When Schools Close in Russia: Effects on Parental Labor Supply and Work from Home*

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ABSTRACT: This study provides new evidence on the impact of COVID-19 school closures on parental labor supply in Russia. Using data from the 2017-2022 Russia Longitudinal Monitoring Survey and newly-assembled data on grade-specific regional school closures, difference-in-differences (DD) estimates show that school closures induced a 10-15 percent decline in employment among mothers of school-aged children. Mothers also increase remote work and short-run evening/nighttime work in response to school closures. These results are robust to accounting for heterogeneous and dynamic treatment effects. In contrast, fathers' labor supply was largely unaffected. Our findings highlight strong gendered effects to school closures in Russia.

KEYWORDS: school closure, labor supply, work from home, COVID-19, heterogeneous treatment effects, stacked difference-in-difference, event study, Russia.

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1 Introduction

The COVID-19 pandemic brought school closures to the forefront as a widely implemented policy to mitigate virus spread. With the possibility of future pandemics (e.g., Morens and Fauci 2020), understanding the impact of such measures has become increasingly important. School closures impose sudden and binding childcare constraints on parents while largely leaving labor demand unchanged, providing a natural setting to study how households adjust labor supply to childcare shocks. These adjustments may occur along multiple margins, including employment, hours, and work arrangements, and are likely to vary across families depending on marital status, children's age, and other characteristics. This paper examines how parental labor supply responds to school closures in Russia.

Prior research has predominantly focused on the U.S. and other high-income countries (Amuedo-Dorantes et al. 2023, Hansen et al. 2024). This study broadens that geographic scope by focusing on Russia. Several features of Russia's COVID-19 policy make it particularly well suited for analyzing parental labor supply responses to COVID-19-related school closures. Russian regional governments enacted school policies independently, resulting in substantial regional variation in the timing and duration of closures. At the same time, workplace shutdowns and stay-at-home orders were imposed largely at the federal level, facilitating a cleaner separation of school closure effects from other potentially related pandemic policies. In addition, unlike the U.S., where homeschooling surged to 11.1% in fall 2020 and hybrid schooling was common according to the U.S. Census Bureau's Household Pulse Survey, homeschooling in Russia remained rare (<1%) (Petryaeva et al. 2024), and no region in our data implemented hybrid schooling during the period we study. As a result, regional school closures affected nearly all parents with children in the relevant age groups.

While Russia is not a high-income country, many characteristics of its labor market and population resemble those of developed economies, including near gender parity in labor force participation and relatively low prevalence of multigenerational households.[1] However, the gender gap in unpaid domestic and care work is substantially larger than in Western economies, though smaller than in many Asian, African, and Latin American countries.[2] Pre-pandemic remote work in Russia was less common than in many Western economies, but more feasible than in lower-income settings (Dingel and Neiman 2020). Thus, the Russian setting allows us to examine the effects of school closures on parental labor supply in an environment with high female labor force participation and limited extended family support, but low pre-pandemic adoption of remote work and pronounced gender asymmetries in unpaid domestic and care work.

To answer our research questions, we compile a novel dataset that records daily, grade-specific school closure policies, collected directly for each regional decision-making body (governor, regional government, or legislature) from 2020 (at the start of the pandemic) through 2022. In contrast to many existing studies that rely on aggregate or proxy measures of school closures (e.g., anonymized smartphone data) or are limited to early stages of the pandemic,[3] our dataset allows direct measurement of COVID-related school closures, distinguishing them from regular school breaks, extended holidays, or weather-related closures.

The analysis draws from six years of data from the Russia Longitudinal Monitoring Survey-Higher School of Economics (RLMS-HSE), covering the period from 2017 to 2022. This panel survey collects data on parental labor supply and various characteristics for the same individuals and families for three years before the pandemic and three years after its onset. With known interview dates, we are able to align the timing of school closures with the survey's reference periods.

Our empirical strategy exploits variation in the timing of school closures across 32 regions and

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1 Around 15% of parents in our sample cohabit with older generations, which is comparable to the share of multigenerational households in the U.S (Pew Research Center 2022).
2 See World Bank Group Gender Data Portal.
3 Some examples of previously used school closure measures include Google searches (Kong and Prinz 2020), changes in school visits based on mobile phone data (Garcia & Cowan 2024; Hansen et al. 2024), percent of the state's population exposed to school closure (Amuedo-Dorantes et al. 2023).

30 year-months using individual-level panel data. Our use of panel data allows us to estimate two-way fixed effects (TWFE) difference-in-differences (DID) models that include individual fixed effects to control for time-invariant individual unmeasured heterogeneity and isolate within-individual changes in labor supply associated with school closures. We complement this approach with a stacked DID design to avoid potential biases in TWFE estimates that may be caused by heterogeneous and dynamic treatment effects.

We find that moving from full-month in-person instruction to fully closed schools reduces employment among mothers of younger children (those attending grades 1-8) by approximately 10-15 percent. Falsification tests on childless adults and event-study analysis using stacked DID estimates generally support a causal interpretation of these estimates. Additionally, we find that school closures increase working from home among college-educated mothers, and lead to short-run increases in evening/nighttime work, results that are also consistent with the hypothesis that mothers increase childcare provision in response to school closures.[4] In sharp contrast, fathers of younger school-aged children exhibit comparatively smaller labor supply adjustments in response to school closures, suggesting that childcare responsibilities fall disproportionately on mothers. In summary, the gendered response to COVID-19 school closures found in Russia are consistent with findings from several Western countries, including the United States (Hansen et al. 2024), Canada (Lillard et al. 2026), Germany (Imre et al. 2026), and South Korea (Bak et al. 2026).

The remainder of the paper is organized as follows: Section 2 describes the survey data, Section 3 presents the panel data estimation methodology and results, Section 4 reports stacked difference-in-difference model estimates, Section 5 concludes with final remarks.

[4] This finding contributes to the literature on the heterogeneous effects of school closures on parental labor supply (see Goldin (2022), Garcia and Cowan (2024), and Hansen et al. (2024)).

2 Data

2.1 Sample

This study uses data from the 2017-2022 rounds of the RLMS-HSE survey, which averages approximately 16,000 respondents per round.[5] The RLMS-HSE survey is residence-based, and our sample is restricted to individuals who do not move over the sample period during which they are observed. The survey period covers two major COVID-19 waves in Fall 2020 and Fall 2021. Our main estimation sample consists of parents aged 18-60 with school-age children (ages 7-14) attending grades 1 to 8. We focus on younger school-aged children because a substantial share of Russian children (40-50 percent) transition to vocational training after grade 9, and grade 11 is the final year of high school in Russia.[6]

We restrict the sample to persons who respond to at least two survey rounds (given our use of individual fixed effects models). We note that attrition does not pose a significant issue in this sample.[7] For parents with multiple school-age children, we link schooling policies to the youngest student, who typically requires more care.[8] In addition to the main sample, we construct a "placebo group" that should be less affected by school closures: adults without children under age 18.[9]

[5] The RMLS-HSE Survey draws from 38 randomly selected primary sample units from 32 out of 83 federal subjects of the Russian Federation The RLMS-HSE shares similarities with the U.S. Panel Study of Income Dynamics (PSID) in its sampling design. It is based on a stratified multistage random sample that represents the country's overall population, although it does not provide a representative sample for each region.

[6] There are only 200-250 mothers of 9th-11th graders in each year of our sample, rendering separate analyses very minimally powered.

[7] The percentage of singleton observations does not exceed 4 percent of the total 16,102 observations in the initial sample of parents with school-age children.

[8] We also considered an alternative approach that sums all days of school closure across all school-age children in the household. As shown in Appendix Figure A4, in 2020, the duration of school closures differed noticeably only between students in grades 1-4 and those in grades 5-8. Only 356 parents in our sample had children in both groups, and just 82 of them experienced school closures that occurred on different dates. Given this limited overlap, we focus on the youngest school-age child. This approach also simplifies the linking of children's characteristics to the parent.

[9] We also report the results for parents with younger children (under age 6) in Appendix A9.

2.2 Labor Supply Measures

We capture labor supply along both the extensive and intensive margins, as well as the location of work. *Worked* is a dichotomous variable set equal to 1 if the respondent engaged in any paid work in the last 30 days, including both primary and secondary jobs; it is set equal to 0 for those who did not engage in paid work.[10] *Hours* is a continuous variable capturing the total number of hours worked across all jobs in the prior month, including hours worked from home. In our main specifications, we condition work hours on those who report prior-month work and take the natural log of conditional work hours. To capture remote work, we use *Worked from Home*, a binary indicator equal to 1 if the respondent worked from home for at least 10 hours at the primary job in the last 30 days, and 0 otherwise; information on remote work is not available for secondary jobs. Finally, for auxiliary analyses, we create dichotomous indicators for (1) weekend/holiday work and (2) evening/night work to capture non-standard work schedules.[11]

2.3 Measures of Schooling Disruptions

Our measures of schooling disruptions come from the dataset "The Schooling Policy Tracker during the COVID-19 Pandemic in Russia" (hereafter, S.P. Tracker), a daily record of COVID-related regional restrictions by school grade level. The S.P. Tracker covers 83 regions and three academic years from September 1, 2019, to January 31, 2023. The dataset was compiled by the authors based on more than 1,200 official documents and media reports on coronavirus-related educational restrictions.[12]

[10] This measure differs from employment status, as individuals on temporary or sick leave or on vacation are considered employed but not working. However, we use the terms "working" and "employed" interchangeably for simplicity.
[11] Binary indicators for weekend/holiday work and evening/night work equal one if a person worked on a weekend or holiday at least once per month or worked late in the evening or at night at least a few times per month, respectively.
[12] This data collection effort conducted as part of a National Institutes of Health (NIH)-funded project (1R01AG071649-01).

Each non-weekend day in the S.P. Tracker is classified as one of five categories: (1) in-person schooling (schools fully open), (2) no in-person schooling due to COVID-19, (3) scheduled school break, (4) schooling disruptions for non-COVID reasons such as inclement weather, security threats, and elections, and (5) public holidays (federal or regional). For the purposes of this study, we define school closures as days classified as category (2). Moreover, when instruction continues in alternative formats, such as online classes with teachers or studying at home with parents, we consider schools closed when in-person learning is suspended.

