School Closures, Household Responses, and Educational Outcomes in Russia*

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ABSTRACT: Did COVID-19 school closures merely reduce learning, or did they redirect students' educational pathways? This paper studies this question in Russia, where school closures varied sharply across regions and grades and students choose educational tracks relatively early. We combine the 2017–2024 Russia Longitudinal Monitoring Survey with a new daily dataset of grade-specific regional school closures. Stacked difference-in-differences estimates show that closures reduced academic performance and disrupted structured educational inputs, including extended school programs and extracurricular enrichment. Households responded through informal childcare and higher private education spending, but these adjustments were partial and concentrated among more advantaged families. Using cohort-specific cumulative exposure to instructional closure days, we show that exposure shifted students away from general secondary education and toward specialized college tracks, with smaller increases in non-enrollment, especially among students without university-educated parents. The results suggest that school closures affected not only instructional time but also students' progression through the education system, contributing to divergence in educational trajectories by parental education.

KEYWORDS: school closure; COVID-19; human capital; educational trajectories; household investments; stacked difference-in-differences; event study; Russia.

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1 Introduction

School closures were one of the most widespread educational disruptions in modern history, affecting more than 1.6 billion students worldwide at the peak of the COVID-19 pandemic (UNESCO 2020). A rapidly growing literature documents substantial short-run learning losses from these disruptions (see, e.g., Jack and Oster 2023 for a review).[1] Yet much less is known about how families responded to lost instruction, whether compensatory investments mitigated these shocks, and whether short-run disruptions translated into persistent changes in educational trajectories. Existing evidence is also heavily concentrated in the United States and Western Europe, leaving open questions about how school closures affected children in institutional settings where educational pathways, household responses, and access to educational resources differ substantially. Using evidence from Russia, this paper examines both households' immediate responses to school closures and their subsequent consequences for children's educational trajectories in an understudied institutional setting.

School closures in Russia were both widespread and prolonged. Although all regions shifted to remote instruction during the initial wave of the pandemic in spring 2020, disruptions persisted well beyond the first shutdown, with many regions repeatedly suspending in-person instruction through 2021 and 2022.[2] We estimate that the cohort entering first grade in 2015, the most affected cohort in our data, lost an average of 72 instructional days, with cumulative losses ranging from 43 to 140 days across regions. These disruptions represent a substantial loss of instructional time during critical stages of children's academic development.

Educational disruptions may have particularly important consequences for subsequent educational trajectories in Russia because students make high-stakes educational decisions earlier than in many Western countries. After grade 9, nearly half of students enroll in vocational schools and

[1] See also, for example, Engzell et al. (2021) for the Netherlands; Grewenig et al. (2021) for Germany; Blundell et al. (2022) for the U.K.; Bertoletti et al. (2023) for Italy; and Goldhaber et al. (2023) for the United States.
[2] Appendix Figure A1 maps this regional variation in cumulative school-closure days through 2023 for students in grades 1–8.

technical colleges rather than continuing in general secondary education, creating an early margin through which school closures may alter educational and occupational trajectories. These decisions often involve earlier specialization into occupation-specific fields of study, and educational pathways are generally less flexible than in the United States, where students typically postpone specialization until later stages of higher education.

School closures may have disrupted not only formal classroom instruction but also complementary educational investments outside of school that play an important role in children's skill development. This channel is often overlooked in the school closure literature but may be particularly important in Russia, where a well-developed supplementary education sector provides children with access to tutoring, technical clubs, arts and music schools, and other extracurricular academic activities.[3] At the same time, alternative formal schooling arrangements remained rare: fewer than 0.6 percent of children ages 7–13 were not enrolled in school before the pandemic, only a handful were homeschooled, and no region in our data adopted sustained hybrid schooling.[4] As a result, school closures largely reflected instructional loss rather than substitution into alternative schooling arrangements.

While Russia shares important similarities with many high-income countries, including high female labor force participation, broad public-school enrollment, and widespread access to digital technologies, Russia's vast geographic size generates substantial spatial heterogeneity. Regional disparities in income, educational resources, and digital infrastructure are considerably larger than those observed in many Western countries. Combined with decentralized school closure policies that often varied across both regions and grade levels, this created substantial differences in both children's

[3] In the RLMS-HSE, 59 percent of children ages 7–13 participated in extracurricular enrichment activities in the last pre-pandemic survey wave (2019).

[4] Authors' calculations based on the RLMS-HSE. In comparison, homeschooling in the United States rose to 11.1 percent in fall 2020, and hybrid learning became common, according to the U.S. Census Bureau's Household Pulse Survey.

exposure to school closures and their ability to adapt to them.

To capture this regional and grade-specific variation, we compile a novel dataset on daily school closure policies across Russian regions throughout the pandemic. The dataset distinguishes COVID-related school closures from regular school breaks, holidays, and non-pandemic disruptions, allowing us to directly measure the timing and intensity of instructional loss across regions and student cohorts. We combine these policy data with the Russian Longitudinal Monitoring Survey–Higher School of Economics (RLMS-HSE), a nationally representative longitudinal household survey that tracks children and families before, during, and after the pandemic.

Our empirical strategy combines short-run stacked difference-in-differences (DID) models that exploit variation in the timing of regional school closures with longer-run analyses that use cumulative exposure measures based on grade-specific instructional losses across student cohorts. We find that school closures immediately reduced academic performance and disrupted educational inputs, including extracurricular participation and time spent on learning activities. Households responded only partially through increased parental involvement, private spending, and alternative childcare arrangements, with compensatory investments concentrated among more advantaged families. These short-run disruptions translated into persistent shifts in educational trajectories, as students reallocated away from traditional academic pathways and toward alternative post-secondary options, and the magnitude of these shifts varied significantly with pre-pandemic household conditions. The effects were larger for students without university-educated parents, showing that school closures widened inequality not only in short-run inputs and outcomes but also in longer-run educational trajectories.

This paper contributes to several strands of literature. First, it adds to the growing evidence on learning losses observed during COVID-19 school closures; see Engzell et al. (2021), Grewenig et al. (2021), Blundell et al. (2022); Bertoletti et al. (2023); and Goldhaber et al. (2023). We not only expand the geographic scope of the school closure literature beyond high-income Western countries but also implement a rigorous empirical design based on stacked DID and an event-study approach. In contrast, much of the existing literature relies on broader before–after or cohort-based comparisons, whereas our approach exploits finer variation in the timing of school closures across regions and cohorts.

The paper further contributes to research on household responses to educational shocks by examining whether families offset lost instruction through private investments, parental involvement, and caregiving arrangements (see, e.g., Agostinelli et al. 2022; Bacher-Hicks et al. 2021). It is also related to the literature on inequality in the effects of school closures by documenting heterogeneous responses across households (see, e.g., Grewenig et al. 2021; Blundell et al. 2022). We extend this literature by showing that these unequal responses are accompanied by diverging educational pathways after students reach key transition points.

This study adds to the recent literature on how COVID-19 disruptions affected educational participation and progression beyond test scores, including high school enrollment, college applications and entry, and community college enrollment (see, e.g., Chatterji and Li 2021; Schanzenbach and Turner 2022; Dagorn and Moulin 2025; Pope and Yao 2026). While recent papers document short-run or immediate changes in educational participation, our analysis uses cohort- and region-specific cumulative exposure to instructional loss and a richer set of educational tracks, capturing post-pandemic educational trajectories.

Finally, the paper contributes more broadly to the literature on educational disruptions and instructional time (see, e.g., Goodman 2014; Lavy 2015; Aucejo and Romano 2016; Jaume and Willén 2019; Liu et al. 2021) by linking changes in schooling inputs to both household responses and educational trajectories.[5]

[5] These studies examine different types of disruptions, including variation in instructional time (Lavy 2015), weather-related school closures (Goodman 2014), variation in school days and absences (Aucejo and Romano 2016), teacher strikes (Jaume and Willén 2019), and secondary school absences (Liu et al. 2021).

The remainder of the paper is organized as follows: Section 2 describes the survey data, Section 3 reports stacked difference-in-differences model estimates for short-run effects, Section 4 presents evidence on educational trajectories in the post-pandemic period, and Section 5 concludes.

2 Data and Measurement

2.1 Sample Construction

This study uses data from the Russian Longitudinal Monitoring Survey–Higher School of Economics (RLMS-HSE), a nationally representative annual household panel that collects detailed information on children, parents, educational outcomes, and household conditions.[6] The survey follows individuals over time and allows children to be linked to household and parental characteristics. Our analysis uses survey waves from 2017–2024, spanning both pre-pandemic and post-pandemic periods. RLMS-HSE interviews are conducted primarily between September and December, with occasional interviews extending into January. As a result, the survey does not capture the nationwide school shutdown in spring 2020 but overlaps with two major COVID-19 waves in fall 2020 and fall 2021, when many school closures occurred. A valuable feature of the RLMS-HSE is the availability of exact interview dates, which allows us to align children with daily regional school closure episodes.

The empirical analysis draws on two distinct samples because the short-run and longer-run questions concern different age groups and time horizons. The short-run analysis relies on the child questionnaire, which is administered to children under age 14 and completed by parents or guardians. We focus on this age group because the short-run educational-input and household-response measures are not collected in the adult questionnaire completed by older students. This sample is

[6] The RLMS-HSE draws respondents from 38 randomly selected primary sampling units located in 32 of Russia's 83 federal subjects. The survey is based on a stratified multistage probability sample designed to be nationally representative, although it does not provide representative samples at the regional level.

restricted to school-enrolled children observed between 2020 and 2022, when closure episodes occurred. The longer-run analysis of educational trajectories uses individuals ages 16–22 observed between 2017 and 2024, so that educational status is observed after the grade-9 branching point. Some individuals can appear in both samples across survey waves, but the overlap is limited; the two analyses therefore provide complementary evidence on different stages of the schooling process.

2.2 Measures of Educational Outcomes and Compensatory Investment

To examine how school closures affected children's human capital formation, we construct several measures of academic performance and educational inputs. Academic performance is measured using a continuous *GPA* variable on a 2–5 scale based on parent-reported school performance under the Russian grading system, where higher values indicate better achievement.

We also measure participation in structured activities outside regular classroom instruction. *Extended School Day* measures daily hours spent in an extended day program (*prodlyonka* in Russian) on school premises, often including homework support and general childcare. *Out-of-School Enrichment* captures whether the child participates in organized enrichment activities such as arts and music, dance and theater, technical clubs, computer training, foreign languages, and supplementary academic subjects.

We further construct two measures of independent study. *Homework Hours* record daily hours devoted to completing school assignments, while *Reading Hours* capture daily hours spent reading. In addition, *Out-of-School Screen Hours* measure daily hours spent using the internet and playing video or computer games, excluding digital time at school. While this measure does not directly capture educational inputs, it helps assess whether lost instructional time was reallocated toward digital leisure activities that may potentially crowd out human capital investments.

To examine household compensatory responses, we construct several measures of private investments that may offset reductions in school-based supervision and instruction during school closures. *Care by Non-HH Relatives* is an indicator for whether relatives living outside the household helped care for the child for more than three days during the last seven days. *Care by Non-Relatives* captures whether non-relatives provided childcare for more than three days during the last seven days.

We also measure parental involvement in children's activities relative to the annual sample median.[7] *Homework Help* indicates whether parental time spent helping the child with homework exceeds the median. *Additional Study Help* captures whether parental time spent helping the child with supplementary academic subjects exceeds the median. *Arts, Crafts, & Tech* indicates whether parental time spent on creative and technology-related activities with the child exceeds the median. *Private Education Spending* measures household-level expenditures during the previous 30 days on children's education-related needs, including school fees, extracurricular activities, private lessons or tutoring, and gifts for teachers.

To study educational trajectories over the 2017-2024 period, we construct five indicators of current educational status: general secondary school, vocational secondary school (programs for training qualified workers and employees; former PTUs), specialized college programs (associate-degree-type programs)[8], university programs, and non-enrollment in secondary or tertiary education. The vocational secondary, specialized college, and university categories include both current enrollment and completed programs.

Overall, RLMS-HSE provides a rich set of outcomes to study the human capital effects of

[7] Questions on parental time were asked separately for mothers, fathers, and other household members in the 2021 survey. Summing these responses does not produce a reliable measure of total parental time because multiple household members may engage in the same activity simultaneously. To address this measurement issue, we construct indicators for whether parental time in each activity during the last seven days exceeds the annual median.

