.78	17.00
N Clusters	937	937	930	936

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A5. Main effects with controls.

	Composite	Maths	Reading	Spelling
Treatment	-3.12^{***}	-2.97^{***}	-3.22^{***}	-2.98^{***}
	(0.16)	(0.19)	(0.21)	(0.24)
Parental Educ. (low)	-0.74^{***}	-0.75^{***}	-0.98^{***}	-0.44^{*}
	(0.11)	(0.14)	(0.19)	(0.18)
Parental Educ. (lowest)	-0.32^{*}	-0.20	-1.33^{***}	0.35
	(0.13)	(0.15)	(0.18)	(0.26)
Female	0.26^{***}	0.67^{***}	-0.91^{***}	0.70^{***}
	(0.04)	(0.05)	(0.07)	(0.06)
Prior Perf. (middle)	0.55^{***}	0.60^{***}	0.36^{***}	0.64^{***}
	(0.05)	(0.07)	(0.09)	(0.08)
Prior Perf. (bottom)	1.06^{***}	1.06^{***}	0.82^{***}	1.20^{***}
	(0.06)	(0.07)	(0.10)	(0.10)
Grade 5	-0.01	-0.10	-0.32	0.31
	(0.12)	(0.14)	(0.18)	(0.23)
Grade 6	0.02	0.01	0.18	0.01
	(0.11)	(0.14)	(0.17)	(0.19)
Grade 7	0.08	-0.06	0.69^{***}	-0.07
	(0.13)	(0.17)	(0.19)	(0.22)
Year (std.)	0.69^{***}	0.21^{**}	0.82^{***}	0.99^{***}
	(0.06)	(0.07)	(0.10)	(0.09)
Days between tests (std.)	0.48^{***}	0.55^{***}	0.21^{**}	0.64^{***}
	(0.05)	(0.05)	(0.07)	(0.09)
(Intercept)	0.07	-0.23	1.21^{***}	-0.39
	(0.11)	(0.14)	(0.15)	(0.20)
R^2	0.01	0.01	0.00	0.00
Adj. R^2	0.01	0.01	0.00	0.00
Num. obs.	358407	352656	272382	343151
RMSE	11.03	14.85	18.77	16.99
N Clusters	937	937	930	936

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A6. Main effects, complete subject scores only.

	Composite	Maths	Reading	Spelling
Treatment	-3.05^{***}	-3.05^{***}	-3.38^{***}	-2.73^{***}
	(0.17)	(0.22)	(0.22)	(0.27)
Year (std.)	0.68^{***}	0.35^{***}	0.96^{***}	0.73^{***}
	(0.07)	(0.09)	(0.09)	(0.11)
Days between tests (std.)	0.49^{***}	0.56^{***}	0.24^{***}	0.68^{***}
	(0.05)	(0.06)	(0.07)	(0.10)
(Intercept)	0.84^{***}	0.54^{***}	1.12^{***}	0.87^{***}
	(0.06)	(0.07)	(0.08)	(0.12)
R^2	0.01	0.00	0.00	0.00
Adj. R^2	0.01	0.00	0.00	0.00
Num. obs.	259206	259206	259206	259206
RMSE	10.53	14.94	18.79	16.99
N Clusters	929	929	929	929

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A7. Main effects in near-complete schools.

	Composite	Maths	Reading	Spelling
Treatment	-3.18^{***}	-2.98^{***}	-3.26^{***}	-3.13^{***}
	(0.19)	(0.23)	(0.25)	(0.27)
Year (std.)	0.73^{***}	0.21^{*}	0.88^{***}	1.12^{***}
	(0.08)	(0.09)	(0.12)	(0.12)
Days between tests (std.)	0.39^{***}	0.51^{***}	0.14	0.47^{***}
	(0.06)	(0.07)	(0.09)	(0.11)
(Intercept)	0.82^{***}	0.62^{***}	1.23^{***}	0.77^{***}
	(0.08)	(0.08)	(0.10)	(0.15)
R^2	0.01	0.01	0.00	0.00
Adj. R^2	0.01	0.01	0.00	0.00
Num. obs.	248346	244227	190227	235817
RMSE	11.06	14.88	18.79	16.97
N Clusters	594	594	591	593

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A8. Social inequality in near-complete schools.

	Composite	Maths	Reading	Spelling
Treatment	-3.07^{***}	-2.87^{***}	-3.20^{***}	-3.00^{***}
	(0.18)	(0.23)	(0.25)	(0.27)
Treat x Par. Educ. (low)	-1.33^{***}	-1.10^{*}	-1.18^{*}	-1.84^{***}
	(0.31)	(0.43)	(0.53)	(0.48)
Treat x Par. Educ. (lowest)	-1.42^{***}	-1.86^{***}	-0.40	-1.74^{***}
	(0.35)	(0.46)	(0.47)	(0.51)
Parental Educ. (low)	-0.20	-0.28	-0.53^{*}	0.20
	(0.15)	(0.22)	(0.27)	(0.26)
Parental Educ. (lowest)	0.40^{*}	0.35	-0.89^{***}	1.42^{***}
	(0.19)	(0.21)	(0.26)	(0.33)
Year (std.)	0.73^{***}	0.21^{*}	0.88^{***}	1.12^{***}
	(0.08)	(0.09)	(0.12)	(0.12)
Days between tests (std.)	0.39^{***}	0.51^{***}	0.14	0.47^{***}
	(0.06)	(0.07)	(0.09)	(0.11)
(Intercept)	0.81^{***}	0.62^{***}	1.29^{***}	0.70^{***}
	(0.08)	(0.09)	(0.10)	(0.15)
R^2	0.01	0.01	0.00	0.00
Adj. R^2	0.01	0.01	0.00	0.00
Num. obs.	248346	244227	190227	235817
RMSE	11.06	14.88	18.79	16.97
N Clusters	594	594	591	593