In Russia, school closure decisions were primarily made by regional authorities, resulting in substantial regional variation in schooling policies, as illustrated in the map provided in Appendix A1. Figure A2 illustrates the relative number of regions mandating school closures at various stages of the COVID-19 pandemic. Most executive orders on school closure were issued during periods of rising daily COVID-19 cases and deaths. During the initial phase of the pandemic in Spring 2020, all regions closed schools for in-person learning. However, with each subsequent wave, fewer regions enacted such restrictive measures, even amid higher surges in daily confirmed COVID-19 cases. The local school policy variation we exploit occurred during the second and fourth COVID-19 waves, which coincided with the RLMS-HSE fall survey period in 2020 and 2021.[13]

Figure 1 presents a timeline of school closure policies across regions in which respondents were surveyed in the RMLS-HSE in Fall 2020 and 2021 (for grade 5), with each row corresponding to a region and each column to a business day (non-weekend). The figure highlights substantial heterogeneity in both the timing and duration of school closures, with regions entering and exiting closures at different points in time. Closure periods appear as clustered blocks while other periods show predominantly in-person instruction. The figure also illustrates within-region variation over time, which provides the basis for our identification strategy.

[13] During Fall 2022, only two instances of regional-level school closures for flu-like viruses were recorded among RLMS regions, and these closures were also attributed to COVID-related reasons.

We create an individual-level panel that aligns information on school disruptions to parental employment using information on the survey interview date. Because employment questions in the RLMS-HSE refer to the last 30-day period before the interview day, we construct all measures of schooling disruptions for the same period. Our primary measure of school closures, S_{irt}, is the share of business days in prior month t during which grade 1-8 schools were closed in region r (for person i).[14]

Russian school closure policies exhibit considerable variation not only across regions but also across grade levels. As illustrated in Table A3 and Figure A4, middle school students encountered more restrictions on in-person attendance than elementary school students, particularly during Fall 2020, although this grade-level variation became less pronounced by Fall 2021.

2.4 Regional Factors

School closure policies in Russia were frequently enacted alongside other government responses to the COVID-19 pandemic, posing a challenge in isolating the effects of each policy. Only a handful of studies on school closures (prior to this *Special Issue*) have attempted to account for and disentangle the impact of other concurrent COVID-19 restrictions (e.g., Amuedo-Dorantes et al. 2023; Garcia & Cowan 2024; Hansen et al. 2024).

By the time RLMS-HSE survey interviews began in September 2020, most mobility restrictions had been lifted. However, potential confounding effects from workplace closures remain a concern when assessing the relationship between school closures and labor supply. In response to a surge in coronavirus cases in the fall of 2021, Russia declared three business days in early November as "non-working days". Among the 32 RLMS regions, nine extended this federal measure by initiating

[14] Specifically, we calculate the rolling sum of days in each of the five categories (including breaks and holidays) within a 30-day moving interval. The rolling sums for closures and breaks are averaged across grades 1–8 and then divided by the number of business days (30 minus holidays and weekends) within the same interval. These measures of schooling disruptions are then merged with the RLMS-HSE using the interview date and region of residence.

the period of no work as early as October 25 and concluding it as late as November 15. To separate the effects of school closures from those of workplace shutdowns, we control for the 30-day rolling sum of non-working days by region and interview date.

At the regional level, we also control for monthly poverty and unemployment rates, as well as COVID-19 cases (which were often used by local officials as a marker for the timing of school closures). With respect to COID-19 cases, for each region and interview date, we calculate the number of new coronavirus cases per 100 people over the preceding 30-day period. The case counts are taken from the Yandex coronavirus database, compiled from daily government reports published on стопкоронавирус.рф.[15,16]

3 The Effects of School Closures in Russia: Evidence from Panel Data

3.1 Empirical Framework

To analyze the effects of school closures on parental labor supply, we draw a sample of mothers (and then fathers) of school-aged children attending grades 1-8 and estimate the following TWFE DID regression via ordinary least squares (OLS):

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[15] Although this data is widely used by respectable data aggregators such as the World Health Organization, the Coronavirus Resource Center at Johns Hopkins University, and Our World in Data, it is known to severely undercount case numbers and especially deaths. There is a vast discrepancy between the sum of daily cases and the end of the year statistics provided by the Russian Ministry of Healthcare on COVID-19 morbidity and death certificates.

[16] COVID-19 was cited as a primary cause of death on 465,525 death certificates in 2021 alone, corresponding to 316 deaths per 100,000 people per year, one of the highest mortality rates in the world. For the same year, the daily mortality numbers sum to 251,841 deaths. The Ministry of Health Care also published the COVID-19 morbidity rate of 81 illnesses per 1,000 population in 2021. The morbidity statistics count people, not cases; each person is counted once when a coronavirus diagnosis is established for the first time. Even so, morbidity statistics significantly exceed the total number of confirmed COVID-19 cases from daily reports, equal to 50.7 cases per 1,000 population in 2021. Our calculations suggest that daily reports account for only 62 percent of confirmed cases and 54 percent of COVID-19 deaths. However, there is a relatively high correlation (0.72) in annual coronavirus cases across regions between the two data sources. Despite the undercounting, daily data still effectively track the general trajectory of the pandemic, capturing its peaks and troughs. To address the undercounting issue, we adjust the number of reported coronavirus cases over the 30-day period using region-specific discrepancy factors. We acknowledge that our measure of COVID-19 spread may be noisy and should be interpreted with caution.

$$Y_{irt} = \alpha_i + \gamma S_{irt} + \beta X_{irt} + \theta_t + \epsilon_{irt}, \tag{1}$$

where i indexes the parent, r indexes the region, and t is the year-month.[17] We define (Y_{irt}) as labor supply of the parent (employment, natural log of work hours among workers, and working from home), and (S_{irt}) as the share of business days in the last month that schools were closed due to COVID-19 school closures. 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 closure as the share of business days over the past 30 days, (3) parental characteristics (age, age squared, education level, ethnicity, and place of birth), (4) child grade level (grades 1–4 vs. 5–8), and (5) region-by-time poverty and unemployment rates, and the number of new COVID-19 cases per 100 persons.[18] In addition, (θ_t) are year-month fixed effects and (α_i) are individual fixed effects. Because there are no cross-region movers in our sample, our key parameter of interest (γ) will be identified solely by region-specific changes in school closures.[19] Standard errors are clustered at the regional level, and regressions are unweighted.[20]

3.2 TWFE Results

TWFE estimates from Equation (1) are reported in Table 1 for all parents and Table 2 for married parents.[21] Panel A presents results for parents of school-aged children in grades 1-8 (treated group), while panel B shows estimates for adults without children under age 18 (placebo group).

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[17] In alternative specifications, we include day-of-month, month-by-day, and year-by-month-by-day fixed effects to more fully capture high frequency national policy shocks, especially given that we code our treatment variable to be day-specific. The pattern of findings we obtain is qualitatively similar to those reported in the tables below and are available upon request of the authors.

[18] All variables are defined in Appendix Table A6, and summary statistics are reported in Appendix Table A7.

[19] In this case, regional fixed effects are nested within individual fixed effects.

[20] We do not use survey weights because the RLMS-HSE weights are designed for cross-sectional representativeness and not for longitudinal panel estimation. In particular, respondents who move out of the original sampled residence within the same locality receive zero weight (about 40 percent of respondents in our sample). Instead, our specifications control for key demographic and geographic characteristics commonly incorporated into weighting adjustments, including gender, age, education, and location.

[21] Full estimates from the baseline specification with individual fixed effects, corresponding to Equation (1), are reported in Appendix Table A8.

Results are shown separately for men and women within each panel.

Consistent with prior research on women's greater caregiving responsibilities (Bertrand et al. 2015; Blau & Kahn 2007), we find that moving from full in-person instruction (in the prior month) to fully closed schools reduces employment among mothers of younger school-aged children by 10.3 percentage points (pp), or 13.9 percent relative to the mean employment rate (Table 1, panel A, column 1). When we focus on married mothers of school-aged children (Table 2, panel A, column 1), we find similar-sized employment effects (approximately 8.7 pp).

To test whether these findings are driven by unmeasured region-specific time trends associated with school closures and female labor supply, we conduct falsification tests in Panel B of Tables 1-2. For females without children under age 18, we find that school closures are associated with a statistically insignificant 0.05 increase in the probability of employment (Table 1, column 1, Panel B). Similarly, for married females without children, we find no evidence that school closures significantly affect the probability of employment (Table 2, column 1, Panel B). The findings from these falsification exercises suggest a causal interpretation of our findings for mothers of school-aged children.[22] Moreover, when we empirically test for whether our DD estimate for mothers of school-aged children is different from that of childless women, we find that these estimates are both significantly different at the 5 percent level. These findings suggest that COVID-19 school closures widened the employment gap between women with school-aged children and those without children.

In sharp contrast, for fathers of school-aged children, the estimated effect of school closures on employment is non-negative and not statistically distinguishable from zero at conventional levels (Table 1, panel A, column 4). Moreover, for married fathers of school-aged children, the estimated school closure effect is about 50 percent smaller than for married mothers of school-aged children

[22] In Appendix A9, we also report results for parents of children under age 6, which show no statistically significant responses to school closures. The absence of systematic effects in these placebo samples reinforces our confidence that the observed labor supply effects are driven by school closures rather than other COVID-related policies or shocks affecting all households.

(Table 2, panel A, column 4). This finding is consistent with a gendered response to school closures for parents of school-aged children, with fathers in Russia taking relatively less responsibility for childcare.

At the same time, there is little evidence of maternal adjustment to school closures along the intensive margin, as the estimated effect on total work hours (among workers) is close to zero and statistically insignificant (Table 1, column 2, panel A). The effects are also small and statistically insignificant for married mothers (Table 2, column 2, panel B) as well as for all fathers (Table 1, column 5, panel A) and married fathers (Table 2, column 5, panel A).

Although there is no meaningful adjustment in hours, labor supply responses to school closures may occur through changes in work arrangements. We find that mothers of school-aged children exhibit a significantly larger shift toward remote work in response to school closures (Table 1, panel A, column 3) than fathers (Table 1, panel A, column 6), consistent with prior evidence that mothers are more likely to hold jobs with flexible work arrangements (Goldin & Katz 2011; Flabbi & Moro 2012). Placebo tests on females without children suggest a causal interpretation of our remote work estimates for mothers of school-aged children. A similar pattern of results is shown when we examine married mothers (Table 2, panel A, column 3) and fathers (Table 2, panel A, column 6).