[8] Colleges in Russia are specialized secondary vocational or technical programs that train skilled technicians and mid-level workers for specific occupations. They are closest to associate-degree-type programs in the United States, although they are typically more occupation-specific and may begin after grade 9 or after grade 11. Programs usually last about two to three years after grade 11 and three to four years after grade 9. Examples include medical colleges that train nurses and laboratory assistants; industrial and technical colleges that train technicians; pedagogical colleges that train preschool and primary school teachers; and other colleges that train workers for particular industries.

school closures.

2.3 Measures of Schooling Disruptions

Our measures of schooling disruptions come from the *Schooling Policy Tracker during the COVID-19 Pandemic in Russia* (hereafter, S.P. Tracker), a daily dataset of regional schooling policies by grade level compiled by the authors. The dataset covers 83 Russian regions from September 1, 2019, through January 31, 2023, and is based on more than 1,200 official government documents and media reports on coronavirus-related educational restrictions.

We classify each non-weekend day in the S.P. Tracker into six categories: (1) in-person schooling, (2) no instruction due to COVID-19-related school closures, (3) virtual instruction due to COVID-19-related school closures, (4) scheduled school breaks, (5) non-COVID disruptions such as weather-related closures, security threats, or elections, and (6) federal or regional holidays. We define school closures as days in categories (2) and (3), since both involve the suspension of in-person instruction.

School closure decisions in Russia were largely made by regional authorities, generating substantial variation in both the timing and duration of closures across regions. Figure 1 illustrates this staggered timing during the 2020 and 2021 survey waves, showing that regions entered and exited closure periods at different points in time. This variation in closure timing across regions forms the basis of our identification strategy. The event-time structure used in the stacked DID analysis is described in Section 3.

School closures also varied across grade levels within the same region. This grade-specific variation is particularly important for constructing cumulative measures of school disruptions. For each first-grade entry cohort within a region, we sequentially sum grade-specific closure days across grades attended during the pandemic through the interview date. Figure 2 illustrates how cumulative exposure differs across cohorts. Students who entered first grade around 2014–2015 experienced the highest cumulative exposure, as they were enrolled in middle grades during the most restrictive periods of the pandemic, when closures were both longer and more frequent. Older cohorts accumulated fewer disruptions because many had already left school, while younger cohorts remained enrolled but were less exposed because elementary grades faced fewer closures. Consequently, children within the same region experienced different total instructional losses depending on their grade progression during the pandemic. Figure 3 further shows that these cohort differences were amplified by substantial regional variation in closure policies, generating wide disparities in cumulative instructional losses even among students who entered school in the same year.

2.4 Control Variables

Because school closures were often implemented alongside other pandemic-related disruptions, isolating their independent effects on children's educational outcomes requires accounting for both predetermined household characteristics and concurrent regional shocks. We therefore include a rich set of controls at the child, household, and regional levels.[9]

At the individual level, covariates include child gender and age categories. At the household level, we account for whether at least one parent had a university degree before the pandemic, pre-exposure household size, and household location type (regional centers, other cities and towns, or villages). To avoid capturing temporary changes in household composition during school closures, such as grandparents moving in to provide childcare, household size is based only on permanent members who have lived in the household for at least six months. In heterogeneity analysis, we also use pre-pandemic internet access and pre-pandemic household income. The latter is measured using the most recent inflation-adjusted household monetary income observed between 2017 and 2019 to avoid capturing pandemic-related income shocks. These specifications use a smaller sample because pre-pandemic income is not observed for all households.

[9] Appendix Table A2 provides detailed definitions of all variables used in the analysis.

At the regional level, controls capture broader economic and public health conditions that may independently influence children's educational investments and schooling decisions. These controls include poverty and unemployment rates, as well as COVID-19 spread measured using confirmed coronavirus cases and deaths per 100 people. For short-run estimates, we use new confirmed cases during the 30 days preceding the interview date. For educational trajectories, we use three-month cumulative cases to capture recent local transmission intensity and cumulative deaths up to interview date to capture overall pandemic severity.

School closures in Russia frequently coincided with other government responses to the pandemic. To isolate the effects of school closures from other COVID-19-related policies, we additionally account for regional workplace and daycare closures that may independently affect parental time allocation, sibling caregiving demands, and access to extracurricular programs used by younger children. We also account for scheduled interruptions to instruction unrelated to the pandemic, such as school breaks and long holidays. All these measures are constructed from daily regional policy records and merged to children based on their region, interview date, and, where applicable, first-grade entry cohort or current grade level.

3 Immediate Effects of School Closures on Human Capital Formation

Before examining longer-term educational trajectories, we first assess whether school closures generated immediate disruptions in academic performance and educational inputs. Detecting short-run effects helps establish whether closures created an initial shock to the learning process.

3.1 Stacked DID Design

In estimating the short-term consequences of school closures, we rely on the irregular nature of school disruptions in Russia during the COVID-19 pandemic. Regional governments repeatedly shifted between in-person and remote instruction, generating multiple episodes of school closures that differed in timing and duration across regions. These disruptions were temporary rather than permanent, with regions moving in and out of closure regimes multiple times throughout the pandemic period. This institutional setting provides useful variation for identifying short-term effects. At the same time, it makes standard staggered adoption DID designs unsuitable for our context because treatment is non-absorbing and nearly all regions were exposed to closures at some point during the pandemic. In particular, the nationwide school shutdown in Spring 2020 means that no region remains untreated throughout the pandemic period, leaving no suitable set of never-treated regions that could serve as a control group.[10]

To address these challenges, we estimate short-term effects using a stacked event-study framework that treats each closure episode as a distinct event. Our empirical design closely follows our prior analysis of school closures and parental labor supply, which uses the same institutional setting and event construction strategy (Peter and Suvorov, 2026). This approach follows recent work emphasizing the importance of avoiding invalid treatment comparisons in settings with staggered or repeated policy changes (e.g., Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021).

Using the School Policy Tracker for 2020–2022, we define a closure episode as a consecutive sequence of instructional days during which schools operated remotely or were fully closed.[11] Episodes occurring within a one-month window are treated as a single closure episode, although duration is measured using only actual closure days. We ensure that all remaining episodes are separated by more than one month. To focus on meaningful disruptions to schooling, we restrict the analysis to closure episodes lasting more than three instructional days, thereby excluding brief

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[10] Spring 2020 is excluded from the short-term event-study analysis because RLMS-HSE interviews are conducted in the fall and do not capture outcomes during the nationwide spring shutdown. However, this period is incorporated into our cumulative exposure measures discussed in Section 4.1.

[11] Weekends, national holidays, and scheduled school breaks are excluded from this calculation. Thus, closures occurring immediately before and after a weekend are treated as part of the same episode rather than separate events.

administrative interruptions. Using this definition, we identify 50 closure episodes that overlap with the RLMS-HSE survey period.

For each school closure episode (or stack), we construct a control group using regions that did not experience school disruptions from four months before the start of treatment through two months following the closure episode. This restriction reduces contamination from nearby closure events by ensuring that control regions are not recently exposed to similar disruptions during the preceding four months.[12] However, it does not fully eliminate concerns about lingering effects from prior disruptions. For example, if households continue to adjust educational investments or children experience persistent learning disruptions after previous school closures, some control regions may still be partially affected.

The event time is defined at the monthly level for pre- and post-treatment periods relative to each closure episode.[13] We analyze educational indicators for two months prior to treatment (-2, -1), the closure period itself (SC), and two months following reopening ($+1$, $+2$). Observations outside this window are excluded. The timing of RLMS-HSE interviews, primarily conducted in the fall, limits the feasible pre- and post-treatment horizon to a maximum of two months.

Taken together, these restrictions imply that our stacked DID estimates capture responses to new closure shocks over a relatively short horizon. We estimate these effects using the following pooled event-study specification:

$$Y_{iret} = \sum_{j \neq -1} \beta_j \cdot [D_{ret}^{j} \cdot Treated_{re}] + \pi X_{irt} + \alpha_{re} + \lambda_{te} + \varepsilon_{iret}, \qquad (1)$$

where i indexes children, r regions, e closure episodes (stacks), t the calendar year-month of interview,

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[12] Four months represents the longest feasible pre-treatment exclusion window. Many closure episodes begin in October 2020, and extending the window further back would reach May 2020, when schools nationwide were already closed, leaving no eligible control observations.

[13] We use monthly rather than weekly event time because finer intervals produce sparse cells and unstable estimates.

and j event time relative to the closure episode. $Treated_{re}$ is an indicator equal to one if region r is exposed to closure episode e, and zero otherwise. $D_{ret}^{j} = \mathbf{1}(EventTime_{ret} = j)$ indicates whether region r is j months away from closure episode e in period t, with $j = -1$ omitted as the reference period, and $EventTime_{ret} \in \{-2, -1, 0, +1, +2\}$. Event time is constructed from daily school-closure data at the region-episode level, and individuals are assigned to event-time bins based on their region of residence and interview date.

Y_{iret} represents academic outcomes or educational inputs, including GPA, time spent in extended school day programs, participation in out-of-school enrichment activities, homework hours, reading hours, and out-of-school screen hours. The vector X_{irt} includes the following individual- and region-specific time-varying controls: (1) other school disruptions such as school breaks and extended holidays over the past 30 days; (2) COVID-related workplace and daycare closures, measured as the share of business days over the past 30 days; (3) household characteristics (pre-pandemic parental university education, pre-exposure household size, and location type); (4) child characteristics (gender, age); (5) region-by-year poverty and unemployment rates; and (6) the number of new COVID-19 cases per 100 residents in the month prior to the survey interview.[14] The model also includes region-by-stack fixed effects (α_{re}) and calendar month-by-stack fixed effects (λ_{te}) to account for time-invariant regional differences and common shocks within each closure episode. Standard errors are clustered at the region level within each stack.

The coefficients β_j trace the evolution of outcomes before and after school closures and allow us to assess both immediate disruptions and short-term recovery dynamics. Insignificant coefficients in pre-treatment periods ($j < 0$) provide evidence consistent with the parallel trends assumption.

Because the pooled stacked regression implicitly weights episodes by their sample size, we

[14] All variables are defined in Appendix Table A2, and summary statistics are reported in Appendix Table A3.

reweight observations so that each event contributes in proportion to the size of its treated group. Following Wing et al. (2024), weights are calculated as the share of treated observations within each event-specific estimation sample. This ensures that episodes with larger treated populations receive greater influence in the aggregated estimates.

3.2 Disruptions to Human Capital Formation

Event-study estimates from Equation (1) reveal clear disruptions to children's human capital formation during school closures in Russia. Figure 4 plots event-time-specific treatment effects from two months before through two months after school closures based on Equation (1).[15], while Table 1 reports pooled treatment effects (ATT) for the closure period (SC) and the first post-closure month ($+1$) relative to the entire pre-treatment period $(-2, -1)$. Relative to pre-treatment levels, GPA falls by 0.1 points,[16] time spent in extended day programs declines by 0.1 hours per day, and participation in out-of-school enrichment activities declines by 7.8 percentage points (pp) relative to a baseline participation rate of 52 percent. Figure 4 further shows that participation in enrichment activities remains below pre-closure levels after schools reopen. These findings indicate that school closures disrupted both academic performance and structured learning environments inside and outside the classroom, with no evidence of differential pre-trends for these outcomes.

Homework time also declines by 0.3 hours per day during the closure period. However, Figure 4 suggests caution in interpreting this estimate, as homework exhibits a positive pre-trend in the months preceding closures. This pre-trend may reflect anticipatory academic behavior or seasonal variation in school assignments. As a result, the homework estimates should be interpreted more conservatively than the results for GPA and participation in structured learning activities. Nevertheless, we find no evidence that children substituted lost school time into additional homework

[15] Appendix Table A3 reports the full regression estimates for Equation (1).
[16] Similar declines in student achievement during COVID-19 school closures are documented in Engzell et al. (2021), Bertoletti et al. (2023), Goldhaber et al. (2023), and Haelermans et al. (2022).

during the closure period. Reading time also remains unchanged during closures. Thus, lost instructional time was not redirected toward academic self-investment. However, two months after reopening, homework time rises by 0.23 hours per day, which may reflect delayed academic catch-up.

Surprisingly, out-of-school screen time does not increase during the closure period itself; if anything, the point estimate is negative. Several offsetting factors could explain this result. Time spent on remote instruction likely crowds out recreational digital use, while limited access to devices, particularly when parents are working from home, may further restrict non-school screen time. Increased adult supervision during closures may also limit recreational digital activities. After schools reopen, however, out-of-school screen time increases significantly by 0.35 hours per day, on average. This delayed response suggests that greater exposure to digital learning may have altered children's recreational technology use after closures ended.