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A9. Main effects with school fixed effects.

	Composite	Maths	Reading	Spelling
Treatment	-3.17^{***}	-2.92^{***}	-3.29^{***}	-2.93^{***}
	(0.17)	(0.20)	(0.22)	(0.24)
Year (std.)	0.71^{***}	0.22^{**}	0.94^{***}	0.98^{***}
	(0.06)	(0.07)	(0.09)	(0.09)
Days between tests (std.)	0.36^{***}	0.43^{***}	0.04	0.52^{***}
	(0.06)	(0.07)	(0.09)	(0.09)
School fixed effects				
R^2	0.03	0.02	0.01	0.03
Adj. R^2	0.02	0.01	0.01	0.03
Num. obs.	358407	352656	272382	343151
RMSE	10.94	14.79	18.71	16.78
N Clusters	937	937	930	936

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A10. Social inequality with school fixed effects.

	Composite	Maths	Reading	Spelling
Treatment	-3.07^{***}	-2.81^{***}	-3.23^{***}	-2.79^{***}
	(0.17)	(0.20)	(0.23)	(0.24)
Treat x Par. Educ. (low)	-1.30^{***}	-1.06^{**}	-0.95^{*}	-1.93^{***}
	(0.28)	(0.38)	(0.48)	(0.45)
Treat x Par. Educ. (lowest)	-1.33^{***}	-1.76^{***}	-0.49	-1.68^{***}
	(0.32)	(0.41)	(0.43)	(0.48)
Parental Educ. (low)	-0.10	-0.34^{*}	-0.20	0.29
	(0.10)	(0.15)	(0.20)	(0.17)
Parental Educ. (lowest)	0.22^{*}	0.16	-0.47^{*}	0.78^{***}
	(0.11)	(0.15)	(0.20)	(0.17)
Year (std.)	0.71^{***}	0.22^{**}	0.94^{***}	0.98^{***}
	(0.06)	(0.07)	(0.09)	(0.09)
Days between tests (std.)	0.36^{***}	0.43^{***}	0.05	0.52^{***}
	(0.06)	(0.07)	(0.09)	(0.09)
School fixed effects				
R^2	0.03	0.02	0.01	0.03
Adj. R^2	0.02	0.01	0.01	0.03
Num. obs.	358407	352656	272382	343151
RMSE	10.94	14.79	18.70	16.78
N Clusters	937	937	930	936

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A11. Main effects with family fixed effects.

	Composite	Maths	Reading	Spelling
Treatment	-3.26^{***}	-3.03^{***}	-3.14^{***}	-3.19^{***}
	(0.11)	(0.16)	(0.22)	(0.18)
Year (std.)	0.70^{***}	0.16^{*}	0.94^{***}	1.07^{***}
	(0.05)	(0.07)	(0.10)	(0.07)
Days between tests (std.)	0.35^{***}	0.35^{***}	0.06	0.62^{***}
	(0.05)	(0.06)	(0.09)	(0.07)
Family fixed effects				
Num. obs.	145363	143140	114290	139595
R^2	0.24	0.23	0.27	0.26
Adj. R^2	0.04	0.03	0.02	0.05
Num. groups: family	30490	30467	29436	30394

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A12. Social inequality with family fixed effects.

	Composite	Maths	Reading	Spelling
Treatment	-3.16^{***}	-2.93^{***}	-3.12^{***}	-3.02^{***}
	(0.12)	(0.16)	(0.23)	(0.19)
Treat x Par. Educ. (low)	-1.34^{**}	-0.79	-0.61	-2.50^{***}
	(0.45)	(0.63)	(0.89)	(0.76)
Treat x Par. Educ. (lowest)	-1.74^{***}	-2.10^{***}	-0.06	-2.97^{***}
	(0.42)	(0.58)	(0.83)	(0.72)
Year (std.)	0.70^{***}	0.16^{*}	0.94^{***}	1.07^{***}
	(0.05)	(0.07)	(0.10)	(0.07)
Days between tests (std.)	0.35^{***}	0.35^{***}	0.06	0.61^{***}
	(0.05)	(0.06)	(0.09)	(0.07)
Family fixed effects				
Num. obs.	145363	143140	114290	139595
R^2	0.24	0.23	0.27	0.26
Adj. R^2	0.04	0.03	0.02	0.05
Num. groups: family	30490	30467	29436	30394

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$