Compared to studies in other countries[23], our findings indicate a sharper drop in employment and no net change in hours in the aggregate in Russia. The relatively large employment adjustment may reflect structural features of the Russian labor market, which is characterized by low unemployment and high turnover. Each year, 11–14 percent of respondents in the RMS report changing jobs or occupations. While some U.S. evidence (Garcia & Cowan 2024) finds a shift toward

[23] Some studies report no significant employment changes for fathers (Hansen et al. 2024 for the U.S., Bak et al. 2026 for South Korea) or for either parent (Amuedo-Dorantes et al. 2023 for the U.S.; Imre et al. 2026 for Germany). Others find employment declines for both parents, with stronger effects for mothers (Garcia & Cowan 2024; Hansen et al. 2024). For hours of work, studies generally find a reduction either for both parents (Imre et al. 2026) or for mothers only (Bak et al. 2026).

remote work among both parents, our results suggest that in Russia, this shift occurred primarily among mothers. Our estimates of the magnitude of this shift fall within the range reported in the existing literature (Yamamura & Tsutsui 2021; Hansen et al. 2024).

Next, in Table 3, we explore whether there are heterogeneous effects of school closures by education and the presence of an older person in the household. We note, of course, that these observable dimensions may also be correlated with other (some difficult-to-measure) traits of households and parents that could lead to other interpretations than those shared below. Thus, we view this analysis as descriptive rather than causal or dispositive.

Observed differences by education (measured at baseline before the pandemic) may point to the role of job flexibility or in the opportunity costs of time spent away from the labor market. College-educated mothers exhibit a strong and statistically significant increase in remote work, and the difference relative to less-educated mothers is statistically significant according to the *p*-values reported in Table 3. In contrast, less-educated mothers show no meaningful increase in remote work and a larger and statistically significant decline in employment, although we note that the employment effects do not differ statistically between education groups. Perhaps flexible work arrangements allow higher-skilled mothers to absorb childcare shocks without exiting employment, while less-educated mothers face more rigid job constraints or face lower opportunity costs of childcare.

Household composition provides an additional dimension of heterogeneity. In households without an older household member (measured at baseline before the pandemic), mothers experience a statistically significant decline in employment and a significant increase in working from home following school closures. In households with an older household member, mothers' employment remains largely unchanged, while hours worked decline significantly. However, the difference between the two groups is only statistically significant at the 10 percent level with respect to hours worked for mothers. These findings are consistent with the possibility that the presence of another adult in the household may relax the need to exit employment but does not eliminate time constraints.[24]

4 Event Study Analysis

4.1 Event-Study Setup

In this section, we move from the previous TWFE DID specification to a stacked event-study framework. This approach is closely related to recent DID designs with staggered treatment timing, as both rely on variation in treatment timing across units to construct valid treatment–control comparisons over time (e.g., Callaway and Sant'Anna (2021), Goodman-Bacon (2021); Sun and Abraham (2021)). The key complication in our case is that, rather than a one-time policy shift, school closures in Russia occur in distinct, recurring episodes, with regions repeatedly entering and exiting closure regimes over time. This feature makes a staggered DID approach inappropriate in this context. School closures are non-absorbing and transitory, and only two regions are never treated over the sample period, leaving little scope for stable untreated comparisons.[25] In addition, closure episodes often overlap across regions, limiting the availability of clean control units within each event window.

We therefore organize the analysis around distinct closure episodes. Specifically, we adopt a stacked DID design, in which each episode is treated as a separate event.[26] Each "cohort" (or stack) includes observations from the treated region that implemented a school closure at the same time. Counterfactuals in each "cohort" are drawn from non-treated regions that did not experience a school closure within the interval from four months before the start to two months after the end of the closure period in the treated region.[27] This restriction minimizes the likelihood of, but does not

[24] Appendix A10 reports and discusses additional heterogeneity by child age. Across all subsamples, fathers' responses remain small and statistically insignificant, reinforcing the conclusion that adjustments to school closures are borne primarily by mothers.

[25] Over the full COVID period there are no never-treated regions, since all schools were closed in Russia in Spring 2020. That period is not included in our dataset.

[26] As in Cengiz et al. (2019), we allow treated regions to be treated multiple times, provided they are sufficiently spaced apart, and include these events as part of separate stacks.

[27] Four months prior to closure is the maximum pre-treatment window for which we can exclude previously

eliminate, contamination from closely timed interventions. That is because there are no true "never adopting" jurisdictions to which to restrict the set of counterfactuals. For example, the entire nation underwent a school closure at the pandemic's onset (roughly at the same time); if the dynamic labor supply effects of that prior closure were still unfolding at the time our treatment of interest occurred (largely in Fall 2020), then our research design would still yield "forbidden comparisons" in cohorts used for our stacked DD analysis.[28] However, given the nature of how school closures (and reopenings) in Russia (and in many other Western countries) unfolded during the COVID-19 pandemic, this limitation is important to disclose. A further limitation is that we are only able to capture short-run labor supply effects of school closures. With these limitations in mind, we nonetheless view this robustness exercise as important in potentially reducing bias generated by our TWFE estimates due to dynamic employment effects of school closures across regions.

We begin by creating a list of school closure episodes based on the S.P. Tracker in 2020-2022. Episodes apply to students in grades 1 through 8.[29] We define our treatment as dichotomous, *Prominent Closure*, set equal 1 if the school closure lasted for more than 3 days (at least half of the week), capturing meaningful disruptions to schooling rather than short interruptions; it is set equal to 0 otherwise. We identify 53 distinct school closure events that overlap with the survey window. Because outcomes are measured over the preceding 30 days, event time is defined at the monthly level relative to each closure episode.[30] Specifically, indicators are constructed for two months before the closure (-2, -1), the

treated observations from the control group. Since many school closures occur in October 2020, five months earlier would be May 2020, when all schools were closed.

[28] For instance, if the dynamics of prior closures persist over several months following an earlier closure, our estimates could be biased.

[29] Each episode is defined as a sequence of consecutive school closure days, excluding holidays, weekends, and regular school breaks. For example, if a school closure occurs immediately before and after a weekend, it is considered a single episode. For each region and school year, we record the start date, end date, and duration of each closure episode, where duration is defined as the number of closure days.

[30] Shorter event-time windows (e.g., one- or two-week intervals) yield too few observations per period-event to support reliable estimation.

closure period itself (SC), and two months after the closure (+1, +2).[31]

4.2 Estimation and Weighting

We estimate event-time-specific treatment effects using a pooled regression of the form:

$$Y_{irt} = \sum_{j \neq -1} \beta_j \cdot D^j + \pi X_{irt} + \alpha_r + \mu_e + \lambda_t + \varepsilon_{irt}, \tag{2}$$

where j indexes event time relative to the closure episode, and D^j is set equal to 1 if the event is j months before/after the school closure and 0 otherwise, and j = –1 is the reference period. The vector X_{irt} includes individual and regional controls, identical to those used in the TWFE specification (1). Moreover, we also include cohort ("event stack") fixed effects, μ_e. Because the pooled stacked regression implicitly weights events by their total sample size, we reweight observations so that each event contributes in proportion to the size of its treated group.[32,33] While our approach excludes cohort-by-time and cohort-by-region fixed effects (see Wing et al. 2024), the inclusion of these controls leads to a qualitatively similar pattern of findings. If β_j = 0 for all j<0, this would tend to support the parallel trends assumption.

Figure 2 reports event-time estimates of labor supply responses from Equation (2).[34] For mothers (row 1, column 1), we find evidence generally consistent with the parallel trends assumption,

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[31] Observations outside this window are dropped from the analysis. The timing of the RLMS survey, conducted in the fall, limits the available pre- and post-treatment periods. Given that most closure episodes occur in late October, a two-month window captures the longest feasible horizon around each event.

[32] This is consistent with approaches that aggregate treatment effects across events using weights based on the distribution of treated units (e.g., Wing et al. 2024).

[33] Specifically, within each event the weight is computed as the share of treated observations in the estimation sample. These weights ensure that events with larger treated populations receive greater influence in the aggregated estimates. All specifications are estimated separately by gender, allowing for heterogeneous responses to school closures across mothers and fathers. Standard errors are clustered at the region level within each stack.

[34] We show 90 percent confidence intervals, reported separately for females and males, with solid lines corresponding to parents of children in grades 1–8 and dashed lines to married adults without children under age 18 (placebo group). Given the limited number of clusters (32 regions), we emphasize the magnitude of the estimates in addition to statistical significance Recent work shows that conventional clustered standard errors may be severely inflated when sampled clusters constitute a non-negligible share of the population clusters (Abadie et al. 2023).

though there is a slight decline in maternal employment just prior to the school closure. Nonetheless, there is evidence that following the school closure, maternal employment declines, with larger effects occurring with a lag. This finding is not as pronounced for women without school-aged children, suggesting a causal impact of school closures. In contrast, fathers show little employment change during the school closure and a post-closure increase that is broadly similar to that of the placebo group (row 1, column 1). Together, these findings are consistent with a gendered response to school closures where mothers, but not fathers reduce their labor supply in response to the closure. Intensive margin responses are more nuanced, though in the main, there is less consistent evidence that work hours were significantly differentially affected for parents of school-aged children as compared to childless adults.

However, our evidence does point to school closure-induced changes in where work takes place. Both parents are more likely to work from home during closures, but the increase persists only for mothers. For fathers, the increase in remote work is not consistent with TWFE estimates. The placebo group shows no comparable change, and we find no evidence of pre-trends in working from home. We note that mothers' increase in remote work coincides with no change in working hours, implying that closures affected the structure of work rather than reducing work hours among working mothers.

Using our binary treatment indicator, Table 4 presents the overall ATT using a stacked DID estimator.[35] The findings are similar to our TWFE estimates with respect to gendered effects of school closures on working from home and employment. Specifically, Panel A in Table 3 shows that school closures reduced mothers' employment by 4.8 pp and increased the likelihood of working from home by 4.3 pp, while effects on hours worked are small and statistically insignificant. Fathers' labor supply responses are small and not statistically significant across all outcomes. As above, Panel B reports

[35] Appendix A11 reports estimates of the immediate effects of school closures.

estimates for adults without children under age 18, who serve as a placebo group. Estimates for women in this group are small and statistically insignificant across all outcomes. Estimates of the difference in the stacked DID estimates in Panels A vs Panel B show that they are statistically significantly different for mothers' working from home. Taken together, these results suggest that school closures primarily affected the labor supply of mothers with school-aged children, particularly through increased remote work.

Finally, we examine whether school closures led to a reallocation of work across non-standard work hours and days. Figure 3 shows event-study evidence of shifts toward evening and night work for mothers during the closure period. Moreover, while the impact of school closures on weekend and holiday work is not statistically significant in the post-treatment period, the estimated post-treatment effects are positive. These results are consistent with a partial reorganization of work timing, suggesting that mothers may have shifted some work to non-standard hours, which may help explain why school closures do not reduce total hours worked.