In sum, school closures disrupted both academic achievement and structured learning opportunities, while generating little substitution toward alternative forms of human capital investment (consistent with Agostinelli et al. 2022).

3.3 Family Responses and Compensatory Investments

These short-term disruptions raise an important question: to what extent were households able to offset lost school-based inputs through private compensatory investments? In the absence of such responses, the slowdown in children's human capital accumulation may have been even larger. Figure 5 plots event-time-specific treatment effects for family responses, estimated using the same stacked DID specification as in Equation (1).[17] Table 2 reports pooled treatment effects for the closure period (SC) and the first post-closure month (+1) relative to the pre-treatment period (−2,−1). Figure 5 shows no evidence of pre-existing trends in household behavior prior to school closures.

The most immediate response occurs through informal caregiving networks. During the

[17] Appendix Table A3 reports the full regression estimates.

closure period, the probability that children receive care from non-household relatives for more than three days per week increases by 4.5 pp relative to a sample mean of 7.0 percent. Reliance on non-relatives also rises by 3.3 pp from a sample mean of 1.7 percent. These economically large effects suggest that households substituted toward informal caregivers when schools could no longer provide daily supervision. Figure 5 shows that both effects largely disappear after schools reopen, indicating that these arrangements were primarily temporary responses to school disruptions.

Households also respond by increasing market-based educational investments. Spending on children's education rises by approximately 670 rubles (about $10 per month) during the closure period, or roughly 25 percent relative to the sample mean.[18] It appears that some families attempted to purchase substitutes for lost educational inputs through tutoring, supplemental learning materials, or other educational services. However, this spending response is short-lived, as the increase fades after schools reopen.

School closures did not increase parents' direct academic involvement, but they did lead parents to spend more time on other developmental activities with children. Figure 5 and Table 2 show that the probability of above-median time spent on arts, crafts, and technology activities increases by 3.3 pp during the closure period. At the same time, the likelihood that parents spend above-median time helping children with homework remains unchanged, and the probability that parents spend above-median time helping children with additional academic study declines by 6.4 pp during closures. One possible explanation is that schools assigned less academic work during remote instruction, reducing demand for supplementary study at home. Parents may also have shifted time toward childcare logistics, supervision, and remote work.[19] After schools reopen, parents spend more

[18] Private education expenditures are reported at the household level. To maintain a consistent child-level estimation sample, household expenditures are assigned to each child in the household, and event weights are divided by the number of children to avoid over-weighting households with multiple children.

[19] In related work, we show that school closures in Russia increased remote work among employed mothers and shifted work toward evening and night hours, indicating that many parents continued working while reorganizing schedules around childcare responsibilities (Peter and Suvorov, 2026).

time helping children with homework, which may reflect delayed academic catch-up once regular schooling resumes.

Overall, the evidence points to meaningful but imperfect household compensation. Families respond to school closures by relying more heavily on informal care networks and increasing private educational spending, yet they do not immediately replace lost instructional time through greater direct academic involvement, echoing model-based predictions of limited parental compensation in Agostinelli et al. (2022). These responses may have softened the immediate disruption to human capital formation, but they were unlikely sufficient to fully offset the loss of school-based inputs.

3.4 Inequality in Household Responses to School Closures

Parental time and access to financial resources are unevenly distributed across households, raising the possibility that compensatory investments may have widened educational inequalities during the pandemic (Engzell et al. 2021). We therefore examine heterogeneity in responses to school closures along three dimensions: parental human capital, financial resources, and digital access. Specifically, we use parental university attainment, household income, and internet access, measured before the pandemic as baseline characteristics. Let H_{i0} denote one of these baseline characteristics. We estimate the following specification separately for each dimension:

$$Y_{iret} = \sum_{j \geq 0} \beta_j \cdot \left[D_{ret}^{j} \cdot Treated_{re}\right] + \sum_{j \geq 0} \delta_j \cdot \left[D_{ret}^{j} \cdot Treated_{re} \cdot H_{i0}\right] + \gamma H_{i0}$$
$$+ \pi X_{irt} + \alpha_{re} + \lambda_{te} + \varepsilon_{iret}, \qquad (2)$$

where the coefficients δ_j capture differential treatment effects by baseline characteristics at each event time, relative to the omitted pre-closure period, $j < 0$.

To highlight the underlying mechanisms, Table 3 focuses on interaction terms for a subset of outcomes, while interaction terms for other outcomes are reported in Appendix Table A4. Given the rich set of interacted fixed effects, controls, and triple interactions, the specification is demanding, and identification relies on relatively limited within-group variation. As a result, heterogeneity estimates are often imprecisely estimated. For example, we observe that academic performance and homework hours decline during school closures across many groups, with no statistically significant differences between them.

Nonetheless, some differences emerge along specific margins. In a companion paper, we find that university-educated parents do not change their hours but increase their remote work, suggesting tighter time constraints (Peter and Suvorov 2026). In the present analysis, estimates in Table 3 Panel A indicate that university-educated parents are more likely to spend time with children on arts, crafts, and technology, while their children reduce leisure screen time and read more hours per day than children of less educated parents. University-educated parents are also more likely to use non-relatives for childcare, consistent with tighter time constraints. Furthermore, the decline in extracurricular activities is concentrated among children of less educated parents. Overall, the evidence suggests that, in response to school closures, households with higher parental education reallocate children's time toward more structured and development-oriented activities and away from unstructured uses such as screen time.

We proxy financial resources using pre-pandemic household income received in the last 30 days, measured as the most recent available value in real terms, and divide households into income tertiles. Heterogeneity by household income reveals different adjustment margins across the distribution, as shown in Table 3 Panel B and Appendix Table A4. Higher-income households increase private education spending and engagement in arts, crafts, and technology-related activities with children, consistent with substitution toward market-based and development-oriented inputs. Lower-income households exhibit a contraction in inputs: time spent helping with homework declines, and extracurricular participation declines. Other differences are less systematic: there is some evidence of decreased academic performance and lower reading time among children in middle-income households, but these margins do not follow a clear gradient. Overall, higher-income households appear to augment inputs, whereas lower-income households scale them back. These results are consistent with larger learning losses among disadvantaged students (e.g., Engzell et al. 2021; Agostinelli et al. 2022; Haelermans et al. 2022).

Households lacking pre-pandemic internet access (14% of the sample) exhibit more constrained responses to school closures, as evidenced in Table 3 Panel C.[20] Children in these households experience a sharp decline in leisure screen time, likely reflecting reduced access to digital resources previously available through schools or public spaces, along with significant reductions in extracurricular participation and larger increases in reading time, suggesting substitution toward offline activities. In contrast, households with internet access show active compensation. They increase private education spending and rely more on non-relative childcare, while extracurricular participation does not decline significantly in this group. Hence, internet access is associated with greater use of non-school inputs, whereas households without access adjust along a narrower set of margins.

Overall, these results indicate that households respond to school closures along multiple margins in the short run, but the capacity to do so varies systematically with pre-pandemic resources, in line with broader evidence from other countries (e.g., Jack and Oster 2023). Higher parental education, greater financial resources, and internet connectivity each expand the set of available adjustment margins, enabling substitution toward alternative inputs. In contrast, households lacking these resources face tighter constraints and more limited scope for compensatory responses, leading to more unequal investment in children.

4 Educational Trajectories After School Closures

4.1 Outcomes, Exposure, and Empirical Framework

While the previous section documents sizable short-run adjustments in educational inputs, an

[20] Digital access has been shown to be a key constraint on remote learning during the pandemic (e.g., Bacher-Hicks et al. 2021; Bertoletti et al. 2023)

open question is whether these disruptions translate into longer-run effects on children's human capital. Existing evidence on long-term outcomes remains limited, as it is too early to observe impacts of school closures on completed schooling, employment prospects, or earnings,[21] and this is also the case in our setting, where the most exposed cohorts are still in school as of 2024.

However, realized educational status provides an early but consequential measure of longer-run trajectories. We focus on individuals ages 16–22, since age 16 is the typical age at which students are observed after the grade-9 branching point, while age 22 is the oldest age at which individuals observed in 2024 could plausibly have been exposed to school closures while still completing general secondary education. In the Russian education system, students specialize into different educational tracks relatively early: after grade 9, they may either continue in general secondary education (grades 10–11) before entering university or transition into vocational or specialized college tracks. These alternative tracks can also serve as pathways to university, though typically with different timing and a lower probability of progression. Although some movement across pathways remains possible, these choices are only partially reversible and strongly predict future educational progression. Educational status at these ages therefore captures whether students remain on, exit, or redirect longer-run schooling pathways after exposure to school closures.[22]

In contrast to the short-run analysis, which focuses on discrete school-closure episodes and their immediate effects, we now consider cumulative exposure. Specifically, for each cohort c, defined by first-grade entry year, we calculate the total number of school days lost prior to the observation of educational status. We then examine how this exposure affects individuals' educational choices.

Let i index students, r regions, c cohorts, τ academic years measured at the fall interview, and

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[21] Some studies use structural models to simulate the long-term consequences of school closures, calibrated to observed short-run disruptions; see, for example, Fuchs-Schündeln et al. (2022).

[22] Emerging evidence shows that pandemic-related disruptions affected educational progression and persistence, including declines in post-secondary enrollment and re-enrollment (e.g., Schanzenbach and Turner 2022; Dagorn and Moulin 2025).

j the student's schooling status. For each student, we define the outcome as an indicator for each educational category:

$$Y_{ijrc\tau} = 1[S_{irc\tau} = j]$$

where $Y_{ijrc\tau}$ corresponds to one of five mutually exclusive states: (i) general secondary education (grades 10–11); (ii) vocational secondary education; (iii) specialized college (associate-degree-type programs); (iv) university; and (v) not enrolled.[23]

We estimate the following DID specification with continuous exposure to school closures separately for each educational-status outcome:[24]

$$Y_{ijrc\tau} = \alpha_j + \sum_{s=2021}^{2024} \beta_{js} \left(Exposure_{rc\tau} \times \mathbb{1}[\tau = s]\right) + \pi X_{irc\tau} + \delta_r + \theta_c + \mu_\tau + \varepsilon_{ijrc\tau}, \quad (3)$$

where $Exposure_{rc\tau}$ measures cumulative school closure exposure for cohort c in region r prior to the observation of educational status in academic year τ. The measure is constructed from daily school-closure records and scaled by 22 instructional days, so coefficients can be interpreted as the effect of an additional month of instructional closure exposure. Details on the construction of the cohort-specific exposure measure are provided in Section 2.3.

We interact cumulative exposure with indicators for academic years 2021–2024. The estimation sample includes 2017–2020, which helps estimate differences across individuals, regions, and cohorts before and during the first year of school closures. However, exposure is zero for all observations in 2017–2019 and therefore cannot identify exposure-specific coefficients in those years.

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[23] The not enrolled category includes individuals who are not currently enrolled in any educational institution and have not completed post-secondary education, including those who stopped after general secondary school, never progressed to higher levels, attended only short-cycle or non-degree programs, or dropped out of vocational, specialized college, or university tracks.

[24] Because educational status is mutually exclusive, a multinomial logit model is a natural alternative. In practice, this specification is too demanding for our fixed-effects and interaction structure: some outcomes are perfectly predicted within cells, preventing reliable calculation of standard errors. We therefore estimate separate linear probability models, which also allow coefficients to be interpreted directly as percentage-point changes.

We omit the 2020 exposure interaction because 2020 is the first year in which closure exposure is observed; it serves as the normalization year. The coefficients β_{js} therefore show how the relationship between cumulative closure exposure and educational status changes from 2021 through 2024 relative to the initial exposure year. This specification distinguishes effects for cohorts that reached key educational decision points during the pandemic from effects for later cohorts, who were younger when closures occurred and accumulated more instructional losses before reaching those decision points.

The vector $X_{irc\tau}$ includes controls that overlap with those used in Equation (1): (1) household characteristics (pre-pandemic parental university education, pre-exposure household size, and location type); (2) child characteristics (gender and age); and (3) region-by-year poverty and unemployment rates. Several controls are modified for the cumulative-exposure design. Workplace and daycare closures are measured cumulatively, while school breaks and holidays are no longer included. For COVID-19 severity, we use both the cumulative number of deaths and the number of new cases during the three months preceding the interview date. The model also includes region, academic-year, and cohort fixed effects. Standard errors are clustered at the region level.