5 Discussion and Conclusion

This study is among the first to study the impact of school closures in Russia on labor supply of parents of younger school-aged children. Using panel data from Russia and a novel dataset of daily, grade-specific regional school closure policies, along with a difference-in-differences identification strategy, we find strong evidence of a gendered response to school closures: closures reduce employment among mothers by approximately 10-15 percent while having no impact on fathers of school-aged children. This result is consistent with mothers in Russia serving as the primary childcare providers for young children. Placebo tests on married adults without school-aged children and event-study analysis using stacked DID estimates suggest a causal interpretation of our findings. Our results suggest a gender-specific response to COVID-19 school closures in Russia that is consistent with what has been uncovered in the United States (Hansen et al. 2024), Canada (Lillard et al. 2026), Germany

(Imre et al. 2026), and South Korea (Bak et al. 2026).

Across specifications, we find robust evidence that school closures lead to an increase in working from home among mothers of school-aged children, with particularly strong effects among college-educated mothers, who may have greater job flexibility. The persistent increase in remote work and the shift toward non-standard evening and nighttime hours, combined with no clear response in total hours among those working, suggest that school closures lead working mothers to reorganize how work is performed.

Overall, our findings underscore the role that flexible and targeted family leave and childcare policies can play during public health crises, as one-size-fits-all interventions are unlikely to match the diverse needs of households. Future research should explore the long-term consequences of these labor market and educational disruptions on earnings, career trajectories, and child outcomes, and examine how more nuanced school closure policies, such as staggered school schedules or hybrid learning, can mitigate adverse effects in future crises.

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7 Tables

Table 1: TWFE Estimates of the Effect of School Closures on Parental Labor Supply in Russia

	Women			Men		
	Worked	Log (Hours)	Worked from Home	Worked	Log (Hours)	Worked from Home
	Panel A. Parents of school-aged children					
School Closure	-0.103**	-0.001	0.101**	-0.038	-0.004	-0.005
	(0.040)	(0.042)	(0.043)	(0.027)	(0.035)	(0.022)
Mean Y	0.717	5.126	0.067	0.866	5.237	0.030
N	[8,371]	[5,523]	[5,635]	[6,589]	[5,132]	[5,520]
	Panel B. Childless adults					
School Closure	0.031	-0.000	0.016	-0.023	0.016	0.009
	(0.039)	(0.030)	(0.029)	(0.048)	(0.045)	(0.015)
Mean Y	0.624	5.116	0.062	0.651	5.195	0.024
N	[16,807]	[9,566]	[9,796]	[13,564]	[7,701]	[8,341]
p-value: A vs B	0.001	0.982	0.080	0.799	0.737	0.587

Notes: Estimates report the coefficient on school closure duration from TWFE DID Equation (1), capturing its effect on labor supply. Closures are measured as the share of business days closed in the past 30 days (averaged across grades 1-8). Samples include: (A) all parents of school-aged children in grades 1-8; (B) all adults 18-60 with no children under age 18. All regressions include individual fixed effects and control for regional COVID-19 cases per 100 people, poverty and unemployment rates, COVID-19–related workplace closures, extended holidays, school breaks, and other variables. The full list of controls is described in Section 3.1. Variables are defined in Appendix Table A6; full results for Panel A are in Appendix Table A8. Standard errors are clustered at the regional level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Mean Y reports the sample mean of the dependent variable for each column. The number of observations is reported in brackets. *P*-values correspond to tests of equality of coefficients between indicated panels.

Table 2: TWFE Estimates of the Effect of School Closures on Married Parents' Labor Supply in Russia

	Women			Men		
	Worked	Log (Hours)	Worked from Home	Worked	Log (Hours)	Worked from Home
	Panel A. Married parents of school-aged children					
School Closure	-0.087**	0.010	0.146***	-0.041*	-0.003	-0.006
	(0.041)	(0.047)	(0.038)	(0.022)	(0.034)	(0.022)
Mean Y	0.701	5.119	0.065	0.869	5.237	0.029
N	[6,957]	[4,448]	[4,551]	[6,475]	[5,060]	[5,444]
	Panel B. Married childless adults					
School Closure	0.051	-0.001	0.030	0.020	0.026	0.037
	(0.044)	(0.035)	(0.035)	(0.054)	(0.050)	(0.023)
Mean Y	0.669	5.113	0.063	0.773	5.205	0.021
N	[8,363]	[5,116]	[5,266]	[6,905]	[4,685]	[5,115]
p-value: A vs B	0.003	0.843	0.004	0.343	0.605	0.134

Notes: Estimates report the coefficient on school closure duration from TWFE DID Equation (1), capturing its effect on labor supply. Closures are measured as the share of business days closed in the past 30 days (averaged across grades 1-8). Samples include: (A) married parents of school-aged children in grades 1-8; (B) married adults 18-60 with no children under age 18. All regressions include individual fixed effects and control for regional COVID-19 cases per 100 people, poverty and unemployment rates, COVID-19–related workplace closures, extended holidays, school breaks, and other variables. The full list of controls is described in Section 3.1. Variables are defined in Appendix Table A6; full results for Panel A are in Appendix Table A8. Standard errors are clustered at the regional level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Mean Y reports the sample mean of the dependent variable for each column. The number of observations is reported in brackets. *P*-values correspond to tests of equality of coefficients between indicated panels.

Table 3: Heterogeneity in the Effects of School Closures on Parental Labor Supply

	Women			Men		
	Worked	Log (Hours)	Worked from Home	Worked	Log (Hours)	Worked from Home
	Panel A. College-educated parents					
School Closure	-0.084	-0.035	0.168**	-0.053	-0.077	-0.076
	(0.066)	(0.044)	(0.067)	(0.066)	(0.058)	(0.078)
Mean Y	0.794	5.121	0.113	0.927	5.210	0.0709
N	[3,560]	[2,620]	[2,632]	[1,968]	[1,675]	[1,749]
	Panel B. Less-educated parents					
School Closure	-0.108*	0.043	0.029	-0.054	0.006	0.009
	(0.062)	(0.090)	(0.050)	(0.052)	(0.043)	(0.015)
Mean Y	0.655	5.132	0.022	0.837	5.251	0.010
N	[4,503]	[2,704]	[2,804]	[4,348]	[3,254]	[3,557]
p-value: A vs B	0.812	0.445	0.044	0.993	0.168	0.301
	Panel C. Presence of older household member					
School Closure	-0.082	-0.208**	-0.025	0.079	-0.116	-0.095
	(0.070)	(0.105)	(0.105)	(0.074)	(0.138)	(0.070)
Mean Y	0.682	5.142	0.066	0.823	5.245	0.029
N	[1,526]	[952]	[968]	[1065]	[793]	[840]
	Panel D. No older household member					
School Closure	-0.107**	0.033	0.130**	-0.067**	-0.008	0.009
	(0.050)	(0.057)	(0.052)	(0.031)	(0.053)	(0.025)
Mean Y	0.725	5.122	0.067	0.873	5.236	0.030
N	[6,505]	[4,356]	[4,448]	[5,234]	[4,115]	[4,440]
p-value: C vs D	0.786	0.071	0.169	0.083	0.517	0.147

Notes: Estimates report the coefficient on school closure duration from TWFE DID Equation (1), capturing its effect on labor supply. Closures are measured as the share of business days closed in the past 30 days (averaged across grades 1–8). Sample includes all parents of school-aged children in grades 1-8. It is split by parental college attainment at baseline before the pandemic (Panels A-B), and by the presence of an older household member (age>60) before the pandemic (Panels C-D). All regressions include individual fixed effects and control for regional COVID-19 cases per 100 people, poverty and unemployment rates, COVID-19–related workplace closures, extended holidays, school breaks, and other variables. The full list of controls is provided in Section 3.1. Variables are defined in Appendix Table A6. Standard errors are clustered at the regional level. *** p<0.01, ** p<0.05, * p<0.1. Mean Y reports the sample mean of the dependent variable for each column. The number of observations is reported in brackets. *P*-values correspond to tests of equality of coefficients between indicated panels.

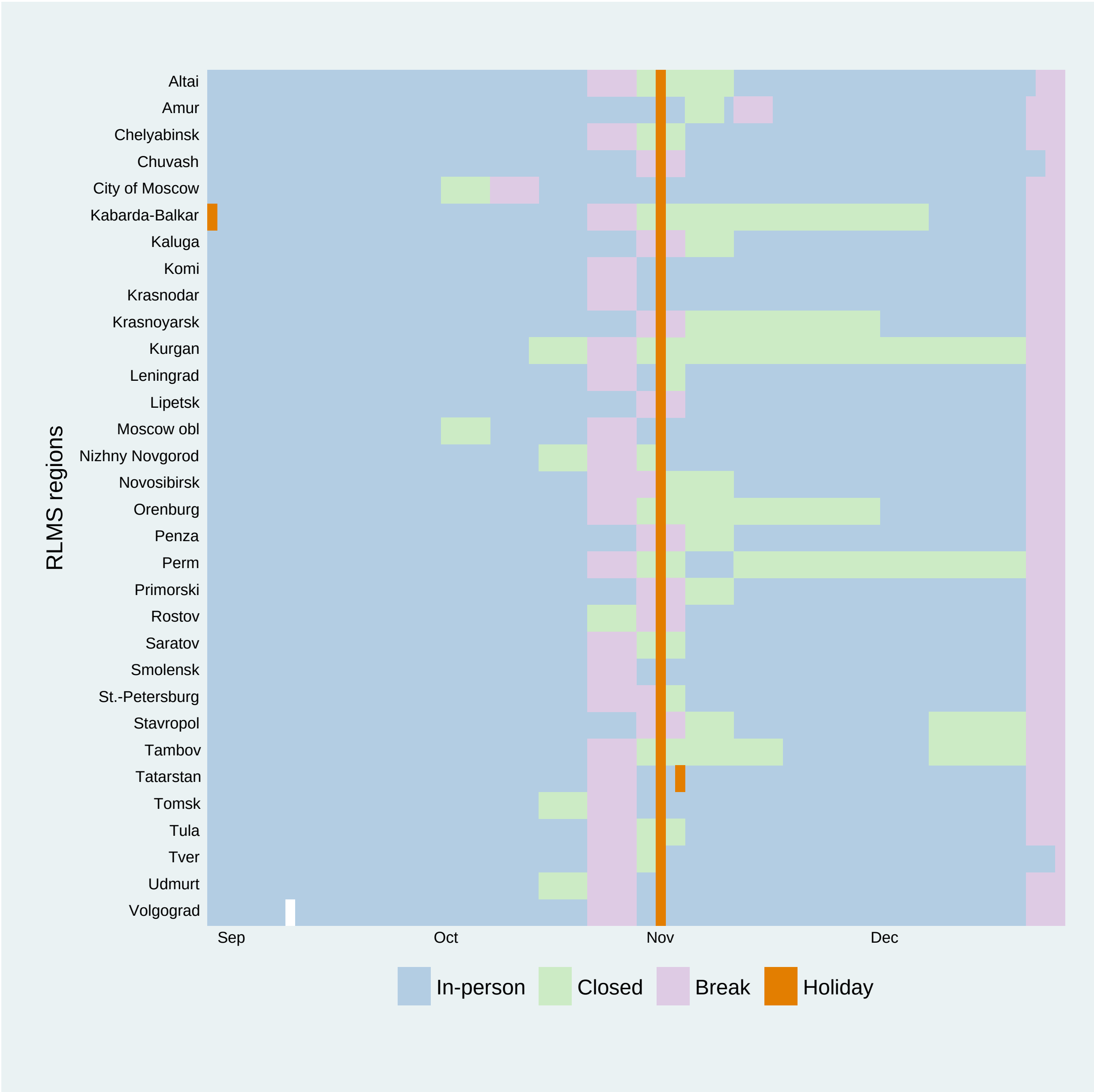
Table 4: Stacked DID Estimates of the Effect of School Closures on Labor Supply

	Women			Men		
	Worked	Log (Hours)	Worked from Home	Worked	Log (Hours)	Worked from Home
	Panel A. Parents of school-aged children					
School Closure	-0.048*	0.022	0.043**	0.010	0.008	0.018
(binary)	(0.027)	(0.026)	(0.019)	(0.019)	(0.020)	(0.015)
Mean Y	0.712	5.091	0.053	0.872	5.228	0.027
N	[23,058]	[15,458]	[15,772]	[18,877]	[14,876]	[15,985]
	Panel B. Childless adults					
School Closure	0.003	0.019	0.010	0.015	-0.015	-0.011*
(binary)	(0.022)	(0.021)	(0.013)	(0.027)	(0.024)	(0.006)
Mean Y	0.616	5.104	0.059	0.667	5.173	0.021
N	[42,016]	[23,945]	[25,077]	[34,591]	[20,859]	[22,635]
p-value: A vs B	0.112	0.903	0.018	0.884	0.508	0.102

Notes: Estimates report coefficients from a stacked difference-in-differences model of the effect of school closures on labor supply. Included are school closure events that last for more than three days. The reported coefficients correspond to the interaction between an indicator for the school-closure period (from the start of closure through the following two months) and an indicator for the treated region. Samples include: (A) all parents of school-aged children in grades 1-8; (B) all adults 18-60 with no children under age 18. All regressions include region, calendar month, and event (school closure) fixed effects and a set of controls for regional COVID-19 cases per 100 people, poverty and unemployment rates, COVID-19–related workplace closures, extended holidays, school breaks, and other variables. The full list of controls is described in Section 3.1, and variable definitions appear in Appendix Table A6. Standard errors are clustered at the region-by-event level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Mean Y reports the sample mean of the dependent variable for each column. The number of observations is reported in brackets. Estimates are weighted by the share of treated observations in the estimation sample. *P*-values correspond to tests of equality of coefficients between indicated panels.

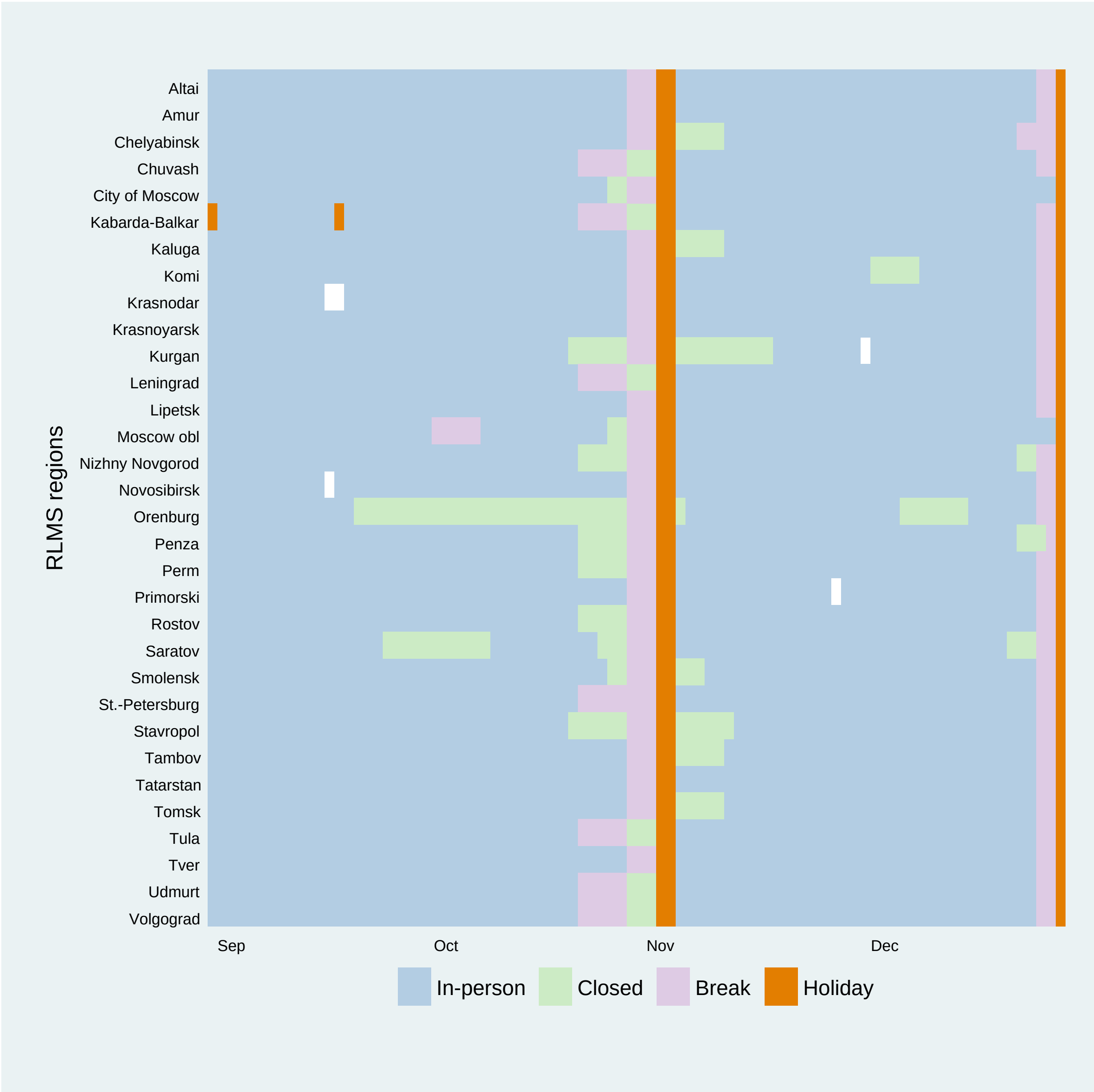
8 Figures

Figure 1: Timeline of School Closings, Fall 2020



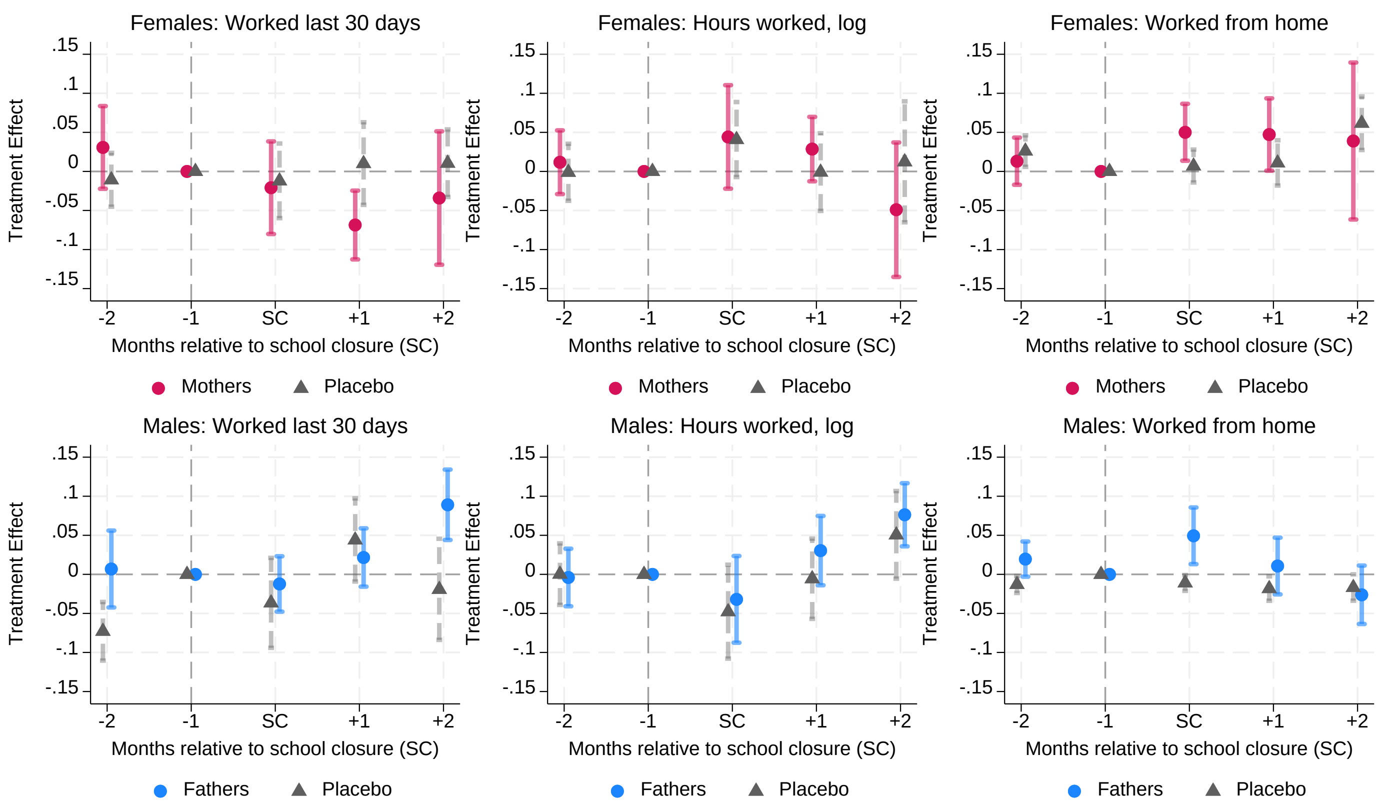
Notes: The figure presents the timeline of schooling modes for 5th graders in 32 RLMS regions from September 1, 2020, to December 31, 2020. The timeline indicates in-person days, no school days for COVID-related reasons, regular breaks, and holidays. Weekends are excluded from the timeline. White cells show days when schools were closed for reasons unrelated to COVID-19 (e.g., voting, security, or inclement weather).