To assess pre-trends, we estimate an event-style specification that interacts a time-invariant measure of eventual regional closure exposure with academic-year indicators:

$$Y_{ijrc\tau} = \alpha_j + \sum_{s=2018}^{2024} \beta_{js} \left(C_r \times \mathbb{1}[\tau = s]\right) + \pi X_{irc\tau} + \delta_r + \theta_c + \mu_\tau + \varepsilon_{ijrc\tau}, \qquad (4)$$

with 2017 omitted as the reference year. C_r is the cumulative number of school-closure days in region r over the pandemic period and is held fixed across cohorts. This specification tests whether regions with higher eventual closure exposure exhibit differential trends in educational status prior to the pandemic.

4.2 Main Estimates and Heterogeneity

Cumulative school-closure exposure altered educational trajectories primarily by redirecting students across tracks rather than inducing large-scale exit from education. As shown in Figure 6, the most consistent pattern is a decline in general secondary enrollment and a corresponding increase in specialized college enrollment. This shift becomes more pronounced in later years, as the most affected cohorts reach key post–grade 9 choices. By 2024, relative to 2020, an additional month of closure exposure is associated with an 8-pp decline in general secondary enrollment and a 7.1-pp increase in specialized college enrollment. By contrast, vocational secondary education changes little, and university effects are small and imprecisely estimated. Non-enrollment also increases in some years, reaching about 1.8 pp in 2023; although smaller in absolute magnitude, this represents a meaningful increase relative to the baseline non-enrollment rate of 9 percent.

These findings are consistent with several mechanisms. School closures may have reduced academic preparedness, making the general secondary-to-university pathway more difficult. They may also have increased uncertainty about students' ability to catch up, perform well on exams, and successfully enter university. If students and families viewed disrupted schooling as lower quality, closures may have reduced the perceived value of remaining in general secondary education. In this context, specialized college tracks may have offered a more predictable alternative, by providing an earlier specialized credential than the longer general secondary-to-university route.

To assess whether these trajectory shifts reflect pre-existing regional differences rather than school-closure exposure, we examine pre-pandemic trends by eventual regional closure intensity.[25] Appendix Figure A5 provides little evidence that regions with higher eventual closure exposure were already on differential educational trajectories before the pandemic. For general secondary, vocational

[25] Eventual regional closure intensity captures total pandemic-period closure exposure in each region and is held fixed across cohorts and years. This exercise is intended as a validation check rather than an alternative treatment-effect specification.

secondary, specialized college, and non-enrollment outcomes, the pre-pandemic coefficients are close to zero and statistically insignificant. The main exception is university enrollment: regions with higher eventual closure exposure appear to be on a somewhat more positive pre-pandemic trajectory. We therefore interpret the university estimates with caution, although the main university effects are small and imprecisely estimated.

The reallocation documented in Figure 6 is unlikely to affect all students equally. If school closures changed trajectories through academic preparedness, uncertainty, and families' ability to navigate disrupted schooling, the effects should vary with pre-pandemic household resources. For the heterogeneity analysis, we collapse years 2021–2024 into a single period to summarize effects for later exposed cohorts and interact cumulative closure exposure with pre-pandemic parental education and household income.

Table 4, Panel A shows clear heterogeneity by parental university education. Cumulative closure exposure reduces general secondary enrollment for both groups, but the subsequent paths differ. Among students whose parents do not have a university degree, one full month of closure increases specialized college enrollment by 4 pp and non-enrollment by 2.8 pp. Among students with university-educated parents, exposure instead increases university enrollment by 4.8 pp, while non-enrollment does not rise. The *p*-values for cross-group differences reported in Table 4 indicate statistically significant differences for university enrollment and non-enrollment. These differences suggest that higher-educated households may have helped students navigate disrupted schooling and preserve access to higher-education pathways, while students from less educated households were more likely to move into specialized college tracks or leave formal education. These results point to divergence in educational trajectories by parental education.

Table 4, Panel B shows that low- and middle-income students largely reproduce the average pattern: cumulative closure exposure reduces general secondary enrollment and increases specialized college enrollment. Low-income students also experience an increase in non-enrollment. The main difference is among high-income students, whose educational trajectories do not appear to respond significantly to closure exposure. This result is consistent with higher-income families having greater resources to buffer schooling disruptions, although differences across income groups are mostly not statistically significant.

5 Discussion and Conclusion

This paper shows that the costs of school closures extended beyond lost classroom time. In Russia, where school closures varied substantially across regions and grade levels, disruptions affected several margins of children's human capital formation. They reduced academic performance, interrupted structured educational inputs, and led households to substitute only partially through private spending, parental time, and alternative childcare arrangements. These responses were uneven, with more advantaged households being better positioned to offset lost school-based inputs.

The longer-run results suggest that school closures also affected how students moved through the education system. Rather than producing large-scale exit from education, cumulative exposure redirected students across tracks, especially away from general secondary education and toward specialized college programs that train skilled technicians for specific industries. This type of reallocation is consequential in settings where educational specialization occurs early and movement across pathways is only partially reversible. It also underscores that temporary disruptions can have persistent effects when they occur near key transition points.

These findings have implications for education recovery policies. Policies focused only on reopening schools or recovering average learning losses may miss important margins of adjustment. Students exposed to closures may need support precisely when they make consequential schooling decisions, including transitions after basic secondary education. More broadly, the results suggest that recovery efforts should address unequal access to compensatory resources outside school, since households differed in their ability to replace lost school-based inputs and preserve educational options.

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7 Tables

Table 1: Children's Academic Performance and Human Capital Inputs

	GPA	Extended School Day Hours	Out-of-School Enrichment	Homework Hours	Reading Hours	Out-of-School Screen Hours
School Closure (SC)	-0.101***	-0.096*	-0.078**	-0.301***	0.031	-0.123
	(0.036)	(0.055)	(0.037)	(0.094)	(0.054)	(0.150)
Month After Reopening (+1)	-0.054	0.218*	-0.064	-0.030	-0.020	0.352**
	(0.037)	(0.112)	(0.048)	(0.109)	(0.036)	(0.141)
Mean Y	4.142	0.290	0.524	1.927	0.649	1.528
Observations	22,848	25,940	26,046	25,560	25,198	25,359

Notes: Estimates report coefficients from stacked DID models comparing the school closure period (SC) and the first month after schools reopen (+1) to the combined pre-treatment period $(-2,-1)$. The full set of event-time estimates relative to period -1 is reported in Figure 4. The sample includes children ages 6–13 enrolled in school and is restricted to a two-month window around each event. Only school closure episodes lasting more than three instructional days are included. Because there are no never-adopters, the control group consists of observations not treated during each event window, excluding observations from regions that experience closures within four months before and two months after each school closure episode. All specifications include region-by-event and calendar-month-by-event fixed effects. The full set of control variables is described in Section 3.1, and variable definitions are provided in Appendix Table A2. Estimates are weighted by the share of treated observations in the estimation sample. Standard errors are clustered at the region-by-event level. Mean Y reports the sample mean of the dependent variable for each column. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2: Family Responses and Compensatory Investments

	Care by Non-HH Relatives	Care by Non-Relatives	Homework Help	Additional Study Help	Arts/Crafts/Tech	Private Education Spending
School Closure (SC)	0.045**	0.033**	-0.040	-0.064**	0.033*	670.159**
	(0.019)	(0.014)	(0.054)	(0.028)	(0.018)	(317.376)
Month After Reopening (+1)	0.036	-0.029**	0.144***	-0.009	-0.003	422.761
	(0.023)	(0.013)	(0.045)	(0.029)	(0.025)	(334.265)
Mean Y	0.070	0.017	0.476	0.080	0.125	2,690
Observations	26,040	26,045	25,375	25,976	25,949	25,804

Notes: Estimates report coefficients from stacked DID models comparing the school closure period (SC) and the first month after schools reopen (+1) to the combined pre-treatment period ($-2, -1$). The full set of event-time estimates relative to period -1 is reported in Figure 5. The sample includes children ages 6–13 enrolled in school and is restricted to a two-month window around each event. Only school closure episodes lasting more than three instructional days are included. Because there are no never-adopters, the control group consists of observations not treated during each event window, excluding observations from regions that experience closures within four months before and two months after each school closure episode. All specifications include region-by-event and calendar-month-by-event fixed effects. The full set of control variables is described in Section 3.1, and variable definitions are provided in Appendix Table A2. Estimates are weighted by the share of treated observations in the estimation sample. Since private education spending is reported at the household level, weights for this outcome are additionally multiplied by the inverse of the number of children in the household. Standard errors are clustered at the region-by-event level. Mean Y reports the sample mean of the dependent variable for each column. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Heterogeneous Effects of School Closures by Pre-Pandemic Household Characteristics

	GPA	Out-of-School Enrichment	Reading Hours	Out-of-School Screen Hours	Care by Non-Relatives	Arts/Crafts/Tech	Private Education Spending
A. Parental University Education							
1-No University	-0.088	-0.174***	-0.037	0.107	-0.003	-0.003	440.783
	(0.063)	(0.041)	(0.069)	(0.240)	(0.010)	(0.026)	(339.294)
2-University	-0.125**	0.001	0.084	-0.338**	0.067***	0.064**	732.447*
	(0.054)	(0.054)	(0.060)	(0.151)	(0.025)	(0.026)	(414.616)
p-value 2 vs 1	0.685	0.012	0.065	0.032	0.015	0.079	0.397
Observations	22,848	26,046	25,198	25,359	26,045	25,949	25,804
B. Pre-Pandemic Household Income							
1-Low Income	-0.042	-0.161**	-0.019	-0.151	0.030**	-0.031	838.202***
	(0.069)	(0.072)	(0.065)	(0.188)	(0.015)	(0.037)	(292.707)
2-Middle Income	-0.194***	-0.025	-0.129**	0.252	0.006	-0.007	815.996
	(0.060)	(0.065)	(0.058)	(0.168)	(0.018)	(0.035)	(536.435)
3-High Income	-0.041	-0.063	0.132	-0.095	0.081**	0.141***	2,133.627***
	(0.071)	(0.076)	(0.094)	(0.216)	(0.034)	(0.038)	(503.587)
p-value 2 vs 1	0.118	0.232	0.143	0.041	0.268	0.673	0.958
p-value 3 vs 1	0.998	0.397	0.052	0.794	0.181	0.004	0.024
Observations	19,415	22,327	21,525	21,727	22,320	22,225	22,129
C. Pre-Pandemic Internet Access							
1-No Internet	-0.143	-0.182**	0.200***	-0.949***	0.008	0.044	397.313
	(0.112)	(0.092)	(0.073)	(0.336)	(0.013)	(0.031)	(377.706)
2-Internet at Home	-0.087**	-0.059	0.000	0.018	0.039***	0.038*	718.350**
	(0.038)	(0.040)	(0.058)	(0.143)	(0.014)	(0.021)	(330.874)
p-value 2 vs 1	0.651	0.233	0.026	0.005	0.031	0.875	0.340
Observations	22,832	26,030	25,182	25,343	26,029	25,933	25,788

Notes: Estimates are based on the stacked difference-in-differences specification in Equation (2). Coefficients report subgroup-specific marginal effects of school closures, obtained from interactions between event-time indicators, treatment status, and pre-pandemic (baseline) household characteristics. Panel-specific rows present estimated effects for each subgroup, while reported *p*-values test differences relative to the reference group within each panel. All other details on sample construction, controls, fixed effects, and weighting follow Table 2. Results for additional outcomes are reported in Appendix Table A4. Standard errors are clustered at the region-by-event level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