Fall 2021



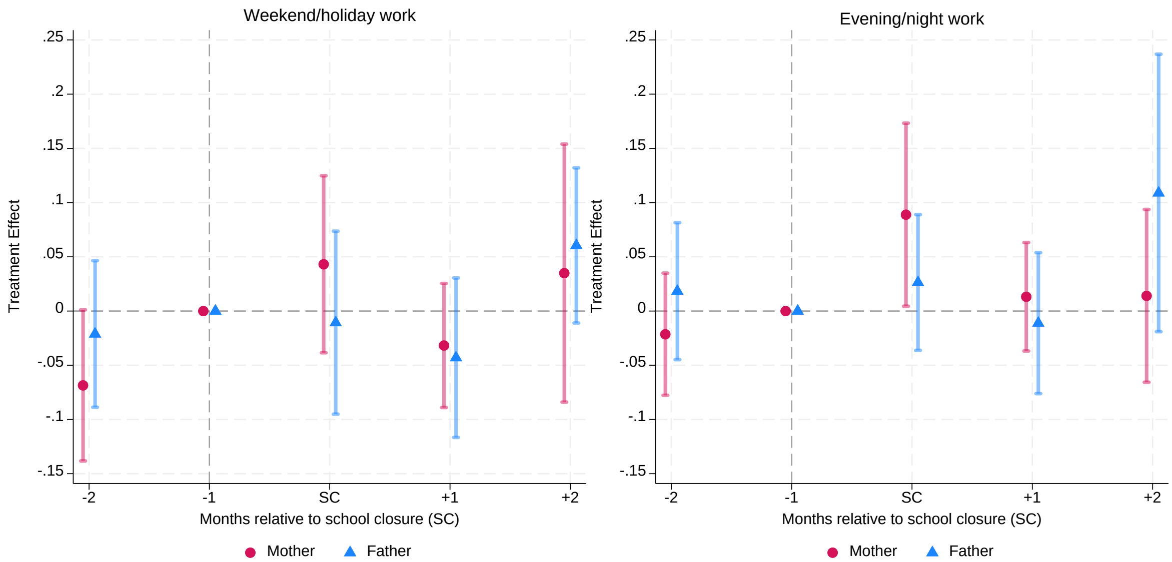
Notes: The figure presents the timeline of schooling modes for 5th graders in 32 RLMS regions from September 1, 2021, to December 31, 2021. The timeline indicates in-person days, no school days for COVID-related reasons, regular breaks, and holidays. Weekends are excluded from the timeline. White cells show days when schools were closed for reasons unrelated to COVID-19 (e.g., voting, security, or inclement weather).

Figure 2: Event-Study Estimates of Labor Supply Responses to School Closures



Notes: The figure reports event-time estimates of the effects of school closures on labor supply outcomes separately for women and men. Estimates correspond to Equation (2), using the same set of control variables as in Equation (1) in Section 3.1. The graphs in the first row correspond to females, and the graphs in the second row correspond to males. Within each panel, estimates for parents of children in grades 1–8 are shown by the solid line, while estimates for married adults without children under age 18 (placebo group) are shown by the dashed line. The sample includes only school-closure episodes lasting more than three days. Because there are no never-adopters, the counterfactual group consists of observations not treated during each event window. The control group excludes any observations from regions that experience a closure within a window spanning four months before and two months after each school closure episode. Outcomes include working in the last 30 days, log hours worked, and working from home. Event time is measured in months relative to school closure (SC), with the month before closure (−1) as the reference period. Points represent estimated effects from a stacked DID specification in Equation (2), and vertical bars show 90 percent confidence intervals. The sample is restricted to a two-month window around each event, and standard errors are clustered at the region-by-event level.

Figure 3: Event-Study Estimates of Work Schedule Responses to School Closures

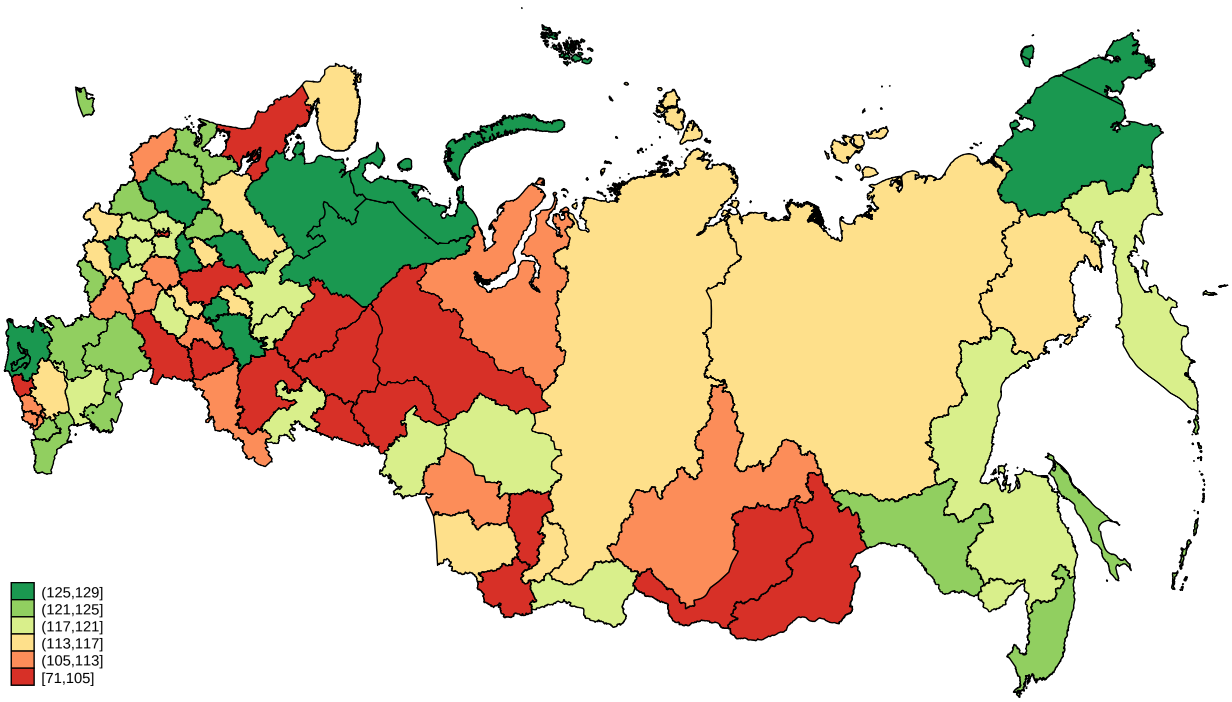


Notes: The figure reports event-time estimates of the effects of school closures on non-standard work hours. Non-standard work hours include weekend/holiday work and evening/night work for mothers (red line) and fathers (blue line). Event time is measured in months relative to the start of the closure (SC), with the month before closure (-1) as the reference period. Points represent estimated effects from a stacked DID specification in Equation (2), and vertical bars show 90 percent confidence intervals. The sample is restricted to a two-month window around each event, and standard errors are clustered at the region-by-event level. The control group excludes any observations from regions that experience closure within a window spanning four months before and two months after each school closure episode

Online Appendix to

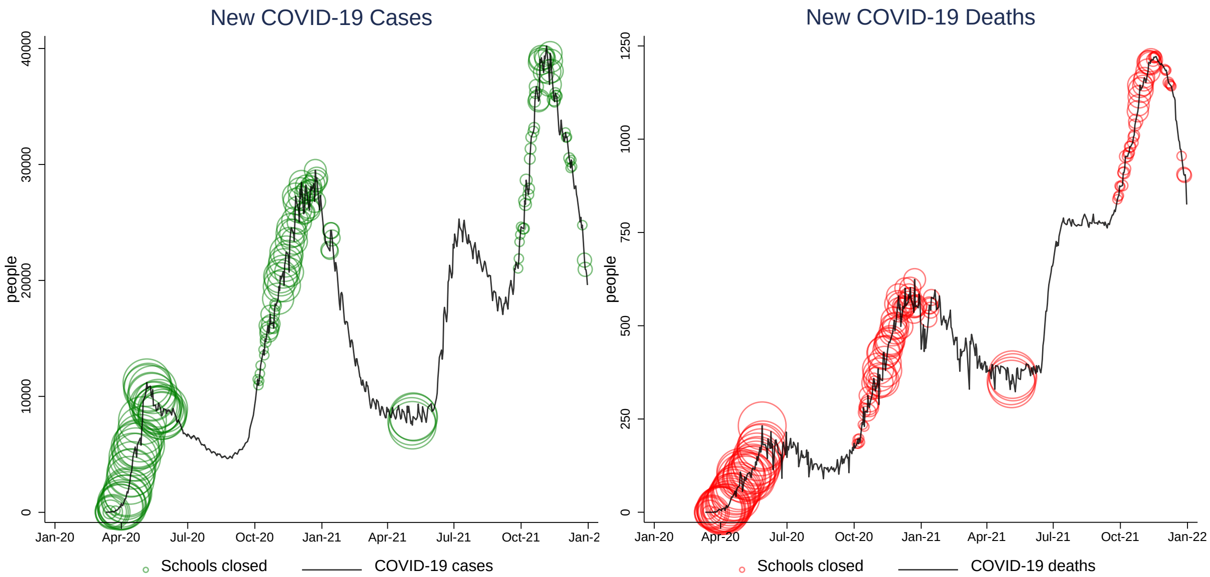
"When Schools Close in Russia: Effects on Parental Labor Supply and Work from Home"

Figure A1: In-Person School Days, 2020



Notes: The map depicts the average number of in-person school days in 2020 for grades 1-8. The regional dispersion is extensive, with in-person days ranging from 71 days in Zabaikalsk Krai (Southeast Siberia) to 129 days in Chuvash Republic (in the Volga Upland). By comparison, a typical year includes about 170 in-person school days. The map reveals no visible clustering of in-person days within a broader geographic area. Both the map and our review of regional government policies suggest that regional authorities made their own schooling decisions in response to the spread of COVID-19, and there is no evident spatial spillover effect.

Figure A2: School Closings and the Spread of COVID-19



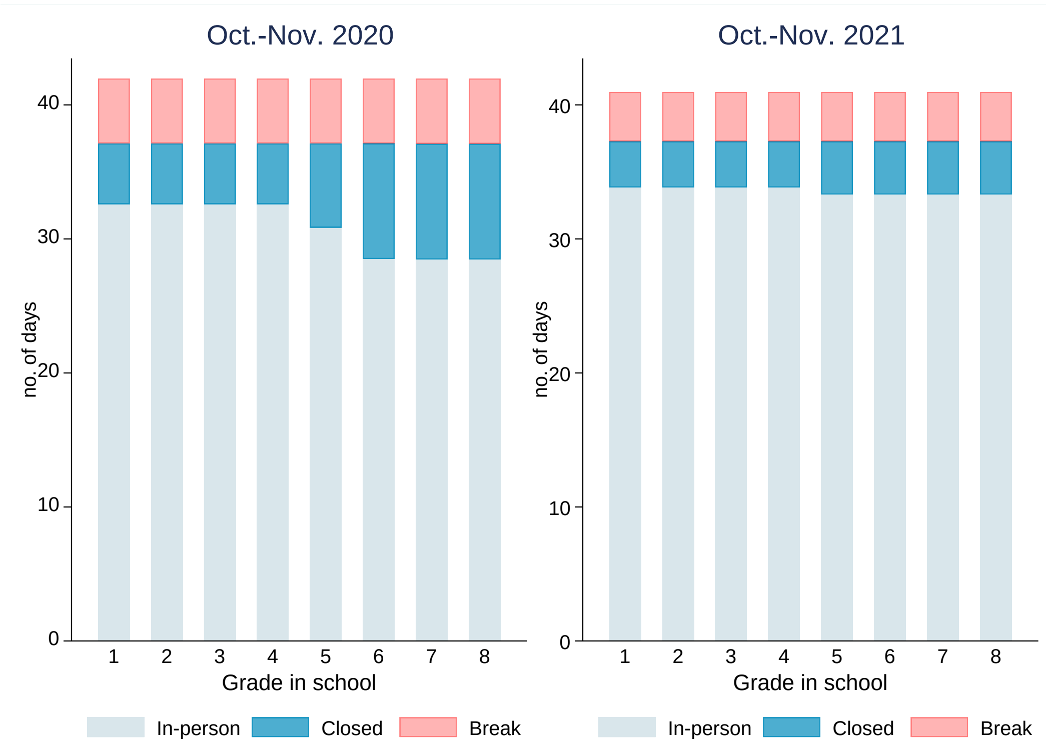
Notes: Figure plots daily confirmed COVID-19 cases and deaths in Russia. The size of the hollow marker is proportional to the number of Russian regions (max 83) where schools have been closed due to COVID.

Table A3: Average Number of School Closure Days by Grade and Month

	2020 m9	2020 m10	2020 m11	2020 m12	2021 m1	2021 m9	2021 m10	2021 m11	2021 m12	2022 m1
				RLMS regions w school closures						
Grades 1-4	…	5.2	5.7	5.0	5.0	3.0	4.8	4.4	4.0	3.1
Grades 5-8	…	6.2	9.9	11.4	5.0	4.5	6.3	4.1	4.0	3.1
				All RLMS regions						
Grades 1-4	0.0	1.0	3.6	0.2	0.3	0.1	1.7	1.8	0.6	0.7
Grades 5-8	0.0	1.4	6.4	3.6	0.5	0.3	2.2	1.8	0.6	0.7

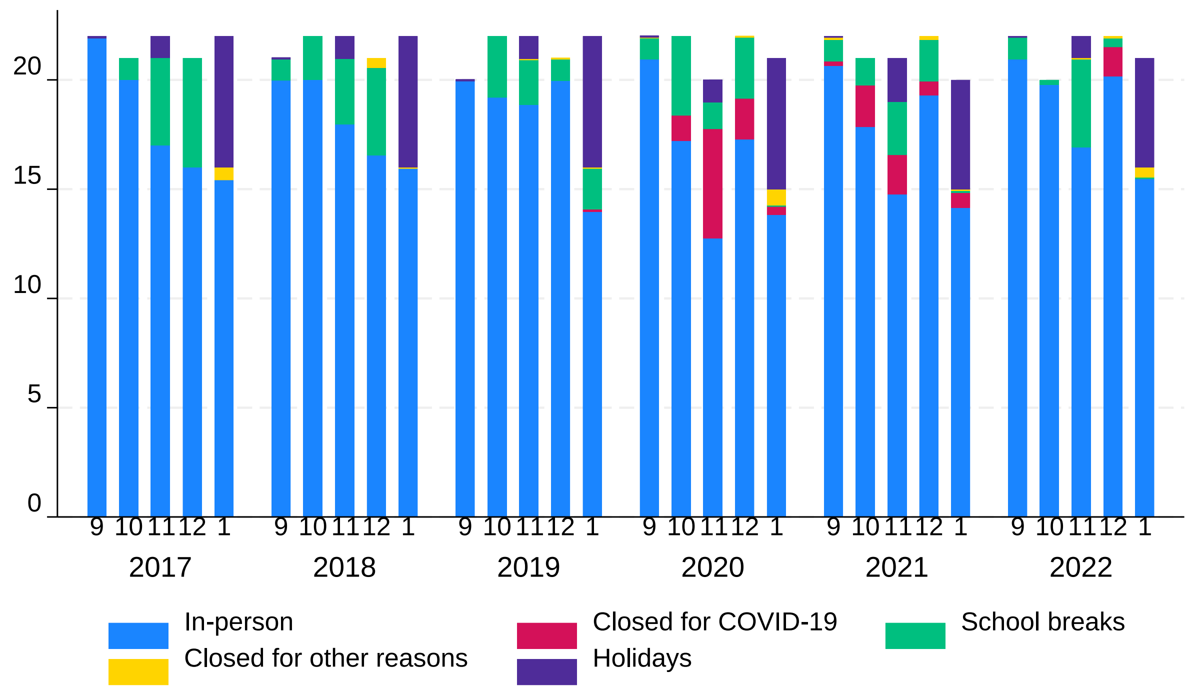
Notes: The table shows the average number of workdays during which schools were closed for COVID-19 reasons. The data is averaged across 32 RLMS regions and school grades 1 to 4 and 5 to 8.

Figure A4: Distribution of Business Days by Grade and Schooling Mode



Notes: The figure plots the average number of business days during which schools were open for in-person learning, closed for COVID-19 reasons, or closed for a fall break. The average is calculated across 32 RLMS regions for the October-November time periods of each year.

Figure A5: Distribution of Days by Schooling Mode and Survey Round



Notes: The figure shows the distribution of non-weekend days during which schools were open for in-person learning, closed for COVID-19 reasons, closed for fall break, closed for inclement weather and other reasons unrelated to COVID, and closed for federal or regional holidays. The data are averaged across 32 RLMS regions and school grades 1 through 8. Years indicate the survey rounds, spanning from September to January of the subsequent year.

Table A6: Definition of Variables

Variable	Definition
	Labor supply dependent variables
Worked	=1 if worked for pay or profit at a primary or secondary job in the last 30 days; =0 if did not work during that period, including those on long-term leave from a job.
Total hours worked	Hours worked at primary and secondary jobs in the last 30 days, including hours worked from home.
Worked from home	=1 if worked from home at the primary job for least 10 hours in the last 30 days; =0 if worked at the primary job but not from home in the last 30 days. No information is available on working from home at a secondary job.
Weekend/holiday work	=1 if worked on a weekend or holiday at least once per month.
Evening/night work	=1 if worked late in the evening or at night (9 p.m.–6 a.m.) at least a few times per month.
	Parents' characteristics
Age	Parental age.

Age squared	Quadratic term for parental age.
College	=1 if the parent has a college degree.
Russian ethnicity	=1 if the individual identifies ethnically Russian.
Birthplace	Categorized into four groups: born in the same place as current residence (omitted), born elsewhere in Russia, born abroad, or unknown place of birth.
Parent's health problems 30d	=1 if the parent had any health problems in the last 30 days.
	Child's Characteristics
Child's grade level	Represents the grade level the child currently attends, obtained from a direct survey question. This variable is used to link the RLMS-HSE with grade-level policies. It is highly correlated with the child's age (corr=0.97).
Middle school	=1 if the youngest school-age child is in middle school.
Child's health problems 30d	=1 if the child had health problems in the last 30 days.
	Household Characteristics
Older household member	=1 if there is an older household member (age > 60) prior to the pandemic.
	Schooling Policy Tracker
School closure	The share of business days in the last 30 days when schools were closed for in-person learning due to COVID-19-related reasons. Occasionally, closures may have been due to other flu-like viruses. Business days are defined as 30 days minus weekends and holidays. This measure varies by region and interview date.
Prominent closure	=1 if the school closure lasted for more than 3 days (at least half of the week) in the last 30 days.
School breaks	The share of business days in the last 30 days when schools were closed due to regular school breaks. This measure varies by region and interview date.
Long holidays	=1 if a federal or regional holiday lasted longer than one working day in the last 30 days. This measure varies by region and interview date.
	Regional characteristics
Workplace closure	The share of business days in the last 30 days that were designated as COVID-19-related non-working days. 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 non-working days. This measure varies by region and date of interview.
COVID-19 spread 30d	The product of new confirmed COVID-19 cases per 100 people in the last 30 days, multiplied by the region-specific discrepancy factor. The discrepancy factor is the ratio of the annual COVID-19 morbidity level to the annual sum of daily coronavirus cases. Sources: Yandex Coronavirus Database, Rosstat Regions of Russia. This varies by region and date of interview.
Unemployment rate, %	The monthly unemployment rate, in percent. This varies by region and month of interview. Calculated using the International Labor Organization methodology. Data source: Rosstat, https://rosstat.gov.ru/folder/210/document/13211.

Poverty rate, %	The monthly poverty rate, in percent. This varies by region and year. Data source: Rosstat, https://rosstat.gov.ru/folder/13723.