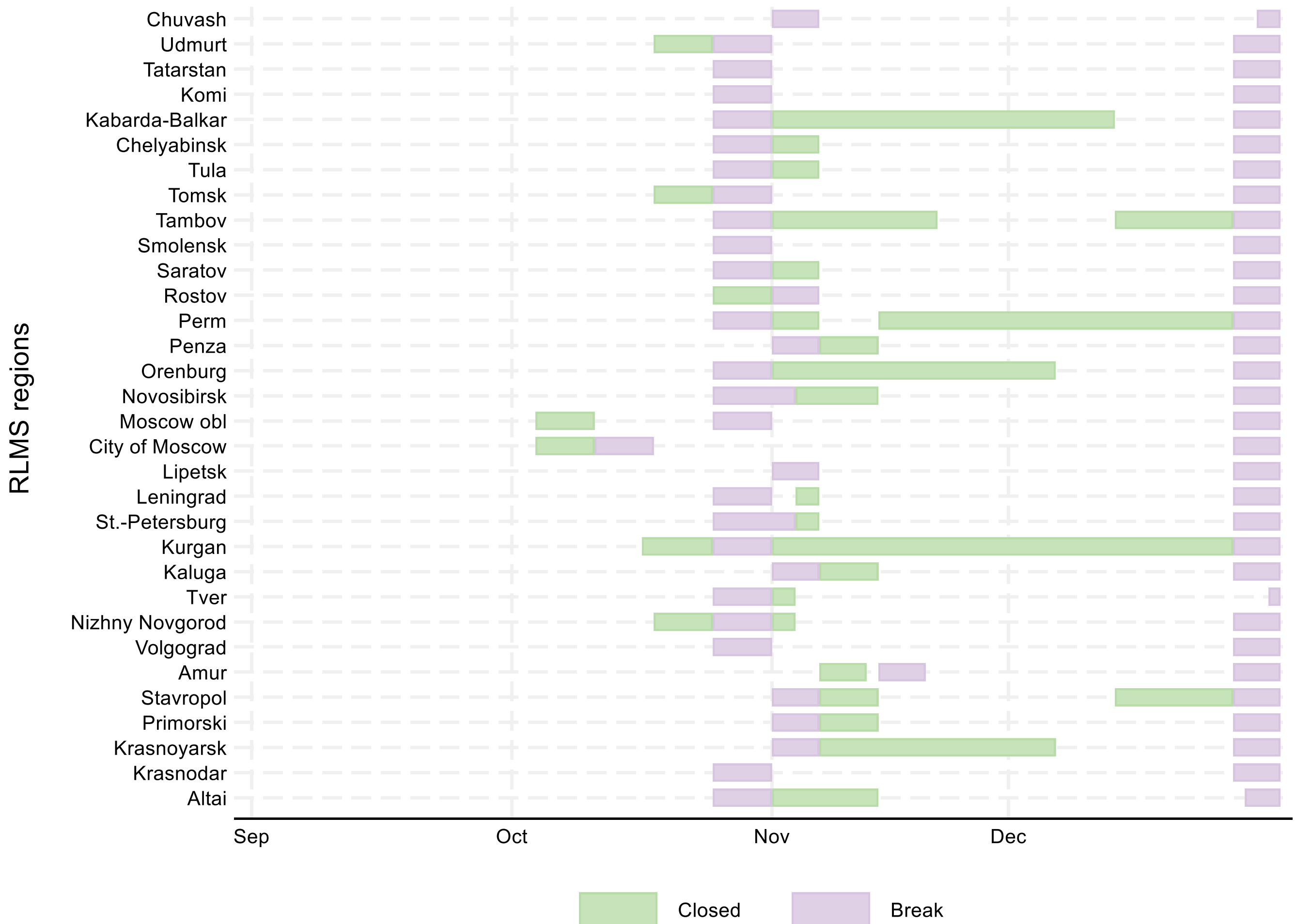
Table 4: Heterogeneous Effects of Cumulative School-Closure Exposure on Educational Trajectories

	General Secondary School	Vocational Secondary School	Specialized College	University	Not Enrolled
		A. Parental University Education			
1-No University	-0.054***	0.002	0.040**	-0.016	0.028**
	(0.010)	(0.011)	(0.017)	(0.012)	(0.011)
2- University	-0.061***	-0.007	0.031*	0.048***	-0.012
	(0.020)	(0.011)	(0.018)	(0.013)	(0.009)
P-value 2 vs 1	0.753	0.589	0.744	0.000	0.017
Mean Dep Var	0.225	0.0863	0.311	0.268	0.110
Observations	10,446	10,446	10,446	10,446	10,446
		B. Pre-Pandemic Household Income			
1-Low Income	-0.032**	-0.017	0.042*	-0.013	0.020*
	(0.013)	(0.015)	(0.021)	(0.015)	(0.011)
2-Middle Income	-0.053**	0.008	0.054**	-0.004	-0.004
	(0.020)	(0.012)	(0.025)	(0.016)	(0.011)
3-High Income	-0.024	0.012	-0.003	0.007	0.007
	(0.017)	(0.010)	(0.026)	(0.026)	(0.013)
P-value 2 vs 1	0.341	0.253	0.770	0.706	0.096
P-value 3 vs 1	0.724	0.099	0.218	0.551	0.423
Mean Dep Var	0.233	0.0869	0.314	0.257	0.109
Observations	9,014	9,014	9,014	9,014	9,014

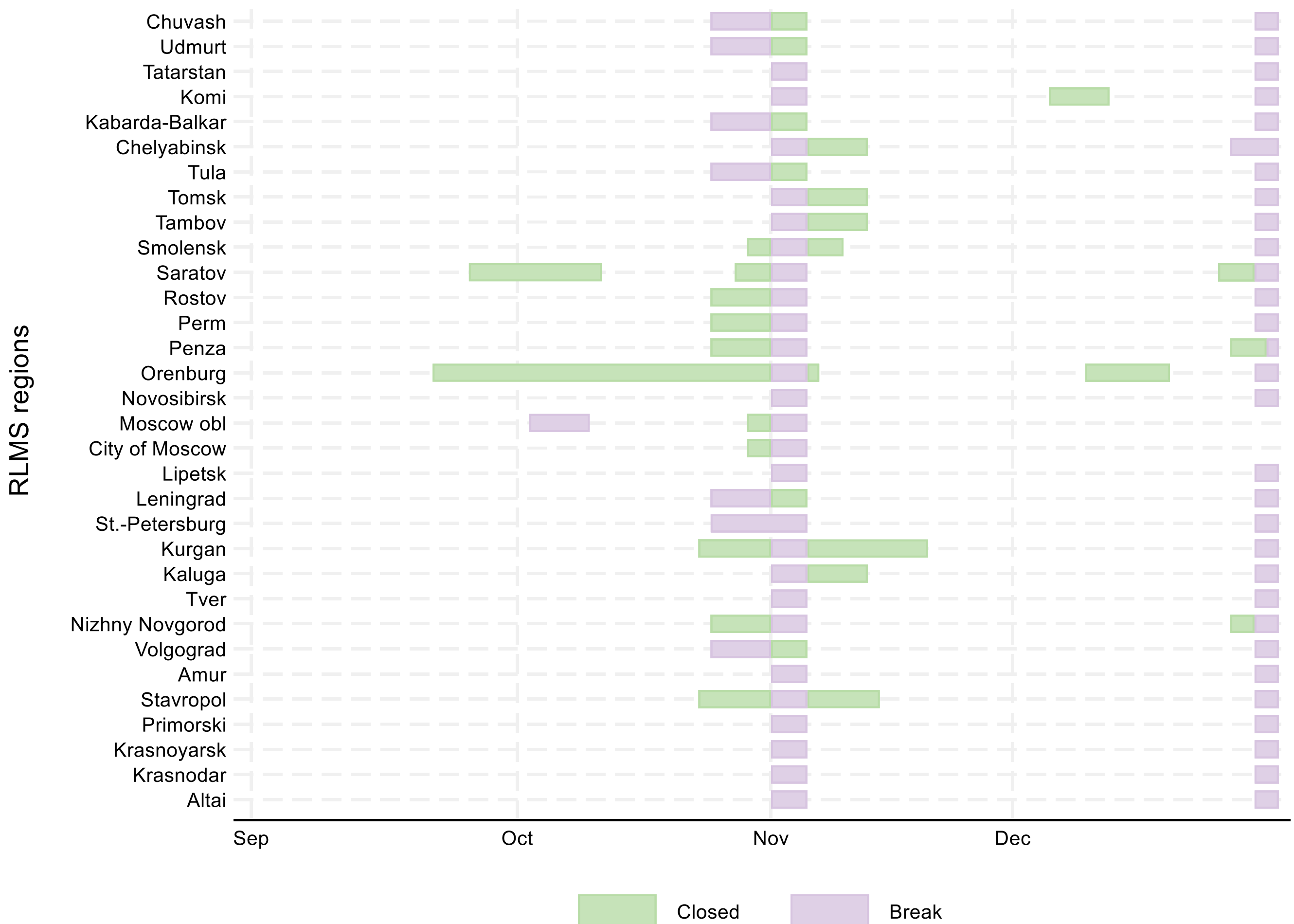
Notes: Estimates report heterogeneous effects of cumulative school-closure exposure on educational-status outcomes. Outcomes are mutually exclusive indicators for general secondary education, vocational secondary education, specialized college, university, and non-enrollment. Exposure is constructed from daily school-closure records, cumulated by region and cohort based on grade-specific exposure, and scaled by 22 instructional days, so coefficients can be interpreted as the effect of an additional month of instructional closure exposure. Panel A reports estimates by pre-pandemic parental university education. Panel B reports estimates by pre-pandemic household income. All specifications include region, cohort, and academic-year fixed effects, as well as the controls used in Equation (3). Standard errors are clustered at the region level. *P*-values test equality of coefficients across groups within each panel. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 1: Timeline of School Closures