Table A7: Summary Statistics

	Mothers	Fathers
School closure	0.024	0.026
	(0.088)	(0.092)
School breaks	0.099	0.101
	(0.125)	(0.125)
Workplace closure	0.010	0.011
	(0.048)	(0.051)
Long holidays	0.069	0.070
Age	37.458	39.604
	(5.640)	(6.333)
College	0.435	0.306
Russian ethnicity	0.891	0.856
Birthplace		
Born elsewhere in Russia	0.329	0.339
Born abroad	0.070	0.081
Unknown place of birth	0.004	0.006
Parent's health problems 30d	0.232	0.179
Middle school	0.402	0.388
Child's health problems 30d	0.276	0.270
Older household member	0.191	0.170
COVID-19 spread 30d	0.345	0.351
	(0.516)	(0.518)
Unemployment rate, %	4.646	4.682
	(1.888)	(1.882)
Poverty rate, %	12.155	12.262
	(4.029)	(4.005)
Observations	8,371	6,589

Notes: The table reports means and standard deviations (in parentheses) of covariates for main estimation sample described in section 2.1. Variable definitions are provided in Table A6.

Table A8: Complete Specification

	Women			Men		
	Worked	Log (Hours)	Worked from Home	Worked	Log (Hours)	Worked from Home
School closure	-0.103**	-0.001	0.101**	-0.038	-0.004	-0.005
	(0.040)	(0.042)	(0.043)	(0.027)	(0.035)	(0.022)
School breaks	-0.084	-0.114**	-0.022	-0.126**	0.075	0.003
	(0.055)	(0.051)	(0.049)	(0.047)	(0.055)	(0.041)
Workplace closure	0.117	-0.194	0.015	-0.033	0.020	-0.137
closure, last 30d	(0.180)	(0.175)	(0.147)	(0.108)	(0.130)	(0.159)
Long holidays	-0.038	0.048***	0.032*	0.020	-0.013	0.011
	(0.025)	(0.015)	(0.017)	(0.028)	(0.024)	(0.016)
Age	0.058**	0.029	0.025	-0.009	0.016	-0.017
	(0.026)	(0.024)	(0.022)	(0.017)	(0.015)	(0.011)
Age squared	-0.001*	-0.000	-0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
College	0.056	0.021	0.066**	0.012	0.007	0.006
	(0.044)	(0.043)	(0.028)	(0.038)	(0.030)	(0.028)
Russian ethnicity	0.026	0.051	-0.011	0.012	0.028	-0.022
	(0.023)	(0.049)	(0.007)	(0.070)	(0.109)	(0.017)
Birthplace						
Born elsewhere in Russia	0.089	-0.057	-0.034	0.059	-0.014	0.000
	(0.061)	(0.069)	(0.033)	(0.058)	(0.051)	(0.003)
Born abroad	-0.008	-0.185**	-0.287*	-0.162	0.002	0.002
	(0.118)	(0.069)	(0.141)	(0.167)	(0.049)	(0.007)
Unknown place of birth	-0.429	-0.204**	-0.060	0.009	0.128***	0.008
	(0.402)	(0.077)	(0.041)	(0.019)	(0.017)	(0.016)
Middle school	0.006	0.020**	-0.003	-0.010	-0.005	-0.008
	(0.011)	(0.008)	(0.007)	(0.007)	(0.012)	(0.005)
COVID-19 spread 30d	0.001	0.005	-0.003	-0.015	-0.013	0.007
	(0.010)	(0.013)	(0.012)	(0.010)	(0.011)	(0.009)
Unemployment rate, %	-0.006	-0.004	0.000	-0.007	0.001	-0.006
	(0.006)	(0.007)	(0.007)	(0.005)	(0.006)	(0.004)
Poverty rate, %	0.002	-0.008	0.004	-0.021*	0.002	-0.004
	(0.006)	(0.009)	(0.009)	(0.012)	(0.014)	(0.007)
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,371	5,523	5,635	6,589	5,132	5,520
R-squared	0.660	0.549	0.613	0.683	0.569	0.598

Notes: The table presents estimates of the baseline specification of Equation (1), including individual and year-month fixed effects. Robust standard errors (in parentheses) are clustered at the regional level. *** p<0.01, ** p<0.05, * p<0.1. Omitted category is *born in the same place* (birthplace). Variable definitions are provided in Appendix Table A6. Variables typically assumed to be time-invariant, such as ethnicity and birthplace, are included due to minor changes in reporting or self-identification over time.

Table A9: Falsification Test

	Women			Men	
Worked	Log (Hours)	Worked from Home	Worked	Log (Hours)	Worked from Home
Panel A. Married adults without children under 18					
0.051	-0.001	0.030	0.020	0.026	0.037
(0.044)	(0.035)	(0.035)	(0.054)	(0.050)	(0.023)
[8,363]	[5,116]	[5,266]	[6,905]	[4,685]	[5,115]
Panel B. Single adults without children under 18					
0.014	0.007	0.002	-0.060	-0.037	-0.047
(0.045)	(0.047)	(0.044)	(0.062)	(0.090)	(0.030)
[8,099]	[4,205]	[4,294]	[6,415]	[2,847]	[3,035]
Panel C. Parents of children under 6					
-0.131	-0.245	-0.218	0.007	0.084	-0.061
(0.125)	(0.190)	(0.152)	(0.125)	(0.088)	(0.076)
[1,821]	[528]	[540]	[1,496]	[1,173]	[1,259]

Notes: Estimates report the coefficient on school closure duration from TWFE DID Equation (1), capturing its effect on labor supply in placebo groups. This table presents a falsification (placebo) test. Closures are measured as the share of business days closed in the past 30 days (averaged across grades 1–8). Samples include: (i) married adults without children; (ii) single adults without children; (iii) parents of children under age 6. All samples exclude parents who had children in grades 1-8 during the pandemic (2020-2022). All estimates include individual FEs. Dependent variables are defined in Appendix Table A6. Standard errors clustered at the regional level. *** p<0.01, ** p<0.05, * p<0.1. The number of observations is reported in brackets.

Table A10: Effects of School Closures on Parental Labor Supply by Child's Grade Level

	Women			Men		
	Worked	Log (Hours)	Worked from Home	Worked	Log (Hours)	Worked from Home
	Panel A. Parents of elementary-school children (grades 1–4)					
School Closure	-0.155**	-0.197***	0.048	0.004	-0.002	-0.015
	(0.060)	(0.053)	(0.070)	(0.049)	(0.060)	(0.033)
Mean Y	0.682	5.123	0.059	0.863	5.238	0.030
N	[3,555]	[2,181]	[2,218]	[2,910]	[2,218]	[2,407]
	Panel B. Parents of middle-school children (grades 5–8)					
School Closure	-0.061	0.126**	0.143*	-0.069	-0.029	0.009
	(0.048)	(0.057)	(0.074)	(0.059)	(0.045)	(0.029)
Mean Y	0.742	5.128	0.072	0.868	5.236	0.029
N	[4,814]	[3,340]	[3,415]	[3,677]	[2,912]	[3,111]

Notes: Estimates report the coefficient on school closure duration from TWFE DID Equation (1), capturing its effect on parental labor supply. Closures are measured as the share of business days closed in the past 30 days (averaged across grades 1–8). The sample is split by child's grade level. All regressions control for regional COVID-19 case count per 100 people, poverty and unemployment rates, COVID-19–related workplace closures, extended holidays, and school breaks. The full list of controls is provided in Section 3.1. Variables are defined in Appendix Table A6. Standard errors are clustered at the regional level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Mean Y reports the sample mean of the dependent variable for each column. The number of observations are reported in brackets.

The results in this table reveal substantial variation in responses by child age, particularly among mothers. Mothers of younger children (grades 1–4), who face the most intensive childcare demands, experience large and statistically significant declines in the probability of working (−0.155) and total hours (−0.196). In contrast, mothers of older children (grades 5–8) show no employment response but increase hours and shift toward remote work. This result suggests that when childcare demands are binding, mothers adjust by reducing labor supply, whereas when constraints are less severe, they reorganize work rather than exit employment, and that working from home may require more total hours. Fathers show no statistically significant responses in either group.

Table A11: Immediate Effects of School Closures on Labor Supply: Stacked DID Estimates

	Women			Men		
	Worked	Log (Hours)	Worked from Home	Worked	Log (Hours)	Worked from Home
	Panel A. Parents of school-aged children					
School Closure	-0.027	0.043	0.046**	-0.014	-0.031	0.043**
(binary)	(0.038)	(0.041)	(0.020)	(0.022)	(0.033)	(0.022)
Mean Y	0.712	5.091	0.053	0.872	5.228	0.027
N	[23,058]	[15,458]	[15,772]	[18,877]	[14,876]	[15,985]
	Panel B. Childless adults					
School Closure	-0.006	0.038	-0.001	-0.015	-0.047	-0.007
(binary)	(0.028)	(0.026)	(0.012)	(0.034)	(0.037)	(0.006)
Mean Y	0.616	5.104	0.059	0.667	5.173	0.021
N	[42,016]	[23,945]	[25,077]	[34,591]	[20,859]	[22,635]
p-value: A vs B	0.624	0.906	0.011	0.981	0.747	0.027

Notes: Estimates report coefficients from a stacked difference-in-differences model of the effect of school closures on labor supply. Included are school closure events that last for more than three days. The reported coefficient corresponds to the interaction between the treated-region indicator and an indicator for the closure period, defined as the month in which the school closure occurs. Samples include: (A) all parents of school-aged children in grades 1-8; (B) all adults 18-60 with no children under age 18. All regressions include region, calendar month, and event (school closure) fixed effects and a set of controls for regional COVID-19 cases per 100 people, poverty and unemployment rates, COVID-19–related workplace closures, extended holidays, school breaks, and other variables. The full list of controls is described in Section 3.1, and variable definitions appear in Appendix Table A6. Standard errors are clustered at the region-by-event level. *** p<0.01, ** p<0.05, * p<0.1. Mean Y reports the sample mean of the dependent variable for each column. The number of observations is reported in brackets. Estimates are weighted by the share of treated observations in the estimation sample. *P*-values correspond to tests of equality of coefficients between indicated panels.

Table A12: Estimated Effects of School Closures on the Labor Supply of Single Mothers

	Worked last 30 days	Total hours	Worked from home
School closure	-0.072	-0.148	-0.141
	(0.079)	(0.107)	(0.173)
N	[1,251]	[938]	[947]

Notes: Estimates report the coefficient on school closure duration from TWFE DID Equation (1), capturing its effect on labor supply. Closures are measured as the share of business days closed in the past 30 days (averaged across grades 1-8). The sample consists of single mothers of school-aged children. All regressions include individual fixed effects and control for regional COVID-19 cases per 100 people, poverty and unemployment rates, COVID-19–related workplace closures, extended holidays, school breaks, and other variables. The full list of controls is described in Section 3.1. Variables are defined in Appendix Table A6. Standard errors are clustered at the regional level. *** p<0.01, ** p<0.05, * p<0.1. Mean Y reports the sample mean of the dependent variable for each column. The number of observations is reported in brackets.