A. Fall 2020

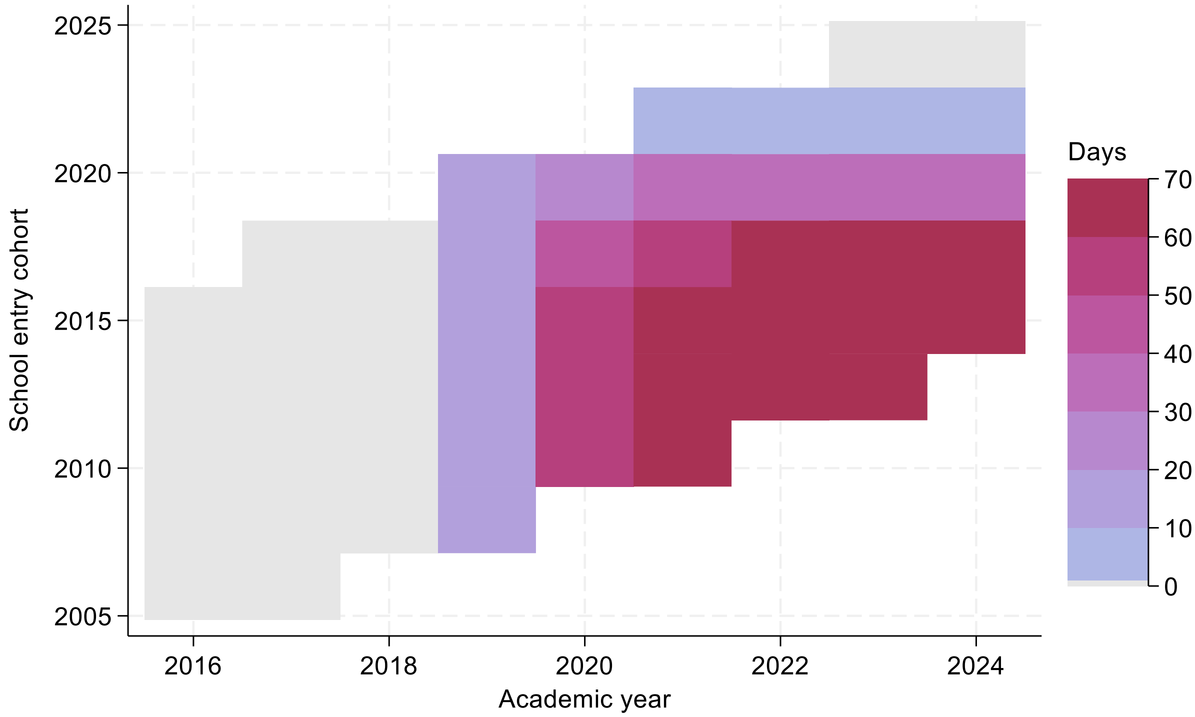


B. Fall 2021



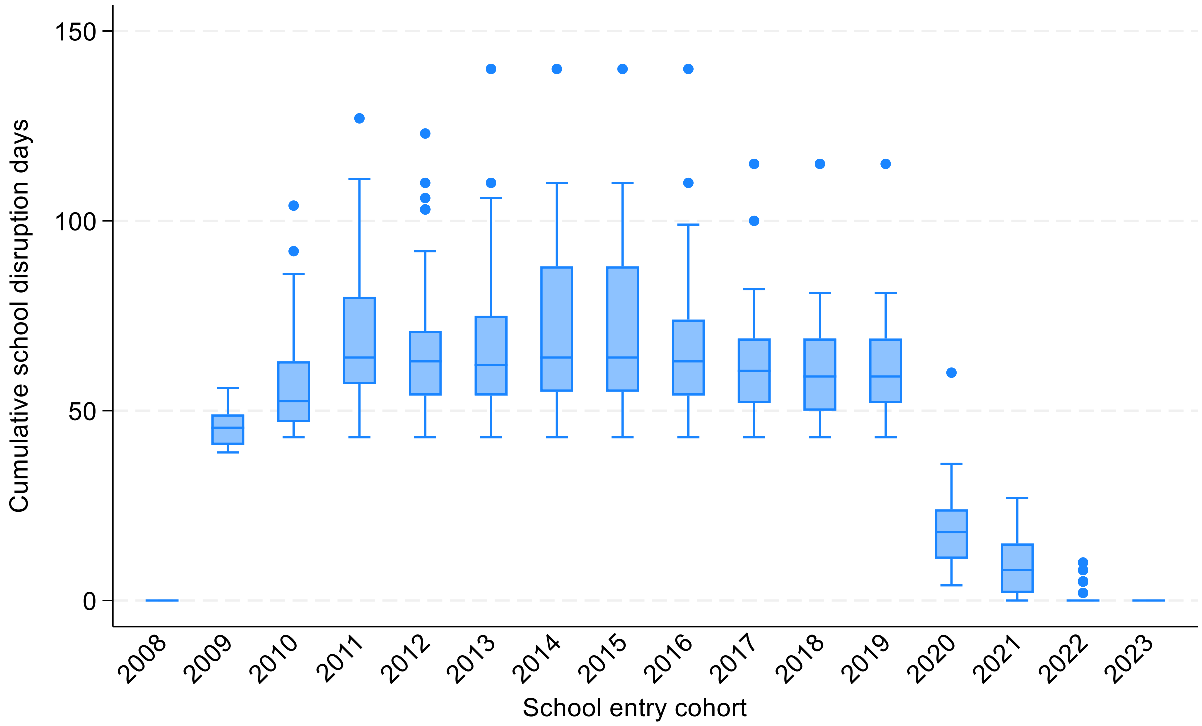
Notes: The figure presents the timeline of school closures and regular breaks for 5th graders in 32 RLMS regions. Panel A covers September 1–December 31, 2020, and Panel B covers September 1–December 31, 2021. Weekends and holidays are excluded from the timeline. Blank periods indicate in-person schooling.

Figure 2: Cumulative School Disruptions by Cohort



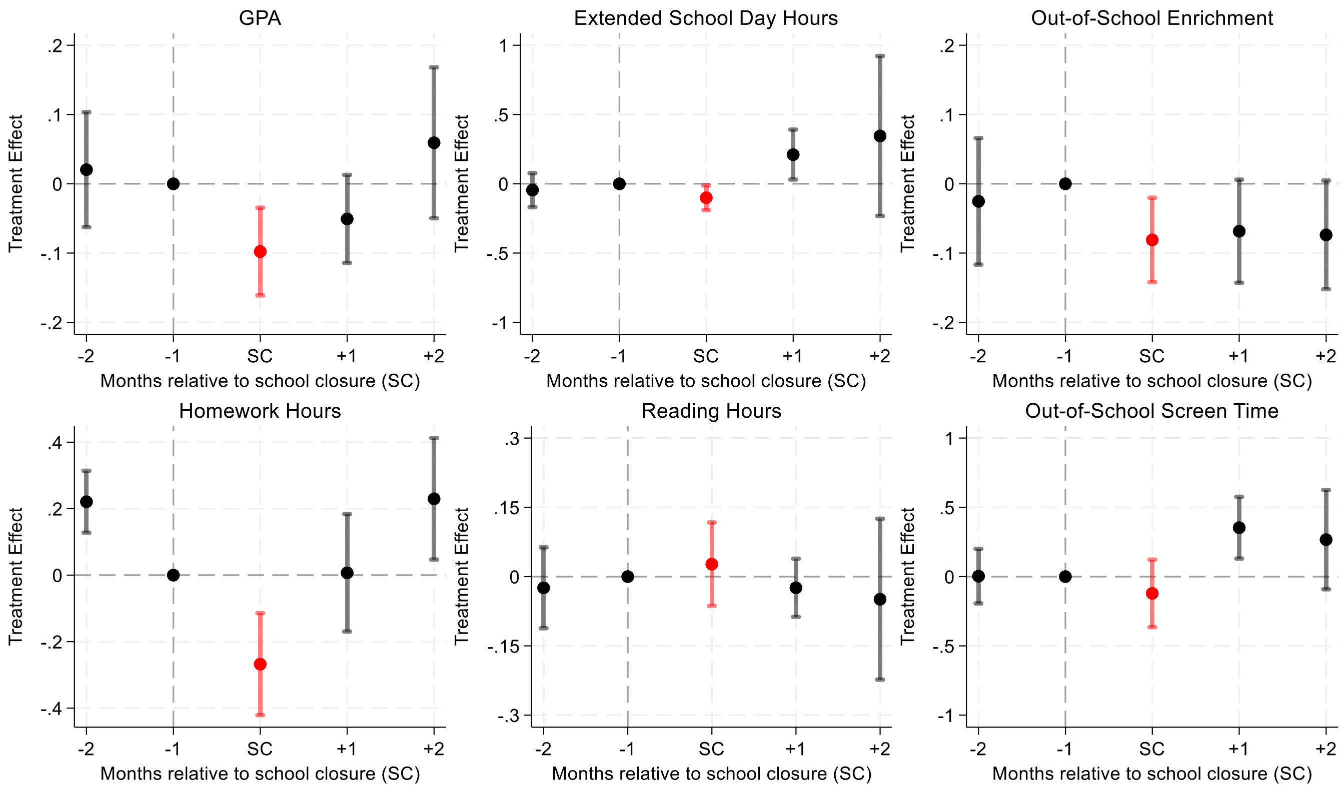
Notes: The figure plots average cumulative school disruption days by first-grade entry cohort and academic year. School disruptions include both full school closures and periods of virtual instruction. The x-axis indicates the start of the academic year (September), while the y-axis denotes the cohort's school entry year. Darker red colors indicate greater cumulative exposure, while light gray cells indicate zero exposure to school disruptions. Blank cells represent cohort-year combinations in which a cohort would not be in grades 1–11.

Figure 3: Regional Variation in Cumulative School Disruption by Cohort



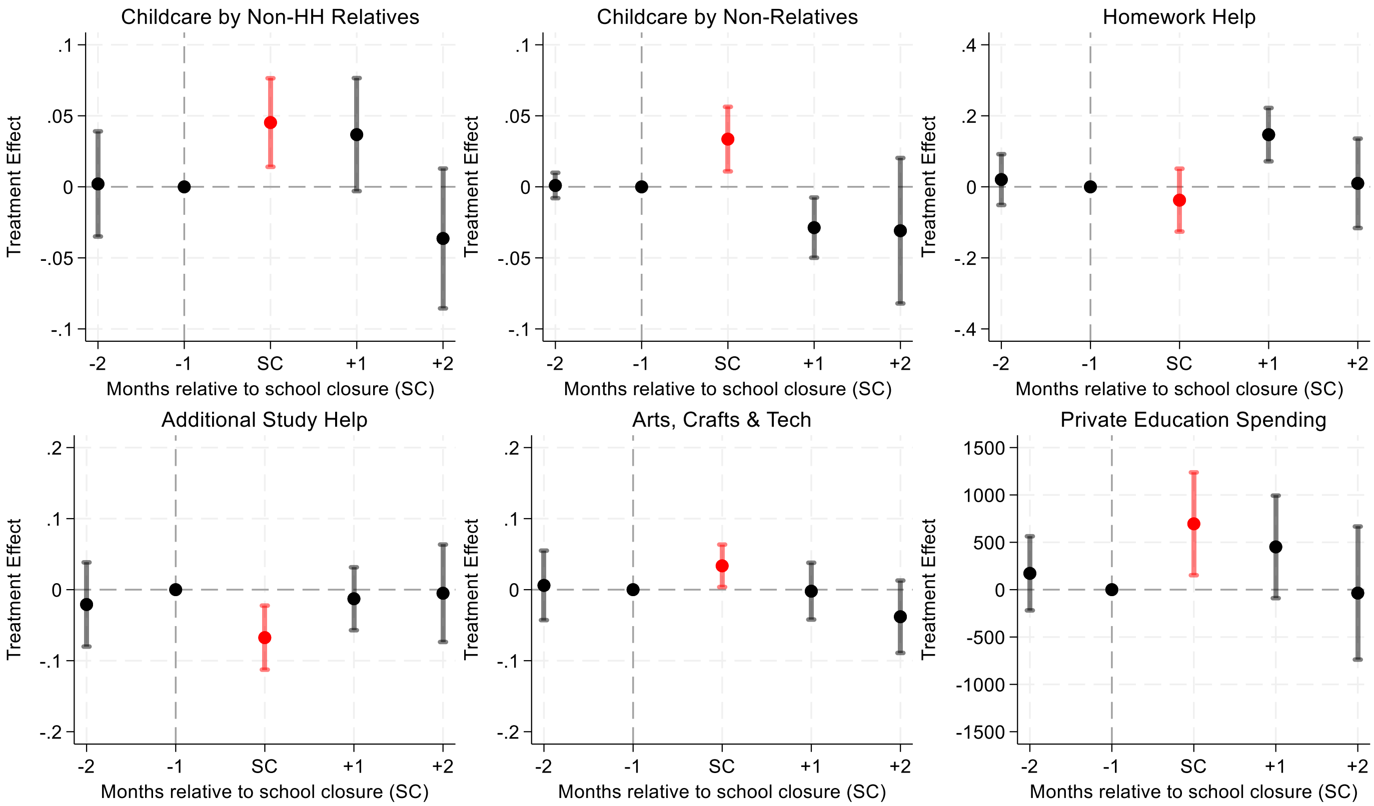
Notes: The figure plots the distribution of cumulative school disruption days across regions for each first-grade entry cohort. For each region-cohort pair, exposure is measured as the maximum cumulative school disruption experienced while the cohort was in grades 1–11. School disruptions include both full school closures and periods of virtual instruction.

Figure 4: Event-Study Effects of School Closures on
Academic Performance and Human Capital Inputs



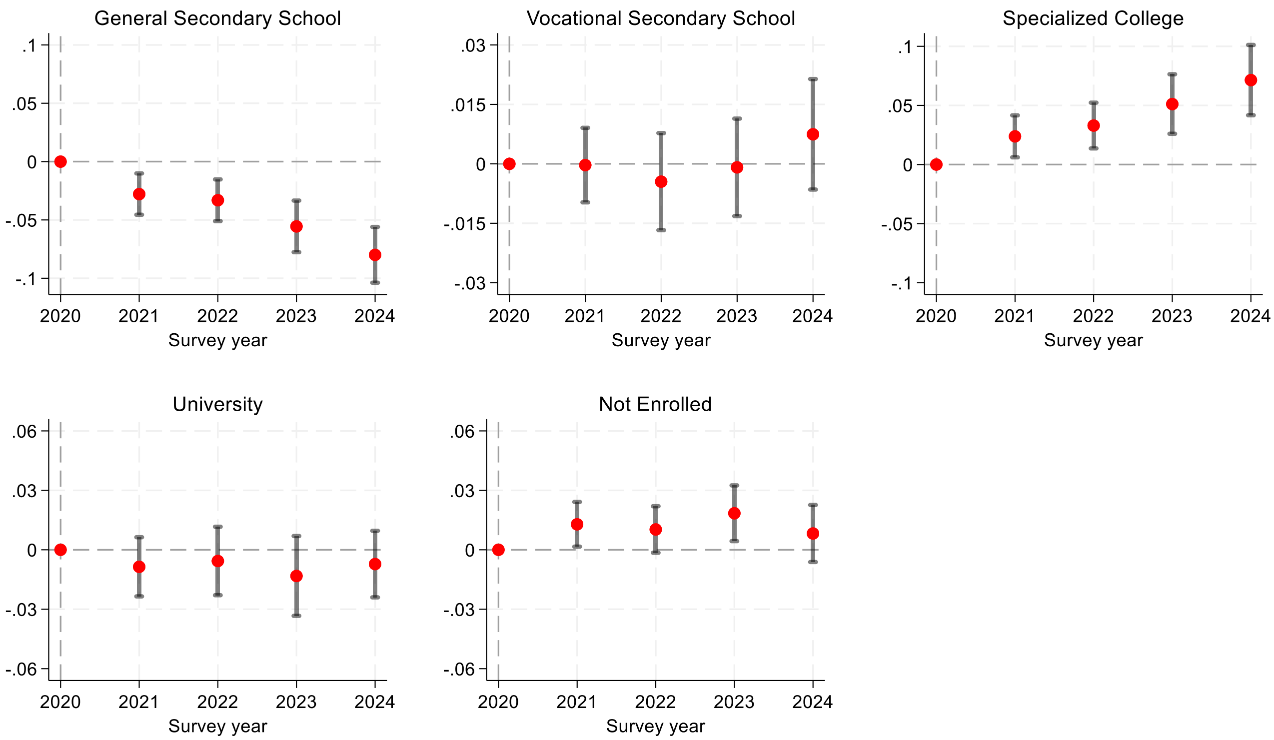
Notes: The figure reports event-time estimates of the effects of school closures on children's academic performance and human capital inputs among children aged 6-13 enrolled in school. Event time is measured in months relative to school closure (SC), with the month before closure (−1) as the reference period. Points represent estimated coefficients from a stacked DID specification in Equation (1), and vertical bars show 90 percent confidence intervals. The sample is restricted to a two-month window around each event and includes only school-closure episodes lasting more than three instructional days. Because there are no never-adopters, the control group consists of observations not treated during each event window, excluding observations from regions that experience closures within four months before and two months after each school closure episode. All specifications include region-by-event and calendar-month-by-event fixed effects. The full set of control variables is described in Section 3.1, and variable definitions are provided in Appendix Table A2. Estimates are weighted by the share of treated observations in the estimation sample. Standard errors are clustered at the region-by-event level.

Figure 5: Event-Study Effects of School Closures on Compensatory Family Investments



Notes: The figure reports event-time estimates of the effects of school closures on compensatory family investments among children aged 6-13 enrolled in school. Event time is measured in months relative to school closure (SC), with the month before closure (-1) as the reference period. Points represent estimated coefficients from a stacked DID specification in Equation (1), and vertical bars show 90 percent confidence intervals. The sample is restricted to a two-month window around each event and includes only school-closure episodes lasting more than three instructional days. Because there are no never-adopters, the control group consists of observations not treated during each event window, excluding observations from regions that experience closures within four months before and two months after each school closure episode. All specifications include region-by-event and calendar-month-by-event fixed effects. The full set of control variables is described in Section 3.1, and variable definitions are provided in Appendix Table A2. Estimates are weighted by the share of treated observations in the estimation sample. Standard errors are clustered at the region-by-event level.

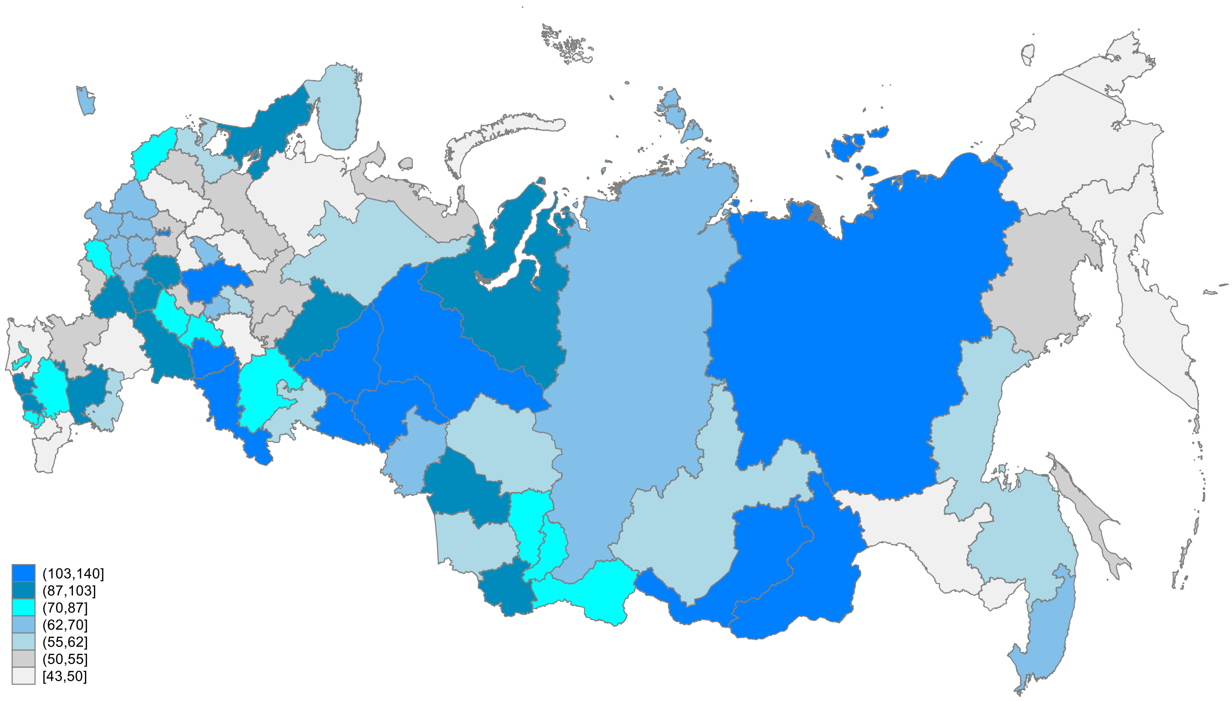
Figure 6: Educational Trajectories Following School Closures



Notes: The figure plots coefficients from regressions of educational outcomes on cumulative school closure exposure (measured in months) interacted with year indicators, controlling for region, cohort, and year fixed effects, as well as additional covariates. School closure exposure varies by region and cohort based on grade-specific closure policies. Outcomes include mutually exclusive categories of current educational status: general secondary school, vocational secondary school, specialized college (associate-degree programs), university, and non-enrollment. The sample includes individuals ages 16–22 observed between 2017 and 2024. School closure exposure is zero in the pre-pandemic period (2017–2019). Coefficients represent the effect of an additional month of school closures relative to 2020. Error bars denote 90 percent confidence intervals. Standard errors are clustered at the region level. Scales differ across panels.

APPENDIX
"Beyond Lost Instruction: School Closures, Household Responses, and Educational Outcomes in Russia"

Figure A1: Geographic Distribution of School Closure Days in Russia



Notes: The map depicts the cumulative number of school closure days through 2023 for students in grades 1–8. Regional variation is substantial, with closure days ranging from 43 in the Republic of Tatarstan (Volga Upland) to 140 in the Kurgan Oblast (Urals). Counts exclude weekends, school breaks, and holidays.

Table A2: Definition of Variables

Variable	Model	Definition
General remarks		1. The *Model* column indicates the empirical specification in which each variable is used. **SR** denotes short-run estimates and **ET** denotes educational trajectory models. **All** indicates variables included in both models. 2. In the RLMS-HSE, the child questionnaire is administered to children under age 14 and is completed by a parent or guardian on the child's behalf. Children ages 14 and older respond directly through the adult questionnaire. 3. The **SR** sample is restricted to children enrolled in school and under age 14 (covered by the child questionnaire), spanning years 2020-2022. 4. The **ET** sample is restricted to young adults ages 16-22, spanning years 2017–2024. 5. Policy variables such as school closures, daycare closures, workplace closures, school breaks, and holidays, as well as COVID-19 spread measures are constructed from daily regional records and merged to individuals based on their region, interview date, and, where applicable, first-grade entry cohort or current grade level.
		Educational Outcomes
GPA	SR	Continuous measure of academic performance on a 2–5 scale constructed from reported grade categories under the Russian grading system, where higher values indicate better academic performance. Children with no grades assigned are coded as missing.
Extended school day hours	SR	Daily hours spent in an extended day program (*prodlyonka*) on school premises, which typically includes homework support and childcare after regular school hours; =0 otherwise.
Out-of-school enrichment	SR	=1 if the child participates in organized enrichment activities outside school, including arts and music, dance and theater, technical clubs, computer training, foreign languages, or supplementary academic subjects; =0 otherwise.
Homework hours	SR	Daily hours spent completing school assignments.
Reading hours	SR	Daily hours spent reading.
Out-of-school screen hours	SR	Daily hours spent using the internet and playing video or computer games outside school, excluding screen time related to school activities.
		Household Compensatory Investments
Care by non-HH relatives	SR	=1 if relatives living outside the household helped care for the child for more than three days during the last seven days; =0 otherwise.
Care by non-relatives	SR	=1 if non-relatives helped care for the child for more than three days during the last seven days; =0 otherwise.
Homework help	SR	=1 if parental time spent helping the child with homework exceeds the annual sample median; =0 otherwise.

Additional study help	SR	=1 if parental time spent helping the child with supplementary academic subjects exceeds the annual sample median; =0 otherwise.
Arts, crafts, & tech	SR	=1 if parental time spent on creative and technology-related activities with the child exceeds the annual sample median; =0 otherwise.
Private education spending	SR	Inflation-adjusted household-level expenditures (in 2024 rubles) during the last 30 days on children's education-related needs, including school fees, extracurricular activities, private lessons or tutoring, and gifts for teachers.
		Educational Trajectories
General secondary school	ET	=1 if the individual is currently enrolled in general secondary education; =0 otherwise.
Vocational secondary school	ET	=1 if the individual is enrolled in or has completed vocational secondary education (PTU/technical track); =0 otherwise.
College	ET	=1 if the individual is enrolled in or has completed a college-level program (tekhnikum; associate-degree–type vocational programs below the university level); =0 otherwise.
University	ET	=1 if the individual is enrolled in or has completed university education; =0 otherwise.
Not enrolled	ET	=1 if the individual is not currently enrolled in any educational institution and has not completed post-secondary education, including those who stopped after general secondary school, never progressed to higher levels, attended only short-cycle or non-degree programs, or dropped out of vocational, college, or university tracks; =0 otherwise.
		Individual Characteristics
Female	All	=1 if the individual is female; =0 otherwise.
Age	All	Age in years.
Grade level	All	Current grade level, as reported in the survey. This variable is highly correlated with age (corr=0.97).
		Household Characteristics
Parental university education	All	=1 if at least one parent has completed a university degree prior to 2020, based on reported graduation dates, and zero otherwise.
Household size	All	Number of household members who have resided in the household for at least six months at the time of the interview, based on reported duration of residence.
Location type	All	Categorized into three groups: regional centers, other cities and towns, villages.
Household income	All	Inflation-adjusted household monetary income received during the last 30 days (in 2024 rubles), measured using the most recent pre-pandemic observation available between 2017 and 2019. Income includes earnings, pensions, scholarships, and other monetary transfers received by all household members. The variable is categorized into tertiles.
Internet access	All	Indicator for whether the household had access to the Internet prior to the pandemic, measured using the most recent observation available between 2017 and 2019.

		Pandemic School Disruption Measures
School closure event time	SR	Timeline around school closure episodes and includes two months prior to closures (**−2**, **−1**), the closure period itself (SC), and two months following school reopening (+1, +2). SC denotes episodes in which schools in the child's region were closed for in-person learning for more than three business days in any grade 1-8. The month immediately preceding school closures (**−1**) is omitted as the reference period. Any school closure episode affecting grades 1–5 is treated as a closure exposure.
Cumulative school closure	ET	Number of days when schools were closed for in-person learning due to COVID-19-related reasons by region-cohort, where cohorts are defined by first-grade entry year. Cohort exposure is constructed by summing grade-specific closure days sequentially through the relevant date. In ET, measured in months as total closure days accumulated through the interview date, divided by 22 instructional days per month.
Daycare closure	All	Number of days when daycare centers were closed for in-person attendance due to COVID-19-related reasons in the child's region. In SR, measured as the share of business days in the last 30 days. In ET, measured in months as total closure days accumulated through the interview date, divided by 22 instructional days per month.
School breaks	SR	Share of business days in the last 30 days when schools were closed due to scheduled school breaks. This measure varies by region, grade level, and interview date.
Long holidays	SR	=1 if a federal or regional holiday lasted longer than one working day during the last 30 days. This measure varies by region and interview date.
		Regional Pandemic and Economic Conditions
Workplace closure	All	Number of days designated as COVID-19-related non-working days in the child's region. Non-working days are declared by either federal or regional governments as paid days off during periods of high coronavirus spread. Except for essential businesses, all enterprises are closed during these periods. In SR, measured as the share of business days in the last 30 days. In ET, measured in months as total workplace closure days accumulated through the interview date, divided by 22 instructional days per month.
COVID-19 deaths	ET	Cumulative confirmed COVID-19 deaths per 100 people by region, measured through the interview date.
COVID-19 cases	All	Confirmed COVID-19 cases per 100 people by region. In SR, measured as new confirmed cases during the last 30 days. In ET, measured as cumulative new confirmed cases during the previous three months through the interview date, respectively.
Unemployment rate, %	All	Regional unemployment rate, measured in percent using the International Labor Organization methodology. This varies by region and year. Data source: Rosstat, https://rosstat.gov.ru/folder/210/document/13211.
Poverty rate, %	All	Share of the population with monetary income below the subsistence minimum, measured in percent. This varies by region and year. Data source: Rosstat, https://rosstat.gov.ru/folder/13723.

Table A3: Full Stacked DID Estimates with Summary Statistics

	Mean (SD)	GPA	Extended School Day	Out-of-School Enrichment	Homework Hours	Reading Hours	Out-of-School Screen Hours
Event time							
-1	0.289	0.023	0.120**	0.032	0.020	-0.010	0.145**
	(0.453)	(0.019)	(0.050)	(0.021)	(0.034)	(0.017)	(0.061)
SC	0.117	0.019	-0.087*	-0.031*	-0.002	0.051***	-0.052
	(0.321)	(0.019)	(0.053)	(0.018)	(0.035)	(0.019)	(0.046)
+1	0.164	-0.006	-0.082	-0.025	-0.032	0.086***	-0.236***
	(0.370)	(0.026)	(0.058)	(0.023)	(0.037)	(0.027)	(0.059)
+2	0.046	0.018	0.012	-0.054	-0.104	0.213***	-0.305***
	(0.210)	(0.044)	(0.136)	(0.042)	(0.072)	(0.045)	(0.106)
(-1) x Treated	0.018	0.020	-0.046	-0.025	0.221***	-0.024	0.003
	(0.132)	(0.051)	(0.075)	(0.056)	(0.057)	(0.053)	(0.120)
(SC) x Treated	0.014	-0.098**	-0.101*	-0.081**	-0.268***	0.027	-0.121
	(0.119)	(0.039)	(0.054)	(0.037)	(0.094)	(0.055)	(0.149)
(+1) x Treated	0.013	-0.051	0.211*	-0.068	0.007	-0.024	0.353***
	(0.112)	(0.039)	(0.110)	(0.045)	(0.108)	(0.038)	(0.136)
(+2) x Treated	0.004	0.059	0.345	-0.074	0.230**	-0.049	0.267
	(0.062)	(0.066)	(0.352)	(0.048)	(0.111)	(0.106)	(0.218)
Female	0.500	0.274***	-0.055***	0.223***	0.159***	0.103***	-0.236***
	(0.500)	(0.009)	(0.016)	(0.007)	(0.011)	(0.008)	(0.023)
Child age	10.582	-0.040***	-0.052***	-0.011***	0.136***	0.031***	0.176***
	(1.801)	(0.002)	(0.007)	(0.002)	(0.004)	(0.002)	(0.004)
Parental university	0.528	0.207***	-0.047**	0.120***	0.159***	0.099***	-0.121***
	(0.499)	(0.009)	(0.020)	(0.009)	(0.020)	(0.009)	(0.020)
Household size	4.632	-0.009***	0.002	-0.019***	0.013**	-0.000	-0.009
	(1.827)	(0.003)	(0.006)	(0.002)	(0.005)	(0.002)	(0.007)
Location type							
Other cities	0.287	-0.015	0.063	-0.034	0.317***	-0.089***	0.101**
	(0.452)	(0.020)	(0.050)	(0.021)	(0.057)	(0.022)	(0.047)
Villages	0.276	-0.091***	0.072**	-0.180***	0.077	-0.086***	-0.334***
	(0.447)	(0.013)	(0.028)	(0.014)	(0.064)	(0.024)	(0.039)
School breaks	0.097	0.148***	-0.299***	-0.411***	0.088	0.010	-0.963***
	(0.126)	(0.046)	(0.088)	(0.060)	(0.093)	(0.050)	(0.179)
Daycare closure	0.017	0.092	0.112	0.705***	0.311***	-0.293***	0.215
	(0.061)	(0.102)	(0.114)	(0.162)	(0.085)	(0.100)	(0.203)
Workplace closure	0.015	0.623**	-1.568**	-0.591**	1.384***	-1.428***	-1.333**
	(0.048)	(0.286)	(0.758)	(0.263)	(0.373)	(0.262)	(0.582)
Long holidays	0.131	-0.066***	-0.328***	0.124***	-0.250***	0.069***	0.288***
	(0.338)	(0.024)	(0.042)	(0.023)	(0.031)	(0.019)	(0.047)
COVID-19 cases	0.292	0.259***	0.226*	-0.069	0.076	0.354***	-0.154
	(0.290)	(0.039)	(0.132)	(0.042)	(0.087)	(0.050)	(0.141)
Constant		4.252***	0.839***	0.640***	0.232***	0.166***	0.169**
		(0.030)	(0.086)	(0.023)	(0.065)	(0.030)	(0.077)
Observations	22,848	22,848	25,940	26,046	25,560	25,198	25,359

R-squared	0.238	0.106	0.174	0.205	0.116	0.183

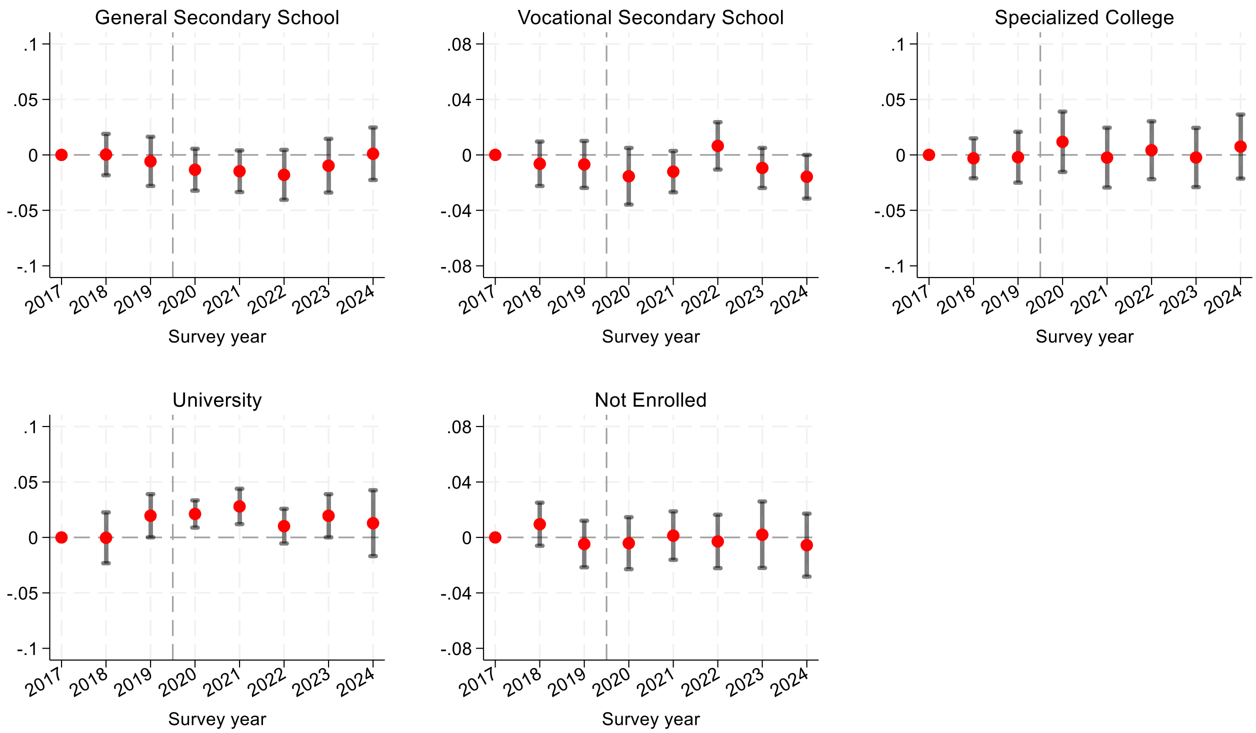
Notes: Estimates report coefficients from the stacked difference-in-differences specification in Equation (1). The sample includes children ages 6–13 enrolled in school and is restricted to a two-month window around each event. The first column reports the sample mean and standard deviation of covariates used in the GPA specification. Only school closure episodes lasting more than three instructional days are included. Because there are no never-adopters, the control group consists of observations not treated during each event window, excluding observations from regions that experience closures within four months before and two months after each school closure episode. All specifications include region-by-event and calendar-month-by-event fixed effects. The full set of control variables is described in Section 3.1, and variable definitions are provided in Appendix Table A2. The omitted category is regional capital (location type). Estimates are weighted by the share of treated observations in the estimation sample. Standard errors are clustered at the region-by-event level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4: Heterogeneous Effects of School Closures by Pre-Pandemic Household Characteristics

	Extended School Day	Homework Hours	Care by Non-HH Relatives	Homework Help	Additional Study Help
	A. Parental University Education				
1-No University	-0.190	-0.282**	0.042*	-0.011	-0.038
	(0.155)	(0.117)	(0.024)	(0.069)	(0.023)
2- University	0.006	-0.319***	0.045*	-0.060	-0.089**
	(0.119)	(0.097)	(0.026)	(0.056)	(0.037)
P-value 2 vs 1	(0.438)	(0.718)	(0.928)	(0.471)	(0.104)
Observations	25,940	25,560	26,040	25,375	25,976
	B. Pre-Pandemic Household Income				
1-Low Income	-0.258	-0.301***	0.062*	-0.123*	-0.067***
	(0.179)	(0.091)	(0.034)	(0.070)	(0.023)
2-Middle Income	-0.373**	-0.238	0.016	0.068	-0.058
	(0.180)	(0.178)	(0.028)	(0.082)	(0.051)
3-High Income	0.170	-0.509***	0.051	0.063	-0.090**
	(0.187)	(0.134)	(0.046)	(0.078)	(0.037)
P-value 2 vs 1	0.380	0.730	0.302	0.037	0.839
P-value 3 vs 1	0.218	0.162	0.864	0.086	0.557
Observations	22,243	21,886	22,319	21,738	22,272
	C. Pre-Pandemic Internet Access				
1-No Internet	-0.077	-0.163	0.055	0.012	-0.010
	(0.250)	(0.129)	(0.042)	(0.103)	(0.037)
2-Internet at Home	-0.103*	-0.317***	0.046**	-0.045	-0.072**
	(0.054)	(0.103)	(0.021)	(0.056)	(0.028)
P-value 2 vs 1	(0.919)	(0.319)	(0.846)	(0.574)	(0.046)
Observations	25,924	25,544	26,024	25,359	25,960

Notes: Estimates are based on the stacked difference-in-differences specification in Equation (2). Coefficients report subgroup-specific marginal effects of school closures, obtained from interactions between event-time indicators, treatment status, and pre-pandemic (baseline) household characteristics. Panel-specific rows present estimated effects for each subgroup, while reported p-values test differences relative to the reference group within each panel. All other details on sample construction, controls, fixed effects, and weighting follow Table 2. Results for other outcomes are reported in Table 3. Standard errors are clustered at the region-by-event level. *** p<0.01, ** p<0.05, * p<0.1.

Figure A5: Pre-Trends in Educational Status by Eventual School-Closure Intensity



Notes: Estimates plot coefficients from event-style regressions (4) of educational-status outcomes on eventual regional school-closure intensity interacted with survey-year indicators. Eventual school-closure intensity is measured as the cumulative number of school-closure days in each region over the pandemic period, scaled by 22 instructional days. The omitted year is 2017. Outcomes are mutually exclusive indicators for general secondary school, vocational secondary school, college, university, and non-enrollment. All specifications include region, cohort, and academic-year fixed effects and the controls used in Equation (3). Standard errors are clustered at the region level. Vertical bars show 90 percent confidence intervals. The vertical dashed line marks the onset of the pandemic period.